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JULY, 1853.

A. HESTER, M. D.,

EDITOR AND PROPRIETOR.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*

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TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

The following books, pamphlets, etc., have been received since our March issue:

Continued Fever. By F. E. Gordon, M. D., Marion, Alabama.

On Dysentery. By Dr. F. W. Moodie.

Principles of Organic and Physiological Chemistry. By Dr. Carl Lowig; Doctor of Medicine and Philosophy; Ordinary Professor of Chemistry in the University of Zurich; Author of "Chemie der Organischen Verbindungen." Translated by Daniel Breed, M. D., of the United States Patent Office, late of laboratory of Liebig and Lowig. Philadelphia. A. Hart, 1853; pp. 480. (From the Publisher, per J. B. Steel, N. O.)

The Transactions of the American Medical Association, Instituted 1847. *Vol.* 6; Philadelphia, 1852.

Atlas of Pathological Histology. By Dr. Gottlieb Gluge, Professor of Physiology and Pathological Anatomy, in the University of Bruxelles, Member of the Royal Academy of Sciences of Bruxelles, etc. Translated from the German, by Joseph Leidy, M. D., Pathologist to St. Joseph's Hospital, Philadelphia; Fellow of the College of Physicians, Philadelphia; Honorary Fellow of the Medical Society of Virginia; Corresponding Member of the Biological Society of Paris, etc. With 120 Figures, plain and colored; on 12 copper plate engravings. Philadelphia, Blanchard & Lea, 1853. (From the publishers, through J. B. Steel, 60 Camp street, New Orleans.

Character. A Valedictory Address to the Graduating Class of the Memphis Medical College, delivered by H. V. Wooten, M. D., Professor of the Principles and Practice of Medicine, at the Annual Commencement, February, 1853.

Introductory Lecture. By H. R. Robards, M. D., Professor of Surgery, in Memphis Medical College, Memphis, Tennessee.

Report of the Eastern Lunatic Asylum, in the City of Williamsburg, Virginia, 1853. Richmond, Va., 1853.

A Clinical Phrase Book, in English and German. Containing the usual questions and answers employed in examining and prescribing for patients; questions in asking for and buying medicines, etc., with an English-German and German-English Pronouncing Lexicon, of all the words occurring in the phrases, with the cheif technical terms of Medical Writers and Apothecaries; grammatical appendix, table of idioms, etc., designed to aid Physicians and Surgeons, in Hospitals, etc.; also Pharmaceutists in dispensing prescriptions. Ry Montgomery Johns, M. D; Philadelphia; Lindsay & Blackiston, 1853. (From the publisher, per T. L. White, New Orleans.

The Principles of Botany, as exemplified in the Cryptogamia, for the use of Schools and Colleges. By Harland Courtlas. Philadelphia; Lindsay & Blakiston, 1853. (From the publishers, per T. L. White, 105 Canal street, New Orleans.

Elements of Health, and Principles of Female Hygiene. By E. J. Tilt, M.D., Senior Physician to the Farringdon General Dispensary and Lying-in-Charity, and to the Paddington Free Dispensary for the Diseases of Women and Children. Philadelphia. Lindsay & Blackiston, 1853. (From the same, through T. L. White, 105 Canal street, New Orleans.

Natural Science in New Orleans. By Dr. B. Dowler. (*We hope every intelligent citizen of Louisiana will read this eloquent appeal in behalf of Natural Science.*

General Board of Health. Second Report on Quarantine—Yellow Fever With Appendixes. Presented to both Houses of Parliament, by com. mand of her Majesty. London, 1852. (From the Secretary of State's Office, through H. B. M. Consul, at New Orleans.

Appendix to the Report of the General Board of Health, on the Epidemic Cholera of 1848, '49. Abstract of a Report by James Wynne, M. D., on Epidemic Cholera, as it prevailed in the United States, in 1849, '50. Presented to both Houses of Parliament, by command of her Majesty. London, 1852. Through H. B. M. Consul, at New Orleans.

A Report on the Health and Mortality of the City of Memphis, for the year 1852. By Charles Todd Quintard, M. D. (From the Author.)

An Essay on the Unity of Disease, read before the Alabama State Medical Association, December, 1852. By H. Backus. (From the Author.)

General Board of Health. Minutes of information collected in respect to the Drainage of the Land forming the Sites of Towns, to Road Drainage, and the Facilitation of the Drainage of Suburban Lands. London, 1852. (From Wm. Mure, Esq., H. B. M. Consul.)

General Board of Health. Minutes of Information collected with reference to works for the Removal of Soil Water or Drainage of Dwelling Houses and Public Edifices, and for the Sewerage and Cleansing of the Sites of Towns. London, 1852. (From H. B. M. Consul, at New Orleans.)

Ocean Springs, Lynchburg, Jackson County, Mississippi. With some account of their origin, attributes, and the remarkable cures they have accomplished; with the testimony of distinguished physicians and eye-witnesses. New Orleans, 1853. (From Dr. Austin, the Author.)

Dr. H. A. Ramsay's Letter to Dr. James Bryan, on the Southern Negro, etc., March 1, 1853. Philadelphia. (From the Author.)

Galvanism: the Application as a Remedial Agent. By C. H. Cleveland, M. D. New York.

Treatment of Stricture of Urethra, by Rapid and Free Dilatation: Illustrated with Cases. By Paul F. Eve, M. D., Professor of Surgery, in the University of Nashville, Tennessee, 1853. (From the Author.)

Essay on the Sudden Coma of Typhus and Typhoid Pneumonia. With Illustrated Cases. By L. Lewis Smith, M. D. Reprinted from the New York Journal of Medicine, 1853. (From the Author.)

A Treatise on General Pathology. By Dr. J. Herrle, Professor of Anatomy and Physiology, in Heildelberg. Translated from the German, by H. C. Preston, A. M., M. D. Philadelphia. Lindsay & Blackiston, 1853. (From the Publishers.)

The Action of Medicine in the System; or "On the Mode in which Therapeutic Agents introduced into the Stomach, produce their peculiar effects on the Animal Economy. Prize Essay of the London Medical Society, etc. By Frederick William Headland, B. A, M. R. C. S., etc. Philadelphia. Lindsay & Blackiston, 1853. (From the Publishers.)

Also, all our customary Exchanges.

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MEDICAL AND SURGICAL JOURNAL.

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Part First.

ORIGINAL COMMUNICATIONS.

I.—REPORT ON THE LOCALITY OF PLANTS,

Or the Law of the Vegetable Kingdom, giving to some plants peculiar or superior properties, in certain localities, not possessed by the same plants elsewhere, and the application of this law to the cane and cotton plants.

Read before the Louisiana State Medical Society, at the annual meetiug, March, 1853,

BY SAMUEL A. CARTWRIGHT, M. D., NEW ORLEANS.
(Late of Natchez.)

The influence of soil and climate, on plants of the same species, is well illustrated in many of those which are in daily use for medicinal purposes. There is a secret something in locality influencing the properties of plants, which cannot be explained by soil and climate. Thus, Rhubarb and Opium can be grown almost anywhere, and so can Jalap and Scammony; but for medicinal purposes they cannot be relied on, unless the Scammony comes from Aleppo; the Jalap from that locality in Mexico from which it derived its name; the Opium from Turkey and the Rhubarb from the banks of the Wolga. The Romans named it Rheum from *Rha*, the ancient name of the river Wolga. The best Rhubarb grew there then and it grows there now. The Greeks called it Rha Ponticum, fixing thereby its locality beyond the *Pontus* or Eux.

ine Sea and on the river *Rha*. The Rhapontic Rhubarb is what is now called the best Turkey, and is imported into that country from the Wolga, and has not, like Opium, its proper locality there. Mr. Bolton, a member of this Society, deserves much credit for the pains he takes in procuring the Rhapontic Rhubarb. It is decidedly preferable to any other kind. It is so much prized by the profession, that in Europe its market price is about five dollars a pound, while the same root, grown in other localities than the Wolga, will scarcely command fifty cents. Although grown in apparently a similar climate and soil, the East India Opium has to be given in much larger doses than the Turkey to produce the same effects. There is something in locality influencing the properties of plants. Different plants have their special localities, or *empires*, as they may be called, especially belonging to them. When grown beyond their localities, or out of the limits of their respective empires, the virtues of those plants, used as medicines, are often so much altered or diminished, that they cannot be depended upon with any certainty. In England, and a great many other countries, the Rhubarb is cultivated, and makes a tolerable purgative in large doses. But it does not possess all the virtues which the Greeks ascribed to the Rhaponticum or the Romans to the Rheum. A little speck of it chewed and the saliva swallowed, will not remove constipation and strengthen the tone of the stomach, as it does when grown in its own empire on the banks of the *Rha*, and which has been its empire as far back as history extends; and as far back as history extends it has been tried in other places, but has invariably been found to lose more or less of its virtues as a medicine.

The limits of the locality, or proper empire of each plant, can only be ascertained by experience and observation; a knowledge of climate and soil is not sufficient to fix its boundaries. With this unalterable law of the vegetable kingdom in view, much labor might be saved and many useless and expensive experiments avoided. The celebrated Dr. Cox of Philadelphia spent much time and trouble in trying to grow Jalap in Pennsylvania. He succeeded very well in making the plant grow, and has left a history of his experiments with it, including his correspondence on the subject with Hulse, Barton, Nutall, and other distinguished botanists of his day;* from all which it plainly appears, that the medicinal Jalap of Mexico and the esculent root, the Mechoacan or wild yam of Georgia and Florida, are identical in their botanical characters; being the same plant endowed with different properties by different localities. There is good reason, therefore, to believe, that

* See Dr. Cox's Dispensatory, 9th edition. Philadelphia, 1830, p. 405 to 425.

if Dr. Cox had lived to have continued his experiments long enough in growing Jalap in Pennsylvania, instead of getting a purgative he would have got a sweet potato—the sweet potato being the Convolvulus Jalapi changed by soil, climate and locality from a medicinal into an esculent root. The same plant, which in the high table lands of Xalapa furnishes the purgative, was found by Thiery de Menonville growing in the vicinity of Vera Cruz, whose inhabitants, he says, were ignorant of its medicinal virtues. He might have added, for the plain reason, *it had none;* the root, in the low lands of Vera Cruz, as well as in Florida and Georgia, loses its purgative properties, and in common language, takes the name of wild yam—a variety of the sweet potato. But the Jalap is not the only instance of plants being endowed with different properties or with greater or less energies, according to the different localities in which they grow. It is well known, that many vegetables of the same kind, growing on mountains and on the plains, are found, when gathered, to have such different properties, that they would not, if judged by their properties, be supposed to belong to the same family or genus of plants, yet even in species they are identical in their botanical characters. Thus, the Heracleum Sphondylium or Cow Parsnip, in high, dry localities, affords a good pasturage for cattle; but the same plant, when growing in certain low, humid places, is poisonous to them.

The luscious melon and the bitter drastic Colocynth agree in their botanical characteristics so nearly, as to lead to the supposition that the former originated from the latter. We know that the garden celery is nothing but the acrid apium graveolens, of the salt marshes, changed in its properties by change of locality and by cultivation.

It is not always in their native locality that plants come to their greatest perfection. They are not all, like prophets, more honored abroad than in their own country, but some of them are. The Irish potato is an instance. It has found in the Emerald Isle, a locality or empire more congenial to it than its native Peru, where it was first found growing at the side of the tree which furnishes the Peruvian Bark. Since its distribution from its native equatorial region of the Andes to the rest of the world, it seems to have chosen Ireland as its empire proper—as it is there where it comes to its greatest perfection, and has developed properties unknown in its native Andes of Peru and Chili. Its subterranean crops are not only more abundant in that country, but are sweeter and richer in odour and flavor than in any other land. It is more nutritious, as its fecula is united with a quantity of resin, and a peculiar substance like animal matter. The sugar cane

and the cotton plants are natives of the tropical regions of Asia, Africa and America. At least, they are found growing in those regions in a wild state. Yet, each of those plants have found, in the Southern States of this Union, a climate and soil where they come to greater perfection than in their native tropical climate, or in any other locality on the globe. This is so much the case with the cotton plant, that it has almost ceased to be cultivated in tropical America. In Asia, its culture is continued in consequence of the cheapness of labor. The sugar cane will no doubt follow the cotton plant, and leave the torrid zone for the temperate, as a little cold weather is as essential to the full development of the properties of cane juice as it is acknowledged to be to the full development and perfection of the cotton staple. The proper locality of the cotton and cane plants does not cover the whole region where those plants grow luxuriantly, and are well developed. Neither does the proper locality or empire of the vine cover the whole region where vines grow in the greatest profusion and luxuriancy. Nor does the proper climate for Indian Corn. It grows as luxuriantly in Italy as in Illinois. But the *gran Turco*, as the Italians call it, does not come to the same perfection. Its fruit or grain is shrivelled, soft and insipid, and is as inferior to the grain of the Illinois maize as West India is inferior to Louisiana Sugar. So inferior is the grain of Indian Corn grown in Italy, that little or no use seems to be made of the grain itself, by the half-starved population of that country, who cultivate the plant more for the stalk and shuck than for the corn itself. In proof of its luxuriant growth, travelers often see houses for silk worms and hovels for the peasantry, as well as fences for gardens, made of corn stalks ; while ropes and mattresses are made of the shucks, which seem to mature better on the banks of the Tiber and the Po than on the Wabash and the Mississippi. Indian Corn is a native of tropical Asia, and is supposed to be also of tropical America. Yet, like cotton, it has established its empire far beyond the tropics. It has leaped over the cotton region proper into that of hemp and tobacco—while the empire of the sugar cane will no doubt be found, not in the tropics, but in the United States, on the Southern border of the cotton region. Each different variety of tobacco has its peculiar localities, where certain properties it possesses reach their greatest perfection. Although it grows well over a very extensive region, both in the Western and Eastern hemisphere in the torrid and temperate zones, its proper empire, where its peculiar properties are fully developed, covers a very small portion of the earth's surface. The proper empire of that variety of tobacco, used in the manufacture of snuff, is found in Mary-

land; the chewing tobacco, in Virginia, on James River; and Cuba for that variety which is the best for smoking. What is of the highest importance in giving tobacco its desirable properties for snuffing, chewing or smoking is a peculiar or specific aroma, fragrance or flavor. In all countries and places where it grows it has more or less of this aroma. But, only in a few very limited localities it attains that delicacy and subtleness to charm the world. It is the delicacy and perfection of the aroma, or peculiar flavor, which gives the value to the different wines, made from the grapes of certain well known localities. It is the same thing which gives to Louisiana sugar its peculiar excellence as an aliment, making it more wholesome and nutritious than any other kind, and arming it with an antidote against the injurious effects of tea and coffee upon the nervous system. Even under the old process of making sugar, the first quality brown, of the Louisiana plantations, preserves so much of that delicate aroma peculiar to the cane juice, as to be superior for table use to the double-refined loaf or the clayed sugars of the West Indies, if the preservation of health be considered of more importance than a beautiful white color, to please the eye. The learned Avequin, long ago, detected by chemical analysis, in fresh cane juice, an unique substance, which he called cerosie, resembling natural alcohol and found in nothing else. Dumas, Liebeg and Gerhardt, the most celebrated chemists of Europe, have, in their voluminous chemical works, given the great chemist of New Orleans full credit for his important discovery of the natural alcohol or unique substance, which he demonstrated, to the entire satisfaction of the scientific world, to exist in fresh cane juice. He also found by repeated experiments, that pure lime water, while it completely defecated the juice by coagulating the albumen and gluten, and by precipitating the phosphates it holds in solution, has no action whatever on corosie or the spirit of sugar.

The preservation of the natural alcohol or spirit of cane sugar, driven off by the usual process of refining, so far from being an indication of impurity, is the highest evidence of the superiority of that sugar which retains it. The new process of making white sugar directly from the cane juice, using Avequin's pure lime water as the sole defecating agent, has accomplished the great desideratum of obtaining a perfectly pure sugar, with the aroma preserved. I have examined two different specimens of white sugar, made at the Paragon Plantation, St. James, of this State, directly from the cane juice, where nothing but pure lime was used as the defecating agent, and the sugar made, drained, dried and put up ready for market in three days after the canes were cut in

the field, and found both specimens to be perfectly free from all impurities whatever. Each specimen, when tried by Soliel's saccharimeter, was found to rotate the plain of polarization of polarized light 100 degrees to the right—thus coming up to the standard of a perfectly pure *dextrogyrate* sugar. Whereas, the best double refined loaf never reaches that standard of perfect purity, as it always contains a small per cent. of the unwholesome substance, technically called *lævogyrate*, from its rotating polarized light to the left. The new method of using nothing but pure lime as a defecating agent, and steam to reduce the juice to a chrystalizable state, does for sugar what age does for wine—frees it of impurities, and at the same time preserves the aroma on which most of its virtues depend. In the infancy of chemical knowledge, the radical error, now so prevalent in regard to the juice of the cane, attached to that of the grape. It was supposed, that the purification of the latter, by distillation, would give a product possessing all the properties of vinous beverages, in a purer and more concentrated form. Experience, however, soon proved, that the spirit of wine (as alcohol was formerly called,) is a different thing, producing different effects upon the system from the wine itself. Thus, by distillation Claret was found to lose its diuretic and aperient properties; Port was deprived of its astringent and tonic virtues; Madeira no longer sustained the sinking energies of old age, and the fragrant Sherry ceased to be the mirth giving Sack of Shakespear. It was at length discovered that barley, wheat, rye, maize, apples and potatoes yielded, on distillation, just as good spirit of wine as the famous grapes of Xeres. All these and a number of other substances being found by distillation to yield a substance identical in its properties, the name was changed from spirit of wine to that of alcohol. But, as yet, there is no name to distinguish pure cane sugar, with the aroma preserved, from that large class of inferior sugars, which though hard, white and beautiful to the eye, can be made, and are made, from a variety of substances besides fresh cane juice, as from molasses, impure brown sugars, the black East India *goor* and *jaggery*, the sap of the maple and the palm trees, pumpkins, cornstalks and beet roots. Like alcohol, they have but one property possessed by all and each alike, the peculiar essence or aroma of the plant, from which they are extracted, being driven off by the process of refining. It is well for all of them, except that made from cane, that it is so; but for cane sugar the usual refining process is an evil, as it expels a virtue which no other sugar possesses. When the aroma is preserved, there is found to be the same variety of fragrance and peculiar properties in the sugars made from the cane of different

sorts, soils, climates and localities, as in wines and tobaccos, and the same range of choice obtains. The important fact is beginning to be every day better known in the West, that Louisiana plantation sugar of the first quality, is far better than any other kind whatever—being harder grained, better flavored, and having a stronger saccharine body than the imported sugars.

The consumers in our Eastern States, and in Europe, are behind our Western people in the knowledge of this important fact, not having had as good an opportunity of learning it from experience. From Avequin's experiments it plainly appears that the ribbon and violet cane, the kinds cultivated in Louisiana, contain much more of the unique substance or natural alcohol, he discovered in cane juice than any other; hence the superior flavor of Louisiana sugar. But the locality is a better one for the maturity of the juice than the West Indies or any tropical region, as a little cold weather is as essential to the perfection of the cane, as it is acknowledged to be to the cotton plant. The sugar in the cane is found, by recent scientific researches, to be a vital and not a chemical product. Like fibrin or muscle, it is subject to the laws of vital organized matter. When the canes are cut in hot climates, as the West Indies, for instance, chemical changes occur so rapidly, that decomposition begins in less than twenty minutes after the juice is expressed. It is not putrefaction but fermentation. The product of the fermentation is lœvogyrate or unwholesome sugar—breeding worms, causing scurvy and injuring the teeth. It is this substance which makes the West India molasses unfit for table use, and injures the quality of the sugar, of that and all other tropical regions. Whereas, in Louisiana, owing to the cold, dry, frosty weather, when the canes are cut, the formation of that unwholesome product of fermentation is, in a great measure, prevented; entirely prevented as analysis proves, if the juice be properly defecated, and immediately converted into chrystalized sugar, as is beginning to be practised on many of the Louisiana plantations, the Paragon and fifty others; where the vacuum principle variously modified is in use; making the past season upwards of thirty thousand hogsheads; approaching, perhaps, in some instances equalling that sugar in purity. Our townsmsn, Champomier, whose eyes are open to everything connected with the sugar trade, begins to see in advance of the commercial world, that the clarified and white, made by "*first process*," directly from the cane juice, "*ought to be considered as a distinct article*;" and so expresses himself in the last edition of his "*Statement of the Sugar Crop.*" (No. 10 Customhouse street, price $5.) Truly it ought, as this kind of Louisiana sugar possesses peculiar

and superior properties to any other in the world, and is the very thing which the celebrated Dr. Dutrone spent the greater part of a long and useful life, in trying to manufacture, viz: a pure sugar from the cane, with the aroma preserved. He did not succeed in freeing it entirely of the above-mentioned unhealthy product, known by its rotating to the left when tested by the polariscope, and found in the best double-refined Loaf of eommerce, yet he succeeded so far in doing so and in preserving the aroma, as to induce him to regard the article made by his process, "as the invigorator of infancy, the improver of the complexion, the restorer of health, and the renovator of old age."

The law or secret principle of the vegetable kingdom, giving to some plants, when grown in certain localities, peculiar or superior properties, not possessed by the same plants elsewhere, is the very law from which the sugar grown in Louisiana derives its superiority as an aliment and hygienic agent. The same law gives length, strength, fineness and durability to the staple of the cotton grown in that belt of country embracing South Carolina, Northern and Middle Georgia, Alabama, Mississippi, Southern Tennessee and Arkansas, Northern Louisiana and a portion of Texas; or in other words, the empire proper of the cotton plant. As yet, that law in regard to sugar remains a dead letter, as it did for a long time in regard to cotton. Lately, however, certain manufacturers, of coarse cotton fabrics, have turned it to practical account, and have made it realize for them the fable of Aladdin's lamp. When they first began to fill their warehouses with a flimsy-looking, slack-twisted, coarse cotton cloth, made by the neighboring farmers', mechanics' and shopkeepers' daughters, who could play upon the piano, but had little or no experience with the spinning-jenny, they were laughed at by the old manufacturers of coarse cotton goods, and it was confidently predicted, that the article would be unsaleable and worthless. But to the astonishment of all Europe and America, the Lowell cloth, as it is called, soon drove everything in the shape of coarse cotton goods out of the domestic market, and built a city, as if by magic, around the mills that made it. Scarcely had this wonder of the world become generally known, and its location marked on the map, when the people of the several States of this Union, then in an angry dispute on the tariff question, were hushed into stillness and peace by the astounding news, that the product of the factories of the new city had boldly rode the mountain waves around Cape Horn and the Cape of Good Hope, to the far-off markets of Asia and South America, and was actually putting the most distant nations of the earth under tribute to the lords of the Lowell looms. Even now, the product of the same

looms is daily going forth, by every conveyance, on land and water, to dispute the field, at home and abroad, with the fabrics of flax, hemp and wool. For a long time the cause, giving to Lowell cloth its buckskin strength and durability, was so little known, that here in New Orleans, a factory established, some years ago, for the express purpose of making a similar species of coarse cotton goods, so much in demand for negro clothing, failed from not knowing it—a judgment for its neglect of such practical scientific men as Avequin, who will be better known to generations yet unborn than to that which he has so faithfully served.

The factory, however, which has been lately established in Woodville, Mississippi, has got the secret, and is now making a coarse cotton cloth, equal if not superior to the best Lowell. The clue to it will be found in that unalterable law of the vegetable kingdom we have been considering. It simply consists in operating upon the strong staple raw material, grown in the cotton region proper, or in that locality where the cotton plant attains its greatest perfection, in the manufacture of coarse cotton goods, instead of, as heretofore, making those goods out of the raw material, damaged by long exposure to the weather before it was gathered, or from the rotten staple cotton of climates too hot or too cold for the plant to attain its full vigor and maturity. Within its proper empire or locality, above designated, it yields or rather secretes a fibre exceeding that of wool or hemp for strength and durability. Hence, the goods made from the strong staple cotton of the cotton region proper were found by experience to wear like leather, instead of tearing to pieces, and being of little or no value for clothing, as those made from East India and other inferior cotton. It is the strength of the fibre of American cotton, derived from the law we have been considering, and not from any particular skill in manufacturing it, which has enabled the American manufacturer of coarse cotton cloth, not only to drive out the foreign article from the home market, but to enter on a successful competition with all foreign manufacturers in the markets abroad.* The same law, when traced to its source, will demonstrate the superiority of Louisiana sugar, which like the cotton fibre is a *sui generis* vital secretion, and not a chemical product. The important fact, that Louisiana sugar, when properly separated from the cane juice, is the best in the world, will require, perhaps, much time before it is generally known and admitted. The same truth in regard

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* In the year ending June 30, 1851, $7,241,205 worth of cotton manufactures were exported from the United States, chiefly to China, the British Colonies and South America; nearly six millions being Lowell. See Report of the Secretary of the Treasury.

to the superiority of cotton, from that belt of country a little north of the Louisiana sugar region, and south of that of hemp and tobacco, required many years before it wrung from the manufacturers and consumers of cotton goods a reluctant acknowledgment. But that ackowledgment is another proof of the existence of the secret law of the vegetable kingdom, giving to some plants peculiar and superior properties when grown in certain localities, which the same plants do not possess elsewhere. According to Mr. Bazley, the President of the Manchester Chamber of Commerce, the supply of American cotton, in the last eleven years, has increased 77 per cent., or nearly doubled; while the supply from the British possessions in Asia, has diminished 16 per cent. But he does not attribute this difference to its true cause, the law of the vegetable kingdom above referred to. He erroneously supposes, that it is in consequence of American railroads, and by making the same improvements in India, he thinks that country will be able to compete with the Southern States of this Union in the production of cotton. He does not seem to be aware of that inscrutable law of the vegetable kingdom, under which the cotton plant has chosen its empire in a particular locality in the United States, from which no power, short of Omnipotence, can dislodge it. In vain may the hundred and fifty millions of the British empire in India, although encouraged by every inducement, which the wealth and power of Great Britain can give them, and the assistance of several hundred thousand Chinese, who have been imported to aid them, endeavor to turn nature from her course; in vain may India be checkered with railroads, still the staple of the cotton grown there, will be no stronger than it now is.

The manufacturers of it, then as now, will gradually lose their customers, owing to the worthlessness of the goods made from it, and fail in their business, while those, who operate on the strong staple cotton of America will gain, instead of lose customers, and continue to grow rich, as they are now doing. Science abhors exaggeration, yet it would scarcely be any exaggeration to say, that half Virginia, for half a century, have been looking for another James River, but have not found it. The empire of chewing tobacco is not more firmly fixed on James River in the Old Deminion, than the empire of strong, long and fine staple cotton in South Carolina and the region lying due west of it. In proof of which, the politicians, merchants and manufacturers of Great Britain have, for many years, been ransacking the whole earth for some locality as favorable for cotton as our Southern States, but have not yet found it. The experiment, now in progress, on the gold coast of Africa, at Monte Video, and in New South Wales,

promises no better success than those already made in India and elsewhere. When the truth becomes more generally known, as known it must, in time, that neither cotton nor sugar comes to full perfection any where else than in the Southern States of this confederacy, the effect will be to enrich them. The price of the article which furnishes the world the material for the cheapest, best and most durable clothing, has not yet reached its true value in the market, as it is still sold at only a very small advance above the weak staple cotton of other countries; while the other article, which affords the best, most wholesome and nutritious aliment the earth produces, is positively underrated in the commercial world, in consequence of its superior properties not being appreciated, from not being known. If these two great staples of Southern agriculture, cotton and sugar, were to command in the market of the world their full value, no one South would trouble himself in raising horses, mules, beef, pork and such things, as he would spend his time and labor to double and treble the profit in growing cotton and sugar. Railroads and other communications would, as a necessary consequence, be constructed, linking the Great West to its best market, the rich South. Trade must and will tend towards the best market, wherever it be; and the best market for the Great West, even as far west as the Pacific, being the rich South and its emporium, New Orleans, thitherward would the railroads of the Western World be pointed. But the law of the vegetable kingdom giving to the cotton and sugar of our Southern States their superior properties, if better known, would bind the two greatest nations of the earth, Great Britain and the United States, in the bonds of perpetual peace. It would also serve as a guarantee against foreign interference with the institutions of the South. For more than a quarter of a century, the fixed and settled policy of the British Government and influential classes in that kingdom, has been to sow the seeds of dissention between the different sections of our republic on the subject of negro slavery, and to do every thing in their power to inoculate our Northern and Western people with a species of rage or wild fanaticism, known as the negro mania. The object of that policy has become too transparent to be any longer concealed under the mantle of philanthropy and liberty, which has been thrown around it. It is not the sufferings, real or imaginary, of our two millions of happy, contented, sleek and well-fed negro slaves, which have instigated that policy, or it would have been directed to the relief of the suffering Irish and to the millions of the starving poor of Europe and Asia. The wise men of Great Britain, having tried the experiment, know, as well as we do, that giving negroes liberty to do as they

please, is the same thing as tying their hands and sending them back to want, wretchedness and barbarism—the same barbarism, irreligion and wretchedness which have overshadowed Africa and Hayti, and to which Jamaica and the other West India Islands, where they have been emancipated, are rapidly tending. The giving of our American negroes liberty is the same thing in the eyes of the British statesmen, looking at Jamaica, as a total abandonment of the cotton and sugar culture of the United States, and the handing over of those rich products of our agriculture to the conquerors of India and the exterminators of the colored race in New Holland; but with the law of the vegetable kingdom staring them in the face asserting the decree of nature, that neither good cotton nor good sugar can be grown in the Eastern hemisphere, or anywhere else than in that region of country within the limits of the American republic, they will surely relax in urging a policy which, if carried out, would not only bring ruin upon them and us, but on the world at large.

The law, giving to some plants peculiar or superior properties in certain localities, has a bearing on many subjects, not strictly within the province of the physician, which it was necessary, however, to glance at, in order to see the full extent of its influence on those which are strictly medical.

II.—IMPERFORATE VAGINA AND DIFFICULT LABOR—WITH SOME REMARKS ON THE USE OF CHLOROFORM.

BY SAMUEL B. MALONE, M. D., OF COLUMBUS, MISS.

Mrs. N., aged twenty-one years, medium size, dark hair and eyes, sanguine temperament, married seven years, first pregnancy; was married when thirteen years and nine months old, at which time she weighed just seventy-five pounds, (75 lbs.)

Sometime in October, 1852, I was notified that my professional services would be expected, and that great difficulty was anticipated "*as the parts were very small.*" November 26th, was called up at two o'clock in the morning, to visit her, nine miles off. Arrived at half-past three. Was informed that for several nights she had been suffering all night. Considered *her time* as past, by near a month. But up to this time there had been no regular labor pains. An *examination* was at first strongly objected to. But on proper representations she

submitted. Found an opening just under the pubic arch, scarcely, if at all larger than the natural opening to the bladder; rendering it impossible, after all the efforts the patient would allow me to make, to introduce the finger to the first joint. The husband, a very intelligent and well-informed gentleman, then assured me most positively, that notwithstanding their seven years marriage, and *her present situation*, he had never *cohabited* with her in the ordinary way, "rem in re."

Being much excited by the pain resulting from the attempt at examination, as well as from great natural timidity, and there being no decided labor pains, I gave her an anodyne, and lay down for the remainder of the night. In the morning, finding her more composed, I insisted on a more satisfactory examination. I found that by perseverance in the use of some force, the finger could be passed beyond the obstruction, which was from an inch to an inch and a half in extent, and that beyond that point, there was sufficient room in the vagina. She was now free from anything like labor pains. Her face was much flushed and swollen. Opened a vein in the arm, and took some twenty or twenty-five ounces of blood; gave another anodyne, and set out for home, with a promise to return by night.

On returning at 5 o'clock, P. M., she told me she was quite well; and for a time I thought of going home again; but as night approached, her uneasiness increased, and I remained.

About 9 o'clock, the pains increasing, I made another examination; and although I could now introduce the finger, I found it impossible to reach the os uteri. With some effort I now introduced the second finger; but so firmly were they wedged under the arch of the pubis, and so short a time did she allow me to continue the examination, (as it gave her intense suffering,) that I know began to entertain fears of a want of capacity in the bony pelvis. I now felt that I might need assistance, and Dr. A. N. Jones was sent for.

From this time, regular and strong labor pains set in, and continued until the Doctor's arrival, at half-past three o'clock, on the morning of the 27th; during which time I had made other efforts to reach the os uteri, but with like want of success. Though able to ascertain clearly its position, as the connexion between the womb and vagina could be distinctly traced all around, except along the sacrum, I now stated the condition of matters to Dr. Jones, and he verifying them by examination, suggested, that we administer chloroform, and dilate the vagina fully, by introducing the hand. Which suggestion was adopted, and as the system had been well relaxed by a second free bleeding, the Doctor administered the anæsthetic, the patient passed beautifully

under its influence, in four minutes. I now gently, but firmly, pressed in my fingers, arranged conically, through the stricture, until the whole hand was introduced into the vagina, the os uteri was found at the promonitory of the sacrum *very* dilatable; two fingers were introduced, and it was easily brought in the proper axis of the pelvis, and there retained until the next pain came on; during which she roused up, totally unconscious of what had passed, but complaining of considerable "smarting."

By this manœuvre, three important points had been gained: first, I was satisfied there was sufficient capacity in the bony pelvis for the delivery of an ordinary infant; second, the os uteri had been brought in the proper direction; third, a very firm and unyielding structure had been overcome, which might have been found very troublesome at a subsequent period of the labor. From this time pains continued regular, and apparently efficient, the vertex advancing slowly, until morning, when Dr. J. left. Matters now came to a stand, although the pains continued with great force. At 11 o'clock, finding the strength of my patient failing, her fortitude fast giving way, her hopes and confidence lessening; and, finally, her pains becoming weaker, I hinted at the use of the forceps: their action was explained, accompanied with the assurance, that it would compromise neither the safety of the mother nor the child, as a matter of necessity. The idea was eagerly caught at by the patient, who almost frantically implored me to save her from further suffering, at whatever risk.

The forceps were introduced, and the head disengaged from its impacted position in the bony pelvis, and aided by strong uterine contractions, the head was delivered. Remaining in this position some time, I passed my forefinger into the axilla, and with the assistance of another pain the delivery was completed. And I had the high gratification of presenting the delighted mother and father with a fine male child, of full ten pounds weight. It may readily be supposed, that it was a matter of great rejoicing, as all hopes of an heir had long since been abandoned. And it was the first grandchild of the aged parents of the mother, who were both present.

What light this case may throw upon the various theories of the act of conception, I here leave for the speculations of others, while I confidently vouch for the accuracy and truth of the facts as detailed. It is impossible for me to say what influence the very early marriage, and very small size, and probable imperfect development, may have had in producing the state of things found to exist.

I had intended, in conclusion, to append some remarks on the use of

Chloroform, but as my notes have already extended beyond the limits for the report of ordinary cases, I will only say in short, that I have been for several years in the constant habit of resorting to it in all, or nearly all, of my surgical operations, and in many obstetrical cases, particularly where any serious interference is demanded; and in no instance have I met with any alarming or continued unpleasant effects from its use. In one case where it was necessary to remove a large diseased growth from the lower jaw bone, and where the entire cheek was laid off to get at the diseased structure, and where the operation was continued for three-quarters of an hour, resorting successfully to the use of the saw, chissel and mallet, and cutting forceps, the patient was kept under the anæsthetic influence during the whole time, and yet the external incisions all healed by the first intention, and the patient resumed his field labors in about three weeks.

In several obstetrical cases I have kept the patient under the partial and entire influence of chloroform, for the space of two hours, or more; during which time I have used as much as two ounces, and no effects have followed, more than to give the patient some hours of quiet sleep after all was over. My observation teaches me never to give chloroform, when it can be avoided, except on an empty stomach. I always, when practicable, have a reliable physician to administer it for me. In some cases, when the pulse has become too weak, and the breathing difficult, I have resorted to brandy, which has always acted like a charm in relieving all unpleasant symptoms.

III.—OIL OF TURPENTINE AS A DRESSING FOR THE UMBILICAL CORD—ITS PROPHYLACTIC VIRTUES AGAINST TRISMUS NASCENTIUM.

BY T. J. GRAFTON, M. D., OF MISS.

Dr. Hester:

I beg leave through your journal to call attention to the use of the Oil of Turpentine as a dressing to the umbilicus of new born children, as a prophylactic against Trismus Nascentium, a disease so universally fatal when once developed, as almost to preclude a hope of cure from any treatment.

So far as my experience enables me to speak, I have never known the disease to occur, in any child that had been subjected to the turpentine

dressing. With whom the practice originated I have not been able to learn; my attention was directed to it several years ago by a very intelligent lady, who had used it upon her negroes, and at whose suggestion it has been adopted by others in the neighborhood, with the most flattering results.

This should not surprise us, as turpentine has been long considered by some as a prophylactic in Traumatic Tetanus. Dr. Armstrong, in his Lectures upon the Practice of Medicine says: "Mr. Stewart, who has seen a great many cases of Tetanus, never knew a case arise where turpentine had been applied to the local injury;" (page 337, 1st American edition, 2d vol.) My own experience so far is corroborative of the above extract.

I am led to make this communication with a view of multiplying the number of observers, some of whom will doubtless make known to the profession the results, as the opportunities of no single individual would be sufficient to establish it as a fact.

I am well aware, that turpentine has been used as a stimulating application to the umbilicus, as a means of cure after the disease was developed, yet I do not remember to have seen it mentioned by any writer as a prophylactic.

In no case of the disease have I been able to detect the displacement of the cranial bones, as suggested by Dr. Sims, my own belief is that the umbilicus is the source.

The mode of using the turpentine is as follows: at the first dressing a few drops of the undiluted turpentine is applied immediately to the umbilicus, around the cord, and it is annointed at every succeeding dressing, the turpentine being diluted one-half, or two-thirds with olive oil, lard or fresh butter, which dressing is continued more or less diluted, according to circumstances, (or rather according to the irritation produced) until healed; though I usually make use of no precaution to prevent contact with the skin of the abdomen, yet I have never known distress follow its use; it would be well enough to prevent too extensive contact.

IV.—REMARKS ON RACHITIS—WITH A CASE.

BY JOSEPH R. SMITH, M. D., OF ALA.

As the disease which heads this article is comparatively a rare one, and is but seldom treated of in the different Medical Journals of

the day, I have thought proper to make it the subject of a few remarks; not that I have anything new or novel in its character to advance in relation to its pathology, but that I may add a few more facts to those heretofore put forth, touching what I conceive to be its true pathology.

It is not necessary that I should go into a lengthy detail of facts, to prove that Rachitis is dependent on, or rather the result of, a scrofulous diathesis; as this is pretty generally admitted by the profession. Neither is it necessary that I should detail all the symptoms of Rachitis, for the purpose of illustrating the one fact put forth by Dr. Stokes, of Dublin, and so ably taught for the last ten years, by the able and distinguished Professor Cross, late of Memphis, Tennessee, viz: that the scrofulous diathesis is an imperfect condition of the human body, and is to be attributed to arrest of development.

To prove the validity of the position last taken, it only becomes necessary to name some few of the most prominent symptoms of a Rickety patient. We seldom or never see Rachitis appear in children before the first or second year after birth; and then, the disease is so insidious in its character, that it is almost completely developed before the parents are aware, that their offspring is of the scrofulous diathesis. Among the first and most obvious symptoms may be reckoned, a soft and flabby condition of the muscular system; flesh hangs loose on the bones; abdomen protuberant; complexion pale and sallow, and the blue veins may be traced under a transparent skin; fontanelles open, in consequence of imperfect ossification; joints appear enlarged, caused by the spongy nature of the epiphyses of the long bones; sternum projects forward in a ridge, in consequence of the ribs becoming flattened on their sides; distortion of the spine, causing the gait to be reeling and unsteady; and probably the little patient may be unable to walk at all, notwithstanding it once had the use of its limbs.

Where is the well-read physician, who can fail to detect an arrest of development in the above named symptoms! Can anything more plainly show it than the sallow and marble appearances of the skin, or the spongy nature of the epiphyses of the long bones; all plainly indicating a deficiency in the supply of the proper nutritious properties of the fluids. The great size of the head, liver and abdomen, all plainly show arrest of development; the head, because we know that children of the scrofulous diathesis are particularly predisposed to hydrocephalus.

The enlarged condition of the liver, says Dr. Stokes, is not the result of disease, but a fœtal condition, that obtains "because an equal and

proportionate increase of other parts have not gone on." The same may likewise be said of the abdomen. Monstrosities may also be referred to arrest of development.

In the scrofulous diathesis, it is a well known fact, that the blood is deficient in fibrin, while in the same disease it contains an excess of albumen, a white fluid. We are informed by Professor Cross, whom we consider the best authority on this subject, that in persons of the scrofulous diathesis, the white fluids do not only superabound, but that they, the fluids, have become altered in their composition.

Professor Cross again says, in the first place it should be stated, that the saline principles which predominate in the white fluids, are the phosphate and carbonate of lime and the chloride of soda, and that it is chiefly of these principles that scrofulous tubercles are composed. We are told by M. Baudeloque, that Labillardiere has proved there is seven times as much phosphate of lime in the milk of a cow laboring under *pommeliere* (a tuburculous affection) as in that of a healthy one. This is certainly an exceedingly important fact, and it will doubtless throw much light upon the essential nature of scrofula. Taken in connection with those causes that are considered the most productive of this disease, it will aid much in explaining their mode of action, and may furnish a useful hint in its treatment. In confirmation of the altered state of the fluids, it should be stated that the greater part of the secretions in the scrofulous, are more acid than the same secretions in those that are healthy. Now, that, this superabundance of acid in the fluids of a person who is the subject of the scrofulous diathesis, has been observed by every physician at all conversant with scrofula or rickets. I have not a doubt, it may not only be detected in the fæces, urine, and fluids of the stomach, but also in the perspiration and saliva. When received in this light, (and this we conceive to be the true one,) how can we expect healthy solids from such materials. Here nature withholds, or rather fails to furnish the necessary healthy fluids for the proper nutrition of a perfect and sound organization. Consequently, through the medium of the fluids the solids become secondarily, or we might say almost simultaneously affected; thereby giving us an imperfect organization; which is the same thing as arresting nature in the healthy performance of her duty. There is another important fact, when taken in connection with this subject, which will go very far to prove the position above, viz: that in the strumous habit, so soon as we restore the chylopoietic viscera to an healthy action, thereby increasing the fibrin of the blood, and lessening the tendency to the formation of

the white tissues, our patient immediately begins to improve; and here, at this stage of the case, by rejecting the *diet obsolue* of the French, and feeding our patient with a liberal hand, and that too, of rich nutritious viands, with proper exercise, we will be able to cure nine cases out of ten. Scrofulous patients, as a general thing, are by most of the physicians, too much restricted in their diet and exercise; and this mainly occurs from a mistaken notion of its pathology. I am perfectly convinced (and this conviction arises from experience in the treatment of the disease) that were rickety and strumous children fed with a more liberal hand, after first restoring to a healthy action the chylopoietic viscera, that nine out of ten might be saved; and that, too, with a strong and healthy constitution, as a living monument to a true and rational pathology, and a comfort and solace to fond parents in their declining years.

Out of a number, I selected the following case to illustrate and prove the above positions:

On the 1st of March, 1842, I was requested to see the daughter of a blacksmith, in the neighboring village, aged about three years, who, I was informed by the parents, had not walked a step for eighteen months; notwithstanding, at fifteen months, she was apparently as hearty and pert as any child of her age. I found on my arrival, a little pale, sallow, emaciated child, sitting in a small arm-chair; its spine curved almost, forming a semicircle; head unusually large in proportion to rest of body; face somewhat smaller, presenting a kind of leucophlegmatic transparency; whole muscular system soft and flaccid; the wrist and ancle joints greatly enlarged, though there was no distortion of the long bone, which is by no means an unusual sequence in protracted cases; the ribs were flattened, causing the sternum to project forward, in a sharp ridge. The fontanelles appeared to be perfectly closed, and the blue veins might be traced under the wan and sallow skin of a very prominent forehead. There was general emaciation, and that too was connected with a voracious appetite. This I took to be morbid, for its parents told me its stools were almost entirely of a white mucous nature, and so frequent as to simulate chronic diarrhœa; its food passed off in a crude and undigested state; slight fevers at night.

My little patient was, at this time, by the advice of its physician, living entirely on a vegetable diet, except a little milk. Not being able to see this patient more than once a week, and believing my treatment would not meet with the approbation of the parents, I was unwilling to undertake the case; but the parents assured me they would follow

any directions I would give, for they had now almost despaired of their child's recovery.

I caused the child to be placed on a hard mattress, and directed that it should on no occasion be permitted to sit erect until further orders, to be hauled on a little wagon, once a day, in the open air; but always in the horizontal posture. Prescribed an emetic of Vin. Antim. once a week, for four weeks, to be followed at night by a dose of Calomel and Rhei; the emetic to be taken in the morning, on an empty stomach. After the operation of the emetics and cathartics, to take on the intermediate days the following: ℞. Iodine grs. iv., Hyd. Potass grs. 30, Aqua font. ℥ fiii, mix; dose ten drops three times a day, to be increased every week five drops, until the dose reached a drachm. The vegetable diet was discontinued, and a liberal use of animal food substituted in its place, to be given at regular intervals. I was very particular to try to impress its parents with the necessity of a free and generous diet, having in view the grand desideratum, the supplanting of the *white* by the *red* tissues, which I have always found difficult to accomplish but by a generous diet and properly regulated exercise. During the first week there was no perceptible change; but during the second and third week it began to be more lively, and would play with the other children, which it had not done before for six months. At the end of six weeks my little patient was much pleased to find it could straighten its back; at this time more regular in its bowels; food digests well. At the end of two months much improved in color, quite lively and playful, stools natural in color and consistent, bowels entirely regular, without the use of purgatives. Here I suspended the medical treatment for four weeks, but still enforced my rules in relation to position, diet and exercise. At the end of the third month from the beginning of the treatment, the same medicinal agents were resumed, and continued as at first, for one month, at the end of which time she could sit erect, in a chair, without any support at her back; joints greatly reduced in size, strength and color greatly improved. About this time I noticed the miliary eruption on the surface, which Iodine has a tendency to produce, indicating the acme to which this drug may be carried.

This patient steadily improved, without any untoward symptom, except about the fifth month of the treatment, it had one or two convulsions, evidently the result of irritation in the alimentary canal, caused by lumbricoides; for she discharged quite a number from the use of Spigel. Marilandica and Calomel. The Iodine mixture was continued, with an occasional dose of Calomel, Rhei, for eight months

from the time it was first used, at which time it began to walk as well as it ever did; color good, appetite also, digestion perfect; in fact, all the secretions and excretions were performed so regular that my patient was dismissed, and has not for the space of ten years been under treatment for any of her old symptoms. I have seen her during the present year, (1853,) and she looks to be of as healthy and vigorous constitution as any lady of my acquaintance.

V.—A REPORT OF TWELVE CASES OF TYPHOID FEVER, TREATED ON THE ABORTIVE PRINCIPLE, IN THE CHARITY HOSPITAL, NEW ORLEANS.

BY D. MACGIBBON, M. D., VISITING PHYSICIAN TO THAT INSTITUTION.

The subject of the treatment of Typhoid Fever has of late been attracting some attention among the profession here and elsewhere; chiefly in consequence of the several publications which have appeared in the English journals, and republished in this country, advocating the free use of Sulphate of Quinine, which we are assured cuts the progress of that disease short; and, also, in no slight degree, owing to communications which have emenated from parties nearer home, advocating the same "abortive" treatment. Some of which have appeared in this journal.

Educated in one of the best nurseries of Continued Fever in the Old World; having caught too the Infectious Typhus, in my more juvenile days, and experienced during five weeks, in which I was bedridden, something of what it is—and more than I wish any other mortal to know through like means; and, also, at a later period having been laid up the ordinary time with an attack of Typhoid Fever, I should know something of it likewise. If I have yet a vivid recollection of some of the horrible dreams in the long continued delirium of the former; so, too, have I not forgotten the scorching pain occasioned by the application of mustard cataplasms to the abdomen in the latter.

It will be seen too, that I have had an opportunity of becoming, in my professional capacity, in some degree familiar with these diseases, through my connection with the Charity Hospital of this city, where I have had charge constantly, summer and winter, since before the severe

visitation of Typhns Fever which occurred in the winter of 1847 ; and where I still am enjoying the same privileges ; for the same disease, either as Typhus or Typhoid Fever continues among us, though with gradually lessening severity, as it would seem. That I have not altogether neglected these unequalled opportunities, or failed to give trial of those remedies, which in the hands of others are asserted to cut short the particular ailments above referred to will be also seen. It is only since I came to reside in the South, that my admiration of Sulphate of Quinine, as a heaven-bestowed agent for aleviating many of the ills that humanity is heir to, when judiciously administered, has received an intelligent augmentation.

If I cannot, in the case of Typhoid Fever, from what experience I have yet had of its use, in cutting short that complaint, speak so exultingly as some others have done ; so neither can I from that speak so condemnatory as several permit themselves to do. Though, as I shall take occasion to confess, my first experiments with it had any but an agreeable result associated with them, as will be supposed when the circumstances themselves are related.

In the winter of 1849, the abortive treatment of Typhoid Fever, by means of large doses of Suphate of Quinine, was submitted first to my consideration, at least on such rational grounds as secured my attention. The first patient was one who had been sick for some days, in the Charity Hospital. He had a draught containing, I think, a Scruple of Sulphate of Quinine, with half a drachm of Tinctura Opii, administered for two days in succession, without any seeming advantage having been derived, or injury been produced thereby, when it was discontinued, and the ordinary treatment pursued. He had been too long sick before the abortive treatment was attempted was the conclusion. The next selected was not in this predicament : he had been sick two days when he entered, and was supposed to be the right kind of subject to look for a different success from. There was as yet no high febrile action present, as in the previous case; and, what I deemed of consequence, no brain symptoms had shown themselves either. These latter were what my chief fears, at this time, led me to have some trepidation about, their possibly being aggravated by this treatment it was made me hesitate a little. The above draught was, however, ordered to be given him at bed time ; and the nurse of the ward was ordered to keep an eye upon him during the remainder of the night. Shortly after receiving the draught, his headache and other pains were so much relieved, and he altogether felt so soothed by it, that he felt disposed to go to sleep, which he did, when the nurse left him, supposing all would go right for

the remainder of the night. Whether he became delirious, and if so, whether the Quinine and Opium had anything to do with having brought on this sudden change, are matters which I was then only left to guess at; but the fearful fact, that he had somehow, and unknown to those around him, got out at the window and, from a height of three stories, had fallen on the pavement of the yard beneath and was killed, was fully realized by finding the dead body there in the morning. When such accidents occur under our care, no matter though the blame should not in the most remote degree be attributed to us, we cannot but feel, for the time being, at least, shocked; and such I did when I learned the circumstances, and resolved henceforth to make no more of these abortive attempts to cut short Typhoid Fever, lest, perchance, I might contribute to cut more than the disease short; and in a way I least wished or expected.

The above resolve, like many others made under like circumstances, gradually lessened by the lapse of time, and was ultimately so far dispelled by reading the reports of those who had, on a scale which I could make no pretensions to, given the treatment a fairer trial; and, as several of these reported, with the utmost safety and advantage to their patients; so that I felt it would be something more than cowardly in me not to give mine whatever benefit in their sufferings the said "abortive treatment" could be made to afford them; with what difference of results from those which the outset afforded, will best be seen by a perusal of the cases themselves, as reported at the bedside.

Case I. Thomas McDonough, aged 28 years, an Irish laborer, and only eight days arrived in the city, from Ireland. Became sick three days before admission to the Hospital, which took place on the 29th of November, 1852. Thinks he caught a cold, felt chilly all over; then became hot, and the heat continued on him. Pulse, on entrance 132; tongue somewhat brown. Diagnosis, typhoid fever. Ordered the following draught to be taken immediately:

℞. Sulph. Quininæ, ℈j.
Tinct. Opii. ℥ ss.
Sulph. Acidi. dil q. s.
Aqua menthæ ℥ ss. M.

Lemonade for drink, and low diet.

30th. The surface hot, and capillaries slightly congested; the eyes watery; tongue as yesterday; pulse 108.

Repeat the draught: one-half morning, the other at night.

1st Dec. Feels much the same. Slumbered some yesterday and last night. Surface dry; tongue dry and brown; pulse, 100; bowels open. Changed medicine.

℞. Liq. Ammon. Acetas ℥ v. Sp. Etheris Nit. ℥ j. Syr. Simplicis ℥ ji. M. Take a tablespoonful every hour.

2d. Better; perspiring freely, tongue moist; pulse, 88. Repeat yesterday's medicine.

3d. Better. Slept well; tongue cleaner; pulse 60. To have chicken soup for dinner.

4th. Better still. Pulse 52 An ounce of Castor Oil, to open the bowels.

5th. Same as yesterday. Remains in bed, and complains of nothing but weakness. To have more nourishing diet allowed. On the 8th he was moving about, and on the 10th he was discharged, quite well. His pulse having, within the last few days, risen to 60, at which it remains.

Case II. Michael McDonough, aged 24; brother to the foregoing, and like him recently arrived from Ireland. Both were admitted at the same time. Got wet four days ago, and thinks he caught a cold. "Pains in all his bones;" and, on admission, was shivering in some degree. Tongue moist, pulse small, and not very quick. Put him on the same treatment.

30th. Better. Pulse 92; surface moist. Repeat the draught; and take one-half morning, the rest at night. To have a mustard foot-bath at night.

1st Dec. Better. Tongue slightly yellow and moist; pulse 60. Ordered him the same diaphoretic mixture as his brother.

2d. Better. Perspiring freely. Pulse 52. Repeat medicine.

3d. Much better. Pulse 52; tongue clean, bowels open, sleeps well. Dicontinued medicine; to have chicken and soup.

4th. Complains of slight cough; pulse 48.

5th. Better. Pulse 48. Complains of nothing but weakness. To have full diet.

6th. Eats heartily; pulse still slower, down to 40 beats.

On the 10th he was discharged quite well. His pulse was then 52. He returned on the 12th, with some fever on him. For this, the student who saw him prescribed a Quinine draught. The following day he was better. His tongue was coated slightly; probably from over indulgence while out. He was kept on moderate diet for a few days, when he was again discharged well.

Case III. 2d Dec., James Kinally, aged 21, three days in city from Ireland. Came in yesterday, suffering with headache, and with pains in bowels. He got a draught of Sennæ and Salts, which operated freely. This morning, tongue still foul; and some tenderness in epigastrium; pulse 112; considerable headache; face was dusky look-

ing. To be kept under the bed clothes, and to have the Quinine draught immediately.

3d. Better. No pains in head, except a little over mastoid. Bowels open; pulse 76; perspiring freely. Ordered the same draught; one-half to be taken in the morning, the other at night.

4th. Infusion of Senna with Salts, to open bowels now; and in the evening, one-half of the former draught. Complains of some pains in the back and shoulders. Pulse 68.

5th. Better. Bowels open; pulse 68. Take remainder of draught at night.

6th. Was up this morning; discontinued medicine; nourishing diet allowed.

7th. "As well as ever." Wants out. Discharged.

Case IV. James Stewart, aged 22, Scotch, and eight days off ship. Came in December 2d, at visit hour. Says he became sick the night before last, when he felt chilly all over. Since then has been hot; and he has had headache with pains and debility in lower extremities. Walked to Hospital. Pulse 116. Tongue has a yellowish fur; bowels open free enough. Put to bed, and to have the Quinine draught immediately. Lemonade and low diet.

3d. Better. Headache remained up till last night; since which he perspired a good deal, and is easier. Skin now moist; tongue still white; pulse 96. Ordered the same draught repeated as yesterday.

4th. Much worse. Has been troubled much with cough during the night. Sputa tough, and scanty; skin hot; pulse 104. Respiration 34. Over his chest, in front and rear, a number of small vesicles, each having a red base and white apex, have made their appearance; also upon sides of neck. In left chest, below the scapula, slight mucus rale is heard, and a little lower some fine crepitation is present. At the base of right lung behind, fine crepitation is also heard. Difficulty in breathing is what he mainly complains of this morning, and this is pretty urgent. Tongue foul, and bowels not opened for two days. For this he had infusion of Senna and Salts ordered, and the following pills every second hour: ℞. Mass Hydrarg. gr. xxiv.; Tart. Antimoni, gr. ii.; Pulv. Scillæ ℈j; Gummi Opii, gr. iv., M.; fiat pil. no. xii. A Cantharides Blister, 8 inches by 6, to chest behind.

When he entered, and up till last night, he had no cough nor any uneasiness in the chest, and was not auscultated. My impression is that Pneumonia has just set in, and is intercurrent; and if this be so, it would tend to show that Quinine, in the form administered at least, is no specific against inflammatory action in the tissues of the lungs;

however much it may be depended on to subdue inflammatory action in the fluids of the body, or hyperæmiæ.

5th. A little better to-day; cough is easier; skin over breast especially is still hot; pulse 110; respiration 30; no nausea; tongue foul; bowels open. Blister rose well. Repeat the pills as yesterday. Lemonade and low diet.

6th. Worse. Some blood mixed with tough sputa in his bowl; respiration much obstructed; bronchial respiration heard behind, where abnormal sounds were at the previous examination. The respiration in front is clear; pulse 112; respiration 39. Continue medicine.

7th. Better to-day. Slept a good deal last night; cough not so troublesome. Sputa slightly tinged brown. Repeat the pills.

8th. Still making better. Sputa yellow; pulse 72; respiration 28. Mucus rale heard over the hepatized portion of the lungs behind. The pills to be repeated, and taken every third hour now. Mouth not in the least affected. The vesicles still out on breast, but fainter. Is evidently improving. To have light nourishing diet.

9th. Still better. Pulse 72. Discontinue pills, and take a tablespoonful every hour, of the following mixture:

℞. Liq. Ammon. Acetas ℥ iv; Tinct. Opii Camphorata, Syrupi Tolut. *àà* ℥ ji, M.

10th. Still doing well. Sleeps well at night; pulse 64; respiration 24. Repeat the mixture.

11th. Much as yesterday. Continue the medicine.

12th. Improving rapidly: ℞. Carb. Ammoniæ ʒ iss; Tinct. Opii Camphorata ℥ ji; Syrupi Tolutaniæ; Mist. Camphoræ *àà* ℥ jii, M. Taken as before. This was continued for a few days after; and on the 17th, finding himself much improved in strength, he asked to be discharged; which was done.

Case V. Dec. 5th. Roger McDermott, aged 28, entered the Hospital this morning, at visit. Says that he has been sick three days, with headache and pains in the lower extremities. Taken first with a chilly feeling, when he afterwards became hot. The fever and headache have continued since. The pains in limbs are worse, so that he can scarcely now place them under him, or walk; pulse 112; tongue slightly coated; bowels open with purgative medicine, which he took the preceding day. Ordered him to be put to bed, and to have the Quinine draught immediately.

6th. Much the same. Perspired freely yesterday, after taking the medicine, and also last night. Skin, at present, only slightly moist;

tongue dry and brown; pulse 104. Ordered the same draught repeated.

7th. Much the same as yesterday; has been perspiring freely, but skin only slightly moist now. Has still the pains in limbs as bad as ever. Ordered Cathartic medicine now to open the bowels, which have not been open for two days, and in the evening to have his draught repeated.

8th. Says his pains are nearly gone; perspired freely last night; bowels opened well with Cathartic; pulse 84. Take one-half of Haustus Quina, at night.

9th. Much the same as yesterday; pulse 68. Take the remainder of draught at night.

10th. Vomited dinner and draught yesterday. Feels much the same; tongue has a slight white fur, the edges red; no tenderness over stomach; bowels not open for two days. Ordered Cathartic Powders to remedy this. Discontinue Quinine.

11th. Bowels open. Tongue much as yesterday; has been vomiting some; pulse 74; mustard plaster to stomach, and ice water for drink. Low diet.

12th. Better; no vomiting. Chicken soup.

13. Still better; walking about. Wants out; discharged.

Case VI. Thomas Gillick, aged 19. Came in December 30th; was sick two days before admission; has headache; had been vomiting and could eat nothing. Got from the student who saw him, ten grains of Quinine in draught. The following morning I found him suffering with headache, and vomiting much; tongue coated white. ℞. Sulph. Quininæ, gr. x; Sulph. Morph. gr. i; Mucil. Acaciæ, q. s., M. Fiat. pil. No. iv. Take two, morning and night. Mustard plaster to stomach; Gum Water iced, for drink; Arrow Root, Rice and Milk.

The following morning, January 1, 1853, found that he had been more or less delirious all night. Still vomiting some; skin continues hot, and pulse quick. Did not vomit the pills, nurse says. Increased the Sulphate of Quinine to fifteen grains, and to be given the same way as yesterday.

2d. Better. Pulse slower; delirium much less last night, and he slept soundly this morning. Vomiting has stopped. ℞. Sulph. Quininæ, gr. xv; Gummi Opii, gr. ii; Mucil Acaciæ, q. s., M. Fiat pil. No. iv. To be taken as before.

3d. Still doing better. Repeat the pills.

4th. ℞. Haustus Quinina; take one-half at night.

5th. Doing well; take remainder of draught at night.

6th. Discontinue medicine; nourishing diet allowed. He went on improving, and was discharged on the 9th, well.

The day on which he was discharged he washed himself down in yard, and otherwise exposed himself. The following morning he was in the same bed, not having gone out, and with a considerable fever upon him. Thinking he had been imprudent in eating, ordered him Cathartic medicine; and to be put on low diet.

Jan. 11th. Still the same febrile condition; pulse quick and skin hot; tongue moist and clean. Ronchus heard over anterior of chest, in middle lobes of both lungs, and there is some cough present this morning. Ordered a Quinine draught to be taken now; and the Hydrocyanic Acid Cough Mixture of the house.

12th. Condition the same as yesterday. The medicine to be repeated as before.

13th. Condition much the same. Ronchus over middle lobes of lungs more sonorous; pulse quick; tongue moist. Determined to change the medicine, and ordered a tablespoonful of the following every hour: ℞. Liq. Ammon. Acetas, ℥ v.; Syrupi Scillæ ℥ ji.; Tinct. Opii Camph. ℥ i.; M. Lemonade and chicken soup.

14th. Better. Pulse slow; all febrile action gone. Ronchus more moist. ℞. Carb. Ammon. ʒ iss.; Syrupi Tolutaniæ ℥ jii. Mist. Camph. ℥ iv.; Tinct. Opii Camph. ℥ i.; M. Taken as before. Repeat diet.

15th. Still better. ℞. Mist. Acidi Hydrocyanici ℥ vi. A tablespoonful when the cough is troublesome; pulse slow. Chicken and soup.

16th. Better. Discontinued all medicine. Appetite good.

20th. His pulse continued abnormally slow till this date; some days it was as low as 40 beats. Complains only of being weak. Still keeps his bed. Has full diet allowed.

23rd. Has been moving about the last three days, and was this day discharged well.

Case VII. Robert Denny, Scotch, aged about forty-five, entered the Hospital on the 27th December. Said that he had the chills and fever in Missouri, from which place he has just arrived. He is anæmic looking, and has an abscess on the right knee, which I opened, when a considerable portion of pus was discharged. There is also a small ulcer lower down, on same limb. The leg was poulticed for a few days, then bandaged, and got well. He had port wine and full diet allowed him, and his general health had improved much; when, on the

14th January, 1853, he began to complain of losing his appetite, and did not feel so well.

15th. Tongue somewhat dry and brown; pulse quickened and skin hot; does not feel so well. ℞. Liq. Ammon. Acetas, ℥ viii. A tablespoonful every hour. Lemonade and low diet.

16th. Thinking it might have been an ephemeral fever, which would pass off with slight attention, I did not yesterday suspect Typhoid Fever, but am now satisfied that he has caught this latter, probably from patients in the house. On the supposition, as two days have scarcely elapsed since that set in, there would yet be a good opportunity for trying the abortive treatment, the Quinine draught was ordered to be taken immediately. Appetite gone; tongue dry and brown, and the bowels have been loose. Gum Water for drink; Arrow Root, Rice and Milk for diet.

17th. Rather worse; pulse 106; tongue as before. Repeat the draught now, and give the same drink and diet as yesterday.

18th. Much the same; pulse 94; tongue not so dry, but still with a thick brown coating; bowels better. Repeat the draught now; same drink and diet.

19th. Rather better; tongue moist; pulse small but still quick. To have one-half of draught at night. The two preceding nights, the nurse says there has been some delirium present.

20th. Delirious last night; and is so, to some extent, at present. Knows me, but is under the impression that he has been transferred to some other ward, in which he is detained; pulse quick and small; tongue moist but clammy. Fearing that the Quinine and Opium may contribute to aggravate the brain symptoms, they were discontinued, and the following given: ℞. Carb. Ammoniæ, 3 iss; Spiritus Etheris Nit. ℥ j; Mist. Camph. ℥ v; Sya. Simplicis, ℥ ji. M. A tablespoonful every hour. Port wine with milk for drink, and chicken soup.

The following day he was worse; muttering in his sleep, and evidently sinking; the wine and diet continued. On the 22d he died. No post mortem.

Case VIII. Thomas Mackin, aged 27, a steamboat hand, came into the Hospital on the 6th February, 1853. Saw him on the 7th. Says that he has been somewhat unwell for three days. Thinks he caught a cold by sleeping on deck; felt chilly all over, and indisposed to do anything. The student, who saw him last evening, prescribed pil. Calomel and Opium, No. iv; one every fourth hour; with a tablespoonful of the Syrupi Scillæ Comp., every hour. Complains of pain in region of the heart. Nothing abnormal found, either in the lungs or

heart, on auscultation. No cough; tongue dry and brown; pulse 116. Expression of countenance easy; skin of face and hands dusky looking. Has been laid up for two days; bowels open. The case seemed to me distinctly that of Typhoid Fever; and the abortive treatment was adopted. He had a draught, with a Scruple Quinine and half a drachm T. Opii., morning, and the same again at night, ordered. Lemonade and low diet.

8th. Tongue moist, but foul; pulse 104; has perspired very freely, and feels better; bowels rather open. To have the draught repeated, morning and night, same as yesterday; and, for drink, Gum Water iced.

9th. Pulse 80; tongue moist and foul; perspired freely and slept well; feels altogether better; bowels better. Repeat draught as before. Lemonade and low diet.

10th. This morning is worse; coughed a good deal last night; a dry and hacking cough present; pulse 104; respiration 34. On auscultation, fine crepitation heard in left chest in front, in middle lobe, under axilla, and behind in lower lobe; right lung unaffected. Intercurrent Pneumonia diagnosed. Was the Pneumonia present when he entered? I think not. Did the Quinine treatment keep it in abeyance for a time? At all events, it shows that this medicine has little influence in cutting short this particular kind of inflammatory action. Similar to the case of Stewart; this one had double doses allowed: ℞. Tart. Antimoni et Potassæ, gr. vi; T. Opii. Camph. ℥j; Mist. Camph. ℥jii; Aqua Menthæ, ℥iv. M. Take a tablespoonful every hour. ℞. Emplas. Canthar., 12 inches by 6, placed across left chest, under axilla. Lemonade and low diet.

11th. Feels rather better; did not vomit any; took all his medicine; pulse 100; respiration 30. In front of left chest bronchial respiration heard, indicating hepatization; fine crepitation elsewhere same as yesterday. Repeat medicine and diet.

12th. Feels better; pulse 92; respiration 30. Repeat medicine; half diet.

13th. Still better; pulse 80; respiration 30. Coughs up some, the sputa gelatinous looking, not rusty; respiration in front returning. Mucus rale is heard; and under axilla the crepitation is becoming coarser, approaching rale. Resolution seems to be taking place very well; tongue foul. Repeat the medicine; no vomiting or nausea produced by it; bowels have not been open for two days; to have an ounce of castor oil.

14th. Pulse 80, and soft; respiration 25; skin cool; expectoration

getting more loose; sputa still tough looking. ℞. Liq. Ammon. Acetas, ℥ iv.; Tinct. Opii. Camph. ℥ ji; Syr. Tolu., ℥ ji, M. Take a tablespoonful every hour. Lemonade and low diet.

15th. Feels better; very little sputa; pulse 76; respiration 24; tongue foul; no appetite yet. Respiration in front getting clear; that under scapula and axilla becoming coarser; sleeps well. Repeat medicine and diet.

20th. Doing well; but little cough and sputa; getting now Carbonate of Ammonia, with other expectorants. Port wine three times; half diet; pulse 54; respiration 20.

25th. Discharged well.

Case IX. Cristopher Tracy, aged 12. In the house for the last twelve months; came in then sick, from off ship; and after recovery remained, having no relations in the city. Has been making himself useful; enjoyed good health; face florid. Became sick on the 10th of February, when the "Sisters" placed him in the ward, in bed, where I found him, on the morning of the 11th. Says, that on the preceding day he felt chilly all over, and felt weak in the limbs. This was followed with pains in head, and indeed all over. At present skin is hot; pulse 134; tongue with a white net-work coating; bowels open; respiration good. From all the symptoms, Typhoid Fever was diagnosed; and, as he only became sick yesterday, a good chance for the abortive treatment presented. He was ordered the usual Quinine draught immediately; having thus a scruple Quinine with half a drachm T. Opii., at one dose. Lemonade, milk and gruel.

12th. Perspired freely yesterday and last night; skin cooler than yesterday and moist; pulse 128. Says that he is better; tongue a little deeper coated; bowels open. Repeat draught immediately. Lemonade and same diet.

13th. Much as yesterday; perspired freely at night; pulse 128; bowels open. Repeat draught as yesterday; also lemonade and diet.

14th. Feels better; pulse 96. To have one-half of former draught now; with diet as before.

15th. Still better; pulse 68; skin moist; bowels open. Take remainder of draught at night. Lemonade and low diet.

16th. Scarcely so well this morning; pulse 88; skin moist and cool. Complains of having felt pains last night, and still, in knee-joints, and up on sides of neck, which kept him from sleeping. Changed the medicine, and gave the following: ℞. Liq. Ammon. Acetas, ℥ v; Sp. Etheris Nit., ℥ j; Syrupi Simplicis, ℥ ji; M. Take a tablespoonful every second hour. Lemonade, and low diet.

17th. Feels a good deal better; pains nearly all gone; took the medicine every hour instead of every second as ordered; pulse 80. Medicine to be repeated, and given every second hour. Lemonade and low diet.

18th and 19th. Much the same; medicine continued.

20th. Still doing well; tongue nearly clean; pulse 80. Repeat the medicine, and to be kept still in bed. Chicken soup.

21st. Not so well; pulse 100, but small; continue medicine. Low diet.

22d. Pulse still higher, 112. Repeat the medicine and diet.

23d. Pulse 128; tongue getting gradually deeper coated. Repeat medicine and diet.

24th. Pulse 96; begins to get better again; bowels have been open all the time; continue the medicine. Lemonade, and chicken soup.

25th. Pulse 68; feels better; continue medicine and diet.

26th and 27th. Pulse the same as above; medicine continued, with the diet. On the former of these days there were observed white elevated patches, with red margines, over the lower extremities, the abdomen and chest, resembling urticaria very much. There was felt at the time a pricking and painful sensation in the parts thus affected. To-day the eruption all gone. He had not been eating any fish, nor any indigestible food, that I know of, to account for this rash; which came out suddenly, and went off in the same manner, without any particular attention. All medicine after this was discontinued. He kept walking about for a few days, getting nourishing diet; and on the 1st of March he was discharged as a patient, quite well.

Case X. Lawrence Toole, aged 15, entered the Hospital on the 17th March; had been sick two days preceding admission, with headache and pains in all his bones; pulse 96; tongue slightly coated white; skin hot; bowels open; diagnosed Typhoid Fever; and as the case seemed favorable for the abortive treatment, that was agreed to be tried. In conversation with Dr. Fenner regarding the previous case, that gentleman suggested that the want of success might have arisen from the doses being too small, or not continued long enough; though, considering the patient's age, I was disposed to think that these were heroic enough. But in this case I determined to set out more boldly, and give the patient whatever benefit large doses repeated might afford him. On the 18th, therefore, he was ordered to have the usual Quinine draught in the morning, and another in the evening. He would thus have two scruples Sulphate Quininæ, with a drachm Tinct. Opii, taken in two equal parts, in twenty-four hours; or rather at the expiration of

twelve hours the whole of that quantity would be taken into the system, to affect it, as I hoped, beneficially. I had seen enough of it now to believe that in this abnormal condition the medicine was safe.

19th. Much the same; had perspired freely yesterday and last night. There was present last night a good deal of raving; pulse 92; skin moist. Repeat draughts as yesterday.

20th. Pulse 68; perspiring very freely. Still occasionally raving at night; tongue moist and white. Repeat the same draughts as before.

21st. Skin cool and moist; pulse 68. Has some difficulty in passing urine; bowels open. To have the same draughts repeated, and to have a teaspoonful Sp. Etheris Nitrici till urine passes more freely.

22d. Skin the same as yesterday, and pulse remaining at 68, the normal standard; urine free. To have only half the former draught, morning and night.

24th. Was prevented from seeing him yesterday. Student prescribed a tablespoonful of Liquor Ammoniæ Acetatis, every second hour. To-day is doing very well, and the medicine of yesterday ordered to be repeated.

27th. He had been doing pretty well since last report; had the same medicine continued, with chicken soup, till this morning, when he is found with some return of fever. Sat up in bed a little while yesterday, for the first time, and towards evening got feverish; pulse 92; but he has no headache; tongue slightly coated white; skin rather dry. Ordered him to have the scruple doses of Quinine with opium, morning and night, as formerly. Lemonade and chicken soup.

28th. Perspired some last night; this morning pulse 100; so that instead of being lower, it is higher than yesterday. Discontinued the Quinine and gave him tablespoon doses of Liquor Ammoniæ Acetatis every second hour; leaving the case otherwise to nature. It has turned out much as the previous boy's case did; the fever for a time strangled as it were by the Quinine, but again getting ahead, and having much its ordinary course. For a few days after this he got Carbonate of Ammonia, in Camphor mixture, with light nourishing food, and was doing very well. He improved rapidly, and on the 2d April was discharged.

Case XI. Catherine Fenny, aged 20, came into the Hospital on the 3d of May, complaining of headache and pains in all her bones. Arrived in the city, from Ireland, eight days since; did not feel very well then: had aching pains all over, but did not feel sufficiently bad to take to bed till two days preceding admission. Stout and otherwise of a healthy constitution. On the night of admission she had from the

student who saw her, the house draught of Quinine, under the impression she had intermittent fever. On the morning of the 4th I first saw her, when I diagnosed Typhoid Fever; skin was hot, pulse 104; the tongue was somewhat dry, with a whitish fur; fever continued in character. As she had not been long sick, the Quinine treatment was continued. She had one house draught in the morning, and another of the same at night. Lemonade iced, with low diet.

5th. Pulse 92. Had perspired yesterday and last night very freely. Nurse says there was some raving present yesterday and last night. Somewhat deaf this morning; has to be spoken sharply to in order to get an answer from her; arising partly from the deafness, and also from some degree of stupor; skin moist; bowels freely open. Ordered the same draughts to be repeated as before.

6th. Much the same; pulse 100; still somewhat deaf and not easily roused; occasionally raving; tongue dry with a slight whitish fur; bowels open. Ordered the same draughts to be repeated as before.

7th. Feels much the same; complains of nothing particular when roused up to speak, and has not done so since the first day, when the headache and pains in bones abated much; face dusky-looking; eyes injected and suffused; pulse 100; not full, but rather suppressed in feel; twitchings observed in tendons of both hands. Ordered the same draughts as before.

8th. Seeing that the fever did not seem to be cut short by the above means; (the pulse remaining at 100; and the same brain symptoms becoming more marked; as seen in the occasional confusion during the day as well as night, with the fixed partial stupor previously referred to; and the tongue dry and slightly furred as before,) the medicine was changed for Liquor Ammoniæ Acetas and Spiritus Etheris Nitrici. Her bowels were freely open

9th. Feels much the same; pulse 100. Continue the medicine.

10th. Pulse 112. Repeat the medicine.

11th. Pulse 116; eyes still injected; tongue much as before; no tenderness of the abdomen; ice-cloths to head; brain symptoms much as formerly; bowels open. Repeat the same medicine. She was a pretty severe case. After this she gradually improved. She was, however, troubled with bed sores. On the 19th, she was convalescing well.

Case XII. Shortly preceding the admission of this last case, a female patient, of whom I took no full notes at the time, came into the Hospital with Typhoid Fever. She reported herself sick two days prior to admission. She complained much of aching pains in head, and of muscular pains all over; which are two of the most prominent symptoms in

the cases, which have come under my observation this season; and but little either of gastric or enteretic irritation have I observed. This woman's pulse was 100 on admission; that same night she had a house draught of Quinine, (which contains a scruple of that salt, with half a drachm of laudanum,) and on the following day she had one of the same in the morning, and another at night. The following day her headache and pains in the bones were generally gone, and the pulse reduced to 92. The draughts were ordered to be repeated as before. The following day the pulse was down to 75, and all her symptoms improved. That day she had half of a draught ordered, morning and night, which she took; and the following morning she felt so well that she desired to go out. Her pulse, however, was again up to 100, and I remonstrated with her about going out in her condition; but she had some clothes left behind, which I found she was anxious about, and so went out. Whether the febrile symptoms returned again, as they had in some of the cases above, I cannot know, but suspect; or whether the agitation about getting out to secure her clothes caused the increased frequency of pulse observed this morning, while the attack itself was cut short, as it at the first promised, are more than can be affirmed. But I thought it worthy of stating among the others. It shows like several of the others the marked relief of symptoms, often secured by these draughts, which is something.

The preceding cases were selected for the abortive treatment, from among several others which, from one cause or other, were deemed less favorable for it, and treated by me in the ordinary way the same time as the former were, and with results which, all circumstances considered, did not differ very much from those obtained by the Quinine treatment. I kept no notes of these others, but the above is the impression which both modes have left behind on my mind; and it is too recent to be far wrong.

The first three cases especially gave at the time some satisfaction, and encouraged me to proceed still more boldly, in the hope that still larger doses might prove successful in arresting the disease altogether; instead of merely shortening the stages of that, as seemed to have been accomplished in the cases referred to; unless they can be snpposed to have been of an unusually mild type, at any rate, such as I have occasionally met with, this season especially. The greater number of the cases which followed did not, however, meet this expectation. The only two cases indeed, those of 9 and 10, where the disease seemed really about to be cut short, proved after all fallacious; as the disease, which seems to have been suppressed, *not arrested*, by the

free doses given, again sprung up, and went through its ordinary course after. In the case of the last of these especially, this occurrence took place while large doses were yet continued to be administered with the view, if possible, of preventing this, but of no avail. The manner in which the pulse was controled, as shown by the number of beats it made in the minute, in some of these cases is sufficiently striking. Nor, unless in those cases where local inflammation had sprung up, and complicated the cases, (as in 4 and 8 it did; and to some extent in 6 also, where a return of the febrile symptoms, complicated with bronchitis, occurred, and where the pulse became influenced to a remarkable degree;) did this medicine seem to have had, as far as I could judge, any other than a beneficial effect on the system, in the abnormal condition in which it existed in these cases.

That Quinine is capable, if given in large doses, and in the forming stage, of arresting continued fever, once that this is fairly set in, is more than my experience as yet would lead me to expect. While my but partial success with it, and previous prejudice, lead towards an opposite conclusion, as I have no wish to hide. The plan of treatment I have, however, no intention of abandonding; it is still *sub judice;* and, should it be continued, it will be less with the expectation of arresting the disease itself, than with the hope of finding in it a valuable means of mitigating the headache and relieving the muscular pains; symptoms which are as distressing as they are common in the Typhoid Fever which prevails here at least, and which were in nearly all the above cases deprived of their severity more effectually, and quickly, than I have seen done, as a general rule, by our ordinary diaphoretic medicine. I think it also not unlikely that its specific influence in thus allaying the nervous irritation, and controlling the heart's action, together with the profuse diaphoresis which it produces, may all contribute to eliminate from the disordered fluids, the disturbing cause of this disease, so as to send the patient quicker through its stages to convalescence, than is usual by our ordinary treatment.

In this I refer more especially to the *Typhoid type*, in which I alone have had an opportunity of witnessing its effects. And even here we require to know a good deal more about it before even this can be looked for with any degree of certainty. Such is the most that I can say of this plan; those who have not given it even the trial which I have done may conclude that it is more than is justified by the facts I have presented. While others, again, who have tried it, and met with success such as I can make no pretensions to, may in their enthusiasm deem my remarks as cold, and my conclusions as far different from what they

should be. I can only remind both, that if over-scepticism has prevented, for a time, the adoption of what has ultimately proved valuable in medicine, and in this way has done injury to mankind; so, too, on the other hand has over-enthusiasm, by claiming far too much for a really valuable medicine, often led to the same being altogether for a time discarded; to be afterwards brought into use, in a less pretensious form, by a more prudent set of inquirers.

Dr. Dundas, on the other side of the Atlantic, and Dr. Fenner, and those who think with him, on this side, take the position, which I am far from believing is a correct one, that intermittents, remittents, yellow fever, and all the grades of continued fever are but modifications of one another. If this were so, how could we account for the fact, for instance, that in the city of Glasgow, where this latter is found to prevail at all seasons, not a single case of Intermittent Fever is to be seen among the thousands of patients in the Hospital there, unless it be in the person of some old invalid soldier, returned from foreign parts, who may once in an age show them what the chills and fever really are. As to the Yellow Fever, if it is ever to be met with, it must only be when it is imported, as was lately the case in an English seaport. But I must leave this subject.

These same gentlemen also contend, that all these diseases (or, as they would say, modifications,) are capable of being arrested in the forming stage, by means of large doses of Quinine. It is true, that from a remark made by Dr. Fenner, in the last March number of this journal, we might be led to suppose that neither Dr. Dundas or any of his coadjutors entertained the idea of cutting them thus short in the forming stage, as he proposes; but this is a mistake, which will no doubt be corrected when pointed out. In reference to arresttng the disease more immediately under review, the language of Dr. D. is explicit, and in no essential differs from that published by Dr. Fenner as his; as the following, taken from the British and Foreign Medico-Chirurgical Review, for October, 1852, will show. It is there mentioned, among other things appertaining to this subject, "that Dr. Dundas states, that if Quinine be used at the commencement of continued fever, in doses of ten or twelve grains every two hours, the disease will be arrested in the great majority of cases." Any difference in their manner of exhibiting these large doses, is surely not an essential matter. The chief difference which I see between them is not connected with the early stage, which both agree can be cut short by the employment of large doses of Quinine, but concerning the later stages, which Dr. Fenner, in his latest publication, continues to advise us to make no attempts

with the abortive treatment after the disease has got beyond the third day, but to treat them by the ordinary method till it goes through its course, while Dr. Dundas, on the other hand, and also Dr. McEvers, have carried the war boldly beyond this, and given it in cases where the disease reached all stages; and, if we can believe themselves, with a degree of success which has highly rewarded them for their temerity; and which has evidently astonished even Dr. Fenner. Indeed, from the manner in which he refers to the results published in the "remarkably interesting paper" of Dr. McEvers, of Ireland, (which has been widely circulated in the medical journals of this side of the Atlantic also,) one would expect to find that he has abandoned all his previous precautionary notions about tampering with this remedy at a later period than that above referred to, as partaking of old fogyism; and that he is now. from the high estimate he places upon Dr. McE.'s results, heroically giving the remedy at all stages also.

For my own part, not putting the same high estimate on the cases there reported, I have not ventured to follow the practice; and I think a little more scrutiny into the facts which he has given, will show that the advice to be cautious as to how we tamper with this particular treatment in the later stages of this disease, is one which should not be abandoned, till at least something more satisfactory, than has yet been furnished by this writer, be produced.

In support of this, let me for a moment refer to the cases themselves, which have been briefly enough reported by that gentleman, in which he used the Quinine treatment in the Cork Fever Hospital. His first, Pat. Ryan, aged 28, was in the thirteenth day of his illness; his pulse 112; tongue parched, with sordies on the teeth and lips; and raving a good deal. It was at this stage of the disease that he had ten grains of Sulphate of Quinine every second hour, with broth and four ounces of port wine allowed him. The next day we are told the changed condition was most remarkable; the pulse was considerably reduced, the tongue was moist and cleaning; the man slept well. A continuance of the same treatment another day, and he is convalescent. Now it is surely not such a very remarkable thing to see a patient at this period of the disease, get "the turn;" and sometimes, as here represented, this change for the better is as sudden as it is marked; and we are perhaps but too apt to ascribe it to whatever medical treatment should have preceded it. In the above case, supposing it to be as represented, the Quinine gets the credit of all; the port wine and the broth, which are known to be potent in this condition, go for nothing in the calculation, or next to nothing.

There is a necessity of not letting our enthusiasm in these isolated cases carry us too far one way, any more than our scepticism the other way. I could for instance relate a case in a similar condition, which was brought into the Charity Hospital, at the beginning of the present year, where a young man was taken from off an emigrant ship in the twelfth day of this disease. I saw him as I was about leaving the ward, and after the "prescription book" had been sent down to the apothecaries; his tongue was dry and covered with a brown coating, and he was looking considerably exhausted. He was simply ordered to be put into bed, with the expectation that the party paying the noon visit would further see to his case. The following morning I found him altogether much improved, the tongue was moist, the pulse had greatly abated in frequency. In the interval he had received no medicine whatever; the simple drinks taken, with a clean bed, and a more airy apartment than he had occupied on board the ship, in all likelihood contributed to give him "the turn," and that sooner, and more completely, than would have been the case in the same time had he remained on board the vessel, and in less favorable circumstances.

For this disease to reach its crisis on the twelfth or fifteenth day is not, therefore, anything so very remarkable. Dr. Watson, in his lectures, we find, testifies to its not being uncommon in Ireland, in certain seasons, to see it do so as early as the ninth day of the attack.

His second case is that of a young man, aged 19 years, whose complaint does not seem to have been made out very well till the ninth day, and I am not sure that it even then was. On the eight day his breathing became hurried, and the surface became "remarkable palid." Congestion of the lungs seems to have been suspected. He got in this condition stimulants, and a sinapism applied to his chest, which was followed with relief to the more urgent of his symptoms. But the following day a "purple patch" was discovered occupying the part to which the mustard had been applied; this led, we are told, to further investigation, which resulted in the conviction that the case was Typhus Fever, and was well adapted for the administration of Quinine. He was accordingly ordered ten grains every two hours, together with broth and port wine; but the third dose produced violent headache, and the medicine was discontinued. The symptoms were, nevertheless, improved; and the following day the medicine was ordered in the same way, and with a like result; it had again to be laid aside, and was no more attempted. The patient improved rapidly; but whether from the Quinine, or in spite of it, people will differ in their opinions; as they also will about whether this was a case of Typhus Fever at all; or a complication of some

other disease; such as indeed it was deemed to be, till the ninth day, when the unfortunate, but natural, appearance of purpura, where the pungent mustard had been applied, led to a different diagnosis and treatment, which thus further established the power of Quinine to cut short Continued Fever.

Of John Eames, the third case reported, it is only necessary here to say that he had a bad attack of Typhus Fever, with bronchial complications, from which he recovered, and was discharged. He had a relapse and returned to the Hospital. Here he was placed for this on ordinary treatment, and up to the tenth day there was no improvement. He was then ordered to have ten grains of Quinine every second hour; with broth and port wine; from this period his condition improved. The treatment was continued in diminished doses for two days longer, when he was convalescent.

And this is put down as another of the vaunted cases of Typhus Fever, which have been "cut short" by Quinine; as if there was anything at all remarkable in a case of ordinary relapse beginning to show signs of improvement, as this one did, on the tenth day; which it usually does, whether any specific treatment be used or not. The smaller number it is, of those which I have seen, that hold out, even as long as this one did, with our ordinary treatment.

The fourth case was that of a young woman, who was ten days ill with the usual symptoms of Typhoid Fever; when Quinine, in the above doses, together with the broth and port wine were ordered her; and on the day following we find her somewhat improved; the treatment is continued for the two days following, in diminished doses; and on the 14th day she is pronounced convalescent. From the comparatively more subdued tone of this report, I conclude it must have been a mild case; and if so, what was said about the period of getting "the turn" in the first case, without our ascribing any very wonderful efficacy to the treatment pursued, may be made to apply to this last also, and prevent the necessity of our handling the "young woman" any further.

A few other cases are simply alluded to, where the same treatment we are told was pursued, and with similar success; which success, after what has been said, need not be esteemed as very wonderful.

At all events, it seems to me that it does require from these few cases of Dr. McEvers, a considerable leaning towards the marvelous, with no small share of enthusiasm in ones composition, to draw therefrom any marked indication of a fast approaching "important revolution in the pathology and treatment of Idiopathic Fevers," as has been done by Dr. Fenner.

That Typhoid Fever, at least in this quarter of the world, in the general run of cases shows signs of improvement about the twelfth or fourteenth day of the attack is what, at least, my experience has led me to believe; and I must have had several hundred under my care, of which to judge from. This last season too the cases have not only been fewer but, as already mentioned, milder in type than in any of the preceding seasons; and this should not be overlooked when the effect of our remedies are under consideration. I have seen cases under the ordinary treatment recover as rapidly as cases 1, 2, 3 or 5 did, with the Quinine treatment. These cases we usually look upon as having been of a milder type than the others which linger longer, though getting the same medicine. Some cases turn out, as is well known, to be so mild indeed, that we do not consider it proper to put them down, when they recover, as cases of Typhoid Fever at all, but rather call them ephemeral or febricula, names which apply to them better; none of these facts, I again repeat, should be lost sight of in reviewing our cases, or calculating the effect of our treatment.

The following statistics will exhibit the prevalence of Continued Fever, and the rate of mortality from that cause, in this city, during the few past years; as also the fact of the gradual abatement of the disease among us. Its mortality at the last report is still higher than I should have anticipated; that of this last season has not as yet been published. I think it will be smaller.

The distinction made in this city, between Typhus and Typhoid Fever, I believe is, for the most part, founded on the severity of particular symptoms rather than upon any conviction of these diseases being specifically different, as is contended by many excellent authorities.

In the year 1848 there were 1882 patients treated for Typhus Fever in the Charity Hospital, and of that number 344 died, about 18 per cent. In 1849 there were 970 cases of the same treated there, and of that number 224 died, about 23 per cent. In this as well as the preceding year, some of the medical attendants, students and nurses, as well as the "Sisters of Charity," who came in contact with the patients, caught the fever, and in not a few instances were cut off by it. The type had much of the virulence of that seen in larger cities on the other side of the Atlantic; and it was no uncommon thing to see the characteristic spots upon the breast and bodies of these patients, at the period referred to. In 1850, there were 1043 Typhus Fever patients, and 208 Typhoid Fever ones in that institution; 146 of the former died, about 14 per cent, and 34 of the latter, about 16 per cent. In 1851, there were 1301 Typhus and 271 Typhoid Fever patients treated there;

278 of the former died, about 21 per cent, and 66 of the latter, about 24 per cent. In 1852, the last published, there were 1008 Typhus Fever patients and 498 Typhoid Fever; 150 of the former, died about 14 per cent, and 106 of the latter, about 21 per cent.

Statistics are valuable, and frequently necessary to correct erroneous conclusions. In the present instance, guided by impressions drawn from my own particular experience, I could not have supposed that the mortality was so great, as I find in the above years it has been from these diseases. This refers more especially to cases of Typhoid Fever; the mortality of which is high; but not higher I find than the same in British and in Northern Hospitals. There is surely, besides what can be done by improved sanatory arrangements in the way of diminishing these diseases, ample room for something being accomplished beyond what yet has been, by improved medical treatment to lessen this mortality. Whether the Quinine treatment is a step that way time is required to tell; but I must draw to a close.

VI.—THE ABORTIVE TREATMENT OF CONTINUED FEVER.

BY E. D. FENNER, M. D.

I propose, on the present occasion, to notice briefly some of the comments that have appeared in the Medical Journals of the day, upon the *abortive treatment* of continued fever, advocated by me in this journal and the second volume of my Southern Medical Reports. As I write only at considerable intervals, it may not be amiss to state again, and very succinctly, the positions I have taken in the discussion of the subject, before replying to my commentators. Let me say, *in limine*, that in offering some further defence of these positions, I do not wish to be considered as the uncompromising advocate of certain views, merely because I have published them. I am not in the position of the paid attorney, who is employed "to make the worse appear the better cause;" on the contrary, I trust I am an earnest seeker after *truth*, in the investigation of a subject of vital importance to mankind. In submitting to my professional brethren the results of my observation and experience, it is not with a view of *dictating*, but rather of soliciting inquiry into their correctness. If they are corroborated by the experi-

ence of others, they will doubtless prove beneficial; but, if, after a fair trial, they are not, I shall have reason to doubt their accuracy, and to give them a more rigid investigation.

As the Editor of the Southern Medical and Surgical Journal very justly remarks, (March No., 1853,) "It is a question to be decided at the bed side, and without reference to preconceived notions or theories. It matters not whether typhoid and malarial fevers be regarded as identical or dissimilar in origin, cause or nature; what we need is a remedy—a controlling agent. Let us then weigh the question calmly and without prejudice—try fairly the plans proposed—and the truth will soon be determined." This is all I have asked in regard to my plan of cutting short continued fevers; but it seems, that as yet, not a single one of my commentators has done me the justice to give it a *fair trial*.

The positions I have assumed, are—

1st. That all the continued fevers originating in the Southern States, are *but varieties of endemic malarious fever*, and are controllable by the same remedies, if judiciously applied at their commencement:

2d. That all these fevers, most probably, proceed from an ærial poison, which enters the blood; first exerting its malign influence upon this fluid, and thence conveyed through the channels of the circulation to every part of the system:

3d. That the primary perturbation of the system generally recognized as an *attack of idiopathic fever*, consists for the most part in *functional derangement without any special organic lesion*, and is controllable by such a remedy or combination of remedies as is capable of equalizing the excitement and circulation, restoring the healthy action of the secretory organs, allaying pain and depurating the blood. It is believed that such a combination of remedies may be found in blood-letting, large doses of quinine and opium, and some mercurial, as calomel or blue mass:

4. That the *organic lesions* found on post mortem examinations of fever cases do not fully indicate *the disease* that had existed *ab innitio;* but rather, *the secondary and ultimate results of disordered action proceeding from a blood poison;* which morbid action or functional derangement might probably have been corrected, and the blood-poison *neutralized* or *eliminated* by the judicious application of appropriate remedies.

The idea of the close relationship subsisting between our endemic continued and paroxysmal fevers was thrown out more as a *suggestion* rendered extremely probable by the facts that had presented themselves to my observation, and been reported by others, than as a *demonstrated*

truth; but I have now to add, that the more I have investigated it, the more I have become convinced of its truth. The reader who will go to the trouble of carefully perusing my paper in the last November No. of this journal will there find such facts and rude arguments as I was able at the time to throw together in support of the foregoing positions. Further observation and reading have supplied me with others, which will appear in the course of this or a subsequent paper. These views have elicited critical remarks from various quarters, and I shall now proceed to notice them, commencing with the last and most elaborate that has met my eye, viz: the paper of Dr. R. L. Scruggs, of Shreveport, La., "*On the use of the Sulphate of Quinine in Typhoid Fever,*" which appeared in the last number of this journal.

Soon after the publication of my paper before-mentioned, in reply to the remarks of Dr. Boling, "On the use of Quinine in Continued Fever," I took occasion, in a social letter to Dr. Scruggs, to invite his special attention to the subject, but implored him not to criticise my plan of treatment without first giving it *a fair trial.* It was not long before I received a somewhat lengthy communication from the Doctor, intended for publication in this journal, but addressed directly to me, with the request that I would hand it over to the editor, with such comments as I thought proper to make. My surprise may well be imagined when, upon opening this communication, I found in the *second sentence,* that my friend had done the very thing I had particularly requested him *not to do,* and left undone that which he ought to have done, viz: he had criticized my plan of treatment without giving it a trial. On seeing this, and finding nothing new in his paper—in short, only a repitition of some of the prevailing opinions of the day—I took the liberty of sending the paper back to him, with the expression of my disappointment, and requesting that he would either suppress it, or alter its address. The next I heard of it was its appearance in the last number of this journal. I might in all justice, dismiss the paper without comment, as it contains no trial of my plan of treatment; but my personal regard for the author induces me to notice some of its salient points. In the second sentence Dr. Scruggs remarks—

"In your letter to me you request me not to criticise your plan without first giving it a fair trial; but I fear I shall never have the boldness to do this, since it would appear from your writings, that you consider a larger quantity of quinine necessary to arrest a mild case of typhoid fever than would be required to break up the chain of morbid catenation in cases of pernicious intermittents or remittents."

The reason here offered for rejecting the proposed plan *withou*

trial is certainly not very forcible, for it by no means follows as a necessary sequence that, because it requires a larger quantity of quinine to cut short a mild case of typhoid fever than a pernicious intermittent or remittent, the former cannot be cut short by quinine at all. The typhoid type of fever, although generally mild and insidious in its approaches, is known to be very obstinate in its persistence, and *may* absolutely require larger doses to control it than more severe attacks of the other types mentioned. In my first remarks on the subject—(note appended to Dr. Fearn's paper, in *Vol. II, Southern Medical Reports*)—this fact was distinctly stated, and the reader was cautioned not to tamper with the remedy at all, unless he had the boldness to administer it in the doses and at the stage mentioned. Now, if Dr. S. could not possibly summon sufficient boldness to make a single trial of the plan proposed, he had better withhold his condemnation. It is vain for him to "assure me that long before he saw the idea suggested by me he entertained similar views, and laughed at the idea of permitting any fever to run its course, without an attempt to arrest it with quinine—that his opinion was based upon a long and satisfactory use of it in the worst forms of malarious fevers; and, therefore, that it is not probable the remedy failed to have a fair trial at his hands." As an offset to all this, we have *his own admission*, that he had not "*the boldness*" to try the remedy as I have prescribed it, and *never has tried it.* He, therefore, has no right to condemn it. But, I would respectfully inquire, why is he afraid of it. He has the testimony of Dr. Boling to the effect that even where it failed to produce *all* the good effects that were expected from pretty free doses, it still did no harm, but afforded considerable relief to the most distressing symptoms; and there is plenty of testimony to the same fact. The method followed by Dr. Scruggs, as stated in his late paper, is the only one that is universally condemned. What is this method? He says, "I never give more than twenty grains at a dose, in any fever; and more generally than otherwise, five grains is the dose prescribed by me—to be repeated every hour or every two hours, until 25 to 40 or 50 grains are taken."

Now, I would respectfully ask whether Dr. Scruggs has yet to learn, after all that has been written on the subject, indeed, after his own extensive experience with the article, (for he says he has used it freely,) that the effect of quinine given in *large* and in *small* doses is altogether different—that in large doses, say 30 or 40 grains, it is directly *sedative*, whilst in small doses, say 2 to 5 grains, it is tonic and *stimulant*—if he has, I shall have to give him up as being incorrigibly *stationary* in his notions. Dr. Scruggs may succeed

in cutting short *pernicious intermittents and remittents*, with the doses he "more generally prescribes," viz: 5 grains every hour or two; but I am inclined to think that not many others do. Even the Northern writers, Drs. Wood, Bell and others, advise larger doses in these fevers than that which Dr. S. *never exceeds*, viz: 20 grains. It is astonishing what repugnance is manifested by the great body of the medical profession, to the admission of any bold or decisive *innovation* upon the generally received practice of physic. It is but three years since there was an animated discussion in the Philadelphia County Medical Society, as to the propriety of giving quinine in remittent fever before febrile action had entirely subsided. Drs. Parish and Jewell maintained the affirmative, whilst Dr. Samuel Jackson, of Northumberland, contended for the negative with all his might. Even in the last number of this journal, we find Dr. Boling reporting facts and cases, to show that quinine has the power of reducing the pulse below the natural standard, which had been questioned recently by a Western Professor. Yet, strange to say, *twenty years ago*, some of the British Army Surgeons, in the West Indies, and some of the physicians in our Southern States, had learned and made known that severe remittent and continued fever, and even yellow fever could be cut short by large doses of quinine, given in the early stages, and while the febrile excitement was still raging.*

Dr. Scruggs says he never gives more than the quantity of quinine before stated, "because experience has proven to *his* satisfaction that this quantity is amply sufficient, and both reason and experience teach him that more than enough is likely to do harm, cannot possible do good; and is, furthermore, a foolish waste of a costly medicine." In my humble judgment, the Doctor's "reason" is totally at fault in the premises, and he has not learned as much from the lessons of "*experience*" as he might have done. If he had given his "40 or 50 grains," in one or two doses, instead of *small fragments at short intervals* in the course of the day, I am sure he would have derived more satisfactory results, and furthermore, instead of it being "*a foolish waste of* a costly medicine," he would have found it to be a *wise saving;* as was demonstrated to the Chief of the Medical Bureau, at Washington, a few years since, by our Army Surgeons in Florida and Arkansas.

So much for the Doctor's method of giving sulphate of quinine in

* The reader, who desires to see some interesting information on this subject, should consult the work of Dr. Blair "*On the Yellow Fever Epidemic of British Guiana.* London: 1850. He will there see what important facts have been overlooked by the teachers and authors of the present day. F.

continued fever, the results of which have been about as unsatisfactory as I should have expected. Unless he can improve his *courage* he will never learn from *his own experience*, the full powers of this invaluable medicine. But I hope his courage *will* improve. Physicians of my acquaintance now unhesitatingly prescribe ten or twenty grains of quinine at a dose *in the febrile state*, who, ten years ago, would have shuddered at such a prescription as almost certain death. The great truth so long hoped for, that *endemic fever of every type can be cut short*, is slowly gaining ground. It will get into the schools after a while; and then our young graduates will go forth better prepared than they ever have been, to combat the most formidable enemy of our race—*fever*.

Dr. Scruggs confesses himself under everlasting obligation to a certain "sage son of Æsculapius" for *two ideas*, which he considers highly important, viz: 1st, That *typhoid fever cannot be cured;* 2d, An *infallible method of determining whether a case is typhoid or not*—which is by observing the effects of quinine; if the fever is cured by it, it is *not typhoid;* if it is not benefitted by it, it is *typhoid*. Now, in my opinion, the *former* of these ideas is about as *true* as the once prevailing one, that it was impossible to prevent small-pox by vaccination; which, history informs us, was maintained to the last by almost every physician in Great Britain who had attained the age of forty years when the immortal Jenner proclaimed his great discovery: The *latter idea* is about as *valuable* as the generally received doctrine almost to the present day, that Peruvian bark and quinine can only be given with benefit during the *intermission* or *distinct remission* of fever. I am as well satisfied as Dr. S., or any one else can be, that *a mild expectant plan* of treating *typhoid* fever is far better than any active medication I have as yet seen published by other American physicians, including the 5 and 10 grain doses of quinine, which seem never to have done well in this country. It is, indeed, a safe plan, and will most generally lead to recovery; but is it not a terrible calamity to be kept in bed from 20 to 40 days, even though the suffering be not very acute; and is not the prevention of such a calamity well worthy of continued experiment. I have shown in my previous paper that Louis, Bartlett and Flint have hopefully predicted the attainment of this object some day or other, and encouraged us not to cease our efforts on account of the failures hitherto made. I am now advocating a plan which, as far as it has been tried, promises well. We have seen enough of it to know that it may, at least, be tried with safety, and will very seldom do harm when it fails to do good. On this point I find myself quite at variance with Dr.

Scruggs, whose "experience is (he says) that quinine is never productive of good effect in these cases, will *always* produce distressing head-symptoms, will frequently convert a mild into a severe or dangerous case, and sometimes, (particularly when given in large quantities,) cause death." This experience is quite at variance with that of others who have given quinine much more boldly than Dr. S. seems ever to have dared. Dr. Boling and Dr. Flint found 20 grain doses to do *no harm*. Dr. Stone testifies to the same—and even Dr. Macgibbon of this city, whose experience is given in this number, was so much pleased with the effects of the remedy in relieving painful symptoms, that he says he shall continue to prescribe it with this view, although he does not think it can be relied on to cut short the fever.

On this point, Dr. W. Taylor, of Alabama, speaking of quinine in continued fever, says, "When given in large doses, say ten grains every two or three hours, and continued until thirty or forty grains are taken, it seldom fails to be followed by the happiest results. I know there are those who entertain a contrary opinion, but as yet I have never seen any injury result from prescribing it, in however large doses, or however long continued; but in the great majority of instances, a marked amelioration of all the febrile symptoms has immediately followed its administration. And even in those cases in which the febrile symptoms were not abated, the patient was subjected to no inconvenience save, perhaps, some unpleasant ringing in the ears, and a slight temporary derangement of the head."*

Dr. L. H. Anderson, of the same State, in a *note* to his Prize Essay "*on the Summer and Autumnal Fevers of South Alabama*," says, touching this point—"I visited a patient occasionally some years since, whose friends gave him during an attack of typhoid fever, 380 grains of quinine; generally in large doses. Except the complication of a severe swelling in both lower extremities successively, resembling phlegmasia dolens, during the fourth week, there was nothing unusual in his case, and he made a good recovery. I could not see any bad effects that could be fairly attributed to the quinine. The latter, however, appeared to be about half mannite."†

Dr. Scruggs thinks I regard too lightly "a knowledge of the intestinal lesions peculiar to this disease"—alluding to the ulceration of Peyer's glands—he even goes so far as to say he is "well satisfied

* Proceedings of the Medical Association of the State of Alabama, at its Sixth Annual Meeting, December, 1852.

† Ibid.

from *his own observations*, that disease of these glands commences with the fever, if it does not precede it." Now, as nothing but *necroscopic observation* could *positively* settle this point, and the Doctor's "plan of treatment, adopted in Tennessee and pursued in this State for the past three years, has been so entirely successful," I am at a loss to conceive how he could have so well satisfied himself in regard to it from *his own observations*. For my own part, I think it must *necessarily* remain a matter of *conjecture* whether these glands are inflamed or not in the *primary* stage of the fever, for want of opportunities to settle it: because so few die of the disease in this stage. I have at least shown, in my first paper, that some able men concur with me in supposing that this organic lesion is *secondary*.

Dr. Scruggs says a "*typhoid condition* of badly treated and protracted cases of bilious remittent fever has undoubtedly often been mistaken for the disease in question, (typhoid fever,) and that too by physicians of considerable reputation." He thinks this error existed in the cases reported by my friend Dr. Fearn. Now, if typhoid fever really depends on a *specific anatomical lesion*, it cannot exist without that lesion, nor should the lesion exist without producing that fever—because they are mutually dependent upon each other. *Fever* (used as a generic term) is but a morbid condition of the system; and the various recognizable *types* of fever can alone be referred to the different morbid conditions that uniformly accompany them; thus, the *typhoid condition* makes typhoid fever, the *congestive* condition makes congestive fever; the *hæmorrhagic condition with yellowness* makes yellow fever, etc., and when any of these types of fever prevail, its characteristic lesion or morbid condition is generally found to accompany it. It follows then, that wherever *the condition* of a type is found, *that type must exist*; and as Dr. Scruggs says the *typhoid condition* appears in protracted cases of *remittent fever*, it proves incontestably, if the foregoing premises be true, that typhoid fever and remittent in some of its stages are the same. My own observation justifies me in saying, that in protracted cases of yellow fever presenting the typhoid condition, ulceration of Peyer's glands is generally found after death.

Dr. Scruggs maintain that "typhoid fever is a disease *sui generis*, and does not belong to the class of miasmatic fevers—neither is it typhus." He is not alone in these opinions, but I think they are erroneous. I have not time at present to discuss this point further than was done in my first paper, which the reader will please refer to, but will merely state a few facts that bear upon it. It is well known that

the endemic fevers of any special locality undergo great changes in their general character in the course of time, and consequently demand a marked difference of treatment. Dr. Watson, in his lectures, cites a remarkable instance. He says that the fevers of London were so greatly changed after the first epidemic of cholera, that they very seldom required the use of the lancet; whereas, previously it was indispensable. It was only after the first epidemic of cholera in this country, that the *congestive type* of fever prevailed so extensively in our Southern States, as Dr. P. H. Lewis has shown in his Medical History of Alabama; but that type has had its day, and is now comparatively but seldom seen. Typhoid fever was its successor, and is still the most prominent prevailing type. Dr. English and other physicians of Alabama, state that it has pretty much superseded the severe remittent and congestive types that once prevailed in that State.

I find in the last volume (1853) of the *Proceedings of the Medical Association of Alabama*, some interesting remarks on this point. One of the contributors, Dr. W. Taylor, of Talladega, in an able paper on the "*Changeability of Disease*," says "He that has yielded in any considerable degree to the migratory character of our people cannot fail to have noticed the gradual change of disease in our newly settled States. First comes autumnal intermittent and remittent fevers, from the mildest to the most malignant grade. As the country becomes older and more settled, these diseases gradually give way, presenting in the mean time every phase of their respective types, until they are finally supplanted by dysenteries and continued fevers."

In a *Prize Essay* read before this Association by Dr. L. H. Anderson, of Sumpterville, "*on the Summer and Autumnal Fevers of South Alabama*," I find the following—"Dr. Drake in his great work on the disease of the 'Interior Valley' predicts, among other changes that are probably to occur, that 'autumnal fever will decrease, and typhus and typhoid fevers become more prevalent.' The change would seem to have already commenced in South Alabama; for autumnal fevers have notably declined af late years, and the typhoid, a case of which we never heard of here till within the last five years, has become no stranger among us. The changes will, perhaps, be thus gradually effected by the merging of miasmatic fever into typhoid; and we have had cases that seemed evidently of a mixed character, commencing with bilious symptoms, and assuming those of typhoid during their progress. On the other hand, the latter will occasionally go off with an obscure attack of intermittent, requiring the use of anti-periodics."

In the valuable documents of the General Board of Health of England, which have been sent to me recently, I find it *demonstrated* by Dr. William Lee, that all the types of fever, intermittent, remittent and continued, arise from local causes, are often seen prevailing together, and are certainly *preventible* by the removal of filth and proper attention to drainage.

The typhoid type of the present day in these parts will probably soon pass away as others have done before it, and to my mind it is far more natural to look upon it as *a variety of endemic local fever* than as a disease, *sui generis*, brought to us from abroad, and regenerating itself by *infection* or a *materies morbi* conveyed from one person to another. Let gentlemen pause and reflect upon these facts, to which many more might easily be added if time were allowed. But if we succeed in curing typhoid fever with the same remedies that cure intermittents and remittents, (as I confidently hope we shall,) there will then remain "not a loop to hang a doubt upon" as to their close relationship, if not *identity*.

The remarks of Dr. Scruggs relative to the weight that is due to the testimony of Dr. Thomas Fearn are altogether *gratuitous*. Dr. F. has only related some extraordinary facts which fell under his observation; and to this extent, if no further, his testimony is as valuable as any man's, living or dead. If my friend Scruggs should ever establish such a reputation in his profession as was enjoyed by Dr. Fearn,I shall cheerfully tender him my congratulations.

So much for the observations and opinions of Dr. Scruggs. I have been drawn very reluctantly into a discussion with him, but have endeavered to represent him fairly in the points on which we differ. When he addresses me again *through the press*, I hope it will be with reports of *facts free from prejudice, and experiments fairly made*.

In the *January No.* 1853, of this journal, I find a very brief notice of my plan of treatment in typhoid fever, from Dr. Patton, of Mississippi. He says, "During the first four days all the abortive means advised by Dr. Fenner and others were, I conceive, most faithfully employed; quinine was given in doses from 20 to 40 grains, repeated *occasionally*, according to effects." He also gave the veratrum viride a trial; but neither of these remedies, in his hands, arrested the fever. After giving the abortive treatment, what he calls "*a fair trial*," (though not more fully stated than I have just quoted,) and finding it to fail in cutting short the disease, he says, he "calmly informed his patient that he labored under a *very peculiar fever*, termed typhoid, and that it would

run its course in spite of all the remedial measures that could be used." From this time, he "*watched* him by day and prayed for him by night, gave medicine in small doses, and after a long time, (15 to 42 days,) all his patients (13) save one, recovered." This accout is entirely too imperfect to deserve special notice.

In the same number of this journal is to be found a very imperfect "*Abstract of a Discussion on Typhoid Fever*," in the Physico-Medical Society, on the 20th of November last, in which the remarks of Dr. Stone, Dr. Hunt and myself are reported. For my own part, I had not the least idea at the time that the discussion would be reported for the press; nor had the other speakers, I presume. My surprise may therefore readily be imagined when I saw it published in the journal. In this report Dr. Stone is made to contradict *himself* as well as some of the best writers on the subject, by saying in one place that typhoid fever "*had no special pathology*"—and in another, that "*it was a specific disease; as specific as any of the eruptive fevers are.*" Now, a *specific disease without a special pathology* would be an *anomaly* in nosology. In short, it is a flat contradiction; for nothing but a special pathology can make a specific disease, or one that is different from all others. And where is the writer who contends for the *specifc nature* of typhoid fever, without insisting on the *uniform lesion of Peyer's patches?*

Again, Dr. Stone is made to say, that "When cases commenced with intermittents, (meaning *chills* I presume,) it would be well enough to give quinine; but in those that do not thus commence, it is not beneficial; and this particular fever, *which commences and goes on gradually without these intermissions*, could not be broken up by this or any other treatment he knew of." The contradiction here is at once apparent, and we are left in doubt whether Dr. Stone admits that typhoid fever ever commences as an *intermittent*, as asserted by Louis, Bartlett Flint, and others.

Dr. Stone's testimony is reported *adversely* to the assumption that typhoid fever may be cut short by quinine, though with the following qualified admission in favor of the remedy. He says, "There is one thing he had noticed in regard to giving quinine in typhoid fever, and which may, in some measure, account for any benefit that might be supposed to arise from that mode of treatment—the patients are otherwise left alone, and not made to have too much purgatives and emetics." This is well enough as far as it goes; but my recollection is, that Dr. Stone stated distinctly, that although he had not seen typhoid fever cut

short by quinine, and did not think it could be, yet he had never known this remedy *to do any harm* in such cases; and this recollection has been confirmed by Dr. Stone in a conversation with him since that time. This testimony is of material importance, as it helps to show there is *no danger* in giving quinine boldly in this disease. Dr. Stone is a strong advocate of quinine in liberal doses. He tells me that ever since 1836 or '37, he has been in the habit of prescribing it boldly, in yellow fever and remittents; not as a *mere tonic* or *anti-periodic*, restricted to the intermission or remission, *but in the stage of febrile excitement*, and for the purpose of *putting down and curing the fever*. He does not say he ever applied the remedy in the way I recommend for cutting short *continued fever*, and only gives the result of his observation as he has ordinarily used it. There is no man whose practical observations I consider of more weight than Dr. Stone's, but they should not be arrayed against my plan of treating typhoid fever until he can say he has given it *a fair trial.*

The remarks of Dr. Hunt, on this occasion, are so briefly and imperfectly reported that I shall not attempt to reply to them. As the publication has been made and referred to by the journals of the day, I regret that the report was not more full and correct.

A writer in the Boston Medical and Surgical Journal, over the signature of "*Southerner,*" takes upon himself to condemn the abortive plan of treating typhoid fever proposed by me, not only *without a trial*, but even *before he had read my paper on the subject;* such conduct precludes the propriety of my noticing his paper. I only mention the fact to show the injustice that has been done me. The author might well be ashamed to put his name to such a communication.

Dr. W. Taylor, of Alabama, (before quoted,) says, "It matters little what phases our febrile diseases may wear, or what types they may assume, we have a remedy with which, as a general thing, we do not fear to meet them, even though they present us as many forms as Proteus himself. The sulphate of quinine, as a remedy in fevers, whether bilious, intermittent, remittent, or continued, has never been too much valued or too highly extolled." He bears favorable testimony to the efficacy of quinine in the *continued fever* of his locality, previous to an epidemic that prevailed the past autumn, which, it seems, somewhat baffled the remedy; but I should say, for want of *sufficiently early and bold* administration. He says:

"I would not be understood as assuming the ground that quinine is capable of arresting all cases of typhoid fever; on the other hand, I believe it is not.

Within the last three months, Dr. Moore and myself have administered quinine in full doses, in numerous instances of the disease, in which it seemed to exert no controlling influence. But in the majority of these cases the disease was remarkably insidious in its approach, the patient often being indisposed with slight headache and fevers, for a week or more before we were consulted. Had full doses of the medicine been given at the very outset of the disease, the result would, perhaps, have been different; but, as it was, the weight of evidence was against its use."

I mentioned in my appendix to Dr. Fearn's paper, (*Southern Medical Reports, Vol. II,*) the difficulty here stated of applying the abortive treatment to typhoid fever, on account of its insidious approach. I never expected to see it so successfully applied to this type as to the bilious remittent, and yellow fever; yet I have brought forward ample testimony to prove that it will most generally succeed, and may always be tried without injury to the patient.

Let me now close this discussion, (already too long,) by reporting some cases that have fallen under my own observation. *Facts* are more forcible than argument.

I shall commence these reports with a lot of cases that occurred among Irish emigrants who came over on the ship "Samuel Lawrence," which landed here from Liverpool about the 24th March last. On inquiry, I learned that there was but little sickness on board the ship during the voyage, but it seems that very soon after landing in this city the emigrants began to get sick, and nearly all the cases presented the continued type of fever—typhus or typhoid. I witnessed the autopsy of several who died, and they all presented ulceration of Peyer's glands, excepting two, one of which died after an illness of only seven or eight days. In this case the glands of Peyer were *very slightly* developed. Some of the cases were very mild and required but little medicine. Others were severe and very difficult to control. The cases were all treated by me—the notes taken by the resident students of the Charity Hospital.

Case. 1. M. H., a robust Irish girl, aged 20, entered the hospital on the 3d of April, was taken sick the day previous. Says she was attacked with chilliness, soon followed by hot fever, with severe pains in the head, back and limbs. These symptoms continued when she was admitted. The house student prescribed a dose of castor oil.

April 4th. I saw her for the first time. She has high fever, face flushed, skin hot and dry, pulse 122, tongue heavily coated, brown and dry in the centre, edges whitish, tip red, abdomen full and tympanitic. bowels freely purged by the oil.

Treatment. ℞. S. Quinine ℈j.; Tinct. Opii, ℥ ss.; Aqua Minth. Pip. ℥ ss. M. S. Take all at once. To have also Liquor Ammoniæ Acetat, ℥ iv. A tablespoonful every two hours.

Evening. Condition much the same. To have Sulphate Quinine, ℥ ss.; Blue Mass, grs. viii.; Pulv. Opii, grs.ii. M. Take at one dose.

April 5th. Looks better, rested well, skin moist but hot, pulse reduced to 104, tongue moist, abdomen still tympanitic, but not tender, bowels open, slight roaring, but no pain in head.

Treatment. Continue the Acetat Ammon. during the morning. Evening, to have Sulph. Quinine, ℥ ss.; Pulv. Opii, grs. ii. M. Take at one dose.

April 6th. Says she feels "very well;" looks better, face still flushed, skin moist and cooler, sweated profusely during the night, pulse increased to 120, tongue still brown in centre but moist, bowels open, less tympanites. ℞. S. Quinine, ℥ ss.; Pulv. Opii, gr. i. M. Take at once.

Evening. Found her bathed in perspiration, pulse reduced to 100, tongue moist and cleaning, no headache, slight roaring in head, bowels open, slight tenderness and tympanites. ℞. S. Quinine, grs. xxv.; Pulv. Opii, grs. ii. M. Take at once.

April 7th. Much better, rested well, sweated freely, skin cool and moist, pulse 90, full and soft, less tympanites and no tenderness, tongue more natural. To have 10 grains Quinine. Continue Acetat. Ammonia.

April 8th. Greatly improved, fever completely broken, pulse 65, no tympanites, tongue moist and white. Complains more of the roaring in the head than when she took the large doses of Quinine. To have lemonade and no medicine. From this time she continued to improve, had no return of fever, and was discharged on the 16th.

Notes by Mr. McKnight.

REMARKS.—Here we see a severe case of continued fever, (*typhus or typhoid*) in which the abortive treatment was commenced in good time, and the fever cut short in *four days*; in which time she took 145 grains of Quinine, 7 grains of opium and ℥ sr. Tinct Opii. It will be observed that there was less *quininism* whilst taking the large doses than the small, a fact well known to the physicians of New Orleans.

CASE II. Catherine Murphy, aged 18, Irish emigrant, came on the ship "Samuel Lawrence," with case No. 1; felt unwell from the time she landed here, was dejected in spirits, but did not say when she was attacked with fever. Entered the Hospital on the 4th of April.

Symptoms. Pains in the head, back and limbs, skin hot, face flushed, bowels loose, thirst, pulse 120, tongue thickly coated, edges red, stomach irritable, abdomen tympanitic but not tender on pressure.

Treatment. To have a dose of Castor Oil, and after it operates, the following: ℞. Sulph. Quinine, ʒ ss.; Mass Hydrarg., grs. viii; Sulph. Morphia, gr. ss. M. Make pills. Take all at once. She threw up the oil but retained the pills.

Second Day, April 5th. Feels no better, rested rather badly, pains continue, face still flushed, skin moist, pulse as before, bowels open, tongue rather dry, brown and thick, little sordes on the teeth, impatient and restless.

Treatment. To have Aqua Ammonia Acetatis during the day; at night to have the following: ℞. S. Quinine, ʒ ss.; Pulv. Opii grs. ii. M. Take at one dose.

Third Day. Feels a little better; skin moist and cooler, less headache, tongue more moist, some tympanites, pulse 115, stomach still irritable.

Treatment. Same as yesterday, Acetate of Ammonia through the day; Quinine ʒ ss., and Opium grs. ii at night.

Fourth Day. "Feels no better," face and eyes somewhat injected, expression listless, sweating pretty freely, pulse 110, tongue coated, thick and red on the edges, tympanites, *sudamina* over the abdomen and chest, has some cough, debility not great.

Treatment. Blisters to epigastrium and nucha. ℞. S. Quinine ℈ i., Sulph. Morph. gr. ss., M. Take at one dose.

Fifth Day. Feels better, slept well, still drowsy, bathed in perspiration, less pain in head and limbs, pulse 74.

Treatment. As the fever is now subdued, stop the quinine, but continue the acetate of ammonia, lemonade and light soup.

Sixth Day. Feels a great deal better, but still has slight uneasiness in head and limbs, pulse 85, skin cool and sweating, tongue moist and cleaning, no tympanites, bowels open but not loose, countenance more expressive and calm. Wants to sit up and says, "she would be entirely well now, if she could get plenty to eat."

Treatment. Linement for limbs, lemonade and soup.

Seventh Day. Rested well, pains entirely gone, has a good appetite, fairly *convalescent.* She continued to improve till the 14th April, when she was discharged from the hospital. After remaining out a few days, she had a *relapse* and returned to the hospital; but the fever yielded readily to treatment, and the girl was again discharged, *cured.*

Notes by Mr. Taney.

REMARKS.—Here again was a *severe* and obstinate case that would, in all probability, have run on for twenty or thirty days under ordinary treatment; but by the plan pursued it was cut short on the 6th day of treatment. She took 210 grains of Quinine, one of Sulph. Morphia, and 4 of Opium. She seems never to have complained of *quininism* at all.

CASE III. Mary Brien, aged 20, Irish emigrant, came on the same ship; entered the Hospital on the 29th March, been sick three days.

Symptoms. Face flushed, skin hot and dry, pain in the head and limbs, tongue furred and white, pulse 130, bowels costive.

Treatment. Cathartic enema immediately; then ℞. Sulph. Quinine, ℈j; Mass. Hydrarg. grs. xv; Sulph. Morphia gr. ss. M. Make pills. Take all at once. In six hours to have Ol. Ricini, ℥jss. At bedtime, to have the *quinine draught* of the Hospital, which is a scruple of quinine and half drachm Tinct. Opii, in mint water.

March 30th. Feels much better, bowels freely purged yesterday, complains of some pain in the epigastrium, slight headache, pulse 120.

Treatment. ℞. S. Quinine, ʒ ss.; Pulv. Opii, grs. ji. M. Take at once. Emplast. Cantharid. to nucha and epigast. At night, to have *Haust. Quinine.* Infus. ulmi.

March 31st. Learn that her stomach was quite irritable yesterday, vomited half an hour after taking the first dose of quinine and opium, but retained the draught at night. Seems better this morning, slept and sweated pretty well, pulse reduced to 105, bowels open, some nausea.

Treatment. ℞. Sulph. Quinine, ℈jiss.; Tinct. Opii, ℈j.; Mucilage, ℥ji. M. Give by enema. At night, to have Sulph. Quinine, ʒ ss., Pulv. Opii, grs. ii. M.

April 1st. Much better this morning, slept and sweated pretty well through the night; still sweating; still has some pain in the stomach, vomited this morning. Complains of deafness and roaring in the head, bowels open, pulse 100, tongue moist and less coated, thirst, headache greatly relieved.

Treatment. The quinine draught followed by Liquor Ammonia Acetat, a tablespoonful every two or three hours; lemonade freely.

April 2d. Much improved, rested well, skin cool and moist; no pain, less roaring in head, bowels freely open, pulse 75.

Treatment. No more quinine thought necessary, as the fever is completely broken. To have Liquor Ammonia Acetat through the day.

April 3d. Rested well and sweated freely; complains of nothing; is

fairly *convalescent.* Allowed soup. From this time she continued to improve and was discharged. A few days afterwards she had a return of fever, but of short duration. [*Noted by Dr. A. C. Robertson.*]

Remarks. Another severe case, in which the fever had proceeded *three days* before the treatment was commenced, yet it yielded on the fourth day. She took 190 grains of quinine.

Several other girls from the same ship came in at different periods, with severe attacks of continued fever. They were treated by me in the same way and with similar results. The fever was generally arrested by the fourth day of treatment. After a convalescence of from four to seven days most of them had a relapse; but, excepting one case, the second fever was mild and of short duration.

There were two fatal cases, in women advanced in years, one aged 51, the other 48. The former was treated on the ordinary plan, *without quinine*, and died of exhausting diarrhœa, on the tenth day of treatment. Her attack was not thought severe, on admission. The *post mortem* examination revealed considerable evidences of inflammation of the mucous membrane throughout the intestinal canal. The cœcum and two or three feet of the ilium presented a rosy hue, but the plaques of Peyer were *not prominent and not at all ulcerated*, as might have been expected after so much diarrhœa.

The second case was a very violent attack; entered the Hospital on the fourth day of sickness, and died on the morning of the 7th day. Unfortunately, none of the prescriptions in this case were properly carried out. She was ordered to be cupped freely and well purged before taking quinine; both very imperfectly done. She was again ordered a cathartic, to be followed by quinine and morphine. These prescriptions were *reversed.* The consequence was, she sank from congestion of the brain, evidently from want of timely and sufficient depletion. As opportunities do not often occur of making post mortem examinations in so early a stage of continued fever, we looked with much interest to the condition of Peyer's glands. The mucous membrane of the whole ileum was of a pink color, in parts arborescent. A few solitary glands were apparent; but the *Plaques of Peyer were hardly perceptible.* In the fatal case of a man from the same ship, who died in a more advanced stage of this fever, treated without quinine, the glands of Peyer were found *extensively ulcerated.* The case will be given presently. Before leaving the female wards I will give one more case that originated in the city. Treated by Dr. Robertson.

CASE IV. Ann Sawyer, aged 35, has lived in the city two years; was admitted on the 26th March; been sick 7 days. Ordered by the ward student, Ol. Ricini, ℥jss.

March 27th. First seen by Dr. R. *Symptoms*, hot and dry skin, small and frequent pulse, tongue heavily coated, bowels freely opened by the oil.

Treatment. ℞. Mass Hydrarg. grs. x; Sulph. Quinine, ℈j; Sulph. Morph. gr. ss. M. At one dose.

Evening. To have a scruple of Quinine and half a drachm of Tinct. Opii, in a little mint water. (This is the *Haustus Quiniæ* of the house.)

March 28th. Found her a good deal better, pulse 95, skin cool and moist, feels entirely easy, bowels open.

Treatment. ℞. Sulph. Quinine, ℈ji; Pulv. Opii. grs. ii. M. Make two powders. Take one morning and evening. Lemonade, half diet.

29th. Still better, pulse 90, tongue moist and cleaning, skin cool and moist, has slight uneasiness in bowels, which are rather loose.

Treatment. ℞.S. Quinine, grs. xv.; Pulv. Opii, grs. jss. M. In two powders; one, morning and night. Mistura Cretæ for diarrhœa.

30th. Cool and quiet, bowels easy, pulse 80, tongue nearly clean. She continued to improve from this date, and was discharged in a few days.

In the male wards of the Hospital, (my own,) I treated a number of emigrants who came over on the ship "Samuel Lawrence." They were taken sick at different periods after their arrival; some within a few days, others after twenty or thirty days. Some had gone to work in the city; others on the plantations. They all presented the same type of fever, but entered the Hospital in different stages: some recent, others advanced. All the cases which had not advanced beyond the third or fourth day were promptly cut short by the quinine and opium treatment. I will report two cases that came together, which will show both the efficacy of the treatment and the nature of the disease. These men had come over in the same ship, and had gone to work together ditching, on a plantation above the city. One was treated with quinine, the other without it. Reported by Mr. McKnight, House Student.

CASE V. Thomas Hare, a very large strong Irishman, aged 22, entered the Hospital April 24th; has been in this country about a month came over on board the vessel "Samuel Lawrence," went up the coast about 28 miles and labored as a ditcher; slept in a shanty in the woods, surrounded by swamp, was much exposed to the sun; has been

sick seven days. He speaks the vernacular dialect of Ireland, and but little English, but through an interpreter we gained from him the particulars of his case. No passage from bowels for several days, complains of great head-ache, skin hot and dry, tongue dry and brown in the middle, red at the tip, pains in bones and back, pulse 120 per minute; sudamina over chest and abdomen, tympanites of abdomen, is very drowsy. Dr. Fenner remarked, "that he was too far advanced for the abortive treatment, for he did not claim to cut the fever short after the third or fourth day of illness, that he only gave the quinine before any organic lesion had taken place."

Treatment. ℞. Liq. Ammoniæ Acetatis ʒ viij, a tablespoonful to be taken every two hours.

April 25th. Appearance the same, rested badly during the night, skin moist and hot, severe head-ache, tongue dry and brown, pulse 90 per minute, bowels open. Continued the same treatment.

April 26th. Worse, stupid and drowsy, rested very badly during the night, pulse 110 per minute, skin warm and moist, abdomen tympanitic, sudamina increased.

Treatment. ℞. Ammoniæ Carb. ℈ ij., Mucilage G. A. ʒ iv. Mix. A tablespoonful to be taken every two hours. Blister to nape of neck.

April 27th. Appearance somewhat better, not quite so stupid and drowsy, skin hot but moist, pulse the same, no head-ache, bowels not open. Continue the carbonate of ammonia; enema purg.

April 28th. Has fallen back into his drowsy comatose state, pays but little attention to questions, was quite delirious during the night, had to be confined to bed per force, passes his stools and urine in bed, involuntarily; eyes look heavy and dull, tongue dry, brown and cracked, sordes collected on his teeth, bowels open, tympanitic, sudamina enlarged, pulse rather weak, sweating.

Treatment. ℞. Ammonia Carb. ℈ ij., Mucilage G. A. ʒ iv. M. Take as before. Milk punch freely.

April 29th. Is worse, he continued to sink, lying in a stupor all the time, passed his stools and water in bed, delirious during the night, tympanites continuing, sudamina increasing and becoming inflamed, tongue dry, brown and cracked; unable to put it out, teeth covered with sordes. The same treatment was continued, enlarging the dose of ammonia to 10 grains every two hours, and taking strong milk punch freely.

He died on the 5th of May, about twelve o'clock at night, and was examined by Dr. Fenner, at nine o'clock next morning. I will leave the *post-mortem* appearance to be inserted by him.

AUTOPSY.

Brain. Vessels of the dura mater greatly engorged, serous effusion underneath; effusion under the arachnoid, substance of brain redish, considerable effusion in the ventricles.

Chest. Old pleuritic adhesions on left side, posterior portion of lower lobe engorged, probably from hypostasis.

Heart. Sound.

Stomach. Mucous membrane greatly engorged, amounting almost to ecchymosis in large curvature.

Intestines. Distended with gas, contained some healthy-looking fœces, duodenum and jejunum somewhat engorged, the latter stained with bile.

Ilium. Upper portion of pink color, in lower portion the plaques of Peyer were extensively ulcerated, in some parts so near through that the coats gave way under the pressure of water running through to wash them. The cœcum and colon were of slate color in patches.

Liver. Rather flabby; *Gall-bladder*, full; *Spleen*, Normal; *Kidneys*, the same.

Remark. I think this case had advanced beyond the *curable* stage when admitted.

Case VI. Thomas Fletcher, an Irishman, came over in the same vessel with the other, and was working in company with him, aged 40 years; entered the Hospital April 24th, has been sick four days; taken with a slight chill followed by a slight fever with aching in his bones for several days. Skin moist, pulse 100 per minute, tongue dry, slightly coated, bowels regular, abdomen tympanitic, tenderness over epigastrium.

Treatment. To be cupped freely over stomach, afterwards the following: ℞. Sulph. Quinine, grs. xxv., Mass. Hydr. grs. x., Sulph. Morphiæ gr. ss. M. Make pills, take at one dose.

April 25th. Appearance about the same, skin hot but moist, pulse 100, tongue moist and coated white, abdomen tympanitic, no tenderness over epigast., bowels open.

Treatment. Nothing ordered this morning. At 5 o'clock P. M., the student found him in a kind of stupor, skin cool, tongue moist, pulse weak but frequent, 112 per minute; and gave him some carbonate of ammonia.

April 26th. Much better, no pain, moist tongue, pulse 100.

Treatment. Ol. Ricini ℥ i. Enema purg. to be taken if oil does not operate.

April 27th. Still better, seems to be improving.

Treatment. ℞. Quinine Solution ʒ vi. Take a tablespoonful three or four times a day.

April 28th. Decidedly convalescent. Continue the small doses of quinine. He continued to take the solution of quinine, and was discharged on the 2d of May, perfectly well.

Remarks. It will be perceived that this patient took but one large dose of quinine, after which the excitement became so low as to demand a stimulant. His convalescence was rapid and he had no relapse, but it can hardly be doubted that if the case had gone without treatment as long as the previous did, it would have been difficult to cure. The two cases having been similarly exposed and attacked, were most probably the same disease.

Case VII. John Gunn, Irish, aged 16, arrived in the city 10th May, 1853, on board the ship Northampton, from Liverpool, out seven weeks. Says there were 300 emigrants on the ship and a good deal of sickness, eight or ten deaths on the voyage. John says he was sick with continued fever for ten days before his arrival here; staid one day at the house of a sister in the city. Entered Hospital on the 12th of May; House Surgeon prescribed a solution of the chlorate of potassa, and lemonade.

May 13th. At my morning visit I found him with hot skin, frequent pulse, dry tongue, great thirst, pains in the head and limbs, slight tympanitis, but no strong evidences of organic lesion.

Treatment. I remarked to the attending student and visitors that it was rather late for the quinine treatment, but as there appeared to be no serious organic lesion I would give it a trial, feeling assured it would do no harm if it failed to do good. Ordered—℞. Sulph. Quinine grs. xxv., Sulph. Morphia gr. ss., Mass. Hydrag. grs. x. M. Make pills, take all at once. Lemonade.

May 14th. Much better, had slept well and sweated freely; skin now cool and moist, pulse 80, less thirst, still has some pain in head.

Treatment. Ordered emplast. cantharid. to nucha. Solution chlorat. potass., a tablespoonful every hour or two. Lemonade.

May 15th. Entirely relieved, cool and quiet, sweating freely.

Treatment. Continue chlorat. potass. Wine whey.

May 16th. Convalescent, ordered two grains quinine to be taken three times a day for several days, to prevent relapse. He did not relapse and was discharged, well, on the 27th May.

Remarks. This was a mild case of ship fever, that would probably have recovered soon under any treatment, or even no medicine at all;

yet the action of the quinine and morphia was certainly very handsome. A short time previously, I treated a young man who had come over on the "Samuel Lawrence," and was here two or three weeks before he was taken sick. His attack was pretty severe, but the fever was promptly cut short by the quinine treatment. As so many of his companions had relapsed after the fever was cut short by quinine, I resolved to continue small doses with a view to prevent relapse. He convalesced well, for five or six days, when he was thought to be out of danger and the quinine was stopped. The very next day a relapse occurred, but the fever was again cut short by a large dose of quinine, and did not return.

CASE VIII. *August* 23, 1852. Was called to see a negro girl, aged about 12 years, very small and thin, property of Mr. A. F., at the house of Mr. P., on the Gentilly Road, back of the city. Was told that she had been unwell for several days, with irregular intermittent fever; had taken some small doses of quinine and blue mass, but had complained of such curious head symptoms that the remedy was discontinued by the lady, Mrs. P.

At 6 o'clock, P. M., I saw her for the first time. I found her with headache, slight delirium, dimness of vision, restless, pain and tenderness in the abdomen, gurgling in the right iliac region, skin dry and somewhat warm on the body and head; pulse frequent and weak, (about 110 to the minute,) tongue rather white, considerable thirst, slight nausea, bowels costive and slightly tympanitic. She had taken 10 grains of calomel in the morning, and afterwards oil, but had no stool.

Treatment. Ordered enema of Infus. Senna with Salts. After the bowels are freely purged, give the following: ℞. Sulph. Quinine, grs. x; Tinct. Opii, gutt. xv.; Aqua Font, ℥j. M. S. Take all at one dose. Apply a warm poultice to abdomen, hot mustard foot bath; sage tea for drink.

August 24*th.* Found patient much better; had three bilious stools from the enema, and then took the quinine; soon broke into a profuse sweat, and rested well all night. This morning is cool and quiet, pulse 90, no pain in head or belly, but little thirst, mind clear, etc.

Treatment. Repeat enema of Senna and Salts. After the bowels are moved, to have the same dose of quinine and laudanum, morning and evening.

August 25*th.* Found patient entirely free from fever, and begging for something to eat. Mrs. P. informed me that her bowels acted freely, without resorting to the enema, she had three dark bilious stools; took

both doses of the quinine as directed; each of which produced copious perspiration and good sleep. Pronounced *convalescent*. Ordered 5 grs. quinine, morning and evening; chicken broth; 5 grains quinine tomorrow morning. Dismissed. The fever was cut short and never returned.

I might report many more cases, but it would extend this paper to too great length. Suffice to say, I have as yet had no case of continued fever, either in the Hospital or out of it, which, if called to within the first two or three days, I have not been able to cut short by the quinine treatment, when properly applied. I have applied it in more advanced stages, when I hardly expected to cut short the disease. In these cases the febrile excitement was reduced, and distressing symptoms were greatly alleviated, but the disease continued its course to convalescence or death. Last winter, a young man came into one of my wards, in the seventh or eighth day of typhoid fever. He had hot and dry skin, frequent pulse, pains in the head, back and limbs, abdomen tympanitic, etc. I remarked at the time to the students in attendance, that the disease was too far advanced to be cut short by quinine and opium, but that we would try it. All painful symptoms were relieved, the skin became cool and the pulse reduced to 80; yet I could see that the man was not convalescent. He died, and we found extensive ulceration in the ilium.

In October last, I visited my friends in Clinton, Mississippi, where typhoid fever had been prevailing for the last two or three years. I conversed freely with the highly intelligent physicians of the place, Drs. Banks and Stokes, who were altogether skeptical as to the possibility of cutting short typhoid fever with quinine. They thought they had given it a *fair trial*, when they carried it to the extent of 36 to 40 grains a day; but I found on inquiry that they hardly ever gave more than 12 or 15 grains as a dose; and more generally 5 or 10 grain doses, at intervals of two hours. I was allowed to prescribe for some eight or ten cases of fever, in the early stages; cases which were said to resemble the early stage of typhoid fever, but they could not pronounce a *definite diagnosis*. I found no difficulty in arresting them with *my doses* of quinine and opium; but the doubt still remains whether the cases really were typhoid fever or not. Dr. Stokes informed me that he has since succeeded with my doses in cutting short one or two cases in a very astonishing manner; but Dr. Ed. Banks still remains skeptical. I must again beg my friends in the interior to give my treatment a *fair trial* before they condemn it. Let me now refer to the paper of Dr. Macgibbon, which appears in this number of the journal.

k REMARKS ON DR. MACGIBBON'S CASES.

Dr. Macgibbon has extended me the courtesy to let me examine his paper in this number of the Journal, *On the Abortive Treatment of Typhoid Fever*, that I may give it such notice as I think proper on the present occasion. I thank him for the kindness. Knowing that he differs entirely with me in my views of fever and its treatment, I very naturally had some curiosity to see the results of an opponent's observation. I have read his reports of twelve cases, and am happy to find that they present no stronger facts than they do in opposition to the views I am advocating. I am willing to submit to the impartial reader whether the testimony these cases afford is not more *in favor* than *opposed* to my positions. I must say, I hardly hoped for so much support from an opponent. He has displayed but little skill in the application of my favorite remedies; yet the results were highly favorable. Although the writer is still sceptical, I think he shows that *continued fever can be promptly arrested by quinine and opium.* If *relapse sometimes* occurs, it is but what has been observed under any plan of treatment in which the duration of the fever was short, and less frequent under this than any other yet tried. It will not be forgotten that this tendency to relapse was observed so often in London, that Dr. Jenner has made of it a *distinct type* which he calls "*relapsing fever.*" When once we have learned how to arrest, with certainty, the progress of typhus and typhoid fevers, the discovery of a method to prevent relapses will be a much easier task. Dr. Macgibbon carps at my remark that there is quite a revolution going on at this time in the pathology and therapeutics of fever, and taunts me for claiming support from the cases of Dr. McEvers, which he thinks, afford no support whatever to my views; Dr. Macgibbon misconceives me altogether. I did not refer to Dr. McEvers' cases to illustrate the *Abortive Treatment*, but to show the happy effects of quinine *even in the advanced stages* of continued fever, which was something beyond my experience. I stated at the time, that Drs. D[illegible]das and McEvers appeared not to have attempted the *abortive treatment* of fever, yet that they should be considered benefactors of mankind if they proved that continued fevers might be cured by quinine after they were fully formed and were in advanced stages. His comments upon Dr. McEvers' cases may pass for *what they are worth*, which will, most probably, be considered *very little.* For my own part, he may think as he pleases of my views of fever, if he will only accompany his reflections with such *facts* as he has presented in the cases now published. I will now briefly recapitulate the results of Dr. Macgibbons' cases.

Case 1. An imperfect trial, but with satisfactory result.

Case 2. Result entirely satisfactory.

Case 3. Promptly cut short.

Case 4. The fever reduced by the quinine and opium; afterwards pneumonia appeared, which is not uncommon under any plan of treatment.

Case 5. Fever cut short, and did not return although the remedy was discontinued.

Case 6. At first, only took 5 grains quinine with half grain morphine, night and morning; then 7½ grains, and then 10 grains quinine; when the fever disappeared and the patient discharged. Exposed himself, got wet, and was attacked with bronchitis. The quinine draught twice repeated was found insufficient to arrest this, and was abandoned.

Case 7. An old broken down constitution, that probably would have sunk under any treatment. Died.

Case 8. Sick ten days before quinine was prescribed; probably had slight pneumonia or a strong tendency to it from the first. Dr. M. says he had no cough when he first saw him, but the student who saw him first must have found cough, or he would hardly have prescribed the syrup of squills. At all events the fever was entirely subdued by the fourth day of treatment, when pneumonia appeared openly, and was treated without quinine. Recovered.

Case 9. Boy 12 years of age. Attack severe, and contracted in the Hospital. Quinine promptly applied and with happy effect till the fourth day, when the pulse was down to 68, skin moist and the fever apparently subdued. It then commenced rising again, and the quinine was stopped, (improperly I think.) Under the Acetat. Ammoniæ, however, it soon declined again, and four days afterwards, he appeared to be convalescent again; when the fever again ran as high as ever. It soon went down and he convalesced. This case was not permanently cut short by the quinine, but the symptoms were greatly alleviated. If it had been continued longer, together with the Acetat. Ammoniæ, the case might have been shortened. There was nothing to contra-indicate it.

Case 10. Boy, aged 15. Treatment commenced on the fourth day of sickness. Took 40 grains a day, with a drachm of laudanum. In two days the fever was promptly relieved, and remained off seven days, when a relapse occurred. The quinine was again boldly tried, to the extent of two doses; but as it did not promptly subdue the fever, it was discontinued. The patient was put on the Acetate of Ammonia and soon recovered.

Case 11. A very severe case; probably sick six or seven days before the treatment was commenced. The quinine was boldly pushed, but without cutting short the fever. When it was discontinued, the fever rose higher, but finally subsided under the Acetate of Ammonia. If this had been used with the quinine, and a blister applied to the nape of the neck, the case would probably have yielded earlier.

Case 12. Was promptly cut short by the quinine, though the Doctor is evidently quite loth to admit the fact.

Such is the brief summary of Dr. Macgibbon's cases. Let the reader judge of their import. If all who doubt the efficacy of my treatment would give it as fair a trial as Dr. Macgibbon has done, I should be satisfied.

In the December No. 1852, of *The Stethoscope*, a valuable medical journal, published at Richmond, Virginia, may be found an exceedingly interesting paper on "*The uses and effects of Sulphate of Quinine*," by James McCaw, M. D., which was presented to the Chirurgical Society of Richmond, on the 26th November, 1852, and gave rise to considerable discussion. The author of this paper gives the results of his experience with the remedy, during a period of *nine years*, and seems to have been fully acquainted with its wonderful remedial powers. I regret that the length of this paper prevents my giving the substance of Dr. McCaw's remarks. I hope it will be extensively circulated, though I fear it may be passed over by the medical journals on account of its being so far in advance of the opinions of the day. Dr. McCaw fully sustains me in my views of fever and its abortive treatment.

In the March No. 1853, of the same journal, may be found an interesting paper "*On Large Doses of Quinine as an Abortive Remedy in Typhoid Continued Fever*," by Dr. Otis F. Manson, of North Carolina, in which the views I advocate are fully sustained. Dr. Manson says, that since 1843 his practice has been to cut short fevers in the early stages, by liberal doses of quinine. He does not claim the credit of introducing the use of quinine "as a sedative febrifuge," but he *does* claim to be "the first anywhere, as far as his information extends, who successfully employed it as an abortive remedy in continued fever." He says, his views and practice were violently opposed at the time by his professional brethren and others, "but now, (he says,) they talk very learnedly of the abortive and sedative action of quinine, and have forgotten the name of him from the lips of whom they first heard it proclaimed. They have 'stolen our thunder,' and we shall *not* forget them." Perhaps the doctor's "thunder" was not loud enough to be heard at a great distance. Perhaps if he would examine more exten-

sively into the history of quinine, that the "thunder" about its wonderful powers, as a febrifuge, did not belong *exclusively to himself*, and, therefore, he ought not to complain of the *theft*. If he will examine the second volume of the New Orleans Medical and Surgical Journal, 1846, the same year he published his first paper, he will find an essay, by Prof. Thos. D. Mitchell, of Transylvania University, in which the remedial powers of quinine in typhoid fever are as highly extolled as he had done himself. At all events I am very glad to have the aid of Dr. Manson in support of the abortive treatment of continued fever, and hope he will continue to let us hear his "thunder" in its behalf.

It is but justice to say that Professor Mitchell was one of the first, and has continued to be one of the strongest advocates of the abortive treatment of all fevers, by quinine. (See his writings in the Medical Journals, and his late work on Therapeutics.)

I am authorized by Dr. McCormick, Assistant Surgeon, U. S. A., stationed in this city, to say that for years past it has been his uniform practice to cut short continued fever, (*typhus and typhoid*,) in the *early stages*, by large doses of quinine and opium, and that he has but seldom failed in the effort when called on sufficiently early. Dr. McCormick has been a visiting physician to the Charity Hospital and is one of the visiting surgeons at this time. His writings published in this journal in 1846 bear strong testimony to the abortive power of quinine both in fevers and inflammations.

I do not know that I could offer a better *finale* to this paper than the following communication from a physician of talent and extensive experience, who has retired from public practice, but is continually called in consultation by the younger physicians of his neighborhood. He is now a wealthy tobacco planter, in Kentucky, but still feels an interest in the profession. I trust his testimony will not be repudiated on account of his retirement and the ease and affluence with which he has the good fortune to be surrounded in the evening of life. I have no personal acquaintance with Dr. Tuck, but having heard, for more than ten years past, of his bold and successful use of quinine, when I contemplated the execution of this paper I took the liberty of addressing him by letter, and requesting him to give me the results of his experience. The following is his reply:—

LAFAYETTE, Christian County, Ky., May 9, 1853.

Dr. Fenner:

MY DEAR SIR—Your polite favor of the 4th April reached me in due time, by Mr. Lancaster, and would have been answered before this, but

for pressing engagements. And even at this time, not having my mind professionally turned, I am unable to answer you as I would wish, but think I will do so in the course of the summer. * * * *

On the 20th of October, 1829, I ventured on the use of quinine, whilst the system was under considerable febrile excitement, in three grain doses. The case was bilious remitting fever, had been obstinate, but yielded quickly to the quinine. The unauthorized treatment made a strong impression on me, and is as distinctly recollected as if it were yesterday. From this period I continued to use it in similar cases, after preparing the system by an emetic or purgative, in increasing quantities, (though not exceeding, as well as I can recollect, 20 grains during the twenty-four hours,) until 1833 or 1834, I am not positive which, but certainly one of the two years. The ordinary antiphlogistic adjuvants were used in the treatment, as circumstances seemed to require, and the results, compared with my former practice, were highly satisfactory.

In 1833 or 1834, I am not positive which, I was attacked severely with fever, most probably bilious remitting fever, which I managed differently from any preceding case. On my own person I had the right of innovation. The symptoms were alarming, the head extremely pained, and altogether, they were exactly such as ushered in attacks that had lasted me from four to nine weeks under a different treatment. These attacks I suffered in Virginia, where I practised physic, and were at least five in number—more, including some attacks of pleurisy. The treatment in this attack, (1833 or 1834,) was exclusively quinine, except one bleeding for the head, bathing it in cold water, and occasionally drinking a soda-powder to alleviate thirst. I took the quinine in the height of the fever, and continued it three days closely; but took less on the third day, as my fever was nearly off. On the fourth I could sit up and eat liberally; and this was an end of the case. From this time forward, I lessened the preparing the system for the quinine until 1835, when I pretty well abandoned it entirely. During this year, (I speak from memory as to the year,) my family and the whole country near me suffered severely with fever of the type spoken of. Many of the attacks were very severe, and in one case in my family I gave 40 grains quinine in the first twelve hours; much more than I had ever given before, and continued it the next thirty-six hours, but in less quantity, without any other means at all. The third day the fever was broken, and on the fourth the fellow was in the field, able to work moderately. Under the old practice, I should have drawn at least a quart of blood the first day, but I am fully satisfied that he did better without it. Many of the cases commenced without rigors or chills, but

with severe pain in the head and limbs. These also yielded promptly to the quinine influence.

From this period, and perhaps a little earlier, I extended the application of quinine to the treatment of other fevers, and with so much success, that I am compelled to believe it is worth more than the aggregated value of all other medicines now known in the treatment of non-infectious epidemic diseases. Even in infectious diseases, or pure symtomatic fever its value is undoubted at some stages, but the causes of such fevers, cannot be expected to be thrown off by the aid which the quinine gives the system in its eliminating powers. Whenever the virus of the infectious diseases, such as small-pox or measles, becomes chemically mixed up with the elements of the system so as to impair or upset their healthy relations, nothing can restore health until the system relieves itself by throwing off its variolous element. When this element is fully thrown off, small-pox cannot return; but when partially, it may; but in the form of a varioloid. And that there is such an element in the human body, so far as I can reason upon the subject, I am unable to doubt. That an invisible speck of variolous matter should be able to reproduce itself or multiply itself by detention ten thousand times or more, as it is said to do in small-pox, seems unreasonable and opposed to the well established laws of nature. But to suppose that speck, as well as many other particles of matter, can chemically change or disturb the properties of another body, is not only reasonable and probable, but is certainly the case as regards inanimate bodies. In measles I have satisfied myself fully that the virus cannot be expelled, at least by quinine, after febrile action has been set up. But when the eruption is out, then it can frequently be used with benefit; particularly when there is alarming lesion of any vital organ. Two cases of this disease, which came under my care, have much interest in them. Small-pox, judging from analogy, would resist elimination as much or more than measles.

In the management of all fevers which come under my notice, particularly in my own family, I rely upon quinine as the remedial agent. It is necessary frequently to use auxiliaries to aid it in its sustaining or febrifuge effects, but without it, I would want confidence in the result, and with it, would feel little doubt. In pneumonia and pleurisy I sometimes bleed and blister, but very rarely. I sometimes would do the same in severe rheumatism. Carbonate of potash or soda I almost invariably use as a common drink, and when much preferred, have it accidulated. Any mild and pleasant drink, none better than water, are allowed and encouraged with a view to increasing the urinary secretions, etc. I

generally give the quinine, in from five to ten grain doses, as soon as reaction takes place, every four or five hours, sometimes not so often, until the system yields to its particular influence, which can be always known by a buzzing or ringing in the ears, or by a partial deafness. This state of the head I wish to produce early, if it can be done by doses which will not prostrate the stomach, and keep it up by smaller or larger doses, as the system may require, until there is a solution of the fever. And then it should be kept up, by smaller quantities, at least three days later, to make the recovery certain. I sometimes keep up its influence longer when the attack has been severe. The stomach and bowels I greatly prefer to be quiet; and when they are otherwise, I resort to such means as seem most likely to quiet them. Much inquietude of either vastly interferes with the efficacy of the quinine, and unless arrested will defeat the curative powers altogether. When reaction fails to take place early, I then use the quinine, (not in large doses,) in the state of depression, combined with such drinks as contribute to reaction. No symptom that I have any knowledge of, except inactivity of the stomach to bear it, contra-indicates its use in the early stage of epidemic fevers. And when the stomach is unable to bear it, it should be used by injections. In coma and delirium I have used it with the happiest results. In pneumonia and dysentery I think it as valuable, perhaps more so than in any other disease. These diseases, considering the number of cases, particularly the latter, when it has been an epidemic, have generally been more fatal than other fevers in the country, have, if my memory is not at fault, yielded to the quinine treatment and its auxiliaries. Whenever a fever commences with such violence as to produce quickly lesion of a vital organ, the lungs or stomach for example, the quinine then, although highly necessary in the case, cannot be expected to attain its usual effects. In such cases a new source of disease is set up, (at first a consequence of the miasmatic cause,) which will continue to harrass and disturb the system until the health of the organ is restored, or the radiating point of disease is removed. This consequence, which rarely follows if quinine is used in due time and in sufficient quantities, has contributed much to bring the medicine into bad favor with many medical men. In other words, when it ceases to be a specific under an unskillful administration, they fall back on their old staff, mercury, whose effects in no disease whatever can be anticipated or controlled, with any degree of certainty, when given as formerly. When I use it, it is in minute doses, and then with reluctance. Sometimes, in chronic bowel diseases of children, I can find no substitute for it.

You inquire of me what I think of the use of quinine in typhoid fever? I am yet a perfect unbeliever in the existence of such a disease. And if I had been a believer before I read Dr. Bartlett's work on that disease, he would have changed my opinion. His book, I think, is a perfect failure in proving the non-identity of typhoid fever with typhus. Typhoid and typhus are as nearly allied to each other as mild and malignant scarlatina; and in my opinion, should always be treated with a view to this relationship. Whatever produces one, I have no doubt produces the other. The diseases here called typhoid fever, yield, so far as my observation goes, readily to the treatment I have mentioned. I have had them frequently in my own family, and have had no difficulty in getting the cases through from four to seven days. Seven days is a very long time to have a case of fever on hand.

Local inflammations, organic lesions and deranged secretions are the most common symptoms attended to by the physician, as if they constituted the disease itself. I view them all as sequelæ growing out of the constitutional disturbance, and presenting no contra-indication to the use of quinine. The first indication that should be attended to is the expulsion of the poison or miasmatic virus, and to effect this we have to trust to the eliminating powers of the system, giving it at the same time all the aid in our power, by keeping the stomach, the bowels, the lungs, etc., etc., in as sound and as quiet a state as possible. Any great distress of any of the organs impairs, in my opinion, the eliminating ability of the system, and should be removed as quickly as possible. When the poison is eliminated, and no serious injury done to any particular organ, the system will soon right itself and health will be restored.

Whatever will aid the system in throwing from its fluids or solids (which ever you may prefer; I prefer the former;) poisonous matter, most effectually constitute the proper remedies for the disease. When the lungs are kept free, the pores of the skin open, and the kidneys performing their functions well, the great outlets for elimination, I always expect my patient to recover quickly.

I have already written much more than I expected when I commenced, but finding every branch as well as the trunk of my subject endless, it is necessary to close. I will remark, however, that I will be glad to hear from you at any time, and upon any subject you may wish. It is quite likely that I might be able to put into your possession some valuable medical facts, which in my hands are worth but little.

Yours, with much regard and respect,

D. G. TUCK.

I will here close this lengthy paper. It was begun with reluctance, but I thought it was called for to counteract unjust criticism, and to show that many erroneous impressions and ungrounded fears still prevail in regard to the effects of a remedy that may justly be called the *magnum donum Dei* to the inhabitants of the most fertile portions of the earth. If, in this paper I have shown that it is unjust to *condemn without fair trial*, a remedy that is recommended to the profession on respectable authority; that the prejudice against this remedy, *as recommended*, arises from ignorance of its wonderful remedial powers when thus administered, and false inference from its known effects when given in a different manner—that the remedial power now claimed for the sulphate of quinine, in continued fevers, is not more incredible or astounding than its now admitted power in paroxysmal fevers would have been to our predecessors a quarter of a century ago; if, finally, I have brought forward testimony and good reasoning to show that the endemic fevers of the Southern and Western States have undergone continual changes from the first settlement of the country, that the various types are closely allied to each other, and are all curable by the same remedy judiciously applied; I have accomplished my purpose. I trust I have, at least, awakened inquiry, and doubt not that it will lead to beneficial results.

I beg to be understood, that I do not deny that typhoid fever may be treated successfully and safely on a mild, expectant plan, with very little medicine; but I do consider it *most desirable* to cut short the disease in its incipient stage, and thus prevent a long and tedious illness; which I think can be done by the plan I advocate. It is useless to discuss its theory any further; let us bring to it at once the test of *experience*.

Part Second.

EXCERPTA.

I.—*Contributions to the Pathology of the Heart.*

BY DR. M'DOWELL.

[Concluded from last No.]

CASE III.

Timothy Houston, aged 61, was admitted into the Whitworth hospital, under my care, January 6, 1852. He had labored for several years under the symptoms of heart disease, but dropsical symptoms had only lately appeared. A year previously he had incomplete paralysis of the left arm and leg, for which he was in the Whitworth hospital, in Dr. Gordon's ward, and which was removed by treatment. Increasing dyspnœa and anasarca were of late the symptoms of which he chiefly complained.

On admission he was universally dropsical. Ascites existed to a slight degree only. The lungs were congested, and cough was very troublesome. Breathlessness and dyspnœa were constantly present, but at times there were paroxysms of difficult breathing, with palpitations, which rendered the horizontal position insupportable.

The pulse was small in volume and regular. The heart was manifestly enlarged, and that too to a considerable degree. Its impulse was quite out of proportion to the smallness of the pulse. The sounds of the heart had a muffled character, and were difficult to analyze; but no abnormal bruit could at any time be detected. The jugular veins were turgid. The face was invariably pale.

Soon after his admission the patient was attacked with hemoptysis. The blood was florid, and was expectorated in considerable quantity, mixed with thin, frothy mucus. The signs which indicated pulmonary apoplexy were these: A sharp crepitating rale over the lower lobes of both lungs posteriorly, with absolute dulness on percussion, but without the bronchophony or bron-

cophonia which would accompany the same degree of loss of sonoriety, in a case of pneumonic consolidation. Moderate venesection, at this crisis, was well borne, and gave great relief. The pulmonary hemorrhage continued for a fortnight, and then gradually ceased. But whilst the crepitus disappeared, the healthy resonance of the chest was not restored.

From this period the paroxysmal attacks of dyspnœa became more periodic, and assumed all the features of distressing angina. This painful complication continued to the close of life. The lower extremities had, by this time, become so distended, that a few punctures were made with a small needle: a copious flow of serum was the result, by which the anasarcous swellings were greatly diminished. Towards the close of life there was less suffering than might have been anticipated, for a drowsy condition became manifested, accompanied by decided imbecility of mind. He now almost ceased to suffer, and dosed away nearly all his time, either in an easy chair, or propped up sitting in bed. It was surprising how long life was protracted in this condition. For upwards of three weeks this state of torpor continued, when complete coma occurred, followed, after a few days, by death.

Post Mortem Examination. The lungs were rather small in bulk, although they were emphysematous. Their lower lobes had been the seat of pulmonary apoplexy. On incising them, several large, dark colored masses appeared, which were evidently old coagula. The intervening portions of pulmonary tissue were somewhat condensed, whilst the remainder of the lungs was essentially healthy.

The heart was very much enlarged; it extended greatly beyond its normal limits. When washed out and separated from all its connexions, it still weighed twenty-eight ounces. In form it was not much altered, except that its apex was more rounded than usual. There was hypertrophy, with excessive enlargement of the ventricles, especially of the left. The muscular tissue had its natural firmness. The valves were perfectly free from disease, and the most rigid examination failed to detect the slightest imperfection in them.* The carneæ columnæ were not hypertrophied, as they are generally observed to be in cases where regurgitation has been allowed. The aorta, throughout almost its entire extent, was brittle, from extensive atheromatous depositions, and its elasticity, from this cause, was sensibly diminished. There were no evidences of recent inflammation of the aorta.

CASE IV.

Very similar to the preceding was the case of a man named John Clarke, aged 60, who was under observation for a short time in the latter end of Aug-

*The valves were severally tested as to their adequacy in the following manner: The pipe from a water cistern was introduced into the aorta and secured to it by a ligature. On allowing a full stream of water to flow into the artery, it was found that none of the fluid passed the sigmoid valves. Their competency to prevent regurgitation was therefore manifest. To test the mitral valve, the aortic sigmoid flaps were broken down and the aorta secured on the pipe as before; the water was now allowed to rush into the left ventricle, but though the mitral flaps were floated up and even made to bulge upwards, owing to the pressure to which they were subjected, yet none of the fluid passed into the left auricle. The mitral valve was thus shown to be perfect. The integrity of the valves on the right side was similarly tested and with the same result.

This method, which, with reference to the aortic valves, the writer first had seen adopted by Dr. Corrigan, is so easy of application, that it may be employed whenever it isdesirable to determine with certainty the adequacy of the valvular apparatus of the heart.

ust, 1851. He was admitted into the Whitworth hospital, laboring under ascites and anasarca, accompanied by extreme difficulty of breathing. His other symptoms were as follows: His face was bloated and congested; there was much dyspnœa, with the signs of œdema of the lungs; the pulse was small, weak, but regular. The emphysematous condition of the lungs interfered somewhat with auscultation of the heart, but it was satisfactorily ascertained, that although the impulse of the heart was strong, yet no abnormal sound existed. The urine was highly albuminous, with a diminished specific gravity. He died soon after coming under observation.

Post Mortem Examination. Some clear serum was found in the pleural cavities; the lungs were congested and emphysematous; the heart was enormously enlarged; externally it presented the appearances which are generally impressed on the organ by aortic patency; there were the same elongation and increased development of the left ventricle. Impressed with this idea, the aortic valves were tested in the manner described in the last case, but no regurgitation was allowed. On opening the heart, not only the aortic, but all the valves were found to be perfectly normal. The enlargement of the heart was chiefly due to dilatation with hypertrophy. The left ventricle was principally affected. The walls of this cavity were much thicker than natural, but the size of the cavity was out of proportion even to the thickened walls; dilatation, therefore predominated. The right ventricle presented the same changes, but in a slighter degree. The tissue of the heart was of healthy consistence. The aorta was extensively diseased; its interior presented almost universally the yellow discoloration produced by atheromatous depositions in the subserous tissue. In some places the inner coat was replaced by large patches of bone. In this case, as in the former one, fat was deposited in considerable quantities, not only around internal organs, but also in the subcutaneous tissue. The kidneys were greatly enlarged, and smooth externally, whilst a section exhibited the "fatty degeneration" in an advanced stage of development.

These cases very clearly illustrate the influence of dilatation of the heart, as a direct source of obstruction to the circulation, since in both of them the signs of obstruction were very fully developed, and dilatation of the ventricles was the only lesion found.

Cases I. and II. were less conclusive on this point, as in them there was softening in addition to dilatation.

Dilatation of any of the cavities of the heart implies that the amount of pressure exercised on the inner surface of such cavity must have been relatively greater than the power of resistance of its walls. In cases I. and II., independently of any obstruction to the circulation, the muscular tissue was softened; the muscular power was consequently defective; the pressure from within, although not more than normal, overcame the resistance from without, and dilatation was the result. Many doubt that any considerable degree of enlargement of the heart can occur independently of valvular disease or of pulmonary obstruction. In the four cases which have now been detailed, however, it existed to a very marked degree, and in the form of dilatation with hypertrophy. In cases I. and II., as has been already stated, the enlargement of the heart depended on softening of its tissue. In cases III. and IV. no cause for enlargement was found within the heart itself, but extensive disease existed in the tunics of the aorta, which, it is more than probable, is a sufficient cause for cardiac hypertrophy in general.

How far this condition is to be recognized as capable of producing in all cases the fatal train of morbid changes which has just been described, the writer is unable to affirm, but as it seems almost certain that a direct connex-

ion existed between the pathological conditions and the symptoms recorded in cases III. and IV., a few observations on the subject may not be inappropriate.

The elasticity of the larger arterial trunks is, by modern physiologists at least, acknowledged to be an efficient auxiliary in carrying on the circulation of the blood. "The elastic arterial wall, stretched by the contraction of the heart, reacts with a power which approximates more closely to that by which it was dilated, according as the arterial tissue is more or less elastic. The arteries are thus made to contract upon their contained blood, and to drive it onwards, or from the heart, and backwards, or to the heart. Its course in the latter direction is speedily checked by the sudden and forcible closure of the aortic valves under the pressure of the regurgitating current. Therefore, the great mass of blood rushes onwards towards the capillary system, propelled first by the heart's impulse; and secondly, by the elastic reaction of the arterial walls."*

If the elasticity of the aorta, which is here stated to be effective in moving the blood be much impaired, as occurred in cases III. and IV., we may suppose that the heart is called upon to make unwonted exertions to overcome the vis inertiæ of the fluid lying in that vessel. An impediment to the circulation will thus be established, and hypertrophy and enlargement of the heart will result. As the obstruction from the cause assigned becomes greater, the muscular parieties become proportionally thicker. But at last hypertrophy finds its limit, whilst enlargement of the cavities is still progressive. Thus dilatation predominates, and the symptoms of obstructed circulation become fully developed. For when once established, excessive dilatation becomes a direct cause of obstruction. Though the dilated cavities contain much blood, they are able to receive or to expel but little, the heart becomes embarrassed and oppressed, especially when the circulation is hurried; it beats with increased rapidity to compensate for diminished power, and hence arise palpitations with irregularity of action, dyspnœa, hemoptysis, œdema and congestion of the lungs, dropsy, anasarca, and perhaps an interruption of the functions of the brain.

Thus it is, that an excessively dilated heart, by its inefficiency to unload its chambers, produces symptoms similar to those which are the result of obstructive valvular disease.

Since dilatation of the ventricles, uncomplicated by any other cardiac lesion, may produce the fatal results just mentioned, it will be readily admitted that dilatation may prove a serious complication in cases of pre-existing valvular disease.

Much variety is observed in the duration of different cases of valvular disease. Some run on to a fatal termination in a very short space of time; others, on the contrary, live for many years, suffering no doubt from various distressing symptoms, yet at intervals in the enjoyment of tolerably good health. The difference in the amount of valvular disease in any two cases will rarely suffice to explain this circumstance; for in the case soonest fatal, there may have been the least amount of valvular derangement. The accidental lesions to which the subjects of organic heart disease are so liable, such as pneumonia, bronchitis, or fresh endocardial inflammation, will no doubt account for the fatal issue at an early period in many instances. But the great difference in the duration of life in all cases will be more obviously and more easily explained, if it shall appear that in all organic diseases of the valves or apertures of the heart, the urgency of the symptoms is not so much proportional to the me-

* Todd and Bowman's Physiological Anatomy.

chanical derangement, as to the changes produced thereby in the capacity or the muscular development of the several cavities.

Again, we are familiar with the fact, that an identity of symptoms is often met with in the advanced stages of different valvular diseases. This apparent anomaly would be easily explained if it should appear that there is a liability for all forms of valvular disease to be ultimately complicated with the same structural changes of the muscular tissue.

Hypertrophy of the muscular parietes of the cavities of the heart is the inevitable result of mechanical obstruction, and owing to it, in such cases, a more equable circulation is maintained than could otherwise be hoped for. But with this there is also a tendency to enlargement of cavities, which, so long as it is accompanied by a proportional amount of hypertrophy, may not be productive of inconvenience. As soon, however, as the increased bulk of the muscular wall ceases to be proportional to the increased capacity of the cavity, in fact, whenever dilatation is predominant, the cavity enlarging, as it were, at the expense of its walls, then a great aggravation of symptoms is the result. Where the original disease has been valvular inadequacy, the signs of obstruction are superadded, and where the primary lesion has been essentially an obstructive one, the symptoms depending on obstruction all become exasperated.

The following cases are illustrative of these remarks:

CASE V.

Margaret Foy, aged 42, a servant, was admitted into the Whitworth hospital under my care, February 27. 1850. She had acute rheumatism four years ago, which affected all her joints, and from which she suffered seven weeks. Last Christmas she caught cold, and then first suffered from palpitation of the heart, and from difficulty of breathing, with severe cough. These symptoms becoming more aggravated, she was compelled to apply for hospital relief.

On admission she had much cough, and the breathlessness of cardiac disease. The least exertion brought on violent palpitation. There was dyspnœa, which was intermittent, usually coming on at night, after she had slept for a little. Sometimes the paroxysm would last all night. During its continuance she was invariably obliged to sit up in bed.

On examining the chest, the heart's action was felt over a large extent of surface, which, with increase of dulness, indicated considerable enlargement of the organ. The usual signs of aortic patency were fully developed. The larger arterial trunks throbbed violently, and their course was distinctly visible, as each vessel was thrown up out of its bed at every beat of the heart. The pulse was jerking and large. A distinct double bellows murmur was heard along the sternal region, but its greatest intensity was over the valves of the aorta, and opposite where that vessel makes its first curve. The diastolic portion of the murmur was the most intense. Aortic regurgitant disease, with considerable enlargement of the heart, was diagnosed.

Leeches were applied, from time to time over the region of the heart, and on one occasion six ounces of blood were abstracted from the arm with marked relief. Morphia and the tincture of lobelia, given at bed-time, materially diminished the severity of the paroxysms of dyspnœa. Ten days afterwards pneumonia attacked the apex of the right lung; it was subdued by appropriate treatment, but from this period the paroxysms of dyspnœa recurred more frequently and with greater urgency. They now assumed much of the character of angina.

March 15. Jaundice appeared and the sputa was tinged with blood. The feet now became anasarcous. Diuretics and diffusible stimulants were directed. For the next fortnight the symptoms underwent but little change, the

attacks of angina were occasionally very severe, but the lobelia seldom failed to procure relief. The anasarca increased.

29*th*. A severe paroxysm of dyspnœa occurred this morning, accompanied by profuse hemoptysis. Free cupping to the chest and diffusible stimulants.

30*th*. The hemorrhage still continues, but the dyspnœa is not quite so urgent.

April 2. Dyspnœa of the most urgent kind is present. The face is pale and the pulse failing. No treatment now afforded even the slightest relief; she continued to suffer intensely, and died after a painful struggle on the morning of the 3d.

Post Mortem Examination. Heart greatly enlarged; eccentric hypertrophy of the left ventricle. The capacity of this chamber was enormously increased; the chambers of the right side were also enlarged, and the right auriculo-ventricular opening dilated. The aorta was dilated at and above its origin, its lining membrane was opaque, yellowish, and roughened by fibrinous deposits. The sigmoid valves were of normal size, but thickened, and inadequate to close the mouth of the dilated vessel, as was proved by experiment. The mitral valve was perfect, but the serous membrane which covered it, as well as the endocardial lining of the left auricle, was of a yellowish color. The lungs presented a remarkable specimen of pulmonary apoplexy. Cirrhosis of the liver and granular degeneration of the kidney likewise existed.

CASE VI.

Michael Connor, aged 30, a porter, was admitted into the Whitworth hospital, under my care, December 12th, 1850. Three years previously this patient labored under rheumatic fever, for which he had been treated by Doctor Corrigan. He recovered perfectly, and remained well until the month of May, (seven months before admission) when the first symptoms of heart disease were observed. On admission, palpitation of the heart and breathlessness were present, whenever he exerted himself even slightly; in addition to which he complained of dyspnœa at night, which used to occur suddenly during sleep, and was paroxysmal. His feet were anasarcous.

The following physical signs existed at this period. There were extensive cardiac dulness, and a double bellows murmur over the base of the heart and along the sternum. Over the apex of the heart, which had passed considerably towards the left side, a single loud systolic bruit was evident.

In a fortnight this patient left the hospital much relieved.

He was re-admitted January 4th, with all his former distressing symptoms much aggravated. The dyspnœa was most urgent, and the fits of angina at night threatened, at each paroxysm, to terminate his existence. Mr. Hill, the clinical clerk, reporting his condition in one of these seizures, stated, that he found him laboring under the most intense dyspnœa, which had come on with extreme suddenness, bathed in perspiration, and breathing with the utmost difficulty. He was supported in a sitting posture, with his shoulders raised. The alæ nasi were dilated, the lips blue, the pulse weak, small and compressible. He complained of excessive pain in the sternal region, and of coldness of his feet and legs.

These paroxysms generally lasted each for three or four hours. The dropsical symptoms had increased, and the signs of pulmonary congestion were very evident.

January 7. Breathing very difficult, hands and feet cold, the pulse 132, weak and thready. Pressure on the epigastrium could not be borne. The lower extremities were much swollen, and fluctuation was distinct in the ab-

domen; the physical signs, as before enumerated, were unaltered. He died two days afterwards.

Post Mortem Examination. The heart was greatly enlarged; the right cavities were simply dilated. The left presented dilatation with hypertrophy, but dilatation was the predominant change. Tissue of the heart much softened, so as to be readily torn or broken down. Left auriculo-ventricular opening much dilated. The left auricle, as has been stated, was very capacious, besides which its lining membrane presented a well-marked yellow color, such as generally indicates that regurgitation has been freely allowed. The mitral valve was healthy. The aortic valves were thickened and shortened, so as to have been altogether incapable of meeting. Aortitis existed to an extraordinary degree; the lining membrane of the aorta was swollen, and presented a villous appearance, with the color of scarlet cloth; it was likewise soft and pulpy to the touch.

In the lungs blood was found freely extravasated, and the vessels were greatly gorged. The liver presented the "nutmeg" appearance.

CASE VII.

John King, aged 30, was admitted into the Whitworth hospital under my care June 13, 1852. He had been complaining five or six months of vague feelings of ill health, but had no symptoms to lead him to imagine that he had disease of the heart, until very lately, when his feet became swelled and his breathing difficult.

On admission, there were presented, general anasarca, slight ascites, face bloated, breathing difficult, general bronchitis, congestion of the lower lungs; the liver was enlarged and projected three fingers' breadth below the ribs; the pulse thrilling.

The heart beat over a large surface; cardiac region dull, with loud double bruit.

Local depletion, diuretics, etc., were prescribed with advantage, but the dropsical symptoms could not be got rid of. July 20 he began to spit up blood. A frightful paroxysm of dyspnœa occurred that night, which obliged him to stand for hours at an open window.

21*st.* The dyspnœa during the night was extreme, and occurred in paroxysms. He sank rapidly, and died that night in the course of a violent convulsion.

Post Mortem Examination. The heart was greatly enlarged; weight 25 ounces; the aorta dilated; the sigmoid valves thickened, and their pliancy diminished; but their inadequacy was plainly owing to dilatation of the vessel. There were patches of lymph in the left auricle, and also in the left ventricle. The latter cavity was much enlarged, and its walls hypertrophied. The mitral orifice was greatly dilated.

Blood was extravasated in small quantity under the external serous membrane of the ventricles, from the rupture of a small venous branch. There were the traces of old pericarditis, which had not terminated in adhesion, for lymph in patches of considerable size was found on the exterior of the auricles.

In the three cases last detailed, the primary lesion was valvular inadequacy, in which the signs of an obstructed circulation do not necessarily exist; yet dropsy, engorgement of the right side of the heart, pulmonary hemorrhage and hepatic congestion, were in these instances as fully developed as they would be in cases of obstructive valvular disease. To what, then, are we to refer these symptoms, since the mechanical derangement of the valves is insufficient

to account for them ? That they depended chiefly, if not altogether, on excessive dilatation of the ventricles, with which softening was combined in Case VII., is more than probable; and the writer is the more inclined to adopt this opinion from having, in so many instances, found dilatation alone, that is, uncomplicated with valvular disease, capable of developing all these symptoms in the highest degree. (Cases I., II., III., and IV. were examples of this·)

These cases of aortic inadequacy were, towards their close, accompanied by symptoms usually found where contraction of the left auriculo-ventricular opening has existed. This illustrates the observation already made, "that *dissimilar valvular diseases* are liable to be attended ultimately with the *same symptoms*," because there is, in all of them, a tendency to the development of the same changes in the muscular tissue of the ventricles.

In the subjects of aortic patency, death generally results from a failure of the muscular power of the heart ; and the great principle to be observed in their treatment, as has been clearly pointed out by Dr. Corrigan, is to seek to avert, by such stimulants as may be suitable to each individual case, this tendency of the disease, whilst at the same time local congestions are to be relieved by suitable depletion. But where aortic patency is complicated with excessive dilatation, it would appear, from the preceding cases, that death is rather the result of apnœa. Angina, more or less modified, was present in all these cases. In Case VI. aortitis existed in a well-marked degree ; but in Case V. and VII. extreme dilatation of the heart was the only probable source of this distressing complication.

Herein we see contrasted the effects of hypertrophy and of passive dilatation. In aortic patency, the hypertrophy of the left ventricle so invariably associated with that lesion, has been well called "conservative"* ; for so long as no farther change occurs, little distress, comparatively speaking, is experienced ; but as dilatation supervenes, the force of contraction of the ventricle is gradually weakened, the benefits of augmented muscular power are counteracted, and an obstructed circulation, with all the symptoms which invariably attend it, are ushered in.

As aortic patency, when uncomplicated, produces no obstruction to the circulation, so in like manner, of mitral patency—mitral regurgitant disease. As a sequel of rheumatic endocarditis, mitral patency is very common. It may exist for years without producing much inconvenience. To illustrate the phenomena of mitral regurgitation, and to contrast them with the symptoms developed at a later period, when dilatation has been superadded, the following case at present under observation, may be briefly narrated :

CASE VIII.

Maria Benson, aged 15, was admitted into the Whitworth Hospital under my care, June 28, 1852. After a severe wetting in September last this young girl was attacked with acute rheumatism in a very severe form. For four

* By this expression, as I believe, is generally understood, that as the function of the aortic valves is annulled by their inability or insufficiency to meet behind the blood, the left ventricle is called upon not only to propel the blood by its systolic contraction, but, during the diastole of the heart, to support the column of blood in the aorta as the valves do in health. Hence hypertrophy is developed. But I apprehend it may be understood to mean more than this. When the aortic valves have become inadequate the influence of the elastic power of the aorta in propelling the mass of blood is more or less diminished, because the resistance of the valves behind is essential to enable the elastic pressure of the artery to urge the fluid onwards.

Hypertrophy of the left ventricle, therefore, is necessary to compensate for the loss of this important auxiliary in moving the blood. For this twofold reason the hypertrophy which attends aortic patency is always excessive, and as it enables the circulation to be carried on with more efficiency than otherwise, it has been aptly termed "conservative."

weeks she was confined to bed; after this, and even when able to walk about, she was not free from a certain amount of swelling and deformity of the joints; in December the disease was yet lingering in a subacute form. Under the use of the iodide of potassium the rheumatic affection was completely removed, and she quickly regained health and strength. At this period she became liable to palpitation of the heart, and was conscious of the unusually violent action of that organ. Being of an extremely uncomplaining disposition, she did not mention this for some months afterwards, when she grew alarmed at the continued force of the heart's action. At present she has all the appearances of robust health, being large and well formed for her age; her appetite is good; she sleeps well; she makes no complaint except of the force with which the heart beats, and suffers from palpitations only on making any unwonted exertion.

The impulse of the heart is very strong and can be felt over a large space. The parietes of the chest are visibly displaced by the violent propulsion of the organ; there is an increase in the extent of cardiac dulness, indicating considerable enlargement of the heart; the force of the impulse denotes that this is due to hypertrophy; a loud prolonged systolic bellows murmur heard over the region of the apex, which lies to the left of its usual position, denotes free mitral regurgitation; over the sternal region the sounds of the heart are heard unaccompanied by any abnormal murmur; between the spine and the vertebral border of the left scapula a single *bruit* can be distinguished. The pulse is quick, ranging from 110 to 120; its volume is disproportionate to the force of the heart's impulse. There has been no pulmonary hemorrhage or œdema of the feet. The signs of obstruction are wholly wanting.

July 20th. This case has now been for about three weeks under treatment. The force and rapidity of the heart's action have been considerably diminished by the careful use of digitalis, with mild aperients, and by maintaining mind and body free, as far as possible, from any over-excitement.

Cases such as the preceding will be familiar to every practitioner as a sequence of acute rheumatic endocarditis. The symptoms present a striking contrast to those which are developed when excessive dilatation of the ventricles has been superadded, which still further corroborates the remarks which have been already made on the influence of dilatation, especially when softening is superadded, in causing obstruction of the circulation.

The following case from Dr. Hope's work furnishes this contrast.*

CASE IX.

"Elizabeth Dennis, aged 50. Admitted into St. George's Infirmary under Sir J. Clarke, December 9th, 1830, affected with *all the symptoms of organic diseases of the heart in their most severe form.* Has been affected with ascites and anasarca. Bellows murmur accompanying the first sound below the middle of the heart, but not in the region of the aortic valves; impulse strong. Pulse irregular, unequal, and extremely feeble, later than the ventricular systole.

"*Autopsy.*—Hypertrophy and dilatation of the heart; all the valves healthy except the mitral, the free margin of which was thickened by fibro-cartilage, and the chordæ tendineæ were shortened in such a manner as not to allow the layers of the valve to come into apposition, hence a space judged to be about as large as a finger was left, through which regurgitation would take place."

The next case not only illustrates the influence of dilatation in producing the phenomena of obstruction, but also the effects of mitral regurgitation, and of a dilated heart, on the neovous centres.

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* Dr. Hope on Diseases of the heart. Third Edition, p. 573.

CASE X.

Anne Connor, aged 30, was admitted into the Whitworth Hospital, under the care of Dr. Banks, November 10, 1848.

For six months previously the unequivocal symptoms of heart disease had existed. Menstruation had been irregular for three months. Since then she frequently complained of sensation of weight in her head, dimness of sight, with great depression of spirits, to which was superadded an uncertain and tottering gait. Symptoms of hemiplegia then became developed. On awaking one morning her tongue felt swollen, and she was unable to articulate distinctly. In twenty-four hours there was complete paralysis of the left side.

This patient left the hospital in a fortnight, and was readmitted three weeks afterwards, under my care. The prominent symptoms in the case remained unaltered. The left side was hemiplegic, without any diminution of the ordinary tactile sensibility of the surface. General dropsy had supervened. There was dyspnœa from congestion of the lungs; the face was bloated; the pulse small and frequent She died two days after her return to hospital.

Autopsy. *—The heart was considerably enlarged, especially its systemic portion; the left auricle presented a remarkably capacious cavity, with thickened walls; in its interior was found distinct evidence of previous endocardial inflammation, for on the free surface of its lining membrane a patch of rough, granular lymph was deposited. This extended into the left auriculo-ventricular opening, and implicated the mitral valve.

This valve was found thickened, its edges rough and irregular, whilst several of its chordæ tendineæ were ruptured.

The left auriculo-ventricular opening was rather larger than natural; the left ventricle presented a dilated cavity, whilst its walls were diminished in thickness; the aortic valves were healthy.

In the heart it appeared, therefore, that every condition existed to allow of regurgitation through the left auriculo-ventricular opening. The lungs were very much congested, but were otherwise healthy.

The morbid condition of the brain was exactly limited to the corpus striatum on the right side, the interior of which was extensively softened. The remainder of the brain was perfectly healthy, and no traces of inflammatory action, either of the brain or of its membranes, could be detected (*exsanguineous ramollissement.*)

In this case, death occurred so soon after admission that there were not opportunities sufficient for carefully analyzing the physical signs. The symptoms of a lesion of the nervous system were predominant, but from the history of the case, which stated the previous existence of the symptoms of heart disease, together with the existence of general dropsy, and a loud bellows murmur over the heart, the conclusion was arrived at, the heart was the organ primarily engaged, and that the lesion of the brain was consecutive to, and depended on, it.

This case furnishes another instance of phenomena of an obstructed circulation as associated with dilatation of the left chambers of the heart, and of the left auriculo-ventricular opening.

It has been stated that cerebral lesions, in cardiac disease, are not peculiar to mitral contraction. In the case last recorded, extensive softening of the brain accompanied an opposite condition of the heart. But the explanation is not difficult. "The imperfect condition of the mitral valve (the result of endocarditis and subsequent rupture of some of its tendinous cords) induced 'permanent patency' of the left auriculo-ventricular opening. The lungs, no longer protected by the mitral valve, as they are in health, were gorged by the refluent blood of the left ventricle, whilst the system at large was proportionally de-

* Published in Reports of the Patholigical Society, January 13th, 1849.

prived of its due supply of arterial blood: and in this instance, as in other similar ones, as also in cases of large aortic aneurism, some part of the grey substance of the brain, which so especially requires a large amount of red blood for the due performance of its functions, was found in a state of *ramollissement*. A contracted state of the left auriculo-ventricular opening, by obstructing the flow of the blood into the left ventricle, would induce the same morbid changes in the brain equally with a permanently patent condition of the same opening; for in each case the effect is the same—a diminution in the energy and efficiency of the systemic circulation." *

In those cases, and they are of very frequent occurrence, in which cardiac and renal disease co-exist, there is an obvious and a direct cause for a diminution in tone of the muscular fibre. It is now universally admitted, that with degeneration of the kidneys there is a diseased condition of the blood (it matters not with reference to the present question, whether this depravation of the fluids *preceded*, or was the *consequence* of the renal unsoundness), and when the elements of nutrition are diseased, the heart, for obvious reasons, must be early affected. In some of the preceding cases disease of the kidneys existed, and, no doubt, exerted a direct influence on the cardiac affection (Cases IV., V.) It is unnecessary however, that I should do more than allude to this subject here; it has been fully illustrated by the genius and researches of Dr. Bright, who first opened up an extensive field of inquiry, which has since been extended by the labours of many talented observers.

I have thus dwelt much on dilatation of the heart as a direct cause of obstruction to the circulation. It occurs under a great number of circumstances, which, if traced back to their primary source, may be reduced, probably, to three varieties:

1. Mechanical obstruction may be the proximate cause of dilatation, which is then generally combined with hypertrophy. The several forms of valvular disease furnish numerous examples of this class (Cases V., VI., VII.), to which must be added, obstructions in the aorta. A loss of elasticity from disease of the coats of this vessel I have shown, in two instances, to be capable of producing dilatation of the heart equally with valvular disease (Case III. and IV.).

2. The muscular tissue may yield to the pressure from within, owing to inflammation of the substance of the heart, either acute or chronic: carditis is a generally admitted cause of softening.

3. Dilatation may exist, and yet be less a local disease than the result of deficient or depraved nutrition, as occurs in the different forms of "Bright's disease" of the kidney. In such cases, valvular disease is often co-existent; but, on the other hand, there may be no other evidence of disease in the heart than excessive enlargement. Dilatation, which will then be the *primary cardiac* affection, is after all, in such instances, but *secondary to renal disorganization*.

The following are some of the more important inferences deducible from the preceding observations:—

1. Excessive dilatation of the ventricles of the heart is a direct and an efficient cause of obstruction of the circulation.

2. It contrasts, in this respect, with hypertrophy, the benefits of which latter condition in valvular disease are often subsequently counteracted by progressive dilatation.

3. Dilatation may exist independently of diseased valves, and may produce the general symptoms of obstructive valvular disease, or when accompanied by softening, it may develop the signs which specially indicate mitral contraction.

4. Dilatation may occur as a complication of all forms of valvular disease,

* Pathological Reports, as cited above.

modifying their signs, and producing an identity in the final symptoms of dissimilar diseases.

5. Hence, in valvular diseases not essentially obstructive, when dilatation is superadded, the signs of obstruction become developed. The occurrence of pulmonary apoplexy, and other phenomena of an obstructed circulation in aortic patency, may thus in general be explained.

6. Atheromatous disease of the aorta causes obstruction from the loss of elasticity of the vessel. Enlargement of the heart may thus be induced, which, as dilatation becomes established, proves fatal, though the valves of the heart are free from disease. (*Dublin Journal.*)

II.—*Disease of Woman unconnected with Pregnancy.*

Final Cause of Menstruation. —Dr. F. Ramsbotham has published a short paper enunciating views which have given rise to considerable discussion. He admits to the fullest extent the ovular theory of Bischoff and Pouchet; but, in addition, he propounds the hypothesis, that the menstrual secretion and the dicidua are convertible phenomena, the one or the other occurring according as impregnation does or does not take place. This identity of the two products, he thinks, is established by the following considerations:

An ovule, he observes, ripe for impregnation, parts from the ovarium and is grasped by the Fallopian fimbriæ. At the same time, nature establishes an action in the uterus for the purpose of preserving it, provided it becomes impregnated. In this case the fluid formed is retained in the uterus, and becomes gradually converted into deciduous membrane. If, on the contrary, for want of impregnation the ovule perishes, then this fluid, being no longer required, is allowed to pass away and becomes the menstrual fluid.

This view, the author thinks, is strengthened by the fact, that the menstrual fluid and the decidua seem both to be the product of the same tubular glands; that the decidua, when first formed, is a viscid fluid; and that, in dysmenorrhœa, a membrane is not unfrequently formed in the virgin uterus, which has very much the external characters of decidua; that those females who menstruate irregularly or paintully, are not so obnoxious to pregnancy as those in whom the function is normally performed; and that in lower animals, in which there is no menstruation, there is no deciduous membrane.

(*The Half-Yearly Abstract of the Medical Sciences.*)

III.—*On Sudden Death in the Puerperal State.*

BY ALFRED M'CLINTOCK, M. D., Dublin.

(Dublin Medical Press, March the 10th, 1852.)

(The author of the following paper has endeavoured to elucidate a subject which has not met with much attention. According to him, though numerous instances of sudden death after delivery have been recorded, there has been no systematic inquiry into their causation, and it is this defect which he is anxious to remedy. He thus proceeds):

"Writers on medical jurisprudence recognise three diseases which may rapidly extinguish life and leave no morbid appearance; which are, the simple apoplexy of Dr. Abercrombie, syncope, and asphyxia. No unequivocal instance of the first in puerperal patient, has come to my knowledge; but of

the latter two some instances may be abduced. Idiopathic asphyxia causes death almost instantaneously, or in a few minutes. The symptoms are those of fainting; and the only appearance in the dead body is flaccidity of the heart, with unusual emptiness. Of this an example has been recorded by Dr. Beatty.

"M. Chevallier's original paper on the disease was published in the first volume of the "Medico-Chirurgical Transactions," and he there narrates an example of sudden death from this cause, in the person of a lady who had given birth to twins about three hours previously. He himself conducted the *post-mortem* examination of the body, and from what he there found, he inferred that death could only be attributed to this peculiar species of asphyxia. The same author also cites from Morgagni a case of rapid death in childbed, in which the necroscopic appearances led him to think that the woman's existence was terminated from the same cause. I am much indebted to the kindness of Mr. Barker, of Cumberland-street, for the permission to mention here the circumstances of two cases that came under his own observation some years ago, which serve very forcibly to illustrate this part of my subject. In each of these cases, death took place quite suddenly and unexpectedly, not very many days after delivery. In both instances a coroner's inquest was held which was the occasion of Mr. Barker's knowing anything about them. As may be well supposed, he submitted the bodies of these women to a very extensive and close scrutiny, but he failed in discovering anything to account for death, except an unusual flaccidity of the heart, with a complete absence of blood in its cavities. We may fairly conclude with him, therefore, that dissolution was the result of idiopathic asphyxia, or of some cognate syncopal affection.

"These cases require no comment. The evidence they contain of death having been produced by the operation of a cause similar to that pointed out by M. Chevallier, is, to my mind, conclusive. That there are not more instances of the kind to be found recorded may in some measure, be accounted for by the attention of observers being too exclusively directed to the abdomen in their examination of these cases *post mortem;* and secondly, from the fact of the subject of M. Chevallier's paper not having been as generally known and understood as it ought to be. If the actual possibility of such a cause of death as this be admitted, there is no reason that I can see why a puerperal woman may not be the subject of it. Further, if we look upon the idiopathic asphyxia of M. Chevallier as nothing more than a variety or form of syncope, the liability of its invading a woman in childbed becomes still more apparent, from the state in which her constitution is left by the act of parturition—a state of which the prominent characteristics are, an unusual proclivity to diseased action—an excitable condition of the vascular, and a morbid susceptibility of the nervous system. The shock of labour is not recovered from for many days, and during this period (the length of which necessarily varies under different circumstances) the *vis vitæ* is minus: hence, any impression of a severe kind, whether affecting the mind or body, is not met by some vital resistance as at other times. With these well-known facts before us, there need be little hesitation in our drawing the conclusion, that many of the unexplained cases of sudden death in the puerperal state, are to be ascribed to idiopathic asphyxia, or fatal syncope.

"Let us now pass on to the consideration of some of the other reputed causes of this catastrophe. It is an acknowledged law, that protracted pain exhausts the principle of life, and in this way it is attempted to account for some of the anomalous cases of speedy dissolution after delivery. Touching this point, Mr. Travers has given some observations which it would be culpable to omit, coming from so high an authority. 'Pain,' says this author, 'when amounting to a certain degree of intensity and duration, is of itself destructive. Difficult and protracted parturition is, every now and then, fatal from this cause; and even in cases in which neither extraordinary difficulty nor protraction was experienced, a fatal prostration has sometimes supervened which has admitted of no other explanation. The delivery has been complete, without any degree of

physical injury, and not more than an ordinary quantity of blood has escaped from the vessel of the uterus. Yet the woman, in spite of the encouragement derived from the consciousness of safety to herself and infant, and of comfort from the conclusion that her sufferings were at an end, has never rallied either in strength or spirits ; but after an interval, not exceeding a few hours, passed in a low and sinking state, has, unexpectedly, and with little perceptible alteration expired.'—(*Inquiry*, 2d edit., p. 48.)

"In a large proportion of the cases, where this state of prostration or collapse has manifested itself, there had existed, some time previously, a strong mental impression or foreboding of disaster, which presentiment, as it is termed, must have contributed materially in bringing about the fatal result. That a lengthened occupation of the mind by one dominant idea of a gloomy character should exercise a marked depressing influence upon the vital energies, is a fact of which every physician is fully aware, and of which there are innumerable examples on record."

(The author quotes several cases illustrative of this form of sudden death, from Dr. Ramsbotham, Mr. Travers, and Dr. Gartlau, of Dundalk; he then continues to notice the further literary history of the subject, alluding to the entrance of air into the uterine sinuses as a cause of the catastrophe ; which reputed cause we have had occasion to notice in a former volume.)

"About the year 1808, Le Gallois, in the course of some experiments upon animals, observed, in three different cases, air to penetrate into the vena cava from the uterine veins, and that this was followed by instantaneous death. His son, writing twenty-one years afterwards—viz., 1829, after citing these experiments, asks this question : In many of the cases of sudden death after delivery, might not this event have been caused by the entrance of air into the circulating system through the uterine vessels ? We find Olivier repeating the same suggestive query in 1833, in the article 'Air' of the 'Dictionaire de Médecine.' Since then, the advance of obstetric knowledge has placed nearly beyond a doubt the possibility of such an occurrence, and thus added one other to the manifold causes of death in the puerperal state. To Dr. Rose Cormack belongs the praise of having elucidated this very obscure subject ; and of his instructive essay I have largely availed myself in the subjoined remarks. His experiments and reasoning, together with subsequent observations, justify our drawing the following conclusions—1st, that the admission of a certain quantity of air into the current of the circulation is capable of destroying life almost instantaneously—a fact, indeed, which the records of surgical practice fully corroborate ; 2dly, that the possibility of air occasionally finding an entrance into the vascular system through the uterine vessels, seems highly probable; and 3dly, that in some few instances of sudden death soon after delivery, the only cause for the catastrophe which a minute inspection of the body could discover, was the existence of air-bubbles in the heart and vena cava.

"It would be irrelevant to my present purpose to enter into the general question of the history and pathological effects of the presence of air in the veins. Those who are desirous of an enlarged acquaintance with this interesting topic, I would beg leave to refer to an essay by the late Dr. John Reid, published in the same volume with his other researches. This will be found to contain a most able and comprehensive analysis of all that is known on the subject.

"The mechanism, so to speak, by which the introduction of air into the uterine veins can be effected, admits of being explained in a few words. The veins of the gravid womb present four remarkable characters—namely, their extraordinary large size ; their freedom of inosculation ; the total absence of valves ; and their termination on the internal surface of the uterus at the site of the placenta, by large open orifices. If the uterus be examined soon after delivery at the full term, the majority of these apertures will readily admit a goosquill, and some will even allow the little finger to penetrate without laceration. During contraction of the uterus, all these openings are hermetically

closed, but when it is relaxed they again become proportionately more or less patulous. From this it is manifest that the same condition of the organ which causes flooding, is exactly that which is indispensable for the ingress of the air: so that the latter, when it does take place, is almost of necessity preceded or accompanied by hæmorrhage. This fact is of some value, viewed in connection with the history and progress of those cases where it was supposed that air had gained admission into the circulation through the uterine veins after delivery; for Amussat found in his experiments upon the entrance of air into the venous system, 'that the period of death was hastened considerably in those animals whose vessels had previously been depleted of part of their blood.' (Reid). But it will naturally be asked, does the air ever gain access to the uterine cavity, for otherwise it could not possibly find its way into the vessels of the womb? This question I am of opinion, can safely be answered in the affirmative. Confining ourselves to the simple matter of fact, it may suffice to state, that professor Meigs assures us he noticed the explusion of air from the uterus immediately after delivery, 'a great many times.' Dr. Rose Cormack has made the same observation; and I have myself remarked a similar occurrence on at least three or four different occasions. Dr. Meigs in his Letters to his Class, minutely describes the process by which the air is drawn up into the uterus; but it is unnecessary here to quote his remarks. With these considerations before us, then, we are in a position to adopt the language of Dr. Cormack:—'I have, therefore (writes this gentleman), not only no difficulty in believing, but am constrained to admit that, should any impediment be offered, in such cases, to free exit of air by the os uteri, it must be forced into the uterine veins, were their mouths not protected by coagula; and thence it would rapidly pass, by the current of the circulation, up the vena cava into the right auricle.' ('London Journal of Medicine, vol. ii. p. 941.')

"The intensity of the symptoms when air is taken up by the uterine veins would seem, as in other cases, to depend very much on the quantity, and on the condition of the patient. Death may ensue in a few moments from the rapid distension of the right auricle with air, and its consequent inability to contract. This first danger over, she may still perish at a remote period from asphyxia, induced by gradually augmenting pulmonary obstruction.

"Dr. Cormack refers, in support of his views, to seven cases from different authentic sources, in all of which death was supposed to have been more or less directly occasioned by the passage of air through the uterine veins into the vena cava and heart. These cases, taken collectively, form a body of evidence which it is hard to refute. In six of them, the presence of air in veins was demonstrated upon inspection of the body, and no one of these cases exhibited any other morbid lesion adequate to account for death. In all, with a single exception, where there was prolonged retention and putrefaction of the after-birth, the fatal event took place within a very few hours after parturition. The symptoms which presented themselves in these cases were very various; and those most frequently observed were by no means pathognomonic. Great anxiety of countenance, embarrassed respiration, with a sense even of impending suffocation, and a weak, rapid, faltering pulse, seem to have been the prominent features of the cases where there was time for the development or observance of symptoms.

"Besides the seven instances above alluded to as being adduced by Dr. Cormack, I find another recorded in the 'Provincial Medical and Surgical Journal,' for November 27, 1850, by Mr. Berry. The leading features of this case it may be well to give. A woman, æt. 22, was delivered of her first child after a natural labour, at seven in the evening of June 17, 1850. The placenta came away in twenty minutes, unattended by any immoderate loss of blood. At half past eight, she expressed herself comfortable, and at eleven took some gruel. At one o'clock of the same night, her husband, who lay in the same room with her, became alarmed by the patient's difficult breathing and feeling of faint-

ness, and immediately sent for her medical attendant, but before his arrival, at two o'clock, she was dead. She lived seven hours after delivery. 'The cause of death could not be accounted for, as there was no hæmorrhage, and apparently nothing in the condition of the patient to prognosticate such a termination. . . . Upon opening the abdominal cavity, the uterus was seen midway between the umbilicus and pelvis, the peritoneum covering it, and the intestines healthy, but pale; the stomach contained a small quantity of fluid; liver healthy; the kidneys presented a granulated appearance, and the urine which remained in the bladder was ascertained to be, by the application of heat slightly albuminous. Upon cutting into the uterus it was found empty, and the vessels where the placenta had been attached, patulous; the vagina contained, at its superior part, a moderately-sized clot of blood; within the chest, both lungs were congested, and contained scattered tubercles within upper lobes; the heart was the size of a male heart, and apparently distended. Upon making an incision into it, a gush of air escaped, and the organ became flaccid; no blood was found in its cavities. About an ounce of serum was observed in the pericardium. The brain was healthy in every respect. No signs of decomposition existed in any part of the body.' From the remarks of the writer of this case, it is plain the impression on his mind was, that the immediate cause of death could have been no other than the air in the heart. If this conclusion be denied, we are met by the question—How, then, is the woman's sudden decease to be accounted for? It is hardly possible, I think, that the granular disease of the kidneys which she appears to have had, could have brought about the fatal event. This, however, I leave for the Society to determine. One point in the case deserves some consideration before admitting it to possess any value, and it is this, the examination of the body was not made for at least fifty hours after the woman's death, which, be it remembered, took place in the month of June. Mr. Berry has expressly stated that there were no signs of decomposition present; still, the fact I have mentioned diminishes in some degree, perhaps, the importance that would otherwise justly belong to the unusual circumstance of air being present in the heart. Dr. John Ramsbotham narrates a case which I am tempted to introduce here, from the resemblance in many of its features to the foregoing history, and from the presumptive evidence it affords that if special search had been made for it, air might probably have been found in the heart, and thus explained the cause of the patient's unexpected death. It was the lady's first child, and the labour was tedious, requiring the use of the forceps. 'A dead child was soon produced into the world without any particular difficulty or accident, and as soon as it was born a quantity of offensive gas, with that olive coloured fluid elsewhere mentioned, escaped from the vagina. Uterine action did not seem disposed to return, and after waiting some time a separated placenta was withdrawn. After this the uterus felt well contracted, and the woman was left in a favourable state between two and three o'clock. In the evening my friend called to inform me that this poor woman had died very suddenly and unexpectedly between five and six. All he knew about the matter was, that he was called in a hurry to the poor woman, who was represented to be in a fit, but he found her dead, with her belly much swelled. Anxious to learn the cause of so melancholy an occurrence, leave was obtained to open the body, which was inspected the next morning. . . . . On dividing the parietes the intestinal canal was seen somewhat distened with gas, but the rest of the viscera were healthy. — The uterus was much extended and felt flaccid; and on pressing it a quantity of fetid gas escaped per vaginam; after its escape the organ became still more flaccid. On opening into its cavity there was only one small coagulum at the os uteri. The appearance of the uterus on dividing the abdominal parietes was not unlike one at the fifth or sixth month of pregnancy. I must confess (continues Dr. Ramsbotham) that before the uterus was handled or opened

I suspected death to have been occasioned by internal hæmorrhage : that certainly was not the case.' (Op. cit. p. 122.)

"Now, from what has preceded, it may be safely asserted, that if the possibility of death from the admission of air into the uterine veins be not established on conclusive evidence, enough has still been adduced to show the absolute importance of making special examination for its presence in all obscure cases of sudden death following parturition. In conducting this examination our attention should be chiefly directed to the heart and vena cava. If air exist in the latter, it will probably be discoverable through its coats ; at all events, before cutting into it the heart should be taken out. Previously to doing this the great vessels leading to and from the organ should be tied, and then after its removal the right auricle and ventricle are to be carefully opened under water, by which process the escape of any air will at once be demonstrated.

"There are strong grounds for believing, as has been already hinted, that the idiopathic asphyxia of M. Chevallier is merely another name for syncope. Discarding all preconceived opinions, and looking only to facts, we find very many examples recorded of sudden death from fainting, in which the condition of the heart was precisely similar to that described as having existed in M. Chevallier's cases. The decision of this question, however, does not affect my present object, nor the remarks which I have ventured to offer, though I admit that it is one of no small interest and importance."

(*Half-Yearly Abstract of the Medical Sciences.*)

IV.—*The Effects of Different Remedies in Diabetes.* *

The subject of Dr. Frick's observations was placed under peculiarly favourable circumstances for ensuring all necessary certainty. He was a prisoner in the Maryland Penitentiary, in the prime of life, not fleshy, but healthy and strong in appearance. The case itself presented no unusual peculiarities.

In the course of the investigation, abundant proof of the general and not local character of the disease was obtained, sugar being found not only in the urine but also in the sweat, fæces, chyme, bronchial mucus, saliva, as well as in the blood and in the purulent matter of an abscess in the hand. Sugar was also found in the ordinary excretions, and in considerable, though in diminished quantities, when all sugar or sacharifiable substances were carefully excluded from the food ; it was even found in the chyme when the previous meal had consisted of meat and eggs alone.

Dr. Frick's conclusions upon the influence of diet are somewhat surprising. In the matter of *drinks* he finds it not necessary to subject the patient to any privations. He found, indeed, that the *specific gravity* of the urine varied according to the amount of fluid drank, but that the sacharine contents in a given time underwent little or no variation. And equally so with respect to food ; for though the symptoms are ameliorated when the diet is made to consist of animal matters, "there is no real improvement, and the deprivation of sacharine and amylaceous matters is not counterbalanced by the diminished thirst and less frequent calls for micturition."

We have been most struck, however, with what is said upon the effects of remedies in the treatment of diabetes, and it is this part of the article that we would reproduce here. Many remedies were tried, and their effects carefully

* A Case of Sacharine Diabetes, with observations on the Results of Treatment By Charles Frick, M. D., Baltimore. American Journal of Medical Sciences, July 1851.

noted for a week at a time, every precaution being taken to preserve the patient as much as possible in the same predicament. The remedies were given in the dose indicated below, and repeated thrice daily, and the result is shown in the corresponding figures, which indicate the amount of sugar passed in the stools as well in the urine during the week in which each remedy was tried.

		Grains
Strychnine	$\frac{1}{6}$ gr.	3369
"	$\frac{1}{7}$ gr.	3565
"	$\frac{1}{10}$ gr.	6250
"	$\frac{1}{15}$ gr.	6425
"	$\frac{1}{20}$ gr.	6360
Mur. tinc. ferri	10 drops	6900
"	20 "	8264
Aqua ammonia	5 "	12550
Iod. potass.	3 gr.	14270

	Grains.
Without medicine . . .	14520
Creosote and naphtha .	15028
Cod-liver oil, 6 ℥ per week	15058
" 10 ℥ per week	16108
Ergot	17150
Cod-liver oil, 20 ℥ per week	20160
Whiskey	20504
Calomel and opium . .	24230
Ergot, strychnine, and iron	24340

Strychnine.—The amount passed without medicine is obtained from the average of eleven analyses. We see, therefore, that the influence of strychnine exerts by far the greatest control over the quantity of sugar passed in the urine and fæces. The patient was kept under its influence for various periods, amounting in all to four months. It is here shown that, under doses of one-twentieth of a grain, the amount is diminished to less than one-half, and under one-sixth of a grain to less than one-fourth. For three successive days he was kept upon a meat diet, and one-sixth of a grain of strychnine administered three times daily. The quantity of sugar, on the third day, was diminished to 132 grains. This was the 30th of October, and was the smallest quantity we ever found in this patient's urine.

Tinc. Ferri. Mur.—This remedy, in doses of ten drops, diminished the sugar one-half; but on increasing the dose to twenty drops, a notable increase manifested itself, though still showing the beneficial effects of the medicine.

Aqua Ammonia.—The diminution here amounted to one-seventh. Larger doses were tried, but they produced so much uneasiness that they had to be discontinued.

Iodide of Potass.—The effect of this remedy over the excretion of sugar was little or none. It produced pain in the bowels and diarrhœa.

Creosote and Naphtha.—These also produced great inconvenience, and their effect was to increase slightly the quantity of sugar.

Cod-liver Oil.—In whatever doses this medicine was administered, its effect was to increase the amount of sugar. When six ounces per week were taken, the difference was slight; but when increased to twenty, one-third more sugar was passed. One fact, however, is worthy of notice. The patient, under this remedy always gained weight, and, with the exception of the period when ergot was administered, only at that time. In fourty-four days, on four pounds of oil, he gained nineteen pounds.

Ergot.—The patient, under the influence of this remedy, gained in one week nine and a half pounds, but the amount of sugar increased one-sixth.

Whiskey.—This increased greatly the quantity of urine, as might be supposed, and also the sugar, which amounted to one third-more than when he was taking no medicine.

Calomel and Opium.—This was continued for two weeks, till the patient was brought decidedly under the influence of the mercury. The calls to urinate became more frequent, and the amount of sugar became nearly doubled.

Ergot, Strychnine, and Iod. Ferri.—Under this combination the excretion of sugar was about the same as the preceding. He complained greatly of the mixture, and the largest quantity of urine was passed during its administration.

(*Half-Yearly Abstract of Med. Sciences.*)

V.—*Excision of the Spleen.*

The surgical practice of Kentucky furnishes two cases of partial excision of the spleen; one by Dr. Daniel C. Caldwell,* of Russelville, and the other by Dr. William Byrd Powell, † formerly of Newport, now of Memphis.

In the first case, the patient, a negro, was stabbed with a large knife, on the left side, between the last true and the first false rib, "about the point of their greatest convexity." Through the opening thus made an oblong body, from three to four inches in length, protruded, and which was sopposed by Dr. Caldwell and the attending physician to be the small extremety of the pancreas; but which, judging from all the circumstances of the case, could have been nothing else than a portion of the spleen. An attempt to replace the organ into the abdomen was made the day after the accident; but, this failing, the protruded part, now in a state of strangulation, was cut off with a bistoury, after which the edges of the wound in the skin were brought together with adhesive strips. No hemmorrhage followed the operation; and the man, whose age is not mentioned, soon recovered.

In the case of Dr. Powell, that of a man of thirty, the wound was situated on the left side between the second and third false ribs, about four inches from the spine, and was inflicted by a knife, or large dirk, passing downward, inward and forward, into the cavity of the abdomen. After cleansing the wound, which was covered with soot and flour, to arrest the hemorrhage, a portion of the spleen, nearly two inches in length, and having a considerable slit at its extremety, was found to protrude trough it, and to be so firmly embraced by its edges as to render it impossible to replace it. As it was late in the evening when Dr. Powell first saw the case, he concluded to wait until the next morning, at 9 o'clock, when the projecting part was encircled by a strong piece of tendon, and amputated close to the surface of the body. All attempts to restore the remainder of the organ to its natural situation proving fruitless, the external wound was dressed in the usual manner; and the patient being put to bed, was bled at the arm to the extent of a pound and a half, and placed upon a light diet. On the third day, high constitutional excitement manifested itself, followed in a short time, by all the symptoms of peritoneal inflammation, for the relief of which, the ordinary remedies were employed. All treatment, was discontinued at the end of a fortnight, when the wound was nearly cicatrized; and nine months afterwards, when Dr. Powell met the patient, he was in the enjoyment of excellent health.

(*Transactions of the Kentucky State Med. Society.*)

VI.—*Treatment of Sprains by "Firing."*

BY JAMES DICKINSON, ESQ.

The advantages of "Firing" in many forms of rheumatic and neuralgic affections have been pointed out by Corrigan, Day and others, (vide "Abstract," vol. III., p. 199.) Its use in sprains of the back, seems to be one from which the most strikiug benefit may be anticipated, as is seen in the following remarks:

Sprained backs are cases which give the surgeon much trouble and annoyance, appearing in many instances to resist every remedy. Many cases have come under my notice, and finding that blisters, cupping, stimulating liniments,

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* Transylvania Journal of Medicine and the Associate Sciences, vol. 1, p. 116 Lexington, 1826.

† American Journal of the Medical Sciences, vol. 1, p. 481. Philadelphia, 1828.

&c., failed, I tried "firing," and the results have been most successful; patients who for many weeks have evinced the greatest agony, have, after the first or second application, been perfectly cured. The plan to be adopted is as follows: Heat a metal button, the shank of which is fixed into a wooden handle, to such a temperature as can be borne with slight pain. Pass it lightly over the affected part, without inducing vesication, which is unnecessary. The pain produced is severe, but is transient. In long-standing cases two or three applications are required; in recent ones one will be found sufficient. *—

Prov. Med. and Surg. Journal.

VII.—*Lithotomy and Calculous Diseases in Kentucky.*

From the Transactions of the Kentucky State Medical Society, already noticed in our May Number '53, we select for this issue, some interesting observations, among which we deem the following worth of notice.—Dr. Gross who employs the knife and pen with equal skill, in his report on Kentucky Surgery, says, speaking of Lithotomy:

Reduced to figures, the number of cases of lithotomy, positively known to have occurred in Kentucky, stands about thus:

Dr. B. W. Dudley,	207	Dr. John C. Richardson,	1
" E. McDowell,	32	" John Craig,	2
" A. Goldsmith, uncertain,	50	" W. H. Donne,	1
" W. Gardner,	14	" Walter Brashear,	0
" J. M. Bush,	6	" E. L. Dudley,	1
" John Shackleford,	4	" D. W. Yandell,	4
" Henry Miller,	2	" L. P. Yandell,	4
" John Hardin,	5	" S. D. Gross,	30
" S. B. Richardson,	2		

It is an interesting fact, with reference to the etiology of calculous affections, that most of the above cases occurred among the inhabitants of Kentucky and Tennessee. The remainder were brought from Alabama, Mississippi, Indiana, Illinois, Missouri, Ohio, and Virginia; a few, perhaps, from other States. Nearly all occurred in white males, the number of blacks being, comparatively, very small. The great majority of the patients resided in limestone regions.

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* This remedy sometimes affords relief in cephalalgia, and other forms of neuralgia.—Editor Buffalo Medical Journal.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Bibliographical Remarks and Reflections on Professor Carl Löwig's Organic and Physiological Chemistry.*

"*Principles of Organic and Physiological Chemistry.* By Dr. Carl Lowig, Doctor of Medicine and Philosophy, Ordinary Professor of Chemistry in the University of Zurich; author of Chimie der Organischen Verbindungen. Translated by Daniel Breed, M. D., of the U. S. Patent Office; late of the Laboratories of Liebig and Lowig. Philadelphia: A. Hart, late Carey & Hart. 1853." Octavo, pp. 481.

Chemistry is not only progressive, cumulative, and, almost boundless in its scope, but it serves as the connecting link between the physical, physiological, pathological and therapeutic—between realms organic and inorganic, ranging from the inert to the vital—from the lifeless, angular crystal, to the living circular cell—from the homogeneous, senseless, involuntary masses hurled through infinite space, to the complex, knowing, feeling, willing, self-moving and rational. Its analogies, identities and differences, aid in drawing the line of demarcation between the organic and inorganic kingdoms; affording in many instances satisfactory criteria for scientific classification and nomenclature.

A difference there is, and a great one, between knowledge and nomenclature, and even between knowledge and science. A simple knowledge of phenomena without any generalization of their agreements, relations, differences, rational import, and without natural ar-

rangement, can scarcely deserve the name of *science*, while the most retentive memory would sink hopelessly exhausted, if burdened with the numerous unrelated details, constituting the entire mass of scientific facts already known and appreciated; were it not for the facilities afforded by the ingenious, convenient, and happy nomenclature of modern times; as, for example, in the science of chemistry. Although it is but little more than half a century since the head of Lavoisier (that illustrious chemist and nomenclator) rolled, full of great thoughts, upon the gory scaffold of Revolutionary France; yet the facts and discoveries which have been accumulating in the progress of chemical science during this short period, would transcend the limits of the memory, be consequently comparatively useless, if named at random, without regard to elementary analogy and mutual relations—without regard to simples and compounds, oxides and carbonates, alkaloids and acids—without classification into organic and inorganic groups, etc.

The nomenclature of Professor Lowig is so difficult, that a new dictionary, as large as the work itself, would seem necessary to the English reader.*

Indeed, Dr. Breed (the translator of this work) seems to have had a glimpse of this necessity, both as it respects words and ideas; for, not only are names wanting in English vocabularies; but, alas! *science itself is wanting in the English mind*, in order to raise it up to the level of the great thoughts of German chemistry! At least, Dr. Breed thus thinks, for he says, "The translation of a purely scientific work like this is attended by many difficulties, not the least of which arises from the very necessity of its being rendered into English; I mean the *absence of anything of a like nature in our language;* and the necessity of a right apprehension of a thousand abstract truths. * * * Should this volume meet with sufficient success to justify the undertaking, and show that *our chemical public are ready for such a work*, it is proposed to offer to them Dr. Lowig's 'Chimie der Organischen Verbindungen,' (Chemistry of the Organic Combinations,) in an English dress, as speedily as possible after the appearance of the third German edition"—(Trans. Pref. x.) a work be it remembered, of 3000 pages! The present work—formidable within—numbering 481 pages, each page presenting a serried phalanx of empirical and rational formulæ, is but the precursor or introduction to thousands yet to come!

* Professor Lowig, let it be understood, does not treat of the metaphysics or abstract principles of dynamic chemistry. Hence, the best dictionary for the explanation of his "many-sided" names and combinations is the laboratory—things, not words—the materialistic, not the dynamic—effects, not causes—organic results, not vital processes.

There is scarcely a page of this prelusory volume that can be extracted, studied, and analyzed without damaging one's digestion, straining the understanding, or exhausting existing Lexicons. Surely it is not prose, but these chemical formulæ, figures, and symbols, which constitute the antitheses of poetry, fine writing and seducing theories! If such books and such hard studies be necessary to the physician, he must be of all animals the most miserable! Peace and ease he can never enjoy, but must be a slave to study all his life, without enjoying the elegant leisure, the refreshing liquors and merry companions of the bar-room, during the half of his lifetime. He must resign that happy routine; Oh, how hard! which he has followed all his life! Instead of visiting seventy-five patients (as in days of yore) and writing the same prescriptions for seventy-five suffering unfortunates, (which he can now do, and think nothing of it) all before breakfast—instead of making the practice of physic only a pleasant mode of airing himself, and saluting the public, and making himself agreeable to all people of fashion, he must go into his gloomy laboratory, using microscopes and chemical apparatus, to analyze the saliva of one patient, the blood of another, the bile of a third, the urine of a fourth, the expectoration of a fifth, the dropsical effusions of a sixth, the black vomit of a seventh, the scrofulous, cancerous, choleraic, tuberculous, diabetic, ulcerous, dysenteric, and febrile fluids and solids of the residue. It may be objected that chemical researches do not pay, and that they are generally not only difficult, but unproductive in a practical point of view. These objections, however, are not valid: for digestion and indigestion, may be elucidated, if not wholly explained upon chemical principles. Chemical processes not only imitate those of digestion and absorption in the living economy, but they are to a certain degree homological or identical. It is but too true, that, excepting in albuminuria, diabetes, gravel, and a few other diseases, the practitioner of medicine has received but little aid, comparatively and chemically speaking, from the professor of chemistry. But this little preludes more as the dawn precedes the day.

To the labors of the laboratory, not to name those of the microscope, must be added the labors of the dissecting room; the labors of the study among grinning skeletons and ghostly books (remains of dead thinkers) the labors of thinking, reasoning, writing, and determining for one's self, both as it regards the actual state of knowlege and ignorance in any particular case of disease daily presenting itself for treatment, involving the most complex and difficult problems, whereby life and death may depend—problems, which neither the public nor the pre-

tenders to physic suffer to agitate their sublime tranquility. "*Quid times? Cæsarem vehis.*"

It is, hence, very evident that the Simons, the Liebigs, and Lowigs, and all others who write on the physiological and morbid chemistry of man, are. virtually, enemies of a certain class of happy physicians, seeing that the writers aforesaid aim to impose scientific researches, burdens and responsibilities wholly incompatible with ease, pleasure, indolence, extensive sleep, and pleasant morning visits to seventy-five patients, including a few easy hieroglyphics, as calomel ℈ j, one scruple; quinine ʒ j, one drachm; cupping ℥ xvi, sixteen ounces; blood-letting ℔ ij, two pounds; and so on. There is an easier way than even this, and one which I have several times witnessed in epidemics: You put from 12 to 15 yellow fever patients in as many beds, in one room. You fix on a very pleasant prescription, that is, iced lemonade for each patient, without any examination whether his stomach be not already as sour as a vinegar vat. At each subsequent visit, you write two words, neither more nor less, that is, "Repeat Drinks." Continue this for one week, and your patients will certainly die, or they certainly will not. This is no fancy sketch.

Simon, Liebig, Lowig and others, ought to make the road easier, knowing as they must, that while physicians generally praise their books and freely acknowledge the importance of organic chemistry, not one in five hundred attempts to apply chemical principles in the practice of medicine, so as to ascertain, either qualitatively, or quantitatively, the morbid products, both solid and fluid, in epidemics, and in acute and chronic diseases generally. Is this neglect of the certainties of medicine justifiable, on the principle that we fully believe and diligently practice the uncertainties? Is physics nothing in physic? Reader! did you ever ascertain by means of a thermometer, before ordering a hot mustard bath, that your patient was not already too hot from the crown of his head even to the soles of his feet? Did you never see a patient affected with an algid disease (as cholera, congestive, and the like) with a temperature already from 10 to 20 degrees below the standard of health, plunged shivering into cold water? Is it a matter of indifference whether you administer heat or cold—the physical or the hypothetical—the remedy the most fashionable, or the remedy the most efficacious?

Faust, after his retirement from practice, did not escape remorse upon reviewing his and his father's prescriptions, while the public were paying them honors for their great success. Faust spoke thus:

Faust.—"Thus did we with our hellish electuaries, rage in these vales and mountains far worse than the pestilence." * * *

Wagner.—"How can you make yourself uneasy on that account. Is it not enough for a good man to practice scrupulously the art that has been entrusted to him?"

Were physic properly practiced, it may be doubted whether the vast swarms of doctors which are sent forth from the medical hives, (colleges) every spring, are too numerous; but, as the practice now too generally is, could not twenty doctors, in the city of New Orleans, do the routine work of two hundred, and do it allopathically, too? As to surgery, Dr. Physick, a few years ago, just before his death, declared that, in the great city of Philadelphia, there was not employment enough for one surgeon. The more true medical science abounds, the less necessity is there for surgery. Much surgery, much reproach.

Who thanks Simon, Liebig, and Lowig, for their learned researches, the understanding of which gives one infinite trouble, wastes lamp oil, perhaps causes headache, and makes a doctor appear to the public as wanting in genius, seeing that he needs thermometers, chemical apparatus, and much study, in order to know how to cure a sick man. A glance, an air of wisdom, is far more satisfactory to the public—far easier.

Simon, Liebig, and Lowig, have mistaken the signs of the times. Reason is not yet altogether popular. Lowig's "Principles of Organic and Physiological Chemistry," and doubtlessly that other work (the third edition) of 3000 pages, are a little in advance of the text books of the day, and must prove detrimental to doctors who finish their education on the day in which they get a diploma in arid Latin.

"Repose," says Sidney Smith, "is agreeable to the human mind; and decision is repose. A man has made up his opinions, he does not choose to be disturbed; and he is much more thankful to the man who confirms him in his errors, and leaves him alone, than he is to the man who refutes him, or who instructs him at the expense of his tranquility: the object is not to know the truth, but avoid the shame of appearing to have been ignorant of it."

What shall be done with those difficult treatises now issuing from the press on physiological chemistry? Can a student during two courses of lectures nominally three or four months long, but, generally, still shorter, master all their necessary processes? Are these books to be studied, ignored, or burnt as nuisances? The Rev. Sidney Smith says that "in the late rebellion in Ireland, the rebels, who had conceived

a high degree of indignation against some great banker, passed a resolution that they would burn his notes; which they accordingly did, with great assiduity; forgetting that in burning his notes they were destroying his debts, and that for every note which went into the flames, a correspondent value went into the banker's pocket." This method would benefit the author and bookseller, and would at the same time keep the student in happy ignorance of his own ignorance.

While the brilliant track of light which chemistry impressed on the scientific heavens during the first half of the present century is augmenting in the latter half, now flowing, its nomenclature is becoming so complex, strange, unharmonious, uncongenial, irregular, multiform, terrifying, that one (not fully initiated) is led to fear there may be something wrong; that the simplicity of nature has not been fully comprehended, in the immensity of modern research, and that singulars have multiplied in a ratio disproportioned to generals or universals. Let us hope that happier generalizations will lead to a better classification and an easier nomenclature. German metaphysics—the most abstruse propositions in Kant's Critique of Pure Reason, concerning synthetical or *a priori* judgments, are infinitely easier of comprehension than German chemistry, masked as it is with hard words; yet, these are exactly the two sciences in which German philosophers excel all others! A satirist has said, that the Germans have the art of making the sciences inaccessible! If this censure be at all just, it applies to modern chemistry.

Philosophical discovery, and philosophical classification, and language, do not always keep pace with one another. In proportion as discovery advances, the dictionary becomes obsolete. New facts, or a new appreciatien of old ones—new methods, and new abstractions, create a new nomenclature. To a profound intellect what can be more abstract and metaphysical than a concrete, tangible fact, with all its essential relations—its ultimate scientific end, aim and import? A fact is often simply as a block of marble to a finished statute; a pile of bricks to a temple; a box of printer's types to the printed Iliad; the means to the end. The complete cognition of a fact in its entireity and its reference to the proper department of knowledge to which it truly belongs, can alone give it value; to appreciate this value (so easy to sciolists) has, in many instances, occupied the acutest intellects unsuccessfully from generation to generation, until, at length, the truth so long desiderated burst forth in all its grandeur. The facts of gravita-

tion and the heart's circulation always existed; but not Newton, not Harvey. Hence, discovery paves the way to a new nomenclature.

One chemist, attracted by certain phenomena, experiments, compares, analyzes, and, perhaps, discovers and names; while at the same time certain residual phenomena escape him. Another investigator attempts, and succeeds in making discoveries in these unnoticed, or unconsidered residues: "Thus Arfwedson discovered lithia," says Herschel, "by perceiving an excess of weight in the sulphate produced from a small portion of what he considered as magnesia in a mineral he had analyzed. It is on this principle, too, that the small concentrated residues of great operations in the arts are almost sure to be lurking places of new chemical ingredients; witness iodine, brome, selenium, and the new metals accompanying platina in the experiments of Wollaston and Tennant. It was a happy thought of Glauber to examine what everybody else threw away."

Hence, from the nature of the case, from the progress of the sciences, new ideas, principles and facts require new names, which latter, having been once well chosen, defined, fixed and made known, give ultimately precision, clearness and brevity to style. If the word physiology, pathology, or oxygen was always accompanied with a full definition every time the term is used, science would move like Pharaoh's chariots, which, after the removal of the wheels, "drave heavily." Nomenclature, terminology and symbols in botany, natural history and chemistry, notwithstanding their complexity, are achievements in philosophical language, as introduced by Linnæus, Lavoisier, and their successors, alike honorable to modern genius, and useful to the learner. Nevertheless, chemistry, like pathology, groans under an enormous load of synonyms; the former, however, has a constant tendency towards systematic exactitude; its aquafortis, aqua regia, arbor Dianæ, cream of tartar, dragon's blood, glauber salts, flowers of zinc, ivory-black, scheele's green, red precipitate, king's yellow, blue vitriol, white lead, lunar caustic, infernal stone, and the like, have been substituted by meaning, scientific and methodical names. It is far different with medicine—it has all kinds of irregular and unscientific names for diseases—green, yellow, scarlet, purple, spotted, white—French, Polish, Greek, Siamese, Pott's, Bright's, painter's, chimney-sweep's disease—the falling, the fidgetty, the dancing, the touch-me-not, the convulsive, the honey-like, bilious, dropsical, drum-like, including diseases represented by the elephant, fish, crab, cow, dog, frog, hog, fly, worm, fire, scales, polyps, nettles, and night mares.

Probably, every 100 pages of Prof. Lowig's work will present a thousand or more words like the following :

Division of the radical : carbyls ; hydrocarbyls ; both terminating in *yl.;* azocarbyls, termin. in *an*, e. g., *paraban;* hydroozocarbyls, termin. in *en*, as *uren ;* chlorcarbyls, bromocarbyls, iodocarbyls, chlorhydrocarbyls, etc., etc. Pairlings : methyl group ; formyl group ; acetonyl group, etc. Hydropolycarbils ; oleyl group ; members—terecryl, camphyl, moringyl, doeglyl, erucyl, succyl group ; nicyl group ; hydro-nicyl, hydro-methyl, etc. Furfurol, coumaryl, anisyl, salicyl, anisol, phenetol, chinon, chloranil, benzid, tolid, xyloid, cumid ; hydrobenzid, etc. Sulpho-benzid, etc. Nitryls, imide, amide, acetamid, chloracetamid, valeramid, benzamid, succinamid, oxamid. Stibethyl, imasatin, etc. Allyl, odmyl, fersulyl, chlorpikrin, xanthogenamid, kakodyl, etc. These terms in their prefixes, suffixes, terminology, brevity and euphony, compared with many others that have been omitted are what innocent babes are to ragged desperate bandits—flower-gardens to gnarled live oaks, canebrakes, and honey-locust thickets.

Among the terms above mentioned, taken at random, not more than one, that is, *formyl*, is remembered as having a place in Professor Dunglison's Dictionary, (7th edit.) a work alike remarkable for accuracy and copiousness.

A slight examination of a small part of Professor Lowig's second group, the FORMYL, under the first division of the HYDROCARBYLS, presenting a greater amount of details than is usually found in the entire department of organic chemistry in most of our ordinary text-books, omitting tables, symbols, figures, and formulæ, may not be improper as an illustration of the "many-sidedness" of Professor Lowig's mind; although names, rather than propositions, must suffice on this occasion: FORMYL GROUP : Component—C 2 H 2 ; active molecule : formyl—C 2 H ; 29 members known, the following only are named : Acetyl, propionyl, butyryl, valeryl, capronyl, œnanthyl, capryl, pelargonyl, caprylyl, cocyl, Laurosteryl, myristicyl, benyl, palmityl, margaryl, stearophanyl, behenyl, cerossyl, cerotyl, melissinyl ; radicals of this group, with three atoms of oxygen form acids ; the lower members giving equivalent combinations with sulphur and the halogens ; others with 1 atom of oxygen form oxides ; the higher may be converted into the lower—producing derived radicals ; formylgas ; formic acid salts ; sulphide, iodide, bromide, cholide and terchloride of formyl : oxychloride, sulphochloride, iodochloride, bromiodide and formate of oxychloride of formyl ; formyl-sulphochloride-sulphuric acid ; derived radical, ahloro-

formyl, C 2 Cl; chloroformic acid; chloride, oxychloride, sulphochloride and chlorosulphide of chloroformyl; chloroformate of oxochloride of chloroformyl; sulphite of chloride of chloroformyl, sulphite of terchloride of ditto; chloroformyl sulphochlorid-sulphuric acid: PAIRED RADICALS OF THE METHYL GROUP AND FORMYL: THR ELAYL GROUP: Methylen (hydroformyl;) elayel (methyl-formyl; bisulphide, tetra-sulphide, iodide, bromide, chloride, and oxychloride of elayl; elayl-hydrosulphuric and sulphelayl-sulphuric acids; elayl-platinum; sulphate of elayl; elayl-sulphuric acid, (sulphate of carbyl;) ether-sulphite of elayl; bromide of propylen; chloride of do; valylen; bichloride of do; amylen; oleen; elaen, (including paramylen, ceten, metamylen, ceroten, and melen,) hatschetin (ozokerite;) paraffin, etc.

These terms have been selected from a vast many, all occurring in the exordium to the members of the formyl group. Let the reader multiply these terms by 29, corresponding with the 29 members proper of this family, occupying only 50 pages, and then let him multiply the whole by ten!

Among the terms enumerated, the reader will have noticed the TERCHLORIDE OF FORMYL—Fo cl 3, a clear thin-flowing liquid, boiling at 60 °. (cent.,) having the specific gravity 1.480, insoluble in water, but mixing in all proportions with alcohol and ether, that is, chloroform, the great anæsthetic—the realization of the fabled ληθη, the River of Oblivion,

> "Whereof who drinks,
> Straightway his former sense and being forgets—
> Forgets both joy and grief, pleasure and pain."—MILTON.

"Words," said Hegel, "are crystalized thoughts." Although lexicographers undertook to make a dictionary expressly to explain Kant, the greatest of modern thinkers, yet some of his plainest terms are almost constantly misunderstood; for example, the word *transcendental*, is often used to denote that which is fanciful, absurd, and incomprehensible. *Transcendental*, relates not only to the possibility, but to the origin and attainment of knowledge not strictly sensuous or empirical, but derived directly from the pure intuitions of understanding, as the infinity of space, and infinite duration, the non-existence of which is wholly inconceivable. * * * Transcendental Philosophy is not contrary to, but rises above the actualities of the experimental; it is a synthetical, not an analytical judgment; a necessary, self-comprehending, not a contingent truth; it is knowledge *a priori*, derived necessarily and directly from the understanding itself, not being *a posteriori*, dependent on experiment, and involving the idea of contin-

gency and limitation. Chemical experiment proves that animals and vegetables are composed chiefly of oxygen, hydrogen, nitrogen and carbon; not that they might not have been constituted differently. Chemists cannot say that all animals and plants in the universe are composed exclusively or essentially of these elements. Thus, empirical processes do not in any case reach the great thought of strict universality and intuitive necessity, so that their opposites cannot be even conceived among possibilities. Necessity and strict universality are sure signs of a transcendental truth, which, is more than any chemical formula can claim. Gravitation may be suspended, but space and duration cannot be limited, though all else were annihilated. Isomerism shows an apparent, nay an absolute empirical identity of atoms and proportional unity of constitutional elements, while the resultant compounds are quite different! The empirical philosophy may say, occasionally, so far there is no exception to this or that chemical law, but it cannot pronounce its laws as so many pure intuitions, bearing the sublime impress of both necessity and universality. But let us return to Prof. Lowig, who says, "The only object of chemical symbols is, to express through the formula the mode of union of the chemical elements corresponding to the reactions which have been observed in the mutual influence of the chemical combinations. By denoting acetic acid (C 4 H 3) O3, it must be assumed that C4 H3 unite as a whole with O3; this formula corresponds to all the known reactions, since O3, can be substituted by Cl3, Br3, S3, etc. The same is true of the formulæ (C4 H5) O, (C4 H5) S, (C4 H5) Cl. etc. These formulæ, in certain respects purely empirical, do not affect the question *how* the elements in C4 H3 and in C4 H5 possibly unite, first with each other, and then with O, S, Cl, etc.

Professor Lowig lays down the following weighty postulates concerning the constitution of the organic compounds, as compared with the inorganic, to which will be added several important statements illustrative of his views:

"The organic compounds which constitute the great mass of plants and animals differ from the inorganic: 1. In the small number of elements in which the former consist; 2. In the complicated atomic proportions in which these few elements in the organic are found to unite; and 3. In the impossibility of producing organic matter direct from its elements, since the co-operation of vitality, or of the other predisposing forces, is necessary to its formation."

"The difference between the organic and inorganic compounds is not

in the diversity of the elements, but in the different manner of their union. The multiplicity of the inorganic compounds is dependent upon the number of the elements, and that of the organic upon the property, particularly of carbon and hydrogen, of uniting in greatly complicated proportions. Oxygen alone enters into organic bodies, in the same atomic proportions as in the inorganic; in none of the former does it suffice for the full oxidation of the carbon and hydrogen atoms. If the chemical formula gives only the relative atomic proportions of the elements of a compound, it is called *empirical;* if it express the constitution of the compound it is called *rational.* Thus, the empirical formula for sulphate of potassa is K S O4; the rational, on the contrary, is K O, S O3. If an atom of an element unite with one, two, three, or more atoms of another, or if it give with different elements a series of equivalent compounds, it is thence called the radical of the different compounds. Thus, nitrogen is the radical in N O, N O2, N O3, N O4, N O5. The idea of a radical is indeed only relative, and one and the same element, according as it takes place in the chemical series, and as the elements with which it is united can enter as a radical, but also appear as an element in the combination in which the atom with which it is united plays the part of a radical."

"Since the elements which occur in organic compounds in plants and animals, are *carbon, nitrogen hydrogen* and *oxygen*, and the latter never enters as a constituent radical, it follows that only the first three elements, part in singular, part in binary and ternary union, can be the constituents of radicals, which are the ground work of natural organic compounds; all radicals, which are composed of the above-named elements, are therefore called *primary.* But it has been already intimated, that artificial radicals are obtained in which also other elements, and particularly the halogens—nay, even compound bodies, as N O4, enter as constituents. * * * Partial substitutions almost always occur when the primary radical consists of a great number of carbon and hydrogen atoms. All derived radicals, equally, if the substitution be partial or total, give the same compounds as the primaries out of which they arise, only the derived have a more negative character than the primary, in proportion as the replacement of H by Cl, Br, I is the more complete. All the primary radicals, and the thence arising derived radicals, like those of the corresponding compounds, form as it were a natural family."

"It is the task of organic chemistry, whether the organic compounds occur already formed in plants and animals, or are obtained artificially,

to trace back their primary, derived, and paired radicals, and to investigate the conditions under which they form and are able to be united in higher combinations. Chemistry of organic combinations and chemistry of organic radicals have thence the same meaning. Only a few organic radicals have as yet been obtained isolated; by reason of their compound nature they mostly separate by the attempt to withdraw them from their combinations into new radicals, and combinations less complex. By the use of more suitable means of reduction and electrical influence, chemists have lately succeeded in isolating those radicals most important, and whose bearing is most direct upon the theories of organic chemistry, thus establishing the fact of their existence beyond a doubt. * * * Most of the inorganic combinations can be produced directly from their elements; and analytical action can again reduce them into their proximate and their remote constituents. In the same manner the organic cannot be produced. The formation of the latter results by decomposition of inorganic compounds: 1. By the vitality of plants under the co-operation of light; 2. Without the co-operation of vitality. * * * The decomposition of organic compounds without the influence of chemically reacting bodies follows: by electrical, calorific, fermentative, putrefactive, and vital influences."

Professor Lowig, notwithstanding the multiplicity of his formulæ (generally unaccompanied by explanations, practical processes, or the elementary numerical proportions in the constitution of special substances) seems, nevertheless, deeply impressed with the importance of a philosophical classification by which organic substances may be individually characterized and associated in groups agreeable to such laws as relate properly to each group, as well as to the whole. He says, "In this work, as in the 'Chemistry of Organic Combinations,' the theory of the organic radicals forms the basis of the systems. I have gone only one step further, in considering the radicals no longer as a collective whole; for I distinguish in them an active compound controlling part from some more passive components. By this means it was possible, with fewer elementary substances, to combine not only a great part of the organic compounds, in a manner at once simple and corresponding to facts, but also to discover a cause for the different chemical relations of the radicals. For example, there were the phenomena of substitution, i. e. the entrance of hydrogen through the *halogens*, which I sought to harmonize with the theory of the organic radicals; by this, without my wishing or seeking it, to a certain degree a union between the radical theory and the nucleus theory was revealed.

* * * How the elements in a chemical compound really do unite with one another; in what the act of chemical combination really consists, whereby the entire change of qualities of the elements in their union is controlled, are questions to which a positive answer can never be given. It is alone what is chemically created—chemically completed—that is subject to observation. The *how* of these creations, in inorganic as well as in organic nature, is entirely concealed from our view; and, in this respect, all microscopic investigations have brought nothing to light. Therefore, also, the questions regarding *atoms* is immaterial; the fact is, the chemical union of substances take place in certain atomic proportions, and the word *atom* is nothing more than an expression of this fact. * * * The organic compounds, like the inorganic, separate into groups, whose individual members are distinguished by common characteristics; but whilst the difference of chloric, bromic, and iodic acid from potassa, soda, and lithia, is caused by the different quality of the elements, the variations in character of formic, acetic, and propionic acid from wood—spirit, alcohol, and amyl-spirit, depends upon the difference in quantity of the same elements, and all new investigations have led to the same results. * * * In these *Principles*, it is quite apparent that each speciality cannot be considered. Should any one miss a few of the latest discoveries, it may serve as my excuse, that the greater part of the labor of this work was performed in 1850; the General part, especially, was written long before Kolbe made known his views upon the constitution of organic compounds. Indeed, I fear more complaint will be made of the *too-much*, than of the *too-little*. And now little volume go forth into the world, and give friendly greeting to the chemical public.

LOWIG.

Zurich, October, 1851." (XI. XII. XV. XVI.)

So saying, Lowig shot forth his arrow into the air. Its own inherent impetus, with favoring currents, bears it on from the Rhine to the Mississippi, from the Blue Mountains of Zurich, to the gold-shimmering vales of California.

Without having carefully studied every page and tested every formula in this work—without asserting that the author has given a full and complete enunciation, physical, histological, and chemical, of all the processes and principles characterizing organized beings in all their

physiological and pathological structures, phases, metamorphoses, functions and conditions, an opinion will be, nevertheless, ventured, namely, that competent judges will accept Prof. Lowig's Physiological Chemistry as a contribution to science of the highest value.

BENNET DOWLER.

New Orleans, June, 1853.

II.—*The Principles of Botany, as exemplified in the Cryptogamia.* By Harland Coultas. Philadelphia. 1853.

As a branch of natural history, few are more attractive and fascinating than the history, habits, organization and properties of the vegetable kingdom. To the traveler who roams over our interminable forests and boundless prairies, an acquaintance with the science of Botany is a never ending source of pleasure and profit; with each plant, shrub and cryptogam, he may renew his acquaintance and enlarge his knowledge of the organic world. As many of our most useful and active medicines are derived from the vegetable kingdom, it becomes obligatory upon the Medical Practitioner to make himself, at least, familiar with these vegetables, that he may know how to obtain and apply their active principles to the treatment of disease. To obtain such knowledge and to make it when thus acquired, available, we would recommend the little work before us to the profession. The subject is illustrated with elegant plates. White, 105 Canal street, has the work.

III.—*Annals of Science.* Being a record of Inventions and Improvements in Applied Sciences. Conducted by H. L. Smith, A. M. Published twice monthly. Price $1 per annum. Cleveland, Ohio.

As few works of this character are to be obtained in the United States, we can see no valid reason why the present one, so full of interesting matter to the man of science, should not receive a liberal support. The work treats of the following subjects: *Chemistry, Analytical Chemistry, Manufactures, Microscopical Optics, Physics and Miscellaneous.*

It is much to be deplored, that on this side the Atlantic, so little interest can be excited in subjects of a purely scientific character; hence, efforts to originate and sustain periodicals devoted to physical science, have not been cordially responded to by the public. We hope a better state of things may soon dawn upon our happy land.

IV.—*Character.* A Valedictory Address to the Graduating Class of the Memphis Medical College. By H. V. Wooten, M. D., Prof. 1853.

The Address by Prof. Wooten was finely conceived and well expressed; the picture drawn of a physician of "character," must have proved salutary to the graduating class, to whom it was addressed. His appeal to his class to be industrious, patient, toilsome and *firm* in their purposes, is an admirable part of the address; and it would be well, if many who have entered the world would read this portion of the valedictory and profit by it.

To accomplish anything truly worthy a man of spirit and ambition, incessant labor and sleepless vigilance are absolutely required at the present day.

V.—*Ocean Springs, Lynchburg, Miss.*

These springs, already famous for the extraordinary cures in many obstinate chronic diseases which they have effected, are now attracting much public attention; and from the facts before us, we do not hesitate to predict for these waters, greater popularity than that claimed by any in the country. At a convenient distance to the citizens, both of Mobile and New Orleans, it must, in time, become the focus of attraction for the invalids of both cities, imparting health and strength to those who may seek its pure and salubrious waters.

Accessible at all seasons of the year, and surrounded by scenery at once beautiful and picturesque, the Ocean Springs must become the "*Eureka*" of the South, at whose waters the weary invalid may quench his thirst and never die.

In this pamphlet, the original discovery of these Springs is clearly traced to the Aborignes, who flourished at that remote period. So convinced were they of the superior virtues of these waters, that they designated

them the "Holy Drink;" and whither the women and children of their tribe flocked to purify themselves of all disease. We have already, on a former occasion, enumerated some of the diseases for the removal of which these Springs have been found so efficacious. As ample accommodations, commensurate with the wants of the invalid, have been provided at these Springs, we hope those of our citizens who may require the tonic influence of these waters, will give them a fair trial.

For full particulars, certificates of physicians, and all requisite information, we refer to the classic and elaborate pamphlet, written by the ingenious late discoverer of the Ocean Springs, Dr. Austin.

V.—*The Obstetric Catechism, containing* 2347 *Questions and Answers on Obstetrics Proper.* By Joseph Warrington, M. D. 152 Illustrations. Philadelphia. Barrington & Haswell. 1853.

Dr. Warrington has long occupied a high position as private teacher of Obstetrical Science, in the city of Philadelphia; and hundreds of his pupils, now actively engaged in the practice of medicine, in various portions of the United States, give every proof, on suitable occasions, of having been thoroughly indoctrinated in the principles and practice of obstetrics.

This little volume, the author tells us, in his "*Introductory Address*," has been written for them—to revive their knowledge of the rules and precepts which the Doctor taught them during their pupilage; states candidly that he does not design it as a "*text*," but as a *test-book*"—not to tell anything new, but to enable them individually to decide upon the best course of conduct which may be safely persued, in cases of doubt and difficulty, when remote from living counsel.

The book opens with the rudiments of the science, and step by step, hurries forward the student, and finally ushers him into the *sanctum*, into the presence of the parturient female. The questions are plainly and definitely stated; and the answers follow with equal precision and conciseness. This mode of teaching the practical precepts of a science has been found quite successful; becauses it fixes on the reader's mind the well established truths of that science.

The various subjects discussed in this little volume are very well illustrated on almost every page of the work.

Finally, we can but believe that this effort of Dr. Warrington, to present in a neat and condensed form, the leading facts and principles

of Obstetrical Science, will be well received, especially by the junior members of the profession. We would direct those in want of a copy to T. L. White, 105 Canal street, New Orleans.

VII.—*What to Observe at the Bedside and After Death, in Medical Cases.* Published under the authority of the London Medical Society of Observation.

The plan of this useful little volume was originally projected by the Medical Society of Observation, soon after its formation, under the auspices of such men as Walshe, Jenner, Parkes, Beck, Hare and Sieveking. The first-named gentleman had already conceived a plan for the arrangement of the symptoms and post-mortem appearances, which was referred to a committee of the Society, who afterwards "expanded and altered various parts of it, and finally threw it into its present shape." The book aims to indicate to the practitioner the plan he should pursue at the bedside, to gather such facts as may not only lead him to a precise and correct knowledge of the case under advisement; but, also, when recorded, enable the reader to comprehend all the particulars of such cases, that useful and important statistical deductions may be drawn from the whole. Without some similar plan, by which all may be guided in the investigation of disease and their symptoms, experience, that most eloquent of all teachers, would be restricted almost entirely to the individual, and consequently a great number of curious and interesting facts would be lost to the profession.

But with this volume, as a guide, specifying all the phenomena of disease, and detailing what should be recorded at the bedside during life, and at the post-mortem, the mere tyro in the profession may be enabled to treasure up much useful information.

This work has, however, been anticipated by our honored and learned *confrère*, Dr. B. Dowler, of New Orleans; for some years since, he submitted for our inspection his memoranda on this subject, in which were detailed all the most important phenomena that required notice at the bedside and after death. Although not so minute as the book before us; yet his notes embodied all that was really important for practical purposes. In the examination of *medico-legal* cases, this work will serve as an admirable guide; and for those who wish to prepare works on clinical practice, it is indispensible.

A copy of the book should have a place in every physician's library. T. L. White, 105 Canal street, has the work.

VIII.—*General Board of Health. Minutes of Information collected with reference to works for the removal of soil water or drainage of dwelling houses and public edifices, and for the sewerage and cleansing of the sites of Towns. London*, 1852.

Of the Drainage of the Land forming the Sites of Towns—and of the Drainage of Suburban Lands. London, 1852.

As in the arts, sciences, commerce and manufactures, England stands pre-eminent, so likewise is she a model for the other nations of the earth in the strictness of her Sanitary Police and in the enforcement of those hygienic laws by which the health and happiness of great and populous cities may be best secured. As England protects the life, property and personal liberty of her subjects, so in like manner does she apply her knowledge to the amelioration of the physical infirmities to which her harsh and humid climate constantly subjects her millions of inhabitants. By the researches of her scientific medical men, the mortality in the British empire, especially in the large towns, has been greatly reduced, and judging from the evidence now before us, we have no doubt that in time this mortality will be brought down to the lowest possible figure.

Below we introduce some of the remarks contained in this "Report" in the deleterious influence of cess-pool emanations, and atmospheric impurities, on public health.

"The testimony of such medical men as have duly observed the antecedents of disease is now unanimous to the effect, that no population living amidst cesspool-emanations, or in air rendered impure by such causes, can continue to be healthy. The strong may withstand these noxious influences for a time, but even their general health is eventually lowered, and their constitutions undermined by continued exposure to such emanations, while the effect, especially when concentrated upon the weakly and susceptible, is, in certain atmospheric conditions, axtensively and rapidly fatal.

The presence of atmospheric impurity produced by the decomposition of animal and vegetable matter is now established as a constant concomitant of the excessive ravages of typhus and other epidemic diseases in towns; and a proportionate exemption from such maladies has marked the removal of the sources of aërial pollution. In proportion as perfect cleanliness has been obtained in prisons, the gaol fever has ceased to exist; and a comparative exemption from the entire class of zymotic diseases has followed the progress of purification in every description of habitations.

These sanitary views are in conformity with great primitive religious ordinances, both for household and personal purification. For instance, by the law of Moses it was forbidden that even the open camp should be defiled with

human ordure, which it was expressly ordained should be deposited at a distance, and immediately covered with soil. Many of the positive observances directed by the Mosaic law had a similar sanitary purpose. The ultimate object of the chief engineering appliances for the sanitary improvement of towns is precisely the same as that of the Mosaic ordinances for the preservation of the camp from defilement ; and the result of those engineering operations will be the practical fulfilment of the Mosaic regulations for the cleanliness of the person, and for the cure or removal of the "leprous house."

The habits of a people with respect to cleanliness, and more especially with respect to their care to protect their habitations from pollution by excrementitious matter, are a clear indication of their progress in civilization. Archdeacon Paley was accustomed to direct the particular attention of travellers in foreign countries to the mode in which the people dealt with their excreta, stating that from this single fact a greater insight might be gained into their habits of cleanliness, decency, self-respect, and industry, and in general into their moral and social condition, than from facts of any other class.

It is a deplorable proof of the want of information and of due appreciation of the circumstances on which the improvement of the moral as well as the physical condition of the population depends, that the existence of filth in houses and towns, the prevalence of filthy habits among the people, and the efforts to remedy or mitigate the attendant evils, are often treated with unconcern, as if the subject was of no consequence, or fitted only to excite disgust. It may not be needful that persons on whom no duties in this behalf devolve should enter closely into the details in question, but it is incumbent on the local administrators of the law for improving the sanitary condition of the people to show by their manner of dealing with its provisions that they regard them as the means of fulfilling, and as being, in fact, when completed, the practical fulfilment of the primitive ordinances for personal and household purification ; and it is important that they should treat even the minutest and most repulsive details with the like scrupulous and anxious care with which physicians deal with the most offensive particulars attendant upon sickness, suffering, and mortal disease."

Should strict attention be given to the foregoing suggestions in relation to public cleanliness, we apprehend the mortality from the class of zymotics would be greatly reduced in our crowded and populous towns. In this country and especially in New Orleans, nothing is regarded as prejudicial to health, that is not positively offensive to the olfactories ; and hence the draining and cleansing of our gutters, streets, etc., are left to our periodical deluging showers which frequently visit us most opportunely, and save us perhaps from the ravages of frequent endemic and epidemic diseases. The effects of a thorough system of drainage, in promoting the health of towns, have long been admitted, and recently fully tested.

The works before us indicate in the most scientific and precise man-

ner, the mode by which soil and particular localities may be most effectually drained; and as this subject is exciting some interest in this city (New Orleans) we would recommend our authorities and the officers of our draining companies, to study its theory, and test as much of it as may apply to our particular locality and soil. The good that has already resulted to our city from the partial drainage of a portion of the swamp lands lying between the old and new canals, induces many of our intelligent citizens to believe, that if the entire district, comprised between the city and the lake, were thoroughly drained and ditched, the yellow fever, now ascertained to be a preventible endemic, would be forever banished from New Orleans. In view of these facts and surmises, does it not become the duty of our authorities, at whatever cost, to give their almost undivided attention to this important subject.

Will we hesitate to take the necessary steps in this great work, until another epidemic shall fall upon us, and sweep from our midst hundreds and thousands of our unacclimated population? Let the authorities recall the memorable summer and fall of 1837; let them visit in a body the various cemeteries in the suburbs of the city, and enumerate those who were cut off in the meridian of life, leaving behind them, in hundreds of instances, orphans and widows, destitute of the comforts and means of life; thus taxing our charitable institutions far beyond their exchequer. By and by, when the Angel of Death doffs the yellow robe, in which he whileome clothed himself, when about to pay us a visit, appears in our midst, to claim his victim, and to rupture the ties of blood and kindred, then it will be too late to remedy the evil—and we shall have only to bear with fortitude and christian resignation the calamity that might have been averted at an earlier day. We regret, that we cannot make room for some extracts from these valuable Reports.

IX.—*The Transactions of the American Medical Association.* Vol. 5. Philadelphia. 1852.

We have already given a synopsis of the proceedings of this Association, of which this volume is the offspring; and we now take much pleasure in directing the attention of the profession, to the large amount of valuable matter embodied in the several Reports of the various Standing Committees. A large delegation was in attendance, at Rich-

mond, Virginia, to take part in the deliberations of this Association; and the first question, after the preliminaries were closed, had reference to the "Amendments of the Constitution." This subject was discussed at considerable length by Drs. Hays and Storer; but as the amendments proposed have doubtless been scrutinized by the profession, we omit any further allusion to the suggestions contained therein.

The first paper that meets the eye is the Prize Essay, by Dr. Flint, of Buffalo, "*On Variations of Pitch in Percussion and Respiratory Sounds.*" As we have already expressed our high appreciation of the merits of this effort, we deem it useless to reiterate this conviction expressed on a former occasion. We, therefore, pass on to the second Report, "*On the Blending and Conversion of Types in Fevers, by Prof. Dickson, of Charleston.* This is a fruitful subject, and might be extended into volumes; but the able Charleston Professor has displayed in the selection of his materials and in the arrangement of his argument his usual ability and learning, bringing within a small compass all the striking features of this subject. He recognises the following distinct types of fever.

1st. The periodical, including (after Bartlett,) the intermittent, remittent, and congestive.

2d. The continued, comprising typhoid, true typhus, simple fever, ephemera, febriculæ, British epidemic fever, relapsing fever.

3d. The exanthematous, variola, scarlatina, measles, dengue.

4th. Yellow fever, the "hæmagastric pestilence" of Copeland, causus of Mosely, typhus icterodes of Cullen, malignant remittent of Rush.

5th. Catarrhal fever, known, when epidemic, as influenza. All fevers of a malarial or miasmatic origin may be convertible—the one into the other; may be blended, the one with the other; but in what manner this can occur in the "exanthemata," we are not told; nor are we persuaded that such a thing is practicable. That the one may supersede or be substituted for the other, no one can for a moment entertain a doubt; we behold at one and the same time, and in the same subject, either the diffuse blush of scarlatina, the rash of rubeola, or the pustules of variola; and it were as reasonable to say that two bodies can occupy the same space at the same time, as that either of these three eruptive diseases can exist in a distinct form at one and the same time, in the human subject. Moreover, the specific character of the poison of these respective diseases, precludes all possibility of being *converted* one into the other; and for the self-same reason, they cannot be made to *blend* together, like oil and water, when thrown together; the one

rises above the other—they have no affinity for each other. For these and other reasons which might be given, we, with becoming deference to a distinguished writer, think the exanthemata make an exception, and might have been omitted in the list.

The 3d Report, "On the action of water on lead pipes, and the diseases proceeding from it," is from the pen of Dr. Horatio Adams, of Massachusetts. This is, we think, the most elaborate and practical essay in the "Transactions;" and the author considers—1st, the action of water on lead pipes; and 2d, the diseases proceeding from it. He then proceeds to analyse this subject in all its bearings; states what no one would deny, that pure water exerts no effect on pure metallic lead; but as such fluid is not found in nature, he must consider its behaviour with this metal as we meet with it in our cisterns, wells, etc. As he sets out with the broad proposition, "that all natural water acts on lead," producing either an oxide or a chloride of that metal, both of which are more or less soluble, the chances of introducing this metal into the system becomes greatly multiplied; and hence the frequency of lead poisoning where proper precaution is not adopted to guard against it.

But we cannot follow up this interesting subject—a subject which touches upon public hygiene and is of vital importance to every large community.

Of the diseases proceeding from lead, Dr. Adams enumerates a truly formidable catalogue, giving in many instances cases, illustrative of the form of lead-poisoning under notice. On this point he avails himself, to a large extent, of the labors and observations of others; awarding much credit to Dr. Fenner of this city, for his efforts to direct public attention to the deleterious effects of leaden pipes in soda founts, hydrants, etc.

The 4th Report, "On water and its uses in Surgery," by Dr. Pope, of Missouri, is perhaps not inferior in merit, as an elegant production, to the one just noticed; but as it has already received some comments at our hands, in a former issue, we shall simply state that it deserves a careful perusal.

The balance of the "Transactions" is filled up with reports on the Epidemics of the different States of the Union, Medical Botany, etc.

X.—*Elements of Health and Principles of Female Hygiene.* By E. J. Tilt, M. D., Senior Physician to the Farringdon General Dispensary and Lying-in Charity, and to the Paddington Free Dispensary for Diseases of Women and Children.

Health is not inaptly termed the greatest blessing of life, since without it the fairest and richest of earthly gifts fail to awaken in us the highest zeal and the most undaunted energy. The valetudinarian may vainly sigh for pleasures he once enjoyed, for pursuits in which he eagerly engaged, for activity of mind with the power of endurance, and the invigorating effect of athletic exercises, of the *physique*—that "sterner stuff"—fail to sustain our wonted energy, our harps are indeed hung upon the willows, and momentarily catching the inspiration as we awaken to some dulcet sound, or as the stimulus of duty or of profession spur us on, we find it but delusive hope. Yet how few prize this great jewel sufficiently—at any rate not until sad experience has taught its value, and with some it may be opined in the words of Byron, not

> "Till hope has fled
> And passion's over."

Learned Doctors may give curtain, or other lectures, to their truant patients upon their imprudencies—Doctors Johnson, Smith, Tilt, etc., may write upon the salubrity of certain localities, and the mephetic atmosphere which pervades others—they may tell blushing maidens of sixteen that they should not marry, and amuse immature mothers with dietic scraps—yet until the head and front of offending be modified, reformed—until the management of early childhood be judicious, and the training of youth salutary, we shall have to deal with *more* than the "elements of health," excellent though they may be as coming from the hands of Dr. Tilt, we shall have to *enforce* the remedy; whether by moral suasion, or definitive laws (which latter mode is perhaps impossible) we shall not now stay to consider, but proceed to the highly interesting book before us, which is replete with sound sense and mature judgment.

It may be well to state, *imprimus*, that with the health and diseases of man, Dr. Tilt, in the present volume, has dealt little, "the constitution and affections which are peculiar to woman," being a more inviting pathway for the pleasing style and facile illustration of the author.

As might naturally be expected, before considering woman as she is, in the plenitude of heaven's choicest gift to man, with the moral elements of her nature fully developed, and exercising a potent influence

in the great drama of life, now enforcing her opinions by a commanding intellect, and then alluring by the harmonious blending of the softer emotions, we are carried step by step, from the cradle to the meridian of life, and refreshed at its several stages by the deliberations of a careful observer.

In the tender age of childhood, Dr. Tilt is a strict and judicious disciplinarian, advising early lessons and regularity in the hours allotted to sleep, with which neither tears nor anger should in the least interfere, and giving a salutary caution to mothers not to allow their infants to sleep in the nurse's bed, in the following language:

"Wishing to ascertain how many infants had been overlaid during the space of the last year, we made inquiry at the office of the Registrar-General, and we ascertained that in the year ending with the last winter quarter, 129 infants had been returned as *overlaid*, or suffocated in their nurse's bedclothes. Amongst the lower orders this occurrence generally took place on Saturday night, and may doubtless be attributed to the state of intoxication in which one or both parents may have been—a fact which was proved from some of the cases by a coroner's inquest."

When we reflect upon the faulty management of children, by over tender and injudicious mothers, whose sense of propriety consists in excessive indulgence, and whose care is exhibited in a surfeit of their appetites, we have more reason to wonder that they survive the ordeal, than that they should sink under the mistaken kindness. We again quote from our author:

"It is difficult to make a young, inexperienced mother understand that the crying of a child does not always indicate that it is suffering; much less hungry: and it has been truly said that "it is a great mistake to treat crying as an infallible sign of an empty stomach. New as the infant is to the surrounding world, he shrinks instinctively from every strong sensation, whether of heat or of cold, of pressure or of hardness, of hunger or of repletion. His only way of expressing *all* disagreeable feelings is by crying. If he is hungry, he cries; if he is over-fed, he cries; if he suffers from the prick of a pin, he cries; if he lies too long in the same position, so as to cause undue pressure on any one part, he cries; if he is exposed to cold, or any part of his dress is too tight, or he is held in an awkward position, or is exposed to too bright a light or too loud a sound, he can indicate his discomfort only by his cries: and yet the one remedy used against so many different evils is not to find out and remove the true cause of offence, but to offer him the breast! No doubt silence is sometimes obtained by the apoplectic oppression of a stomach thus distended, but no sane being will seriously contend that such quiet is really beneficial, or is such as any mother ought to content herself with procuring.

"It is, indeed, no less a mistake to be over-anxious always to put an immediate stop to crying. To a considerable extent, crying is an intentional provision of nature, and is called into play by every new sensation of any force. It is only when often repeated, long-continued, and evidently caused by suffering, that it is detrimental."

If the mother be troubled by an excess of milk—if by overflowing it saturates her under-garments, the too abundant supply may be moderated by taking cooling medicine, and by diminishing the quantity of drink.

Too long an interval must not be left between the periods of suckling the child ; certainly not more than two or three hours ; for the secretion of milk is one of the simplest of all secretions, and greatly influenced by the stimulus of the child's sucking ; or less milk is secreted, and it is of a poorer quality, when the child is put to the breast at too long intervals.

Dr Donné relates that he profited by this circumstance to prevent the milk of a lady, remarkable for good health, giving rise to continued intestinal derangements in her infant. He had remarked that the milk was extremely rich in fatty globules, so he advised the lady not to give the breast so often, which had the desired effect. It is also well to bear in mind that the milk first drawn is much poorer than what comes last, and that when two children are suckled by the same nurse the proverb is reversed, for the first served is the worst off. If we were guided by the laws of the creation, it would seem that the nurse's milk should be sufficient for the child until he is provided with teeth. It is so amongst animals, and it is not very uncommon amongst the poorer classes to give no food except the mother's milk until the child is twelve or even eighteen months old. The prevailing custom, however—a custom from which we see no reason to depart—sanctions giving to the infant a small quantity of arrow-root or gruel, two or three times a day, after the first months of lactation. If these additions to the child's food do harm, it is perhaps to be accounted for satisfactorily by taking into consideration how difficult it is to get even simple things prepared with cleanliness, and without permitting ourselves to be scared from our food by the hobgoblin insects depicted in the "Lancet," and afterwards reproduced in "Punch," and with the firm conviction that, if the parent can digest oysters, the infant can do the like for microscopical animals, it is still fair to believe that adulterated arrow-root, or bread mixed up with adulterated milk, sweetened with adulterated sugar, and seasoned with a dash of acidity, owing to the neglect of cleanliness on the part of the nurse, cannot be the very best food for infants, and helps to explain the frequency of their stomach disorders."

The facts set forth in the following table do not sustain the popular opinion that injury is done to the child if allowed to continue at the breast after the appearance of the catamenial flow:

"Having carefully interrogated 100 women in whom the monthly flow had returned at different periods of lactation, we learned that the quantity and quality of the milk remained the same at that time in	45
(The thriving of the children was our estimate of the quality of the milk.)	
That it was diminished at the time in	8
That it was completely arrested in	1
That it was impoverished at the time in	5
(By impoverishment the woman meant that the milk looked like whey, and sickened the children.)	
That it impoverished the milk then and thenceforth in . . .	2
That it was increased at the time in	24
That there was a rush of milk afterwards in	15

Upon the important subject of vaccination, the advice of Dr. Tilt is, never to delay it longer than the second month, in order that the accompanying fever may be over before teething begins.

This is an earlier period than is usually recommended—from two to six months being the time generally chosen for the performance of this

protective process. The reason assigned by the writer for selecting the second month is however valid; he also recommends upon the authority of fourteen gentlemen connected with the National Vaccine Establishment, to vaccinate in three different spots in each arm, "in order that, by a greater saturation of the system, it may be effectually protected." And observes:

"This is not only the opinion, but the practice of most of these gentlemen, whose experience of this matter is of the greatest value, and it is warranted by the fact that when, in after years vaccination is performed, it only takes in a very small per centage of those who present six good scars on the arms, while it follows its usual course in a large number of those who only offer one, two, or three of these marks."

By the National Vaccine Establishment, in England, which went into operation in 1840, much good has been accomplished; although the system, in some respects, may be defective; for instance, the institution supplies lymph, but "has no power to secure vaccination;" and the Poor-Law Board, which is charged with the administration of the act, has no means of knowing where vaccination is neglected till the end of the year, when cases of small-pox have occurred and carried off its victims.

The defect in the system might be remedied by instituting some stringent mode of punishment for parents who neglect to have their children vaccinated.

It is by the lower classes chiefly that vaccination is neglected, and that too, when by the aboved-named institution it is gratuitously offered.

Mr. Wakely, the accomplished editor of the London Lancet, some years ago, introduced a bill into Parliament, which was passed, rendering it penal to inoculate for small-pox. If the bill for the National Vaccine Establishment had made vaccination obligatory, as in France, Germany, etc., we should not be presented with such frightful bills of mortality.

In some of the Unions, according to the report of the Poor-Law Board, not more than 2, 3 or 4 per cent. are vaccinated by the appointed persons; the highest number being 21 per cent. out of 21,598 births, in 13 Unions in London.

That part of the "Elements of Health" which treats of food, bathing and clothing might be read with much profit by mothers and nurses; as upon proper attention to these internal and external appliances mainly depend the healthfulness or the sickness of offspring.

As all cities have their wet days and muddy streets, we give the words of the author for the benefit of those ladies on this side the Atlantic, for whom the "war of elements" and the dash of mud, has

no restraining influence upon their out-door propensities. We have now passed infancy, and are now verging upon womanhood—the womanhood of Dr. Tilt's book.

"When however, girls are promoted to the dignified "long clothes" of womanhood, there should be a clear understanding as regards what they are to do with them when walking in the muddy streets of London, or in the equally clogging lanes of the country. In other words: as 178 days out of the 365 of the year, are in London wet under foot, what is then to be done with the long petticoats and dress? In respect to walking, ladies may be divided into three classes:

I. Those who never raise the dress, but walk through thick and thin, with real or affected indifference to mud. These are generally country ladies, who have never been abroad, and but little in town.

II. Those who raise the dress, but allow the mass of under clothes, like the mud-carts in Regent-street, to collect the mud and beat it up to the middle of the leg. This class is most common.

III. Those chosen few, who without offending the rules of modesty, which of course must take precedence of all others, know how to raise both dress and petticoats, so as to protect both."

Having disposed of both dress and petticoats, and having raised the query, "Is there anything indecent in showing a neatly-dressed ankle?" which we imagine, the author's good taste made it needless for him to answer; the reader at once falls upon another article of *feminine attire*, in the shape of *stays*, to which, on account of the important part they are made to perform in the economy of many women, several pages are devoted; from which we find, "that between the ages of 15 and 30, the very time that tight-lacing is most employed, the deaths of females are more by 13 per cent. than those of males."

The stays of the most approved construction, and which gained the only prize for that article at the Crystal Palace Exhibition, under the direction of Professor Roux, Lawrence, etc., are from the establishment of that caterer to the female form, of Mrs. Caplin, of Berners street, London.

The subject of marriage is rationally treated; we shall content ourselves with one extract, and pass on to some of the natural consequences of that state.

"Marriage should be emblematic of the union of mind to mind, and heart to heart. It is well to build matrimonial happiness on physical sympathy, better still on the sympathy of heart responding to heart: but the mental adaptation, and a similarity of views relative to the grand principles of action and the events of society, should also be taken into consideration; for the bodily perfections must fade, the ardour of affection may cool or be diverted into another channel, but the mind's fixity of purpose is more to be depended upon, its energies diminishing but slowly with increasing years. A marriage founded upon this mutual understanding has little chance of being wretched; both parties ever finding the self-same mental beauty they once admired, and constantly deriving from each other the benefit of mutual interchange of thought, live

together as monitors, their two beings become indissolubly chained by habit, and they really form but one personality, though having, it is true, a masculine and a feminine side."

"In those who are predisposed," says Dr. Tilt, "to consumption, pregnancy becomes a determining cause of the spread of disease in the lungs; so it is in nowise prudent to rest on the belief that the work of consumption is by nature kindly suspended during pregnancy."

The selection of climate for consumptive persons is of vital importance, and upon which medical authority has strangely agreed to differ, arising, doubtless from a desire in all to render a ruthless disease in some measure amenable to its influence, and stay the onward progress of a malady that decimates thousands; slowly, yet surely, assailing the lusty, on whom, in imagination, the rude winds of heaven might play and still leave scathless—and securing in its mission the tender flower that blossoms—too soon to fade. Truly Dr. Tilt remarks, that "equability of temperature is even more important than warmth of climate, for phthisis is less frequent in India, and in the Polar regions, in Stockholm and St. Petersburgh, where the temperature is uniform, than in London, Paris, Rome, Holland, or Florence, where it is proverbially changeable."

The author does not agree with the fashion of sending consumptive patients to Montpelier and the South of France, nor to winter at Nice, Naples, or Rome, where cutting winds and drenching rains follow in quick succession.

We give the writer's concluding passage on this part of his subject:

"If consumptive patients are to be sent abroad, there are three places to which they can rationally be sent: the one is Palermo, the second, Madeira, and the third, Egypt. Each place is remarkable for the equability of its temperature, and for the absence of sudden transitions from heat to cold. Steam has placed Palermo at an easy distance from our shores, and it is surprising that so few of our countrymen have taken advantage of its beautiful climate. Without having those temptations which render a sojourn at Rome so dangerous to an invalid, the town presents an interesting combination of Saracen and Norman remains with Italian structures. The neighbouring country is splendid, and excursions are rendered easy to invalids by a regular line of steamers, which make the circle of the island, stopping at all points of interest. The inland excursions are also very picturesque. Living is cheap, and those who prefer an hotel, will find in the "Trinacria" one of the best in Europe. — The Romans of old used to send their consumptive patients to Egypt, and the example is worthy of imitation, for the climate of Cairo is very temperate during the winter, and the town has excellent hotels, and a variety of sights, sufficient to give it interest as a place of residence; while the excursions up and down the Nile, to Jerusalem, and the Holy Land, are as interesting as they are conductive to health."

Numerous books are now presented to the general reader, with the view of imparting a knowledge of the laws of health, yet we are forced to the conclusion, that whether read by the damsel of sixteen, or the matron of fifty, the rules contained in them are lightly regarded in no instance is this more evident than in the management of pregnancy, and in the lying-in room. If the minds of women were properly prepared for the pains of labor, unnecessary alarm quieted and patience and resignation earnestly sought, terror, confusion and dismay would give place to firm resolve and sure confidence—instinctive alarm would be diminished, and pain with parturition be regarded as natural as that day should follow night; or as that the gently falling dew should refresh the earth.

The danger of parturition is well expressed in the words of the author, the low rate of mortality may well give comfort and consolation to those whose joy is at hand.

"Mysterious to all thought,
A mother's prime of bliss,
When to her eager lip is brought
Her infant's thrilling kiss."

"The danger of death during confinement is very small; for even when women have long wanted the common necessaries of life, and are thus forced by poverty to leave loved, familiar faces, and enter a lying-in institution, the mortality amongst them is not much more than 1 per cent., and it is certainly less amongst those attended at their own homes, and in easy circumstances.—Let all, then, be comforted at the prospect of the joyful conclusion of their trouble: for "a woman, when she is in travail, hath sorrow because her hour is come; but as soon as she is delivered of the child she remembereth no more the anguish, for joy that a man is born into the world."

We must here close our remarks upon the "Elements of Health," under the conviction that the desideratum which the author intended to supply, in the affections of women and children, has not been idle speculation, or vague theory.

The book is for sale at White's, Canal street.

G. T. B.

New Orleans, June, 1853.

XI.—*A Report on the Health and Mortality of Memphis, Tennessee, for* 1852. By C. T. QUINTARD, M. D.

This is an admirable Report, full of useful suggestions, and giving all necessary statistical details in relation to the climate, health and mortality of one of our most flourishing Southern cities.

From a *table*, drawn from the records of the "Board of Health," we learn that the deaths from the prevalent diseases were as follows: Cholera, 74; Diarrhœa, 43; Consumption, 57; Pneumonia, 29; Typhoid Fever, 40; Inflammation of Brain, 30; Convulsions, 25; Congestive Fever, (chill,) 26; Measles, 41; Unknown, 111. The total deaths from these and other diseases, enumerated in the table, make for the entire year, 705; but Dr. Quintard does not think the records of the Board of Health reliable. Memphis has never been visited by an epidemic of yellow fever, says Dr. Quintard, notwithstanding Dr. Drake, in his great work on the diseases of the Valley of the Mississippi, asserts that it prevailed in Memphis, in 1828.

The municipal records, as far back as that date, make no mention of any such epidemic, and hence, Prof. Q. concludes, the late Dr. Drake was misinformed on that subject, but if he will inquire of his colleague, Dr. Shanks, he will find that there was some yellow fever there at the time mentioned, and distinctly traceable to a local cause.

Had we space, we should make valuable extracts from this interesting Report; but we must close these brief remarks; not, however, without returning our thanks to Prof. Quintard for a beautiful copy of his able Report.

Part Fourth.

MISCELLANEOUS MEDICAL INTELLIGENCE.

1.—*Does a Discharge of Blood from the Ear necessarily indicate a Fracture of the Cranium?*

In a diagnoctic point of view, the determination of the above question is highly important; and Velpeau has recently (*vide Revue de Therapeutique, Janvier*, 1853,) thrown out some valuable suggestions on this point of surgical practice.

A man, who had fallen from a ladder, came under the care of M. Velpeau; the fall was so severe as to cause the loss of consciousness for three hours. Sensibility, with the power of motion and intelligence were then recovered; after which nothing strange developed itself; but a phenomenon usually more grave manifested itself at the time of the fall, viz: a profuse hemorrhage from the right ear. A fracture of the base of the scull may be perfectly cured; sometimes more readily than many other fractures, because they are not ordinarily attended with any displacement. But is it impossible to conceive of a more or less profuse hemorrhage from the ear without a solution of continuity of the bony structure?

M. Velpeau answers this question in the negative, and says—a violent blow or shock imprinted upon the cranial vault, may be communicated to the internal ear—producing thereby a rupture of the drum of the tympanum. A too sudden shock to the assicula of the ear gives rise to a discharge of blood internally without a fracture of the temporal bones. In all such cases, however, he advises us to prescribe such a treatment as might be adapted to such a contingency.

The patient mentioned at the beginning of this article, probably, had a fracture of the base of the scull; he was treated on this supposition; he was dieted; bled several times, and actively purged. Although he possessed his intelligence, and had neither delirium nor other cerebral excitement, yet a

strictly antiphlogistic treatment was adopted, more as a matter of precaution, than to combat actual symptoms.

In all serious shocks to the brain, notwithstanding the speedy recovery of the patient from their immediate effects, the patient should avoid all causes calculated to excite the brain, or stimulate the circulation.

(*Ed. N. O. Med. Jour.*)

II.—*Death by Chloroform.*

A death recently took place at the Hospital d'Orleans by the inhalation of chloroform. The subject was a soldier, who was operated on by M. Vallet, the surgeon in chief, for a lapsus of the face. He expired after inhaling the vapor about five minvtes. The Minister of War ordered an inquest over the body.

(*Revue Med. Chirurg., Fev.* 1853.)

III.—*Gutta Percha dissolved in Chloroform.*

A physician in Bamberg, Dr. Rapp, recommends for surgical purposes a solution of gutta percha in chloroform, in lieu of collodion; the proportions are, one part of gutta percha to eight or nine of chloroform. This preparation is more adhesive than collodion, and may be spread with greater facility than this latter article, over surface, by means of a pencil or brush. Besides, collodion will not adhere to the surface when moist; when the solution of gutta percha in chloroform is not at all affected in its adhesive properties by any degree of moisture. (*Ibid.*)

IV.—*External Use of Ipecac.*

The *Bulletin de Therapeutique*, (Revue Medico-Chirurgicale, 1853,) contains a paper by Dr. Detroux, on the external use of Ipecacuhana as a valuable counter-irritant. The pommade of Ipecac possess the same advantages, says Dr. D. as the ointment of Tartar Emetic, provided that it be incorporated with some fatty body, and be then rubbed over the surface of the part sought to be irritated, for a few minutes. Thus employed, it will speedily develope a characteristic exanthema in the shape of small papular elevations of an intense rose color, very numerous, and often curfluent. Soon they assume the form of genuine pustules, but small, depressed in the centre—umbilicated—inclined to suppurate, and soon disappearing if the ointment be not renewed.

It causes much less pain, and leaves behind smaller cicatrices than the Tartar Emetic Ointment. He uses the following formulæ:

℞	Pulvis Ipecac,	1 part,	
	Ol. Olivæ,	1 "	
	Axungia,	2 "	M

Fit. ungt.

It is well adapted to children and delicate nervous subjects, on whose system the Tartar Emetic would be likely to produce evil effects.

V.—*Phlegmasia Dolens relieved by Cold Water.*

Dr. A. Hester:

Dear Sir—The following case, isolated as presented, may not be of very great interest or importance, either to yourself or the readers of your most excellent Journal; for which reason I would not even claim for it a place among the many instructive productions found on its pages. If I succeed by the report of this case in eliciting from you or your contributors an opinion in regard to the treatment adopted, my object will be fully accomplished.

And as the object of an editor is to collect from every possible source, materials out of which to form conclusions and to arrive at *facts;* which, when expressed to the profession, we feel under obligations to receive as such, founded upon true data; perhaps this may be of some service in making your collection of cases.

On the 2d day of April, 1852, I was called to attend Mrs. A. in her first confinement. The labour progressed rapidly, and she was soon delivered of a fine, healthy child, with no untoward symptoms except excessive flooding, which, after great prostration was arrested by the ordinary remedies. She was doing well up to the 9th, when she complained of a pain, which she described as a "cramping pain," in the calf of the right leg; this continued to increase during the following twenty-four hours, until it became of the most excruciating character; then successively the thigh, groin and hip became affected, the pain becoming more severe as the disease advanced; at the same time the limb was hot and swollen; in short, I might say, that there were present all the symptoms of a veritable case of *Phlegmasia Dolens*, perhaps, more properly termed Crural Phlebitis, commencing as it sometimes, but not frequently, does at the lower instead of the upper part of the limb. This case was treated in the ordinary way with the exception that general depletion was not resorted to, which was inadmissable, on account of the great debility occasioned by excessive flooding at the time of her accouchment. The remedies seemed merely to act as palliatives, without checking the progress of the disease; for on the 20th, the same symptoms began to make their appearance in the left leg that had been complained of in the right.

Being satisfied that if my patient was to suffer again what she had just passed through she must certainly succumb (for it had already become necessary to use stimulants pretty freely,) I determined upon a different course of treatment. I ordered a tub of the coldest spring water, directing it should be constantly poured upon the left leg for half on hour, after which wet cloths were to be applied for the same length of time. These applications were made to the whole limb, for the thigh had now become affected.

The next day my patient informed me, that the limb to which the water had been applied felt much better, though it was still very painful, and I discovered on examination, that the redness along the course of the vessels and swelling had somewhat subsided. The right leg was still painful. I directed the same application to both limbs to be repeated, at least twice during the day; which was again followed by very great relief. Indeed, it was only repeated for four successive days, when the inflammatory action had entirely subsided, and my patient was free from pain. It is unnecessary to state that her recovery was speedy from this date.

Without comment, I leave it with you and the profession to decide upon the propriety of the indiscriminate use of cold water in such cases, before the cessation of the lochial discharge.

In this case there were no bad effects; no suppression of the discharge; but what the consequence of its application at the onset of the disease might have been I do not pretend to say. I also leave the case for the blind exultation of the Hydropathist, without going into an argument to prove that the use of water in this case is not empirical, but that it is scientific practice, founded upon the true pathology of the disease; which is essentially of an inflammatory character, whether this inflammation be seated in the absorbent or venous system.

I am very respectfully,

G. Y. L.

Mississippi, May 20, 1853.

VI.—*Case of Inverted Uterus, with Retained Placenta.*

Dr. A. Hester:

Sir—The following sketch is of a case which recently came under my observation, and which I place at your disposal if, indeed, it should merit an insertion in your valuable journal.

Mrs. S., aged 28 years, healthy and well-formed, is the mother of four children; two of whom are living; was confined last October, and gave birth to twins, both of whom died of cyanosis. She again became pregnant, and aborted on the 29th of April last, at 4 o'clock, P. M.; no one being in attendance save an aged midwife. All was thought to have gone on well. Her husband, however, becoming alarmed at her situation, dispatched a messenger

for aid; I arrived about 8 o'clock, next morning; about 16 hours after the occurrence, I found the patient in a state of quiescence, approaching almost to languor—and learned from the midwife, (whose aged experience in dressing children was by far of more intrinsic value than her knowledge of parturition, that as yet the placenta (or to use her own phraseology, the after-birth) had not been detached. Although, (as she expressed it) it had protruded beyond the *vulva*, and that she had endeavored to remove it by force of traction. I immediately made an examination, and found a complete inversion of the uterus, with a protrusion of the fundus beyond the vulva, and with the placenta strongly adhering; at which the midwife had been pulling; and probably would have succeeded in removing the uterus and appendages, had not the force exerted given pain to the patient. On close examination, I found the os uteri flaccid and yielding. My first attempt was to remove the placenta, but finding my efforts fruitless, I grasped the whole mass, and succeeded in relieving the tumor without any difficulty, which was facilitated, doubtless, by the relaxed condition of the os uteri. Ordered a weak brandy toddy and left her. I saw her again at 5 o'clock, P. M.; patient had been up to stool, when the inversion again occurred. After some difficulty, I succeeded in detaching the placenta and returned the uterus without much difficulty, to its proper position; ordered an enema, per vaginam, of solution of acitate of lead, and left her. She is now doing well, and has been up for a few days.

W. S. COATES, M. D.

Walnut Bend, Ark., May 20, 1853.

VII.—*Annual Meeting of the American Medical Association.*

We can only make room for the following brief account of the proceedings of this body, which we take from the *Philadelphia Medical and Surgical Journal.* It seems that twenty-five States were represented on the occasion, but we have to regret that our own was not. May we not hope to see the Profession in the South-West more fully represented at the next meeting, which is to take place at St. Louis. From its inception we have felt deeply interested in this Great Institution, and shall continue to do all in our power to urge its claims to the support of the Profession throughout the Union. We are astonished that there is not a greater demand for the *immensely valuable volumes*, annually published by this Association. Wonder, why a few copies of them are not placed on sale, in all the principal cities of the Union.

The American Medical Association held its sixth annual meeting at New York, on Tuesday, the 3d instant, in the Presbyterian Ghurch, Bleecker street, the President, Dr. Beverley Wellford, in the Chair. The morning was occupied by the Committee of Arrangements, in receiving Delegates from the several States. At 11½ A. M., the meeting organized, and the President welcomed the Delegates to the city. There were nearly five hundred gentlemen present.

Dr. F. Campbell Stewart, Chairman of the Committee of Arrangements and Reception, before reading the list of Delegates, congratulated the Association upon the recurrence of its anniversary. Seven years had elapsed since the preliminary meeting of the Convention, which recommended the organization of this National Congress; and, as New Yorkers, the committee indulged in feelings of proud satisfaction at the triumphant success which had attended so important a movement, originating in their State. The labors of the learned body had been most arduous, but the result had not disappointed the expectations of the friends of reform and of progress in their profession. Mighty objects were aimed at; important achievements had already been accomplished; and a guarantee was afforded of the eventual fulfilment of the desires of those whose aspirations for the advancement and perfection of medicine, led them to propose the formation of the Association, which, representing the whole fraternity throughout our wide-spread country, assembled annually, for scientific discussion, and to consult upon matters pertaining to the general welfare of the entire Faculty. The five published volumes of their Transactions afforded abundand and conclusive evidence of the zeal by which they were actuated, and the ability which characterized the scientific labors of members. The meeting for this year had been generally anticipated with peculiar interest. An unusually large attendance had been expected. Numerous papers would be presented, and valuable reports were to be rendered; much of their time would be required for the consideration of subjects of grave importance. He assured them that their advent had been looked for anxiously, and their colleagues had been desirous to manifest their appreciation of the course in which they were engaged, and their estimation of the favor conferred by the Association in selecting this metropolis as the place for holding the present session. In the name of the united Profession of New York, the Committee tendered them a sincere, hearty, and cordial welcome to our city.

Dr. S. then called over the list of delegates, and announced that a majority were present.

The Vice-President and the Ex-President of the Association were requested to take their seats on the platform with the President.

On motion, a recess of fifteen minutes was taken, to allow the delegates to select one of their number from each State, as a committee to nominate officers for the ensuing year.

Dr. Atlee, of Pennsylvania, moved that immediately after the report of the Nominating Committee, the President be called upon to read his address, but, at the request of Dr. Condie, withdrew his motion until the report of the Treasurer should be read.

Dr. Pope, of Missouri, extended an invitation to the Association to hold their next annual meeting, for 1854, at St. Louis.

Dr. Condie, of Pennsylvania, said he was instructed to invite the Association to hold their next annual meeting in Philadelphia.

Dr. Hays, of Pennsylvania, although desiring that the Association would meet in Philadelphia, was forced to give his voice for the next year in favor of St. Louis. He hoped that at their next meeting, the claims of Philadelphia would be remembered.

The motion to meet at St. Louis was put and carried.

Dr. Condie, of Pennsylvania, Chairman of Committee on Publications, submitted a Report from the Committtee, with resolutions appended, making the assessment for the present year five dollars; authorizing the Committee to decide upon the terms at which the volume of Transactions for this year shall be furnished; and further authorizing them to take such measures in relation to the disposal of the copies as they may deem expedient. Dr. Condie stated that a valuable paper would be submitted at this meeting, the mere illustrations of which would cost $1000 for printing.

The resolutions were adopted.

The Treasurer's Report showed the total receipts for the past year to be $1,905 ; paid out, $2,015 ; balance due to Treausurer, $110.

On motion of Dr. Condie, the Committee was authorized to furnish the Chairman of Committees on Epidemics with extra copies of their reports, respectively, at the expense of the Association.

The Secretary read communications inviting the Association to visit University Medical College, the Anatomical Museum, and Bloomingdale Lunatic Asylum.

Dr. F. C. Stewart presented a report recommending the admission of Dr. Marshall Hall of London, Surg. Mower, U. S. A., Surgeons Bache, Pinckney, Brownell and Simpson, U. S. N., Doctors Leonard and Betton, Florida, Hon. Dr. Bartlett, N. Y. Senate, Dr. Harris, Canada, Dr. Rodder, Canada West, Doctors McIlvaine and Pittman, American Medical Society, Paris, to participate in the proceedings by invitation.

On motion of Dr. Cox, a committee was appointed to wait on Dr. Marshall Hall, and conduct him to a seat on the platform.

The President then read a lengthy and very able address, reviewing the origin, progress and benefits achieved by the Association.

Dr. Hays of Pennsylvania, moved the thanks of the meeting be presented to the President for his elegant, appropriate and eloquent address, and requesting a copy for publication in the Transactions of the Association. Carried.

The Secretary read a resolution passed by the Medical Society of Virginia, recommending the appointment of a well-qualified chemist to analyze the most prominent nostrums of the day, and publish the results monthly in the leading newspapers of each State. Also, a communication from the President of the American Medical Society at Paris, appointing Drs. Pittman, Walton, and McIlvaine to attend this meeting.

On motion of Dr. Atlee, the Committee on Publications were directed to send a full set of the Transactions of the Association to the Society in Paris.

A communication was received from Dr. Ramsay, of Georgia, inclosing documents on a personal matter, which were laid on the table.

The Committee on Nominations reported the following officers for the ensuing year :

For President—Dr. Jonathan Knight, of Conn.

Vice Presidents—Drs. Usher Parsons, of R. I. ; Lewis Condict, of N. J.; Henry R. Frost, of S. C. ; R. L. Howard, of Ohio.

Secretaries—Drs. Edward L. Beadle, of N. Y.; and Edwin L. Lemoine of Missouri.

Treasurer—Dr. Francis Condie, of Penna.

The Committee reported St. Louis, Mo., as the place to hold the next annual meeting.

The report was adopted.

Drs. Gooch, Watson and Atlee were appointed a Committee to conduct the President elect and other officers to their seats.

Dr. Knight, on taking the chair, returned thanks for the honor conferred on him.

Dr. Atlee, of Pennsylvania, moved a vote of thanks to the late President, Dr. Wellford, for his dignified, courteous and efficient manner in the chair. Carried unanimously, the members rising.

A vote of thanks was also passed to the retiring Secretary, Dr. Gooch.

On motion of Dr. Hopkins, of Maryland, it was resolved that no member speak more then ten minutes on any subject at one time.

Dr. Steward, on part of Committee on Arrangements, suggested that the Association meet each day from 9 to 12 A. M., and from 1 to 4 P. M.

On motion of Dr. Gooch, the meeting adjourned to 9 A. M. Wednesday.

Wednesday.—We have no space to give the transactions in full for the rest of the session, but present the following abstract : Reports of chairmen of

standing Committees. Dr. C. D. Meigs on Diseases of the Cervix Uteri ; Dr. F. D. Condie, on Tubercular Diseases. Dr. Emerson, "On the agency of refrigeration, produced by the upward radiation of heat, as an exciting cause of disease." Dr. Campbell, of Georgia, on Typhoid Fevers. Dr. Atlee, of Lancaster, Pa., on the Epidemics of New Jersey, Delaware and Pennsylvania. Dr. Sutton, of Tennessee, on the Epidemics of that State. Fifteen voluntary Essays were received, and premiums awarded to two : On the Cell, its Pathology, &c., by Waldo G. Burnett, of Boston ; Fibrous Diseases of the Uterus, heretofore considered incurable, by W. L. Atlee, of Philadelphia. Dr. March, of Albany, read an Abstract of a paper on *Morbus Coxarius.*

The rest of the transactions of the day had reference to Teaching, Quackeries, conferring degrees upon quacks, the admission of the medical officers of the Army and Navy to the Association.

Governor Fish, Doctors Woods, Delafield, Anderson, Detmold, and Ex-Mayor Kingsland, entertained the delegates last and this evening.

Thursday.—Dr. N. S. Davis of Chicago, Ill., read a lengthy and able report on Medical Education, which was referred to the Committee on Publications. Nostrums : the publication of their chemical analysis by law. Quackeries, &c., were extensively discussed, and nothing definite in reference to them done. Notices of Drake, Parrish and Horner were passed.

Dr. Yandell read Dr. Gross's report "On the Results of Surgical operations for the relief of Malignant Diseases." Five hundred and sixty-six delegates were reported as being in the city. The domestic adulteration of drugs was attended to, and Dr. O. H. Edwards requested to petition Congress on the subject. Resolutions recommending Medical Colleges to bind their graduates to abide by the ethics of the Association were passed, and one to require the abrogation of the diploma, by the same bodies, on conviction of quackery, was *not* passed. Our neighbor, Dr. Zeigler, indroduced a resolution on the subject of Medical Societies appointing committees to make original investigations. We have a word to say on this matter, at a future period.

After sundry votes of thanks to the officers, medical men of New York, city of New York, &c., the Association adjourned *sine die.*

The evening was spent in Metropolitan Hall, in the enjoyment of the magnificent banquet prepared by the medical men of New York ; and the delegates sat and spoke and caroused until the sma' hours bid them gang awa—some never to see the light of life again.

VIII.—*Some account of a Fever produced by the Decomposition of Potatoes, in the village of Almont, Michigan.*

BY F. K. BAILIE, M. D.

We invite special attention to the following interesting account of a *Typhoid Fever*, distinctly traced to *malaria*, arising from vegetable decomposition. We shall extract as much of it as we can make room for, as every thing that relates to the origin of Fever must be interesting to the physicians of the South.

(*Ed. N. O. Med. and Surg. Jour.*)

It is proposed, in this communication, to give a description, as well as may be, of a disease which commenced in this place in the autumn of 1846. Up to this time, from the first settlement of the town, the diseases in this region were of the same character as those usually met with in newly settled places

in the West. The country is generally level, and well timbered with beach, maple, oak, etc.

During the summer of 1846, there was erected in this village a large building, to be used in the manufacture of potato starch. The farmers, within five miles in each direction, contracted to produce potatoes, and draw them to the factory. As soon as the middle of October, there were delivered and deposited in one reservoir, about thirty thousand bushels. The machinery for grating not being in readiness as soon as was expected, this amount of potatoes laid some two or three weeks. The weather being warm, the whole mass soon began to rot. A small proportion of them were diseased when drawn, but they were generally in a sound condition. As soon as it could be done the pile was overhauled, and not far from twenty thousand bushels removed into the yard in front of the mill. This mass was some three or four rods in extent each way, and four or five feet deep in the centre. The smell emanating from this offensive heap of vegetable matter was almost intolerable.

The fevers during the summer and the first of the fall months had been of a milder type than usual, yielding to proper treatment in a few days. During the months of November and December a typhoid tendency could be perceived in every case that occurred. It was evident that some modifying cause was operating. One man, who was engaged in removing potatoes from the mill, was attacked with a remitting fever; but about the seventh day, congestion of the brain set in, and he soon died. This case will be alluded to again.

Another case was that of a young lady, who had spent the summer some six miles from the village, in a neighborhood where fevers of the common type were prevalent, and had during the time an attack of remittent, but partially recovered before returning home. In the fore part of November she was taken with a relapse, in which the disease assumed a typhoid form. The case will also be referred to again.

During the fore part of December many cases of fever occurred, assuming a continued form, and none of the common type. As I took no notes at the time, it will be impossible to give in detail all the appearances in any one case, but will endeavor to describe, in general, most of the symptoms characterising a majority of the cases.

This disease seldom commenced abruptly with a chill, but the patient complained of an uneasy sensation in the head, back, and limbs. Many, in describing their feelings, said they thought they should have the ague. There were languor and lassitude, no distinct chilliness, but an inclination to draw up to the fire, and an indisposition to exertion either physical or mental. The tongue was found to be coated with a whitish fur, there was a disagreeable taste in the mouth, and the breath was fœtid. The state of things just described, continued sometimes a week; but generally the disease became confirmed in five or six days. Usually, at the expiration of this forming stage, a chill more or less distinct would usher in the disease, after which no more coldness would be experienced. At other times, the alternate cold and heat would gradually give place to continued heat and excitement, rendering it difficult to ascertain the exact time to date the outset.

The author gives the following synopsis of the symptoms and appearances that characterised the disease:

1. The pulse was frequent and quick. With an occasional exception, it was not less than 100 in a minute, and frequently going as high as 140 or 150 in cases that recovered. It was sometimes full, but always easily compressed beneath the finger. In the worst cases, and in feeble constitutions, it was sometimes found to intermit, which was usually an unfavorable symptom.

2. The breathing was hurried, frequently so much so as to occasion panting.

3. Morbid sweating was common, and was considered indicative of danger.

4. Diarrhœa was almost unfailing, and in many instances very excessive. Sometimes its existence was coeval with the attack, but frequently induced by the action of cathartics taken by the patient before calling medical aid, which developed the disease, that otherwise might not have occurred.

5. Tympanites was an attendant generally where there was diarrhœa, but sometimes when the bowels were torpid.

6. Cough was common, and frequently the first symptom complained of. The patient would seem to have a cold, cough a little for a day or two, when other symptoms supervened, and the disease was established.

7. Hæmorrhage from the bowels occurred in some cases, to such an extent as to exhaust the patient in a few hours. Sometimes the blood flowed unmixed, but commonly attended by morbid secretions.

8. The nervous system was in a marked degree affected in every case, more or less. Subsultus was common in the comparatively mild cases—worse, however, in persons of a strictly nervous temperament. Inability to confine the attention, indifference to surrounding objects and their own condition was apparent. But little solicitude was manifested in respect to recovery.

9. Delirium was common in the most cases, in some of which the patient would be almost uncontrollable. In a majority of cases the patient was so stupid, that it was difficult to obtain a correct answer to an inquiry. He might, perhaps, be aroused enough to answer one question, but would relapse immediately into a snoring sleep.

10. Deafness was almost unfailing, and to such a degree, in some cases, as to render it difficult to make the patient hear.

11. Retention of the urine was an occasional source of trouble, most common in females.

12. Forgetfulness of what had occurred during the disease was common in the severe cases, and in the mild the memory was confused. Unconsciousness of the lapse of time may be mentioned in this connection; and, if a patient was unable to tell at any time how long he had been sick, or could not remember the day of the week, the prognosis might be considered as doubtful.

13. The rose-colored eruption attended a sufficient number of cases to be considered as diagnostic.

14. Subsultus and hiccough also were common—the former in cases of every grade, but the latter only in the worst cases.

Respecting the nature of this disease there can be no doubt. The foregoing enumeration of symptoms render its peculiar character conclusive.

The following facts are given to show that the Fever was more or less contagious:

The smell of the potatoes was noticed little or none in parts of the village directly north from the mill, while N.E. and N.W. it was almost intolerable. The subjects were generally young persons from eight to thirty years of age. In some instances, whole families were affected. Facts may be related which would lead us to consider the disease as contagious. About the time that Miss B. (the young lady referred to) died, a daughter of Mr. K., who lived in the house adjoining, was attacked. About the same time, a sister of Miss B. was attacked, and died on the seventeenth day of the disease. A young lady (Miss F.) who assisted in the family of Mr. K. was also taken sick, and remained in the family. A son of Mr. K. was taken about ten days after the daughter, and Mrs. K. was attacked before the rest recovered. After the younger Miss B. died, and previous to the attack of the other, the family

removed to a house across the street. The wife of the man who went into the house Mr. B. had left, was attacked about the same time. Her husband also had some of the symptoms, but they were thrown off. Miss A., who went into the family of Mr. K. after Miss F. was taken sick, began to complain in about a week, and went home to an adjoining town ten miles distant. She had the disease and died. Three or four of her father's family were afterwards attacked, and a brother died. No other cases occurred in that neighborhood. A sister-in-law and niece of Mr. K., who had assisted considerably in the care of the sick in his family, had the disease, but in a mild form. They resided on Main-street, out of the range of the miasm, and were not in Mr. K.'s house more than one-third of the time. Mr. K. had some of the premonitory symptoms, but the disease was not developed. A sister of Miss F., who was with her about four weeks, was attacked and went home. She had the disease, and a brother, who resided in the family, also took it from her and died. A young man who resided in the family was attacked, and before he recovered, his father, who resided about twenty-five miles south, and came and took him home. After his arrival at his father's, some of the family was taken sick, and from thence the disease spread until the whole household (the parents and seven children) died; the one who was taken sick here alone escaping.

After the death of the second Miss B., her father, a sister, and three other members of the family were attacked, and had a severe form of the disease. Many others of the neighbors who had watched with these families, either had the disease or felt the symptoms.

The cause seems to have spent its main force in the fall and winter of 1846 '47. The fever disappeared in the spring of 1847; but reappeared in September, under a modified form.

After the opening of Spring no cases occurred until September, when the disease reappeared. In the main, it presented the same characteristics during the autumnal months, as it had during the previous winter. The principal variation was a tendency to assume a periodical form after the crisis, there being a chill, succeeded by the hot and sweating stages, as in ordinary intermittents, and susceptible of being arrested by the prompt administration of anti-periodics.

In such cases there seemed to be, in addition to the local miasm, the usual general influence tending to produce intermittents; and the typhoid tendency predominating, and expending its force first, left the system a prey to a predisposition less potent. This fact goes to prove that more than one cause may be operating in the atmosphere at the same time, each in its turn producing its effect according to its power to do so.

Such results occurred in cases where there was no organic lesion in any part, but a debilitated condition of the system, produced by the effect of the continued form of fever. When the crisis arrived, the system was in a state to convalesce; but the ordinary causes of intermittents being in operation, a paroxysm would come on as stated above. Recovery would follow rapidly after the periodic tendency was interrupted, as after ordinary intermittents.

Here we see an instance of the prevailing tendency among physicians to account for the various types of fever, by imagining the existence of a number of distinct causes in operation at the same time and place. To our mind it is far more rational to suppose that the original cause was becoming exhausted, and as it grew weaker, gave rise to modified and milder types of fever. The facts here presented, serve to show, at least, the close relationship between Intermittent and Continued Fevers.

The following facts will remove all doubt as to its being genuine Typhoid Fever:

Post-mortem examinations were made in three cases. The first was that of the young lady first mentioned. There were ulcerations through the whole extent of the intestines, from the duodenum to the rectum, the deepest being in the small intestines. The ulcerated patches were from the diameter of two lines, to that of a pin head. The intestines were found filled with black blood, and before death some quarts had passed off. She might be said to have died of hæmorrhage, as the other symptoms had not been particularly unfavorable. She, it will be remembered, died in December, 1846.

The second was in the case of a little girl, four years of age, who died in the fall of 1848. She had had whooping cough for two or three weeks at the time the fever set in, and her death was caused by congestion of the brain. The brain was filled with blood, and the lungs also were considerably congested. The mucous membrane of the small intestines was inflamed to a considerable extent. There were small elevated spots upon the internal surface of the bowels, which were of a bright red. The case was an interesting one, from the fact that we had an opportunity of examining the condition of the entire mucous membrane at an early stage of the disease, which we could seldom have done, as the fever was not sufficient cause to produce death. There was much more disease than any external sign indicated, from which we might infer that in all cases there was inflammation to a greater or less extent.

The third case was that of a young man, twenty-two years of age, who died in the autumn of 1847. He had resided during the summer in St. Joseph's County, and had suffered while there from a remittent fever. At the time he was attacked with continued fever, he had not recovered from his former sickness, but was able to be about the streets. He was not as sick to appearance as many others had been who recovered. He died on the twenty-second day from the time of attack. Half-an-hour before his death, he asked to be helped into a chair, was cheerful, spoke of his hopes of recovery, and took some food. He very soon complained of a severe pain in the abdomen, and a deathly faintness. Stimulants were administered, but he soon sunk and died in the chair. In this case there were extensive ulcerations of the small intestines, throughout the whole extent. About a foot of the ileum was gangrenous, with perforations in two or three places, the largest of which was an inch and a half in length. The intestines were diseased through the whole length to some degree, presented a livid appearance, and considerable effusion had taken place in the abdominal cavity. He had strength enough to walk across the room within five days of his death.

The author gives us nothing new or important in the treatment. He says, he "frequently knew the disease to be completely broken up by a free dose of calomel, given when the first symptoms made their appearance; but generally, mercury in any form failed to produce any good results." When carried to salivation, it did positive injury. He says nothing of the abortive treatment by quinine. This paper is somewhat of a *poser* to those writers who contend for the *specific nature and animal origin* of Continued Fever.

(*From the North-Western Med. and Surg. Journal, Feb.*, 1853.)

The New-Orleans Medical and Surgical Journal.

Vol. X.] NEW-ORLEANS, JULY 1, 1853. [No. 1.

HEALTH, MORTALITY, &c.

Measles and Scarlatina have been the chief prevalent diseases, worthy of notice, in this city, since our last publication. The junior portion of our population has suffered to a considerable extent during the last two months, from these two eruptive diseases; chiefly, however, from the measles, which has caused death in numerous instances, by involving either or both the lungs and bowels. Once it fixes its grasp upon the bronchial tubes, or the mucous membrane of the intestines, the greatest care should be exercised, and the most judicious treatment adopted to extricate the child from his perilous situation. Often parents imagine all danger is past, because the eruption has disappeared, when a new train of symptoms—consequent on some slight exposure—change of weather, or improper diet, springs up to the great alarm of the friends and serious embarrassment of the medical attendant.

We regard the sequalæ of rubeola as more destructive to infantile life than scarlatina and trismus combined; and, however light and mild may be the eruptive stage of the disease, we must continue to watch the symptoms for several days after all evidences of the primary attack shall have totally disappeared. From negligence on this point, and inattention to the condition of the infant, we have seen more than one death recently from attacks of this disease. In consequence of the prevalence, to a limited extent, of these two affections in our city, the mortality among those under 10 years of age, has been unusually large for one or two months; whilst the deaths from all other diseases have been comparatively small for our population.

We have had less than our usual amount of warm weather up to this period; and during the month of May, the mornings and evenings for the most part were quite chilly—rendering winter clothing not only acceptable, but even indispensable to comfort. The consequence of all this was some slight attacks of derangement of the primæ viæ, mascular rheumatism—and neuralgic affections.

For sometime past our public and private hospitals have been comparatively free of disease—the best evidence of the universal prevalence of public health among all classes of our population. The time has arrived when our city authorities should devote special attention to the condition of our streets, canals, sewers, and such other localities as may become the receptacle of stagnant water and decaying vegetable and animal matter. We are almost persuaded that with the enforcement of a strict and stringent *sanitary police*, we may destroy the germ from which spring all our endemic autumnal diseases. Compel then the Street Commissioner and his deputies to perform their duties with promptness and fidelity; and if deemed advisable, reorganize a

Board of Health, or appoint a Health Committee, whose duty it shall be to supervise and point out the localities deemed obnoxious to public health—and we hazard the assertion, that we shall escape every species of epidemic disease. We have again and again endeavored to fix public attention on this subject; but despairing of our high purpose, we must be content to note the results of negligence and inattention.

On the 28th of May, there was one death from Yellow Fever, with *black vomit*, at the Charity Hospital, and up to this writing, (June 22d,) there have been eleven other deaths from the same disease, at that institution, besides two or three in private practice; but we will reserve our memoranda on this subject till our next number, by which time it will be seen whether we have an epidemic or not. The general health of the city is extraordinarily good at this time.

DEATHS IN THE CITY OF NEW ORLEANS,

For the 9 weeks ending June 18th, 1853.

		Cholera.	*Fevers.*		*Total.*
1853	April 23d,	3	12		128
	30th,	10	17		137
	May 7th,	4	11		151
	" 14th,	1	15		162
	" 21st,	1	16		145
	" 28th,	0	13	(Y. F. 1.)	140
	June 4th,	1	17	" 1.	157
	" 11th,	4	20	" 4.	157
	" 18th,	5	29	" 7.	147
	Total,	29	150	13.	1324

Of these there were under 10 years of age 699—Colored, 271.

TO OUR READERS AND CONTRIBUTORS.

We lay before our readers, the first No. of the Tenth Volume of the *New Orleans Medical and Surgical Journal;* we are determined to do our utmost to please our friends and conciliate our enemies, and leave the rest to time; we shall strive to make the tenth volume better and more worthy the support of our subscribers than the ninth; and if our contributors will second our efforts, we may safely promise to give the profession an excellent practical Journal.

This Journal has already been instrumental in eliciting much useful intelligence from sources which might, in the absence of such a medium, remained forever silent on many important points in medical science. Each successive year sheds a brighter light over the future prospects of the Journal; and if we may judge by the past, we shall in the progress of time, circulate copies of the work through every village, county and postoffice in the Southern and Western States.

Hereafter, we shall devote more time and attention to the pages of the Journal, we shall extend our exchange list into foreign countries, and receive in return all the earliest and most reliable medical intelligence, and promptly spread it before our readers. Our Review department will, in consequence, contain a full and critical analysis of the best standard works as soon as they issue from the press, both in this country and in Europe—thus notifying our subscribers of those books worthy their perusal. The division devoted to Excerpta shall be made as full and complete as practicable—containing a digest of such papers on practical medicine as shall come well endorsed and fully recommended. Much, however, will be expected from contributors for the original department of the Journal, as without material aid from such sources, no medical periodical can look for popularity or receive adequate encouragement. As our terms, to which we intend to adhere, when practicable, are in advance, we shall immediately on issuing this number, inclose accounts to many of our subscribers; all of whom are requested to remit by mail to the Editor, with as little delay as practicable.

Of such as are in arrears to the Journal, we have a right to *insist* upon an early and full settlement, as the laborer should ever be considered as worthy of his hire, however undeserving he may be in other respects.

We again beg to remind our subscribers that we have no Agents abroad, engaged in collecting accounts for the Journal.

As this number begins a new volume, an excellent opportunity is now presented to those who wish the work, to forward their names and one year's subscription in advance; and in all such cases, the Journal shall be promptly enclosed to their address.

LOUISIANA STATE MEDICAL SOCIETY—CORRECTION.

In the published proceedings of this Society the name of *Dr. Benedict* is spelt incorrectly. It should be as it is here, instead of *Benedick*. Dr. B. having declined to act as Chairman of the Committee on *Midwifery and the Diseases of Women and Children*, the President has ths pleasure to announce that Dr. Baldwin, of this city, has been appointed in his place. We invite special attention to the following Circular addressed to individual physicians, in all parts of the State:

"New Orleans, June, 1853.

SIR:

The Committee of the "*State Medical Society*" *on Practical Medicine* being desirous of obtaining all possible information on subjects within the limits of their special duty, would be exceedingly gratified if you would favor them with any communication on or before the 1st of December next, connected with "Practical Medicine," which your experience or that circumstances may suggest.

The Committee would respectfully call your attention to the following points for notice:

What is the Topographical character of your Parish? Is there much land under tillage? Is it marshy? What is the prevalent or common type of your fevers? Have you genuine Typhus or Typhoid? Are your Typhoid fevers the sequelæ of Intermittents? Is the tendency to Typhoid condition more common now than formerly? What has been the prevalent disease during the year? Can you assign any special cause for such diseases? What is the result of your experience in the use of Quinine in Typhoid fevers?

With great respect, allow me to remain your obedient servant,

P. B. McKELVEY, M. D., Chairman."

PROF. WOOTEN ON THE VERATRUM VIRIDE.

We have received from Dr. Quintard, Editor of the *Memphis Medical Recorder*, the proof-sheets of a very interesting paper on the above subject, by Dr. Wooten, which will appear in the July No. of the Recorder. These sheets come to us too late for an extended notice, but we may be allowed to say the author presents strong and most favorable testimony to the remedial virtues of the Veratrum Viride. No new remedy has ever made more rapid and steady progress into favor than this. If all that is claimed for the Veratrum Viride and Sulphate of Quinine in large doses be true, we may hope soon to have complete control of two of the most dangerous diseases we have to contend against, viz: Pneumonia and Continued Fever, and the physicians of the South will be entitled to much credit for the improvement. We invite special attention to Dr. Wooten's paper.

PROFESSOR AGASSIZ.

Louis Agassiz, one of the most distinguished naturalists of the age, is now delivering popular lectures in the Lyceum Hall of this city, to large and fashionable audiences. Professor A. cannot complain that the money making, money spending, amusement seeking citizens of New Orleans do not appreciate him, seeing that among his auditors are not only hundreds of learned lawyers, physicians and teachers; but hundreds of merchants, and still other hundreds of the fair sex; all of whom listen attentively time after time, to the recondite details of embryology, histology and zoological classification. It is obvious to all, that Prof. A. is completely master of whatever subject he introduces before them. His style of lecturing is plain and substantial, without any attempt at rounded periods, or special ornament. Standing in front of a large black board, with his hands behind him, he enters upon his subject without preface, rarely moving a hand, until some graphic illustration is required, then he takes a piece of chalk in his right hand, and perhaps a dry napkin in his left, and turning his back mostly upon the audience, without for a moment ceasing in his discourse, which from the distinctness of his enunciation, is still perfectly audible in every part of the hall, he rapidly produces elegant chalk drawings upon the black board. In this matter he shows wonderful skill and facility. The napkin follows and removes the chalk mark as required. Thus he keeps his subject, one detail after another, in a manner visible before his audience; and thus he makes it plain and easy to the commonest apprehension.

To discover, collect, classify and describe such species of fish in our waters as may be unknown to science, was the prominent object that induced Prof. A. to visit us. He has thus far met with about ten new species. It is to be hoped that by his precepts and example, a taste for the cultivation and practical pursuit of natural history may spring up in our midst.

New Orleans, May, 1853. J. L. R.

IRRITABLE STOMACH.

"In attacks of Cholera Morbus especially, but often in the progress of febrile diseases, the stomach is so irritable that it is impossible for it to retain any medicine. Dr. Bell, of Louisville, had suggested, under such circumstances, the application of a large dry cup, a tumbler for instance, on the pit of the stomach. We have had this to fail, while we regard it a remedy entitled to great confidence. We asked Dr. Robertson, what plan, if any, would always compel the stomach to tolerate medicine. He replied that the stomach was disposed to be more refractory in Cholera Morbus than in any other disease, and that in his early practice he had known patients lost from this cause alone; that 100 drops of laudanum at a dose, without any other medicine, would relieve any case of Cholera Morbus that he had ever been called on to treat, and for a great number of years he had compelled the stomach to retain tha

dose in this disease. This he accomplished by grasping the little finger of his patient, and forcibly and *painfully* flexing the third phalanx upon the second. The medicine was swallowed while the grasp was firmly retained, which was continued for a few moments afterwards. "They have tried to knock me down," said the venerable physician, "but I defied them to throw up the medicine." (*Nashville Journ. Med. & Surg., June*, 1853.)

We would respectfully add to the above prescriptions for the distressing affection mentioned, that in cases where every thing else had failed in our hands, we have found the happiest effects from good champaign wine and ice. Let a tumbler be half filled with crushed ice, and fully covered with the wine, and drunk as soon as it is cold. It will be found exceedingly grateful and very apt to be retained. We have applied it in the advanced stages of Cholera Morbus, the protracted vomitings of Bilious and Yellow Fever, and the irritable stomach of pregnancy, with the happiest effects. This remedy is quite familiar to to many physicians in New Orleans.

E. D. F.

TYPHOID FEVER.

This number of our Journal abounds in matter relative to Typhoid Fever, and one paper had to be postponed after it was set up; but we again invite attention to the paper of Dr. Bailie, which shows that genuine Typhoid Fever may undoubtedly spring from vegetable decomposition.

ANNUAL MORTALITY IN NEW ORLEANS, IN 1852.

Total Mortality 1852.	Under 10 years of age.	Colored	Zymotics	Nervous System.	Respiratory System.	Digestive System.	Circulatory System.	Generative System.	Urinary System.	Locomotive System.	Integumentery System.	Age.	Non-Specified.	Sporadics.	External Causes.
8693	3217	1199	3792	1273	1187	677	82	64	22	23	11	96	624	436	376

The heading *Age*, includes the *non-viable* and the deaths from *old age*. The mortality being only published *weekly*, we are not able to make out the precise number of deaths for each month. Estimating the average population throughout the year at 175,000, one hnndred and seventy-five thousand, which is probably above the truth, this table would show a mortality of within a fraction of 5 per cent, or 1 in 20. It is worthy of remark that the deaths among recent emigrants from Europe, which occur at this port, are all included in this Bill of Mortality; which, we believe is not the case either in New York, Boston or Philadelphia. Our list is also considerably enlarged by invalids who come South on account of Pulmonary complaints.

TABLE showing some of the principal diseases that caused death—extracted from the above.

Fevers.	Yellow Fever.	Cholera.	Dysentery.	Trismus Nas.	Tetanus.	Convulsions.	Consumption.	Still-Born.
1491	456	1329	355	188	66	450	787	284

The first two deaths from Yellow Fever occurred in the first week of August; both in private practice. The last deaths (2) from this disease occurred in the last week of the year. The largest number of deaths, from Yellow Fever, 62, occurred in the first and second weeks of November.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1853.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich.

WEEKLY — 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days.	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
April 28	87.0	55.0	32 0	30.10	30.00	0.10	SE.	2.00	2	0.020
May 5	81.0	66.0	15 0.	30.10	29.95	0.15	S.	2.14	1	1 700
" 12	83.5	66.0	17.5	30.15	30.00	0.15	SW.	2.57	2	1.175
" 19	87.0	65.0	22.0	30.12	30.10	0.02	SSE.	1.85	2	1 615
" 26	86.0	65.0	21.0	30.15	30.10	0.05	NW.	2.70	1	0.005
June 2	91.0	71.0	20.0	30.20	30.05	0.15	SE.	1.85	1	0.275
" 9	91.0	75.0	16.0	30.20	30.07	0.13	S.	1.80	1	0.005
" 16	87.5	73.0	14.0	30.18	30.10	0.08	SE.	2.57	4	1.015

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For April and May, 1853.

	SEX.	APRIL.		MAY.	
ADMISSIONS -	Males	667		651	
Do. - -	Females	251		265	
			918		916
DISCHARGES -	Males	655		692	
Do. - -	Females	224		259	
			879		951
DEATHS - -	Males	68		61	
Do. - -	Females	28		30	
			96		91
BIRTHS - -	Males	5		3	
Do. - -	Females	8		6	
STILL-BORN -		5		0	
			18		9

H. VANDERLINDEN, Clerk.

UNIVERSITY OF LOUISVILLE.

MEDICAL DEPARTMENT.

The Lectures in this Department will commence on the last day of October next and terminate on the last of February.

Charles W. Short, M. D. Emeritus Professor of Materia Medica and Medical Botany.

Benjamin R. Palmer, M. D. Professor of Descriptive and Surgical Anatomy.

Lunsford P. Yandell, M. D. Professor of Physiology and Pathological Anatomy

Samuel D. Gross, M. D. Professor of the Priciples and Practice of Surgery.

Henry Miller, M. D. Professor of Obstetric Medicine.

Lewis Rogers, M. D. Professor of Materia Medica and Therapeutics

Benjamin Silliman, Jr. M. D. Professor of Medical Chemistry and Toxicology.

Austin Flint, M. D. Professor of the Theory and Practice of Medicine.

T. G. Richardson, M. D. Demonstrator of Anatomy and Dissector in Pathological Anatomy.

The Fee for admittance to the Lectures of each Professor is $15, ($105 in all) payable invariably in advance. Matriculation and Library Fee together, $5. Graduation Fee $25. Practical Anatomy and Dissection $10—ticket to be taken at least once before Graduation. Rooms open from 1st October.

A preliminary Course of Lectures, free to all students, will be delivered during the month of October.

Clinical instruction is given twice a week at the Louisville Marine Hospital: Ticket $5, to be taken once before Graduation.

A Clinique has also been established in connection with the University, at which operations are performed, and cases prescribed for and lectured upon in presence of the Class.

Good Boarding can be procured at from $2 50 to $3 a week.

L. P. YANDELL, M. D.

July, 1853. Dean of the Faculty.

THE

NEW-ORLEANS

MEDICAL AND SURGICAL

JOURNAL.

SEPTEMBER, 1853.

A. HESTER, M. D.,

EDITOR AND PROPRIETOR.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*

N. O. Charity Hospital.

NEW-ORLEANS:
PRINTED BY JOSEPH COHN, 31 POYDRAS STREET.
1853.

TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

Since our last, we have received the following books, circulars, pamphlets etc., etc. A paper from Professor Mitchell.

Abrégé de Patholigie Medico Chirurgical, ou Resumé Analytique de Medicine et de Chirurgie. Par M. E. Triquet, Docteur en Medicine, Ancien Interne à l'hopital de Tours, etc. In 2 vols. Paris, 1852.

Address to the Graduates in Medicine, at the University of Buffalo, April, 1853. By Frank H. Hamilton, A.M., M.D., Prof. of Surgery, etc. (From the author.)

Copy of the Proceedings of the American Medical Association. New York, 1853. (Dr. Bulkley will please accept our acknowledgments for his politeness.)

A Review of a Report of a Committee of the American Medical Association. Manual of the active principles of Indigenous and Foreign Medical Plants, as alkaloids, resinoids, and allied principles, chemically obtained at the American Chemical Institute, New York. Keith & Henderson, proprietors, New York. 1853.

On Diseases of the Liver. By George Budd, M.D., F.R.S., Professor of Medicine, in King's College, London; and Fellow of Caius College, Cambridge. Second American, from the last and improved London edition. With colored plates and wood cuts. Philadelphia, Blanchard & Lea, 1853. From the publishers, through J. B. Steel, 60 Camp street, New Orleans.

Principles of Medicine, comprising General Pathology and Therapeutics, and a brief general view of Etiology, Nosology, Semiology, Diagnosis, Prognosis and Hygienics. By Charles J. B. Williams, M.D., F.R.S, Fellow of the Royal College of Physicians, etc. Edited, with additions by Meredith Clymer, M.D, Fellow of the Philadelphia College of Physicians, etc., etc. Fourth American edition, revised. Philadelphia. Blanchard & Lea, 1853. From the publishers, through J. B. Steel, 60 Camp street, New Orleans.

Essays on Asylums, for Persons of Unsound Mind. Second series. By John M. Galt, M.D., Superintendant and Physician of the Eastern Lunatic Asylum, of Virginia, at Williamsburg. Richmond, Va., 1853. (From the author.)

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. Part xxvii. July, 1853. Stringer & Townsend, New York.

Catalogue of the Trustees, Faculty and Students of the Medical College of the State of South Carolina. Session 1852–'53 Charleston, S. C.

Analysis of the Alabama Vichy Springs—belonging to Mr. Charles Cullum, of Mobile. By J. B. Avequin, New Orleans, 1853.

Sanitary Reports Reviewed. By Daniel F. Wright, M.D. A "Report on the Health and Mortality of the City of Memphis, for the year 1852, by C. T. Quintard, M.D."

Removal of a nail from the Lungs by Tracheotomy. By Paul F. Eve, M. D., Professor of Surgery, in the Nashville University. From the Author.

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL

SEPTEMBER, 1853.

Part First.

ORIGINAL COMMUNICATIONS.

I.—ON CONTINUED FEVERS.

BY F. E. GORDON, M.D., MARION, ALABAMA.

It is strange that so little is accurately known, and so much diversity of opinion exists as to the varieties of continued fever. Dr. Good, in his learned work on the "Study of Medicine," describes ten different kinds of continued fever, without mentioning the typhoid, which had not then attracted the attention of pathologists. He includes, however, puerperal peritonitis, which is now properly excluded. Dr. Jenner, of London, distinguishes between "typhus fever, typhoid fever, relapsing fever, and febricula, though commonly confounded under the name of continued fever." Dr. Dickson, of Charleston, South Carolina, in his report to the American Medical Association, of 1852, assumes the establishment of the following varieties of continued fever: "typhoid, true typhus, simple fever, ephemera, febricula, British epidemic fever, relapsing fever." Even with regard to the forms of typhoid and typhus there is much disagreement in the medical world. Watson, in his excellent "Practice," treats of them under the same head as maculated typhus, and many eminent British physicians still continue to hold this opinion.

The school of Louis & Chomel, in Paris, have long ago demonstrated their non-identity, and this is, unquestionably the dominant belief in France. In Germany too, abdominal and exanthematic typhus have been separately described, but the relation between them is considered uncertain, though the pathological changes of the former are well understood. In the United States, owing to the writings of Jackson, Gerhard, Pennock and Shattuck the views of Louis are generally entertained. Dr. Bartlett, in his work on typhus and typhoid fevers, published first in 1842, and enlarged in 1847, embodies the same opinions systematically. Professor Wood likewise adopts them in his treatise on the Practice of Medicine, which is fast becoming a text book for American practitioners, and presents suggestions as to the treatment of typhoid fever, derived from his own experience, of the highest value To add to the confusion already prevailing, Dr. Robert Dundas, Physician to the Northern Hospital, Liverpool, and for 23 years Medical Superintendent of the British Hospital, Bahia, comes forward in 1852, to announce "the essential identity of fever in all countries," and finally appeals to the undoubted specific influence of quinine, (when opportunely and adequately administered,) in controlling the fevers of tropical climates, and to its equal efficacy in controlling the fevers of this country, as has been fully proved by the cases treated at the Liverpool Northern Hospital, the Liverpool Fever Hospital, St. Thomas' Hospital, London, and elsewhere; as conclusive proof, that in their essential nature these fevers are identical, and differ only in form and degree."

After premising this much of the opinions of those who have enjoyed the greatest advantages of observation and research in populous cities, and in climates temperate and tropical, some indulgence may be asked for the oddities too often thrust upon the world, and the want of diagnostic skill evinced by such of us as are "cabin'd, cribbed, confin'd" within the precincts of a country town.

It was during the summer of 1849 that continued fever first occurred in my practice here. I saw but a few cases as I have elsewhere observed, the lesion seemed to be confined chiefly to the circulating system. Increase of the heart's action and of the temperature of the skin were the most prominent symptoms. The tongue was generally clear and the appetite good. The following year the same kind of fever prevailed as an endemic, and under the heroic treatment proved, in many cases, fatal. I soon discovered the persistency of the fever, and after a few efforts to cut it short with such doses of quinine as I was in the habit of using successfully in remittents, I abandoned its administration.

I can recollect no case where I thought its action beneficial. The pulse was not reduced and the nervous system greatly excited by it, while the skin, though bathed in moisture, was usually hot. I relied on antimony tincture sanguin, canadensis, and cold effusion, with a view to control the circulation and diminish the heat of the skin; and on small doses of neutral salts to move the bowels, which were either in a natural or costive condition. In about forty cases treated in this way, in 1850, only one lasted three weeks, a very few fourteen days, and the great majority recovered between the seventh and ninth days, no death having occurred. The premonitory chill was absent, I think, in every one of these cases, and I soon came to make this a test for the non-exhibition of quinine. I have since found it occasionally fallacious. I was unwilling to pronounce this disease typhoid fever, and so reported to the Alabama Medical Association. During 1851, I met with our usual types of remittent and intermittent fevers, but with only an exceptional case of the continued form. On August 11th, 1852, I saw the first case of what I was subsequently led to believe is the modified typhoid fever of this latitude, the subject was a plethoric negro, aged 30, and employed at a livery-stable. His attack was ushered in by a chill, and his bowels had not been moved for two days previously. There was not the gastric irritability and the severe head-ache of the remittent, but the patient complained much of soreness of the muscles of the neck and of pain in the back. He was put under the influence of quinine, and kept so until the morning of the 13th, when his tongue first began to redden and his bowels to be irritable. He had taken, meanwhile, the mildest purgative, followed by an anodyne. By this time he had become greatly distressed with roaring in the head, and notwithstanding the discontinuance of the quinine, delirium supervened. On the 15th severe epistaxis ensued, which caused a swoon. The loss of blood and the continued cold applications to the head relieved the delirium, but his dejections remained thin and frequent, requiring the restraint of Dover's Powder, for weeks. The red and tremulous tongue, the diarrhœa, the loss of strength and unvarying fever formed a group of symptoms which needed but maculæ and sordes to answer the description of the most rigid observers of typhoid. Sudamina were present about the clavicles in the third week. This man was discharged on the 31st of the same month, convalescent, but very weak, and his bowels requiring to be watched. He afterwards suffered from suppuration of the parotid but finally recovered.

In nearly every case which I subsequently treated there were repeated bleedings from the nose, though none so alarming as described above;

and diarrhœa never failed to exist or to be developed upon the exhibition of the slightest cathartic. For the latter symptom I gave turpentine in mucilage, commencing much earlier than is recommended by Professor Wood, and the happiest results followed. I have sometimes combined it with sanguinaria, though rarely, as the arterial excitement comes down with the subsidence of the intestinal disease. In an interesting young lady of fifteen, which proved to be another protracted case, the bronchitic symptoms were prominent for the first week, that I did not suspect the form of disease until the epistaxes aroused my suspicion. It is remarkable too, that though the bowels were at first quiescent, they became deeply implicated in the end, being in fact the only case in my practice in which intestinal hemorrhage occurred. She was put on vegetable bitters, and dismissed on the 22d day, being quite feeble, though able to sit up for an hour or two.

During the latter part of the winter I treated several cases with veratrum viride, with great satisfaction, and one of a little boy of ten years of age, that I fear would have been fatal under any other treatment. About the fifth day after he came under my care, having been previously unwell for a week, slight delirium made its appearance, and continued deepening for three days despite of the free use of cold water to the head, until I feared a serious lesion of the brain. He had been always a delicate child, and his bowels having been but partially controlled by the turpentine and such opiates as could be prudently given, and having suffered frequent bleedings from the nose, his strength was greatly diminished. On the eighth day I commenced giving the veratrum, and in twenty-four hours it reduced the pulse from 120 to 100, and effectually relieved the brain. Its use was persisted in for four days, interrupted then for a day, and finally continued for four days longer, until convalescence was established. During the interruption which was caused by the patients somnolency, dilated pupils, etc., he was kept upon small doses of Ipecac; but as the unpleasant symptoms rapidly passed off, and the fever rose, I thought it best to return to it again. Three drops every three hours, after a short time, were sufficient to keep the pulse down to 80, and the dose was decreased gradually after that.*

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* I have given the veratrum in a wide range of cases, and consider it an admirable *febrifuge*. It is peculiarly applicable to children, from the ease with which it can be administered, and by giving a medium dose, and not increasing it upon repetition, it is decidedly safe. The physician has but to measure the water accurately, and to drop the medicine into it carefully himself, allowing five drops for every tea-

In December last, I saw some cases in consultation, about fifteen miles distant from town, where the quinine had been freely given. The attending physician is a warm advocate of the quinine treatment of all fevers, and had read Dr. Fenner's late articles with approval. With unwearied assiduity he watched his cases and exhibited the quinia, and what ever other conclusions he arrived at, he certainly must admit that he could not cut short an attack with an oz. of quinia. For my own part, with all due deference to him, the nervous excitement was so great and unusual that I could attribute it to nothing but the remedy. In one case the tongue had to be fixed by a spoon, in order to get a good view of it; so entirely uncontrolled by the will were its movements. In all of the cases treated in this way, there was hemorrhage from the bowels, of a peculiar character, and excessive muscular mobility.

Upon a review of these two forms of continued fever, one presenting a lesion of the circulating, and the other of the digestive system, the question arises what relation do they bear to each other? Some of my medical friends insisted at the time that the former was true typhoid, and by active interference to cut short a hypothetical inflammation, and the injudicious use of mercurial and other purgatives created intestinal irritation of a grave character; but if my observations are correct, this was an artificial change, and not characteristic of the disease in question.

Again, between the prevalence of these two forms of fever, in the spring of 1851, dysentery was epidemic, and has never been fairly absent since, sporadic cases occurring with much greater frequency than formerly. What effect might this latter affection have in modifying the type of fever previously existing—might it not add a lesion of the bowels somewhat similar to that of typhoid?

In this connection, inquiries force themselves upon us, as to the unchangeable nature of the types of fever through all ages, their capacity for blending or conversion among themselves, and with other diseases not ranked as fevers.

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spoonful of the solution, to be taken every three hours by an adult, and proportionately according to age, and he need have no fear. In pneumonia it acts promptly, and in most fevers, whether essential or symptomatic, it reduces the excitement, and sooner or later subdues them. I have never given it in a remittent, but in the hot stage of intermittents it acts finely, and would probably answer equally well in the former. I prefer it to sanguinaria, which I formerly used, as it controls the circulation, often without nausea, and acts as an anodyne besides.

It may be stated, finally, that maculated fever is unknown to this community, as I would infer from never having witnessed the eruption myself, or heard it alluded to by those who have had many cases of continued fever in their families.

II.—ALCOHOL AND THE ETHIOPIAN;* OR, THE MORAL AND PHYSICAL EFFECTS OF ARDENT SPIRITS ON THE NEGRO RACE, AND SOME ACCOUNT OF THE PECULIARITY OF THAT PEOPLE.

BY SAMUEL A. CARTWRIGHT, M.D., NEW-ORLEANS.

Physicians are often consulted in regard to the propriety or impropriety of allowing negroes to partake of spirituous liquors. What good is to be expected from the use of such drinks, in quantities ever so moderate; or what are the evils—physical and moral—to be apprehended? are questions often propounded, but not satisfactorily answered. Medical books are mostly silent on the subject of negroes, and the schools say little or nothing about them. The Medical Journals are, therefore, the proper organs to supply the deficient information. This can best be done by the physicians of the South contributing the result of their observation and experience. A few imperfect and hasty remarks are herewith presented to the profession, hoping thereby to attract the attention of other medical observers to this important subject. With the negro, the physician has not only to deal with the physical, but the moral man. He is not only above the overseer, but he is, (or ought to be,) above the master, in all questions relating to the slave's physical and moral well-being. The physician is, par excellence, the slave's friend; and it is through him that abuses are to be corrected and improvements made. He best consults the interest of the master by making the negroe's comfort and happiness a paramount consideration—a point first to be attained.

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*This communication was elicited as a reply to some interrogations propounded to the author, by the Rev. C. K. Marshall, of Vicksburg; and, also, as a reply to various questions touching the negro's origin, and physiological and psycological peculiarities, referred to him for solution, by several of his medical acquaintances.

That the effects of alcoholic beverages on the Ethiopian race are pernicious, is sufficiently proved by the fact, that plantation property diminishes in value in proportion to its proximity to villages, grog shops, or those places where slaves can procure ardent spirits without leave from their masters. It is also proved by the fact, that no sensible planter, although he may have more land than he wants, will dispose of ever so small a lot, even at an extravagant price, for fear that a grog shop or grocery, where his slaves can obtain ardent spirits, will be erected on it. Hence, for many miles together, through the planting region, the weary traveler often finds no resting place or tavern. The owners of the soil will not sell land enough to build a public house upon, for fear that his negroes will become corrupted by the facilities that such places generally afford, to the easy attainment of ardent spirits. Samuel Davis, Esq., opposite Natchez, has four times as much land as his negroes can cultivate; yet neither love nor money can induce him to sell a single piece, the size of a town lot, for fear that a grocery or retail liquor store may be established on it to corrupt his negroes. The Parish of Concordia and a few private individuals own a few lots, opposite Natchez, constituting the little village of Vidalia, consisting of a court-house, jail, lawyer's offices, a printing press of one of the best papers in the South, and two hotels. Yet Mr. Davis cannot be prevailed upon to enlarge Vidalia, by selling lots, although offered New Orleans prices. If he were to do so, he knows very well that grog shops would soon be erected upon them, and his negroes would become worthless, from the facilities of obtaining alcoholic beverages.

They have not the same power of self-control as white people have, and cannot resist the temptation of ardent spirits so well. They have the same natural appetite or propensity for strong drink that they have for tobacco. Negro children will drink it, if they get hold of it; whereas the appetite for it is not natural, but acquired, with white people, unless their parents were addicted to its use. The habit of using tobacco, in chewing or smoking, is an acquired habit with the whites, but natural with the Africans. There are exceptions to this general rule, in regard to tobacco and ardent spirits. Some negroes are naturally sober, and dislike ardent spirits; but it is the fewest number. The same remark is true in regard to the Indians. Some few will not drink alcohol in any form; the majority will, however. Negroes are never so happy and so well satisfied as when they abstain entirely from alcoholic beverage. Never taste it—never smell it. If they smell it, they will break locks to get it. The old Africans have a tradition, that the negro was originally a white man, but the Devil got hold of him and changed

him into a black man, like himself; the Devil being the only true negro, in their opinion, in existence. Hence, they do not like to be called "negroes." It is an insult derived from the tradition of the Devil being a negro, and trying to make them like him, by blacking them.

According to the tradition, "The Devil beat their father with the soot bag, and made him black—clapped his red-hot hand on his head, and crisped his hair into kinks—struck him in the face, mashing his nose broad and flat—and causing his lips to swell and puff out further than his nose—then made him carry such heavy loads on his head as to bow out his legs, and to make his feet flat. The old negro afterwards left him in the woods, with his head too much addled to make a living for himself. He had to live in hollow trees, under rocks, and in caves, without clothing, and with nothing to eat but ants' eggs, caterpillars, and such things. Whenever he would try to do anything for himself, the old boy would come and get in him, and prevent him." Such is the tradition—it might have added, "That after the black man had had many children, ant eggs and caterpillars get scarce, brothers hurried brothers and sisters to the sea shore, and gave them away to the British slavers for a few worthless trinkets, as glass beads, and the chippings of tinners' shops. The British made fortunes out of them, by selling them to the Virginia, Maryland, and Carolina planters. Their new masters made them do something for themselves, besides working for them. They had provision grounds given to them—log cabins to live in—with large fire places and plenty of wood. They had meat, bread and fish, with plenty of vegetables of their own raising; and shoes and clothing to keep them warm. When the old boy would envy their happy condition and get in them, causing them to fight one another, or to refuse to work, their master would kindly come and whip the Devil out of them; and then they were as good as anybody, and would love their masters for making them industrious and peaceable. Their masters took them to church and gave them a chance to become Christians, and to get to a better world when they died; where every body are alike, and all are happy; where the Devil would not be permitted to come, or any of those in his service. The old boy was so sorry to see them do so well and on the road to heaven, where they would be above him and all his folks, that he persuaded the British to go for them and bring them back again. The British came in large ships, with big guns, and offered them land, liberty, money, and plenty of whiskey, if they would go with them. A few ran away from their masters, and were taken by the British to Canada, to London, to Sierre Leone, in Africa. In all these places the Devil got hold of them

again. He entered into them, most commonly, in the shape of alcohol, destroying all their happiness in this world, and cutting them off from all prospects of obtaining happiness in the next. The great majority, nine hundred and ninety-nine in a thousand, would not go with the British, but continued to hoe corn, to raise pigs and poultry, sheep and cattle to supply Master Washington's army, then in Pennsylvania, with provisions. At length, the British sent large armies among them. They staid at home—continued at their work—took care of the women and children, while their masters went to Yorktown and whipped the British entirely out of the country. Instead of finding the country, where the white man takes care of the black man, the weakest part and easiest conquered, the British found it the strongest, and were there conquered themselves, in both wars, they waged against the black man's protector—Yorktown and New Orleans, for instance. Their white masters, after whipping the British, put their black people to making cotton, sugar, molasses and rice, in addition to the other things they had been making, and took great pains in preventing the Devil from coming among them in the shape of the strong drink the white people call alcohol. With all their care, however, the Old Boy would occasionally slip among them, disguised in that and other shapes. Notwithstanding this partial evil, they continued to increase and multiply, and to live happily and contentedly. Great numbers were added to the church, and were on their way to heaven. The poor black people, without white masters, never found that way. If the good missionaries would put them in the way, the Old Boy, in the shape of alcohol, would come among them and soon make them lose the way. The Devil was so enraged at the good fortune of those black people, who had masters to protect them from his artifices, that he was determined to try another project to get hold of them. He went to the East India Company, and told the Company what a fine, large country they had robbed the people of Asia of. That it was rather an indifferent country for cotton, sugar, hemp, tobacco, maize, rice and such things; but that all these articles were made in such abundance by the negroes of America, in a better soil and climate, that unless they could stop the negroes from working there, the land they had robbed the people of Asia of, would be of little or no use to them. The Company prevailed on the British Government to set a few negroes free, that its subjects owned in the West Indies, in order to encourage the hundred and fifty millions of conquered people in Asia to go to work in making sugar, cotton and such things, and give the British half. As soon as the West India negroes got free, the Old Boy came among them in the shape of

alcohol, and not only made them quit work, but got them out of the churches into the prisons and penitentiaries. At length the Devil tried to work upon the hearts of the British people, by telling them many lies about how hard the black people in America, called slaves, were treated, and what an obstacle slavery was to the spread of Christianity. Some of the churches sent agents over to inquire into the truth of the matter, viz: two deputations, one headed by Dr. Cox, from the Baptist Union of England and Wales, and the other headed by the Rev. Dr. Reed of the Congregational Churches.

The agents counted the number of Christian negroes and white people in the churches of Virginia, Maryland, North and South Carolina, Georgia, Kentucky, Tennessee, and some other States. The church statistics, thus gathered and reported, proved that there were more Christian communicants among the negroes, called slaves, in these States, than were found among a similar number of the laboring classes in London, or any part of England, Wales or Scotland, and that the entire number of communicants, in South Carolina and Georgia, exceeded the entire number in pious New England, in proportion to the gross population, white as well as black.

These statistics also showed, that the Methodists of Charleston, South Carolina, and the immediate neighborhood, had a greater number of slave communicants in the churches, in and near that place, than the whole number of colored communicants in all the churches of the free States, of every denomination, put together. This so enraged the Devil, to think that a few slave-holders were making Christians out of a people he claimed as his own, and had actually made more Christians out of three millions, than the British and all the missionaries, of every country and climate, had been able to make in a thousand years, with twenty times the number of free negroes to operate upon, he determined to try another expedient. Instead of having preachers sent as spies, he prevailed on the British abolitionists to send over a few lords and noblemen. Lord Morpheth, the present Earl of Carlisle, came—also, Lord Murry and Mr. George Thompson, a member of Parliament, and many others. Lord Morpheth was not received as a spy, but with open arms, and was taken by the planters to their plantations, and permitted to mingle with the negroes and to converse freely with them. He confessed, that there was not a peasantry in Europe so happy, so well provided for, so comfortable and contented, and who had lighter tasks to perform, than the negroes on the sugar and cotton plantations of Louisiana. The Devil, however, tempted him to look around and behold the great wealth that might be his, ere long, if he and the British aris-

tocracy would help to free the negroes of America, and to make the abolition of negro slavery a fixed and settled policy of the Government, to be carried out at any cost or sacrifice. The Devil not only held lucre in one hand—the lucre of transferring the agricultural wealth of America to India and Australia; but he held up terrors in the other—showing him the terror, horror and destruction, which would ere long fall upon the Dukes and Duchesses of England, if they, fools like, permitted the example of a prosperous Republic to grow up under their noses, when they had the power to crush it, if they would help him to incite the Northern States against the Southern. He then assumed the form of a Massachusetts witch and wrote a book, full of romantic lies, to inflame the people of the North against the institutions of the South, and he prevailed upon this same Lord Morpheth, now Earl of Carlisle, to sell his soul and to write the preface."

Such a historical amendment might properly be added to the tradition.

John Fletcher, the author of the immortal Studies or Lessons on Slavery, thinks it was sin which worked those physical changes in the negroe's organization; separating him so widely from the white man, and disqualifying him for the enjoyment of the white man's kind of liberty, viz: the mark put upon Cain. Natural historians have discovered such marked differences in the organization, physiological and pyscological phenomena of the several varieties of the human race, as to justify their reference to three species—which may well be represented by the three sons of Noah. That Ham was a black man is proved by his name, which in Hebrew means *black* as well as *hot*. C. Stockius, in his Hebrew Lexicon, published in Leipsic a hundred years ago, gives both meanings to the word, "Itaque Chami nomen vel a calore, vel a nigredine." The Septuagint version of the Old Testament, made before the Christian era, when translating the appellation of Ham's posterity into Greek, designates them by the Greek term Anglicised into Ethiopian; meaning in Greek *black faces*. Plutarch also calls them by the same name. The Romans used the term niger, from which our word negro is derived. There is no necessity, therefore, of supposing that the negro belonged to a race of men unknown to the Sacred Writers; or, that he is descended from a monkey or a baboon. He is the black man of Scripture. How Noah came to have a white, red and black son, is a question for theologians to determine, and not for physicians. The latter have to take things as they find them. From not knowing, however, that the Ham or Cham of Scripture was a black man, physicians are apt to conceive a theory of the origin of the negro race, tending to call in question Sacred History. This is the

very thing the scheming British abolitionists want the slave-holders to do, in order to bring the influence of all Christendom against them. There is no necessity for it, Natural History is not in conflict with the Sacred Record. They both harmonize beautifully. It is important, also, to know, that common observation, comparative anatomy and physiology confirm the Scriptural account of the existence of a slave race of men of a black color. There is such a race among us held in slavery. They are black. There was such a race among the Jews. They were designated by a Hebrew word, meaning *black*. The Jews reduced them to slavery, and held them as slaves by Divine commandment. The branch of that portion of the black or Hametic race reduced to slavery, were called Canaanites—literally translated, "*knee benders*." They not only differed in the color of their skin from the Jews, their masters, but in their disposition. Their masters, the Jews, we are told, were "a stiff-necked people." The Canaanites, as their name imports, and all Hebrew names are significant, were "*submissive*;" a people who submitted themselves. The Gibionites, a portion of the Canaanites, submitted themselves to the Jews; and even made use of artifice to induce the Jews to enslave them. They did not belong, therefore, to that race of men whose motto is, to "live free or die;" but to a race, which slavery suited better than freedom—a race of people to whom slavery brought blessings, and liberty evils. Hence, God in his mercy permitted them to be subjected to that kind of government which suited them best. We have subjected them to the same kind of government. It is called slavery. If they had been mentally, physically and morally constituted like the Jews, or like us, the order to enslave them would have been an unjust order, and could not have come from a just God. Voltaire brings this as an argument against the truth of the Scripture. His argument is founded on the assumption that all men are alike. His premises are disproved by daily observation as well as by the Scriptures. If Voltaire had lived among negroes in slavery and negroes in freedom, and had been apprised of the fact, that the Canaanites were negroes, he would have seen at once the mercy and justice of the order given to the Israelites to take possession of the land of Canaan, to strike down the rebellious and to reduce the submissive portion of the inhabitants to slavery. For the plain reason, that negroes are happy, contented and useful in slavery, while they are always unhappy, discontented, vicious and a pest to society, in freedom. It is, therefore, a great error, in the treatment of negroes in health or disease, to apply the same rules and principles to them as if they were like white people in every respect, except the color of their skin. This error is founded

on an utter disregard of the truths taught by Natural History, as well as those taught by common observation. It is founded on an utter disregard likewise of those matters and things revealed in the Bible. Because all these harmonize in establishing a difference—a broad difference—between the nature of the negro and the white man. Voltaire and many of the abolitionists, from not seeing this great truth, blindly accuse the God of the Hebrews with injustice in tolerating slavery, and reject the Bible as a fable. On the other hand, certain advocates of slavery, deluded by the false philosophy of some artful English and French writers, have gone to the other extreme, and reject the Bible as a fable because they fancy that it conflicts with the truths taught by Natural History and common observation. They have not taken the pains to ascertain what it does teach. Hence, as soon as they get a little smattering of Natural History they begin to form theories of their own in regard to the origin of man. They throw away the Bible without examination, and because Ham is not translated *black man* as it might be, and Canaan has not been translated in the English version, "*knee-bender*," "*he is submissive*," "*he submits himself*," "*he is broken*;" they jump to the conclusion that the negro was unknown to the Bible—that the Book is untrue, and that the negro is not properly a human being, but originated from an ape or baboon. From such advocates of slavery, the South may well say, "Good Lord deliver us." Also from these who would base the defence of our Southern institutions upon the fact that the Greeks and Romans had slaves. Natural History and the Revealed Word of God is the only sure basis to stand upon; because it is the only true and solid one. We do not keep the negro in slavery from some fanciful idea that he descended from a monkey, or because the Greeks and Romans had slaves, but because experience and observation have proved that slavery is the best condition for him; because the facts gathered from history prove his inability to govern himself; because Natural History points out his disability in the organization of his system; and, lastly, because the Revealed Word of God ordained him to be "servant of servants to his brethren." Higher authority we could not have. No institution ever had or can have a higher authority than this. By leaving this solid rock for the quagmires of English and French speculation, we do injustice to the institution, to the negro and to ourselves. Besides, great injustice is done to the negro by regarding him as a brute, instead of a man and a brother—a weaker brother, a poor, blind brother, requiring our arm to lean on. The Abolitionists do great injustice to their poor, blind brother of the human race, by wanting to deprive him of all the advan-

tages he derives from that friend and protector, called a master. He has eyes to see and they suppose he can see, and they take for granted that he can see, as they can, into the morrow or the next day. But if they would take the pains to observe him more closely, they would find that ne cannot see beyond the present moment. He looks at the last stick of wood burning in the hearth, but cannot see the snow-storm that is coming, or the necessity of gathering fuel until the cold pinches him and makes him feel it. He looks at the last measure of meal, about to be baked into bread, and regards the flowers of spring and hears the singing of birds, but he cannot see the necessity of planting corn. He wants a friend to remind him of these things—to tell him what he has to do, and if he will not do it, *to make him.* In other words he wants a master. Dull as the negro's sight is into the future, he can scent alcohol from afar, better than a white man. The odor of alcohol has a stronger effect in mesmerizing his will than it has on the white race. He cannot resist it, unless he has a master to restrain him. With all his master's care to keep him from it, he too often obtains it by the most ingenious artifices, which are so much above his natural reasoning powers on other subjects, as to look like instinct. Imprisoning him for offences, as the Abolitionist do, is no punishment to him, provided he has a place to sleep and something to eat. In Massachusetts, during the ten years ending the 30th September, 1850, there have been five times more free negroes sent to the penitentiary than in Virginia, of the same class of people, during the same period of time, in proportion to their number. Massachusetts, during the same ten years, sent three times as many white people to the penitentiary, in proportion to the numbers, as Virginia, during the same time. The statistics of crime prove, that the institutions of Virginia are three times more favorable to morality than those of Massachusetts as far as the whites are concerned, and five times more so for free negroes. In Connecticut, in the year 1850, 1-159 of the whole population of free negroes were in the penitentiary; while in Virginia, only 1-3201 of the free negroes of that State had been sent there, and scarcely any slaves at all. In New York, 1-5304 of the entire white population were in the penitentiary for the year ending 1st December, 1851; while in Virginia, the average for ten years was only 1-23,002; New York having nearly five times as many convicts as Virginia. Placing the negro on an equal footing with the white man, and treating him like a white man, as has been done in Massachusetts, Connecticut and New York, is a violence done to nature and has tended greatly to increase the amount of crime in those States—both among the whites and the

blacks—as is proved by the greater amounnt of crime existing there than in Virginia and the slave-holding States.

It brutalizes a negro to be treated like a white man. It makes him sulky, insolent, indolent and vicious, and he becomes a ready prey to alcohol. The practice, founded on the theory that he is a lampblacked white man, brings unnumbered ills upon him, and drives him back to that barbarism, irreligion and vice from which slavery rescued him. On the other hand, the theory which refers his origin to the monkey tribes, if reduced to practice, would cause him to be regarded more in the light of a brute than a man, and to deprive him of moral and religious instruction. As yet he is very seldom denied the opportunities of acquiring moral and religious instruction, even by those who have fallen into the error of denying him the privileges of intellectual culture, under the supposition that if he could read and write, he would be less faithful as a slave. According to my observations, however, and they have been very extensive, and extend through a long period of time, from my earliest youth to grey hairs, intellectual culture makes the negro slave more faithful and trustworthy, provided moral culture go with it. Nature has made him a slave and has put the stamp of slave on his organization of body, and has attuned his mental and moral faculties in conformity thereto. By cultivating those faculties nature will be aided instead of thwarted in her purpose of making him a trusty, faithful, obedient, useful slave. It is only when those faculties are permitted to lie entirely dormant, and the mind and morals uncared for, that the negro becomes faithless, disobedient, reckless, impertinent and hard to manage. All the trouble which overseers have in managing negroes, when alcohol is out of the way, comes from the ignorant, stupid, superstitious, wild negroes approaching barbarism, with mind and morals untrained. They have no trouble with those whose minds and morals have undergone a proper training; on the contrary, they find them useful assistants in keeping the wild negroes, as they are sometimes called, in subjection. Although the laws in some of the States forbid that the slaves should be instructed in what is called book learning, yet it is well known that such laws are disregarded, and that slaves are often so instructed. Almost without an exception they are the most faithful, provided their morals have been attended to and the use of ardent spirits has been prohibited. Those, also, who have had some degree of mental training, as the acquisition of some trade or art, are generally considered to be worth twice as much as the rough and the untaught. The latter are the lock-breakers. The former carry the keys. Yet, whether they have had intellectual, moral and

religious training or not, it ruins negroes to treat them like white people, or to put them on an equality with white men and white women. It is a great shame, however, to treat them like brutes. They should be treated like negroes and no one else. Chastised for disobedience or insolence, but not abused. It insults them to be struck with the fist, or stick, or with any other weapon than a switch or whip. An Indian would murder the person who would whip him with a switch, twenty years afterwards, if he were to meet with him—but would bear no malice for being shot at, stabbed, or beat half to death with a stick, provided it were done openly and fairly. Whereas, to be tied and whipped on his bare skin, would be an unpardonable offence to commit against an Indian. It is also so with white people. The party inflicting such punishment do it at the risk of their lives, well knowing that it will be revenged. A negro, however, so far from feeling any revenge against his overseer or master, for punishing his faults in that manner, actually loves and respects him the more for it, provided it be done mercifully, yet effectually; and the person inflicting the chastisement keeps his temper. The negro has a perfect contempt for the master or overseer who would strike him with a stick, or his fist, or draw any deadly weapon against him. He resists any such procedure, in the shape of punishment, as an abuse. Whereas, nothing is more common than for an overseer to go unarmed and alone among an hundred negroes, and to flog one or more of them. He seldom meets with the least resistance, no matter how weak or feeble he may be in body, (if he be a man of courage, and the negroes he attempts to chastise are not under the influence of spirituous liquors.) If they be, he should wait until they get sober before chastising them. It does a negro no good to whip him when drunk, and it ruins him when sober to be whipped by an angry or drunken man. He will run away from an intoxicated master or overseer, or from a look of rage, but will nearly always trust himself to the white man and quietly take of his shirt to be whipped, if the white man be sober and unmoved by any violent passion. It insults him to be whipped over his clothes. These facts will scarcely be credited by those unacquainted with the nature of negroes. A million of witnesses, if put under oath, will vouch for their truth. They are sufficient, if they stood alone, to estabtish a broad difference in pyscology and physiology between the white man and negro, which has not attracted sufficient attention from the medical profession. The mind of the negro begins to develop itself at an earlier period, and comes to maturity sooner than that of the white man. Negro children, from this principle of precosity, are in advance of

white children of the same age and opportunities, but their minds, after they arrive at a certain age, five or six years earlier than white people, cease to be further developed. That there are three distinct species of men, typified by the three reputed sons of Noah, is proved by the fact that the mixing of them, like the mixing of any other different species of plants or animals, produces bybrids. That the mulattoes are hybrids, is proved by the fact that they cannot, for any great length of time, perpetuate their like. A nation of mulattoes would be an impossibility, viz: a cross of the negro and the white man. The cross between the white man and Indian—Japeth and Shem—is fast running out, as is seen in Mexico—the Indian blood predominating. After a few crosses with the white man, the negro blood entirely runs out. Nothing perpetuates the hybrids but a crossing of the typical races. Ardent spirits have the same injurious effects upon the hybrids as upon the typical races from which they sprang. The use of alcohol in any form tends greatly to destroy what little fecundity the hybrids possess. Its use greatly diminishes the fecundity of the typical races. Thus, on plantations, where the negroes are in the daily habit of using ardent spirits, the decrease is greater than the increase. Whereas, on plantations where intoxicating drinks are entirely prohibited, the increase of the negro population is about three per cent. per annum. It is highly important for those physicians who are, or intend to engage in a country practice in the South, to make themselves fully acquainted with the anatomical, physiological and pyscological peculiarities of the negro race. From not doing so they have lost much of the practice they otherwise would get—which has fallen into the hands of quacks and overseers. Unfortunately they can find very little information on the subject in medical books or at medical schools. They cannot get it at all unless from observation, if the Medical Journals do not supply the deficiency. The conductors of those Journals, in the general way, seem to forget that we have more than three millions of people among us of a different race from Europeans, and requiring peculiarity of treatment, who are almost entirely unrepresented in medical schools and books, and that the treatment best adapted to the diseases of the half-starved paupers of the European hospitals, is so little applicable to our full-fed negroes, as to drive their owners from all those doctors who pursue such a practice in the treatment of their people. Not because they dislike European learning, but because their negroes die under it. Yet, unless a medical essay is fashioned after some European model, it will scarcely be considered as coming properly within the province of a Medical Journal to publish or to notice it. Southern physicians,

who value their reputation and wish to succeed in practice—the most profitable kind of practice—that among nnegroes, should do something to correct the evil. Everything, which relates to the habits and peculiarities of negroes, their treatment, their food, clothing, lodgings, their drinks, moral, religious and intellectual improvement, however foreign such things may be to European medicine, or however useless a knowledge of them may be to Europeans physicians practising among a different class and race of people, is eminently serviceable and of great practical utility to Southern physicians. Negroes have not only to be regulated in their diet, (as they would eat too much fat meat and throw away their bread and vegetables, if they had their own way,) but they have to be regulated in every other matter, even in their religion; without some one to regulate them in the latter, it evaporates in wild fanaticism, disordering both body and mind. The overseers are often ignorant men, and those who undertake to give the blacks moral and religious instruction too often belong to a class of people entirely unacquainted with the habits and peculiar nature of the people they come to instruct, and consequently so liable to do more harm than good, as to cause some planters entirely to reject their services. Services which would be very valuable, both to the negroes and their owners, if they were under the direction of a theologian of experience and observation, or in lieu thereof, was acquainted with the pyscology, physiology and anatomical organization of these people, who ought to be represented in Andover as well as in medical schools. Without such knowledge nothing can be done to improve their condition, either in a physical or moral point of view. The want of that knowledge causes many unnecessary restrictions to be imposed upon them, and gives rise to many groundless fears of general insurrections and disturbances, which, any one acquainted with their nature knows positively never can happen. For the want of a true history of the insurrection of St. Domingo, and those which have occurred in other places, many groundless fears and forebodings are entertained by numbers among ourselves, in regard to the negroes of our Southern States. In Hayti, the mulattoes and their slaves, instigated by the British and aided by Sonthonax, an abolitionist, in command of the French army, fought against the whites and their slaves, and ultimately exterminated white authority in the island. But such a thing as a general rising of the negroes against their masters never has occurred, never can occur and never will occur; even if they were an hundred times more numerous than the whites. A few negroes under the influence of whiskey and fanaticism have occasionally rebelled, but all such disturbances have been confined to

isolated neighborhood action. There is not only a want of sympathy, but there exists an actual repugnance between negroes of different sections, utterly excluding the possibility of any general rebellion, or concert of action among them. Even on plantations, this want of sympathy for one another is so marked, that the overseers have to keep a sharp look out to prevent one party from instigating the punishment of another party entirely innocent. When negroes are punished, whether rightfully or wrongfully, the majority of their fellow servants are pleased instead of being displeased. The idea of such a people engaging in a general insurrection is preposterous, and never can occur, until their organization of the body and the physiological laws incident to that organization be changed—which may happen, "when the Ethiopian changes his skin and the leopard his spots;" but not before. It is very well known to those who have paid any attention to the subject, that when absolved from the authority of the white man, the negro always leads an ungodly, wretched life, becomes a slave to his appetites and relapses again into barbarism. There are more than sixty millions of negro barbarians in the world—rendered more barbarous by alcohol, when they can get it—and nothing but the want of means to purchase it prevents them from exterminating themselves with it, as the Indians are doing. Yet the British Government, desirous of transferring the cotton and sugar culture to India and Australia; and the Northern abolitionists to satisfy an abstract and erroneous idea they have about liberty, (not knowing the truth, that the people called slaves in America have more real liberty than the laboring people, called freeman in England,) would absolve the wholesome relation subsisting between the whites and the blacks of the South, and turn the negroes loose from their friends and protectors, to become the prey of alcohol. Lord Morpheth knows, that alcohol works the same changes on the negro's moral nature, that the Devil was supposed, by the tradition, to have worked on his physical organization. It blackens all that is white, bright and fair in the moral character, as effectually as the soot bag, according to the tradition, blackened the skin. Yet he and the abolitionists will do nothing for the relief of the sixty millions, the slaves of alcohol and the most gross and sensual appetites—but spend all their money and all their sympathy on a few millions who are protected from the evils of alcohol, are above their sympathies, and better off than their own laboring peasantry; having more christianity among them and more of the comforts of life at their command. In the South, where the negroes are under wholesome restraint, made work for themselves and their masters, and debarred from the use of spirituous liquors, they

are becoming *white men again,* as far as their moral nature is concerned. Whereas, in the North, where they have no friends who will presume to wring the whiskey bottle from their grasp, for fear of offending some abstract metaphysical notion of liberty, the use of alcoholic drinks crisps and sears the negro's conscience as thoroughly as the Devil's red hot hand crisped his hair. It mars the image of God in the moral man, more than the blow on the face, which flattened the nose, marred the beauty and symmetry of the physical man. It causes all the malignant and beastly passions to grow, swell and protrude themselves, like the blow from the real devil caused the lips to swell, thicken and to become more prominent than that organ of the human face which the Hebrews call the avenue to life and intellectuality. In a word, the use of alcoholic drinks makes a negro of the soul as well as the body. Physicians cannot study too deeply the effects—moral and physical—of alcoholic beverages on both master and slave; nor can they err in using all their influence in keeping it away from both. With that evil out of the way, the road will be open for the science of Medicine to be the forerunner of many important improvements, resulting in countless benefits both to the master and the slave. As a medicine, alcohol is of little or no value. Pepper tea and a hundred other things is a much better stimulant. A cup of coffee, sweetened with good hard-grained sugar, is a much better antiphogmatic than any alcoholic bitters that were ever invented. A glass or two of good pure water in the morning, and occasionally through the day, is a much better preventive of diseases of all kinds, than any julep or alcoholic beverage that the ingenuity of man ever mixed. Dr. Fenner deserves much credit for calling the attention of the profession to the injurious properties of water impregnated with lead—and also to the necessity of freeing water of all its impurities by properly constructed filters. The purification of water, by what is called surface action, deserves more attention than has been bestowed upon it, and will require as many people to attend to it properly as are now engaged in making and vending alcoholic beverages. Rain water is generally purer than spring, river or well water. But that contains more or less organic matter. If the water, before it runs into the cistern, be made to fall upon rock, and pass through properly constructed filters it will keep without fermentation or taint of anykind, and those who drink it, provided they avoid ardent spirits and confine their diet to good, sound, wholesome provisions, and take care to remove all substances from or near their dwellings, which have a tendency to contaminate the air, must, from necessity enjoy better health, and their diseases will be of a mild and manageable nature.

A supply of good water, and the absolute prohibition of the traffic in intoxicating liquors, would be a step in political progress, which the high and honorable profession of Medicine will, no doubt, be ready to use all its influence to encourage the people to take. Neither drunken negroes nor drunken white people have much use for doctors. They die, or are as good as dead before the doctors can get to see them; whereas, the temperate live for many years, and after passing three score, they have many ailments, more or less grave, incident to the declining energies of human nature, requiring constant aid and solace from the science of Medicine—any dozen of whom contribute more to the substantial support of the profession than a hundred inebriates, who always treat their minor ailments, by quacking with additional stimulating potations and never think of sending for a doctor until they are about to die, and they nearly always die before they arrive at that age when mankind need the services of the profession most.

III—REMARKS ON SYPHILIS.

BY A. R. NYE, M. D.,

Late Assistant Resident-Surgeon, Charity Hospital, New-Orleans.

That many valuable remedies have fallen into disuse, from an indiscriminate administration or misapplication, is well-known to every medical practitioner. The tendency of medicine, like every other science which is founded upon observation, and which has no fixed basis, is to run to extremes. This often occurs in the medical profession from the ardor of those peculiar minds that adopt the one ideal system, and select a specific for all diseases. Having selected their weapons from the medical arsenal, they go forth to battle against disease, always dealing their blows in exactly the same way, regardless of the position or defence of their adversary.

That this has been and is still emphatically true with regard to the treatment of syphilis, no one can doubt who has been at all conversant with the disease. From the time of Hunter to the present hour, mercury has been regarded as a specific for this disease in all its varied forms. By one class of practitioners it has been given indiscriminately,

without regard to time or temperament. Acting on the principle that if a little is good—more would be better—patients have been drugged with this mineral, until "the last state of that man was worse than the first."

Another class, going to the other extreme, have wholly eschewed mercury and banished it from the list of remedies employed in syphilis. They have even gone so far as to ascribe all the constitutional effects of syphilis to mercury. Quackery, profiting by the ultra-mercurialism of one and the sweeping assertion of the other, has fattened at the expense of suffering humanity, by advertising to cure *all* cases of syphilis without the use of mercury. That they do this, or rather that primary syphilis is seldom followed by constitutional symptoms, whatever system of medication is adopted, is true. They, therefore, gain credit for a preventive treatment which is entirely unnecessary; and if unnecessary, actually hurtful.

"Truth lies between extremes;" and no one should be considered a trimmer who adopts a medium course in this matter. In this article it is not expected that any new light will be thrown on the pathology of syphilis; but if anything is done by it to reconcile the conflicting statements of authors, or to simplify the varied treatment of this disease, the object of the article will be more than accomplished. My aim bas been to test the treatment of received authorities, and then adopt the one found most successful.

The following observations (if they deserve the name) are founded on a series of experiments made in the Female Venereal Ward of the Charity Hospital. The method of treatment adopted, was original in only a few instances. To be able to judge of the comparative merits of treatment and remedies, I have selected patients of a similar constitution and similarly affected, and placed them upon different courses of treatment. From these observations it is not expected that any precise course for the treatment of syphilis can be indicated, for this must vary with time and temperament—but merely a general course, which will apply to a majority of patients. That these observations are subject to many sources of fallacy, no one can doubt—but I submit them to the profession for what they are worth.

If pus be taken from a chancre during the progressive stage and inoculated, it will produce another chancre. But if the matter be previously dipped in either diluted alcohol, alkalies or acids, or solutions of sulphate of zinc or copper, its inoculable power is destroyed. These are therefore prophylactics, and if properly applied, early, before the

matter has gained a foot-hold, will destroy the virus. But a virus being once established in its favorite seats—they are totally inefficient.

The characteristic of primary syphilis is chancre. Chancres are most conveniently divided into simple, phagedænic, sloughing and indurated. *Primarily all chancres are simple; but they assume different forms, after a few days, according to the habits and constitution of the patient.* The inflammatory chancre of some authors, is a form of simple chancre in young and plethoric subjects, who live high. The phagedænic and sloughing generally occur in anæmic, irritable or dissipated subjects, or in those of a syphilitic diathesis. The indurated is the form which the simple chancre assumes when the disease has become constitutional.

The primary object in the treatment of chancre is to destroy the virus as early as possible, and thus prevent constitutional infection. This is accomplished by caustics—a variety of which are recommended. The application which I have found most efficacious, is a concentrated solution of nitrate of silver. A solution is to be preferred to the solid caustic, because the liquid penetrates to every part, while the solid often barely smears over the surface of the ulcer, and leaves some point of infection beneath. Mr. Graves says, that solid caustic is more likely to give rise to bubo—if this be so, it probably follows because the virus is imperfectly destroyed by the solid, and absorption takes place. Previous to the application of the caustic the parts should be thoroughly cleansed and dried—otherwise some of the virus may exist on the neighboring skin, and if not removed will re-inoculate the sore, when the eschar, formed by the caustic, comes away. Immediately after its use, dry scraped lint should be applied to absorb any excess of fluid or exudation of matter.

Nitrate of silver is preferable to any other caustic, for it is milder and often sedative in its operation, and when the eschar formed by its application comes away we often find the sore perfectly healed, while other caustics produce inflammation and pain, and coming away by a deep slough, leave an ulcer to be healed by granulation—and the former is equally efficacious when properly applied. If the proper degree of care and cleanliness have been observed, a re-application of the caustic will seldom be required. Cleanliness, with dry lint changed daily, will generally suffice to effect a perfect cure. Some surgeons cauterize daily, until the sore is healed. But I have noticed that the cicatrices of ulcers healed in this manner, are hard and contracted, and therefore liable to laceration. Acute inflammation is generally supposed to contra-indicate the use of nitrate of silver—but if it be ap-

plied to the *whole* inflamed surface, it will not only diminish the inflammation, but will allay the pain. For which reason patients will often request its re-application.

The local treatment of phagedænic and sloughing chancres is of course modified by circumstances. A wash, composed of one or two drachms of nitric acid, to six ounces of saturated acqueous solution of opium, will act favorably in a majority of cases.

The treatment of the indurated is much the same as that of the simple chancre, except that care must be observed in the application of the caustic, or the sore will become irritable and gangrenous. Black, yellow and astringent washes do well in these cases.

Our attention has hitherto been confined to the local, to the exclusion of constitutional treatment. Nor is such treatment necessary, except in cases of indurated chancre. But, unfortunately, this is by no means the universal opinion—for I have repeatedly seen patients enter the Charity Hospital, who had been profusely salivated for the slightest primary accidents.

Our best authors say, that if the abortive treatment be applied to simple chancres before the third, or even the fifth day of their existence, constitutional infection will be prevented. But it is evident that no definite period can be fixed as a limit to infection. This depends entirely upon the constitution of the patient—the absorbents being much more active in one person and at one time than another. A much surer and safer indication is the state of the chancre and its consequences. If there exists no induration of the base of the chancre, no enlargement of the inguinal glands, or if there be specific suppuration of those glands, there is no danger of constitutional disease.

As a case in point, M. J., 18 years of age, entered the Female Venereal Ward of the Charity Hospital, in the spring of 1851. She was robust and perfectly healthy, with the exception of four simple chancres, in the vicinity of the labia minora. It may be well to mention in passing, that in a great number of cases which I have examined with the speculum, I found only one chancre near the uterus. They have always been about the entrance of the vagina. This patient had discovered these chancres nine days previous to her entrance into the Hospital, but had done nothing for them. Inoculation produced a specific sore. The chancres were treated with a solution of nitrate of silver, and healed kindly in four or five days. She then left the Hospital. She returned again in the spring of 1853, with simple chancres. The same treatment was again applied, with a similar result. In the meantime, from the spring of '51 to that of '53 she had been perfectly

free from any signs of constitutional syphilis. Does not this plainly show that chancres may exist for an indefinite period, without infecting the system? She had had chancres for nine days (and probably for a day or two before she discovered them) and although no constitutional treatment was adopted, yet she escaped infection.

If M. Ricord's assertion is true, that "when no specific treatment has been adopted—when the disease has been left to itself, six months never elapse without the manifestation of symptoms of the syphilitic intoxication." She should have presented those manifestations. Numerous other cases of a similar import might be adduced, but one will serve to illustrate our position. Had this patient been placed upon constitutional treatment, would it not have been worse than useless? M. Ricord and Mr. Wilson both say, that when the poison of syphilis is once absorbed, it is never eradicated from the system, but that a diathesis is established. M. Ricord also says, that constitutional treatment immediately after chancre, does not *prevent* the manifestation of constitutional disease, but barely *delays* it. Then, in a case of chancre, presenting none of the signs of infection, no treatment is necessary; and even doubtful cases should be left to time—for if absorption of the virus have taken place, it will be manifested within six months, and we can then adopt a specific treatment; while, if we commence at the outset, and attack an equivocal disease, we only delay its appearance should there really be infection; and if there be none, we subject the patient to useless inconvenience; or worse still, we may actually produce disease by our system of medication, especially if mercury be used without care or judgment.

Phagedænic and sloughing chancres are never followed by constitutional symptoms. Therefore, no specific treatment is necessary; we have only to treat the patient according to indications. A tonic course, especially of the mineral tonics, with occasional full doses of opium will succeed in a majority of cases, in renovating the system and removing that irritability peculiar to such patients.

Leaving the primary form of this disease, we now come to its graver aspect, when the virus has invaded the system and penetrated to every portion of the animal economy. This is indicated in a great variety of ways, and the disease assumes a multiplicity of forms, according to the time, temperament and treatment of the patient. It is indicated primarily by indurated chancre and enlargement of the inguinal glands, followed sooner or later, by affections of the skin or mucous membranes, and by ganglionic enlargements, particularly of the posterior cervical ganglia. All which accidents are considered secondary; whereas,

affections of the cellular, fibrous, serous, periosteal and bony tissues, constitute tertiary syphilis.

There are very few cases of secondary syphilis, in which mercury may not be used with signal success. The exceptional cases are mostly anæmic, dibilitated and scrofulous subjects. But it is by no means intended that mercury should be indiscriminately used. Much care and discrimination are necessary in suiting it to each particular case. While some patients may, with benefit, take one grain of calomel, or protiodide of mercury three times daily; others cannot go beyond one-fourth, or even one-sixteenth of a grain. Let us not forget in our impatience to effect a cure, that we are dealing with a chronic and not an acute disease. Here we have a poison circulating or seated in the system which we wish to remove. How are all poisons eliminated from the system? By the secretions. Therefore to eliminate a poison like that of syphilis, we stimulate the secretions; and this is most effectually done by mercury. But mercury in excess, after a time, will not only check secretion, but it will go farther—it will give rise to inflammation. Thus, instead of removing, it may actually engender disease. We cannot, therefore, be too careful of its administration or too vigilant in watching its effects. It is rarely necessary to push it to salivation—only the slightest evidence of its effects are required. The mercurial course should be followed by a course of the iodide of potassium, which acts much better, and more speedily after mercury. The antecedent course of mercury seems to induce a state of the system highly favorable to its action. The mercurial generally preferred, is the protiodide—for it is slower in its action, and therefore not so liable to produce salivation or other disturbances of the system. But where the evidences of the syphilitic intoxication are acute, in Iritis—it is too slow in its action, and calomel should be substituted for it.

Of the treatment of the syphilitic skin diseases, very little can be said—for this class of diseases is not common in this region, There is something in a tropical climate unfavorable to their development. Those forms of eruption which I have witnessed, viz: lepra, lichen, lupia, tubercles and psoriasis, were benefitted by a course of mercury and iodide of potassium combined; I have had two cases of psoriasis palmaris, and one of lupus in private practice. The cases of psoriasis were treated with the biniodide of mercury internally, and the citrine ointment locally, with very favorable results. The case of lupus was treated with Donovan's solution, and the biniodide of mercury ointment, and recovered. These three cases were very clearly traceable to syphilitic infection. Donovan's solution is extensively used, in various

forms of skin disease, with very happy results. Is not this probably, entirely owing to the mercury held in solution? And does it not tend to prove the position of Mr. Wilson, that nearly all skin diseases are dependant upon the poison of syphilis?

In the British and Foreign Medico-Chirurgical Review, for July, 1851, will be found an article by Dr. Williams, recommending iodide of potassium, in preference to mercury, in a majority of secondary accidents. But the weight of authority is against him. Probably, a perfect cure cannot be relied on by this agent alone. I have seen several instances of various secondary accidents, as affections of the skin, ulcerations of the throat, etc., with enlargement of the posterior cervical and cervio-cephalic glands, which were very much *benefitted* by the use of this remedy; but I have never seen a permanent cure from it. In several instances the affections of the skin and throat disappeared, but the enlargement of the above-mentioned glands remained; and thus happened, notwithstanding ounces of the iodide had been taken for weeks and months together. On the other hand small doses of mercury have never failed to remove the enlargements of these ganglia, in a few days. No doubt the engorgement of these ganglia alone, (to which M. Ricord has called attention) are valuable pathognomonic signs of constitutional syphilis; and a patient should never be considered safe while this state of the gland continues.

We now come to that form of syphilis (the tertiary) in which the iodide of potassium is more extensively used, and with more satisfactory results. But even here there are very few cases in which mercury cannot be judiciously administered, either before or in conjunction with the iodide. Patient will improve much more rapidly by this course than by the iodide alone. But here mercury must be given with much more care, and in smaller doses than in secondary syphilis; for we have to deal with a more chronic form of the disease. Occasionally we meet with a patient, who is so much dibilitated, or on whom the disease has made such sad havoc, that this course would be ruinous. For such we can do more by a tonic treatment.

Perhaps, the foregoing article might have been rendered more interesting by a fuller interspersion of cases, or by merely a report of cases. It may be interesting to the medical historian to know the train of reasoning and experiment by which we arrrive at certain conclusions; but to the medical practitioner, it is much more interesting to know the conclusions themselves. Therefore, we have preferred to give results instead of cases.

IV—A CASE OF PLACENTA PRAVIA.

BY H. E. FRITCHELL, M.D., ALA.

Nancy, the property of H. L. R., a stout robust woman, aged about 34 years, pregnant with her eighth child since about the first of October last, was attacked with uterine hæmorrhage, accompanied by pains, on the 10th of June, and which continued at intervals, and to a greater or less degree, until the 13th, when a very considerable increase of the bleeding occurred, in consequence of which I was called to her at 6 o'clock, P. M., of the same day. On my arrival, I was told by the "granny" (in whose hands the case had been detained up to this period,) that her pains were hard and that she had just lost about a quart of blood. Instant examination ascertained the soft parts to be well relaxed, and the os uteri already dilated; and as was anticipated, the placenta was found occupying the entire circle of the womb's mouth, centre to centre. Turning was immediately determined upon, and the patient placed in the proper position. It was easily known, from the tense and rounded form of the placenta, that the head was presssing upon it from above; but of course impossible to detect the exact relative position, and in the hurry of the moment not reflecting that that position of the child, in which its vertex is to the mother's left, is much the most frequent, and that consequently, the chances were by so much increased for my left? (being the proper hand with which to operate.) I introduced the right, gently separating the placenta at its edge, and without difficulty passed into the womb, finding to my great mortification that the palm of my hand was to the child's back, and turning impracticable, until I should change hands. This was the work of an instant, the hæmorrhage being frightful on the withdrawal of my right, but speedily arrested by the introduction of the left. The remaining portion of the placenta was now detached, the hand passed to the feet, and the child easily turned and drawn down, so as to have its limbs and hips occupy the entire circle of the os uteri. A dose of ergot, which had been previously prepared, was now administered to ensure the speedy contraction of the uterus after delivery; which was hurried as much as possible, with the hope that the child might yet be saved from the consequences of an entire separation of the placenta, before its respiration had been established and the loss of blood to which it was still subjected. But, although, upon delivery, it exhibited some feint signs of life, for the want of an intelligent assistant, artificial respiration could not be instituted in time for resuscitation, as the condition of the mother required

my attention until the womb contracted and the hæmorrhage ceased. The woman is now doing well.

The chief point of interest, and the only reason of my sending you this case for publication, is to impress upon the minds of practitioners the advantage of using the left instead of the right hand, when the exact position of the child is not known. A rule, the importance of which will be at once recognized by any one, who, like myself, may be subjected to the painful necessity of changing hands under such circumstances, and to the neglect of which in this case, the life of the child was evidently sacrificed, and the mother's danger increased. True, in using the left, the accoucheur may not know he is right, but the chances are increased by so much, as this position of the head is more frequent than that in which the vertex is to the mother's right; and according to Madame Boivin this vertex left position occurs in more than three-fourths of head presentations. This being the case, it would seem extraordinary thoughtlessness to act as I did, but called upon the spur of the moment to officiate under pressing and alarming circumstances, without much time for reflection, it is not to be wondered at, that errors will sometimes creep into our practice.

June, 1853.

V.—THREE CASES OF PHTHISIS, IN WHICH THE MORE RARE FORM OF MILIARY TUBERCLES EXISTED, WITH THE POST MORTEM RESULTS.

BY D. MACGIBBON, M.D.,

One of the Attending-Physicians to the N. O. Charity Hospital.

The following three cases, in which an immense deposite of miliary tubercles was found in the respective bodies after death, occurred under my charge in the Charity Hospital, and are chiefly interesting from the fact, that this form of phthisis is somewhat rare. This will be understood from the fact mentioned by Watson in his "Practice," who, notwithstanding the extended field for observation which he occupied, had then seen "three or four only in number" of these. Those

referred to above, occurred close on one another, as will be seen by their respective dates.

Another point of interest connected with this form of disease, is the difficulty with which it can be diagnosed. The same excellent authority already referred to, admits that in the cases seen by him, the true nature of the disease was not suspected till after the patients' death. The cases are condensed, from notes taken at the time, for insertion in the Journal.

CASE I.

June 12, 1851.—Mary Doyle, a little girl, aged about 7 years, who had been for sometime kept about ward 35, among the sick, having no relations to go to, was at the above period under my care as a patient. She was of a scrofulous diathesis, and threatened with mischief in the lungs. About three weeks preceding this she had a slight attack of bronchitis, which confined her to bed a short period. She soon got well of this and began to walk about the house as usual. A residence in the Hospital, with her delicate constitution, was more likely to injure her general health than otherwise, and so it proved. She was soon seized with a second attack, which commenced much as the former, only more severe.

On auscultation, a wheezing sound was heard in the left infra-clavicular region on each inspiration. Behind, again in the same side bronchial respiration, with sub-crepitation, was heard. This side of the chest, owing to a slight curvature of spine, looked somewhat larger than the other, and made some who saw her suspect pleuro-pneumonia. Respiration in the remaining portions of the chest, though not full was at this time clear; a slight cough was present.

The former attack seemed to be confined to capillary bronchitis; this time the larger bronchiæ also were obstructed, as indicated by sibilus; while the sub-crepitation heard directed suspicion to a deposition of tuberculous matter going on in that quarter. Under appropriate treatment her symptoms for a time seemed to improve; but her general health soon after was observed gradually to become more and more impaired. On the 22d July, the following note of her condition was made: "Mary Doyle, who sometime since was ailing, and got better, is again confined to bed. For some nights past she has had attacks of remittent fever, for which she had quinine. She is also rather loose in her bowels. In the sub-scapular regions bronchial respiration is heard louder, and the same is heard in the infra-clavicular region, though less distinctly. In the upper lobes behind, other abnormal

sounds of a cooing and clicking character are also heard; while in the lower lobes respiration is clear; if anything rather too puerile. She has a good deal of cough, especially at nights, when it comes on in paroxysms. Three nights ago she had slight hæmoptysis."

She had a cough-mixture allowed her; also a mixture to check the diarrhœa. The evidence of obstruction in the respiratory organs, from increased tuberculous matter, went on rapidly increasing, till it involved all portions of the lungs. She gradually became more and more debilitated; though her appetite still remained good. She had hectic fever at night; her tongue became red and smooth-looking, indicating greater derangement of the bowels.

She was all this time a patient creature, giving little trouble to any one. Formerly it was difficult to retain her in bed; now, however, she had no desire to leave it. The dysenteric symptoms increased in severity. The air entered the lungs more and more scantily; she spoke in a low whisper. On the 3d of August she died. There were no brain symptoms whatever till the day preceding her death; when she would frequently get up and go to another bed. Her features were much sharpened, and she was otherwise greatly emaciated by her disease.

The *post-mortem* examination of the body, showed the most extensive deposition of tuberculous matter in the lungs and other parts I had ever witnessed in any subject. Innumerable minute tubercles studded both the costal and pulmonary pleuræ of both lungs, completely gluing them together; in the left chest this agglutination was especially complete and difficult of separation. The bronchial glands, on this side especially, were greatly enlarged; in the neighborhood of the biffurcation several were very large; one of them indeed as large as a pigeon's egg. When cut into they exhibited the crude stage of maturization; one of them alone was a little softened, having in its centre a little yellow matter.

The substance of both lungs throughout was crowded with the same kind of miliary tubercles; that of the right more so than the left however. The tissue surrounding these was of a gray normal color. A little frothy matter was yielded by the cut surfaces when squeezed between the fingers. One small cavity, where suppuration had taken place, was noticed in the left side.

Neither the *heart* or *pericardium* had any of these. The *liver* had a few cheesy points under its serous covering, and in its substance one little granule, tinged yellow, was found. The *spleen* was normal; and so also were the *kidneys*. The great omentum was in a special mannner strewn with these; and when held up between one and the light,

myriads of these minute granulations were seen most distinctly and beautifully displayed. The mesenteric glands were very much enlarged; these, encysted and globular, along the inner course of the small intestines, were clustered together into so many bunches, and presented a very different appearance from the more common infiltration of tuberculous matter in hypertrophied tissue, usually met with in the young scrofulous subject, affected with tabes mesenterica. The coats of the large intestines were thickened. A portion of slightly opaque fluid, on opening the cranium was found between the brain and its membranes; and in the lateral ventricles was a little of the same. The brain itself was somewhat softer than usual; on the periphery of the right hemisphere, near to the apex, two small white tubercles were found under the membranes, and partially imbedded in its substance; when cut into two more were found in the substance on the same side, about the size of peas. The other hemisphere of the brain as well as the cerbellum were examined, but none found in them.

Both from the symptoms during life, and the condition found after death, it would seem that the bronchial glands were the earliest affected, and soon thereafter all parts of the respiratory organs became rapidly filled with this peculiar tuberculous deposition. The absence of inflammatory action in the chest, as evidenced by the state of the tissues after death, showed the correctness of the diagnosis, that phthisis, not pulmonary inflammation was the complaint, though at the outset it is probable, that there was more or less of this present also. The condition of the brain was not suspected.

CASE II.

August 20, 1851.—Eliza Doyle, aged 6 years, sister to the former; like her had been kept sometime about the wards of the Hospital. She also was pale looking, and evidently of a scrofulous diathesis; and living in the Hospital, among the sick, was not calculated to improve her constitution. She began to complain about this time, and became my patient. At first she had a slight remittent fever, which would leave her in a great measure during the day, when she would move about the house, and in the evening it would return again, when she would take to bed; her face flushed and her skin hot. Her bowels were also constipated. She had a draught containing ten grains sulphate quininæ, and some opening medicine allowed her; when she soon got better and was again running about. The same symptoms soon again showed themselves; and this time there was also some vomiting, which persisted more or less for two days. For this latter, she had hydrocyanic acid

prescribed, which checked it. From this period she kept her bed constantly. A degree of restlessness was observed in her manner, and she would occasionally give a sudden scream. She complained of no head-ache, but there was an evident disinclination to hold up the head. While thus disposed she would, nevertheless, now and again hurriedly start up and call for her clothes to get up. At times a slight confusion in her memory of things was observed. The temperature of the head remained natural; and there was no suffusion of the eyes. Some cerebral disturbance or other evidently existed; and both from the symptoms and what was observed in her sister's case, scrofulous softening of the brain was suspected. Auscultation did not at this period detect any chest affection. The respiratory sounds were, if anything, rather puerile, which was the only suspicious circumstance connected with them. There was no cough.

She was put upon the protoiodide ferri solution, five drops of which she had three times a day in a little syrup, and she had as much nourishing food as she could take. There was no enlargement of the abdomen, and the bowels were rather torpid than otherwise, a better condition however than the opposite. The brain symptoms gradually grew graver. A degree of stupor soon came on, from which however she could be aroused by a question put to her, when she would answer clearly enough. Her pulse was quick, but there was no unnatural heat of the skin. She continued to take her food well. To relieve the irritation of the brain, tartrate of antimony ointment was now rubbed on the nape of the neck; in the arm-pits and groins blue ointment was rubbed in the same way, with the view of changing the abnormal action going on in this organ. But the mischief went rapidly on. Raving, with fixed stupor supervened, and she died on the morning of the 27th August.

Shortly after her demise, a *post-mortem* examination of the body was made, and the following pathological results obtained: The cranium was opened first. There was no effusion on the surface of the brain; but in the left ventricle a considerable portion of straw-colored serum was found, and in the right a little of the same also. The most marked change, however, was that found at the base, near the junction of the cruri with the pons, where ramollissement existed; which on the left side extended into the ventricle.

On the dura mater behind and close to the falx were a few minute yellow granulations, such as are not unfrequently met with; and on the surface of the brain, in this neighborhood, a few also were observed.

On the surface of the left lung, a number of tubercles were found deposited; some of them pretty large and flat. These infiltrated tubercles caused the pulmonary and costal pluræ on this side to adhere together. There were no such adhesions in the right chest. An immense number of minute tubercles were found scattered throughout the substance of the left lung; and its areolar tissue was somewhat engorged with blood. The right lung also contained a great number of the same kind of granulations; but there was no engorgement of its tissue. The bronchial glands were not affected. In the pericardium a considerable portion of straw-colored serum was found. The serous covering of the liver had a number of these small granulations on it, which glued it to the abdominal walls at several points. On the surface of the spleen were a number of the same. The mesenteric glands were enlarged; some of them considerably. The coats of the intestines were much attenuated; this was so with those of the large intestines in a special manner, which were almost transparent.

The strumous diathesis was strongly marked in this girl. The condition of things found in the brain was not unexpected, but agreed with the more prominent of the symptoms present in her last illness. No doubt brain symptoms exist where no such change can be detected at the post-mortem; and the reverse of this is also true, as was, to some extent, exemplified in the case of her sister. West, in his "Lectures," speaking of these discrepancies says, "to the best of my knowledge, however, the brain in the immediate neighborhood of the tubercular deposit does not present any sign of softening in cases which have been characterized by the absence of the signs of cerebral disturbance." The cases of these girls would bear out the matured opinion here expressed.

The condition of the respiratory organs was unexpected. As she lay in bed, in her last illness, I frequently applied my ear over the front of the chest, but failed to detect any abnormal sound which would indicate such a deposition of tuberculous matter as was found in the lungs at the post-mortem. In the former case, as well as the one to be now given, the sounds were different, and guided by these the diagnoses were correct.

CASE III.

August 12, 1851.—Ellen Russell, aged 22 years, entered the Hospital on the 5th instant. She stated that about a month ago she caught cold while living with a family up the coast, since which she had not

been well; she also stated, that she had prior to that been in the enjoyment of good health.

Her breathing was at this time much affected; it was short and hurried. She also had a bad cough, and complained of occasional pains in different parts of her chest, especially the front. Her face was pale, and her lips were blanched; yet her features did not seem to have lost much of their fullness. She had evidently been a very beautiful woman, with light brown hair. She had been married, and had been the mother of a child. She was of late accustomed to sit closely sewing for her living, which may have contributed to impare her health.

On auscultating her, which I did the day she entered, both lungs were found to have their action greatly obstructed. At each inspiration the air entered scantily. In no portion whatever could vesicular respiration be heard. A dry crackling respiratory murmur was heard wherever the ear was applied; and most distinct on expiration. Little or no ronchus was present. Acute phthisis was diagnosed; and, from the sounds resembling much the sub-crepitation heard for the most part in Mary Doyle's last illness, I suspected a similar deposition going on in the present case; and that, too, rapidly, as the short time which elapsed since she began to complain, with the wide spread which the deposition of tuberculous matter within that period had attained, too well evinced. In thus resembling more what is popularly known as "galloping consumption." As yet there was little evidence of softening having occurred. Her sputa, which was rather tough and frothy, like what occurs in the acute stage of bronchitis, was scanty; and occasionally this was tinged with blood. There were also some few dots of yellow matter observed in it. Her pulse was rapid, and her skin hot. But little hopes were enteriained by me of checking the progress of the complaint, and my chief efforts were directed to relieving the symptoms, to soothing and sustaining her as well as possible.

To relieve the pleuritic pains, she was blistered on the front of the chest, with relief. The difficulty of breathing became every day more and more imminent; and, to all but herself, it was but too evident that she was rapidly sinking. On the morning of the 11th, having given her a full opiate draught the preceding night, which procured her a better night's rest than she of late had been accustomed to, she reported herself as much better; and, for the first time made some inquiries of me regarding her ultimate recovery. It would seem that the "sister" in charge of the ward, in answer to similar inquiries put the preceding day by the patient, mentioned the unfavorable opinion I had formed at

her entrance, (for I expressed my suspicion of her rapid dissolution to the "sister" then) and she advised her to rather prepare for the worst; hence, the anxiety evinced on this occasion to hear from myself what I thought of her case; and, from the manner in which the question was put, it was evident she expected an answer other than that I thought it a hopeless one. It is thus that to the last she buoyed herself up with false hopes of recovery—nor was it ours needlessly to destroy them. In this, as well as in her personal appearance, she realized much more truthfully, the pictures non-medical writers are accustomed to draw of consumptives, where the cold finger of death is represented as having touched the fair and the beautiful, blighting them henceforth, unconsciously to themselves, in a mysterious manner—than the greater number of the victims of consumption do, that at least come in the way of our Hospitals, and are seen in our dead-houses. The same afternoon she calmly breathed her last.

On the morning of the 12th, the body was examined, and the following *post-mortem* results were obtained: The body seemed but little emaciated. The disease ran its course so rapidly, and there was at no time any diarrhœa present, so that the more usual causes of extreme exhaustion in these did not exist in this subject.

In both lungs in front considerable pleuritic adhesions existed. Behind again the pleural surfaces were firmly attached, not with bands of organized lymph, but with a thick setting of miliary tubercles, such as that already described. The substance of both lungs throughout was crowded with a deposit of miliary tubercles, most of them little larger than a pin's head. Nearly all these had reached the same stage of maturization; a few alone had commenced to soften. No cavity was observed. The substance of the lungs when pressed between the fingers crepitated throughout. The crop of tubercles sown throughout both lungs was indeed immense. No wonder that respiration had been so obstructed, and the arterialization of the blood so defective. The pericardium contained a larger portion of serum than usual. The heart was a little larger and flabbier than natural. Both ventricles contained clots of black blood. The mesenteric glands were in no way affected; and no tubercles were observed in any part of the abdominal cavity. I had a desire to open the scull to see the condition of the brain, but a vilanously bad saw, as well as some repugnance to deface features which yet retained much of their beauty, induced me to spare further interference.

It is not improbable, that some deposition had taken place in the lungs prior to the period she dated her ailment from; and that this had

by its presence at the time she spoke of taking the cold, begun to excite inflammatory action there; so that this inflammation, instead of being the cause, was itself the result of her disease, and by combining with it, tended to bring this to the rapid termination we have seen.

VI.—HÆMATOKINETY, OR THE NEWLY DISCOVERED MOTIVE POWER OF THE BLOOD.

BY SAMUEL A. CARTWRIGHT, M.D., NEW-ORLEANS.

While the new doctrine of the motive-power of the blood was in process of publication and experiments were being instituted to establish its truth, it was assailed by Albert W. Ely, A. M., M. D., in the July No. of this Journal for 1852. But scarcely had the celebrated reviewer received the congratulations of his friends, for the able manner in which he had put the new doctrine of the blood-moving power, called hæmatokinety, down, when he himself instituted an experiment on his own child, that did more to put it up again than all he had written had done to put it down. His child died, and he brought it to life by bringing into action the very hæmatokinetic or blood-moving power which he had been laboring to prove had no existence. After this, I made some remarks in explanation of the new doctrine, and published the same in the September No. of this Journal, for the year 1852. In the November No. following, Dr. Ely was down upon it again, in a long article headed "*Motive-power of the Blood*,"

I did not reply to it, because all those well versed in Natural History would perceive that it needed no answer, his data being incorrect, and it was perfectly useless to get into a controversy with those who had not studied the subject. Dr. Ely had got hold of an incorrect translation of Cuvier's works, and turned the same against the new doctrine with tremendous effect in the eyes of all those unacquainted with Comparative Anatomy, and who had never seen the Napoleon of Naturalists in any other than an awkward English dress. Some very celebrated scientific men, as Rogers at the North, and Dowler and Riddell at the South, did not see the full value of the experiments, which had been made to establish the new doctrine. They even misinterpreted them, and supposed that they did not prove what they were cited to prove

They doubted whether tying the trachea would kill an alligator in any reasonable time. They thought with Dr. Ely, that the ligation of that tube might perhaps cause an alligator to die in the course of two or three days, but the experiments were not sufficiently conclusive to convince them that it would destroy life and muscular irritability in less than an hour, by stopping the play of the newly discovered hæmatokinetic power. They believed that an animal, in a state of asphyxia or apparent death, might revive after inflating the lungs or after doing many other things, but the experiments were not sufficiently conclusive to convince them that the mere inflation of the lungs could bring the dead to life, or even remove asphyxia, by awakening into action any, heretofore, unknown motive-power. Instead, therefore, of entering the field of controversy against such able champions, with the hopeless purpose of beating the truth into them by dint of argument, I preferred enticing them to make a few experiments themselves or to witness them made by others, that they might be more fully convinced of the utter fallacy or truth of the new doctrine, called hæmatokinety. Dr. Rogers, who had written and published some violent articles against it, in the Boston Medical and Surgical Journal, was the first to lead off with an experiment to put down hæmatokinety forever. He made the experiment on a dead child—or one supposed to be dead—and to his utter astonishment, instead of its having no effect, as he supposed it would have, the child came to life as Dr. Ely's child did. Both the children are living yet. So was Miss Griswold brought to life in the same manner, by some person at the North. The poor girl was drowned, and instead of burying her, some one brought into play the hœmatokinetic power and restored her to life. But such cases were not sufficiently demonstrative to Dowler and other Southern unbelievers in the hœmatokinetic or newly discovered motive power. They wanted to see the power stopped and the power set to going again, before they could believe. They had seen it stopped and put into action again in a number of instances, yet they thought there might be some error, some experientia fallax, lying at the bottom. To give them a chance to fish up these errors, if there were any, I procured a number of alligators, some of very large size, and invited them to be present at a series of experiments, commencing the 1st of June last. The hæmatokinetic or blood-moving power was ordered to stop, and it did stop by a simple string thrown around the trachea, which did nothing more than to exclude the atmospheric air—thus proving that atmospheric air, admitted into the lungs, is the blood-moving power. The animal died in less than one hour, even muscular irritability was abolished. This animal

lay by the side of one which Dr. Dowler was experimenting on. He had cut off its head, and cut out its spine, and taken out its viscera, yet it was still alive, would jump and kick when fire was applied to it, or when pierced with a knife, and would make intelligential motions to get rid of the torture. Its head was off, but its blood was alive—muscular irritability was preserved, because no means had been used to stop the hæmatokinetic power which the blood derives from the atmospheric air. Fire was applied to the other alligator whose hæmatoknetic power had been arrested by ligating the trachea. It moved not. Its nerves were pierced and torn. It felt no torture which could be inflicted upon it. It was dead. It was then cut open and the viscera exposed preparatory to putting into motion the hœmatokinetic power again. In doing so a large artery was cut, and it lost much blood. There was not enough left, when vivified, to restore life to the general system. Inflation was suspended, and the feeble manifestations of life which it had caused, soon ceased, when the hæmatokinetic power, awakened in the remnant of blood by that process, had become exhausted. A third alligator, also killed by tying the trachea on the same day and hour of the two above-mentioned, was cut open, its heart and lungs exposed, and afterwards brought to life by putting the newly discovered power into action, by the simple process of insufflation.

Again, on the 6th of June, in another experiment on a crocodile, some six feet long and as thick around as a common sized man, (and had swallowed a stone as large as a child's head,) the hæmatokinetic power was stopped by a string, in less than an hour applied around the trachea. After death occurred the viscera were exposed and in opening the pericardium the auricle of the heart was accidentally pierced—The loss of blood was so great that there was not enough left to fully restore life by having the hæmatokinetic power restored to the balance. Some feeble manifestations of life were all that were elicited by the insufflation. On the 13th of June another saurian of the same large size was subjected to a similar experiment. The hæmatokinetic power was arrested by shutting out the atmopheric air. Death ensued. Flames of fire directed against the most tender parts of the body produced no quivering or flinching—cutting the muscles caused no motion or contraction, because muscular irrtibility had ceased to manifest itself. Motionless the animal lay on the table with the dissecting instruments in full play upon it. The viscera were exposed by an incision from the upper part of the chest to the cloaca ; a cross incision was made through the muscles and the flaps fastened back with hooks. The pericardium was slit open and the heart taken out, hanging only by

its vessels. The lungs were exposed and the dark stagnant fluid contained in the pulmonary vessels could be seen through the diaphanous tissues. The latter were pale and apparently bloodless. Means were now instituted to bring the newly discovered hæmatokinetic power into play, and make it a subject of occular demonstration. The means were simple—they consisted in cooling the body with ice-water by frequent washing or rubbing with ice, the introduction of the nozle of a common fire bellows into the trachea and blowing with the same as if kindling a fire, occasionally depressing the thorax and abdomen with the hand to expel a portion of the air. Soon the diaphanous membranes began to be injected with blood of a scarlet hue. The irritability of the muscles began to be restored and to contract and quiver under the application of fire. The muscles of the tail were the first to move. After the insufflation was continued some time longer, the power of muscular motion was restored to the whole body except the head. The divided intercostal muscles were then stiched together—the heart returned to the pericardium and the slit in that, some five inches long, sewed up. The bellows was then removed and the animal breathed of its own accord. At length the head came to life and the glazed eye-balls lost their dullness and assumed the brightness of life. Resusciation was complete and the crocodide crawled about the yard for a whole week before it finally died. A number of physicians and other persons daily called to see it. This experiment was decisive in proving that there is such a thing as a hæmatokinetic or blood-moving power, which was stopped by excluding air from the lungs and put into action again by its admission into those organs. Dowler and all others, who witnessed these experiments, gave up their doubts and became convinced that tying the trachea would soon kill an alligator, and is the most expeditious of any other method in producing death. This was tantamount to admitting that the exclusion of the air from the lungs and preventing the escape of carbonic acid, arrested the chief motive power of the blood. It should be remembered that the heart was the last organ to cease to act, and that the circulation stopped while the heart was acting. The heart, therefore, cannot be the chief motive power of the blood; because the circulation stopped and life became extinguished (at least in its visible phenomena, in all other parts of the body,) long before the supposed motive-power of the blood ceased to contract and expand. The contraction and expansion of the heart, therefore, cannot be the chief motive power which circulates the blood, because the blood ceased to move as soon as atmospheric air was excluded from the lungs and while its supposed chief motive power was

hard at work. Again, the doubters were compelled to admit, that after all the phenomena of life ceased to manifest themselves, that the forcing of air into the lungs would restore the circulation and resusciate the animal after all other means had failed.

The burden of Dr. Ely's objections to the new doctrine was chiefly founded upon the doubts of Dr. Dowler and others in regard to the experiments of 1852—doubts which have been entirely removed by the experiments of 1853. They thought it possible that the ligation of the trachea might produce a temporary state of asphyxia, and that the resusciation attributed to the insufflation might have occurred without it It was also thought that the dissection, preparatory to the insufflation. by exposing the serous membranes to the action of the air might have aided in recovering the animal more than the artificial respiration. Also in the cases where the lungs had been reported as accidentally cut, it was supposed that they might have been lacerated by the artificial respiration itself. To settle these doubts a new series of experiments were instituted. They began June 18th, 1852 with the Battleground Crocodile, so called. In that experiment, after ligating the trachea, time was given to see if the animal would not revive of its own accord. When satisfied that it would not, the serous membranes were extensively laid bare, to see if the absorption of oxigen and the elimination of carbonic acid would not restore animation. Failing in that, the nerves were laid bare, pinched, pierced and subjected to the action of fire ; but it was found that the nerves had lost their sensibility and the muscles their irritability. Artificial respiration was then attempted, but the crocodile had not only lost the most of the blood in its body, but the lungs were found to be too much lacerated to hold air, and the attempt was abandoned.

It is very true that I was very much mortified after all my cautions against cutting the lungs and the larger blood vessels, to those who had been trying to revive the animal by exposing the viscera and serous membranes to the action of the air, to find that the lungs had been lacerated and the large blood vessels cut. But in all other respects, except that of resuscitating the reptile, the experiment was eminently successful and went directly to establish the new doctrine ; viz : that the respired air is the motive power of the blood, because the blood stopped moving when the air was excluded ; before the heart had stopped acting, and that not only life, but muscular irritability was extinguished in less than an hour.

Dr. Dowler may have thought at that time, the attempts at artificial respiration, through a tube, one end of which was inserted in

the trachea and the other in the mouth of the operator, had ruptured the lungs, but in subsequent experiments he saw a much greater force used by a bellows in inflating those organs without any rupture whatever. In the experiment of the 6th of May, 1852, so much dwelt on by Dr. Ely, where the animal revived while the ligature was around the trachea, all present on the occasion agree that the lungs were ruptured or pierced. But there was a difference of opinion whether the laceration of the lungs was caused by the dissecting knife or by the force used in blowing air into them. Subsequent experiments have proved that the lungs are very liable to be cut in opening the thorax, and that it is impossible to lacerate them by blowing into them with the mouth. Professor Forshey, in his statement in regard to the first experiment made 1851, where the animal came to life, expressly states that "he blew with all his might," yet he did not rupture the lungs; nor were they ruptured in subsequent experiments in 1853, when a common sized fire bellows was the instrument used to inflate them. The experiments of 1853 completely removed all the doubts upon which Dr. Ely has founded his principal objections. He is referred to the parties whose doubts, in 1852, led him into error, to be led out of it again. They can tell him that the alligator's lungs are not so easily ruptured by blowing in them with the mouth, and that they have subsequently seen them preserve their integrity under the strong action of a fire bellows. They will tell him that tying the trachea will kill an alligator in less than an hour, if the air be pressed out of the air sacs when the trachea is tied. They will tell him that artificial respiration will bring the animal to life, if the lungs be entire and no large blood vessels cut, provided the insufflation be commenced before the blood has undergone chemical decomposition. They will tell him that no other means than insufflation has been attended with success in withdrawing la grande dragone from the cold abstraction of death produced by ligating the trachea.

Instead, therefore, of entering into a controversy with him I refer him to those whose doubts deceived him, for a controversy, if he wants one. In regard to the authority of Cuvier, which he quoted against the new doctrine, in his article on "The motive power of the blood," published in the November No. of this Journal, 1852. I appeal from the English abridgement of Cuvier's writings, which he quoted against me, to Cuvier's great original work on fishes, written in French, and contained in 24 large volumes, to be found in the library of the University of Louisiana, entitled "Histoire Naturelle des Poissons." Instead of

there being a muscular organ or anything like a muscular apparatus on the left side in fishes, to propel the blood through the aortic system, as Dr. Ely inferred from the wording of the loose translation of Cuvier, which he quoted with so much triumph against the doctrine of hæmatokinety, he will find from the original that Cuvier expressly and in the most emphatic terms repudiates such an idea. At page 510, Vol. 1, of the work above referred to, in treating of the circulation in fishes, Cuvier says: "Leur charactere propre consiste en ce que leur circulation branchiale a seule à sa base un appareil musculaire, au ventricule droit des animaux dont nous venons de parler, et qu' il n y a rien de semblable à base de système de la circulation de corps; c' est-à-dire que les analogues de l' oreillette et du ventricule droit leur manquent entièrement, et que les veines branchiales s' y changent en artères sans être enveloppées des muscles." The above extract contains all the facts in regard to the circulation of the blood in fishes which I contend for,—viz: that they have a single auricle and ventricle on the right side to assist in propelling the blood into the gills, but no heart or any muscular apparatus on the left side to aid in propelling it through the aortic system. Dr. Ely asks with great apparent triumph, "what becomes of Dr. C.'s assertion that it is known to naturalists that of the 12,000 species of fishes, not one of them has any muscular organ answering to the left ventricle of mammals." It is proved by the 24 volumes of the Natural History of Fishes, written by Cuvier himself, and not only by him but by every other distinguished writer on the subject. Yet strange to say, Dr. Ely's paper on the circulation is almost half-filled with arguments to disprove an anatomical fact, founded entirely upon a misconception in the translation of a single passage in Cuvier's great work—although the anatomical fact is known to every naturalist, that fishes have no ventricle or any other muscular organ at the base of the systemic circulation, yet Dr. Ely would not admit it. The admission of that truth would make the platform of Harvey, based on muscular propulsion, too narrow to hold the 12,000 species of fishes, which have no heart or other muscular organ at the base of the systemic circulation to propel the blood. This truth, which no one but him has ever to my knowledge attempted to deny, proves the existence of some other hæmatokinetic or blood-moving power, besides that derived from the propulsion of a muscular organ.

The anatomical conformation of insects also proves the existence of some other propelling power of the blood besides that derived from muscular contraction. The platform of Harvey excludes a class of the animal creation, out-numbering in species and individuals all other ani-

mals put together. It is true that the dorsal vessel of insects has been called a heart; it is true, as Dr. Ely says, that little holes, opening into the dorsal vessel for the admission of the juices of the body, have been discovered, secured by valves to prevent the fluids from returning. He might have added, that in addition to these small tubes opening into the dorsal vessel, others have been more recently discovered leading out of it at its anterior extremity, conveying streams from the head along the sides of the body backwards to the posterior end of the dorsal vessel or so called heart—that some of these streams are confined in vessels in some insects, but generally they are not confined in any vessels. The circulation of some insects, particularly the Ephemera Marginata is a mixed kind of circulation, partly diffused and partly vascular. It is admitted that the contraction, or rather the vermicular motion of the dorsal vessel propels the blood through its upper extremity and diffuses it into the spaces which intervene between the layers of the integument, thus bringing it in proximity with the tracheæ or minute air tubes penetrating every part of the body, but at the same time it is contended that the propelling power of the fluid, thus diffused, which makes it available for the purposes of nutrition, is derived from the air brought in contact with it by the innumerable air tubes called the tracheæ, Without these air tubes the hæmatokinetic power, derived from the air contained in them, the blood, loosely floating through the insect's body, could not be made available for the purposes of secretion, assimilation and nutrition.

Dr. Ely takes great exception to my style and manner of treating the subject in my first article on Hæmatokinety, published in the September No. of this Journal, for the year 1852, as being too metaphorical and smacking too much of the politician. I thank him and stand corrected. But as one good turn deserves another, I would call his attention to his articles on the Motive Power of the Blood, as containing a great deal of matter which politicians would characterize as intended expressly for Buncombe. Certain it is, that the Buncombe naturalists have been exulting and crowing for a year over what appears, in thir eyes, to be the ruins of Hæmatokinety made by his club. They will not be a little surprised at finding that the Natural History of Fishes, as taught out of an erroneous translation of Cuvier, as the text book, has deceived no one in any other quarter. Everywhere else, no fact in natural history is better established than that fishes have no aortic heart or any other muscular apparatus on the left side to propel the arterial blood. Everywhere else it is also well understood, that although the dorsal vessel of insects may be called a heart, that it is

no more like the heart of mammals than like the stomach, and all arguments based upon the supposition of its similarity to the heart of the higher classes of animals have no weight or force whatever. The dorsal vessel is a heart only in a metaphorical sense. The expression in the English translation of Cuvier, quoted by Dr. Ely with so much triumph —"*Which trunk is the left or systemic ventricle of the heart and sends the blood throughout the body of the fish,*" was evidently intended by the translator to be understood in a metaphorical sense, just as I intended the expression of "*Riddell in the woods*" to be understood. By giving the expression a literal meaning, Dr. Ely appears to have been led to believe that the "*trunk*" alluded to was a superadded muscular organ, placed on the left side to propel the blood. Whereas, the "*trunk*" that Cuvier was speaking of, was nothing more than the aorta itself, and he expressly says that there is no muscular organ on the left side to propel the blood. By the expression of "Riddell in the woods," I meant to convey the idea, that the important discoveries, which that able professor has made with his improved microscope, cannot be explained until Hæmatokinety be received as the true doctrine.

Dr. Ely complains that no attempt has been made to show how the discoveries of the professor prove the existence of a hæmatokinetic power herctofore unknown. I did not want to show how, until Prof. Riddell had got through with his experiments and had convinced himself that the phenomena he witnessed, and the discoveries he made in the microscopic world, cannot be explained by the present received physiological doctrines.

The able professor begins to see that he has one of two things to do—either to see his discoveries trodden under foot as things of no value, because they are inexplicable, or to bring them forward in support of the new doctrine they so beautifully illustrate, and by which they can be so clearly explained. The time is approaching when America will look back with wonder that there ever was an age in which her people were humbugged by a physiology, imported from Europe, shrouded in so much darkness as to place the *chief motive power* of that nourishing fluid, called the blood, in the mechanical propulsion of a muscular organ which not half the animal creation have got—no young embryos and very few animalcules.

Canal street, July 21st., 1853.

VII.—CHLOROFORM IN DELIRIUM TREMENS.

BY E. T. OWEN, M.D., MISSISSIPPI.

Dr. A. Hester:

Dear Sir—The following case, in which chloroform was used with signal success after the failure of the ordinary mode of treatment, I submit, with the hope that it may prove of value to your readers.

Captain K———, aged 52 or 53 years, of sanguine temperament, slender, active, general health impaired by habitual intemperance, had been drinking freely, from the 11th to the 14th July, when I was called to see him. During this time, his wife informed me he had eaten nothing and had not slept. I found him with the characteristic symptoms of mania-a-potu—red face, fiercely glaring eyes, a frequent but compressible pulse, soft creamy tongue, restless and figety with his hands, and talking incessantly. Scores of spotted devils, he said, were streaming into his room, dancing fantastically around, and leaping upon him. As his bowels were regular and he had eaten nothing for three days, I considered it unnecessary to premise an emetic, which I think proper in most cases; but began his treatment by giving opium and camphor, aa gr. ij.; followed in thirty minutes by the same, with French brandy, ℥ i. This treatment I persisted in, until informed by the patient that his old family physician had never succeeded in making him sleep, either by opium or any other means. Several of his friends around confirming his statement, I desisted from this plan of treatment after having given six grains of opium and as many of camphor without any obvious effect.

Here it occurred to me, that if the pathology of mania-a-potu be "*nervous irritation*," and especially of the great source of the nervous system—the brain—then that remedy would be most appropriate, whose effects would be to tranquilize that irritation, most directly and speedily. Accordingly, having directed the patient to lie upon the bed, I proceeded to administer chloroform by inhalation. It acted like magic; and in a few minutes, I had the pleasure of seeing my patient in a profound and sweet sleep, which lasted from 12 P.M. till 6 A.M. the next day. He awoke much refreshed.

But, being a self-willed man, he repaired to the grocery and continued to drink on the 15th and till the night of the 16th, when I found him indescribably miserable; alternately calling for drink and clamoring for sleep. The devils, who before merely danced in hideous glee,

now were lashing each other into fearful fury, and threatening to tear him to pieces. Another exhibition of chloroform speedily buried these frightful apparitions in the oblivion of profound slumber, from which he awoke the following day perfectly rational. Since then he has been improving daily, both in strength and appetite, and will doubtless entirely regain his wonted health and vigor. I am aware it is bad philosophy to deduce a general rule from a single experiment, yet I earnestly recommend this remedy to at least a trial by your readers.

Springhill, Miss., June 19, 1853.

VIII.—ON THE OPERATION OF TRANSFUSION—BEING THE REPORT OF A COMMITTEE.

Read before the Louisiana State Medical Society, at its Fourth Annual Meeting, March 16th, 1853.

BY N. B. BENEDICT, M. D., NEW-ORLEANS.

The "Committee on Midwifery and the Diseases of Women and Children," beg leave to submit the following report:

That in their endeavor to discharge the duty confided to them, they have not been forgetful of either the delicacy or the seriousness of their trust.

The admirable Report of the lamented Dr. Wm. P. Hort, which was read at our last annual meeting, and published in the ninth volume of the New Orleans Medical and Surgical Journal, has left for us little more than, in general terms, to reiterate and approve his conclusions. A few passing remarks may, however, be added to some of the topics which he has so ably discussed.

(1.) In relation to *partûs acceleratores* generally, we would suggest the applicability of the quotation which he has made from Dr. Baird,

concerning instrumental officiousness, to the effect that 'the person who, in proportion to the extent of his practice, meets with most frequent occasion for the use of *ergot*, knows least of the powers of nature, and is a very dangerous man.'

(2.) To his remarks on the anæsthetic agents, we would append the conclusions reported by the Committee of the American Medical Association," at the annual meeting of May, 1848: (a.) 'That the pain of parturition may be partially or wholly relieved. (b.) That even in small quantities, anæsthetics benumb the acuteness of sensibility, and thus allay the fear which is so injurious to the parturient female. (c.) That this tranquilizing impression may be kept up for hours, with entire safety and the greatest benefit, in tedious labors, where the condition of the parts admits no active interference; thus enabling the patient to live through sufferings which would otherwise destroy her. (d.) That their use is followed by relaxation of the soft parts and of the os tincæ, and by increased secretion of mucus.' "That it is the opinion of many accoucheurs and of some of the committee, that they increase the expulsive efforts of the uterus, and decidedly aid in the expulsion of the placenta."

(3.) There is no doubt of the efficiency of bleeding from the temporal artery in puerperal convulsions; but it has been very judiciously objected that the peculiar advantage of employing this artery is likely to be diminished if not countervailed by the pressure and warmth of bandages about the head to arrest the flow.

(4.) In connexion with the accident of inversion of the uterus by undue traction of the cord, we desire particularly to call the attention to a sure means of ascertaining when the placenta is detached, which we believe was first suggested by Professor C. D. Meigs, and which, in some cases, is of inestimable value. On tightening the cord with one hand, if we can trace and distinguish its insertion in the placenta with a finger of the other, appearing to expand or diverge like roots of a plant, we are assured of its detachment with infallible certainty; for that root-like divergence can never be reached by the finger while the placenta remains *in situ*.

(5.) In cases where the tampon is proper, we would suggest that it is often tedious, difficult, and painful, either to introduce or to remove; as much so, perhaps, as any other operation of minor surgery. All this trouble may be simplified, by first introducing a cylindrical speculum, and then pushing through it the requisite quantity of tow or "raw" cotton. When this tampon is to be removed, we have only to introduce

along the finger, a probe of coarse common wire, having its end in any manner slightly roughened, which, by pressure and twisting, is readily entangled in the fibres, affording a convenient handle by which to withdraw the tampon.

(6.) With these brief remarks we pass to another topic, to which Dr. Hort merely alluded, but which we purpose to examine more in detail. From the great liability of the pregnant and parturient female to those dangers which are the consequence of excessive loss of blood, your committee believe that they cannot more fitly execute the task assigned to them, or render a more valuable service, then by inviting attention to a surgical measure which has been proposed and, to a limited extent, practiced in certain extreme cases. They refer to the operation of

TRANSFUSION: "the act of transferring the blood of one living animal into the vascular system of another."

History.—It is exceedingly probable that the vague general idea of transfusion may have existed from the remotest times. Nothing, indeed, would seem to be more natural, in any age, than for him who witnessed a fatal loss of blood from one who was dear to him, to inquire if some means could not be devised to restore or replace it. "Those mighty masters of antiquity who first discovered the principles of things," have left to us little else but the task of 'exploring those tracts of knowledge which they originally pointed out, and this, perhaps, among the rest; for passages occur in the writings of the ancients, which are scarcely obscure or equivocal. In the Metamorphoses of Ovid, book vii, v. 332 to 334, occurs the following:

"Quid nunc dubitatis inertes?
Stringite, ait, gladios; veteremque haurite cruorem,
Ut repleam vacuas juvenili sanguine venas."

Why now idlers, do ye hesitate? Bind the gladiators, and exhaust the old man of his blood, that I may fill the empty veins with the blood of the youth.

The earliest account of this operation which is known to your committee was by Libavius, in 1615, whose work, entitled "Defensione Syntagmatis Arcanorum Chymicorum," contains the following passage: "Adsit juvenis robustus, sanus, sanguine spirituoso plenus: adstet exhaustus viribus, tenuis, macilentus, vix animam trahens. Magister artis habeat tubulos argenteos inter se congruentes, aperiat arteriam robusti, et tubulum inserat muniatque: mox et ægroti arteriam findat, et tubulum fœmineum infigat. Jam duos tubulos sibi mutuo applicet, et ex sano sanguis arterialis, calens et spirituosus saliet in ægrotum,

unaque vitæ fontem afferet, omnemque languorem pellet."—*Libav. art. ii, p. 8—cited by Dr. Kay, in "Cyclopedia of Practical Medicine."* There is present a robust, healthy youth, full of lively blood. There is also present one exhausted in strength, weak, enervated, scarcely breathing. The operator has silver tubes passing between them; he opens an artery of the healthy one, inserts the tube, and secures it. Next, he finds the artery of the patient, and adjust the receiving tube. Now he adapts the two tubes to each other, and the arterial blood of the healthy one, warm and lively, dances in the [vessels of the] sick one, and immediately it produces the appearance of life, and removes all languor.

The first direct operations upon the blood, within the veins, have commonly been attributed to Dr. Christopher Wren, the Savillian Professor at Oxford; who, in 1656, injected various medicines into the blood-vessels of brutes and of criminals. The writings of Dr. Major, the Professor of Medicine at Kiel, in Germany, in relation to Dr. Wren's experiments, seem to have been the immediate cause of the attention of the profession being directed to the subject. But, whoever will take the trouble to refer to Lemprier, art. "Æson" and "Medea," and to the Seventh Book of Ovid, ver. 285 to 293,* for an account of the measures employed by Medea for the rejuvenescence of the decrepit Æson, will be reminded of the saying that "there is nothing new under the sun."

Harvey's first publication, on the circulation of the blood, was in

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* "Quod simul ac vidit, stricto Medea recludit
Ense senis jugulum; veteremque exire cruorem
Passa, replet succis. Quos postquam combibit Æson,
Aut ore acceptos, aut vulnere; barba comæque,
Canitie positâ, nigrum rapuêre colorem.
Pulsa fugit macies; abeunt pallorque, situsque;
Adjectoque cavæ supplentur sanguine venæ;
Membraque luxuriant. Æson miratur, et olim
Ante quater denos hunc se reminiscitur annos."

Translation.—Which, [the decrepitude of Æson] as soon as she saw, Medea opened the throat of the old man with a drawn sword, and suffering the old blood to ooze out, filled it with juice [of plants.] After Æson had imbibed it, either by mouth or the wound, his beard and the hair of his head (the grayness being displaced) received a black color. The lameness, being driven away, fled; the pallor and mouldiness departed; and the great liver-veins swelled with the supplied blood, and the limbs grew fat. Æson was astonished, and called to mind that he was such formerly, when he had been born forty years. (Was but forty years old.)

1628; but it is well known that about thirty years elapsed before the truth of his theory was generally received. Simultaneously with its establishment, the minds of men "were seized with a sort of delirium," on the subject of transfusion. "It was thought that the means of curing all diseases was found," of rejuvenating the aged, and even of "rendering man immortal."—*Dr. Kay, loc. cit.* The true cause of all disease, of decay and of death, was attributed to deprivation of the blood; and consequently, to prevent or remedy them, all that was necessary was to abstract the impure blood, and replace it with pure blood drawn from a healthy animal. It was gravely proposed to effect an entire exchange of the blood of one individual for that of another; of young for old, healthy for diseased, the cold or hot, or tame, or wild, for their several opposites—animals, reptiles, and birds interchangeably—for the specific purpose of altering the nature, the habits, the temperament, and even modifying the identity of the subjects on whom it should be practised. The following curious passage occurs in the Diary of Samuel Pepy's, under date November 14th, 1666. "Dr. Croone told me that at the meeting of Gresham College, to-night, (which, it seems, they now have every Wednesday, again,) there was a pretty experiment of the blood of one dog let out, till he died, into the body of another on one side, while all his own ran out on the other side. The first died upon the place, and the other did very well, and likely to do well. This did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop, and such like; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health for the amending of bad blood, by borrowing from a better body.—*Diary and Corresp., vol. iii, cited by Dunglisson, Physiol. ii, p.* 190.

The first trials of transfusion of blood were made upon animals, and as was alleged, "with complete success." A dog having lost the greater part of his blood, received that of a sheep, "and recovered." Another dog, old and deaf, "regained his hearing, and seemed to re-recover his youth." The blood of three calves was transfused into three dogs, by Denys, who state that "all continued brisk, and ate as well as before;" and the blood of four wethers being introduced into the veins of a horse, twenty-six years old, caused "much strength and a more than ordinary appetite." In the experiment which Dr. Richard Lower frequently exhibited in the Universities, of connecting the carotid artery of a sheep with the jugular vein of a dog, and continuing the process until the sheep died; it is stated that the dog (whose own blood being allowed to escape, was entirely replaced by the arterial blood

of the sheep,) "generally leaped from the table immediately after the operation, shook himself, and ran away without ailment."—*Cyclopedia Practical Medicine*, vol. iv, p. 468.

The first who ventured to practice transfusion upon man were Denys and Emmerets—the first a physician, the other a surgeon of Paris. The experiment was made June 15th, 1667, "On a young man, 15 or 16 years of age, who had been much weakened by repeated bleedings. He had become very languid, torpid, and slightly dropsical. Denys resports that the first operation restored him to perfect health."—*Loc cit*, p. 469.

"Baron Bond, son to the First Minister of State of the King of Sweeden, being affected with a malady which terminated in mortification of the intestines, underwent the operation twice; was reported to have been strengthened by it the first time, but died soon after the second operation."—*Gaspar de Gurie de Montpoly, cited by Dr. Kay.*

The recovery of a case of leprosy, and of a case of quartan ague succeeded transfusion; they were *post hoc* if not *propter hoc*. It was practiced, also, upon "several healthy persons, without any disagreeable result." The blood of a calf which, "from its mildness and freshness, it was expected would allay the heat and ebulition of the patient's blood," was introduced into the veins of a young man—an idiot—in greater quantity than had been drawn from him, "and he appeared to recover his reason." He, however, became a maniac shortly after the experiment; and, on a repetition of it, his pulse speedily rose, becoming soon afterwards very irregular; a copious perspiration covered his face; he complained of pains in his loins, faintness, and sense of suffocation; vomited freely and frequently through the early part of the night, but slept soundly afterwards; continued very drowsy for three days, voiding very dark urine, and bled so freely from the nose, as to require venesection. A lucid interval succeeded, which lasted about two months, when he was attacked with fever, occasioned by his own excesses. Transfusion for the third time was no sooner commenced than the patient was seized with shivering, complained of great oppression, and cried out to the operator to desist—that he was suffocating. The process was suspended, and the patient died the next night. A young prince of the blood royal was also the victim of it, and the Parliament of Paris prohibited transfusion.

'In November, 1667, Dr. Richard Lower and Sir Edmund King injected ten ounces of the arterial blood of a sheep into the veins of a man named Arthur Coyn, who professed to have received great benefit from the operation; and, it is said, no ill consequences ensued.'—*Loc cit.*

During the two following years, Dr. Lower practiced transfusion upon five person, of whom "two recovered of disorders under which they labored; one, being in perfect health, suffered no inconvenience from it; and two, who were ill before, died." In this account, which is taken from the London Encyclopedia, it is added that "whether such experiments can ever be made with safety on the human body, is a point not easily determined. They might be allowed in desperate cases, proceeding from a corruption of the blood, from poison, *et cetera*, as in hydrophobia."

G. Riva (who, with Manfredi, introduced transfusion into Italy) having performed it upon two persons who died of it, the practice was prohibited by the Papal authority.

For more than one hundred years, the operation was in disrepute, and universally neglected, until Dr. Harwood, Professor of Anatomy at Cambridge, in his thesis, in 1785, attempted to revive the inquiry concerning its utility. He exhibited many experiments on animals, in his lectures on comparative anatomy. One which he several times repeated was, to take from the jugular vein of a dog eight ounces of blood, and replace it with the same quantity from the carotid artery of a sheep. "During the operation, the dog showed evident marks of uneasiness, but was little affected in any other way, till about twenty-four hours after, when he had a shivering fit, succeeded by a considerable degree of heat, thirst, and the usual symptoms of fever, all of which disappeared in the course of the next day, and the dog remained afterwards in perfect health. The quantity of blood being occasionally increased or diminished, the feverish symptoms were observed to be more or less violent, in proportion to the quantity of the arterial blood introduced." Suspecting that these effects might be due to arterial blood, he took from a dog two pounds, and replaced it with *three* pounds of venous blood from another dog. "The recipient animal was severely affected with vomiting and purging; afterwards was very drowsy and stupid; then suffered much from fever, which terminated in a copious evacuation of blood by stool, by urine, and by vomiting. He took no nourishment for three days after the operation, except water," 'and was reduced with a rapidity that was most extraordinary.' "He gradually regained his health, and lived some years." Another dog having been bled until convulsed and apparently expiring, on being replenished with blood from the jugular of a sheep, began to respire in half a minute; and when the quantity restored was equal to what had been lost, "he leaped from the table and walked home, without experiencing any ap-

parent inconvenience, either then, or at any subsequent period."—*Hutton's abridgement of Philos. Transact.*

This experiment was frequently repeated, on a variety of animals, and in presence of crowded meetings at the University; all the facts are so well authenticated and were so carefully conducted as to entitle them to consideration; and yet, Dr. Harwood's conclusion, "that the blood of an herbivorous animal may be substituted for that of a carnivorous animal, and *vice versâ*, without danger or even inconvenience to the animal which receives it," is, by the progress of physiology, proved to be erroneous—not to say absurd. 'The size of the red corpuscles,' even in the same species, is not perfectly uniform; thus in different men it is known to vary from the one forty-hundredth to the one twenty-eight-hundredth of an inch; but there is a general average size, in any given species, which is constant: that of a man being about the one thirty-four hundredth of an inch, and that of the mammalia not generally departing very widely from this standard. In the camel tribe they are oval, their long diameter equalling that of man's, while their short diameter is but half; and in the musk-deer they are but the twelve-thousandth of an inch. In birds, the corpuscles are larger, and usually oval, with the two diameters to each other as one-and-a-half to one, or two to one; in the ostrich, the long diameter is about the one sixteen-and-a-half-hundredth, and the shorter, the one thirty-hundredth of an inch; in sparrows and finches, one twenty-fourth-hundredth by one twelve-hundredth; in the frog, one eighteenth-hundreth by one ten-hundredth; in the siren, one eight-hundredth by one four-hundred-and-thirty-fifth; and in the proteus, the long diameter is so great as one three-hundred-and-thirty-seventh of an inch—almost distinguishable by the naked eye.'—*Carpenter's Elements Physiology*, p. 135.

Considering all these varieties in the size of corpuscles, and adding thereto the great differences that must occur in the constituent elements of the blood, from original conformation and from varieties of food taken by man, by carnivora and herbivora, by reptiles and birds, it is not difficult to understand or credit the unanimous conclusions of Prevost, Dumas, Dieffenbach and Bischoff, that "the vivifying power of the blood does not reside so much in the serum as in the red particles. An animal bled to syncope was not revived by the injection of water or of pure serum at a proper temperature; but if the blood of one of the same species was used, the animal seemed to acquire fresh life, at every stroke of the piston, and was at length restored." These physiologists all agree as to the deadly influence of the blood of animals when injected into the veinsof ducks and other birds. "This influence,

according to Müller, is also in some way connected with the fibrine of the blood; and experiments have certainly shown that blood deprived of fibrine acts most injuriously when injected into the vessels."—*Dunglisson's Physiology*, vol. ii, p. 191.

If blood be obtained from any species whose globules, though similar in size are different in form, the patient "is only imperfectly restored, and can seldom be kept alive more than six days." "The pulse becomes quicker, while the respiration may remain undisturbed; the heat of the body falls with remarkable rapidity, if it be not artificially sustained; and the dejections become mucous and bloody, and continue so until death.—*Annales de Bhimie et de Physique*, vol. xvii, p. 281.

In considering the foregoing account, we cannot fail to be struck with the characteristic error of all the experimentors except those last named—to wit, the error of presuming that the blood of one genus of animals might be substituted, indiscriminately, for any and every other genus. The very converse of this proposition should seem to have been the most natural, as it was the most rational and probable. True, the ancients were ignorant that the blood is compounded of fibrine, albumen and salts, forming a nearly colorless fluid—the liquor sanguinis—in which are freely suspended corpuscles of forms and dimensions perfectly defined, some of them being achromatic, while others give to the blood its characteristic color. But they did know, in the *first place*, that the blood of man, of most quadrupeds, and of birds is hotter than the average temperature of the medium in which they live; while that of fishes, reptiles, and hibernating animals exhibits the most remarkabl contrast in that respect, in so much that they were commonly called "cold-blooded," from the earliest times. In the *second* place they could not have been ignorant of the different appearances presented to the eye by the blood of different animals, and by venous as contrasted with arterial blood; both of which kinds seem to have been employed pretty much at random. *Thirdly*, had they themselves failed to perceive differences in color, consistency and odor, they were not ignorant that very marked differences must exist, as proved by the conduct of animals—the bovine, for example—who never fail to recognize the blood of their own species, and who exhibit an intelligence and frantic distress that are perfectly shocking. *Fourthly*, they were aware of the very marked difference in the degree of coagulability of blood obtained from warm or from cold-blooded animals. *Fifthly*, they believed Life to be an Entity or Being—so to speak—dwelling within the animal body, and constituting its essential individuality—its *ille ipse*. This Being, Entity, or Life was considered not merely to reside in the blood,

but was identical with it—was blood itself; and hence, in all ages and nations, blood has been more or less regarded with a kind of superstitious reverence. It is, therefore, truly surprising that they should have employed transfusion upon man with blood from animals; for, although they might disregard the Mosaic distinction of "clean" and "unclean," it yet seems incredible that persons acknowledging the authority of the Bible should have attempted to replace "the spirit of man that goeth upwards, [with] the spirit of the beast that goeth downward to the earth—*Ecclesiastics*, c. iii, v. 21—or should have forgotten the reason so strikingly reiterated for the Levitical and Apostolic interdict concerning blood: "*For* the life of the flesh is in the blood; *for* it is the life of all flesh; the blood of it is for the life thereof. * * * * Ye shall eat the blood of no manner of flesh; *for* the life of all flesh is the blood thereof."—*Levit.*, c. xvii, v. 2 and 14; also *Acts*, c. xv, v. 29.

The death of the idiot in France was unquestionably owing to the introduction of air into the veins; for it is expressly stated that the operation had scarcely commenced, when "the patient was seized with shivering, complained of great oppression, and cried out that he was suffocating." But all these transfusions were founded upon erroneous opinions; they had been preceded and established by no course of rigorous and well-devised experiments; they had been applied to no rational purpose; though frequently harmless, they had in no perfectly authenticated case, been followed by unequivocal benefit; they had often proved pernicious, and sometimes even fatal; and their prohibition and entire disuse was, in view of all the circumstances, wise and proper. The utmost liberality can admit but this conclusion: "that small quantities of the blood of animals of some species may be introduced into circulation of animals of certain other species, without occasioning death; though the operation is generally followed by unpleasant consequences.—*Cyclop. Pract. Med.* v. iv, p. 472.

To Dr. James Blundell, Lecturer on Midwifery and Physiology, at Guy's Hospital, is due the honor, not so much of reviving transfusion as of discovering or contriving a great therapeutic measure of inestimable value, whose *basis* was a course of systematic experiments, so thorough as to leave nothing to conjecture, nor a single question unanswered—whose *principles* are simple and rational, doing violence to no physiological law, nor to the demands of common sense; and whose *success* has exceeded the most sanguine hopes of its advocates. The grand distinction between transfusion as practiced by Dr. Blundell and that practiced by preceding experimentors, consist in this—namely—to those who were dying from recent hemorrhage, he restored blood as

nearly as possible identical in all respects with that which they had lost; selecting invariably for his source of supply only individuals of the same species; and using neither more nor less than should just suffice for the end sought. The early experiments (in which, from some fanciful notions concerning the purity and innocence of lambs and calves, their blood was preferred both for dogs and for men.) bore little more resemblance to the transfusion which is now justifiable, either in their objects, their methods, or their results, than did the vagaries of a Paracelsus to the rigorous analysis of Liebig.

Dr. Blundell, in his lectures on Midwifery, says: "The operation of transfusion I consider to be of so much importance to mankind, that having made it the subject of much thought and experiment, I seize the opportunity which now offers, of treating the topic more at large. * * * * If I have, myself, any claim, however small, to rank among the supporters of transfusion, it lies entirely in this, that, undeterred by clamor or skepticism, I have endeavored to bring the operation into notice; and further, to show, by experiments on animals, and observations on the human body, that it may be performed by the help of a syringe, by means of which human blood, of the kinds above fit for the operation, may be safely infused into human veins. In the original operation, brute blood was employed, which, if taken indifferently from animals, and in large quantities, is always fatal; and the presence, moreover, of some animal in the bed-chamber was indispensable. What then was to be done in order that transfusion might, in an emergency, in some improved form, be rendered safe and available by the bed-side of the sick? A dog, it is true, might come when we whistle, but the animal is too small; a calf or a sheep might have appeared fitter for the purpose, but it has not been taught to walk promptly up the stairs; and though any or all these might be commanded in the lecture room, with its conveniences and assistants, yet these would be impossible in the lying-in-chamber; although transfusion might be performed by means of the force which would be exerted by the artery of a bystander through a simple tube, yet the safety of such a method must be very doubtful; and a more ready mode of rendering the operation practically valuable appeared to be by employing the syringe. By a variety of experiments I satisfied myself that blood may be transmitted through the syringe as through the heart; that if deterioration occur, it is not such as to render it unfit for the animal body. Repeatedly I have drained the blood of dogs until they lay in a state of apparent death, the blood ceasing to issue even from a tubule inserted into the carotid towards the heart, the circulation therefore being entirely

arrested, and all respiratory effort having ceased: in one very remarkable case, as stated by Dr. Kay, "during five minutes."—*Cyclop. Prat. Med.*, vol. iv, p. 472. "Dogs thus prepared I have replenished, by means of the syringe, with blood from other dogs, and found—where the operation has been well performed—that the animals, to all appearance irrecoverable, have shortly arisen from the table as if they had experienced a resuscitation from the dead." "It is true, indeed, that for two or three days, a little cachexia or ill-health has hung about it; but in the course of a few days more the animal has seemed to recover itself completely, becoming as well as before the operation was performed. * * * * It has not been in a few, but in many experiments, that I have found this result; for how could I multiply experiments too much on a subject so important? Who that venerates reason, and has the love of mankind in the heart as well as on the tongue, will dare to tax such physiology as brutal?" To assure himself that neither the passage of the blood through the syringe, nor its temporary exposure to contact with the air, (which occurred in all these experiments,) would unfit it to sustain life, he contrived the following, which may fairly be regarded as the *experimentum crucis*. He opened the femoral or the carotid artery of a dog, and placed near it a cup, into which the blood rushed in a constant full stream. In this cup he placed the imbibing orifice of a syringe constructed like the stomach pump, absorbed the blood into the barrel of the instrument, and, through the ejecting orifice, returned the blood into the corresponding vein of the same animal; so adjusting the return of the blood to its eruption from the artery, that not more than one ounce was allowed to accumulate in the cup at one time. In some of these trials, the process was continued, without intermission, during twenty and thirty minutes; so that every drop of blood in the animal must have passed several times into the air and through the apparatus. The experiment was repeated and varied many times, yet always with the same result; "the dog not appearing to suffer materially in consequence." In this connexion it is mentioned by Dr. Kay (*Loc cit.*) that Dr. Blundell deprived a dog of all food whatsoever, and supported him during "three weeks by the mere transfusion of blood into the external jugular vein; having, in this period, injected nearly eighty-four ounces of blood from other dogs."

Quantity of Blood necessary.—When it is decided to practice transfusion, a very moderate quantity of blood is ordinarily required; as a general rule, not less than seven ounces, nor more than sixteen. A recent comparison of all the cases in which it has been employed with success, showed the mean quantity to be nearly fourteen ounces.

If our sole object is to save life, the smaller quantities—even so little as 4 or 5 ounces—may turn the scale in our favor; but if we seek to speedily restore strength and vigor, the larger injections must be practiced.

Apparatus.—The instrument which Dr. B. recommends, is either a syringe and basin contrived by himself and made by Lundy, or an instrument which he calls a gravitator, which, from the very meager description given, we conjecture to consist of a basin to catch the blood as it flows, having attached to its bottom a long flexible tube which terminates in a tubule to be inserted into the vein of the patient; the principle of propulsion being hydrostatic pressure, which may be increased or diminished by elevating or depressing the basin; resembling, in a word, a French apparatus, in common use, for administering an enema to one's self. Dr. Kay recommends a syringe to which "a tubule and basin are adapted, as likely to supersede that of Dr. Blundell;" but he gives no description of its peculiar features. Dr. C. H. F. Routh, physician to St. Pancras' Dispensary, advises the use of an instrument made by Ferguson, of Smithfield, as being 'the most perfect of any that has been contrived;" but the peculiarities of its construction are not described; he only suggest that the basin which accompanies it "should be cased like a hot plate that it might contain warm water, and that the pipe conveying the blood from the syringe to the patient should be flexible, so as to prevent its displacement by movements of the limb of the patient or of the instrument.

The first great danger to be guarded against in the construction of an apparatus for transfusion is, that of the introduction of air or any other foreign matter. To this end it should be of simple form, of accurate workmanship, and of materials not liable to corrosion. The second danger to be guarded against is deterioration of the blood during its transit, from exposure to the air, from rest, and from cooling. The instrument should therefore be susceptible of speedy adjustment; of continuous or intermittent action, at will; and of disconnexion, cleansing and re-adjustment without delay or embarrassment after any temporary suspension of the process.

Blundell's Method.—Your Committee are not aware of the operation having ever been performed in America; nor that any instrument for the purpose has ever been imported or constructed here; and they conceive that one great obstacle would be removed, if it could be made apparent that a safe and effective apparatus is readily attainable. The instrument successfully employed by Dr. Blundell, in his earlier experiments, was a common, plain, brass syringe, tinned internally, of two ounces capacity, carefully constructed to work smoothly and air-

tight, perfectly clean, and its pipe three or four inches in length, diminishing to a tubule, whose tip was cut off obliquely, so as to facilitate its entrance into the vein of the patient. The blood was caught in a wide conical tumbler. A glass or other funnel having its apex stopped with a cork, might serve the same purpose. Into the syringe (previously warmed to the proper temperature by being placed in blood-warm water*) the blood was absorbed from the apex of the receiving vessel, through the pipe of the syringe almost exactly as fast as it flowed; neither allowing more than a dessert-spoonful to accumulate, nor any air to enter. The syringe was then held vertically, the pipe being upwards, and the handle slowly pushed up until every particle of air and a little of the clear blood escaped. Then stoppping the orifice with the tip of the finger, to prevent air from re-entering the tubule was, without delay, gently insinuated into the vein of the patient (previously prepared) to the distance of half an inch, towards the heart.

To any one familiar with all the principles involved, and possessing the kind of tact which is seen in greatest perfection in the laboratory of the chemist, and the calm self-reliance of the experienced surgeon, no difficulty need be apprehended as to the success of transfusion, even with such imperfect means. But something more complete is required in order to inspire that confidence in the facility of the operation which shall commend it to the profession at large. Your Committee have, accordingly, contrived an instrument which is believed to possess the requisite qualities; but which, although not difficult to construct, could not be completed in time to be presented at this meeting. The following description, however, assisted by the engraving, will be readily understood.†

Proposed Apparatus.—Every part of the apparatus is constructed of silver alone, or of brass, which is silvered or gilded, with the single exception of a portion of the tube through which the blood passes from the syringe to the patient, which is made of a piece of gum-elastic catheter.

A most important feature consists in the employment of a seperate tubule, about one inch in length, which is not attached to the principal instrument, but is inserted, towards the heart, to the depth of half an inch, into the vein of the patient, and left there throughout the operation, carefully secured by a strip of adhesive plaster passing through a little wire-loop on the side of the tubule near its larger end. The

* 102° Fahr. See Carpenter's Physiology.

† The apparatus could not be finished in time for this number; it will appear in the next.—Ed.

tubule is made tapering, its size such as to be admissible within the calibre of the vein, its smaller end cut off obliquely to facilitate its intromission, its larger end widened a little freely, and fitted exactly to receive within it another similar tubule which is fastened to the syringe, and forms the beak of the flexible pipe. The particular advantage of thus employing a separate tubule, which is lodged in the vein of the patient, is somewhat analagous to that of a funnel; it enables us to save most precious time, which would otherwise be lost in awkward attempts to introduce the beak of a syringe.

A cone-inverted or funnel-shaped basin, 4 3-4 inches in diameter and two inches in depth, receives the blood as it flows from the arm of a bystander. In its apex is a valve opening downwards, and held in place by a spring having strength just sufficient to prevent the descent of the blood by its own gravity, but readily overcome by a small additional force. As it is of the highest importance to avoid the imperfection and delay of screw-joinings, they are carefully excluded from every part of the instrument where it was possible to dispense with them. For this reason, the apex of the conical basin terminates in a neck which is a little elongated, and very slightly tapering, to insure a safe and air-tight junction when pressed into an orifice of similar form. The basin is thus *attached to the syringe.*

The syringe is furnished with two orifices, plain and tubular; one upon the upper side of the barrel, and close to its distal end, nicely fitted to receive and securely retain the tapering neck of the basin and admit the blood; the other, exactly similar, at the end of the barrel, for the ejection of the blood, and guarded in a like manner by a spring-valve opening outwards. The barrel of the syringe may have an inner diameter of four, five, or six-eighths of an inch, and its length such that, with the piston drawn back, it has the exact capacity of one ounce, or of some other simple and definite quantity. The object of this exactness being to enable us to know, with certainty, the precise quantity of blood transfused, by merely observing the number of journeys made by the piston.

(*To be continued.*)

IX.—A BRIEF NOTICE OF DR. E. D. FENNER'S LAST ARTICLE ON THE ABORTIVE METHOD OF TREATING ALL FEVERS.

BY R. L. SCRUGGS, M.D., LA.

I might, I think, without the slightest risk of suffering in the opinion of the medical fraternity, permit Dr. Fenner's article, in the July No. of your Journal to pass without notice; but as he has done me the honor to notice me more particularly than any one of his numerous opponents, "and their name is legion," I deem it my duty in the first place to return the compliment, and in the second place my personal regards for him makes this duty imperative.

The Doctor starts off in his notice of me by saying that, "as he sees nothing *new* in my paper, etc., etc." Indeed, I think, he might well have saved himself the trouble of informing the medical world of a fact to which the paper itself made no pretentions whatever. Nothing *new*, I assure him was attempted, but it was simply intended to inform him "and the rest of mankind" that my observations upon the subject were confirmatory of the opinions already advanced by the best authorities, and consequently condemnatory of his. Had I given a few of the nnmerous cases of bilious remittent fever, treated by me during the past year, (all of which have yielded quickly to *my* doses of quinine,) and called them *typhoid*, I doubt not that the Doctor would have been highly pleased with the communication, and concluded that his friend Scruggs did know, "after all that had been written upon the subject," that quinine is a stimulant and tonic in small doses, and sedative in large.

Dr. Fenner says that it is a fact of which history informs us, that "almost every physician of Great Britain who had attained the age of forty years, when the immortal Jenner proclaimed his great discovery, refused to the last to admit its correctness." Now, while I admit that I am about the age, according to this important historical information, to become *stationary* "in my notions," I am at a loss to account for my friend Fenner's progressive qualities upon the same hypothesis, and can account for them in no other way than upon the principle of the old adage, of being "once a man and twice a child," for it occurs to me that I became acquainted with the Doctor nearly twenty years ago, when I was yet a minor and he an *experienced* physician. I hope that this disclosure will not seriously interfere with his matrimonial prospects!

I am old fashioned enough in my views to think that it is better to remain stationary than to progress in the wrong direction, for what is more irksome than to have to retrace our steps after finding that we

have lost our way? Or what more mortifying than to be compelled, at least, to acknowledge an error, persisted in for a long time in spite of all the *gratuitous* information so lavishly bestowed upon us?

I beg leave to inform the Doctor, that the profession throughout the South and West, do not seem to prize *novelties* as much as they do *truths* judging from the numerous letters received by me from private practitioners, as well as from Committees of several State Medical Societies; all asking information upon the subject of fever. In several of these letters I have received the thanks of my professional brethren for information already conveyed to them through my communications in the Journals, and whenever my friend Fenner shall have received such proofs done by *his* new-fangled doctrines, I shall "cheerfully tender to him my congratulations," without inquiring whether or not he has succeeded in establishing a medical reputation equal to that which *was* enjoyed by his merchant friend and sometime doctor in North Alabama.

It is a pity that nobody will give the Doctor's plan a *fair* trial. He is told, time and again, that gentlemen have had the *boldness* to try it. But I think it would be vain to attempt to convince him that his plan has been *fairly* tried as long as the results of said trials proved adverse to his favorite theory. He will always find a "loop to hang a doubt upon," whenever he is told that his plan has been fairly tried and found wanting.

He flatly tells Dr. Macgibbon, that "he has displayed but little skill in the application of his favorite remedies;" and treats Dr. Patton, of Mississippi, still more cavalierly. But I feel mortified to see my friend's *boldness* leave him when he comes to speak of the opinions of Drs. Stone and Hunt. I am reluctantly forced to think, that he *slided* over the opinions of these distinguished gentlemen in a rather *ungraceful* manner. They differ with him in toto, but I suppose that here, he concluded that "prudence was the better part of valor," as in these gentlemen he would have to contend with those who had succeeded in establishing medical reputations fully equal to that enjoyed by his commercial friend.

With the word *fair* ever upon his lips, Dr. F. is anything but fair in his representations of my views, which will be palpable to any one who will take the trouble to read both articles. He concludes his remarks of me by saying, that he has endeavored to represent me fairly; but if he did endeavor so to do, I must inform him that he has signally failed in his efforts, as will be seen by the following: After quoting me correctly as to my minimum doses (5 to 20 grs.) he gratuitously assumes, and strangely enough too, it appears to me, that I give the small doses in

pernicious intermittents and *remittents*, and leaves the inference that the twenty grain doses are given in the mild cases of intermittents. Upon this gratuitous assumption he goes on to say, that I have not learned as much by the lessons of experience as I might have done—that if I have succeeded in cutting short pernicious intermittents and remittents with the doses "which I now generally prescribe, viz: *five grains*, he is "inclined to think that not many others do so." I certainly prescribed, more frequently than otherwise, the smaller dose, for the simple reason that a majority of my cases are mild in their character, and yield readily to these small doses; but it by no means follows that I expect to cut short pernicious intermittents and remittents with the same quantity of quinine. If I gave the smaller doses in the most severe cases, I would respectfully ask the Doctor in what kind of cases he imagines the larger doses were prescribed by me? Why, in the mild cases of course, according to Dr. Fenner's theory—to prevent them from *becoming severe*. This is too bad, my dear Doctor—worse, even than old Dr. Cartwright's premedication in cholera times.

Again, he says, "that as the plan adopted by me in Tennessee, and pursued in this State for the past three years has been so entirely successful, he is at a loss to conceive how I could so well satisfy myself by my own observations as to the pathology of the disease; "as nothing but *microscopic observations* could positively settle this point."

If the success of my plan of treatment has been such as to preclude the possibility of my making these examinations, I think he ought rather to congratulate me upon it, than to taunt me for not having opportunities which he so frequently enjoys. He will certainly grant me the privilege of expressing an opinion upon the subject, based upon my own observations in the sick chamber, and the pathological phenomena observed by superior pathologists, who have enjoyed extensive opportunities for making those examinations. Without allowing this, it would appear to be a waste of time to publish the results. It occurs to me, however, that the object of the publication of these results is, that all men may profit by the light thus thrown upon the subject; particularly those who have not had opportunities of making *post-mortem* examinations.

Dr. Fenner "hopes that my courage will improve;" I say to him,

"I dare do all that may become a—physician. Who dares do more is ——."

Courage without judgment, is like the prowess of the untutored savage—of but little value to its possessor, and often injurious to others; and in this connection I would remind him of the favorite saying of his

extraordinary countryman, which contained true philosophy, "Be sure you are *right*, then go ahead."

Dr. F. has, certainly, displayed a great deal of courage himself, this will not be denied when it is recollected that he is not sustained in his peculiar views by a single physician in his own city. But he doubtless keeps up his courage by remembering that "prophets are never appreciated in their own country."

Knowing the high position occupied by Dr. Fenner, and fully appreciating his talent and ability to urge with great power whatever he thought to be correct, and being myself fully convinced of the error of his opinions upon this subject, I deemed it no less a duty than a privilege to offer such arguments and facts as were at my command, in opposition to them; believing that if generally adopted, they could lead to no other result than disaster to the patient and mortification to the physician. But it has been finely remarked by a distinguished statesman of the olden times, while alluding to those who were in error, "Let them stand, undisturbed, as monuments of the safety with which *error of opinion may be tolerated, when reason may be left free to combat it.*"

In conclusion, I must consign my friend to the tender mercies of the "brethren," hoping they will use no harsh "abortive" means in their attempts to cure him of his heterodox opinions.

Finally, I would offer for his reflection the sentiments contained in the following beautiful lines, by an American poet:

> "Truth crushed to earth will rise again,
> The eternal years of God are hers;
> But *error wounded writhes in pain*
> And dies amid her worshippers!"

Query.—To constitute a disease, *sui generis*, is it necessary that it should be "brought to us *from abroad*, and *regenerate itself* by *infection* or a *materies morbi* conveyed from one person to another?" If I am not very much at fault in my reading, this is another *entirely new idea* of Dr. Fenner's. No suspicion that the disease was brought from abroad ever entered *my* mind in attempting to account for its prevalence here, or elsewhere, nor have I ever maintained that it was contagious. I am aware that several distinguished authorities, (Prof. Flint amongst the number,) have advanced proofs of its contagiousness, but I must beg leave to remark that I have seen no evidence of its being thus propagated, myself; nevertheless, I am not disposed, absolutely, to deny it this property, in the face of such high authority.

X.—ADDENDUM TO DR. GORDON'S PAPER ON FEVER.

Since writing the above, I have seen articles by Drs. Fenner and Macgibbon, in the July No. of your Journal. I have a high regard for Dr. Fenner, personally, and disclaim all intention to cast censure on his "Abortive Treatment." My experience amounts only to this: *That quinine in the doses in which it rapidly cuts short remittents, fails entirely to control or aggravate typhoid fever.*

The cases alluded to by me, as treated by another physician, took five grain doses of quinine, every two hours, for many days. This would make one ounce per diem, an equivalent to Dr. Fenner's doses, though differently administered.

Dr. Fenner will allow me to suggest that his first three cases if tested by Jenner's method of analysis will be found to be of the type of relapsing fever, which has its natural crisis on the fourth day, usually by a profuse sweat. Indeed, after a careful examination of his eight cases, I find only one, (case five,) that I would have pronounced typhoid, and this was verified after death by the characteristic lesion. In the other two fatal cases, (exclusive of the eighth,) briefly recorded, the plates of Peyer were free from disease.

This is a matter of no consequence to those who believe in the identity of all fevers, but of great importance to us who repudiate this view, in estimating the value of remedies.

Though I am unwilling to generalize from a few cases, I am disposed to hold the matter with Dr. Macgibbon, "sub judice," testing it at every proper opportunity in future.

Marion, July 27th, 1853.

Part Second.

EXCERPTA.

1.—*Annals of Micrology.*

BY ROBERT D. LYONS, M. D.

ANATOMICAL AND PHYSIOLOGICAL MICROLOGY.

The numerous and varied investigations which are being now prosecuted with such untiring energy and zeal in the various departments of medicine to which the microscope is applicable as a means of research, and the extreme difficulty of collating and comparing the results arrived at by the many observers of the several European and Trans-Atlantic schools, which lie scattered, in different languages, through the various scientific journals of the day, and in the Transactions and Reports of Academies and Societies, have led to the belief that it is highly desirable that the most important results of recent microscopic research should be brought into the compass of a retrospective report in the English language, which should appear at stated intervals. It is hoped that such a report will be, in some degree at least, useful to the advancement of medical science, and acceptable to the members of the profession, who will thus be enabled to keep their knowledge on a level with the discoveries of the day, in this department, which is, perhaps, now more extensively worked and more productive than any other portion of the entire domain of medical science.

In adopting the term Micrology, and in devoting to this subject a special section of this journal, we do not wish to be understood as implying that we in any way regard this class of subjects as different from those of general medical literature. We have always maintained that the microscope is only *one* of the many means of research at our command, and that to be used successfully it must be employed conjointly with the other aids to scientific inquiry. In placing certain classes of facts and observations in a special category under the term Micrology, we do solely for the sake of convenience, and for the purpose of correlating inquiries which have great points of resemblance in the methods employed to pursue them, and in the results obtained thereby Nothing, therefore, is farther from our thoughts than the wish to separate the

minute structure of an organ or a tissue from its chemical properties; or again, to separate these characters, one or both, from those of the physiological or pathological series, or altogether from the therapeutical indications furnished in this order of induction alone, with anything like scientific precision and certainty. We adopt the term Micrology arbitrarily, but of necessity, and for the purpose of carrying out the great principle of the division of labor. So numerous and extensive, in fact, are the inquiries and observations in this department, that still further division is called for, and must be adopted, if we wish to confine our retrospect to anything like reasonable limits. The investigations going on in Vegetable and Animal Histology are naturally divided from each other. Moreover, we find that the memoirs on the minute structure of the lower animals are so numerous, that it would be impossible to combine them, except occasionally for the purpose of illustration, with the researches in human histology. And here again it is absolutely necessary to separate the pathological from the normal inquiries; we have therefore made arrangements for giving two reports annually, one on Normal Micrology, the other on Pathological. In each we hope to be able to give a succinct and satisfactory *résumé* of the advances which have been made in the preceding year. In commencing such a system of reports no ordinary difficulties present themselves; the amount of investigations of an important kind made within the last few years has been so great, that it becomes absolutely necessary to make selections, and in some cases to omit everything except a reference to the original communication, and a simple statement of its nature. It was also found necessary to limit our notices of memoirs to those which have appeared within the last year—a limit beyond which we have not gone, except in a few instances, for the purpose of completing a particular subject or giving a more clear view of an author's results. We feel confident that, even with great care on our part, many valuable essays have escaped our notice; but we trust that authors will kindly attribute this to its real cause—viz, the great extent of literary ground we had to traverse in our search through reports, transactions, and journals.

The intimate connexion of chemistry with micrological investigation is becoming every day more fully recognized; indeed, Lehmann has introduced the term Histo-Chemistry, to designate these necessarily allied methods of research. We need, therefore, make no apology for introducing accounts of the chemical constitution of tissues, when occasion may require it. It is, in fact, only by the combined employment of Chemistry and the Microscope that we can hope to obtain the solution of many important problems still unsettled.

Our pathological part will appear in October of this year.

In collating the results of different microscopic observers, much perplexity is caused to the reader, by the different systems of measurement employed, the fraction of a line and a millimetre being constantly found in juxtaposition. It is greatly to be regretted that no successful measures have yet been adopted to obtain the recognition of a uniform standard. This would be a real boon to science; and we therefore urge upon microscopical observers the necessity of a combined movement to effect it. As editorial labours are greatly facilitated by working from *early* copies of papers, we will feel particularly obliged to any authors who will be kind enough to forward us copies of their essays. They may be addressed to the editor of this journal, or directly to ourselves, at Dublin. Continental papers will reach us readily if enclosed with exchange parcels of Reports and Transactions forwarded to the Royal Irish Academy.

HISTOGENESIS, OR DEVELOPMENT OF TISSUES.

"*Tissu Cellulaire Artificiel*" *of Melsens.*—The results of chemical investigation have made us familiar with the elementary composition of almost all the organized products which constitute the structures of animals and plants

and even in those instances in which our knowledge in this department has not yet reached the limits of scientific accuracy and precision, we may confidently anticipate that the labours of the numerous inquirers who are now actively engaged in prosecuting researches in organic chemistry, will, at no very distant period, fill many of the more important *lacunæ* which may still be pointed out. On the other hand, histological inquiry has revealed to us the intimate structure and minute organization of every tissue, and with the aid of the microscope we can now, with perfect facility, study the ultimate form of every constituent element of the vegetable or animal, which possesses distinct morphic properties. What is still to be desired on this side of the general inquiry into the nature of organic bodies may be hopefully looked forward to; its accomplishment will no doubt, before long, reward the industry of the present generation of zealous and intelligent micrologists. But while chemistry thus informs us of the elementary composition of organic bodies, and their intimate structure is revealed to us by the microscope, we have yet much to learn with regard to the primary operations by which definite forms are assumed by the elementary tissues, at the moment of their formation. Here, indeed, exists a broad and deep *lacune*, which separates the results of the chemist and the micrologist. It is true, that bold speculations have been attempted to bridge this chasm; yet, notwithstanding what we owe to the splendid generalizations of Schleiden and Schwann, no portion of organic science is more incomplete, or presents so many undetermined problems.

Many considerations concur to establish the probability of the proposition that the fluid state is the first in which the elements of a tissue exist previously to their undergoing a morphic determination, precipitation, histomorphosis, or whatever analagous term may be used to denominate the change which occurs in the formation of a tissue of definite shape. The chemical properties of the fluid are familiar to us; so likewise are the forms which it assumes as a distinct tissue; but what those laws are, in obedience to which the latter condition results from the former, constitutes, I think, one of the most obscure, and at the same time intensely interesting questions which can be found in the whole domain of organic science.

Under the head of *histogenesis*—a term employed to designate the subject in question—many of the most obscure and difficult problems are included. Thus, for instance, we may inquire—what is it that determines the transudation of a blastema? what, the particular nature of this fluid, which cannot, in all probability, be identical in any two normal tissues, not to speak of those of pathological origin! And this latter question, it may be remarked in passing bears a very great analogy to that which demands a solution of the obscure physiological problem, why the liver secretes sugar and bile, the kidney urine, &c., &c.

While science has still to deplore the extremely imperfect and unsatisfactory condition of this portion of its domain, we are yet not without indications of the direction in which researches may be prosecuted. Thus we owe to Ascherson a knowledge of the interesting fact, that the contact of two homogeneous fluids, oil and albumen, results in the production of morphic elements. More recently still, the observations and experiments of M. Melsens have established the possibility of one of these fluids undergoing distinct and very remarkable histomophosis. The experiments of this observer must be looked upon rather as a highly interesting exegetical study,—furnishing a valuable amount of inferential and collateral evidence, capable, to a certain extent, of application to the explanation of the changes which occur within the system,—than as a direct contribution to our knowledge of these operations. *

* We may observe, however, that the action of acetic and other acids on the precipitation of albumen in the presence of neutral salts, had been already fully described by Parkes, (Medical Times, July, 1850.)

As will be seen, the results of this inquirer bear more particularly on the action of physical causes on a solution of albumen in combination with various salts, by which it is not precipitated. The solution was prepared as follows :—The white of an egg was mixed with its volume of water, and filtered; this constitutes the *normal solution of albumen* with a specific gravity of about 1020. The filtered liquid is saturated with salts, which are added in excess, after which the fluid is filtered again, to separate the excess of salts; the fluid resulting from this second filtration may be denominated the *normal saturated solution of albumen.* The normal albumen saturated with chloride of sodium has a specific gravity of about 1200.

"My experiments have been made," says M. Melsens, "with almost all the salts which are without an apparent action on albumen, as well as with those which begin to precipitate it, but whose precipitates are soluble either in an excess of albumen or of the salt; for some salts of baryta, of lime, of magnesia, and of ammonia, &c., the albumen must be left in excess, for in saturating it with these salts it is precipitated, if they are added in excess; when we wish to examine the re-actions in this case, it is necessary to add, little by little, the solution of normal albumen, until we obtain the resolution of the precipitate first formed. I will not pronounce on the nature of the precipitates obtained; but it will appear evident that we must in the generality of cases, admit that the albumen is precipitated in consequence of a particular physical disposition of the liquid; that if at times the precipitation does not occur immediately,—in dilute liquor, for example,—agitation may cause a troubled condition of the fluid, as it occurs in precipitation, crystallization, solidification of water, of sulphate of soda, of phosphorus, &c. . . Tribasic phosphoric acid precipitates normal albumen saturated with salts; certain salts, amongst which are borax, phosphate of soda, acetate of soda, acetate of potash, form and exception to this rule; however, if the fluid be agitated with a glass rod, a troubled condition is slowly produced by the mechanical action."

These examples will be sufficient to show the nature of the results which have been arrived at by M. Melsens; similar experiments were made with a variety of other substances, such as corrosive sublimate, ether, alcohol, creosote, &c., but it is unnecessary to follow out their details. I shall therefore pass to the consideration of a series of experiments, which to many will appear still more conclusive as to the action of physical causes on albumen.

"If," says M. Melsens, "after the experiments which precede, I am induced to believe that the particular physical constitution of the liquids plays some part in the precipitation of albumen, those which follow cannot leave the least doubt as to the action of agitation.

"Some very dilute solutions remain limpid until beaten with a glass rod, when they become troubled, and immediately parcels of fibres may be seen to form under the influence of agitation; under the microscope these appear as very distinct organized fibrous forms, which by juxtaposition and felting together, constitute actual membranes. We have thus a phenomenon conformable to the production of mineral precipitates by the influence of agitation."

It is only necessary to notice briefly some of the remaining experiments. A current of air was passed throug a solution of normal salified albumen, sufficiently dilute not to allow of the froth passing out of the vessel; this froth was seen to be transformed into a solid body, insoluble in ammonia, potash, water, or dilute acids. To obviate two objections which might be started to this experiment, air saturated with the vapour of water, and hydrogen purified by caustic potash and saturated with vapour, were successively employed. Lastly, to avoid all sources of error, a solution of albumen diluted with water was agitated *in vacuo*, by converting the vessel into a sort of water hammer, after expelling the air by heat and an air-pump, the orifice being subsequently hermetically sealed. The solution, perfectly limpid at first, became troubled after a few shakes, and a membrane was rapidly formed.

The solid body thus formed from a limpid solution of albumen by the simple effect of agitation, was subjected to microscopic examination by M. Gluge, from whose report the following extract is taken:

"The albumen of the white of an egg, solidified by mechanical action, resembles false membranes, and even serous. It is presented to our view under the form of membranes covered with granulations of from $\frac{1}{2}$ to 1 millimetre in diameter, white, semi-transparent, about $\frac{1}{4}$ or $\frac{1}{2}$ millimetre thick, and sufficiently elastic. With a magnifying power of 300 we can distinguish an amourphous substance finely punctated, in which are found fibres, sometimes isolated, sometimes united in bundles, like the fibres of cellular tissue, more often easily isolated and elastic. Their diameter varies from one eight-hundreth to one four-hundreth of a millimetre: more rarely there may be seen large and transparent fibres, analogous to those which are met with in fibrine. In the middle of these fibrous bundles may be observed granulations composed of little globules of one-four-hundreth to one-eight-hundreth of a millmetre in size, and enclosing some bubbles of air. These globules are sometimes very regularly grouped, and then form rounded masses. The fibrous aspect of the solidified albumen differs from that which albumen possesses when transformed into pelicles, thin opaque, and much less elastic, such as are obtained by the process of Aschesron. These last present folds, and not fibres fully developed like the former; they appear rather to be formed of very small granules."

I have myself had an opportunity of inspecting a very beautiful and large specimen of albumen membrane formed by the process of agitation in the possession of M. Melsens. To the naked eye it presented the appearance of a membrane of a whitish colour, formed by the interlacement of parcels of fibres, which enclosed spaces of an irregular shape; it was tolerably tough, dense, and resistent.

M. Melsens was so kind as to present me with a small portion of the preparation, which I submitted to a most careful microscopic examination on my return to Dublin, and I had thus the satisfaction of verifying, in the most complete manner, the results of M. Gluge's investigations. As my examination was conducted with powers higher than those employed by this eminent micrologist, and as, consequently, some of the more minute details were better shown, I append a brief statement of the appearances seen in my examinations.

The specimen examined with a power of 420 diameters presented four distinct kinds of elements. Firstly, a granular base; secondly, fibres, which were flat, round, straight, curled, isolated, or interlaced in various ways; thirdly, spherical bodies of different sizes; and lastly, flat, scale-like particles, these being the least numerous constituents.

The granular base formed a very considerable proportion of the entire specimen, but did not appear to be uniformly disposed throughout it, as in some portions it formed nearly the entire mass, while in others it was almost altogether replaced by fibres.

The solidifying force would thus appear not to have acted with uniformity. To determine what modifications of it produced granular matter,—what fibres —what again caused the formation of the little spherical bodies,—are questions of too delicate a nature to admit of ready solution. Could we arrive even at an approximate explanation, a great step would be achieved in the history of the obscure process of histogenesis.

The fibres constituted a very large element; the majority of them presented an extremely clear, decided, and *clean* outline. Many of them lay paralled to each other, as far as they could be traced, formed straight lines. Others again, terminated in rapid curves; in one little parcel, which was very well shown, and which I had thus an opportunity of observing very attentively, they curled and interlaced with each other in a very graceful manner. In their general disposition, as well as in size and shape, they bore a very close

resemblance to the fibres of the yellow elastic tissue: for which, I make no doubt, they would have been readily mistaken by any one casually looking at them through the microscope. Some masses of the fibres lay isolated; others were to be seen taking an origin in the granular base, where their commencement could not be well defined; others, again, lay in contact with the scaler like bodies, to be presently noticed. To me, however, the most interesting of all the structure observable in this preparation, are the spherical bodies. With the exception of the scales, they were the least numerous. I observed them in two situations, in most abundance on a square-shaped mass of the granular base, and in another portion in contact with fibres. The smallest bodies of a spherical shape, but not those to which I shall more particularly refer, were about the size of the ordinary oleo-albuminous granules, and closely resembled them in their optical properties. The *spherical bodies* were nearly uniform in size, grouped quite close to each other, and presented all nearly similar characters. They showed a dark border, their interior varied with the amount of light transmitted, but under all conditions of light, both as to intensity and obliquity, they presented a nucleus, which in all was of an elliptical shape, though the bodies themselves appeared as nearly as possible spherical. This nucleus was in length equal to about one-half the diameter of the sphere, and in breadth about one-eighth. What was the nature of these bodies? They were certainly not either spheres of oil or bubbles of air; there was not the slightest probability of the former substance being present; air-bubbles they also could not be; the specimen had been at rest in spirit for a very considerable time, while, as more positive evidence of their nature, I would adduce the peculiar nucleus, which in all was oblong, and did not disappear under any conditions of light. May we then regard them as nucleolated nuclei, or small nucleated cells? I confess that I can see no objection to this view; it is only to be regretted that it in no way throws light on the relation of the containing body to the contained, as to priority of origin, or necessary connexion of one with the other. It is extremely interesting, however, to learn that albumen is of itself capable of undergoing a celluloid development. The last objects I shall notice, as observed in the preparation, are the scale-like bodies. There was only one situation in which I clearly recognized a group of them, though others existed scattered here and there. The group of scales was in close proximity to a parcel of fibres, and lay partly on a mass of the granular base. They appeared under the form of delicate laminæ, somewhat of a quadrangular figure, their borders irregular and serrated, and some few presented an imperfect nucleus; granules were likewise to be found on the surface of two or three. Were these of the nature of cells? This is a question which I cannot decide; it is possible they may be merely the result of a process of *flaking*, or of the fusion of granules precipitated together in small masses.*

Harting† has investigated the precipitate produced by mechanical means in albumen, and finds, that in chemical composition it is different from the true connecting tissue; and he considers that the appearances are due to *flaking*, such as occurs in many fluids. M. Gluge's and my observations show however, that this is not the case.

In connexion with the researches of M. Melsens, we may consider those of M. Panum on artificially produced morphic elements. We introduce also some interesting results of another class.

Artificial Cells, and Artificial Milk.—M. Panum‡ has been occupied with

* Academie Royale de Belgique, extrait du tome xviii., No. 7 des Bulletins. See, also, Lyons' Report on the 'Tissu Cellulaire Artificiel' of Melsens, Dublin Quaterly Journal of Medical Science, Feb. 1852, p. 237. We have reproduced the chief results of this memoir here, as they have been contested by Harting and Panum.

† Schmidt's Jahrbucher, No. 8, 1852.

‡ Virchow u. Reinhardt, Archiv., vol. iv. p. 155; translated from Bibliothek for Laeger, July, 1850.

researches on the nature of the protein compound (casein of authors) which is thrown down as an amorphous granular mass, when blood-serum, previously rendered neutral in order to contract the solvent force of its salts, is diluted with water. He has already published observations which lead him to think that this compound is identical, not with albuminate of soda, but with casein. He has obtained it in much greater quantity in the serum of the blood of women than in that of men, in greatest quantity in the serum of parturient women, and in small quantity in that of nurses. Thinking that the synthetic method might be brought to the aid of the analytic, to prove the nature of the substance in question, he conceived the idea of artificially forming with it, by the addition of the other necessary elements, some generally known products, such as milk and cheese, in which casein abounds. For this purpose he procured from six to eight pounds of tolerably clear serum of ox-blood, to which he added concentrated acetic acid (about six drops to the ounce), with a view to neutralize the salts ; it was then treated with a large quantity of water. After being allowed to rest for some time, the compound already alluded to settled at the bottom of the vessel, and the supernatant fluid was poured off ; by repeated additions of water, the substance was washed and freed from soluble albumen and other impurities, after which it was suspended in water. The fluid was now heated to 100° Fah., phosphate of soda was added until complete solution was effected, and then butter and sugar in the proportion in which they exist in milk, the whole being well shaken in a bottle. On cooling, this mixture assumed a milk-white colour, and had very much the taste of milk ; it left however, a weak but bitter after-taste.

Under the microscope there was observed a large quantity of small spherules, having much resemblance to milk-corpuscles. On comparison with those of real milk, scarcely any difference could be seen, except that the artificial corpuscles were in general, though not in all instances, larger than the natural.

That these corpuscles possessed a membranous envelope, and were not merely drops or granules of butter, appears probable from the fact, which was distinctly observed, especially in the larger ones, that the envelopes presented very evident though fine fissures ; they were also not dissolved by ether, unless the membrane was previously removed by the action of acetic acid. Besides these small corpuscles, others of larger size, and exhibiting an interior formation were found: these, says M. Panum, if met with in a living organism, would be regarded as nuclei with nucleoli, granular cell contents, &c. When sugar of milk was employed instead of ordinary sugar, the same structures presented themselves but of a much smaller size. In attempting to follow out the supposed identity of the chief ingredient of the artificial with that of natural milk, M. Panum endeavoured to produce coagulation ; but even with the substitution of milk sugar in varying quantities, this could not be effected completely. The artificial fluid was readily drunk by two young cats, who were afterwards killed ; in one, which was killed after it had drunk a very large quantity, the stomach was almost empty, but the chyliferous vessels were filled with chyle, though the animal had not had any other food for twenty-four hours previously. In the second only a small quantity of chyle was found in the vessels.

The production of these pseudo-cells has considerable interest in connexion with similar results arrived at by other processes. Panum remarks their analogy to those of the haptogen membrane of Ascherson. He has likewise observed cellular bodies to be formed by the union of chloroform and serum, which, if shaken together, form an emulsion, from which, on being allowed to rest, a copious sediment is thrown down. This, on microscopic examination, is seen to consist of small oval vesicles of about 0·001‴ in diameter, which consist internally of chloroform, and externally of a membranous envelope of coagulated albumen.

Cell-Development.—Donders* has investigated the elementary tissues, in reference to their morphological, chemical, and physiological properties. The following are his conclusions :—1. In plants and in animals an insoluble is formed from a soluble substance, which in virtue of its constitution takes the form of a cell-membrane. 2. The animal cellulose remain as such, or becomes thickened or is absorbed ; it increases or diminishes with or without connexion with other cell-membranes ; it becomes atrophied and forms fibres, which may again become thickened and grow : this atrophy takes place by a fibrous organization of the intercellular substance. 3. All animal cell-membranes, with their derivatives, have similar chemical and physical properties, and analogous chemical composition. 4. This animal cellulose exhibits strong resistance to most re-agents, has a slow change of substance, but neither contractility nor sensation.

Growth of Individual Cells.—Hugo von Mohl† maintains that the innermost layers of cell-membrane are the youngest, while Harting and Mulder say that the outermost are the youngest. The latter authors are of opinion that the circumstance of thin, recently formed membrane being coloured blue by iodine and sulphuric acid, while in many full grown cells only the inner layers manifest this re-action, while the outer are tinged yellow by these two substances, gives ground for the deduction that these outer layers have been formed subsequently to the others, and that the innost layers of the full-grown cells are the same membranes which alone constituted the wall of the young cell. Mohl shows, by numerous careful observations, that the walls of all the elementary organs of vegetables may be brought, by the action of caustic potash, or of nitric acid (though not by sulphuric acid, which was the re-agent employed by Mulder and Harting), into a condition in which they assume a blue colour with iodine ; the only exceptions being the cuticle and perhaps the intercellular substance of some higher plants. Mohl therefore adheres to his already advanced views with regard to the endogenous deposit of the new materials in vegetable cells. We have introduced this illustration from vegetable cell physiology, as observations are more easily made on the growth of the vegetable than the animal cell. We may here refer to a memoir by Handfield Jones‡ on the development of fat-cells without previously existing nuclei. As the result of several investigations, he thinks that it must be admitted that the fat vesicles are ordinarily developed without pre-existing cells or nuclei, the process seeming to consist in the gradual separation of oil from the blood, or rather from the exuded liquor sanguinis, whereby oil-drops are formed ; these are at first minute, but afterwards enlarge by addition and coalescence, and soon become enclosed in an envelope of protein material, [whence derived ?] This envelope is at first very feeble, perhaps scarcely organized, but afterwards acquires considerable strength, and sometimes persists after the absorption of the oily contents.

Cell-Development.—The extra-cellular development of cells, a condition admitted only by a few authorities in vegetable physiology, has been very extensively received in the domain of animal physiology and pathology, more especially since the promulgation of the cell-theory by Schwann. Remak has, however, from an early period, been strongly opposed to this doctrine, and has pointed out difficulties against its adoption, which are to be found, as he maintains, even in the writings of its chief supporters. In a recent communication

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* Siebold und Kolliker, Zeitschrift, iii. 343. Schmidt's Jahrbücher, 12, 291. (We quote from the latter.)

† Botanische Zeitung, vol. v. p. p. 497 ; also Taylor's Scientific Memoirs, Part I. Nov. 1852 ; Part II. Feb. 1853. See, also, the recent work of Dr. Hermann Schacht, Ueber die Pflantzenzellen.

‡ See London Medical Gazette, 1850.

on the subject* this author cites many examples of admitted cell-development by segmentation—segmentation of the Vitellus [Schwann], by formation of daughter-cells in the growth of organs, and the transformation of embryonal cells into tissues, epithelium, blood-cells, and muscular fibres (Reichert). He also calls attention to the absence of free nuclei in embryonal cartilage [Kölliker], and in the deeper layers of the epidermis [*idem*]. J. Müller has likewise observed that endogenous cell-formation plays a large part in pathological anatomy. Remak himself considers the extra-cellular developement of animal cells as improbable as the equivocal generation of organism. As the result of a series of investigations undertaken to determine this question, he states that he has observed the propagation of the blood-corpuscles to take place by segmentation in the embryos of birds and mammalia; in the larvæ of frogs he has seen the striped muscular fibres to originate in the longitudinal division of cells. Observations since more extensively prosecuted have satisfied him that this is the general method of transformation of the embryonal cells into tissues. He gives the results of researches on the segmentation of the vitellus and the division of its cells. The cells he believes to pass subsequently into permanent tissues; thus the primary vessels are at first solid cylinders, consisting of embryonal cells united together, the external of which form the walls of the tube, while the central or axis cells pass into blood-corpuscles. He considers this theory applicable to pathological as well as normal histogenesis.

False Cellular Appearances.—Harting† of Utrecht, calls the attention of microscopic observers to little bands or halos [coronæ] caused by diffraction and consequent luminous interferences in objects seen through the best microscopes. Thus a bubble of air, which is well suited for making this observation, will appear as if surrounded by a thin membrane, and such errors have been often figured and described. Frequently not only one, but two or three, or even four, such bands may be seen, the margins of which sometimes exhibit prismatic colours. (*Medico Chir. Review.*)

[*To be continued.*]

II.—*Singular Case of Foreign Substances in the Intestinal Canal.*

By D. Hayes Agnew, M. D., Philadelphia.

The following case I am induced to report from its very singular character: On examining the body of an individual who, I believe, labored under some mental alienation during life, my attention was attracted to an adhesion between the parietal and visceral layer of peritoneum over the cœcum, upon the separation of which a small opening was perceived through the walls of the intestine, disclosing a dark-looking substance, which, upon examination, proved to be a large mass of straw, little less than an ordinary sized fist, and firmly impacted in all the space below the ileo-cœcal valve. Noticing the transverse colon very much distended, an incision was made into its cavity, where were found a pair of suspenders, three rollers, and a quantity of thread, interwoven with one another. The webbing, which evidently was his suspenders, exceeded one and a quarter inches in breadth, and must be several feet in length, inasmuch as it extended through the ascending, transverse, and a portion of the descending colon, and doubled in several places upon itself. The rollers were of ordinary muslin, over one inch in width and the same in diameter,

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Müller's Archiv., No. 1, p. 47, 1852.
Edinburgh Monthly Journal, June, 1852, p. 579.

but which must have been of much greater size when swallowed, as they had, in their progress along the intestine, become unrolled, leaving long ends which were encased within layers of fæculent matter. The peritonitis, which no doubt had been the principal cause of death, was not, however, produced by the escape of any intestinal matter into the serous cavity, no such discharge having occurred. the opening into the cœcum only presented itself after the reflected layer of the peritoneum was separated therefrom. Had life been prolonged, it is highly probable that the ulceration would have extended through the walls of the abdomen, and the cœcal contents passed out by this artificial route. [*Med. Examiner.*]

III.—*Therapeutical Record.*

Anchylosis.—In partial anchylosis of the knee-joint, Mr. Hancock (Lancet, Jan. 29) has met with great success from forcible extension, while the patient is under the influence of chloroform. Only in one of six cases did inflammatory symptoms follow.

Ascites.—Teissier (Gazette des Hôp., 1852, p. 120) relates 3 cases of ascites treated by iodine injections, composed of 60 to 70 grains of iodide of potassium, 7 to 10 drachms of tincture of iodine, in 6 ounces of water. The iodine was rapidly absorbed and excreted through the kidneys.

M. Ore (Bull de Thér., Sept. 1852) has used iodine injections in 5 cases: two were cured; three died, but not from the injection. The first effects were sinking, pallor of face, lowering of pulse, and severe pain; in ten minutes these symptoms went off; subsequently, there was heat, pain, fever, some meteorism, colic, and sleeplessness. No important peritonitis, however, ever appeared to come on. The strength used was one part of the tincture to three of a vehicle. The remedy is contra-indicated in ascites, dependent upon liver or heart affection, or when there is kidney-disease. When the ascites is from peritonitis, or follows ague, the injection is to be used.

Bubos.—M. Bonnafont (L'Union Médicale, 1852, No. 155) describes a plan of treating suppurating bubos, by which he says much time is saved, and unsightly scars are avoided. When the bubo is quite ripe, it is pierced in its most tense part, and in the direction of the ileo-pubic axis, by a seton needle, carrying four threads. The two apertures of entrance and exit of the needle must be formed beyond the perimeter of the phlegmasia of the skin. The pus is gently but effectually evacuated by pressure, and then compression is applied by means of pieces of agaric or a dostil, occupying all the space between the apertures without obstructing them. To these scraped lint is applied, and the whole is supported by a spica bandage. In the evening, somewhat forcible compression is again to be applied, so as to empty the abscess; and the pad is to be again applied. This it to be repeated twice daily until complete cure.—Care is to be taken not to draw out the threads unless free discharge is secured, and their ends should be tied together. The medium time a bubo so treated requires for cure is twenty days, although some are cured in ten; and the points made by the seton leave hardly any marks. When the bubo has been opened before the entire gland had suppurated, the case is sometimes more tedious, a clear or sanguinolent fluid continuing to ooze. In such cases, vinous injections, combined with compression, have been found useful.

Cancer.—M. Devay (Gaz.Méd., 1852, No. 52), of the Hôtel Dieu Lyon, has long been engaged in investigating the therapeutical properties of conium in cancer, being of opinion that Storck's experiments should be resumed with the aid of the improved chemical knowledge of the present period. He finds

the best preparations to be an extract and balsam, containing 1 per cent. of conicine, made from the seeds of the plant, gathered when at maturity, of full weight, and of an ash-grey colour. As the result of his researches he states,—1. That an ointment, applied externally, in chronic enlargements of scrofulous glands, possesses a resolvent power greater than that of any other substance. 2. In ingorgements of the uterus, or inflammatory hypertrophy of the organ—so frequently complicating its prolapsus or deviation—this medicine, employed internally and externally, is of great service. 3. In cancerous affections it exerts remarkable calming effects, and in some cases even cures seem to have resulted from its employment, especially in the atrophied form of scirrhous. Its use is less satisfactory in soft and rapidly increasing tumours, but the progress of some of these has seemed to be retarded. In other cases, it has diminished the size of secondary tumours, rendering the primary ones more amenable to surgical operation. As a means of assuaging suffering, whether used topically or taken internally, it is invariably preferred by the patients to opium and all other narcotics.

M. Manec, surgeon to the Salpêtrière, has just obtained a recompense of 2000 francs from the Académie des Sciences (Gaz. Méd., 1853, No. 19), for the perseverance he has shown in investigating the action of Frère Cômes Arsenical Paste in more than 150 cases of cancer, in some of which he obtained unhoped-for results. His experience leads him to these conclusions :—1. That the arsenical paste penetrates the cancerous tissue by a sort of special action which is limited to it. This action is not simply escharotic, for beneath the superficial, blackish layer, which the caustic has immediately disorganized, the subjacent morbid tissue seems struck with death, though it may retain its proper texture, and almost its ordinary appearance. Later, the cancerous mass is separated by the eliminatory inflammation which is set up around its limits. The same paste, which extends its action more than six centimetres deep in a cancer of close texture, when applied to superficial gnawing ulcers, usually only destroys the morbid texture, however superficial this may be, and respects the sound parts. 2. The absorption of arsenic is proportionate to the extent of surface to which it is applied ; and as long as this does not exceed a two-franc piece in size, there is no danger from this source. A large surface should only be attacked by successive applications. 3. Arsenic which is absorbed is chiefly eliminated by the kidneys, during the space of time of not less than five, and not more than eight days, as amply demonstrated by Pelouze. Thus, if we allow nine or ten days to intervene between successive applications, all danger from absorption may be avoided.

M. Gozzi, [Bull. delle Sc. Med., xx. p. 231] strongly recommends the following caustic for the destruction of cancerous growths :—Corrossive subl., ℈j ; caustic potass, 3 ss ; arsenic and cerussa, *aa*, gr. vj.—to be made into a paste with starch and white of egg. While using this or other caustics, emollient poultices, ointments, &c., should be avoided, as diminishing their effects, unless the irritation produced by their application has been excessive. M. Gozzi objects to the usual plan of destroying the tumour, layer by layer, from the apex to the base, the later becoming very indurated after these repeated applications, and offers great obstacles to the approximation of surrounding granulations and their cicatrization. He prefers applying the caustic laterally, in the direction where the tumour seems most inclined to separate.

M. E. Cazenave [L'Union Médicale, ix.] speaks very highly of a caustic formed by pouring hydrated sulphuric acid on powdered saffron.

Convulsions puerperal.—Mr. Bolton [Lancet, Jan. 29] relates a case in which severe puerperal convulsions, coming on immediately before labour, and unchecked by depletion, were completely arrested by the inhalation of chloroform.

Dr. Holst [Neue Zt. für Geburtsk., vol. xxxii. p. 85], in a bad case of puerperal convulsions, attended with great rigidity of the os uteri, threw up warm water against the os uteri for six minutes, as in Kiwisch's method for bringing on labour. The os speedily dilated, and labour was completed by the forceps.

Croup.—Mr. Smith (Med. Times & Gaz , March 5) relates 4 cases of croup in which *tracheotomy* was performed. All the patients were in extremis, and in all the result was fatal.

M. Guersant (Bull. de Thérap., xlii. 293) details one case, and refers to another,in which tracheotomy was performed twice in the same child. M. Guersant has now operated in croup 150 times. The earlier cases were less successful than the later; 13 of the last 40 private cases, and 13 of the last 31 hospital cases, recovered, or at the rate of 36-62 per cent. In 3 cases there was faulty deglutitionafter the operation, and food escaped through the tracheal opening. In such a case the child must be fed through the œsophageal tube passed in through the nares.

Diabetes Mellitus.—Dr. Hanekroth [Schmidt's Jahrb., 1853, p. 173] recommends a mixture of sulphate of iron 3 ss., tinct. cinch. c., acq. menth, *aa.* 3 vj. ; 20 to 30 drops every two hours. In two cases there was perfect recovery.

Mr. Sampson [Lancet, Jan. 8] states that the permanganate of potash [grs. ij.—v. in solution thrice daily] has a marked effect in reducing the quantity of urine in cases of obstinate dyspepsia, and in diabetes mellitus. In a case of the latter disease, the quantity of urine fell from 10—12 pints to its normal amount, but still contained sugar.

Diarrhœa.—Mr. North [Med. Times & Gaz., Feb. 12], in noticing the employment of *dilute sulphuric acid* (in 3 ss. doses every 2 or 3 hours), states that it is in the serous forms especially when attended with cramps, that it is most useful.

Dyspepsia.—Dr. Bennett [Ed. Monthly Journ., Feb. 1853], in a lecture on Dyspepsia, after insisting on the necessity of seeing that there is no excess in eating and drinking that the food is properly masticated, and that proper rest is taken after food, remarks that the sense of load or weight is best relieved by acids, especially the hydrochloric. Acid eructations and cardialgia are best relieved by alkalies and bitter tonics. In cases in which fatty matters do not appear to be digested, liq. potassæ is recommended. When the flow of bile appears deficient, mild mercurials and rhubarb is the best treatment.

Epilepsy.—Dr. Marshall Hall, in a series of articles [Lancet, Jan. and Feb.] brings forward evidence to show the advantage of *tracheotomy* in preventing the severest attacks of epilepsy, by rendering impossible the *laryngismus*, on the occurrence of which depends the violence of the fits.

Dr. Tyler Smith [Lancet, March 5] has performed tracheotomy in a case of "uterine epilepsy." The operation appeared useful but the ultimate issue of the case is not recorded.

Dr. Abbot [Amer. Journ. of Med. Sc., Jan. 1853] gave the *cotyledon umbilicus* (grs. v. night and morning) to a man aged 50, who had been subject to epilepsy every two or three weeks for five or six years. The fits entirely ceased, but the patient began to totter in gait, and had attacks of partial loss of consciousness. The cotyledon was discontinued, and the fits returned.

M. Moreau, physician to the Bicêtre, has been induced, by the recommendation of M. Herpin, to try the oxide of zinc. His results do not support M. Herpin's statements.

Erysipelas.—Dr. Creighton [Edin. Month. Jour. Dec, 1852] speaks highly of the *tinct. ferri sesquichlorid.*, in doses of ♏x. to ♏xv. every 2 or 4 hours. It appears, however, that ant. pot. tart. [gr. ¼] was also given, so that the inference appears doubtful.

Fever Intermittent.—Dr. Pepper [Amer. Jour. of Med. Sc., Jan. 1853] has tried *bebeerine* and *cinchonia*, in ague. The former succeeded in 2 cases, failed in 2; the latter seemed more useful.

Fever, Continued.—Dr. Barclay [Med. Times & Gaz., Jan. 8] records the effect of large doses, [10 grains every 4 hours] of *sulphate of quinine*, in 18 cases of continued fever [chiefly typhoid?] In 5 cases there was marked depression; in two the pulse became slow, without depression; in 11 no physiological effect was produced. The average duration of the fever was not shortened by the treatment, and cinchonism in fever is considered not to be "more speedy safe and effectual" than other methods.

Dr. Dundas [Med. Times & Gaz., Jan. 29] denies the accuracy of Dr Barclay's inferences, states that "no prostration of the vital powers" is produced by quinine, and relates a case of typhoid [?] fever in which the remedy was useful.

Dr. Douglas [Amer. Jour. of Med. Sc., Jan. 1853, p. 282] states that he has never seen the least good follow the use of large doses of quinine in *typhoid fever.*

Fistula in Ano.—M. Alquié [Gaz. des Hôpitaux, No. 48] states that he has found it a very beneficial practice to cauterize the lips of the wound by means of nitrate of silver, after the incision of fistula, premature adhesion being prevented, without the necessity of interposing tents, lint, or other material. The nitrate should be applied, but only to the lips, twenty-four hours after the operation, and repeated first every, and then every other morning. He was led to the practice by observing the condition of wounds when touched by the nitrate. The pellicle or superficial eschar is eliminated in a day or two; the surface so covered being unsuited to contract adhesions. This covering also enables the wound to tolerate the presence of the intestinal discharges which pass over it. Most persons complain little of the pain caused by the application; but in some cases it is severe.

Fractures Compound.—M. Trastour details in a series of papers [Archieves Générales, vols. xxix. and xxx] numerous cases of compound fracture he has witnessed in M. Chassaignac's wards, illustrative of the favourable results that have followed their treatment by "occlusion." As our readers are aware,* this consists in the immediate application of a cuirass of adhesive plaster, which is restrained *in situ* for several days, all surrounding inflammation being kept down by leeching, if necessary. M. Trastour's report is highly favourable; under this plan the wound far more rapidly heals, pain and traumatic fever are much diminished, as is the chance of the occurrence of nervous delirium, tetanus, erysipelas, and purulent infection. In cases in which it may be doubtful whether amputation will be required, it enables us to wait with safety for the decision; and brings these traumatic cases in nearer relation to the cases in which amputation is performed for disease, and in which it results are so much more satisfactory. Since he has adopted this practice, M. Chassaignac never amputates for traumatic injuries of the fingers, however violent the injury may have been. Even when re-union does not take place, very much longer and better stumps result from leaving the case to nature.

Fractures of the lower end of the Radius.—M. Robert [L'Union Mèdicale, 1853, Nos. 6 & 7] observes that the various plans of treating fracture of the lower end of the Radius with antero-posterior displacement, are either insufficient when the amount of pressure is slight, or dangerous when it is great, in consequence of the possible inflammation and gangrene of the skin of the wrist. Moreover, long-continued pressure gives rise to slow inflammatory action and adhesion among the synovial sheaths, leading to stiffness of the wrists and fingers, and an inability to employ the limb for many months after

* British and Foreign Medico-Chirurgical Review, vol. v. p. 262.

the accident. The following is M. Robert's own plan of treatment:—The fracture need not be reduced; but the patient, lying in bed, stretches out his arm horizontally and parallel to the trunk, and puts it on its palmar surface, upon a pad well filled with oat-chaff. This cushion terminates by a thick lower edge, which corresponds to three fingers' breadth above the palmar fold —ie, a little above the fracture, so that the hand hangs pendant over this border, the fingers being prevented coming in contract with the bed. When inflammation exists, leeches, fomentations, &c., are applied to the exposed part. At the end of the eight or tenth day, the patient may get up, supporting the arm in a sling, and still allowing the hand to hang down; or, indeed, he may be up from the first resting his arm on a table. M. Robert has thus treated from fifty to sixty cases, in all of which rapid consolidation, with absence of deformity, has resulted. The greatest advantage, however, attendant upon the plan, is the freedom of the synovial sheaths from adhesion, so that the patient can at once resume his occupations.

Furunculus.—In the furunculoid epidemic lately prevalent, various means are proposed by different medical men; nitro-hydrochloric acid, alterative-doses of mercury, chlorate of potash, quinine, and iron, are among the chief measures recommended.

Galvanism.—Mr. Springfellow exhibited to the Medico-Chirurgical Society [Lancet, March 5] a new form of galvanic-battery, resembling Pulvermacher's but more portable. For a full description, see report in Lancet.

Heart Disease.—M. Beau (Archiv. Gen., Fev. 1853, p. 181), in a long paper on heart-affections, in which several original and disputable views are advocated, recommends *digitalis*, not as a sedative, but as a *stimulant*, and states that it acts by relieving the imperfect action of the heart, or what he calls the state of *a-systolia*, in many cases of hypertrophy. In a case of this kind the heart beats rapidly, perhaps 120 times per minute, but the pulse is feeble and unequal. After the use of digitalis, the action falls perhaps to 60 per minute; but the beats are much stronger, and are more regular; at the same time the face ceases to be injected, and the dyspnœa diminishes. M. Beau believes that the digitalis *augments* the contractility of the ventricle, and thus produces these effects. He calls digitalis "the cardiac cinchona." He gives 20 centigrammes of the leaves infused in a cupful of water, every morning, on an empty stomach. The diet must be good.

Intestinal Obstructions.—Mr. B. Philips (Lancet, Jan. 1) advises, in intestinal obstruction, that the drastic purgatives, such as croton oil, should not be given at an early period; but one or two full doses of calomel and opium (8 to 10 grs. of calomel to 2 grs. opium) should be first given, and large emollient enemata be thrown up every six or eight hours. If these means fail, Mr. Phillips pushes mercury to salivation, mercurial inunction as well as administration by the mouth, being employed.

Neuralgia.—Cazenave (Rev. Med. Chir. in Med. Times and Gazette, Feb. 15) recommends in facial neuralgia, an ointment composed of chloroform 20 part, prussiate of potash 10 parts, and lard 60 parts; a piece of the size of a walnut to be rubbed over the painful parts. An oiled-silk cap is then to be worn for some hours.

Ovarian Cystis.—M. Duplay (Archiv. Gen., Feb. 1853) relates in a case in which an ovarian tumour was tapped, 16½ litres of fluid drawn off, and 250 grammes of a fluid (composition water, 100 parts; alcohol, 50; iodine, 5; iodide of potassium, 5) were thrown into the cyst, and then made to issue out again through the canula, by pressure; all but 2 grammes were discharged. Some sharp pain and fever followed; but in three days all unfavourable symptoms disappeared, and the patient up to the date of report [exact time not given] had continued well.

Palate, Fissure of.—Dr. J. Mason Warren describes [Amer. Jour. of Med. Sc., Jan. 1853] a new forceps to be used in the operation for cleft palate; the instrument has a double curve, one anterior, the other lateral; and the posterior jaw of the forceps is longer than the other. The instrument has double teeth.

Phlegmon.—Polli [Gaz. Tosc. 1, 1852] confirms the statements of Bellini, as to the abortive cure of sub-inflammatory swellings of the skin and subcutaneous cellular tissue by the application of a few drops of liquor of ammoniæ. In syphilitic bubo this method is also useful.

Puerperal Miasmata.—Dr. Busch, Director of the Berlin Midwifery Clinique [Neue Zeitsch. für Geburtsk., xxxii. p. 313], after remarking upon the great difficulty there exists in keeping a lying-in hospital free from puerperal fever, relates the result of an experiment he tried at Berlin. During February and March, 1851, after an epidemic of influenza, one of puerperal fever prevailed extensively amidst all classes at Berlin, the hospital suffering severely during the latter month. It was evacuated, thoroughly cleaned and ventilated for six weeks, and re-opened in May, when however, every woman admitted became affected soon after delivery. Reflecting upon the influence of hot, dry air in destroying contagious fomites, Dr. Busch had stoves introduced into the wards, and all the bedding, utensils, &., were exposed to a temperature of 150° to 170° Fah. during two days. On patients being re-admitted, no more cases occurred, although the disease still prevailed in Berlin. In December, 1851, four women were seized with the disease in one apartment, one of them dying. A heat of 170° Fah. was resorted to, and no extension of the epidemic occurred. To the time of writing, June, 1852, no recurrence had taken place.

Rheumatism.—Dr. Bennett [Edin. Monthly Journal, Dec. 1852] relates six cases of acute rheumatism, in which *nitrate of potash* was used. In all, the medicine was useful; in two the benefit was very marked.

Sarcenæ Ventriculi.—Dr. Hassall [Med. Times and Gaz., Jan. 29.] relates a severe case in which *sulphite of soda* (as recommended by Dr. Jenner) was used with great benefit; infusion of *quassia* and *bicarbonate of potash* were also employed, and seemed useful.

Dr. Bennett (Edin. Mon. Jour. Feb. 1853, p. 168) refers to a case in which the sulphite of soda was ineffectual.

Scarlatina.—Dr. Gillespie (Edin. Mon. Jour. March, 1853) recommends the repeated use of the warm bath, mercurial laxatives on first admission, afterwards small repeated doses of a diaphoretic, containing antimonial wine, spirit of ether, nitrate and liquor ammoniæ acetatis; and the application of a strong solution of nitrate of silver to the throat.

Scurvy.—Dr. Hammond [Amer. Jour. of Med. Sc., Jan. 1853] recommends strongly, from experience of twelve cases in New Mexico, the use of the *salts of potash*, especially the bitartrate. The remedy was first employed in consequence of Garrod's recommendation.

Stomatitis Ulcerosa,—Dr. Mackenzie recommends sponging with the dilute nitric acid of the Pharmacopœia, and giving internally carbonate of ammonia, and citrate of iron.

Syphilis.—Professor Gamberini [Bulletin delle Sc. Med., xxi. p. 253] confirms the favourable opinion expressed by Dr. Daverts, of the *iodide of sodium*, and believes, from an experience of 116 cases, that it may be advantageously substituted for the iodide of potassium. The dose was at firsl 3j., raised gradually to 3ij. per diem.

[*British & For. Medico-Chir. Review.*]

IV.—*Cold as an Anæsthetic Agent.*

We lately witnessed, says the editor of the Lancet, the employment of cold as an anæsthetic, in St. Bartholomew's Hospital, upon a patient suffering from chronic inflammation and enlargement of the knee joint.—A bag of pounded ice was applied for about four minutes, by Mr. Lawrence, over the swollen parts; then, with a heated knife, six or more incisions were made by the side of the patella, extending through the skin and subcutaneous tissue. The patient suffered *no pain whatever*. The counter-irritant effected a marked diminution in the swelling which was of a character sufficiently grave to suggest the idea of the possibile necessity of amputation of the limb

V.—*On the Cause of Cramps and Spasms in Epidemic Cholera.*

BY P. J. MURPHY, M. D.

Cause.—The immediate cause of local cramp would appear to be *a languid circulation in the veins which traverse the substance of a muscle.* This theory will explain why cramps attack most frequently the voluntary muscles furthest from the organ of circulation—the feet and legs; why they are more common in cold weather; why the principal sufferers are females and especially those of weak pulse, pale countenance, and chilly surface; and therefore why they are so seldom absent in chlorosis. It will also explain why the seizure is most usual in the horizontal position, when muscular action, so favorable to the circulation, has ceased. Nearly one-half of the young, growing females, with a tendency to chlorosis, suffer from this annoying complaint; and it is remarkable, that although we have many cases where the cramps are limited to the muscles below the knee. No inquiry being made about this symptom it is seldom mentioned to the physician, and yet it is almost pathognomonic of an enfeebled action of heart, from which the anemic head-ache springs. When this affection is complained of, whether in the aged or the young, no matter of what sex, the necessity for chalybeates is indicated. Dr. Brady makes one proper exception—in cases of pregnant females, but the cause of their languid circulation is mechanical, and therefore, not remediable by medicine. It may very naturally be asked, if a feeble and delicate constitution predisposes to cramp, why it is not an attendant on the last stages of diabetes, phthisis, and other exhausting diseases? Because those diseases produce hectic fever, and hectic is accompanied with a pulse so rapid as to be incompatible with a slow, venus circulation. Nor does it supervene in debility after profuse hæmorrhage; the rapid pulse of reaction and the half empty vessels affording a satisfactory explanation of its absence. The severest cramps we witness from arrest of circulation in the veins of muscles, are those which add so much to the horrors of epidemic cholera, for these cramps, although general, in an exact sense must be considered local, no muscle contracting until the blood in its own veins is arrested. It is an error to attribute the cramps of epidemic cholera to a poisonous quality of the disease, and the recollection that our own autumnal cholera is accompanied with similar cramps, should disabuse us of the mistake.

In neither form of cholera do cramps supervene until the venous circulation through the muscles is almost impeded from the density of the blood, a natural consequence of the exudation of its serous portion. No person can doubt that in epidemic cholera the circulation is from this cause almost arrested; for on puncturing a vein, the blood trickles down slowly, thick and dark as tar. Fatal cases of Asiatic cholera, it is true, are recorded, where few or no evacuations took place, yet cramps, although not violent, were noticed. Such cases might favor the opinion that the cramps depended on the inhalation of a specific poison, did not the post-mortem examinations discover the exuded serum still in the intestinal tube, consequently the cramps were present, but less severe than where the exuded serum was too abundant to be retained in the intestines.

That there are poisons capable of coagulating the blood in its vessels, and that coagulation is attended with muscular contractions, cannot be denied. Dr. Pereira, in his last edition of "Elements of Materia Medica," quotes the *Journal des Chimie Médicale*, to show the physiological effects of bromide of potassium. Thirteen grains, dissolved in water and injected into the jugular vein of a dog, coajulated the blood, and caused convulsions and death in a few minutes. But the same medicine taken into the stomach in larger doses produced vomiting only. As a clinical fact in English cholera, it is interesting to observe how much sooner the cramps cease when the stomach does not reject fluids; for the veins are soon refilled, and thus the portion between the crassamentum and serum is quickly restored. The so-called secondary fever of Asiatic cholera arises solely from congestion of the venous system generally, and this congestion is peculiar, the fault existing in the blood itself being too thick, from having parted with its serum, to be influenced by the action of the heart. Rigors, or rapid involuntary contractions of muscles, are seen after exposure to cold, at the commencement of most fevers, and in the first stage of ague; for the first stage of these diseases is characterized from the superficial to the deep-seated veins, and these rigors or contractions are salutary; for by compressing the deep veins, the blood is restored to the superficial vessels, causing the phenomena of reaction. In fevers there can be nothing more alarming than the absence or prolonged delay of rigors. When scarlatina threatens to be unfavorable within twenty-four or thirty-six hours from its invasion, there are no rigors, for the muscles seem to have lost this salutary contractile power. A cramp may therefore be regarded as a painful but useful action of a muscle to assist in the circulation of venous blood.

VI.—*Microscopical Constituents of Black Vomit.*

As the yellow fever is now prevailing in this city, decimating the poorer and badly-lodged portion of our population, particularly in certain localities, we deem it our duty to bring before the profession every fact tending to elucidate this peculiar disease. To this end, we propose to condense from some of our recent American exchanges, some observations made on the black vomit with the microscope, by Dr. Black, of New York; Middleton Mitchell, of Charleston; and Hassall, of London.

After describing the color and usual physical appearances of black vomit, these gentlemen discovered under the microscope the epithelium of the stomach and its crypts, in three different forms; the conoidal cells of the lining membranes; a few cells from the larger sections of the crypts, and flat cells of

smaller size, from the diseased and minute divisions of the crypts. Those bodies were seen sometimes in sheets of small extent, again as individual cells, many were granular—some contained oil-globules. They detected the following substances foreign to the stomach—blood-corpuscles, which were invariably of a dark yellowish-brown. and in great abundance, forming about *one-fifth* of the entire mass. They were either disk-shaped or globular—grouped or isolated. "Numerous scales of opaque black matter—destitute of any uniform shape, and susceptible of fracture in any direction." They regard this as produced by some chemical change in the coloring elements of the blood. In every instance, the black vomit was found to be decidedly acid in its reaction. Some of the matter obtained from the coffee-ground sediment, presented under the microscope the appearance of coagulated blood, tinged with hæmatin. They also found epithelial cells in every specimen examined. Traces of the mucous lining of the œsophagus, pharynx, fauces and mouth were detected, produced by the acid matter vomited. An inspissated form of mucous may always be found mingled with black vomit. Blood, in an altered and broken condition, is invariably met with in black vomit.

This is indeed an essential ingredient of the true black vomit; the blood-corpuscles are entirely separated from each other, they are never seen in rolls or piles; few disks are seen in it revolving edgewise in the field of the microscope. The blood globules experience precisely the same change as when treated with acetic, nitric or hydrochloric acid.

No animalculæ are found either in fresh or putrescent black vomit.

Note by the Editor.—Black vomit possesses a saline, bitter taste, and a fresh, disagreeable, nauseous smell. It can be readily made by mixing venous blood with muriatic acid and mucilage, diluted with water. This mixture, so nearly resembes the genuine black vomit, that we have seen the most experienced physicians confound one with the other.

(*Ed. N. O. Med. and Surg. Jour.*)

VII.—*Poisoning by Strychnia Cured by Chloroform.*

In the *Medical Examiner*, for June of 1853, Dr. Upsher, of the Marine Hospital, at Norfolk, reports a case of poisoning by strychnia, in which the inhalation of chloroform completely arrested the violent tetanic spasms produced by the poison. The little patient was a child about four months's old, and was attacked with diarrhœa; for which the Doctor ordered, after ineffectual trials with other remedies, 1-30th of a grain of strychnia three times daily. The first dose excited violent tetanic spasms, which continued to augment in force and frequency, until they were arrested by the inhalation of a fresh drop of chloroform. Without the timely influence of this anæsthetic, Dr. U. is convinced that the case must have terminated fatally. A short time prior to

this, 1-20th of a grain of strychnia had been given to the same child, with no other perceptible effect than to arrest a diarrhœa, for which it was ordered; but the disease returning, the same medicine was again given in 1-30th of a grain, and it proved decidedly poisonous, as we have seen in this instance.

(*Ed. N. O. Med. and Surg. Jour.*)

VIII.—*Medical Fees in San Francisco, California.*

The *Gazette Medical de Paris* reports the following as the fee-bill adopted by the physicians of San Francisco, California. If the subjoined items be correct, the medical faculty of *El derado* is looking up.

For consultation and visit	$32.
For each visit	12.
For one consultation	16.
For an extraordinary visit and detention over one hour	32.
For a night's consultation	100.
For a consultation out of town	per mile, 10.
For a medico-legal opinion, written,	150.
For a declaration of opinion before a Judge	200.
For an accouchment	3000.
For a mortuary certificate	100.
For the operation of lithotomy	1000.
For introducing catheter	32.
For dilatation of stricture of uretha	500.
For exploring the anus or bladder, from	50 to 100
For amputation of the finger	100.
For trepaning	4000.

IX.—*Iodine Injections in the Treatment of Dysentery.*

M. Delioux has obtained remarkable success in the treatment of dysentery by injections of iodine. The first effect of these injections is to increase for a time, the alvine dejections; but they speedily diminish afterwards and rapidly change in their appearance, and soon cease entirely. They have never been known to produce any bad effects when used after the following formula:

℞ Tinct. Iodine, 10 to 30 grammes.
Iodide potass. 1 to 2 " Mix.

An emollient enema should be ordered before the iodine injections are given in order to perfectly empty the intestines. Seldom are the patients enabled to retain the injections beyond fifteen or thirty minutes; yet they have time to make a decided impression upon the parts affected, and have been found far preferable to the argentine injections.

(*Revue Medico-Chirurg.* 1853.)

X.—*Imperial Academy of Medicine, Paris.*

At a recent sitting of the Academy, the following distinguished strangers were elected *Membres Associes*, in the order of their names :

Valentine Mott, of New York ;
M. Buffalini, of Florence ;
Warren, of Boston ;
Ribieri, of Turin ;
Vleminckx, of Brussells ;
Grandet, of Lisbon ;

This is a high compliment to American surgeons—two out of six from a number of the most eminent medical men known to the scientific world, being chosen Membres Associès to one of the most learned and scrupulous bodies of savans in the world.

(*Medico-Chirurgical.* 1853.)

XI.—*Aneurism Cured by Injections of the Perchloride of Iron.*

Mr. Pravas was the first to suggest injections of the perchloride of iron into the sack, to cure aneurism. M. Lallemand, imitating this practice, has reported the result in the June No. (1853) of the *Revue Therapeutique.*

The clot says Mr. L. was promptly hardened under the influence of the injection ; and the pulsations in the tumor, (situated at the elbow) ceased when the compression on the artery was removed ; the arteries in the fore-arm continued to beat sometime afterwards. Considerable inflammation was set up in the walls of the sack, which finally ended in fluctuation, and was punctured. The wound readily healed after the matter was discharged, and no hemorrhage followed.

M. Lallemand contends that the treatment of aneurism by injection of perchloride of iron is as certain to cure as the ligature—it is much more easy of application in a large majority of cases—such as aneurism of the crural arteries; of the iliac—of the axillary, and of the subclavian.

"I am well satisfied," says M. Lallemand, "that injecting aneurisms will cause, in the treatment of these affections, a revolution as complete—as essential as that of lithority in calculus diseases."

XII.—*On the Recorded Deaths from Chloroform.*

Before the London Medical Society, Mr. Crisp recently laid some very interesting statistics on the fatal effects of chloroform. He had been enabled to collect from the publications of the day about 42 deaths, brought about directly by the use of this anæsthetic in surgical practice. Mr. C. concludes these

curious statistics in the following words, which we copy from the July No. (1853) of the London Lancet.—Ed.

"The time has gone by when special pleading will long avail on this or any other subject connected with medicine, and if inductive reasoning is to be allowed in such inquiries, I think that I have obtained a sufficient amount of evidence to bare out the following conclusions: 1st, That, judging from the experiments that I have made upon the lower animals with chloroform, this agent will occasionally, and without apparent cause, produce sudden death, and that it cannot be administered to the human subject under any circumstances without some amount of danger, seeing that the fatal cases have generally occurred in young subjects free from structural disease. 2d, That, taking into account the ages of the patients, five of which are over forty, and twenty under thirty years of age, and coupling this fact with the circumstance that fatty degeneration of the heart is comparatively unfrequent in the young, it is a fair deduction, that this condition of the heart, although detected in a few cases (three,) had but little, if anything, to do with the cause of death. 3d, That chloroform acts directly and especially upon the nerves of the heart. 4th, That its administration during parturition is attended with less immediate danger, in consequence of the peculiar excitability of the cardiac nerves, but especially on account of the increase of the conservative power of the system, which often, at this crisis, under the most disadvantageous circumstances, enables nature to accomplish the great end. 5th, That the repetition of the dose, or its long continuance, has more influence, as regards the fatal result, than the mode of administration or the quality of the drug. 6th, That the deduction of the most practical importance is the fact that this agent is cumulative, that its fatal effects in many instances have not been exhibited until the administration of a second dose, and when the influence of the first was unapparent."

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*The Action of Medicines in the System, &c.*, By Fredrick William Headland, B.A., M.R.C.S, etc., 1853. Being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal, for 1852.

Candidly, we believe our knowledge of the "Action of Medicine" is extremely limited; and we therefore welcome every effort to illucidate this obscure subject with real pleasure. The action of certain medicines are plain and manifest to all—even the uninstructed. Every one knows, for example, that gamboge and scammony will purge,—that tartar emetic and ipecac will vomit, but in what way, or by what peculiar influence these respective results are brought about in the living organized tissues, is a matter of profound mystery, and has, up to this time, we believe eluded our investigations.

There has not been wanting many and ingenious theories on these subjects, but after all, no two have agreed on this mysterious point in medical science. We all admit that calomel, in large doses, will act as a cathartic; whereas, by diminishing the dose, and repeating it for several days, a profound impression is made upon the organism—the glan-

dular system becomes stimulated, to use the language of the day, in consequence of which, the secretions therefrom, are sensibly augmented, and as a necessary result, the pulse becomes accelerated, the skin warm, and the entire system takes an artificial febrile state. Now this is all a mystery to the best informed in the profession—for do not brandy, wine and other stimulants act with equal if not greater force upon the entire system, the glandular included, and yet we witness no such effects upon this part of the system by these stimulants as that effected by repeated small doses of calomel. Hence, it is reasonable to infer that something more than a stimulant effect is produced in particular organs by the protracted use of mercury.

What is this peculiar action on the system and how is it brought about? That mercury reaches the organs through the blood, is a physiological certainty, but in what special manner it acts on the circulating fluids so as to determine results and give rise to products essentially different from nearly all other medicines, remains a *questio vexata*,—an undetermined point in therapeutics. Until these and similar points are determined, we think books, aiming merely to classify medicines, according as they may seem to act upon one or more organs will be found useless lumber in our libraries; few indeed approaching in merit and accuracy to Cullen's or Murray's Materia Medica.

We now propose to examine the labors of our author and see if he has really added anything to our present very limited knowledge of the "Action of Medicines." After passing in review the different classifications adopted by his predecessors he proceeds under Chap. II, to consider the "*General Modes of Action of Therapeutic Agents introduced into the Stomach.*"

He discusses this subject under ten propositions; in the *first* of which he investigates the general conduct of medicines after their introduction into the stomach and before their passage into the blood. Six propositions of the ten treat of the subsequent behavior of these medicines which pass into the blood and fluids of the body,—of these, the *fifth* specifies their general course. The *sixth* states that they may experience certain changes in the system. The concluding four treat of the various modes in which these agents may operate in the cure of disease.

Proof 1st.—*States that the great majority of medicines must obtain entry into the blood, or internal fluids of the body before their acum can be manifested.*

Dr. Headland thus denies that the mere contact of a medicine with the stomach is sufficient for the production of its peculiar action, except in

a few instances. They must enter the internal fluids, either directly or indirectly, and through these, whether blood, chyle, or serous fluid, reach the various organs and tissues of the body. He denies that medicines act by mere contact with the stomach—their influence being propagated to distant parts by means of the nerves,—they act, he contends, *through the medium of the blood and fluids.*

The fact that medicine introduced into the system elsewhere will act in the same way as when thrown into the stomach, proves, at all events, thinks Dr. H., that contact with the mucous surface is not an essential requisite for the operation of a remedy. This may be demonstrated by injecting medicines directly in the veins, or even into the cellular tissues, when their peculiar action will be speedily manifested.

Experiments by Brodie, Magendie, and Elsbert, on dogs and other animals, proves that the continuity of nerve is not necessary for the propagation of the action of medicines, such as poisons and the like; whereas vascular connection is absolutely requisite for conveying the specific effects of medicines from one organ to another.

The speedy manner in which some medicinal agents affect the system does not militate against the supposition of vascular connection—because, by experiments, it has been proven that the "circulation of the blood is sufficiently quick to account for the action of the most rapid poisons upon the nervous centres."

Another argument in favor of the absorption and action of medicines through the blood, may be based upon the well-known fact that they may be readily detected in the blood and secretions of the body.

Proof 2d.—Declares that the gastric juice and other intestinal secretions are capable of dissolving the large majority of medicines, and that then by a process called absorption, they enter the circulation and act upon the organ of the body according to their specific peculiarities.

Here our author enters into an explanation according to the observations of Spallanzini and Reamur, of Tiedment and Ginetin, of Liebig and others, of the process of digestion—a subject no doubt quite familiar to the readers, and therefore unnecessary to examine at this time.

We copy the following observations of our author on the absorbing power of the stomach:

"That the stomach is absorbent may be proved by the experiment of placing a ligature round the intestine of a dog, just below the pylorus. It is then found that soluble substances placed in the stomach pass rapidly from it into the circulation. It is probable that all substances which are easily dissolved pass through the coats of the stomach. That the surface of the intestines is absorbent may be proved by the dissappearance of enemata thrown into them. Liebig states that a solution of common salt, in the proportion of one part to

eighty of water, dissappeared so completely in the rectum that an evacuation one hour afterwards was found to contain no more than the usual proportion of salt.—*Animal Chemistry, p.* 77.

"On the mucous surface of the small intestines are a number of small projections, called Villi. Within these are the origins of the Lacteals, a peculiar set of Lymphatic vessels, which are engaged in the absorption of the chyle. This chyle is a thick fluid which is formed by the meeting of the Bile and Pancreatic juice, with that part of the digested food which passes into the Duodenum. It is generally white, from the presence of fat. Now the lacteal vessels are no doubt absorbent, but are they ever engaged in the absorption of medicinal solutions? It seems that they are not in ordinary cases at all concerned in this; for three chief reasons. In the first place it appears from the researches of Bernard and others that the lacteal system is a special arrangement for the absorption of fatty substances, and that other matters, such as albuminous compounds, pass generally into the veins, and thence to the liver. Besides, it seems that these lacteal absorbents are only in action during the digestion of food, when the epithelium on the surface of each villus becomes loosened, in order to allow to the chyle an easier access to the lacteal within it.* So that it is likely that a small portion of a fluid or soluble substance would be insufficient to rouse them to action. And, in the third place, direct experiments of a decisive kind have been made on this point. Magendie has found the ligature of the lacteal trunks does not prevent the occurrence of poisoning from agents introduced into the bowels. And Tiedmann and Gmelin have carefully sought in the chyle for a number of different medicines administered to animals in their food, and have been unable to detect any of them there. So that, with the exception perhaps of fats and fixed oils, we may reasonably conclude that no medicinal substances pass into the system through the lacteals, but that all are absorbed by the veins or capillary vessels.

It seems probable that the Bile and Pancreatic juice may be engaged in reducing to a soluble state certain medicines that are insoluble in the Gastric secretion, and may thus procure the absorption of these substances by the veins of the intestinal canal.

"Having briefly considered the secretions which meet the medicine on its first entrance into the system, we are next to inquire into the manner in which this medicine gains admission into the blood. In the first place, it must be in a fluid state, or it cannot be absorbed at all. It will be most convenient to consider afterwards how different remedies are to be reduced to this condition.

"Now the force or process by which fluids are enabled to pass and repass through an animal membrane, has been named by Dutrochet *Endosmose* and *Exosmose*, according as the current tends inwards or outwards."

Many of the phenomena attending the processes of absorption and secretion can be explained on the two forces, designated by Dutrochet—endosmosis and exesmoses. The density of the liquids, their attraction for the intervening membranes, and the affinity of the two separated fluids for each other, all operate powerfully in facilitating or retarding the process of exosmose and endosmose. Dr. Headland

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*Todd and Bowman's Physiology, vol. ii. p. 230.

* *Vide* Pereira's Materia Medica, vol. i. pp. 101, 106.

lays down the folowing classes, which he believes undergoes solution and absorption in the intestinal canal:

1. Mineral substances soluble in water.
2. " " " " acids.
3. " " " " alkalies.
4. Vegetable products " " water.
5. Animal and vegetable substances rendered soluble by the gastric juice.
6. Fatty and resinous substances soluble in alkali.

The process of absorption is lessened by pressure on one side and motion on the other. Here some remarks are made on the *endosmotic theory of the action of saline purgatives* from which we deduce the inference that Dr. H. believes that salines are, in all cases, absorbed, and "that whether they are subsequently excreted through the kidneys or by the bowels, depends more on the quantity administered than on the degree of dilution of the dose." To prove his position he instituted a series of experiments which, according to his report, are conclusive in favor of his theory. We cannot notice them for want of space.

The third proposition asserts that medicines which are insoluble in water and the gastric juice, cannot gain access to the circulation Even fatty matter does not constitute any exception to this assertion—for this is soluble in bile. It must be conceded that few medicines come under this proposition; and the author instances as examples, charcoal, woody-fibre, and bismuth. By some it has been insisted that certain insoluble substances can be made to enter the circulation; and among those who advocate this view may be mentioned Prof. Oesterben of Dopart, who imagined he had established his position by experiments with charcoal on rabits. To set this matter at rest our author instituted the following satisfactory experiments, to which we invite the special attention of the reader:

"Exp. 1.—Ten grains of calomel were given to a large dog. It was killed after three hours, allowing this time for digestion. A considerable quantity of blood was collected from the Portal vein, and submitted to analysis to determine whether it contained any compound of Mercury in an insoluble form. The blood was dried and pulverized. The result was boiled for some time in water, and the insoluble part collected. It was dissolved in a small quantity of acqua regia, and the clear acid solution placed in a test tube. A slip of zinc foil was folded round a narrow plate made of gold foil, and introduced into the solution. A galvanic current being thus set up, the minutest quantity of mercury, if present, would have been deposited on the gold, so as to tarnish it. But this did not take place, and when at last the zinc was completely dissolved, the gold remained as bright as before. Thus there was *no* Calomel, or compound of mercury, present in the insoluble form.

"Exp. 2.—Ten grains of strong mercurial ointment (containing half its weight of metallic mercury, with some oxide) were given to another dog. He was killed at the same time, and the Portal blood analyzed carefully in the same way, but here also *no* mercurial compound was present in the insoluble form.

Exp. 3.—To a third dog five grains of Oxide of Silver were administered. After three hours he was killed. The Portal blood was dried in a water-bath, and reduced to powder. This was boiled for some time in water, which was separated by filtration. Aqua regia was then boiled on the insoluble part. This would convert any silver into chloride. The acid was evaporated off as much as possible, and the solid remainder heated in a small porcelain crucible to dull redness. The result was powdered, and digested in liquor ammoniæ. It was filtered, and excess of nitric acid was added. There was not any precipitate. Had chloride of silver been present, it would have been dissolved by the ammonia, and precipitated by the acid. Thus no insoluble silver compound was contained in the blood analyzed.

"Exp. 4.—Ten grains of sulphur were administered in the same way to a fourth dog. On killing it and opening the body, the thoracic duct was found to be full. A considerable quantity of chyle was collected from it. Now, as it is asserted by some, that fat passes undissolved into the chyle, and as I believe that sulphur is digested in the neighborhood of the bile duct, this chyle was chosen for analysis in preference to the blood, as more likely to contain any insoluble sulphur. Besides, the blood would be less satisfactory on account of the large quantity of albumen and fibrine contained in it, both of them also containing sulphur. The insoluble part of the chyle was obtained in the same manner as with the blood. It was then boiled in a small quantity of a weak solution of caustic potash. By this any free sulphur would be converted into a soluble sulphate of Potassium. The solution was filtered, and a few drops of a solution of the Nitro-prusside of Potassium added. (This is a salt lately discovered by Dr. Playfair. It is a delicate test for soluble sulphurets, with which it strikes a deep color.) No change was produced. Therefore no insoluble sulphur was present in the chyle."

Proof 4th.—Maintains that "*some few remedial agents act locally on the mucous surface, either before absorption or without being absorbed at all.*" He classes these as—Irritant emetics, stomach anæsthetics and irritant cathartics. This proposition would seem to be at variance with, and contradictory of the third, to which we have just referred. The author, however, admits that a local impression may be made upon the mucous membrane and so modify the nervous system as to extend its influence to organs situated remotely from the original surface, upon which an impression may have been made.

This *will* bring us back to the old doctrine of sympathy, through which many of our therapeutic agents have been supposed to act. The late distinguished Dr. Chapman, of Philadelphia, was an eloquent and able advocate of the "sympathetic doctrines," and stoutly defended it, we believe, to the day of his death. Verily, we must resort to this theory to explain the modus operandi of many of our medicinal agents.

Proof 5th. Declares that the medicine, when in the blood, must permeate the mass of the circulation, so far as may be required to reach the parts in which it tends to act. That there are two possible excep-

tions to this rule: the production of sensation or pain at a distant point, and the production of muscular contraction at a distant point. This proposition is fully examined and illustrated; but we must hasten to a close.

The 6th proposition asserts: That while in the blood, the medicine may undergo changes which in some cases may, in others may not affect its influence. That these changes may be—

1st. Of Combination.

2d. Of Reconstruction,

3d. Of Decomposition.

Below we give some example of the foregoing changes.

"*Changes of Combination.*—The chief, and indeed almost the only way of detecting changes in medicines, is by taking notice of their effect upon the composition of the secretions.

"Now in the blood we have a slight excess of alkali; in the urine, an excess of acid. Acids and alkalies are often given as medicines; and as it is not right that there should be much excess of either in the blood, both are generally soon neutralized in the system, and reduced to salts. This may sometime occur before absorption, but perhaps more often after it. Now, it may seem strange for me to say that this neutralization does not destroy their influence. But it evidently does not; for in being neutralized they diminish in the blood, and in the system generally, the quantity of basic or of acid matter, and thus tend to alter the reaction of the secretions. Though an acid may combine in the blood with Soda, or with salts of Soda, yet by so doing it causes an excess of some other acid, probably an animal acid, which, being set free, acts on the secretion of the urine much in the same way that the first acid would have done. It is easier to render the urine alkaline than to make it acid, for two reasons; first, as I shall show hereafter, an acid may pass out through other glands besides the kidneys; and secondly, an alkali is not so easily neutralized, either before or after absorption, the blood being already more or less alkaline.

"A large quantity of acid would easily overcome the feeble reaction of the blood, and thus, by remaining free, Sulphuric and other acids are enabled to act as astringents on certain of the glands.

"Other changes of combination have already been shown to take place during the process of absorption. Alkalies and their carbonates are more or less neutralized by the stomach acid. Substances soluble in alkalies are probably absorbed in such solution. Calomel, Chalk, Magnesia, and metallic oxides, as well as other insoluble medicines, are taken up in the soluble form, in which only they are capable of acting. But we are now concerned with medicines in the blood. And here I must protest against the idea that all chemical affinities have free play in that liquid. If it were so, many of our most valuable medicines would be decomposed and rendered insoluble there. All the mineral salts would be precipitated by the free Soda in that fluid. Acetate of lead would be immediately decomposed by sulphates, and Nitrate of Silver precipitated by chlorides. It is probable that the vital forces, as well as the viscidity of the plasma, exert a retarding or controlling power over such chemical tendencies.

Some substances are decomposed while passing out of the body. Decomposing matters in the intestines and in the saliva, cause the formation of Sulphuret of Iron, while chalybeates are taken, which blackens the fæces; and

of Sulphuret of Lead, when lead is taken for some time, producing the well known blue line on the gum.

"*Changes of Reconstruction.*—The elements of a body may be disturbed in the system, and combined together anew, without any material or apparent alteration of its properties. Probably many changes of this kind occur, but only some isolated instances have been verified. That Tannic acid, acquiring Oxygen, changes into Gallic. Benzoic and Cinnamic acids are converted into Hippuric acid, which passes out in the urine. Turpentine changes into a volatile oil, which communicates to the urine the odour of violets. Ferridcyanide of Potassium changes in the system into Ferrocyanide. Some of these changes will be afterwards considered more at length.

"*Changes of Decomposition.*—By this I mean such a disarrangement of elements as shall neutralize or reverse the action of a medicine.

"There is free Oxygen in the blood, and the most important change to which all organic substances are liable there is *oxidation.*

"This probably occurs in many cases. It always takes place with the starchy elements of the food, and with those parts of the nitrogen tissues that have done their work, and are preparing to be excreted from the body.

"By this oxidation Wohler has proved that the alkaline salts with vegetable acids are changed in the blood into alkaline carbonates. From being first neutral, they become now alkaline in their reaction, and affect the urine in the same way as free alkalies."

One class of medicines is called by the author *Hæmatics*, which act upon and through the blood, as the term indicates; another is called *Neurotics*, which, passing into the circulation, act upon the nervous system; the third class is denominated *Astringents*, because they are passed through the blood on the muscular fibre, which they cause to contract.

The fourth is ranked as *Eliminatives*, because they act by passing out of the blood through the glands, which they excite to the performance of their functions.

The last chapter, which closes this useful volume, treats of the "action of some of the more important medicines in particular, such as cod-liver oil, potash, quinine, iron, antimony, mecury, colchicum, arsenic, ammonia, strychnia, alcohol, chloroform, opium, iodine, hydrocyanic acid, a comite, digitalis, tannic acid," etc.

As our author's views on the action of quinine may be just now interesting, to at least some of our readers, we quote some of his remarks on this valuable article.

"It appears, from the character and results of its medicinal influence, that it is exerted primarily in the blood, and not on the nerves. It is included in the Restoratives group of Hæmatics, and the general results of its action differ widely from those of a Catalytic Hæmatic. It produces no marked effect upon the system in health. Its operation consists in the cure of general debility, however produced, and in the prevention of periodic disorders in the blood. Debility depends on a want in the blood, and not on any active morbid process; and there are circumstances which render it likely that Ague may be curable by the supply of a similar want.

"Quinine is also serviceable in Gout, Scrofula, Dyspepsia, and other disorders; in all of which other medicines, which stimulate the secretion of the bile, are more or less applicable. Torpidity of the liver is likewise a usual accompaniment of the various forms of debility, and occurs in intermittent, remittent, typhoid, and yellow fevers; in each of which this medicine has been recommended, and used with advantage. In fact it may be said, that in all diseases in which Quinine is used there is a failure in the secretion of the bile; and in all diseases in which there is a failure in the secretion of bile, Quinine is serviceable.

"There appears then to be some connexion between these two things. Certain of the constituents of the bile are formed by the liver out of the blood, for the purpose, apparently, of being again absorbed at some part of the surface of the intestinal canal. One of these, Taurine, has been shown to be chemically analogous to Quinine. Thus it seems to me to be not improbable that this alkaloid may be of service in these disorders by supplying the place in the blood of this biliary matter, which for some reason may be needful in the animal economy, or that it may actually become changed into the latter while in the system. Were this proved, its restorative action would be effectually cleared up."

From the foregoing, it is evident that Dr. Headland is but little acquainted with the sedative effects of large doses of quinine; for he forbids its use in febrile states of the system, unless combined with ipecac or antimony. Steel, 60 Camp street, has copies for sale.

II.—*A Treatise on General Pathology.* By J. Henle, Professor of Anatomy and Physiology, in Heidelberg. Translated from the German, by H. C. Preston, M.D. 1853.

This is a remarkable book, from its inception to its close; remarkable, not only on acconnt of the boldness with which the author proclaims his views; but, also, for the depth of thought and extent of research which stamps almost every line written in the volume. The Science of Medicine, says M. Henle, in his preface, "is now aroused to such a degree of self-consciousness, that she has no superiority over other empirical knowledge; that she can not take a step in advance which she has not first marked out by an hypothesis. And this is well, he adds; for the day of the last hypothesis would be also the day of the last observation. For what other purpose do we collect experience than to support an opinion at the time not established? "Who takes the trouble to ascertain whether the sun rises in the East, or whether water runs down hill?"

The foregoing may serve as a specimen of our author's way of expressing his thoughts; many of which, we fee! certain, will be found novel to many who may read them. We shall glance at some of them,

giving occasional extracts by way of communicating some of the doctrines and views entertained by one of the most erudite and profound pathologist of the present day. "The duty of the physician is to prevent and to cure diseases." This is the first declaration made in the body of the work, and at once proclaims the high aims with which the writer entered upon his task.

In his introductory he examines, in a masterly manner, the various systems of medicines, which alternately swayed the public mind in the different ages of the world; after which he aims to grapple with "*Special Pathology.*"

He arranges Medical Science, abstracted from the division of diseases into internal and external, under three groups, as follows: the *first* embraces what we actually know of disease and of its different forms;—the *second*, comprehends the doctrine of the remedies; the *third*, the rules concerning the administration of these remedies. Under the first group, is embraced Pathology; under the second, Materia Medica; and under the third, Therapeia. M. Henle then proceeds to define the exact boundaries, as near as may be, of these three special departments of medical science; but as this part of his subject is a little prolix, we barely allude to it, and proceed to the "*Contents and Divisions of Rational Pathology,*" which he devides into a general and special part. The first he divides into four sections:

1st. The inquiry into the idea and nature of disease.

2d. The doctrine of the causes of disease in general, or general etiology.

3d. The local relation of disease; the condition of its propagation in the organism; the manner of its transition from one organ to another.

4th. The relations of disease in regard to time, or the general history of disease, its course, duration, and termination.

"If," says M. Henle, "there was but one morbific influence, still we could see as much difference in the specific forms of disease, as of organs: if there was only one organ, still we should have as many species of disease, as different efficient causes. We can, first, proceed from causes, to group the results of determined external potencies, and after that, endeavor to ascertain their influence upon the organic substances in general, then upon single organs and structures; this is the province of special etiology. We can, secondly, proceed from the organs, to concentrate in one totality the sum of the symptoms which appear in the compass of a morbid affection of each organ, in order, after having learned its whole possible mode of reaction, to pass on to the question, under what circumstances the one or the other would actually appear. Symptomatology furnishes the representation of the group of symptoms of each organ: this may be called special, in opposition to a doctrine contained in the third section of the general part, concerning the value and relations of symptoms in general: we must also distinguish it from Semei

ology, which treats of symptoms as the signs of an empirical-pathological species of disease, and therefore only in relation to their position in an empirically determined complex,"

To this succeeds a rather long but very interesting historical account of medicine, from the epoch of Pythagoras down to Mulden, Muller and others, of this our own enlightened times.

This closes the first division of the volume; then we enter upon the "*Definition and Nature of Disease*," which abounds in apt illustrations, profound thought, and a felicity of expression peculiar to the German philosophers. He contrasts the normal with the abnormal.

"The ideas healthy and sick correspond to the ideas normal and abnormal, in so far as the healthy is always also the normal, the sick the abnormal, even though it is not customary to call everything normal, healthy, and everything abnormal sick. Normality and abnormality include, therefore, as the more important, health and disease. It remains to be demonstrated under what restrictions the former characters are transferred to the latter. A crystal which deviates from normality is not called sick; an anomaly which is inborn and continues in like manner during life, is not considered in the rule as a morbid condition; even a deformity appearing during life, a scar, the loss of a limb, etc., as soon as they become permanent, are no more disease. Lastly, identical with disease is the expression, morbid process. All this teaches, that under the term disease only those abnormal events are understood which recognise a change, an advance, a motion; the stationary are abnormalities in a narrower sense, defects, *vitia*. As with the predicate, health, so with the subject disease, it is only applicable upon conditions which are capable of an abnormal motion and development. A crystal cannot be healthy, but only normal. Because health is a motion and a process, so it includes the possibility to pass over into disease; what cannot become sick is not healthy."

M. Henle defines, after other writers, disease, to consist in the *deviation from the normal, typical*, that is, *healthy vital process*." This, says he, is not a mere *negation*, a non-existence; but *another existence*; a deviation from the type according to which organic beings develop themselves. By type is meant that law which determines the form and outline of an organ; and any material deviation from this type is but little less than a miracle.

As it is almost impossible to convey our author's ideas except in his own language, we again give him on typical force.

"The typical force, in the great universe, in which it attains its development, is limited by other forces, being exalted and depressed like any other force which is bound to matter. The force of gravity antagonizes elasticity and cohesion; the cohesive force, gravity. Thus the typical form of the crystal is destroyed by pressure, heat, and chemical attraction (solution.) and undergoes the most various modifications, according as it is obtained by a rapid or slow process; by sublimation or precipitation; from this or that menstruum; by the presence of this or that matter. In the same manner, and not otherwise, is the organic individual in conflict with other crude and subtle, inorganic, and typical natural forces; he conquers or is conquered; maintains himself or perishes, according to the specific measure of resistance suitable to each species. To the metal, it is certainly not of less consequence

to maintain its elasticity, than to the animal his life: both endure certain impressions without disturbance; both change their properties under certain conditions; both lose them, temporarily or permanently, under other conditions. No one of these phenomena is less legitimate, less necessary, or less suitable than the others.

"In consequence of this struggle of the typical force with foreign forces, the individual is every instant only the product of both. What it would be by virtue of the typical force alone, could not be ascertained except by allowing it to develop itself, to and for itself, without any influence of external momenta. We would thus, if the word should have any sense, learn its absolute normality. But that would be precisely the same as if one, in order to investigate the pure quality of a material which is changed by temperature, should demand that we become acquainted with it destitute of all temperature. To be sure, among the influences to which typical bodies are exposed, such may be distinguished as are necessary to the development, others which are superfluous and accidental, The means of nourishment, for instance, belong to the first, allurements of the senses, to the second class. We could represent an organism as absolutely normal, to which exactly the necessary sum of necessary influences, and no accidental ones, had fallen to its lot. But the same world which offers it to the former, exempts it from the latter. Has God permitted this, because it could not be otherwise according to his plan of creation, or is it our destiny that the senses should become perfected by contact with the external things around us? A question, to which physiology must remain indebted for the answer."

He next treats of the "*Morbid Process*," and here again M. Henle is master of his subject, and lays open to the inquisitive mind, a vast field of original thought. We feel confident the reader will thank us for the following quotation:

"The common characteristic of the typical forces of the organic kingdom is the constancy of form during the change of matter. Individuals are the severed shoots from the stem of a species, which stem, like a polyp-stock, always adds a new mass on the one end, whilst it dies off at the other. This relation is repeated in the reference of elementary parts to the individual, visibly in the tissues grown by apposition in one matrix, the epidermis, hair, nails, etc. It is repeated, finally, in the relation of the ingredients to the histological elementary forms. No tissue maintains itself, if the conditions for the renewal of its substance are withdrawn from it; this, in the organs whose function is visible motion, or subjectively perceptible sensation, is shown immediately by cessation of function; in others, whose destiny seems to be only quiet vegetation, it manifests itself at a later period by mortification, desiccation and decay.

"Nothing is more short-sighted than to seek for the cause of this perishableness of organic matter, in the destroying influence of external agents. It is true that the organism can be destroyed by attacks from without; that its own voluntary destruction can be hastened; but if death depends alone upon such influences, then nothing further were necessary than to exclude them in order to have endless life. It is equally incorrect if we represent the activity of the organism as the cause whereby the matter is worn out. The activity is not a cause, but a result of the motion of matter, a motion which, from the beginning, is bound to the typical form of the organism, in the same manner as the *vis inertiæ* to the typical form of the crystal.

"The law, that the living substance should exist in a continual perishing and renewal, undergoes some restrictions and even exceptions. If the function of the brain ceases from deficiency of arterial blood, death does not immediately

appear, but there follows a period during which the organ is capable of resuscitation; it is only apparently dead. Single nerves and muscles which are paralysed by being tied, can be aroused to life again if the ligature is loosened soon enough. Separated parts of the body, or blood drawn from a vein, continue likewise in a stage of apparent death, notwithstanding they are still capable of entering into organic connexion with the organism from which they have been taken, or with one allied to it. The duration of this stage, like the velocity of the reciprocal change of matter generally, is different for different tissues and organisms. In general, with the higher animals, it is quite limited, and perhaps still more limited than appears at the first glance, because in the paralysed, and even in deservered parts, the change of matter does not immediately cease, but is only reduced to a minimum. Limbs, in which the circulation is interrupted, retain their power longer when the veins than when the arteries are tied; the nerves of dissevered parts of the body recover themselves, if we allow them some repose after the exhaustion by stimulants; proof that even the stagnant blood serves still a long time for nourishment."

It is impossible to do anything like justice to this great work, in a hurried notice like this, we can only indicate some of its beauties, and recommend it to those who really desire to study the philosophy of medical science.

III.—*Essays on Asylums for Persons of Unsound Mind.* By John M. Galt, M. D. Richmond, Va. 1853.

The body of the Essay has already been given to the public; but the subject upon which the author dwells is now exciting so much interest in the breast of the philanthropic in every part of the civilized world, that no apology should be expected for giving it the greatest possible publicity.

If, as Grotius asserts, "*the care of the human mind is the most noble branch of medicine,*" then we should begin to study its diseases, that we may be better prepared to correct its vagaries—and to assist in bringing back to its normal domain those faculties which elevate us in the scale of being above the animals of the field.

Every Insane Asylum says Dr. Galt, should have at the command of its inmates, an extensive library, including the magazines, newspapers and best periodicals of the day. A judicious system of reading is now regarded by the enlightened physician as one of the best means that can be employed in the treatment of the insane; particularly among certain classes of these unfortunate subjects. Indeed, every means should be enforced, calculated to divert the mind from old into new channels; to give it healthful exercise, and to disenthral it from the gloomy contemplation of disagreeable subjects.

With these brief allusions to the elegant Essays before us, we must close our remarks, believing that the writings of Dr. Galt will ultimately bring about an improved system of treating the Insane in the United States.

We thank the author for a beautiful copy of his Essays.

IV.—*Principles of Medicine, comprising General Pathology and Therapeutics.* By Charles J. B. Williams, M.D., F.R.S., Fellow of the Royal College of Physicians, etc., etc. With additions by Clymer; 4th American edition. 1853.

The plan of this work and the manner in which it has been carried out by the distinguished author, are now fully understood and justly appreciated by the reading portion of the medical public; hence, it is only necessary for us to point out the valuable addition made to the present edition. Among the most important of these, we find in *Etiology*, the following subjects, embraced in the present volume: *mechanical, chemical and diatetic causes of disease—defective cleanliness, ventilation and drainage.*

In *Pathology*, the tabular views of the elements of disease; reflex action and sympathy; *elementary changes in the blood; congestion; determination of blood; inflammation*, in its nature, manifold results, and modes of treatment; *degeneration of textures; cacoplastic and aplastic deposites*, their treatment, with a notice of the action of *Cod-liver Oil.* Besides these points, we have an entirely new chapter on *Hygienics*, embracing food, clothing, air, temperature, exercise, mental occupation, sleep and excretion.

The work as now presented to the public, is perhaps the most perfect of any other treating on similar subjects; it combines the science and the art—the theory and the practice in a most masterly manner—and we feel confident that as knowledge of the practical views and scientific principles laid down in the book, became generally known, medicine—practical medicine—will advance in the same proportion, to a greater perfection and certainty.

Dr. Williams' work is not an abstraction, abounding in fruitless speculations of obsolete writers on the essence of disease and kindred nonsense; but it lays down the well ascertained facts of the science, and applies them to the practice of medicine. It teaches us in what diseased action consists; how this action modifies the tracture of parts

and organs, and then seeks to point out the remedial measures best calculated to correct such deranged action. Works like this are well calculated to exalt the healing art, and to arm the well-instructed practitioner with such means and appliances as shall place him, in the estimation of an intelligent public, far above the ignorant quack or the unprincipled charlatan. We, therefore, cordially recommend it to the profession. Steel, 60 Camp-street, has the work.

V.—*On Tuberculosis and Scrofula.*

A valuable work, according to some of the reviewers, has lately emanated from the press under the signature of Henry Ancell, on Tubercular Disease and Scrofulous Affections. We propose to furnish a brief synopsis of some of the views of Mr. Ancell on these important questions.

First, then, Mr. Ancell regards tuberculosis as strictly a blood disease; and hence to a primary morbid condition of this fluid he refers all local tubercular diseases. He maintains that a tubercular condition of the blood may exist for years without any local deposit; until some fortuitous circumstance may localise the morbid product in one or more organs, and if this should happen to concentrate upon organs necessary to the maintenance of life, death will sooner or later take place.

Mr. Ancell regards scrofula and tuberculosis as essentially one and the same disease; and both morbid conditions of the blood. Hence, any other treatment than that directed to the improvement and purification of the circulating fluids, is nugatory and of little value.

VI.—*Atlas of Pathological Histology.* By Dr. Gottlieb Gluge, Professor of Physiology and Pathological Anatomy, in the University of Brussells, etc., etc. Translated from the German, by Joseph Leidy, M.D., with 320 figures, on 12 copper-plate engravings. Philadelphia. 1853.

The modifications in the relative weight of the various organs, as produced by disease is a subject, that has, up to the present time, received but little attention; and we are glad to observe that such distinguished writers as Gluge and Quetelet have, at last, been induced to direct their attention to this important pathological desideratum. To

appreciate correctly the alterations, both in size and weight, determined in organs by disease, it is highly necessary to be familiar with their normal condition in these respects. In forming a correct estimate of these modifications, we must take into account the reciprocal influence of one organ on another, in order to arrive at correct results.

The size of the internal organs vary as much in different individuals as any of the external parts; thus of two individuals of nearly equal weight, height, etc., the head, hands, etc., of the one shall be much larger than those of the other; yet it would be counted ridiculous to regard one as abnormal, when contrasted with the other. These are important considerations in pathological investigation; and unless they be taken into account, we cannot expect to arrive at any degree of precision and certainty in estimating the lesions induced by disease, or the degree of departure of one or more organs, from the normal standard. Physicians have too long overlooked the comparative size of organs in a state of health; all of us are eager to inspect the organs after they have been stricken by disease, but how are we to estimate the changes wrought by disease, unless we be previously made acquainted with their healthy, their normal weight, etc.

The work of M. Gluge dwells at some length on these practical points; he furnishes in a tabular form, the measurement of all the various organs of the healthy body, their weights, circumference, etc.; these are to serve as standards, by which an estimate may be made of the changes brought about by disease.

The work contains many observations of the diseases of the several important organs of the body—such as the liver, kidneys, brain, etc.

He next proceeds to make some introductory remarks in "Histology," which comprises the description of abnormal tissues—their individual elements, and their development.

But we have neither space nor time to recapitulate the many excellent features of this splendid work, it must be examined, studied and treasured up by those who wish to make themselves well acquainted with the basis of philosophical medicine. The whole subject is beautifully illustrated with finished plates.

No medical man's library can be complete without this work. Mr. Steel, 60 Camp street, has copies.

Part Fourth.

MISCELLANEOUS MEDICAL INTELLIGENCE.

I.—*The Flowers of the Ash Tree a Remedy for Chronic Rheumatism.*

Dr. Deffris, who was for a long time the victim of Chronic Rheumatism in the knee, finally succeeded in curing himself with a decoction of the flowers of the ash tree.

He remarks that these flowers should be gathered when they exude a species of viscous gum, which is generally in the month of May or June, according to the climate. The flowers should be dried in the shade. We mast take 30 grammes and boil them in a *litre* of water for a quarter of an hour, after which we must add a small quantity of peppermint. Of this tisane, either sweetened or not, the patient must take a wineglass night and morning, for twenty or thirty days.

(*Revue Medicr-Chir.* June, 1853.)

II.—*Queen Victoria and Chloroform.*

Her Majesty, the Queen of Great Britain, in her recent accouchment, was subjected to the influence of chloroform, by Dr. Snow, with the approbation and sanction of Sir James Clark, Drs. Locock and Ferguson. The Queen expressed herself as grateful for the discovery of this means of alleviating pain. The labor terminated safely and happily, both for the royal mother and her precious offspring.

(*Dublin Med. Press.*)

III.—*Injection of Balsam Copaiva for Gonorrhœa.*

Several French physicians speak favorably of Injections of Copaiva as a remedy for Gonorrhœa, especially of long standing. It may be used, prepared in the following manner: Mix about a coffee-spoonful of the balsam and ten drops of laudanum, with the yolk of an egg, to which add two small glasses of tepid water, and inject this mixture night and morning, and a cure will soon be effected.

(*Revue Medico-Chirurg.*)

IV.—*Treatment of Yellow-Fever.*

The following is the prescription usually given by Dr. Bennet Dowler, of New Orleans, in attacks of yellow fever:

℞ Mass Hydrarg.
Calomel,
Quinine, gr. x. *a a.*
Morphine, gr. ii.

Divide into pills, No. x.

Dose.—One every two hours, until the patient is brought fully under the influence of the medicine. During the administration of the medicine, he orders his patients to be diligently spunged with alcohol and water, until the febrile symptoms subside, when the spunging is gradually left off.

He informs us, that with this treatment he has been quite successful—more so, indeed, than with any other plan he has hitherto adopted.

V.—*American Medical Society of Paris.*

We insert the following circular of the "American Medical Society of Paris," and call the attention of the profession to its object:

"American Medical Society,
Paris, Feb. 18, 1853.

Sir—The American Medical Society, in Paris, having made several additions to its Constitution, hoping thereby to give greater stability to its existence, as well as to increase its advantages to American physicians, lately arrived or resident in Paris, has thought proper to make known the same to its former members and the medical profession in general.

Two classes of members have been added to those already recognized by the Constitution, viz: Permanent and Corresponding.

The Permanent class shall consist of past active members, who shall pay within one year after leaving Paris, the sum of ten dollars, or fifteen dollars,

in sums of not less than three dollars annually, until the latter amount be paid.

The corresponding class shall consist of those elected from the past members, or others who shall present an original essay which the Society considers of sufficient merit to entitle the writer to such distinction.

The Corresponding members are requested to communicate with the Society, at least once each year.

After the debates upon medical subjects, and the reports of interesting cases in the hospitals, one of the principal objects in the formation of the Society, was to establish a library, chiefly of the books of American Medical Authors, which would be thrown open gratuitously to the physicians from all parts of Europe as well as of France. Many of these gentlemen are now constantly in the habit of attending the meetings of the Society, and of referring to those works which have already been presented by many authors, or purchased by the Society. The funds of the Society, it need hardly be said, are appropriated for carrying into effect this plan. It is therefore to be hoped, that the above changes will meet with the approval of the former members of the Society, as well as the medical profession in general.

By order of the Society,

D. R. HAGNER,

Corresponding Secretary.

No. 47, Rue St. André, des Arts.

N. B.—All communications and books for the Society will be received and immediately forwarded to Paris, by Mr. Edward Bossange, No. 134 Pearl street, New York, who has kindly offered his services for this purpose."

VI.—*Tribute to the memory of Drs. Chapman and Horner.*

Pursuant to a call made by several of the physicians of Mobile, quite a large meeting of the Graduates of the Medical Department of the University of Pennsylvania was held at Dr. Lee Fearn's office on Monday morning, the 18th instant, to express their sense of the loss that the Profession of Medicine had sustained in the recent death of Professors NATHANIEL CHAPMAN and WM. E. HORNER, and to devise some method of paying a lasting tribute to their memories.

Dr. S Mordecai was called to the Chair, and Dr. Geo. A, Ketchum appointed Secretary.

On motion of Dr. John P. Barnes, a committee of four was appointed to draft resolutions and propose a plan of accomplishing the objects had in view.

The Chair appointed the following committee: Drs. John P. Barnes, R. Lee Fearn, E. P. Gaines and Geo. A. Ketchum.

At a subsequent meeting, held on Tuesday, 19th instant, the committee reported the following preamble and resolutions:

WHEREAS, It hath pleased Divine Providence to remove from the sphere of their usefulness, our much respected and honored instructors, Drs. Nathaniel Chapman and Wm. E. Horner; and whereas, in our opinion, such services as they have rendered the cause of Medical Science and our "Alma Mater," deserve some especial and lasting tribute; and whereas, it was our peculiar privilege to receive instruction from their lips, and to have held up before us their bright example of zeal and devotion to their profession, and high and honorable conduct in their private life—feelings of respect and esteem for them, and pride for the Science which they have so honored, have prompted us to meet together to give form and expression to the sentiments that fill our hearts in view of the bereavement that their deaths have occasioned. Be it therefore

Resolved, That in the death of Professors Nathaniel Chapman and Wm. E. Horner, our time-honored Alma Mater has lost two of its most indefatigable teachers, the city of Philadelphia two of its most respected and esteemed citizens, and the Profession of Medicine two of the most zealous contributors to the facts upon which that science has erected an enduring foundation.

Resolved, That as former pupils of these distinguished Professors, we especially know how to appreciate the loss that our profession has sustained—and though we know that the rich treasures of knowledge that they have left as a legacy to Medical Science will perpetuate their names as long as the truths of that science last—still we, the Alumni of the school, with whose glory their names are so inseparably entwined, would do something more to place their services and their virtues as a shining mark by which to direct the steps of future aspirants for the honors and the fame which reward the Zealous, the Industrious and the Faithful in our Noble Science.

Resolved, That in furtherance of the aboveresolution, we do hereby call upon the Alumni of the Medical Department of the University of Pennsylvania throughout the world to contribute the sum of ONE DOLLAR each, on or before the 25th of December next, to defray the expense of erecting a suitable monument to their memories, in the University yard in Philadelphia—that such contribution be sent to the Dean or any member of the present Faculty, to be used by them for that purpose so soon as, in their judgement, a sufficient sum shall have been received.

Resolved, That all medical journals and all papers which may approve this object, be requested to give the action of this meeting publicity, and to further the objects of these resolutions as much as may be in their power.

Resolved, That a copy of these proceedings be sent to the respective families of the deceased, and to the Faculty of the Medical Department of the University of Pennsylvania.

The above report of the committee was accepted, and

On motion of Dr. F.A. Ross, the city papers were requested to publish the same.

Nothing further brought forward for consideration, the meeting adjourned *sine die*.

S. MORDECAI, Chairman.

George A. Ketchum, Secretary.

VII.—*Prof. Quintard on the Mortuary Statistics of Memphis, Tenn.*

August 11, 1853.

Dear Doctor—In your notice of my "Report on the Health and Mortality of Memphis, for 1852," you appear to think my conclusion on the subject of yellow fever, as having originated and prevailed in this city, in 1828, erroneous; and you say that if I will inquire of my colleague, Prof. Shanks, I will find that there was some yellow fever here at that time, and distinctly traceable to a local cause. I had already inquired of Dr. Shanks and other old and respectable practitioners on the subject, before the publication of my Report, and the opinion was unanimous, that the disease had never originated here. Indeed, Dr. Shanks informs me that Dr. Drake told him, that the description of the disease as given him at the time of his visit to this place, was altogether different from any account he had ever met with; and in fact that he much doubted whether it was yellow fever at all. My recent inquiries on the subject have only served to confirm the view put forth in my Report.

Whatever may have been the causes of the increased mortality in this city during 1851–'52, they seem to be no longer in operation, and in my humble opinion sustain the position assumed in the Report.

The mortality of the past and present year, up to the 1st of August, was as follows:

	Jan.	Feb.	March.	April.	May.	June.	July.	Total.
1852	38	32	30	35	55	119	104	413
1853	50	27	35	25	36	33	38	211
								202

By this, you will perceive our much abused city has been remarkably healthy thus far during the year 1853; and yet there has been as much, if not more, grading done as in any previous year.

Let me here make the amende to my colleague, Dr. Merrill. In my Report, I stated that he ascribed all the diseases to the "one sole cause of street grading." The expression was not intended to convey the idea that there were "no diseases prevailing in Memphis before the grading commenced;" but that Dr. Merrill had, in his address given undue prominence to street grading as the cause of the increased mortality. The expression was rather a random one, and I heartily embrace this opportunity of putting myself *rectus in curia.*

With great regard, yours, etc.,

C. T. QUINTARD.

Note by the Editor.—The remarks to which Prof. Quintard alludes in the preceding paper, were made by a friend during our absence from the city. As his object was to get at the truth, we feel certain he will thank Professor Quintard for the rectification.

VIII.—*On the Chemical Substrata of the Human Body.*

BY DR. G. E. DAY.

[In an interesting and clever review of 'Lehmann's Physiological Chemistry,' Dr. Day gives an excellent epitome of the *substrata* of the animal body' treated of by Lehmann in his first volume. Dr. Day remarks:]

On reviewing the chemical substrata of the animal body, treated of in the first volume, we perceive there are four groups of substances in which the vital processes are most intensely manifested; or, in other words, which most actively participate in the metamorphoses of the animal tissues. These are:

1. *The Albuminous substances or protein-bodies, and their derivations.*
2. *The fats.*
3. *The Carbo-hydrate, and*
4. *The inorganic salts.*

That *albumen* is one of the most important substances in the animal body is sufficiently obvious from the positions in which it occurs: we find it in the greatest quantity in the blood, and in all those animal juices which principally contribute to the nutrition of the organism; however, a chemical investigation of various tissues shows us that albumen only requires slight modifications to enter into other forms; as, for instance, that of sintonin, or muscle-fibrin, the essential constituent of the solid contractile parts by which alone both the voluntary and involuntary movements of the animal body are accomplished. We find it both in the fluid and solid form in that most complex of all structures, the nervous system, both in the nerve-tubes and in their contents. In association with a little fat and traces of sugar, the ovum consists merely of albumen and casein holding salts in solution; and there can be no doubt that with the co-operation of the oxygen conveyed by the blood, all the tissues are formed from the protein-bodies, although we are not as yet in a position to explain with certainty the exact nature of the changes by which the gelatigenous and certain other structures are produced.

The fats next claim our attention. Their physiological value and their mode of origin have been noticed at some length in the first volume; we will here simply mention, that without the intervention of fat no colourless blood-cells, and therefore no red corpuscle, could be formed; indeed, no animal cell or fibre of any kind. While, however, in the normal state, the fat takes an active part in cell-formations in the animal body, we also, in some cases, perceive a tendency to a formation or production of fat in existing cells and tissues whose nutrition has been peculiarly modified. The phenomenon commonly designated as fatty degeneration admits of a double explanation. We may either assume that the pre existing fat, under the influence of certain molecular forms, is accumulated in the older and less vitally active cells, and replaces the nitrogenous textural particles as they become worn out; or that the fat is produced directly from the nitrogenous textures of the cell or fibres, the nitrogen being developed in the form of ammoniacal salts, and the fat being left as a product of decomposition. Lehmann, in his first volume, supported the former view as the least hypothetical of the two, and as the more probable from its simplicity; since the date of its publication Wagner and Liebig have, independently of each other, instituted certain positive experiments which very much strengthen the second view. Wagner made the remarkable observation that the crystalline lenses, pieces of dried albumen from eggs, and other substances poor in fat, which were introduced into the abdominal cavity of birds, were perfectly changed in their texture in the course of from twenty-five to fifty-four days, the residue containing far more fat than existed in the original substance. Liebig has shown that the metamorphosis of the albuminous tissues of the animal body into fat is, in a chemical point of view, not only possible but probable.

Both in the putrefaction and in the gradual oxidation (by chemical means) of albuminous substance, the results, under favorable conditions, are ammonia and fatty acids, such as the butyric and the valerianic.

These experiments and observations of Wagner's and Liebig's are, at all events, sufficient to show that there is no chemical absurdity in assuming that, under certain circumstances, fat may be a product of the decomposition of the protein-compounds.

The carbo-hydrates, constituting the third group, are in many respects closely allied to the fats. The substance of this class occurring in the animal body are, dextrine, milk-sugar, inosite, and glaucose; to which, perhaps, we should add cellulosa, occurring in the investments of the tunicata. Recent investigations have detected sugar, in small quantities, in almost all the fluids subservient to nutrition, as, for instance, in the blood, transudations, lymph, chyle, the albumen of the eggs, etc.

"The sugar," says Lehmann, "which we meet with in the intestinal canal of herbivora and omnivora is due to the metamorphosing influence of saliva and pancreatic juice on starch and other carbo-hydrates; but we also find sugar in the blood of carnivorous animals in no very inconsiderable quantities; this must therefore have arisen from some other source than from the carbo-hydrates conveyed into the system from without; from a number of comparative analyses of the blood of the potal and of the hepatic veins. I believe that I have indicated the probability that the sugar which is found in the liver, where it has also been found by Bernard and Frerichs, owes its origin to the decomposition of albuminates, and especially of fibrin."

When, further, we consider that nature has provided the egg with a small quantity of sugar, and that its amount varies according to the stage of development of the chick, the conviction forces itself upon us that the sugar, like the fats, is intended for some other purpose in the conomy than merely to sustain the animal heat by its slow oxidation.

We are still far from being fully acquainted with the carbo-hydrates and the products of their conversion, which occur in the animal juices. Lehmann believes that we shall find indifferent substances similar to Scherer's inosite in the extractive matter. Our knowledge is more perfect regarding the acids which are formed in the animal body from the carbo-hydrates: formic and acetic acids have recently been found by Schottein, a very promising young chemist, in large quantities, in the sweat; butyric acid occurs, not only in the sweat, but also in the muscular fluid, in the parenchymatous juice of the unstripped muscular tissue of the stomach, the intestinal canal, and the urinary bladder; and lactic acid is found in the gastric juice as well as in most of the above-named fluids.

We regret that we cannot follow our author through his demonstration of the facts that the presence of small quantities of sugar essentially contributes to the solution and digestion of the protein-bodies, and that the carbo-hydrates, or rather their acid products of metamorphosis, discharge an important function in the intestinal canal, in no way directly connected with the process of respiration.

Brit. and For. Medico-Chirurgical Review, January, 1853, p. 89.

IX.—*On the Therapeutic Action of Furfurine, Nickel, etc.*

BY PROF. SIMPSON, EDINBURGH.

[Furfurine is an alkaloid, that produces in poisonous doses upon the lower animals many of the symptoms produced by quinine. The following are the results of the experiments on the properties of the sulphate of nickel. The Professor says:]

Sulphate of nickel has appeared to me to act as a gentle metallic tonic. I have generally used it in doses of half a grain or a grain, repeated thrice daily, and have given it in the form either of simple solution or of pill. In large doses it is liable, like sulphate of zinc or copper, to produce sickness and nausea, particularly if taken on an empty stomach. I have generally requested it to be taken half an hour or an hour after meals. It has appeared to me, as the result of pretty numerous experiments and observations, that the therapeutic actions of the salts of nickel and manganese correspond in a considerable degree with the therapeutic actions of the salts of iron upon the economy; and that these three metals might, under many conditions, be almost used as therapeutic substitutes for each other. But they also specifically differ from each other in some respects. For example, in one most interesting case the sulphate of nickel arrested a severe form of periodic headache, which had previously defied iron in many different forms, and all other kinds of treatment that had been employed. The patient came from Italy last autum, in order to place herself under my professional care; and for some months I was as unsuccesful as my predecessors had been in affording her any relief. But let me give the history of the affection, and the ultimate result, in the lady's own words. She drew up the following note of her case several weeks ago.

"My headaches (she writes) came on soon after my second confinement, in August, 1847, and continued to return every tenth day without intermission, up to February, 1852. During the first four years I was in Italy, and was attended by medical men of all countries, English, French, German and Italian. I also tried hydropathy and homœpathy, the latter for six months, but all without the slightest effect. The pain came on in a small spot on the right temple, and lasted from twenty-four to thirty-six hours. After the first eight hours severe sickness followed, which continued up to the sixteenth hour. During the attacks I had violent cold shivering fits, succeeded by a burning fever. At times I was quite delirious from the violence of the pain. I have taken large doses of steel, iron, and quinine, besides many other sorts of medicines. The quinine I took at first only two days before the attack was expected. I then took six grains every day for a year and a half, but it never put off the headache a moment beyond its day and hour, nor would anything that I could do bring it on before the time. When I first came to Scotland to be under the advice of Dr. Simpson, in August, 1851, he gave me thirty grains of quinine a day, for three days before the headache was to come on; but it returned to its hour, and as severe as ever. This was tried also with the next fit, with no better success. Dr. Simpson then tried successively furfurine, bebeerine, and arsenic; but the headache still continued up to the 1st of February, 1852, on which day I had a most severe attack. On the 4th of February, he gave me the solution of sulphate of nickel to take; since which time, to my astonishment, my usual headaches have altogether disappeared."

To the preceeding account I have merely to add, that if we may judge from the result up to the present time, the cure of this patient from the use of nickel appears entire and complete. And perhaps it is but proper to remark, that this result seems fairly attributable to the action of the nickel alone, inasmuch as there was no relief under the use of any of the means or medicines previously employed for years; while convalescence distinctly began from the date of the employment of the metal in question.

Further, it is perhaps not unimportant to observe, that while the disease had lasted four years, without abatement, its subsidence in February could not be the result of change of climate, as the lady had already resided about five months in Edinburgh, or its neighborhood, without any noticeable amelioration in the recurrence and intensity of the headaches; and at last they disappeared under the nickel, at a period of the year—viz: the commencement of spring—at which, in our climate, headaches and other periodic diseases are known to be specially liable to become increased and aggravated.

In no kind of case is the beneficial action of iron more remarkable than in the treatment of chlorosis and amenorrhœa. I have seen nickel in a similar way apparently serviceable under the same circumstances. In the latter end of last year, I gave it in a case of amenorrhœa of ten years' duration. The amenorrhœa supervened at the age of twenty-two. At the same time a galvanic intra-uterine bougie was introduced, and left for some time in the cavity of the uterus. In the course of three or four weeks menstruation took place, and has recurred regularly from that period. In such a case, however, it is difficult to say how far the result was attributable to the local means used, and what share the nickel had in the restoration of the patient's health.

Monthly Journal of Med. Science, August, 1852, p. 135.

X.—*Efficacy of the Inversion of the Body in cases of Syncope from the Inhalation of Chloroform.*

Some unfortunate accidents having been lately reported from the inhalation of chloroform, much attention has been directed to the discovery of means for their prevention. Among others, exposure to air, tittillating the nares or soles of the feet, introducing the finger into the fauces and raising the epiglottis, insufflation of air from mouth to mouth, etc. M. Nelaton has tried all these without success, and thinks them useless. From his experience, he believes that syncope is the principal cause of death, the subjects of it being pale, rigid, and corpse-like; and the plan which he has found most efficacious is the rapid and complete inversion of the body.

The first time, at the Hospital St. Louis, while performing an operation on the hand, he observed the patient becoming pale, and the pulse faltering, and immediately had him inverted; when the face, before pallid, at once became congested, and the interrupted respiration re-established.

At the Hospital de la Faculté a similar case occurred last year; and lately M. Denonvilliers, having heard of the plan pointed out by M. Nealton, tried it with equal success. In place, then, of the ridiculous proceeding of tittilating the nose or feet in persons who do not feel the amputation of a limb, the body should be quickly inverted, as being the most prompt and easy way of arresting the syncope which threatens the life of the individual.

Jour. de Med. et de Chirur.

XI.—*Analytical Report on the Treatment of Fever by large doses of Sulphate of Quinia.*

Dr. A. W. Barclay, Medical Registrar to St. Georges Hospital, has published (*Medical Times and Gazette*, Jan. 8th, 1853,) a short summary of all the cases of fever admitted into that hospital from the middle of May to the middle of November, so as to exhibit the comparative results of treatment when large doses of quinia were given, and when the ordinary method was followed.

The question, says Dr. B., is not whether under certain circumstances tonics are not required in fever, for that will be admitted by all who have learned that their patients will not bear loss of blood, and that the great object of the practitioner in anxiously watching a severe and dangerous case is to endeavor to obviate the tendency to death by sheer exhaustion; but the question is, whether by giving very large doses of the salt we can decidedly cut short the disease, and restore the patient to health and vigour in a few days, in place of waiting for it to run its course. It must be at once admitted that quinia has not this power in all circumstances, and that at all events cases do occur in which it is perfectly powerless to arrest the progress of the disease to a fatal termination. But it yet remains to inquire whether, in the majority of instances it exerts any such specific power as in ague, or whether its employment even without putting a stop at once to the febrile disorder, does yet in any way shorten its duration. And it must be confessed, that it is something strange to have learned that a patient may take between two and three drachms of the sulphate of quinia in twenty-four hours, either in pill or in solution, at the very height of a severe attack of fever, and yet suffer no harm from it, and this even when we are sure that comparatively little can have run off by the bowels, and that none has been rejected by vomiting.

In watching these cases, one fact has been most striking and unmistakable, and that is, the effect of the remedy when pushed to its full extent; the pulse is depressed, the vital powers are prostrated, and vomiting follows. In some of the more severe cases there was such a tendency to sinking, that stimulants were had recourse to, and seemed urgently called for. But this condition was by no means followed by a remission of the disease, which recurred as soon as the physiological effects had passed off.

The cases treated by quinia are divided by Dr. B., into three classes:

1st. Those in which its exhibition was followed by marked depression.

2d. Those in which the pulse became slower, without general prostration or sickness.

3d. Those in which no decided effect was produced which could be noted at the time.

1. Including all the cases together in which this effect was produced, the number is five. Two have been already mentioned as fatal, one of apparant typhus, one of tubercular inflamation of the brain, to which a third may be added, complicated with albuminuria, which was not detected until the subsidence of the fever, and ultimately proving fatal. The physiological effect of the remedy was produced by very different quantities in different instances, and given at varying intervals. One patient took twenty grains every three hours for nine times; another took ten grains every two hours for ten times; while a third took twenty grains every six hours for only three times; the other two had twenty grains every four and every six hours respectively for eight times.

Of the three uncomplicated cases one died, one was ill ten days before admission, and remained under treatment forty-five days before recovery was complete; the other had been ill a week, and was discharged cured at the end of twenty-four days.

2. In two instances only did the pulse become remarkably slower without depression; one took ten grains every three hours, the other fifteen grains every four hours for about two days, after which the dose was gradually diminished. The first had been ill five days, and was discharged cured in eleven days, having been kept under observation longer perhaps than was absolutely needed to ascertain that recovery was really complete; the second had been ill only two days, and got well in three weeks.

The first case was not severe and had no spots; the second was delirious for the first two or three nights, and had a faint rather indistinct rash on the abdomen; he had also pretty severe diarrhœa, but no evidence of ulceration of the bowels. The pulse fell in each below 50, but it is necessary to state here, that in another instance it fell still lower in which no quinia was given. He made a very rapid recovery, being only ill five days before admission, and leaving the hospital cured in eight days.

3. In eleven cases there was no distinct physiological effect produced by the quinia; and it remains to inquire whether recovery was more rapid under this mode of treatment than any other; and this may be best accomplished by instituting a comparison between them and the whole of the other fever patients admitted during the same time. Twelve examples of a very mild form, which might be called febricula, are omitted, and there then remain fifty-one instances of well-marked fever which were not treated by quinia.

Among these, twenty exhibited fever spots on the chest or abdomen, and six with and five without spots gave unequivocal evidence of ulceration of the bowels. By this is not meant merely the occurrence of thin watery motions, which have been observed in the majority of the patients, but the persistence of diarrhœa, with a patchy, shining, or fissured tongue. We have, therefore, as the basis of our analysis, twenty-six cases, which neither had fever spots nor distinct evidence of ulceration, fourteen with spots but not certain ulceration, and eleven in which the presumptive evidence of ulceration was strong.

The average duration of these cases, was—of the 26 cases, 10 days before admission, and 21 days under treatment; of the 14 cases, eight days before admission, and 22 under treatment; of the 11 cases, 7 days before admission, and 33 under treatment.

Turning now to those in which the quinia treatment was adopted, and almost invariably in ten-grain doses every four hours, they include five cases in which there was pretty conclusive evidence of ulceration of the bowels, three of which were also spotted; four cases with spots, where ulceration was not proved, and only two in which neither condition was exhibited. With the last cases it may be best to classify the two already referred to under the second division, because they are not marked by any very broad line of distinction separating them from the present series, and they exhibit the quinia treatment under its most favorable view.

There are, therefore, four cases without spots or decided ulceration, of which the average duration was eight days before admission, and twenty-three under treatment; four cases with spots only, of which the average duration was ten days before admission, and twenty-six under treatment; five cases with ulcerated bowels, of which three had also spots, and their average is fifteen days before admission, thirty-seven under treatment.

I must here distinctly state that, when I commenced this report, I had no idea what the result would be, and, so far from believing it unfavorable, had hoped that, excluding some unfortunate cases, the treatment of fever with quinia would prove rather more speedy, safe and effectual than by ordinary modes. I am sorry to be convinced that it has no advantages.

It may be well to state in conclusion, that the prevalent type of fever has been what would be called "typhoid," not true "typhus." One or two had the aspect of congestive typhus, but wanted the purple mottled rash. One patient

had this rash very well marked, mixed with ecchymosed spots, and at the same time had distinct ulceration of the bowels, with a chapped and glazed tongue. Some had a very abundant crop of florid slightly-elevated spots disappearing on pressure; some had only one or two of this character. Occasionally the spots are characterized as large, some as small; and individual instances occur exhibiting various degrees of persistency, and various shades of color, from a pale rose to a deep crimson. Spots existed without ulceration, and ulceration without spots, apparantly without any definite rule; and some of the most severe and tedious cases were unaccompanied by either one or the other.

Without entering at present into the *vexata quæstio* of the exact value of fever spots in diagnosis, these facts are mentioned to show that the cases presented very considerable varieties, but I have not been able to ascertain that the quinia treatment was more adapted to one condition than to another. It appeared in only two cases resembling typhus in which it was tried, that the depression was greater, and produced by a smaller quantity of the alkaloid, than in the remainder, but the instances are too few to draw any general conclusion from them. It is still a desideratum that general experiments should be made in the course of a regular epidemic of typhus; for, however otherwise the cases differed among themselves, they had this feature in common, that they were examples of the endemical fever of London.

Amer. Jour. Med. Sciences

XII.—*Medical Jurisprudence and Toxicology.*

Drowning. Is there a fixed period at which a drowned Body will float? If not, what are the circumstances that chiefly control its early or prolonged rising to the surface.—I am indebted for the following case to the kindness of the Hon. William B. Wright, one of the judges of the Supreme Court of this State, who tried it at the circuit held in this city last winter. It contains what may be called the *medico-legal testimony* only, and I submit it in the hope that the main inquiry may receive some attention from medical gentlemen residing in cities and towns where drowning frequently occurs, as to its statistics:—

Voltan and Adams, vs. The National Loan Fund Life Assurance Company. The action was brought by the plaintiffs, as assignees of this policy, to recover, on a policy of insurance, issued by the defendants upon the life of one Conrad Shoemaker. The insurance was for $10,000, and the policy was issued on the 15th May, 1850. The premium on the policy was payable quarterly in advance.

On the 23d of August, 1850, Shoemaker paid the premium for the quarter ending on the 15th of November, 1850. On the 4th September, 1850, the plaintiffs alleged that Shoemaker was drowned, while on a fishing excursion, with one Ottman, a German, in the waters of the Bay of New York, about opposite to Hoboken, and nearest to the New Jersey Shore. The theory of the defence substantially was, that Voltan, Martin, and Shoemaker (Germans) had entered into a conspiracy to defraud the insurance company by causing an insurance to be effected for a large amount on the life of Shoemaker, and subsequently secreting and disposing of him.

To obtain a recovery, of course, it was necessary that the plaintiffs should satisfy the jury of the death of Shoemaker. This they attempted to do; 1st., by the testimony of Ottman, who swore to the circumstances of his drowning, and of the time and place, which was on the 4th of September, 1850, about

dusk, in the Hudson River, opposite Hoboken, and near midway of the river.

2d. By showing that a body *found floating* on the *river* near Jersey City, on the 7th of September, 1850, was the body of Shoemaker.

This body was examined by the coroner of Jersey City, soon after being discovered. The skin was somewhat bleached, and the face disfigured; a part of the lips being eaten off by crabs, lobsters, or fish of some kind. After examination it was interred by direction of the coroner.

It was not attempted to identify this as the body of Shoemaker, except from some of the clothes found on it, and particularly the handkerchief on the neck. The handkerchief on the body was the half of a black silk one with stripes, and cut from its mate diagonally. It was shown by a witness that Voltan, a short period before the alleged drowning, had purchased a handerchief for his son, and at the suggestion of Voltan's daughter, it was cut in two, and half of it given to Shoemaker, after being hemmed by her; the other half to the son. The part retained by the son, and the part found on the neck of the body were exhibited in court and found to match in color and stripes, and when laid together formed a square, and although cut across, the stripes matched in the run and character of the stripes. The pantaloons were also shown to be of the same general character worn by Shoemaker, about the time of his alleged death.

To rebut the presumption that this was the body of Shoemaker, a number of witnesses were sworn on the part of the defence, with a view of showing that, as a general rule, bodies will not rise and float, even when water is of the temperature that it is in the month of September, under from six to ten days. As Shoemaker was alleged to have been drowned on the 4th of September, the body was found floating on the 7th of September, three days afterwards; if it were universally true that bodies do not float until decomposition takes place, in the waters of the Hudson, under from six to ten days, then this could not be the body of Shoemaker.

The first witness sworn on the subject was *Dr. Barent P. Staats.* He testified that he had occasion, in the course of his professional reading, to examine the subject as to how long a body will remain in the water before rising and floating. That it depends on the time of year, and the temperature of the water, and the size and make of the man. When the temperature is 65°, he did not think any body would rise in from less than seven to ten days. On his cross-examination, he said he did not know that he could point out any book that he had consulted.

Dr. Benj. Budd was the next witness called. He testified that he was assistant coronor in New York—has had occasion to see many drowned bodies—some one hundred and fifty. Never knew a body to rise in less than six days, unless some mechanical means were used to raise it. Should judge the body found at Jersey City to have been in the water from ten to twenty days. Has never known a body to be in the water less than seven days that was mutilated by fishes. Bodies that have been hooked up in three, four, or five days, have not that peculiar bleached appearance as those present that come up from seven to ten days. The body will not rise until decomposition has commenced. He is twenty-five years of age, and has only studied the book of experience.

Dr. Seth Geer was then called. He testified that he was coroner in New York for eighteen months, during which time he had examined between three and four hundred drowned bodies. The general rule as to the rising of drowned bodies in the harbour of New York, is from eight to ten days. In his judgment, from the description given, the body found at Jersey City, had been in water two or three weeks. Never knew a body that had been in the water but three days, mutilated by fishes. The hotter the water the sooner the body would bleach.

Andrew Blakeley was then called. He testified that he was deputy coroner in New York a little over two years, during which time he examined rising two hundred and fifty drowned bodies. Drowned bodies would rise in the summer months on an average of from six to ten days, as he found out by experience. He did not remember any case of rising when the body had been in the water but three days. He never saw a drowned body that had laid in the water but three days eaten by fishes. On his cross-examination, he stated that he had never read any medical book on the subject, nor did he know, except from testimony taken as coroner, of a body lying under water seven days. It takes a body from six to eight or ten days to get bleached. He means by bleaching, a soaking of the body—a general softening and whitening of the body.

Henry C. Van Wie was called on the part of the plaintiffs. He testified that he was coroner of the County of Albany for four years. Has held a good many inquests on drowned bodies. Has known two or three instances where the bodies have risen in three or four days. In warm or sultry weather, they will rise in from three to four days. They will bleach out directly in warm weather. They will be mutilated by fishes directly after decomposition takes place. Remembers an instance of holding an inquest on a body drifted ashore and had been drowned four, five, or six days. (This witness related the startling fact of holding in one season, inquests on fifteen infants under three months' old, found floating in cigar boxes near the city of Albany—cases doubtless of infanticide.)

Henry C. Allen called for the plaintiffs. He testified that he had been coroner of Albany County for twelve or fourteen years. He never could make up his mind as to any definite time that a body would remain under water. He knew an instance of a girl of fourteen years of age, who was drowned on Friday at 12 o'clock, and floated on Sunday at 12 o'clock. She was drowned at Greenbush Ferry. Has known instances of bodies rising in five or six days; sometimes sooner. Knew one man by the name of Moreton, who floated on the fourth or fifth day. The girl spoken of had turned a dark liver color. Females float sooner than males.

George E. Cutler called by the plaintiffs. He testified that he was coroner of Jersey City. He knew the case of a young man who was drowned on Sunday, about 7 or 8 o'clock in the morning, and on Tuesday or Wednesday succeeding, about eleven o'clock, he was found floating about two miles from the place where he was drowned. He knew of a female by the name of Smith, who was seen alive on Wednesday evening, about 7 o'clock; on Wednesday about 4 o'clock P.M., he was called to view the body floating. A person of temperate habits will bleach very quick; those who have been inveterate drinkers never will bleach.

John Osborn called by plaintiffs. He testified that he was coroner of Albany County three years. Had occasion frequently to reclaim drowned bodies. Had known bodies to come up in two days; others not in several months. Had a case of an Irish girl; she had been drowned some two or three days; it might have been four. Had another case of a man, McCarregan, an Irish auctioneer, who rose in four or five days.

Silas M. Benton, called for plaintiffs. He testified that he was acting coroner in 1847, 1848, and 1849, in New Haven County, (Conn.) He knew a case of a person whom he saw on Friday, was missing on Saturday, and found floating in the water on Sunday. The man was a German, and a baker by trade.

The verdict of the jury was in favor of the plaintiffs. T. R. B.

Amer. Jour. Med. Sciences.

XIII—*Iodine and Iodide of Potassium in Sore Throat or Chronic Pharyngitis.*

Dr. A. P. Merrill—in a paper published in the Medical Recorder (Memphis) on *Sore Throat or Chronic Pharyngitis*—after giving a summary of the different modes of treatment—all of which had been in his hands inefficient—gives the following as a convenient formula, which may be applied to the throat, with a camel's hair brush, once or twice a day, or as often as the patient feels a tickling sensation, and a desire to cough:

Iodide of Potassium,	℥ j
Iodine,	℥ ss
Water,	℥ j
Gum Arabic,	℥ ij
White Sugar,	℥ ij—mix.

He has been using this remedy in his practice for more than a year, and has succeeded in relieving several cases of Chronic Pharyngitis with it, which had resisted the use of other active means of cure.

Charleston Medical Journal and Review.

XIV.—*Iodine as a preventive of Mammary Abscess.*

By H. C. Stewart, M.D., of Salisbury, Somerset Co., Pa. (The Medical Examiner and Record of Medical Science.) This communication, I presume, will fall under the notice of no physician unacquainted with what is usually termed mammary abscess, both as regards the condition of the breast and the best known means of obviating that distressing condition, to which the "lying-in woman" is so often subjected.

Perhaps no organ of the body is capable of producing a greater amount of suffering to the patient, and vexation to the physician, than the female breast. Situated on a prominent part of the body—delicately constructed—influencing and being influenced at times, by other organs, it is adapted to the performance of an important function, the disturbance of which must necessarily produce a disagreeable and dangerous result, often requiring the best efforts of the physician to counteract.

Seeing, then, that these things are so, we have been led to inquire, is there no remedy? or must our patients after having undergone the agony of parturition, still suffer on, simply because their breasts have not been properly and sufficiently relieved of milk as fast as secreted?

If mammary abscess cannot be *prevented*, it is not because *remedies* have not been *proposed* for it; for amongst all the "ills that flesh is heir to,' there is none, perhaps, for which such a multitudinous variety of *cures* has been tried. This is probably the best evidence of the difficulty of preventing such an occurrence.

The first indication that suggests itself to the mind of the physician, is to remove the tension by withdrawing the milk. But this cannot always be done; for in how many cases do we find a complete obstruction of the ducts; others, again, where there are no nipples, consequently no outlet for the milk. Have we no remedy here, or must we let the gland inflame, and then bleed and apply leeches and "poultices to favor suppuration," and when the abscess forms, open it with a lancet, and run the risk of forming a milk fistula, then apply adhesive strips, and if all this fail—*let it alone?*

In the early part of my practice, I was called to attend a lady, the mother of five children, none of whom she had ever suckled, owing to an inversion of the

nipples, and consequent obstruction of the ducts. So thorough was this obstruction, that the best efforts of the physicians, on former occasions, had totally failed to relieve the breast of a particle of milk · consequently the woman had suffered on every occasion from mammary abscess.

In giving me a history of the treatment at different times, she said that at one time she came near losing both breasts; when the physician, (dead at the time of this conversation,) as a last remedy, applied something which, from the description given me, I believed to be iodine. Knowing the efficacy of that article in all glandular affections, I resolved to try it as soon as the breasts showed any signs of inflammation. On the third day, finding them large, heavy and intensely painful, I made an application to the breasts of iodine ointment spread upon linen, which gave almost immediate relief. After a few applications, I found the breasts "perfectly flaccid, completely cool, and admitting of the freest palpation and handling, without the woman making any complaint." From the favorable result in the above case, I was induced to try it in two similar cases, with the same success, and so far as I know it never failed in the hands of any of my medical friends to whom I have recommended it; but not a few there are who can bear testimony to its virtues.

With these few suggestions, I respectfully submit it to the profession, hoping that it may not disappoint their expectations.

Charleston Medical Journal and Review.

XV.—*Report of Dr. Burrell to the Lords of the Council, on the reasons for the opinions delivered by him as Member of a Board of Inquiry, held at the office of the Army Medical Department* (1849-50) *on the subject of yellow fever.**

1st. Does the yellow or bulam fever differ from the marsh or remittent fever of warm climates, or is it the same fever in a more aggravated form?

2d. Does an attack of yellow or bulam fever give, like small-pox, an immunity from a second attack, except in very rare instances?

3d. Is yellow or bulam fever a contagious disease?

4th. Is it capable of being imported?

I consider that the weight of testimony, both oral and documentary, brought forward in the course of this inquiry, as well as a careful examination of the whole subject, will fully warrant me in submitting the following observations.

I believe yellow fever to be the most concentrated grade of fever in all countries capable of generating it—to be closely allied in its nature and causes to the ordinary fevers, more especially in the Antilles, whether the type of these be remittent as in Jamaica, or continued as in Barbados—and not to be more peculiar or less indigenous to the latitudes in which it commonly occurs, than the common continued fever is to this country.

The form of yellow fever, or that attended by black vomit, which has been assumed by some as the only true model of the disease, acquires this pre-eminence over the ordinary fevers of the West Indies from the presence of the European, whom it attacks with a frequency and certainty in proportion to the shortness of his residence. In the absence of such subjects, the advocates of the bulam being a distinct disease, might safely be challenged to produce any considerable number of cases exhibiting the peculiar group of symptoms

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* Dr. Burrell was the only member of the Board called on by the Lords of the Council to state the reasons on which the opinions delivered by him on the occasion were founded.

and mode of termination which, in their opinion, are sufficient to distinguish it from the ordinary fevers of the country, and to mark it as an essential and separate disease.

Sickly or epidemic seasons, involving the whole population, occur in the West Indies, as in Europe, and very often at the same time of the year, and in these the new comer shows with more certainty his appropriate and peculiar grade of the disease; but it is very certain that ordinary or healthy seasons to the natives are often to him the reverse; his constitutional susceptibility appearing to compensate the want of power in the cause; and in consequence, many of the partial and irregular outbreaks of the disease are confined to this description of subject.

It would appear, the eruption of black vomit fever in the West Indies does not there, as in Europe and the most northern States of America, require always the same, or nearly the same season, or that occasional epidemic influence which seems to be necessary to its wide-spread invasions of countries beyond the tropics, and nearly, if not always at the same season. In other words, while there are causes almost constantly present, capable of producing the disease with new comers in the West Indies—not to be explained by the importation of contagion—some occasional and peculiar constitution of atmosphere would seem to be indispensable to its development in Europe.

Yellow fever, like the fever that devastated the Imperial army in Hungary, the Walcheren fever, and other well-known outbreaks of disease among troops has always been the scourge of armies, leaving the natives comparatively unscathed; the intense cerebral and gastric disorder, the hæmorrhages and other formidable symptoms, and irregular and uncertain invasions of the disease, having usually, like them, been reversed for the stranger; and I conceive we are here forced to one of two conclusions, either that the susceptibility of the stranger is the occasion of the more intense operation of causes producing in the natives a very mitigated disease, or that, for some inscrutable end, he alone is singled out as the victim of a malady depending on causes not only specifically different from those affecting the natives, but as fluctuating as his migrations. Is it credible that the black, and even the bulk of the coloured population in the West Indies, who suffer from putro-adynamic fevers, and all other diseases assailing the white races, should not in some form present a disease indigenous to their country? To say they have it in their youth, and in much milder form; is to fall back to the ordinary fevers of the country, and to relinquish the only diagnostic—the black vomit, by which any line of demarcation can be drawn; in fact, to admit that the mild continued, or remittent, are the forms in which the natives precent the bulam.

The coloured population, and the acclimated European, rarely present the disease in the intense form in which it attacks the new comer; they usually only suffer from the remittent or intermittent, and are sometimes entirely exempt at periods when the stranger is the solitary victim of the bulam; a fact replete with instruction, if the blindness of theory would permit us to apply it. Nothing, I am convinced, can explain this anomaly, but the greater susceptibility of the stranger to the noxious influence of a climate to which the others have become assimilated, the oppertune arrival of contagion at the period he is known to be most obnoxious to the baneful effects of that climate, being a conception too improbable to be entertained.

We appear to have selected the disease of the stranger, who is the least suited to represent the diseases of a country, either in the character or degree of their symptoms or pathological conditions. We should not consider as a fair representation of the diseases of this country, and their fatality, the results which might follow the exposure of the natives of the tropics to its cold, and vicissitudes; why, then, should we take our distinguishing mark, our point of departure in diagnosis, not from the essential features of the disease as it occurs in the natives of the West Indies, but from an occasional symptom, the

exudation of dissolved blood in the stomach, which is scarcely known, in an epidemic form, but with Europeans? The wide spread and malignant fever that assailed the troops at Walcheren, would be no measure of the forms in which it attacks the natives. Let us not believe there is one cause and one disease for the stranger, and another for the native; they suffer under very intelligible modifications of the same disease. "The French," says Dr. Ferguson, "have defined it (yellow fever) in one word, '*la fièvre Européenne;*' well for us had we stuck to this true definition, for it would have saved an infinity of controversy, panic, and delusion."

We are told by McLean, that so new was the disease to the French physicians in St. Domingo, that they ascribed the mortality to the ignorance and inefficiency of the English doctors. The ultra contagionist say that the disease spread to the several islands after the arrival of the ship "Hankey," and the outbreak of the disease in Grenada, in 1793, when, in fact, as subsequent sad experience has proved, this coincidence was entirely attributable to the diffusion of fresh European subjects direct from England; and such has been the almost uniform history attending the influx of new comers, whether of the army or navy, up to the present day.

Of 30 regiments that arrived in the Windward and Leeward Islands between 1816 and 1848, 10 were attacked with black vomit fever a very short time after landing; 2 within three months; 11 within twelve months; 5 within two years; and 2 within three years of their arrival. Of 13 regiments which landed in Jamaica between the years 1816 and 1834, 4 were attacked within six months; 7 within twelve months; and 2 within eighteen months. From 1838 to 1848, 7 regiments arrived in that island, but the emancipation of the negroes permitting the troops to be quartered in the mountains, a few cases only of black vomit fever appeared, within that period, in two of them soon after landing.

Between 1834 and 1838, no new regiments arrived in Jamaica, and during this interval there were very few black vomit cases, and those chiefly among recruits. Of 40 regiments in the Winward and Leeward Islands, between 1816 and 1818, I can only find 10 which have not suffered from the black vomit variety of yellow fever, to a greater or less extent; none escaped in Jamaica which were quartered in the lowlands. In both commands, out of 53 regiments, 33 were attacked with black vomit fever within twelve months after their arrival; showing pretty accurately that we do not require the importation of the disease, but the arrival of the stranger for its development at almost any time or season in the West Indies. I have good grounds for stating that sporadic cases of the disease are of nearly annual occurrence among new comers in Jamaica, and in pretty accurate proportion to their numbers, and their indiscretion as to exposure, fatigue, and excesses of various kinds; causes, it may be remarked in passing, very unlikely so repeatedly to call into existence the alleged specific contagion, from which alone yellow fever has by some been considered to arise. Instances of the very general suffering of new comers to the West Indies could be easily multiplied, but the fact is too notorious to require further illustration.

Cases of the ordinary endemic fevers, whether the bilious remittent, or continued, are very constantly found existing with yellow fever, in greater or lesser numbers, and up to the appearance of what are considered mortal symptoms; passive hæmorrhages and black vomit; those terminating in this way are either with difficulty, or in some instances not at all distinguishable through one-half, or sometimes three-fifths of their course, viz: the stage of excitement, from many cases of the ordinary fevers.

In what are called sickly seasons in the West Indies, the ordinary fevers are often found to precede, accompany, and follow invasions of yellow fever, and cases of both varieties may be found at the same time, in the same community, and even in the same family, under parallel exposure and other hygienic con-

ditions save one, the unassimilated European constitution; the best test, sometimes the only infallible one, of the presence of causes productive of the bulam; and it is opposed to the general history of medicine and of epidemics that two fevers, up to a certain point of their course, so nearly approximated in symptoms, in their rise, progress, and decline, should be considered as radically dissimilar in their cause and essence.

Yellow fever, and the more severe varieties of fever, in all places north of the tropic of cancer, are as decidedly gastric, as the fevers of the eastern hemisphere are cerebral, in their character; yellowness of surface, hæmorrhages, and often an unclouded intellect are also as common and peculiar to the one, as the very general absence of these are to the others; and it appears to me, though practically warranted, we should be pathologically as little correct, in attempting to draw a line of separation at every unusual and excessive amount of cerebral affection and mortality in the one, (which has ever been attempted) as in excluding from their equally legitimate family, an occasional series of cases remarkable for high gastric disorder, in the other. These are climatic peculiarities not to be transplanted, and furnish, it may be here remarked, a much more intelligible explanation of the restriction of the yellow fever to certain geographical boundaries, than that ships never proceed direct to India from the latitudes of this disease. The practical physician in the East has never sought a separate nosological position for any grade of fever, however differing in form and fatality from that usually prevailing, and nothing, it appears to me, could ever have elevated black vomit fever into a distinct disease, but the circumstance that the doctrine of contagion would not accord with the class of fevers with which it is so obviously allied.

To regard the bulam as a distinct disease is a postulate quite indispensable to carry out the doctrine of Chisholm and his followers; but I question how far it is tenable with those who refer the disease to local causes. Here there is still assigned to it a generic peculiarity, not less decided than that assumed by the contagionists, and not to be maintained without inferring a distinct and separate local cause from that producing the ordinary fevers—a cause as fluctuating and contingent as imported contagion; for although an epidemic, or some superadded influence, may be necessary to account for such wide-spread invasions as those of Spain, etc., we cannot doubt, when we find the civil population entirely free, as in Barbados, in 1847-48, and on several other occasions, that the disease may spring up among new comers from ordinary causes, without the apparent aid of any such influence.

On the supposed universally continued type of the bulam, its more concentrated form as a whole, its more rapid course, and generally greater mortality; the frequently pale colour of the liver, and the termination in black vomit, which has been exalted into a pathologonomonic symptom by some, (all, in my opinion, entirely reconcilable with a more intense febrile action in the description of subject to which, in the West Indies, it is almost peculiar,) chiefly rests on the grounds for the attempted elevation of an occasional grade into a generically different disease.

Black vomit, which must be looked upon as a peculiar termination, not a symptom, for it does not occur till a late period of the disease, is the principal characteristic of the bulam; and the absence of which is sufficient with some to exclude all other forms, however closely approaching it in all the leading features. Thus, as Dr. Bancroft says, "selecting" a form of the disease almost peculiar to the unacclimated, and attempting to assign to the most varied of fevers, which, according to Chisholm, would require the "fidelity of a Claude Lorraine to delineate," a character more defined and circumscribed in its phases and phenomena than plague or typhus; and yet so little specific or uniform is it sometimes in duration and mortality, that after exhibiting its usual symptoms and rate of mortality, we find it changed, as in the ships "Hussar," and

"Chichester," to a comparatively mild disease, marked by a long and uninterrupted succession of recoveries, and that simply by a lower temperature, which has never been known so completely to change small-pox, or any other contagious disease, with which it has without the slightest foundation, in my opinion, been compared.

The appearance of black vomit during life occurs more frequently and in greater quantity in the yellow fever of the West Indies, than in that of Gibraltar; for according to Dr. Gillkrest, of 190 cases of the epidemic of 1828, only 6 vomited this matter during life. The disease altogether would appear to be more rapid and malignant in its course, in the West Indies, than in Europe. The affection of the head, and the irritability of the stomach are occasionally found to alternate to some extent with each other, and where the former predominates much, the black vomit is sometimes absent. Dr. Davy, in a note to Dr. Blair's late work on yellow fever, alludes to an epidemic in Barbados, in 1811, in which the gastric symptoms were very considerable, and the fever at the Island of Edam, described by Dr. James Johnson, which approached the yellow fever in some of its symptoms, may be noticed as another of those occasional deviations from the forms common to certain latitudes.

About the rapidity of convalescence, insisted on by Sir William Pym, there is a great difference of opinion. M. Louis, and the majority, state it to be generally rather tedious in proportion to the severity of the cases, several of which do not exceed a mild synochus. With such cases, I may here remark, occurring as they generally do, after a short residence, the absence of sequelæ, or visceral disease, may be readily understood, and cannot, in my opinion, be considered as any proof of a disease different from the remittent, which is not always a marsh fever, or necessarily followed by tedious convalescence or sequelæ, as stated by Sir William Pym.

The leading symptoms of the bulam, and those of what is called the malignant remittent, closely approximate; they differ in the more persistent and intense character of those of the former collectively; but they are chiefly, and sometimes only distinguished by the occurrence of black vomit among the ultimate phenomena of the bulam; a contingency, or as it has been more happily termed, "the accident of a season," not, in my opinion, essential, or sufficient to disjoin them as radically dissimilar; the cerebro-gastric affection, the yellow suffusion in more or less intensity, the irritability of stomach, suppression of urine, hæmorrhages, with occasional dark-coloured vomiting and dejections, proclaim, in my opinion, the malignant remittent, as being closely allied to the bulam; and like it to be frequently the highest grade, conditional to circumstances of locality, of subject, and degree of cause; concurrent differences in these being equal, as in fever everywhere, to effect endless modifications.

With the exception of a pale, orange yellow, or nutmeg colour, and according to O'Halloran and some others, a dry or exanguinous condition of the liver, appearances by no means constant, there is not a morbid alteration in bulam, that is not found in what the ultra-contagionists would call the malignant remittent, not approximating merely, but in absolute amount, sufficient, in the absence of black vomit in the stomach, to annihilate all means of distinction.

Remittent fever, the most common type in all intertropical regions, is not necessarily the product of marshy, or even of humid localities, it occurs where marsh cannot be suspected, and is not necessarily a grade of, or followed, as some assert, by intermittent fever or ague. On the contrary, the remittent is found in places where ague is never met with; thus showing the fallacy of attempting to connect, as a rule, the type of fever with the surface of the soil, or humidity of a place; Kingston, Up Park Camp, Port Royal, and according Fergusson, the volcanic surface of Bailiffe, in Guadaloupe, often giving rise to the same kind of fever as that at Spanish Town, Falmouth, Montego Bay, and other localities, notoriously paludal in their character. As far as relates

to ague, we are able to connect periodicity in fever to some extent with swamp; but here we must stop; the remittent type being nearly universal in all intertropical countries, whether dry or humid.

In protracted cases of acknowledged black vomit epidemics, distinct remissions are frequently observed; and cases presenting the remittent form frequently precede, accompany, and follow cases of the bulam in the same regiment, under similar hygienic conditions. Without therefore maintaining that the bulam is always of remittent type, it is fairly to be inferred that it is frequently so, and only fails to show remissions from the violence and rapidity of course of the disease.

The form and train of phenomena marking the bulam are often grafted on pure remittent, and intermittent fevers, more especially in hot and humid places —a fact corroborated by nearly all whose field of observation has lain there; and it would be, in my opinion, in the present state of our knowledge, to assign a very subordinate agency to so powerful a febrifacient cause as malaria to call this a complication; it is too constant in some localities to be accidental. On the other hand there are equally accurate unbiassed observers who maintain that bulam is always continued in type, and so strong and irrisistible to me are the facts in support of both views, that I am forced to believe that the type of the bulam varies in different localities, and is not essential to the disease, or, at least, not the production to the ultimate phenomena by which it is characterized.

Dr. Bone, inspector-general of hospitals, who had long and extensive experience in the West Indies, speaks of what he calls the occasional "marsh basis," of yellow fever, and Dr. Chisholm must have observed a good deal of the remittent form in his "Nova Pestis," when he called it a compound of that and typhus. "Lempriére describes a remittent yellow fever." But on this point I consider the following conclusive:

"I have just stated," says Dr. Bartlett, of Transylvania University, in his work on fevers, "yellow fever, like other diseases, prevailing in malarious regions, may sometimes assume something of a periodical character. This subject deserving of further investigation has recently been studied by Dr. Lewis, of Mobile. He has described a form of the disease which he calls remittent, and intermittent, yellow fever. During the epidemic of 1843, at Mobile, simple remittent fevers prevailed extensively in the southern part of the city, mostly among the natives and acclimated part of the population. Dr. Lewis says that he attended in this district of the city, 16 cases of the remittent, or intermittent fever, assuming the rank and grade of yellow fever. These cases were all among the unacclimated. Dr. Lewis estimates the number of these cases, during the epidemic of 1843, at 100; 50 of which terminated fatally. He says the intermittents were more fatal than the remittents. With the exception of the periodical element, the disease in these cases did not differ from the oridinary unmixed forms of yellow fever; it went regularly through its several stages, terminated in its usual manner, and at its usual periods."

"Of 28 cases of fatal intermittent yellow fever, all terminated within the seventh day from the initial chill. Dr. Lewis does not give any full description of these cases, but there is no reason whatever for doubting the correctness of his conclusions. He is a competent and trustworthy observer, and he is in no way influenced in his opinions by preconceived prejudices or notions, since he recognizes, without any qualifications, the essential dissimilarity of periodical and yellow fever. In another paper, Dr. Lewis mentions particularly seven cases occurring in 1842, which he calls congestive, simulating yellow fever. They occurred in persons who had been living in malarious regions, and were marked by the symptoms of congestive and of yellow fever."

Dr. Lewis says:

"The pathological appearances of the congestive fever of the interior, and

the yellow fever of Mobile, were both apparent in these cases, so that taken in connection with the symptoms before death, they constituted a perfect example of the blending together of the different febrile poisons, so as to produce a disease of mixed character.

" Dr. Dickson, formerly of Charleston, South Carolina, now in the University of New York, admits explicitly and distinctly the existence of this modified form of yellow fever :"

" In the summer of 1817," (he says,) " many northern and foreign sailors had been induced to go as boatmen up our rivers. Considerable numbers of them were brought into our hospitals with country fevers, both remittent and intermittent, which as soon as yellow fever became prevalent, ran into that epidemic; the fever becoming continued, and black vomit ensuing."

It is very evident that the causes of yellow fever are present in places of very different character, not only as regards the temperature and humidity, but in vegetation, soil, and the commonly supposed sources of telluric miasm. These differences apparantly lead to marked varieties in the forms of the ordinary fevers, and if these are allied, as I believe they are, to the highest grade or yellow fever, it is questionable how far the disease is everywhere the result of the same cause, as has been insisted on by Bancroft, Fergusson and others, or whether different causes, such as may be supposed to exist in places so different as Gibralrar, Jamaica, Brimstone Hill in St. Kitts, and on board ship, may not in fever as in phlegmasiæ, give rise to similar ultimate effects; for I think nothing can be more certain, than that the most perfect representation of some forms of yellow fever is to be found in the Algid variety of intermittent, a form of fever which Gibraltar is considered incapable of producing.

There are but two uniform and appreciable conditions which we can connect with the eruption of yellow fever in the West Indies—heat and the unassimilated European constitution. All farther attempts to connect the disease with soil, surface, dryness, humidity or sources of malaria, fail to assist us; for wherever soldiers may be placed, whether in the dryest locality, or in a marsh, or under the most favorable circumstances as to barraks and dicipline, they rarely escape the disease.

I have shown that high gastric disorder, is the prominent characteristic of all severe fevers of certain latitudes, as marked cerebral determination forms that of the fevers of Ceylon and the whole continent of India, and the irreconcilable contradictions and anomalies with which the subject of yellow fever is beset, would seem to point to the conclusion that fever once excited, climate, the latitude of that climate, and the other conditions under which it occurs, rather than the continued operation *per se* of any uniform specific cause, tend in some of its more concentrated forms to a common and similar train of ultimate phenomena.

We can scarcely, I think, doubt that efficient and similar elements for the production of fever are to be found in all intertropical countries, and yet, with slight occasional deviations, how constantly it takes on the form common to the latitude in which it occurs; and nothing confirms me so much in the opinion of yellow fever being a grade, as its absence from eastern hemisphere, and its close resemblance in the organs affected to all the severe fevers, especially to those of malaria, in all places of a certain temperature north of the tropic of cancer.

In the absence of European constitution, the bulam form of yellow fever would probably be as unfrequent in the West Indies as in Europe, and that in a much less concentrated form. But the occasional absence of the highest and most fatal grade of a disease, will not disprove the presence of more mitigated forms of that disease, and of the causes on which it depends; and I conceive we may, with equal justice, maintain, that common continued fever and its causes are absent from this country, because we have not at all times

those wide-spread, destructive, and peculiar forms of the disease, which occasionally show themseves, as that yellow fever does not admit of any lower grade than that marked by black vomit.

Fever differs in form, and often in type, in different islands of the West Indies, and in different localities of the same island, and the features of one are so often blended with those of another, as almost to defy classification, proving the truth of Dr. Percival's remark, "that those who are most familiar with the aspect of fever, on the large scale, will be least disposed to subdivide it into genera."

In 1834 in Malta, the regiments in garrison had more than the usual number of fevers, all of a remittent character, and in two of them from 20 to 30 cases occurred, presenting great irritability of stomach, yellowness of surface, hæmorrhages, suppression of urine, and death in several cases. In the other three corps, there was not a case of this kind, nor a single death. Now it will be contended that these cases were a totally different disease from that affecting the majority, not only of the garrison, but the majority of the two regiments in which these peculiar cases occurred. In the late epidemic in Barbados, the 72nd regiment was supposed to be free from black vomit fever for seven months; while two other corps were suffering from that form of disease in the same garrison, because, during that period, they had only a few cases monthly, of a mild fever, without a death. Now I ask where is the proof that these mild cases were not identical with the fever, which, from a more intense degree of the same cause, was proving fatal in other regiments. The symptoms essential to fever are few, and the bulam, according to Sir William Pym, cannot, in the beginning be distinguished. The absence therefore of the ultimate phenomena and death, which depend on adventitious causes of agravation, will by no means disprove the identity of the two fevers; and I am the more disposed to this opinion from the circumstance, in many of the epidemics of yellow fever, of cases of great malignancy, coming from certain suspected places, or crowded rooms, and mild ones from others, as if inconsiderale local differences were sufficient to exalt a very mild fever into the most concentrated and fatal.

With a knowledge of the kind of subjects in whom the bulam, and the remittent, occur respectively, are we to include in the category of the former, those cases only, that by peculiarity of constitution, or greater intensity of cause, occasionally stand out in relief by increased mortality and a single superadded hæmorrhage? I think not. No disease, Dr. Gillkrest says, has a wider range in symptom, and none, I may add, is less uniform or determinate in its attributes generally, than yellow fever; the epidemics of different years, and in different places, and the same epidemic at different periods, varying considerably, not only in symptoms, in degree of malignancy, and rate of mortality but even in form and type. This has been more especially exemplified in some of the invasions of the disease in Jamaica, and the Mediterranean, in which last place, at the outset of epidemics, great diversity of opinion has prevailed as to the appearance of any new disease, and which, at length, seemed to be decided by the increasing number of cases, rather than by any marked difference, for a time, from the ordinary fevers. This, while showing the similarity of the disease with the ordinary fevers, proves a much greater capability of modification and grade than some will admit it to possess.

The year 1825. one of universal sickness in Jamaica, as well among the military as the civil population, offers much deserving of notice, not only as to the varied character of yellow fever, and its close approximation at times to the remittent form, but the modifications it undergoes by locality, length of residence, and weather.

The 77th Regiment stationed at Stoney Hill, and 11 months in the island, became sickly in the beginning of February, and the fever is described at this period by Dr. Richardson, as follows:

"The first symptoms of the disease were violent headache, commonly confined to the fore part of the head; severe pains in the lower part of the back and loins, and also in the limbs, especially in the calves of the legs; great prostration of strength, anxiety, restlessness, and not unfrequently nausea and retching, in some patients of a colourless, in others of a greenish fluid.

"The skin was hot, face flushed, eyes florid, *but not watery*, nor *having the peculiar expression so remarkable in the disease some months after;* pulse pretty full and frequent, from 96 to 120. Hitherto the appearance of the tongue was various, in some patients it was clean and florid from the commencement, in others covered with whitish or yellowish mucous: the bowels usually constipated, the urine highly coloured and scanty, but in some quite limpid and inodorous.

"After the first 12 or 14 hours there was generally an amendment of all the symptoms, or more properly speaking, *a considerable remission*, but this was of short duration. An *exacerbation* of fever soon followed with a return of headache, but not severe after the first paroxysm was over.

"The worst symptom, if it had not been present from the beginning of the disease, now supervened, viz: irritability of the stomach, and unless the febrile symptoms were again speedily relieved and subdued, this symptom rapidly advanced till disorganization of the stomach was effected, when the case soon terminated in black vomit.

"I have already stated that, the first case of the epidemic was admitted into hospital on the 1st February, and from that to the 21st of the month, the number of admissions amounted to 35, of which number seven died. During this period the disease was one of considerable excitement and of *pure remittent type.*

"On the 22d February seven cases of fever were admitted into hospital, all of them very bad, and from this date up to the 26th March, the admissions continued at the rate of six a-day on an average, so that in the short space of 33 days we had admitted 198 men into hospital, besides an equal proportion of officers, women and children.

"During this period the epidemic continued to preserve the *remittent type,* but the remissions gradually became shorter and less distinct as the dry season advanced, and the symptoms *progressively* assumed a more dangerous character.

"The fever now was not one of excitement, but quite the contrary, great nervous depression marked its commencement and progress, and it was evident the first influence of the miasmata was exerted directly upon the nervous system. The attack became more sudden, and it was no unusual occurrence for a man who felt perfectly well on going to bed, or in the morning at breakfast, to be prostrate with the disease in less than half an hour."

Here Dr. Richardson gives an admirable description of the symptoms usually attending the most aggravated form of the disease, and continues as follows:

"The pulse, in some instances, I have found preternaturally slow, but the generally state of the pulse, on admission, was small, soft, and frequent, numbering from 100 to 130, but rarely giving much resistance to the finger on pressure.

"The tongue in the worst cases was clean and dry, but in others rather moist and covered with whitish or yellowish mucous towards the centre.

* * * "A general torpor and derangement pervaded the secretory organs and membranous structure, especially of the stomach, where a disorganizing principle was always apparent, and indicated by a painful burning sensation in that organ at an early period of the disease.

"Irritability of the stomach is a very common symptom in endemic fevers, in that, under review, it was an invariable attendant.

"The proportion of mortality during this period was one in four of the number admitted.

"From 26th March to 10th May, 80 cases were admitted; the mortality in these admissions was 21.

"While the dry weather continued the epidemic gradually increased in violence, becoming more of the continued type; the heat more ardent; pulse weaker and more frequent; a greater degree of debility from the commencement of the disease; * * * * the tendency to the dissolution of the stomach greater, which was indicated by the state of the tongue early assuming a leaden, scammony colour, and gradually changed to black. Hæmorrhage from every orifice was *now* a frequent symytom, and very often carried the patient off when fever had been subdued. The disease was still more rapid in its progress, and the system could, with more difficulty, be affected by mercury.

"About the middle of April the rains began to fall, and increasing about the middle of May, the epidemic disappeared, and from the 11th of the month to the 1st of June, there was not a death in the hospital at Stoney Hill.

"From the 1st to the 18th July, there were only four deaths from fever, so that from the 11th May to 18th July, the deaths from fever only amounted to eight, and two from dysentery, thus proving the salutary effects of rain at Stoney Hill, and it was not until after the continuance of six weeks dry weather, that we began to be sickly again.

"The number of admissions from 18th July to 8th September, (of fever) amounted to about 200, of which number 57 died, the greatest proportion of mortality experienced throughout the year, During the month of August the disease was of a more ardent character, and of continued type; * * * the disease now ran its course more rapidly, and was more uncontrollable than at any former period."

The above will show how little defined yellow fever is, either in grade, symptom, type, duration, or rate of mortality, and the power of weather and season to modify, arrest, and renew, its virulence. I will now allude to the sickness in the 33d and 50th Regiments in the same year, the former three, and the latter six years, in the island,

Dr. King, acting surgeon, 33d Regiment, states—

"Five hundred and fifty-five cases of bilious remittent have been treated since the 20th December, 1824, of which number 66 died, making an average of 1 in 8:4. Since the 33d disembarked at Port Henderson on 29th September last, no fewer than 223 cases have been treated in the hospital at Spanish Town, and of which 43 have died, making a proportion of 1 in 5¼,

The disease generally assumed more the appearance of typhus than usual, and exhibited all the varieties of intermittent fever. A remarkable difference might be observed when quartered on the north side. There the disease was frequently combined with arterial excitement, and ushered in with general synocha, whilst, on the contrary, the fever at Spanish Town, from the very beginning, assumed a typhoid type. There was, however, great variety, both in its progress and mode of attack. Two forms of fever might very often be observed, the tertian, the quartan, and sometimes quotidian, but the usual form was bilious remittent.

"When the remissions became less remarkable, the febrile anxiety and restlessness increased; the patient is unable to sleep and complains of thirst, which nothing will allay. Either vomiting or delirium was to be dreaded, and it is difficult to decide which is the most unfavorable, when the latter was accompanied by fever, it was generally a less fatal symptom.

"The paroxysms generally ran into each other, and the disease assumed the form of continued fever, the symptoms all becoming aggravated. In this stage delirium, or coma, generally set in, and the delirium frequently became so furious, that it was found necessary to confine the patient in bed. The skin in such cases was dry and hot; pulse small and very quick; tongue hard,

dry and brown, or covered with a black tenacious crust, occasionally red, resembling beef-steak."

"When the stomach was affected, which occurred in the greater number of cases, vomiting became excessive, and everything was immediately rejected, whether in the shape of nutriment or medicine; the skin assumes a dark brownish hue, afterwards changes to a livid or blue color. These fatal cases have taken place every day, from the third and fourth, to the eighth and fourteenth day, but most commonly occurred between the fifth and eighth day."

The 50th regiment at Spanish Town, six years in the island, which had suffered from the black vomit variety, a few months after their arrival in 1819, at this time labored under a somewhat similar fever to that in the 33rd, and in a strength of five companies, had 378 cases, and 80 deaths, which generally occurred from the fifth to the eighth day.

The period of residence of these several regiments, implying a different constitutional susceptibility, and disease of organs, consequent upon residence and former fevers, will, I think, sufficiently explain the modification in their respective diseases, without supposing a separate and distinct cause. For none but those who contend for this, knowing the almost uniform eruption of black vomit fever, among newly arrived regiments in Jamaica, will doubt that had the 77th been placed in either of the stations of the 33rd and 50th this year, the same description of fever would have assailed them as that from which they suffered at Stoney Hill. In one we have the dissolved blood pouring itself out in hæmorrhages and black vomit, leading, as has been suggested to paleness of the liver, and an apparently normal condition of the spleen; in the other, we find both these organs dark, engorged, and enlarged with "the portal vessels distended with dark fluid blood," differences taken with the symptoms during life, which the pathologist will be able to interpret and reconcile as very intelligible modifications of the same disease. In the one we have a more dissolved state of the blood, or a less arrest of it in diseased organs, giving rise to hæmorrhage "from every orifice;" in the other we find the circulation struggling to the same termination, but as in the malignant remittent generally, more determined to the brain; which in the bulam is sometimes very little disturbed, the stomach appeared to be generally the centre of the mischief. The predominant affection of the one or the other of these organs, not only occasions marked differences in the fevers of the West Indies; but constitutes, according as it may prevail, the great leading difference in all intertropical fevers, which will be found to approach to, or recede from, the character common to the country in which they occur, in correspondence with this more than from, in my opinion, any essential difference in their nature.

We have here, in the fever of the 77th regiment, an alleged specific, defined and contagious disease, varying from excitement to great nervous disturbance, from the remittent to the continued type, from symptoms differing little from those of the endemic remittent, to the most concentrated form, and that apparently by increasing heat and changes of weather, which may modify, but can never, as here, so change the character and arrest the progress of a distinct and contagious disease. The advocates for the distinct nature of the bulam, may contend there were here two diseases; but the transition from the one to the other, was two gradual; the susceptibity of subject, and other conditions too much on a par, and the influence of atmospheric causes too palpable to give any support to such a notion. But this is only one of the many instances of the remittent becoming exalted to the bulam; for though the latter frequently bursts forth at once in its utmost malignancy, there are other occasions in which it seems gradually to rise from a very moderate continued, or remittent fever, to one of great intensity. Cases of the ordinary fevers are now, and again observed in the most malignant epidemics of the bulam. In the year 1827, when the 22nd regiment, at Stony Hill, in Jamaica, lost 122

men and 7 officers, in about two months, by black vomit fever, which attacked them about eight months after their arrival; "the fever," says Surgeon Owwen "was sometimes prolonged to 10 or 12 days, but considerale remissions took place in these cases; relapses were very frequent, and generally fatal." I notice these cases as from their occurrence in black vomit endemics, with fresh subjects, they are less open to question; but the fact of the frequent remittent character of the bulam, in many instances, is too generally confirmed by medical officers, who have seen the disease in the more humid islands, to leave any doubt of its more than accidental connection with the ordinary fevers, and their causes.

I believe the causes of the aggravation and increase of the ordinary fevers, and their assumption of the bulam form to be always an epidemic constitution of atmosphere in the Mediterranean, and the northern States of America, assisted by malaria, in its extended sense; and these equally obtain in the West Indies, and all other places, as far as relates to the native and other acclimated residents; deviations from this depending for the most part, not on the arrival of ships, as is alleged by the ultra-contagionist, but on a more intelligible contingency, the presence of the unassimilated European, under some excess to him at least, in the ordinary causes of disease.

I believe *marsh* miasma not to be an essential element in the production of yellow fever, which would appear to originate from some increase in the ordinary febrific causes of whatever kind, and to obey the influence of climate, constitution, and the other conditions under which it may happen, rather than the action of any single, uniform or specific cause in other words, that continued, remittent, and sometimes intermittent fevers may become exalted to the yellow fever, and derive their resemblance to each other, not from any peculiar exciting cause, but from the tendency of all intense febrile action, to take on the characteristic affection of organs common to the fevers of the latitudes in which they occur.

I believe the bulam to have no specific character, or pathognomic symptom, not to be defined in its course, duration or other attributes; but an occasional variety of a numerous and protean class of fevers, continued, remittent, and intermittent of certain latitudes; and to differ from these only in violence, rapidity of course, and ultimate phenomena; its apparent causes, its leading and essential symptoms, and pathological conditions, with the periods of its rise, acme, and decline, declaring it to be like them, a variety of the same genus, which I am convinced however practically useful, cannot with any pathological correctness, or hope of reconciling the conflicting facts in the history of tropical fevers, admit of subdivision on any discoverable essential difference in its multiform species.

It may be thought I have generalized too much; I have endeavored to avoid it, but the facts will fully bear me out. I cannot regard the bulam as a distinct disease; for thongh it differs in some points from the ordinary fevers, its close approximation in some of the leading and more essential symptoms and morbid lesions, with the strong collateral evidence in favor of its identity with these in nature and in cause, force me to the conclusion that they cannot be disjoined as fundamentally different diseases. It is true we often see the bulam so isolated and apparently so unconnected with the ordinary fevers, as to give to it the appearance of a distinct disease; but on the other hand, how frequently its outset, progress, and decline, marked by cases of these, so nearly approximating to it, as to defy any attempt to say where the one ends and the other begins, or on any just grounds to point to a different cause.

(*Report of the General Board of Health of London.*)

[*To be contiuued.*]

The New-Orleans Medical and Surgical Journal.

Vol. X.] NEW-ORLEANS, SEPTEMBER 1, 1853. [No. 2.

HEALTH, MORTALITY, &c.

About the 26th of May last, the first case of yellow fever entered the Charity Hospital, and after death black vomit was found in the stomach. The first fever cases originated among the shipping along the Levee, in the Fourth District, from which point it extended rapidly through the adjacent portion of the town. A large population of unacclimated persons, living in wooden huts, with floors and timbers soaked in water, and half decayed, were seized with the disease in the most malignant form. For some time previously rain had fallen almost daily, and this added to a hot, burning sun, seemed to give strength to the poison, and lent intensity to the disease. The streets in this vicinity, for the most part, were unpaved, or planked, and the culverts, gutters, etc., were filled with water, saturated with filth and decaying vegetable and animal matter. The crowded state of these huts and low wooden tenements, with their floors steeped in mud and water, is admirably calculated to generate and propogate the germ of a disease which had already been sown in their midst.

The habits of these people, (being chiefly Irish and German laborers,) notoriously negligent and filthy, and utterly indifferent to all those precautionary measures which a limited knowledge of the laws of hygiene should suggest, served only to add fuel to the conflagration which was destined to extend its ravages to every portion of our devoted city. Hence, for some time, the yellow fever confined its work of death within particular localities,—but by and by gaining strength by what it fed upon, it began to travel to other and more distant points,—to extend it arms, so to speak, in every direction, until it grasped the Four Districts within its deadly embrace. For some time the hope was entertained that those who paid proper regard to personal comfort and cleanliness—who dwelt in high, airy, and well-ventilated apartments might escape the disease; but this proved a delusion,—it soon became apparent that as heretofore, the epidemic fever was no respecter of persons,—the master was stricken down with the servant—the mistress with the maid—the proud and wealthy were brought to a level with the humble and needy. All who had not passed through some one of our epidemic seasons were exposed to attacks from the disease. As has been already mentioned, the fever made its appearance in the latter part of May, at least one month and a half earlier than usual, and from the first case up to the present, it steadily increased almost daily, until the mortality per diem exceeded that produced by any epidemic known in the annals of our sanitary history. In recording the fearful ravages of the present epidemic we must not forget that we have remained exempt from any such visitation since 1847, and during this time an immense population of unacclimated persons, both from Europe and the north-western part of our own country, have

been accumulating in our city. The number of unacclimated persons in the city, at the breaking out of the epidemic has been estimated at 30,000 souls; but many of these, it is fair to suppose, have left the city to escape the disease.

The type of the epidemic differs but little from that to which we have been subject in former years; and the belief that persons had died of the disease in six and eight hours from the moment of seizure, can readily be explained by a better knowledge of the antecedent history of the case; for on inquiry it would generally be found that such inividuals have had slight fever and other symptoms of the epidemic for two or three days previously to taking bed and calling in medical aid. This surmise gains additional strength from the fact that the attack, in many instances, has been so insiduous and destitute of alarming symptoms, that it is with difficulty such persons could be persuaded—could be prevailed upon to submit to the usual restrictive treatment.

It is not strange therefore that such cases, which had been neglected for two or three days, in the early and curable stage of attack, should terminate in fatal black vomit, in a few hours after the physician is summoned to the bedside of his patient. So much for the apparent malignancy of the preseut epidemic. In making the foregoing explanation, we aim not to deny the existence of an occasional case of extreme severity; so severe indeed as to terminate in death in a few hours, in spite of the best efforts of the most skillful physician and the most careful nursing.

In some instances the system seems thoroughly saturated with the poison of the disease, from the very moment of seizure, that no system of medication, as yet suggested, seems able to cope with and stay the fatal tendency of the fever. Every medical man who has had much experience in the disease, must remember occasional instances of this kind.

The disease this season, though essentially the same in many of its most prominent features, exacts perhaps, on the part of physician and nurse, more care, diligence and precaution, to terminate favorably, than usual in our epidemics. The slightest imprudence, either in diet, exposure, or excitement of any kind, is almost certain to superinduce a relapse—from which state it is usually very difficult to extricate the patient. Hence, the great mortality among those who are not only ignorant of the peculiarities of the disease; but who are also unable, and in some instances unwilling to pay for the requisite medical aid and attendance.

We refer to our table below, furnished by Dr. Simonds, the active Secretary of the Board of Health, for a full account of the deaths and other particulars which have occurred since the epidemic broke out. By this, it will be seen that yellow fever has done terrible execution among our unacclimated population; has produced a mortality unparalleled in the history of our ill-fated city. Even while penning these lines, the fever is sweeping off over *two hundred per diem*—and from present appearances, it is likely to continue its fearful ravages for, perhaps, weeks to come.

Our quondam asssociate, Dr. Fenner will, in due time, give us a full and detailed history of this epidemic, as he did that of 1847, when the disease shall have run its course and done its work of death.

Below, we give the mortality produced by the epidemic, in the city of New Orleans, from the 28th May up to the 26th August, inclusive, for 1853:

THE EPIDEMIC.

Total number of death by yellow fever and other diseases, from May 28 till date:

Week ending.	*Total.*	*Yellow Fever.*	*Other Dis.*	*Not stated.*
May 28	140—110	1—1	139—139	...
June 4	157	1	156	...
... 11	154	4	150	...
... 18	147	7	140	...
... 25	167—625	9—21	158—604	...
July 2	177	25	152	...
... 9	188	59	129	...
... 16	344	204	140	...
... 23	617	435	182	...
... 31	884—2210	704—1427	138—741	42
August 1	142	106	25	11
... 2	135	115	14	6
... 3	146	124	17	9
... 4	166	135	15	10
... 5	150	128	9	13
... 6	238	194	30	14
... 7	209—1186	165—967	40—150	4—69
... 8	219	187	23	9
... 9	201	166	21	14
... 10	230	193	33	4
... 11	233	192	13	18
... 12	207	180	25	2
... 13	214	179	22	13
... 14	232—1526	191—1288	26—163	16—75
... 15	217	187	24	6
... 16	193	163	19	11
... 17	219	191	21	7
... 18	219	188	22	9
... 19	234	203	15	16
... 20	224	184	29	11
... 21	269—1575	230—1346	24—154	15—75
... 22	283	239	29	15
... 23	258	220	24	14
... 24	222	188	23	11
... 25	218	186	19	13
... 26	193—1074	151—884	29—124	13—66
Total	8336	5934	2075	327

N. B—The returns from the St. Patrick's Cemetery since the 31st July, not having been duly made, cannot be relied on, except for two weeks, when the books were resorted to by the Secretary, to enable him to make a weekly report.

OURSELVES.

Our readers must bear with us for any short comings in this number of our Journal; our attention has been given for the most part, to those stricken down by the epidemic during the last six weeks; and we have had neither the time nor that calmness of spirit which would enable us to collect and arrange our thoughts for publication. Our energies, both of body and mind, have been taxed to the utmost of their power and endurance, in combatting this terrible epidemic; often for hours and hours has sleep, so necessary to our well-being, been denied us; and when a few moments of leisure were vouchsafed unto us, the mind was too weary, and the heart too sad to perform our editorial duties. Need we say more by way of apology on this subject; nevertheless, we think this number possesses a good deal of interesting matter. We shall do better next time.

MEDICAL COLLEGES.

As the Lecture Season will soon be at hand, we would invite the attention of Medical Students to the circulars of some of our most flourishing Southern Medical Schools. It is no longer contended that a knowledge of medicine can not be acquired in the South; as the experience of the last few years has clearly demonstrated the great advantage to be derived from acquiring a practical knowledge of physic and diseases, in the region of country where the young graduate may determine to establish himself in practice. Among this class of Medical Schools, may be ranked the *Memphis Medical College—the Medical Department of the University of Louisiana—the Georgia Medical College—and the Medical College of South Carolina.*

All these schools have able, talented and experienced teachers; under whose instruction, Students of Medicine may be well grounded in the principles and science of the healing art.

New Orleans, August, 1853.

Sir—The Committee of the "*Louisiana State Medical Society on Practical Medicine,*" desirous of obtaining all possible information on the subject within the limits of their special duty, would be gratified to receive from you, on or before the first of January next, any communication with which you may honor them; especially your observations and views, and treatment of *yellow fever.*

Very respectfully,

Your obedient servant,

J. B. M'KELVEY, M. D., Chairman.

NECROLOGY.

Died, in this city, of the prevailing epidemic, the following gentlemen belonging to the medical profession :

Dr. A. R. Nye, aged about 26 years, a native of New York.

Dr. Jacobson, aged 40 years, late of St. Louis, Missouri.

Dr. A. C. Robertson, aged 23 years, a native of Nashville, Tenn.

Dr. Friend, aged — years, a native of Petersburg, Va.

QUININE IN YELLOW FEVER.

Our experience during the present epidemic, with the Sulphate of Quinine, has convinced us that large doses of this salt can not be relied on in the early stages of the attack.

In the commencement of the epidemic, the advocates of large doses of quinine soon found that this article, when given in sedative doses, failed to accomplish a cure, although the febrile symptoms gradually gave way to its use.

As the epidemic progressed, and its type and characteristic symptoms became better known, few, as far as we can learn, ventured to give large and repeated doses of this salt, except in particular instances. In our previous epidemic of Yellow Fever, the quinine practice succeeded best ; but it is generally conceded, as far as we could ascertain, that this season it failed in a majority of cases to sustain its previous high reputation—as a powerful curative agent—hereafter, we shall have more to say on this subject.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1853.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West Greenwich.

WEEKLY. 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days.	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
June 23	92.5	76.0	16 5	30.15	30.10	0.05	SE.	2.15	4	0.765
" 30	92.0	74.0	18.0	30.20	30.15	0.05	E.	2.33	3	0.470
July 7	89.5	74.0	15.5	30.14	30.10	0.04	W.	2.72	7	3.765
" 14	88.5	73.0	15.5	30.20	30.14	0.06	SE.	2.80	7	2.880
" 21	93.0	76.0	17.0	30.25	30.10	0.15	S.	2.14	3	1.575
" 28	87.0	74.0	13.0	30.23	30.10	0.13	NE.	2.40	7	3.155
August 4	93.0	75.0	18.0	30.23	30.10	0.13	NE.	2.00	4	0.805
" 11	94.0	77.0	17.0	30.18	30.10	0.08	E.	2.14	5	2.200
" 18	93.0	74.0	19.0	30.18	29.98	0.20	NW.	2.30	2	0.675
" 25	94.0	77.0	17.0	30.15	30.00	0.15	S.	2.40	2	0.210

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For June and July, 1853.

	SEX.	JUNE.		JULY.	
ADMISSIONS -	Males	816		1412	
Do. - -	Females	311		684	
			1127		2096
DISCHARGES -	Males	635		1005	
Do. - -	Females	250		000	
			885		*
DEATHS - -	Males	87		538	
Do. - -	Females	31		181	
			118		719
BIRTHS - -	Males	5		6	
Do. - -	Females	4		5	
STILL-BORN -		1		3	
			10		14

H. VANDERLINDEN, Clerk.

* No report made by the Assistant-Clerk.

THE

NEW-ORLEANS MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1853.

A. HESTER, M. D.,

EDITOR AND PROPRIETOR.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*

N. O. Charity Hospital.

NEW-ORLEANS:
PRINTED BY JOSEPH COHN, 31 POYDRAS STREET.
1853.

TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

We have received for our January Number, a valuable and elaborate paper from Dr. Ames of Alabama.

Since our last, we have received the following books, circulars, pamphlets, etc., etc., for review:

Transactions of the Tennesse Medical Society, at their 24th annual session, convened at Nashville, May 4th, 1853.

Report on the Geology of the Lake Superior Land District. By J. W. Foster and J. D. Whitney, United States Geologists. Washington, 1851; with a map. Favor of Hon. J. P. Benjamin.

Prize Essay, read before the Ohio State Medical Society, at its 8th annual meeting, in Dayton, June, 1853. By Samuel G. Armor, M. D. 1853.

Proceedings of the American Pharmaceutical Association, at the annual meeting, held in Boston, August 24th, 25th and 26th, 1853. Philadelphia, 1853.

The Physicians' Visiting List, Diary and Book of Engagements, for 1854. Philadelphia; Lindsay & Blackiston. From T. L. White, Canal street, New Orleans.

Hallucinations, or the Rational History of Apparitions, Visions, Dreams, Ectasy, Magnetism and Somnambulism. By A. Brierre de Boismont, Docteur in Medicine, de la Faculté de Paris, Directeur d'un Establishment a Aliénés, Chevalier des Ordres de la Legion d'Honeur, et du Mérite M Militaire, de Pologne, Lauréat de l'Institute, et de l'Academie Nationale de Medicine, membre de plusieurs societies, savants, etc. First American, from the second enlarged and improved Paris edition. Philadelphia; Lindsay & Blackiston, 1853. From L. White, Canal street, New Orleans.

The Maternal Management of Children, in Health and Disease. By Thomas Bull, M.D., Member of the Royal College of Physicians, Author of Hints to Mothers, for the Management of their Health during Pregnancy, and the Lying-in Room. Philadelphia; Lindsay & Blackiston, 1853. T. L. White, 105 Canal street.

The Microscopists; or, a Complete Manual of the Use of the Microscope; for Physicians, Students, and all Lovers of Natural Science. Second edition, improved and enlarged, with illustrations. By Joseph H. Wythes, M.D. Philadelphia; Lindsay & Blackiston. London; Trabner & Co., 1853. T. L. White, 105 Canal street, New Orleans.

A Treatise on Operative Opthalmic Surgery. By H. Haynes Walton, Fellow of the Royal College of Surgeons, England, Surgeon to the Central London Opthalmic Hospital, and Assistant-Surgeon to St. Mary's Hospital. First American, from the first London edition; illustrated by 169 engravings on wood. Edited by S. Littell, M. D., Author of the Manual on Disease of the Eye, Surgeon to Wills Hospital for the Eye and Limb; Fellow of the College of Physicians, of Philadelphia, etc. Philadelphia; Lindsay & Blackiston, 1853. T. L. White, 105 Canal street, New Orleans.

A Practical Treatise on the Diseases of Children. By J. Forsyth Meigs, M.D., Lecturer on the Practice of Medicine, in the Philadelphia Medical Association; Fellow of the College of Physicians, of Philadelphia; Member of the Academy of Natural Sciences, of Philadelphia, and of the American Philosophical Society. Second edition, revised and enlarged. Philadelphia; Lindsay & Blackiston, 1853. All from the publishers, T. L. White, 105 Canal street, New Orleans.

A Treatise on the Eclectic Southern Practice of Medicine. By J. Cam. Massie, M. D. "*He who does his best, however little, is always to be distinguished from him who does nothing.—Dr. Johnson.* Philadelphia; Thos. Cowperthwait & Co., No. 253 Market street. New Orleans; J. B. Steel & Co. St. Louis; A. H. Shultz & Co. 1854. 716 pages. From the Author.

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1853.

Part First.

ORIGINAL COMMUNICATIONS.

I.—HINTS ON THE FEVERS OF THE MISSISSIPPI VALLEY.

BY THOMAS D. MITCHELL, M.D.

Professor of Theoretical and Practical Medicine, in Ky. School of Medicine.

Mr. Editor—I am pleased to find so much interest awakened in the professional mind of the South, on the subject of fever, and will be disappointed if the discussion fail to induce most felicitous results.

Whether we shall ever acquire a certain knowledge, absolutely infallible, touching the essential nature of the calamities, usually designated *fevers*, is a question of less moment than some seem to think. But it is of vast magnitude for the present and for future generations, to arrive at just conclusions in respect of several points, about which medical men are not at present agreed. Thus it is, in my judgment, of great moment, that we ascertain, if practicable, whether, (in the South and South-West at least,) all our fevers, such as intermittent, remittent and continued, are not of *common origin*. As I employ the term *continued*, only in conformity to custom, let me say, that I include in it all the shades of disease commonly called, at present, typhus and typhoid fevers, while in the remittent class, I embrace the various forms of

congestive and yellow fever, such as we find in New Orleans, Mobile and other places.

Are the fevers just named, of common origin? Are they found at any season to prevail in the same locality, and to be in truth, the grand endemics of the soil?

The next quere for solution is this: Are the fevers named, *convertible into each other?* Is there any evidence, ancient and modern, sufficient to establish the doctrine of convertibility? This is a very weighty problem. If the doctrine be real, founded in fact, and therefore not to be gainsayed, it fixes, I think, that other doctrine named above, and viz: the *common origin* of our fevers. A very short course of reasoning will make this probable at least, or I am greatly mistaken. Small-pox and measles never have been and cannot be interchanged; in other words, the measles, in no case, can pass into small-pox, nor small-pox into measles. Each retains its own character, and we infer that each has, therefore, its own causation. But while this has been the doctrine from the days of Hippocrates, and will be to the end of time, it is not so in respect to the fevers to which we have referred. The best of our old writers talk of continued fevers passing into open remittent and intermittent forms, while they tell us also, that intermittents run into remittent and continued fevers. What is all this but the identical position of the ablest men of modern times? As belonging to the former class, we name Pinkard and Lempriere; while among the latter, we find Lewis of Mobile, and Fenner of New Orleans, holding prominent places. Who that has read the simple statements of Lewis, in respect of patients seized with chill and fever, and who died in 30 or 40 hours with true black vomit, of yellow fever, can fail to perceive a substantial repetition of the teachings of a score of older writers on the same great subject?

The *specific* nature of *yellow* fever has been contended for, more pertinaciously than the specific quality of what some have been pleased to call *typhoid* fever. But if yellow fever can possibly be the termination of an intermtttent, or if in any case, it can pass off favorably by assuming the intermittent form, it follows as necessarily as the relation of effect to cause, that these fevers are convertible, consequently in no sense *specific*, and the irresistible inference is, that they have a *common* origin.

I have held for many years, and taught as long ago as 1840, the doctrines hinted at above; not as a partisan, bent on clining to a favorite position right or wrong, but as a patient observer, desirous of knowing the truth, and then resolved to maintain it. It seemed to me, most

rational and philosophical, to refer all the prevailing fevers of our country to a common cause, in view of their histories as presented in the journals for half a century, even independently of the doctrine of convertibility. But when the evidence of the reality and power of the last named doctrine accumulated at every step of my inquiries, it would have been treason to the cause of truth, to have changed my position. I am therefore, and desire that the profession may know it, a teacher of these views in the Kentucky School of Medicine, as I had previously been in the Philadelphia College, and in the Transylvania University. My manuscript on the fevers of America has about sixty pages of clear and palpable proofs of the doctrines contended for, taken from the best works in the profession, both of former times and of the present era. And I venture the prediction here, that the reports of the hospitals of New Orleans, to be made up at the close of this year, will every one of them show, that while yellow fever has been doing the work of death most terribly, there have been cases of intermittent, remittent, bilious, congestive, typhoid and typhus fevers at the same time, in the same locality. Having carefully examined former reports, I may say, that this association of fevers, said by some to be distinct and specific, has been realized in previous years, in the city of New Orleans, and may therefore be anticipated again. Nor is this peculiar to the Crescent city, having been recorded over and over again, as a feature in the history of fevers that met the eye of the most acute observers the world has ever seen.

The third position which I regard as important, is a necessary result of those before referred to. It is, that as a general rule, all the fevers named, may be cut short or cured by the same kind of treatment. This doctrine was particularly defended in a paper prepared for the East Tennessee Medical Society, in 1845 or '46, and which was published in this Journal. All the fevers being viewed as of common origin, and convertible into each other, were supposed to be curable by the same agency. This aspect of the entire subject is yet entertained, because it is believed to be strictly true. We never affirmed that all cases of these fevers of common origin were curable, and might be cured by the same treatment. Our teachings has been, that every one of the varieties of fever named, has changed its features so frequently as to require some alteration in the general management; and that there may be such lesions in the system, unseen or unsuspected perhaps, as to prevent the usual efficacy of the remedial process. Who doubts this doctrine in respect to scarlatina? That is held to be a specific disease, and is known to assnme a mild form, requiring almost no treatment,

and a malignant character, putting the entire powers of medicine at defiance. But who pretends that these opposite varieties constitute two distinct diseases? The world over, it is agreed, that the mildest scarlatina, is scarlatina still, just as certainly as is the fatal form, usually termed malignant. Moreover, we affirm, that there is not a more palpable disparity between any two of the fevers named above, than any practitioner of 20 years experience has been compelled to witness in respect to scarlatina. The argument for the specific nature of yellow or typhoid fever, as contrasted with intermittents, is just as valid as it would be to affirm that the extremes of scarlet fever make separate and distinct diseases.

Now we affirm, on the basis of abundant testimony, quite as reliable as any that can be adduced to the contrary, that intermittents, remittents, congestive, yellow, typhus and typhoid fevers have been and are cut short and cured by the same agency. Nor do we trust to every mere novice in the profession who may have furnished a case, the real nature of which he may have overlooked, in his zeal to uphold a theory. On the contrary, we rely on the testimony of men of standing and experience, strangers to each other, born and resident in different portions of the world, and who could not have been under the influence of any wrong motive, to the utterance of their sentiments. In short, our dependence is not on fiction but on facts, reported with such apparent candor and fidelity, that we dare not reject them—and will not.

The curability of all the fevers we have named, by one and the same agency, is not a question of opinion merely, but one of fact. In a court of law as well as in a court of public sentiment, negative testimony, however creditable, can avail nothing to set aside the positive evidence of equally respectable witnesses. Twenty men may swear that they did not see *A* inflict a fatal wound on *B*, and their declarations will not be controverted. But if two men of equal standing in society swear positively that they were eye-witnesses of the whole affair, that they saw the weapon raised, the blow given, and the man fall, who does not know that their testimony will decide the case? Now, we admit, that the gentlemen who have recently affirmed their want of success in the use of the sulphate of quinine, in the treatment of what are called typhoid fevers, have told the truth. But if their number, instead of being small, amounted to many scores, it would avail nought in deciding the question at issue. The positive declarations of most respectable physicians in Great Britain, France, Germany, the West Indies, and in this country, and the statistics furnished by them, showing that yellow fever and typhoid fever have been cut short and cured

by the salt of quinine, over and again, are sufficient and more than enough to neutralize and nullify all the negative testimony yet offered, or that may hereafter be presented. If this be not the fair, common sense view of the matter, we confess we are wholly incompetent to analyze the problem.

May we not learn something valuable from the history of some of the other forms of fever named in this article? Was there not a time when Peruvian bark was held to be injurious to a patient laboring under a common intermittent, unless given at a certain time; and has not the same difficulty been raised in respect of the sulphate of quinine? But who is so ignorant of our medical literature, as not to know that intermittents have been prevented by the anticipating use of bark and its salts, that they have been cut short by these means; and that the remedy has been successfully employed at any and every period of the attack? Many regarded it as violence done to nature, to administer bark or sulphate of quinine in the hot stage, and they could truthfully declare, that *they* never succeeded in that mode of exhibition, simply because they never made the trial. So, in like manner, has it been the custom of many to denounce the remedies wholly, unless a *preparative* course preceded the administration. Yet, who does not know, that Elliotson abroad and Flint at home taught, as others had before practiced, that intermittents could be arrested safely by the bark and its salts, without incurring the delay incident to preparative treatment?

Some of my readers can call to mind a discussion by a Philadelphia Society, but a few years ago, in which it was most zealously argued that neither bark nor its salts could be safely administered in the hot stage of any fever, intermittent or remittent, notwithstanding the well known fact, that hundreds of our most successful practitioners, in the South and South-West, were constantly exhibiting these articles under the precise condition objected to. Dr. Jackson, of Northumberland, and others equally reliable, could most truthfully affirm, that they never succeeded with sulphate of quinine given in the hot stage of a remittent, for the plain reason that they never ventured the trial, or if attempted at all, it was with so feeble a hand as to make failure almost inevitable. I can remember the time when it seemed wholly out of place to pursue this course of practice, and yet it is now one of the plainest propositions in medicine, in my judgment.

During my connection with the Philadelphia College of Medicine, I taught (because I believed it to be true) the doctrine of the curability of yellow fever by the use of the sulphate of quinine, and referred to the fact, that the same disease had been cured by Hilary and others by

the free exhibition of Peruvian bark. I cited also facts, to show that large doses of bark had suddenly cut short yellow fever, more than half a century ago, and that Dr. Fenner had advocated the same power as belonging to the sulphate of quinine. One of my colleagues, younger than myself, who had resided in the vicinity of Havana several years, and been familiar with the Spanish treatment of yellow fever, half ridiculed my teaching on this point, regarding the alleged curative powers of the sulphate of quinine as purely apochryphal. And yet, the testimony of the late Prof. Harrison, Drs. Beugot, Fenner and others, is just as much entitled to credence as any other evidence on any other point in medicine. It is a matter of history, therefore, that some cases of yellow fever have been cut short, and others cured by the use of Peruvian bark and sulphate of quinine, and this history cannot be nullified by any amount of failure in the hands of others, who may have tried the same expedients.

These remarks have equal force in respect to typhus and typhoid fevers. Need it be urged at this day, that all forms of continued fever known to the profession, have been cured by the practitioners of the days of Sydenham, chiefly by the agency of Peruvian bark? It would seem to be little less than insulting the good sense of the physicians of 1853, to quote proofs of this position. And I might fill several pages of this Journal with the most positive statements of most respectable men, touching the efficacy of the sulphate of quinine, not only in that which has been specially denominated *typhoid* fever, but also in the genuine *Irish typhus*, as reported in the *Dublin Medical Transactions*, from which I quote freely in my lectures. Were these reporters blockheads, or insane, or fanatics? Will any modest man say so, who has failed to realize happy results from the use of the sulphate, in the same fevers? Is that medical philosophy, or common sense? And does it become the junior members of the profession, to call in question the testimony of men much older and more experienced than themselves, on a point of such importance as this?

No sane physician will assert that any known plan of treatment will cure all cases of fever. He is aware that climate, season and individual peculiarities are capable of inducing annual changes in the features of all our fevers, and that lesions may exist which are not perceived, that may prevent the otherwise happy action of the sulphate, and of all other medicine. This doctrine of the varying character of fevers is all important, in estimating the worth of any mode of treatment, and the physician who does not constantly study it, in all its phases, has not yet begun to learn the true methodus medendi.

Fordyce taught the real nature of fever more correctly than any one who preceded him, when he affirmed, that it is a disease, not of any one part, organ or tissue, but that it may invade any and every portion of the animal economy. He knew that if typhus fever seized the brain of one man, chiefly, the knife would develope lesions within the skull, while other organs might be sound; and that if the stomach and bowels of another were the seat of the disease, as manifested by the symptoms, the lesions found after death would not be in the head, but in the alimentary canal. The same is true of *yellow fever*, of the most epidemic and fatal character. I have collected the reports of dissections made by twenty physicians, far remote from each other, some of them having examined more than one hundred bodies, and no two of the reports agree, and all of them presenting points of difference, so palpable as to prove to any unprejudiced mind, that pathological anatomy has never yet thrown any light on the true nature of the disease. All this grows out of the fact, well known to those most conversant with its history, that in different years, and seasons, and places, and even in the same season, in the same ship, as reported by Surgeon *Birthwistle* and others, its features were so varied after the lapse of a month, as to demand an entire change of treatment. This view of the subject accounts to us satisfactorily, why the bleeding plan, reported so fully in *Johnson's Diseases of Tropical Climates*, has been treated as fabulous by some in later years, who regarded the lancet as inevitably fatal. A proper estimate of the whole matter will assure the studious and discerning, that the reports on record, of success by the most opposite treatment, are equally reliable.

We are decidedly of opinion, that the treatment of yellow fever and typhoid fever has been more embarrassed by the attempts that have been made to stereotype their nature, by the use of the knife, than by all other agencies combined. And as this evil is most extensively realized in this region of country, in respect of what is called typhoid fever, we shall present here what we name to our class as the *summing up of the evidence*. Dr. Bartlett and others have not only taught the specific nature of this fever, but, with some of the French, have endeavored to impress the professional mind with a belief of its *exanthematous* character. Making the disease to be as much a specific affair as small-pox or measles. Dr. B., and those who agree with him, hold that it cannot be treated by the same means that are successful in the intermittents and remittents of the country. Now, we deny, most absolutely, that the so-called typhoid fever of the South and South-West is a specific or

distinct fever; and equally so, that it is in any proper sense, an exanthematous affection.

In summing up, we shall put Bartlett and Louis on one side, while on the other we shall place some who have learned all they originally knew of typhoid fever from Dr. Bartlett's book, and who seem even yet to think that it is in some sense, a new disease. Among these persons, Dr. Sutton, of Scott County, Kentucky, is entitled to a prominent place. His history of typhoid fever, of nearly 130 octavo pages, was published in 1850, and has been favorably reviewed by the American Journal of Medical Sciences, and by some other periodicals. His experience ran through nearly six years, during the whole of which period I resided in Lexington, not over 12 miles from the field of Dr. Sutton's observations. I may add too, that an able thesis, written by a graduate, whose home was in the same county with Dr. S., and about six or eight miles from Georgetown, the place of his residence, was founded on the treatment of 150 cases of typhoid fever that occurred in that vicinity, under the care of his preceptor. His thesis affirmed that the *rose colored eruption was not* a part of the disease.

SUMMING UP OF THE EVIDENCE—*First as to the Eruption.*

The rose colored spots are seen in 9 out of 10 case.—*Bartlett.*

I have detected these spots in 49 out of 54 cases.—*Louis.*

Of 70 cases, the eruption was absent in 16.—*Chomel.*

During six years I have not been able to see the eruption, although I have carefully looked for it.

Sutton.

Dr. Emison, who studied typhoid fever in Philadelphia, under Dr. Gerhard, and who saw the cases in Scott County, could not detect the eruption.—*Sutton.*

Dr. Prewett, familiar with the eruption in Boston, where he studied the disease, could not detect it in Kentucky.—*Sutton.*

The thesis referred to, is also to the same point; and my observations in Lexington and elsewhere, have been of the same nature.

Many quotations could be cited from foreign writers, of like tenor.

Second—of the Bowel Lesion.

In *all* cases of typhoid fever, there is lesion of the small intestines. This lesion is peculiar. It is found in no other disease.

Bartlett.

Of 38 typhoid fever subjects dissected, only 14 presented the bowel lesions.—*Andral.*

Of 229 patients dead of typhoid fever, only 92 showed the peculiar lesion, dothinenterite.

Reports la Charttè Hospital.

Of 500 subjects examined in three years, all dead of this fever, only 3 had ulceration of the bowels. *Lond. Lancet*, vol. 1, p. 218, 1850.

On careful examination of these bodies, where these bowel lesions were confidently looked for, I found none.—*Lombard, of Geneva.*

Here then is a question purely of *fact*, and not simply a matter of opinion. *Andral*, and all concerned in the management of *La Charitè*, were as intimately acquainted with what the French call typhoid fever, as any men on earth. If they erred, it is not likely that others will be right. And yet, a vast majority of their own cases, lacked entirely the intestinal lesion, which Bartlett affirms is present in *all* the subjects of typhoid fever. Are we to credit Dr. B., who saw comparatively but little of the disease, and discredit the great teacher of clinical medicine, who saw it on a mammoth scale? Or shall we be governed here, by the rules of evidence that apply to all the transactions of society, that are subjects of controversy?

To our mind it is perfectly plain, that the cutaneous eruption and bowel lesion held by Bartlett to be part and parcel of typhoid fever, so as to be even pathognomonic, are mere contingencies that may or may not be present, according to circumstances. The same eruption has been seen in ordinary intermittents and in protracted dysentery, frequently. I have known a case to be called typhoid fever, in this city, merely because two or three red spots appeared on the chest; and this mistake was made by a Professor too. The man was out of bed in two days after, and was discharged well, in less than a week. Hundreds of mistakes, of the same kind, have been among the fruits of *Bartlett, on Typhus and Typhoid Fevers.* On this point, it were easy to be voluminous enough, if we had space and disposition for the task.

We have learned, and we think truly, that the book of Dr. Sutton was approved by Dr. Bartlett, while in Louisville, as a fair history of typhoid fever, as it prevailed in Scott and Fayette counties of Kentucky. And if we mistake not, the reviewer of Sutton, in the American Journal

of Medical Sciences, (*Condiè*) conceded the frequent absence of the cutaneous eruption, and admitted that there might be typhoid fever without dothinenterite. And it may therefore be a very fit inquiry, are there two kinds of typhoid fever, the one with rose-colored eruptions and ulceration of Peyer's glands, the other having neither? If this be not so, how are we to understand the positive assumptions of some, as to the universality of the lesions named, and the equally positive avowal of others, as to their entire absence?

If the typhoid fever of Bartlett be not exanthematous and, in no sense, specific, what can it be but one of the protean forms of the fevers of the country? What means the statement coming up from various sections of the South, that the old fashioned remittents seem to be disappearing, and their place to be occupied by typhoid fever? How shall we understand intelligent physicians when they say, as they do in the published proceedings of the *Alabama Medical Association*, that these fevers prevail side by side in the same malarious locality, and that often there is such a blending of symptoms as to obscure the diagnosis? Why, in short, is the so-called typhoid fever of the South, found in the very hot-beds of the periodical fevers which have been known ever since the settlement of the country? Let such men as Dr. Ketchum, of Mobile answer, by teaching as they do, that this typhoid fever is but a modification of the malarial fevers of the region, and that just such circumstances are operating to change and vary the febrile aspect, as have ever been recognized in giving rise to palpable alterations in the features of the graver remittents. Thus, we get a key to unlock the supposed mystery of the cure of typhus and typhoid fevers by the free use of sulphate of quinine, as reported in foreign journals and at home.

Let me not be misunderstood, as denouncing any of the disbelievers in the potency of the quinine salt, to cut short yellow fever, or to cure what is called typhoid fever. Their integrity of purpose is not at all questioned by the writer of this article. Yet it is every way desirable, that the entire profession shall more carefully study the etiology and nature of the fevers of the great valley, and not receive as absolutely authorative, the teachings of any man.

II.—TRANSPOSITION OF THE THORACIC AND ABDOMINAL VISCERA.

BY STANFORD CHAILLE, M.D.

On June 8th, Mary Doherty, aged 20, entered the Charity Hospital. She had been unwell several weeks, had some little cough, slight pains in the chest, and was suffering from general debility, but had no violent symptoms of any kind. The expression of her face, which was pale, dusky, a little anxious and distressed, called attention at once to her chest.

Upon auscultating the lungs, the right was found perfectly healthy, the respiration of the left was decidedly feeble, particularly inferiorly. The left lung was also dull on percussion. The heart was found situated upon the right side. On measurement, the left side proved half an inch larger than the right, which was really an enlargement of an inch, the right side being normally a half inch larger than the left. The decubitus of the patient was remarkable, on the right side, diagonally inclined to the left. This position was almost constantly maintained during the five weeks the patient was in the Hospital.

The diagnosis formed was previous pleurisy, which had resulted in effusion. It is true, that the dullness on percussion was not as complete as is usual in hydrothorax; that the intercostal spaces were not dilated as they should naturally have been, by an internal pressure sufficient to effect the heart to the extent observed; that there was no ægophony; and that the respiratory murmur, though feeble, ought not to have been heard at all in an effusion so profuse. Still, by what other hypothesis could the position of the heart and the enlargement of the left side be accounted for? I leave the curious to imagine, and proceed to solve the mystery, by giving the results of the post-mortem examination, for which we were indebted to the yellow fever. It was witnessed by Drs. McKelvy and Pugh.

Upon the right side were found the heart, the lung with *two* lobes, the stomach, spleen, pancreas, sigmoid flexure and rectum; on the left, the lung closely bound down to the thoracic parieties by excessively thick adhesions, and much congested, particularly inferiorly. On this side were also found the liver and the Ileo-coecal valve.

The viscera being totally transposed, I presume a corresponding transposition in the size of the right and left sides of the thorax existed, accounting for the observed enlargement of the left side. The post-mortem examination satisfactorily explains the position of the heart, the dullness on percussion, the feeble respiration, etc.; in fact, all the phe-

nomena except the decubitus. In all other respects, except the entire transposition and the morbid condition of the lung, the viscera were perfectly normal.

About four years since, a similar transposition of the thoracic and abdominal viscera was witnessed in the Charity Hospital, by a number of medical gentlemen, which has never been reported.

III.—PREVENTION OF YELLOW FEVER.

BY SAMUEL A. CARTWRIGHT, M.D.

Where there are three ways leading to the same end—one, short, cheap and safe—another beset with manifold dangers, and the third, not only dangerous but expensive and impracticable, we ought to choose the safe way. Governed by extreme opinions, New Orleans has never done so, but has invariably chosen the two last, and has never adopted the first, in trying to arrive at that desirable object—*the prevention of yellow fever.*

Contagion and non-contagion, the one engendering idle fears and the other encouraging a reckless disregard of danger, have been regarded as things instead of mere abstractions. They have nothing substantial about them to build any system of sanitary measures upon. They are relics of mediæval science, mere terms, invented before either yellow fever or cholera was known. Yet these terms, handed down to us by a barbarous, vain, self-important and non-progressive science—not derived from nature or the observation of facts, but resting upon the dogmatic and logical basis on which the thing called learning in the dark ages stood—have been dignified into a directing principle, governing all those measures heretofore adopted to prevent yellow fever in New Orleans. The consequence is, the community is divided into two parties, contagionists and non-contagionists; and like all parties, more intolerant to opposition from outsiders, or persons belonging to neither party, than from each other. If any objections be made to stretching yellow fever, ship fever or cholera to the contagious dimensions of small-pox, or to applying the means applicable to the prevention of one disease to other diseases governed by different laws, and so essentially different in their natures as to be generated by what would prevent another, he is immediately ejected from the councils of the one party to

fare no better from the other, unless he subscribes to the doctrine that yellow fever, ship fever and cholera are absolutely as incommunicable as a tooth-ache. To speak openly, or even to whisper, that facts indicate that these diseases can be, and have been dropped at wood yards, or communicated under certain circumstances to our citizens by the steamboats and shipping, would ruin the reputation of any man with the non-contagionists. Hence, between the two, true science has become unpopular. Logic cuts at it from every groggery, and the shafts of ridicule are hurled at it from every street corner.

Dr. Bennet Dowler, of world-wide fame, with more progressive knowledge on the subject of yellow fever than, perhaps, any other man in existence, eminently successful in his practice, and unexceptionable in morals, receives no encouragement or patronage from the wealthy or monied men of that city, whose reputation abroad, for science, he has done so much to promote; nor has that progressive knowledge, that true science, which alone can form the basis of sanitary measures to prevent New Orleans from being scourged by devastating epidemics, been regarded of sufficient importance to be called into requisition by the governing authorities. A war of words, between the ins and the outs on the obsolete ideas of contagion or non-contagion, when taken from small-pox and measles and applied to yellow fever and cholera—diseases governed by different laws—has drowned the voice of the scientific Dowler—thrown him into the shade and obscurity, and left the great emporium of the South a prey to those terrible epidemics, that all the wrangling about contagion or non-contagion can never prevent, and which nothing short of ascertaining their laws by the toilsome, unobtrusive and laborious inductive or progressive sciences can ever keep out.

The non-contagionists when in power, to satisfy an abstraction drawn from an obsolete, non-progressive science, admit foul, filthy emigrant ships into the ports of the great Southern emporium, and send the sick emigrants themselves, whether ill with cholera, yellow fever, typhus or ship fever, into that immense establishment, the Charity Hospital, the largest institution of the kind in either Europe or America, as far as acute diseases are concerned. The non-contagion party thinks it would forfeit its right to be regarded as consistent and learned, if it were to refuse to admit them; because such diseases do not come up to its standard and definition of contagious maladies, when tested by the logic which mediæval learning applied to small-pox and measles. But its definition of contagion does not prevent the foul excretions on the persons and clothing of the sick from infecting the wards of the

Hospital, any more than it prevented the noxious effluvia from the prisoners of the Old Baily from infecting the judges, lawyers and by-standers with a mortal malady. That a large number of apparently healthy prisoners, coming out of a foul jail into a crowded court-room, did communicate a most dangerous malady to those present, is a historical fact so far lost upon the non-contagion party, that it admits emigrants ill with cholera, yellow fever or typhus, not only into the Hospital, but into dwellings of the most populous parts of the city. Filled with a ruling idea it cannot see why a little foul air from the hold of a ship should be hurtful to any, than the few on board, who might happen to breathe it when discharged. To admit that poisoned air, thus pent up, could, under certain circumstances, infect the surrounding atmosphere, would make it a contagionist in its own estimation and according to its own definition of the term; if that definition were applicable to the subject. The highest of all authority has said, that "a little leaven leavens the whole lump." But it rejects that authority sooner than yield an abstraction. The contagionists and non-contagionists can see only the two ends or extremes. Their definitions place cholera, ship fever, typhus and yellow fever as wide apart as the poles from contagion, and they will acknowledge no intervening ground. If either party is driven from one pole it flies to the other, and disdains to stop at any intervening point for fear of being thought ignorant of what contagion or non-contagion is, without seeming to know that progressive science has swamped the whole foundation of the doctrine of contagion and non-contagion when applied to the diseases just mentioned. The danger now is, that the non-contagionists, excited by the fear of a repetition of the evils just witnessed, may fly to the opposite extreme and unite with the contagionists on a system of sanitary measures founded upon logical abstractions instead of inductive truths, and better calculated to breed than to prevent the diseases they are intended to guard against. The danger is, that "*the perish commerce*" doctrine, or the same thing—rigid and protracted quarantines—may rise into popularity, and city vie with city—country with country—Europe with America—in harassing commerce, as in former times, with those vexatious, expensive, useless and pernicious restrainsts, which, inductive truth derived from progressive science looking back upon the thousands of experiments already made, declares had no more effect in walling out typhus, cholera or yellow fever, than in excluding caloric, magnetism, or electricity.

It would be tedious and require volumes to enumerate the many sacrifices which have been made of the mercantile and all other interests, in trying to quarantine out various diseases by rules and regu-

lations drawn from the barbarous practices of the dark ages, which vain abstractionists, with but one idea and that erroneous, or true only in part, have pronounced contagious. The merchants themselves, actuated by patriotic motives, and forgetting their own interests, were always among the foremost in lending a helping hand to those measures to promote their own ruin, supposing that the best interests of humanity required the sacrifice. At length, in regard to the terrible typhus of Europe, progressive science made the discovery, that it was an artificial disease, made by bad governments. Governments which created want to diminish wages, and which punished their poor with hunger, cold and nakedness to extort labor at reduced prices, were and are the hot beds of typhus—a disease scorning the most rigid quarantine regulations, and leaping over the sanitary cordons drawn around every misgoverned city to keep it out. But is not typhus contagious? says one party—is it not of domestic origin, and produced by filth? says another. It will spread from one to another in crowded, filthy hovels, unwashed and unventilated. It seems to glory in a confined, malarious atmosphere; but often the most filthy places are entirely exempt from its ravages. While the war of words about contagion and non-contagion was raging with increased violence throughout the various monarchies and despotic governments of Europe, and after both contagionists and non-contagionists had tried their hands, and were unable to prevent typhus from sweeping off hecatombs annually, progressive America, by the example of her glorious republican institutions, showed twenty-four millions of people entirely exempt from the pestilence, except in a few localities where European anti-republican customs have been imitated; thus proving that the contagionists and non-contagionists of the logical school had entirely mistaken the nature of the disease; that it is an artificial ailment, caused by war, and generated among every misgoverned people in time of peace. During the 25 years war in Europe, terminating in 1815, the typhus, under the name of camp fever, malignant, pestilential, putrid, asthenic, nervous, adynamic or ataxic fever, never failed to show itself in every city where there were many prisoners of war confined. Whenever the prisoners were removed from one place to another, the pestilence would follow them and spread to those who had intercourse with them in their narrow, ill-ventulated quarters.

At Vilvorde, in 1802, it began in the cold, close, damp dungeons, in which some prisoners were confined as a punishment for intemperance. It spread to the guards and keepers—from them to the prisoners in the

common jail—then to a great number of mendicants and invalids, and finally to the people of the whole village; and out of a population of four thousand, as many as a hundred died in a day. A few drunkards, shut up in a confined dungeon, generated the infection. They were not prisoners of war, but residents of the department of Dyle, who had committed various offences. The village and the department were both healthy anterior to the appearance of the typhus in the dungeons.

Typhus or pestilential fever has nearly always made its appearance in every besieged city. It appeared in Jerusalem when the Romans were besieging it, and at Acre when Napoleon was before it. The besiegment of a city is the most effectual quarantine that could be instituted—as it cuts off all intercourse from without; yet it is during the siege, when protracted, that it is sure to make its appearance within the walls of the city besieged. The certainty of the appearance of pestilence in every place long besieged by a foreign army, is a fact worthy of the consideration of those who are in favor of besieging New Orleans with a rigid quarantine. There is a great deal of true wisdom and benevolence in Dowler's prayer, for "*a harmless Board of Health.*" Pestilence may be created by the very means that those unacquainted with its laws might suppose would be the best to prevent it—or appears to them to be the safest and freest from harm. But as far as a strict quarantine is concerned, experience proves that it is not harmless; but on the contrary, like everything else which obstructs commerce and enhances the price of the common comforts and necessaries of life, has a tendency to breed disease among the laboring poor. Pestilence is not only the child of war, but also of all these wretched governments, which in graciously condescending to confer the boon of nominal liberty on the laboring classes, have taken from them every right and privilege that makes liberty valuable or desirable. Typhus or pestilence of some kind, included under that generic term, is artificially generated almost every year, in every city, of those monarchical and despotic governments, which create want by taxing exhorbitantly, either directly or indirectly, all the necessaries of life to pamper aristocracies and monopolies, and to put the moneyless classes in their power, to be punished with cold, hunger and nakedness, or to work for a pittance of wages less than what would half feed and clothe a negro. To avoid starvation or being shut up in those white slave depots, called workhouses, where husbands, wives and children are torn from one another and confined in separate apartments of the prison, until a capitalist comes along to hire their services for almost nothing, the unfortunate hirelings often impose upon themselves a

greater amount of labor and fatigue, to satisfy their employers or masters, than human nature can bear with impunity. Thus, the predisposition to typhus is laid by hard work and an impoverished state of the blood, from a poor, vapid diet of cheap articles, as black bread, turnips, potatoes, etc. Rents being high and wages low, a number of laborers have to club together to hire a lodging room or a cottage. A little fuel, to half-cook the cheapest and most unwholesome provisions, is the most that their stinted wages will allow them to get—and as to clothing and bedclothing they can scarcely afford to purchase a sufficiency to keep them from freezing in the cold weather of winter, unless with closed doors. Having no means left to purchase fuel to enjoy the luxury of fires, they have to close the doors and windows, and to depend upon the exhalations of their own bodies, in a confined room, crowded with human beings, for warmth. These are what are technically called the occasional causes of typhus. The predisposing cause is already in the system, the impoverished blood, from the stomach fed on impure food, and the lungs fed on impure air; the energies of the muscular system being worn down by incessant toil, and the nervous system pinched with cold until it almost ceases to feel. Nature can stand no more. The body of some one or more of this unfortunate class of people becomes ripe for typhus. It is stunned or stupefied, as the derivation of the word, typhus, imports. The nerves almost cease to feel, and a stupor or total indifference to all surrounding things steals over the mental faculties. There is no suffering. The misgovernment of him, or her, sitting upon the throne, has already made the poor victim suffer enough. Nature interposes and strikes the sentient system with apathy. This is typhus—representing several species of diseases, generated every winter, in almost every city of Europe. When once generated, it is apt to seize on those who are exposed to the occasional causes, the men, women and children, living in crowded tenements, breathing an impure air. It confines its ravages almost exclusively to those classes of persons exposed to the predisposing and occasional causes above mentioned, in all those cities where great attention is paid to cleanliness, sewerage and the speedy removal of all putrescent and fermentable matter. Hence, London, Paris and some other large cities, contain localities more or less circumscribed, where typhus prevails nearly every winter, to a greater or less extent. When bread is cheap there is little or none of it. The price of food, fuel and clothing, and house rents being known—the wages of labor and the temperature of the weather—the number of cases of typhus and the number of deaths, in a given population, can be figured out

with so much accuracy, as to prove incontestably, that it is a disease of artificial creation—the handy-work of oppressive governments. America has given Europe a valuable lesson in progressive science, by showing twenty-four millions of people entirely exempt from the ravages of typhus, except in a few ill-governed places, where alcohol is king. Until recently, it was the practice to fly from those parts of a city where typhus made its appearance. But observation has proved, that it is less expensive to eradicate it than to move away. By giving the poor fuel, blankets and clothing, to keep warm, enabling them to let the fresh air into their dwellings, plenty of wholesome food to eat and soap and water to keep clean, typhus is quickly eradicated; provided the general atmosphere of the city be pure. But, if that be foul all classes are liable to it, and in some seasons it sweeps through town and country, following the lines of travel in every direction. It falls upon those places where its predisposing and occasional causes mostly abound; on those which are protected by quarantine as well as those that are not. In close rooms and confined places it is communicable from one to another. But it cannot propogate itself, either by the sick, themselves, or the clothing they have worn, in a well-ventilated atmosphere. Repeated observations have proved, that quarantines, sanitary cordons, and the greatest cleanliness and circumspection have never been sufficient to shut it out from any city of the old world, where bread is dear, fuel scarce, the winter cold and wages low. If a rigid quarantine system could make provisions cheap, feed the poor, and prevent the vital energies from being overtasked by toil, in the piercing cold or under a burning sun; if it could not only give an abundant supply of water, but afford facilities for running it immediately off after it has fulfilled its hygienic mission of abluiton, without permitting it to remain a moment to deposite the organic matter it has taken up; thereby removing entirely all those impurities which contaminate the atmosphere; if it could establish public baths—build houses with thicker walls, better ventilated, with verandas extending over the side-walks, to protect the citizens and strangers from the weather and the scorching sun, when walking out breathing the fresh air of the streets; and lastly, if it could dethrone that tyrannical king—the last of the race of tyrants daring to maintain a foot-hold in this republican land—who annually carries into captivity a greater number of sturdy republican freemen, in the bloom of life and usefulness, by mesmerizing their wills with his ethyl breath, than would be sufficient to found another Republican State, or to hew down the largest army of any despot of Europe—then, indeed, would the rigid quarantine system be worthy of adoption as the

most certain, safe and sure method of guarding against pestilence, by eradicating its causes. Experience, however, has proved, that it generates, within the city it is intended to protect, more pestilence than it keeps out. It was reserved for America, by the aid of her great men, Thomas Jefferson, Samuel L. Mitchell, Benjamin Rush, and a few others to devise, plan and put into successful operation, a system of sanitary regulations which, without annoying commerce or obstructing trade, have proved more successful in protecting all these cities, where they have been adopted, from the ravages of pestilence than the most enthusiastic advocates of rigid and protracted quarantines ever dreamed of deriving from that miserable system, which, for many years, came very near annihilating American commerce. So far from doing the smallest particle of good or answering a single end for which it was instituted, the experience of more than twenty years proved that the rigid and protracted quarantine system, with its lazarettos, pest-houses, and sanitary cordons, actually seemed to sow pestilence broad cast over every land that adopted it. See 15 volumes of Medical Repository, for accounts of the terrible ravages of yellow fever, every year, in Spain, for twenty years, during the existence of the most rigid quarantines at every port—continued for a hundred days—declaring American cotton contraband and subject to destruction by fire; showing that Spain lost, in a few of her cities, as many as 124,000 subjects, by yellow fever, in less than two months. See the same work for the ravages of yellow fever, in Philadelphia, during the ten years that her rigid quarantine was enforced—and for its disappearance after adopting the sanitary regulations proposed, in the first instance, by Benjamin Rush.

See "Dr. Samuel L. Mitchell's Report (volume 6, page 460, of the same work,) to Congress on quarantine regulations," (while he was United States Senator from New York,) for a full account of those sanitary measures proposed by him as a substitute for the rigid quarantine system, which have since proved to be so eminently successful in every city that has adopted them. See vol. 9, Med. Rep., page 227, for "additional restrictions imposed upon American commerce," by foreign nations, based upon the confessions, proclamations, and proceedings of the American people and town councils, admitting that "they were polluted with a distemper, malignant and contagious beyond all example," "caused by a subtle venom, more attenuated than the electric fluid, and transportable to all parts of the world." See circular letter of James Madison, Secretary of State, under Thomas Jefferson to our foreign consuls, written with a view to procure "some relaxations of the ruinous and oppressive quarantines on American vessels in

foreign ports." See, also, Albert Gallatin's circular, on the same subject, while Secretary of the Treasury, July, 1801.

For the metaphysical abstractions that gave birth to the humbug that frightened half America, and all Europe; (except the Lancastershire cotton spinners,) that there were two yellow fevers, a non-contagious kind, in the West Indies, and a terribly contagious Bulam pestilence, in the United States, which the lawless republicans would never be able to get out, as it adhered to cotton wool, and many other things, with great and pertinacious tenacity, see Dr. Chisholm's work, published the latter part of the last century, immediately after the munufacturers of Great Britain had made the discovery that the cotton, grown in the United States, was the best in the world for manufacturing purposes; particularly the sea islands of South Carolina, and at the very time when Spain and Great Britain were running a race in the cotton manufacturing business. For the ability with which the humbug was sustained and made subservient to British policy, see the transactions and resolves of the colleges and learned doctors of London, Edinburgh and Glasgow, and all the scientific works and learned periodical literature published between the years 1794 and 1816—for specimens of the most able arguments founded upon metephysical abstractions to prove the contagiousness of the yellow fever of the United States, and its transmissibility to foreign countries by fomites, which have never been surpassed, except by the arguments built upon another abstraction, with which the British literature of the present day abounds, ignoring facts, and proving by idle romances and fiction the horrors of American negro slavery.

For the views of the British writers, of the present day, of the inefficacy of quarantines, see the Edinburgh Review for July, 1853, page 97, and all the late British Medical works on fevers, where the whole system is repudiated as "useless, an incumbrance to commerce, perilling life, fostering and engendering disease, and squandering large sums of public revenue." They are even condemned as ineffectual in guarding against small-pox. For the fruits of the theory of Dr. Chisholm, a surgeon-general in the British service, published to the world upwards of half a century ago, which frightened nearly all Europe against purchasing American cotton, see South Carolina with but one market for her sea islands to the present day; and see the people of more than half Europe, just beginning to open their ports to American upland cotton, and to venture to begin to spin an article which for ages they were afraid to touch, except with a pair of tongues, and then to burn it, and condemn the ship to destruction that brought it. That "there was something rotten in Denmark," see vol. 9, Med. Rep., page 231, where

the Danish Consul, in Philadelphia, notifies the American people, that his government considered "cotton" (and a variety of other things he gave a list of,) "dangerous, as carrying infection, and that if goods of that kind were found aboard an American vessel, in the waters of Denmark, she would be sent to Norway, where all the goods of a nature to carry infection would be burnt."

There is no limit to the evil consequences of letting idle fears and a false theory run ahead of fact and reason. A thing which we incautiously admitted to be *imported*, naturally led Europeans to believe might be *exported;* and hence, in self-defence, to protect themselves from what they supposed to be a real danger, but which was entirely imaginary, they placed the most odious restrictions on American commerce. The doctrine of contagion or non-contagion is founded on abstractions, to which each party has given different definitions or meanings, and like all other questions, whether medical, scientific, or theological, resting on such a basis, the longer the discussion, the further are the parties from agreeing or understanding one another. The merits of the subject can never be reached by fact or argument until it is removed to a more solid platform, and the old question of contagion or non-contagion lost sight of. Epilepsy and St. Vitus Dance are contagious or not, according to circumstances—so is rheumatism, so is influenza, and almost every other complaint. If epilepsy were a new disease, which Europeans had never seen or heard of, until advised of its appearance in America, and that the Americans believed that it was imported from the West Indies, but that the people of those islands denied its paternity, and accused Siam and Bulam with it, the various European governments, on being fully assured, by their Consuls, of the admitted contageousness of the new complaint, and seeing exaggerated descriptions of its terrible symptoms, would not be long in establishing the same rigid quarantines, to keep it out of their respective dominions, as those they established to guard against the yellow fever. Epilepsy, however, is an old disease, the extent of its contagiousness is known to the world. Its contagion can be guarded against by an easier method than by quarantines. All that is necessary to avoid the contagion, is to turn the head away, and not to look at the patient afflicted with the malady when in the fit. Even this precaution need only be imposed on extremely nervous and excitable persons. If Europeans knew that the contageousness of yellow fever could be guarded against, and entirely destroyed by throwing the doors and windows open, they would never think of resorting to the expedient of a rigid quarantine to protect themselves against it, when an hour's ventilation would be all sufficient.

Again, if the people of Europe were to hear that a camp fever had made its appearance among a large army of soldiers in New Orleans, who had for six years been receiving fresh recruits from Northern latitudes, that the whole were under bad discipline, and the atmosphere of the city impure, and that this camp fever, after becoming very rife among the soldiers, had attacked many of the citizens—especially the unacclimated portion, it would not excite surprise; because the occurrence of that fever among soldiers, quartered in a city, is an every day affair in Europe. It is also a very common occurrence for the camp fever to extend to the citizens of the place, in which the army is quartered. Nor would it excite surprise or attract attention, to hear that smaller garrisons, of a similar description of soldiers, quartered in the towns of Baton Rouge, Bayou Sara, Natchez, Grand Gulf, Vicksburg, Mobile, and many other places had also, soon after its appearance in New Orleans, been attacked with the same kind of camp fever, and that it had in some or all those places, spread from the soldiers to the citizens.

If told that in 1852, it had only attacked the new recruits and had spared the old soldiers and citizens, they would not wonder at it at all, because that is precisely what camp fever does in Europe, attacking every year the new recruits, but only now and then extending to the veterans and to the citizens of the place where their barracks are situated. They would not think of instituting quarantines against American vessels to protect themselves against such a fever, because they are well acquainted with it. They know it to be contageous in a certain sense, but they know the full length and breadth of its contageousness to be very short, and easily guarded against by soap and water, and free ventilation. Repeated experience has proved, that quarantines and sanitary cordons, ever so rigid, cannot prevent the camp fever from appearing among large armies of soldiers, and of extending sometimes to the citizens of the place in which they are quartered, if the surrounding atmosphere be impure.

What is camp fever? "It is an artificial disease, engendered by a great number of men collected together from different quarters, eating and drinking what they have not been accustomed to, deprived of the fresh, cool and bracing air of their homes and the invigorating exercise they had heretofore been used to. Placed in such artificial or unnatural circumstances, an artificial disease is the necessary consequence of their abnormal condition—a disease not occurring on a regular march, but only when wiling away an idle and irregular life in camp. The camp fever, febris castrensis or typhus of the armies of Europe, is only

an older chip of the same block out of which the yellow fever of the South, at a later period has been hewed by the operation of a new set of causes, subjecting emigrant armies of northern men, with or without guns in their hands, to the scorching rays of a tropical sun. They both belong to the same genus—typhus. They are older and younger brothers. Yellow fever being what authors call typhus icterodes, the youngest of the family, which is quite a large one. Every species of the whole genus are artificial complaints.

To America belongs the honor of having swept the whole of them, with the exception of the youngest, out of every nook and corner of her free land, where true republicanism has swayed her benign sceptre, unincommoded and unawed by the tyrant Alcohol. The ship fever, another species of the genus, typhus, has been destroyed by limiting the number of passengers—by a good diet, proper cleanliness and ventilation on board. Foreign ships often bring that disease to New Orleans, among the emigrant passengers—a disease artificially generated on the voyage. Another species of typhus artificially generated in prisons, penitentiaries, asylums, houses of refuge, schools, etc., by over crowding, want of cleanliness, an impoverished diet, insufficient clothing, and abnegation of the common comforts of life, has been nearly annihilated in this country, by the hand of progressive science. Yet this disease, under the name of jail fever, is very common in the non-progressive governments of monarchical Europe. The nervous, the slow-nervous, the adynamic typhoid, the typhus gravior and typhus mitior of Great Britain and Ireland; and, also, the putrid fever of Hoffman, which continues to be the terror of the laboring poor of the old world, sweeping off three-fourths of the toiling millions, are proved to be the artificial creations of bad governments, by their being seldom or ever seen in the model Republic. The putrid fever arises from the necessities of the poor, compelling them to eat spoiled provisions, and such things as the dogs of the American negroes would refuse. An eminent Baptist preacher, Malcom, found the British Hindoo subjects subsisting on cotton seed, soaked in water, in his travels through India, some few years since. He was a northern man, unacquainted with our cotton region, and mentioned the fact of cotton seed soaked in water, being used as a diet in India, apparently as a matter of information that might be useful, or turned to practical account by his brethren in our Southern States. But they can tell him that cotton seed are so unwholesome, that the butter made from the milk of cows, that feed on it is not fit to use, as an article of diet.

Adynamic fevers, with gastric derangements and softening of the tissues, called by the humoral pathologists, "putrid," all being species of typhus, desolate British India. They are the artificial creations of that oppressive government which has robbed the people of everything except the name of freemen, and driven them to eat such unwholesome food as cotton seed or starve; a government which can see no evil in anything but American negro slavery. Whatever may be said against American negro slavery it has, at least, freed the slaves from every species of typhus. It has done it by meat and bread, blankets, warm clothing, good fires, and by exacting no more than a reasonable service in return, for all the substantial comforts of life; thus proving that typhus is artificial and not essential. The British typhus, so fatal to the overworked, squalid poor of England, and which is depopulating Ireland, is so clearly an artificial malady, that it rises and falls with the price of wheat. American republicanism, guided by the unpretending inductive sciences, has banished, one after the other, nearly the whole batch of those artificially created typhus plagues or pestilences, which, for countless ages have been reveling in the blood of the toiling millions of the old world. One of them is left, driven from the North by the great Rush and Mitchell, it has fallen upon the South, and in one short season made ten thousand victims in New Orleans. As the whole tribe of typhus plagues, yellow fever included, is the spawn of despotism, ignorance, vice, wasteful extravagance, high taxes, poverty, filth and a disregard of nature's laws, they can only be extirpated by extirpating their causes.

The supposed case of a large foreign army in New Orleans, which had been six years gathering together—under very lax discipline, and among whom the camp fever appeared, extending to the neighboring garrisons and falling upon the citizens, precisely as it did in the long wars that France sustained against all Europe—it is now time to metamorphose into a real one. It is true, that the individuals composing that vast army of emigrants, which have for six years been congregating in New Orleans and the neighboring cities and villages, had no guns in their hands, no officers, no uniform, no regulations in regard to diet or drinks, no regular hours of repose and exercise, and under no discipline; yet they were here. In numbers, they were like the locusts of Egypt. They greatly outnumbered the American armies that conquered Mexico, and revelled in the halls of the Montezumas. They congregated at New Orleans from all parts of the civilized world, inhabited by the white man of the North of Europe, but chiefly from Germany and Ireland. Many of them brought their women and chil-

dren with them. Their principal rendezvous was in that part of the city where the late epidemic first broke out and committed its greatest ravages. They were nearly all as poor and destitute on their arrival as poverty could make them. The despots of the old world had robbed them and their fathers before them of everything, not leaving some of them a second shirt. They depended upon their daily labor for a subsistence. They found few or no shops, carrying on various handicraft employments, open to receive them, where they could work in the shade and be protected from the scorching rays of the sun. They were sufficiently numerous if such shops (which have built up Philadelphia and Cincinnati) had been open to receive them, to have made New Orleans rival both those cities in small trades and handicraft employments. The only shops they found open to receive them were the grog shops. Hence they had to turn out in the broiling sun, to work for wages at the most laborious kind of drudgery work, on the levee, in the streets, and all other hot and exposed places. Soon many of them were seen carrying bricks and mortar up to the fourth story, exposed to a midday's sun, near the summer solstice of a Southern climate.

The wealthy capitalists, forgetting that true republicanism, (that kind which can banish plagues,) requires them to make the comfort and welfare of the laboring classes the governing principle in spending their money, have chosen the summer season, par excellence, to build houses to be ready to rent, in the fall and winter. The city authorities paying no attention to the long war of 30 years duration, *quorum pars magnum fui*, which Professor Merrill, of Memphis, has carried on against all those who would stir the earth of our towns and cities, in the summer time, with a hoe or spade, no matter how pure it may be, put a very large number of the emigrant army to pulling and hauling the filthy mud of the streets and gutters about; the most unhealthy work for the operators, and the most efficient means that could be adopted to fill the city with clouds of miasmatic vapors, if there be any truth at all in Dr. Merrill's doctrine. The emigrant army, working for wages, exposed to the sun in performing all kind of laborious drudgery work, were breathing an atmosphere much more rarified than that which they had been accustomed to in their native country. They had, consequently, not their accustomed supply of oxygen to carry on the vital processes in all their wonted integrity; an insufficient supply of oxygen, by breathing an air of a high temperature, slackens the functions of the lungs, and prevents the due elimination of carbonic acid from the blood. The ingestion of alcohol has the same effect, and conspires with the high

temperature to obstruct the elimination of decomposing matter generated within the system. Moderate labor, in a comparatively cool atmosphere, is healthy, by enabling the system to free itself of the effete matter which is constantly generating within the body. But hard labor in the hot sun causes a rapid degeneration of the tissues within the body of the white man, and an accumulation of azotized compounds and other effete substances, which nature has made no extra provision for throwing off as she has in the body of the negro. In the negro they are eliminated by his enormous liver and by the peculiar construction of the skin, and when hard at work in the burning sun the effete matter, thus thrown off, taints the air with a strong muskrat odor a great distance around. This physiological fact, the rankest abolitionism, which ever tried to push the happy negro from his stool in this Southern climate, and to victimize the white man upon it, cannot deny. It sticks in the nostrils—no logic can dislodge it. Towards the close of Dr. Sam'l L. Mitchell's life, he wrote to me to ascertain the temperature of the open air in the summer's sun of our cotton fields, roads and streets, unprotected by shade. He said the thermometer told him the temperature of the shade in the South, which differed but little from the temperature of the shade in summer weather at the North. He conjectured that the difference of temperature, between the sun and the shade in the South, would be much greater than between the sun and shade at New York, in the North. His conjectures were proved to be correct by actual experiment, made by him at New York and by me at Natchez. The difference was much greater than he had supposed. He wanted further observations by thermometers exactly alike in construction. But before they were obtained news arrived that the great Sam'l L. Mitchell, of New York, was no more. The investigation was dropped, but I learned enough to come to the conclusion, since confirmed by many years of observation, that the summer's sun in this climate is too hot to enable any white man to labor long in it and live. On the other hand, observation proves that labor in the shade, not requiring too much muscular exertion, is healthful and beneficial. All those sanitary measures, therefore, which may be instituted to protect New Orleans against pestilence, would be incomplete and ineffectual unless the practice of making negroes out of the master race of men, and turning them out to labor in the hot summer's sun, be abolished. When the late epidemic made its appearance, a large portion of the whole army of emigrants, at work at all, were laboring in the sun. Those who had last arrived from Europe being the poorest and the most needy had the roughest and hardest kind of labor imposed upon them. Being just from a long sea

voyage, they were debilitated and their blood scorbutic, as could be seen by their spongy gums, receding from the teeth, giving the latter organs the appearance of gourd seed stuck in the alveolar process. In such a state of the system of such persons, hard labor in the hot noon day's summer sun of this climate, might justly be characterized as man-slaughter. The poor emigrants had left behind them the cold bracing air near the arctic circle, but they brought with them many bad habits which had been forced upon them and their forefathers by the despotisms from which they had fled—not the least of which was the habit of crowding together, in small, confined rooms, to lessen the expense of rent. One room often serving for one, two, or more families. Many of them slept on the naked floor, which scarcely gave sufficient space for them to lie down. The old habit of closing the doors and windows to keep the cold out, they still adhered to, to keep the night air out. Although they get good wages—sufficient to enable them to rent more comfortable dwellings—yet they found themselves in a city very much like those of the old world, where the common comforts and necessaries of life are exorbitantly high, owing to the high taxes imposed upon the citizens without their having anything to show for them in return—nothing free—not even water, without paying a Company a high price for it—the Company not taking the trouble to filter, settle or purify it but furnishing it in a crude state, containing about half a pound of mud and foreign matter to the gallon, and being entirely unfit for cooking, washing or drinking, unless the purchaser himself take the pains to purify it—a troublesome business on a small scale, but which could easily be done on a large one, before the water is distributed. To add to the misfortune of the emigrants, they found very few fresh and wholesome vegetables in the market, which persons from a long sea voyage so greatly need. The stalls of the market being taxed, the gardeners cannot afford to raise the vegetables and pay the tax. Their blood being in a bad state from the sea voyage and a want of fresh, antiscorbutic vegetables, exposure to the sun brought out the prickly heat in such abundance as to thicken and disorganize the skin; preventing the effete organic matter generated in the system from duly passing off by insensible perspiration—causing great irritation of the nervous system, and loss of rest at night from the intolerable itching, burning and smarting, attending that complaint in its aggravated form. While the comforts of life, even that of a glass of clear water, were shut against them, the doors of more than a thousand grog shops were open day and night, tempting them into dissipation. The use of alcohol in any form is known to diminish the quantity of oxygen consumed, and to

prevent the normal elimination of carbonic acid by the lungs; thus conspiring with the high temperature and the impure, confined air of their lodging rooms, to retain the noxious carbonic acid in the circulating system, and preventing the effete organic matter from being thrown off. Together with all this, the general atmosphere of the city was impure. But if it had been as pure as that of the granite hills of New Hampshire it would have availed them little or nothing, because the dens and hovels in which they had crowded themselves, contained the most impure atmosphere imaginable; being filled with the confined effluvia of the human body itself. The night closets exhaled the same kind of effluvia; many of them not having been emptied for years. The cleansing out of those noxious depots, the most noxious of all in the city, is not a public hygienic measure as it should be; but, if done at all, imposes a tax of from ten to twenty-five dollars on every tenant; who generally neglects to do it or is not able to incur the expense. If the dead animals and other nuisances in the streets, which caused so much complaint, had been ten times more numerous, they would not have been equal as a source of disease to the depots of confined animal matter, just alluded to. Animal matter in a state of putrefaction in the open air, *in this climate*, if not entirely innoxious, as Dr. McFarlane, Professor Merrill, and a number of very high authorities in medicine contend, is at least so in comparison to that which is undergoing decomposition in confined, stagnant reservoirs, sufficiently close to exclude the admission of the air, but not enough so to prevent the external atmosphere from being polluted by the noxious gasses arising therefrom, as was the case with the decomposing animal matter in the centre of the neighborhood in which the yellow fever, of 1823, originated, and around which it gradually spread, until it desolated Natchez. That year New Orleans had but two cases of yellow fever, and but one death; which occurred during the prevalence of the most terrible epidemic that ever afflicted the City of the Bluffs. Organized matter, when confined, is apt to become fungoid. As far back as the days of Moses, fungoid matter generated in an unventilated house, in Scripture language, "plague spots," has been considered as poisonous.

If Moses had been the mayor or governor of New Orleans, at the breaking out of the late epidemic, he would have made sad work of the houses occupied by the emigrants, where the disease first appeared. Some of them certainly contained a mould or spots of discoloration on the walls, which would have doomed them to destruction. Even the old shoes and things of that kind found in confined rooms, looked as if they were covered with frost, being white or green with mould; while

the summer solstice was approaching, the heat of the weather became more intense. At length, a number of emigrants, just from Ireland and Germany, fell the first victims to that artificially created disease, the yellow fever. The first one died on the 27th May, in the Hospital, on the day of his admission. His name was John McGuigan, an Irish laborer. He had only been one week in the city. He came to it direct from Liverpool. His was a clear case of yellow fever, with black vomit, imported from Liverpool, if imported at all. Three days afterwards, Bremen contributed a case in the person of Gerhart H. Woerte, a sailor. Seven days elapsed, when Ireland contributed another—Michael Mahony—from Liverpool. The same day, Bremen offered up another sacrifice—Herman Brantz—just from that city. Three days afterwards, the 10th June, Old England contributed a case in the person of Thomas Hart, from Liverpool. The next day, New England, not to be out done, brought in a sacrifice under the black vomit flag. The same day that England unfurled that flag to the breeze, the ship Evangeline, from Jamaica, came into port with the same flag flying from her mast head. Fifteen days afterwards, viz: on the 25th June, the bark Mary H. Kimball, from Rio de Janeiro, came into port under the same flag. Jamaica and Rio have each been accused of giving New Orleans the yellow fever. If they did, Ireland, Bremen and England should not be held guiltless, as they were all ahead of Jamaica and Rio, in beginning the mischief. Even New England had a victim dying with the el vomito, though not dead, when the first of the two vessels arrived. Rio, however, has been accused of sending us a contagious kind. But when the Mary H. Kimball arrived from Rio, on the 25th June, the yellow fever, we already had here, was gradually spreading in every direction around the black vomit standard, that Ireland, Bremen, Old England and New England had previously hoisted over the heads of that vast army of emigrants quartered in the vicinity of the Triangular Buildings. By referring to the July number of the Edinburgh Review, for 1853, it will be seen that the Rio yellow fever, if ever so contagious here, was not contagious there; because it did not spread to the citizens of the neighboring town of Persopolis, although the sick from Rio were carried there with the Rio yellow fever. There was no emigrant army in Persopolis to mould the abstraction, called contagion, into a substantial form, as there was in New Orleans, with detachments in Mobile, Baton Rouge, Natchez, Grand Gulf, Port Gibson, Vicksburg and other places. During the month of June, 46 deaths from the disease occurred. It continued to spread like a fire, gradually around its original focus. All other parts of the city were entirely free

from it, except a focus subsequently formed in the lower extremity of the city, containing great numbers of poor, destitute persons. Even up to the middle of July, a month and a half after its first appearance, it had made so little progress in coming down town, that the radii from the upper focus had not reached Poydras street, and those from the lower focus had not crossed Esplanade street, leaving seventeen intervening streets untouched. So entirely exempt were those intervening streets from any taint of the infection, up to the middle of July, that I wrote to an esteemed friend, whose lady was at Pass Christian, while he and the children were on his plantation up the river, that the family could meet together in perfect safety at the St. Charles Hotel—informing them that the fever was in town among the emigrants, but not in that part of the city. The family met together at the St. Charles Hotel, on the 18th July, and none of them took the disease. Up to the first of August it was so strictly confined to the emigrants, and newly arrived unacclimated persons within the infected districts, or who had visited those parts of town, that I could hear of no deaths among the citizens proper, or among the negro population. The editors of the Delta will remember that I requested them, during the first week in August, to advertise to ascertain if such a case had occurred; although upwards of two thousand of the stranger population had already fallen. But the request to advertise had scarcely been made, when the disease clothed itself in all its terrors, striking down every one who came in its way—sparing neither man, woman nor child who had not had it before, or been here more than six years. As soon as it passed from the emigrant army to the citizens, it rapidly spread over the whole city—apparently leaving untouched, or touching very lightly, the occupants of the last row of buildings next to the swamp. Yet it spread, to my knowledge, up the river three miles above Carrollton—where I had seven cases in one family—all of whom recovered, and while people were wondering if it could be the yellow fever away up there, four cases occurred in a neighboring family—nothing was done—no danger was apprehended—the disease not being suspected until the third day, when a neighbor visited the house and found three out of the four lying dead in their beds.

Early in the epidemic I visited the disease in its head quarters, and had an opportunity not only of seeing it, but of seeing its causes. A young Pennsylvanian, who had rented a lodging in a three-story house in that dirty part of town between the Triangular Buildings and St. Mary's Market, was attacked with the yellow fever and sent for me. Soon after he had rented the room, the other vacant rooms were rented

by a swarm of newly arrived emigrants, filling them completely, and leaving no space on the floors unoccupied, after disposing themselves for sleep at night. Passing by the doors of the other rooms they were seen each to contain a great number of persons, of all ages and sexes. Some were sick, some cooking, others eating, and the largest number drinking whiskey, all in the same room, not 16 feet square, with windows not only closed, but cobwebs showing that they had not been opened from time immemorial. A foul fœtid vapour, which not only offended the olfactories, but seemed to stick to the palate, issued from those dens and the filthy privies in the back alley. On regaining the street, one of the most filthy of those which the newspapers were blaming as containing the seeds of the infection, the relief afforded to my respiration told me that the press had fallen into a great error; that it was not that dead dog or that poor cat putrifying in the open street, which caused the New Orleans plague, but it told me that I was right in the paper I wrote twenty years ago, proving by the greater speed with which oxidation takes place, and matter decomposes and enters into new combinations, in the open air, in the locality of New Orleans—covering stagnant pools with plants—and the greater freedom of rain water from organized matter than elsewhere, that it is among the most healthy locations for a great city in the world; it told me that it was not matter in the open air, but that which was confined in those temples of Cloacina and in those over-crowded, unwashed, filthy rooms, excluded from the air and filled to suffocation with human beings—themselves full of disentegrated azotized and other compounds—that caused that artificial form of typhus, known as the yellow fever. If New Orleans were founded on a rock of granite, and the streets were jasper, it would be folly to hope to escape the yellow fever, so long as such a combination of morbific causes, as those just mentioned, are permitted to exist. I was much aided in curing the patient, after I succeeded in getting the windows open, by bringing that powerful artillery, heaven's fresh air, to bear upon the enemy. Yet such is the force of prejudice, that the poor emigrants seemed to prefer dying with the doors and windows closed and covered with blankets, rather than to open them and live. Dr. Dowler informed me that more persons died in that part of the town than the supposed number of inhabitants. They were so closely huddled together, it would have been a difficult matter to have enumerated them. It has been ascertained by Dr. Dowler, that in the present and in all former epidemics, the prisoners in the common jail have invariably escaped the disease. This is an important fact, as it goes to show that houses with very thick or

double walls, like the jail, would be, so to speak, fire-proof against the ravages of an epidemic. Double-walled houses, or those constructed on the principle of ice-houses, by preserving an uniformity of temperature, would no doubt prevent yellow fever if the tenants remained in them and did not expose themselves to the sun.

The new customhouse, now building, will probably be a safe asylum against the yellow plague. But the architecture of houses, generally, is so faulty, that many of them are more like ovens in the hot part of the day than comfortable dwellings. As exposure to the sun is both a predisposing and an exciting cause of yellow fever, the covering of the entire side-walks with verandas or galleries, as in Bologna, would greatly tend to prevent the disease. The substitution of the Parisian *cabinet d'aisance inodore*, for the noxious privy, would be a most desirable improvement. Public baths, better water, (viz: purified of its mud and other foreign matters before being distributed,) and better facilities for running it immediately off, are improvements greatly needed to promote health and comfort. The vicious practice of a large portion of the nurses, of closing the doors and windows of a sick room, greatly tends to spread the disease to other members of the family and diminishes the patient's chances of recovery. In a close unventilated room, matter in a state of decomposition is introduced, at every breath, into the system; of which decomposing matter the patient already has more than he can well eliminate—and if the attendants have as much as they can throw off, the introduction of more tends directly to produce disease. A patient in a perspiration, if the bed clothing be well adjusted around him, has nothing to fear from the fresh air—as its respiration will promote, rather than arrest a salutary sweat. The yellow fever is never communicated in a room properly ventilated. In a close room any disease is apt to communicate itself or some other complaint in its stead to susceptible persons. All means to prevent the occurrence of the tropical typhus among us will be ineffectual unless nature's laws be better respected. Nature scorns to see the aristocracy of the white skin—the only kind known to American institutions—reduced to drudgery work under a Southern sun, and has issued her fiat, that here at least, whether of Celtic or Teutonic origin, they shall not be hewers of wood or drawers of water, or wallow in the sloughs of intemperance, under pain of three fourths of their number being cut-off. Until this immutable law, which has made the white race rulers, and enjoined on them a life of temperance and self-control, to qualify them for the high and responsible trust, of preserving in all their purity those wise institutions founded on virtue, economy and

the fitness of things, bequeathed them by Washington and the sword of their fathers, be properly respected, the deaths arising from its violation will continue to swell the bills of mortality, and to lead the world into the error that New Orleans is a most sickly location. Whereas, the truth is, that apart from those who disregard the law just alluded to, and consequently furnish the fire and fuel to kindle epidemics and keep them in action, there is no city in the Union more favorable to health and longevity. The late terrible epidemic, which spread from the emigrants to the citizens, has only, in a few instances, affected those whose home has been here for more than six years. Northern emigrants, by exposing themselves to the hot summer sun of this Southern climate, by out-door labor and drudgery work, and vitiating their blood by intemperate habits, become so strongly predisposed to yellow fever as to fall into that complaint without the addition of any other apparent cause. Hence we have cases of yellow fever every year, and their proportion bears a very close relation to the number of unacclimated persons who attempt to jostle the negro from his stool, and to take from him those out-door, laborious employments in the sun, wisely given to him as a precious inheritance to lift him up from brutish barbarism upon the platform of civilization, by forcing him to expand his lungs and oxygenate his blood.

New Orleans should not be less careful of those young men and young women, flying from despotism and seeking homes and employment among her citizens, than the planter is of the people under his patriarchical government. The newly arrived he never puts at hard rough work, until they become disciplined and trained to regular habits and get accustomed to the climate. She could better afford to cherish, instruct and discipline the emigrants in the hidden mysteries of true liberty, (which is but another name for health, industry, comfort, security, knowledge, temperance and virtue,) considering they cost her nothing. Without some pains be taken with the newly arrived emigrants, experience proves, that the larger portion become a tax and a burden, instead of aiding in building up the Southern emporium into a vast city, as they would do, if met on the levee with the Main Liquor Law, and properly provided for with the thousand and one, light handicraft employments, this climate is so well adapted to; such fancy trades, for instance, as have made Paris, France; where they could work for wages in the cool shade, live healthy, happy and grow in knowledge, virtue and this world's goods, until able to set up shops of their own and to open their doors to the swelling tide of European journeymen and apprentices. There is an element in that portion of the New Orleans

population of French, Spanish and Italian origin, which is not found in the same perfection among any other people in America. That element is taste—a delicate refinement—grace—a keen perception of the beautiful—a passion for the fine arts, and a love of elegant and becoming costumes, trinkets, gewgaws and jewelry. With proper encouragement from the city government, it could be turned to practical account, and half New Orleans be converted into a magnificent bazaar, by those handicraft employments and fancy trades, which are, at present, almost exclusively confined to Paris, Geneva, Venice and Rome. New Orleans would have no rival in America; because it is only within her limits that the requisite element is found, in a sufficiently perfect state, to give birth to those beautiful creations of fancy which adorn Paris and the cities of Southern Europe. The capital of that portion of our population mostly consists of houses and lots, so greatly depreciated in value, as scarcely to pay the exorbitant taxes imposed upon them. The owners are lacking in energy and disinclined to what is called enterprize. If they were to set up any of the fancy trades, alluded to, they could not retain their apprentices or govern or direct their journeymen without the proper legislative aid from the State and city governments. With that aid—costing the State and city nothing—but the enactment and enforcement of wholesome laws, (founded upon the fitness of things and the necessity of the case,) an abundance of employment, consisting of light fancy work, in the shade, could be given to all the young emigrants, male and female, arriving in this port, which would greatly benefit the emigrants themselves, assist in making New Orleans a great city, and at the same time by banishing the necessity, at present impelling them to seek coarse drudgery work in the sun, would prevent the yellow fever. Experience proves that the newly arrived Northern emigrants laboring in the sun, especially if their habits be bad and their lodging apartments close and unventilated, can generate the yellow fever anywhere in the South—on barren rocks or sandy plains. That disease is not inherent in the soil of any particular locality, but is an artificial complaint, like typhus or camp fever, which can be driven out from any locality whatever, by a wise and cheap republican government—not only seeking the greatest good of the whole number, but the greatest good of every class composing the whole number. Our present oppressive anti-republican system of exorbitant taxation, puts the comforts and conveniences of life beyond the reach of those vast hordes of emigrants constantly arriving in this city, stript of everything and perfectly destitute. Finding no ready employment, except coarse, rough drudgery work in the sun, which their

feeble health, from a long sea voyage, disqualifies them to perform, they sink down to a depth below observation, hiding themselves in alleys and dens of vice, ignorance, filth and misery; where the seeds of disease are in constant fermentation; where they are constantly dying and swelling the bills of mortality, summer and winter, without any one being sick—at least any one known as a citizen. The physicians of the city seldom or never see them—they are sunk too low in ignorance to know the necessity of calling in medical aid when ill, and they are too poor to buy food, much less physic—and if they had it, they are too ignorant and obstinate to use it. There is scarcely a city in Europe, which could show as many wretched, destitute poor, crowded together in the same space, as that part of New Orleans, where the late epidemic broke out and committed its greatest ravages. While the bills of mortality were running up to an hundred and fifty a day, no one seemed to know anything of the people who were dying. So deep were they lost to the eye of observation, that the members of the Howard Association and other charitable persons, among whom were the physicians themselves, had great difficulty in finding them before death had struck them and made them his own. But for the almost superhuman charitable exertions of the citizens of New Orleans in their favor, the figures of mortality would, no doubt, have run up to twenty or thirty thousand instead of five, before the relief, from the generous hearted American people throughout the Union, began to come in. With that opportune and welcome aid, the epidemic was disarmed of most of its terrors, and its mortality limited to about five thousand more, making a little upwards of ten in all, which would otherwise have swept nearly the entire population of the unacclimated poor—in numbers, perhaps, exceeding seventy thousand. That it was an artificially created disease, engendered by the ills of extreme poverty, is proved by the fact, that as soon as the American people reached out their hands, scattering the comforts of life among the poor, destitute emigrants, the terrible scourge began rapidly to disappear. That there should be some instances of its having been, to all appearance, communicated in close apartments, is not a matter of wonder to any one acquainted with the laws of that class of diseases belonging to the genus typhus—as they are all more or less communicable, under certain circumstances, in a confined atmosphere.

It is all a delusion that a quarantine, ever so rigid, can protect New Orleans against yellow fever, while causes continue to exist in her midst, more than would be sufficient to create plague or pestilence in any other city in the world. High taxes are at the bottom of those causes, and the toleration of intemperance and idleness is at the bottom

of the high taxes. In 1821, the rigid quarantine system was tried, and continued in operation four years. On the 19th February, 1825, the Louisiana Legislature repealed the quarantine laws, and directed the grounds belonging to the establishment to be sold. It had proved to be ineffectual. The very next year after its establishment, the city was scourged by a severe epidemic. It was at first supposed, that the fever had been imported from Pensacola by eluding the quarantine, coming in by the Bayou St. John. But it was ascertained by the surgeon of the United States Army, that the first case of yellow fever in Pensacola, in 1822, came from New Orleans a month previously to its alledged importation from Pensacola.

In the first sentence of this essay on Political Medicine, (a science in its infancy,) written by snatches, amidst the labors of professional business, I alluded to a short, cheap and safe way of preventing yellow fever, which has never been tried in New Orleans. The best measures, in my humble opinion, to prevent its generation within the city, have already been glanced at, and it now only remains to say what that short, cheap and safe way is, which can prevent its introduction from abroad—admitting, for the sake of argument, that it can be introduced in that way and spread to the inhabitants. It can be told in few words. It is simply to insulate the shipping with well acclimated negroes, and to let no other class of people act as stevedores, or to come within a specified distance of the wharf—precisely as small-pox is insulated by those who have had the disease in the natural way. Negroes are perfect non-conductors of yellow fever. Whereas, if there be anything contagious in that malady, (and it is admitted there may be, as in other species of the genus typhus, when shut up in a close room, or in the forecastle, or between decks of a ship,) to admit newly arrived emigrants to act as stevedores, or as draymen, to handle the packages before they are sufficiently aired, would be as dangerous and as cruel as to send ignorant persons with gunpowder in their pockets to handle hot ashes. The insulation of the shipping, by non-conductors at the wharf, would be more effectual than the most rigid quarantine at Fort St. Philippe, or anywhere else. Besides it would have the merit of involving no expense, and of throwing no obstructions in the way of commerce.

To prevent the yellow typhus from originating among us, let pure republicanism be called into requisition, to prevent the ills of extreme poverty and its train of attendants, vice, ignorance, intemperance, idleness, exposure to the sun by day and crowding together in small, filthy, close rooms at night; should it nevertheless spring up, to prevent it

from spreading, give it air and scatter republican comforts among its victims, and insulate them by a cordon of non-conductors, in the shape of negroes.

Canal-street, Sept. 30th, 1853.

IV.—HÆMATOLOGY, OR THE MOTIVE POWER OF THE BLOOD.

BY C. SMITH, M.D., MISSISSIPPI.

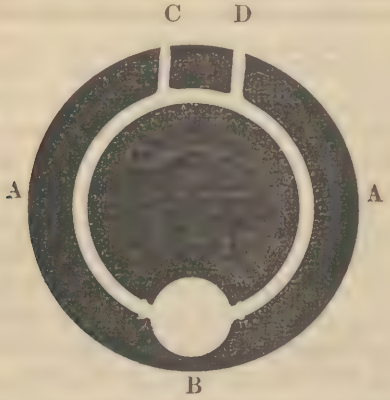
This subject, so frequently presented to your readers, may seem to be worn thread-bare; but having been one of the first to oppose this vague hypothesis, founded entirely on conjecture, I may hope to be excused, by those concerned, for the liberty I now take (for the first time through the press,) to disprove the Willard theory of the circulation of the blood. But as a scholar and founder of educational systems, no lady enjoys a higher position than Mrs. Emma Willard.

At the time Mrs. Willard presented her views to the public, her friends advised her to present them to me, as I was then lecturing upon anatomy and physiology. She did so, and witnessed my demonstrations; but was only more and more confirmed in her belief. About this time she sent a document to a medical gentlemen in Connecticut, and one to Professor Edwards, of Paris. But, before anything was published upon the subject, I came South, and had heard or seen nothing until it made its appearance in the New Orleans Medical Journal. Believing that the science demands the truth, it will be my object to present a few facts, (partly from notes taken at the time) as they appeared in 1840 and 1841.

NOTE.—Mrs. Willard informed me that, on ascending Mount Ida, in or near the city of Troy, she noticed that, as her respiration increased, her system became warm, and thereby the heat of the body was generated in the lungs. But on investigating the subject, she learned that Harvey had made the same discovery long ago. Being disappointed in her plans, and unwilling to give up her favorite subject, she was determined to make something out of her researches, and finally hit upon "*the expansion of the blood by the addition of caloric, and denominated it the primum mobile.*"

I will not give her argument here, but merely show one diagram of her apparatus, by which she expected to prove her theory of the motive power being in the lungs.

It consisted of a bent tube, connected at the bottom (as it was hanging upon the wall) of the circle by an *India rubber bag*, holding two or three gills. The bent tube had two pipes at the summit of the arch, representing the arteria innomata and the left carotid. The whole being filled with water, the philosophy of motion, "*by expansion from caloric*," was proved by pressing the *India rubber bag with the hand.* By pressing the bag of India rubber, being confined to each end of the bent tube by a ligature, the water would rise through the open tubes C and D, *just in proportion to the amount of pressure made by the hand.*



A A. Bent tube.
B. India rubber bag.
C. Artéria innominata.
D. Left carotid.

I will here leave the reader to his own conclusions of the above apparatus and its *modus operandi*; only asking your attention to a few words upon the laws of caloric, capillary attraction and motion.

1st. The law of caloric is to *gain an equilibrium.* It is not for us to prove that the blood has a greater or less amount of heat; but to trace *cause and effect.* What then is equilibrium? It means that the heat is *diffused throughout*, pervading the whole system. This, then, is our condition, not only from birth, but beginning in *embryo.* What causes heat in the latter, and why is the *heart* in the *fœtus*, the *primum mobile* and not in the adult. If "*expansion*" is the cause of motion in the blood, and that too by caloric generated in the lungs, let us see what are the chances for such motion. Are the pulmonary veins (*they being the seat assigned for this expansion*) any nearer the source of heat than the pulmonary arteries? Is the blood changed from dark to florid,

merely to give heat? When does this change take place, in the pulmonary arteries, capillary vessels or pulmonary veins? Is it not produced in *all*, but *chiefly in the capillary vessels?* And if in the capillary vessels, when can you find expansive power for the pulmonary veins? Suppose that the blood in the pulmonary arteries should be 1 ° or 2 ° less than the pulmonary veins. What would the expansive power of the blood, produced by 2 ° amount to? Would motion, produced simply by such expansion, be perceptible? It is very obvious that, to obtain motion in fluids, from the addition of caloric, there must be a great difference in the temperature of the blood in the pulmonary arteries and veins.

2d. Capillary attraction is one source of circulation of the blood in the lungs; yet no one has ever pretended that it was the *primum mobile*. Motion thus produced, to a certain extent, is independent of the law of gravity and motion; but dependent upon a uniform supply of blood,—*adaptation of fluid, and calibre of vessel*. This motion is perceptible, though not attended by force, strictly as in the following.

3d. Motion, philosophically considered, depends upon *force*. Is there any motion to the blood? then there is force. Does the expansion of fluids by caloric exhibit force? So does the *absence* of caloric; as in the case of water freezing in pipes, evinced by bursting the strongest metal. But when a fluid is admitted to free motion; the addition of caloric, to a certain degree, produces *no force or perceptible motion*. The motion of the blood, to a certain extent, in the pulmonary veins, is produced nearly equally, by *inertia—force—and the law of fluids finding their level*. Inertia is a law of fluids; and is applicable to fluids in motion as well as at rest; and therefore is applicable to circulation in the veins. Capillary circulation, though feeble, is a second cause of motion; and the evacuation of the left auricle causes an evacuation of the pulmonary veins also, and thereby co-operating with other causes of circulation.

The laws of *mechanics* and *force* in the structure of the *heart* are so fully and perfectly developed, that it is scarcely necessary to take into consideration, a single cavity of that organ for our present purpose.

Mississippi, Sept., 1853.

V.—MATCH PHOTOGRAPHS, OR CAMERA LUCIDA DRAWINGS OF MICROSCOPIC OBJECTS FOR THE STEREOSCOPE, MADE BY MEANS OF THE ORDINARY MONOCULAR MICROSCOPE.

BY J. L. RIDDELL, M. D.

Professor of Chemistry, in the University of Louisiana.

Professor Wheatstone, the eminent physicist, in connection with his remarks upon the value of the binocular microscope, in the July number of the London Microscopical Journal, suggests that the monocular microscope may be made to give match stereoscopic pictures, by successively changing the inclination of the axis of the objective and ocular to the stage holding the object. This plan, though not easily made applicable to microscopes of the present construction, must, I think give excellent results with the low powers, say with the two inch and inch objectives, and possibly with the half inch. But with the higher powers of large angle of aperture, the close proximity of the front surface of the objective to the thin glass cover of the objects, totally precludes its being put in practice.

The method described below may be readily adapted to any microscope, at an expense comparatively trifling; it is applicable to every grade of objective; and upon fair trial I find it to give satisfactory results.

Behind, and close to the objective, insert an isosceles glass prism, say a half or a quarter inch equilateral or rectangular prism, adjustable for position, and capable of being inclined at pleasure any required number of degrees, on a central axis transverse to the axis of the ocular and objective, said axis being parallel to the polished faces of the prism. When the hypothenuse or reflecting surface of the prism is made coincident in direction with the axis of the microscope, the position of the prism being appropriate, the light traveling from the objective to the ocular, will suffer reflection in its transit through the prism; but the appearance and position of the field, except its reversal in one direction, will be essentially the same as if no prism were there. By inclining the prism a little, other objects are brought into view, as though the slide containing them were moved. If, now, the slide be readjusted, so as to restore the field as at first, the objects will be seen from a different point of view, and will therefore wear a modified appearance.

The mode of proceeding is as follows: two good successive views of the same object are to be obtained, between which there must be a difference of inclination of the prism, say from four to eight or nine

degrees, according to the depth of stereoscopy desired. In each instance, the principal object is brought to the centre of the field, by adjusting the position of the slide. In each instance, a careful camera lucida drawing is to be made, or a photographic impression taken; which, when properly viewed, each by an eye, will be found to coalesce into a single image, manifesting the fine stereoscopic effect, which characterizes the image seen through the binocular microscope.

VI.—ON THE BINOCULAR MICROSCOPE.

BY J. L. RIDDELL, M. D.

Professor of Chemistry, University of Louisiana, New Orleans.

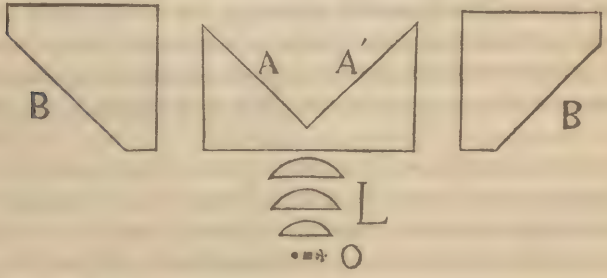
Read before the American Association for the advancement of Science, Cleveland Meeting, July 30, 1853.

The improvements in the microscope, described in the following article, promise important aid to the working physiologist and pathologist. We have, therefore, solicited and procured from the author, a copy for publication.—ED.

It is proper to premise that some brief notices of the binocular microscope, [devised in 1851—constructed in 1852,] have already appeared in Silliman's Journal and elsewhere. I now desire to submit a few remarks and explanations to the members of the American Association, and at the same time to exhibit different forms of the instrument, so that the members interested in the microscope, may form a definite opinion of the value and utility of the improvement.

The following diagram (Fig. 1,) will serve to illustrate the method first devised and put in practice. It shows as a longitudinal section of the position of the objective and the prisms for producing binocularity.

Fig. I.



O represents the object to be seen.

L, the objective combinations, always brought as near as practicable to the prisms.

A. A', two isosceles rectangular prisms of fine glass, in contact by edges somewhat ground away. The light entering the prism A through the objective, suffers internal reflection on the hypothenuse A, and emerges from the prism in the direction of B. Entering the prism B, it is restored to its original direction. So likewise, that part of the luminous pencil entering the prism A', emerges nearly parallel from the prism B'. The prisms B and B' are adjustable to different distances apart, and have likewise an axial adjustment in the plane of the section represented; the first, that they may be made to correspond to the interval, between the two eyes of the observer; the second, that the direction of the rays, traveling from each point of the object through these prisms, may be such as will seem to the observer natural and unconstrained, and with clear, coincident fields.

In the smaller instrument before you, this arrangement is observed. Used without eye-pieces, it gives a stereoscopic and perfectly satisfactory result. This instrument was constructed for a dissecting microscope. I use it with lenses, whether plain, doublets, or achromatics, from ½ inch to 3 inches focal length. The image is erect and orthoscopic. Objects can be viewed as opake or transparent, and there is attached to it a flexible pipe, connected with a delicate cylinder and piston, which in one respect, is made equivalent to a third hand. Tightening a screw, and taking the ivory termination of the flexible tube in the mouth, the focal distance of the instrument can be varied at pleasure with the breath. In very minute dissections, when the two hands are simultaneously employed with the hook and needle, I have found this method of holding a focus of the greatest utility and convenience.

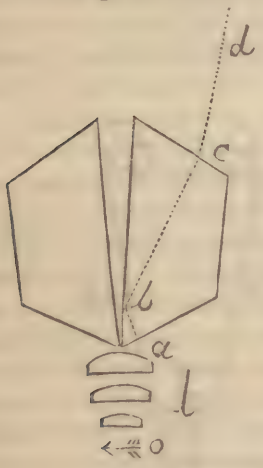
If over B and B', single oculars be placed, the binocular vision is found to be pseudoscopic; that is, depressions appear as elevations and elevations as depressions. With erecting, or double eye-pieces, analogous to those of the terrestrial telescope, the vision again becomes orthoscopic. On this account, I prefer to reserve this form of instrument for use without eye-pieces, in the manner described, and to construct the compound binocular microscope, on a plan which I will soon explain.

Binocular Magnifier.—I have found that for the magnifying glasses used by artists and naturalists, glasses having a focal length of one or two inches and more, a less complex and more economical arrangement can be adopted. Namely, the reflecting surfaces A A' and B B', Fig. 1 can be substituted by pieces of common looking glass, or plate glass

silvered. The first surface reflections are too faint to interfere materially with distinct definition. The two mirrors of the pair, on each side of the nose, are hinged together on the principle of the parallel rules. The whole arrangement is mounted something like a pair of spectacles, while the requisite lenses are adapted to be centrally attached when required. I regard the binocular magnifier as supplying a great desideratum, to large classes of persons pursuing a great diversity of callings. The effects, so often prejudical to vision, of inordinately using one eye, are thus avoided. A perfectly natural relief, or definition of bodies in depth, as well as in extension, is thus attained.

Binocular Compound Microscope.—In the larger instrument before you, only two prisms are used for subdividing the light after its passage through the objective, and for directing the luminous pencils to the separate oculars. In this case orthoscopic vision is produced by the ordinary single oculars. The light suffers one, instead of two reflections, as in the instrument before described. The arrangement of the prism is shown in section below.

Fig. II.



o. the object to be seen.

l. the objective, above and near to which, is shown the two prisms.

The internal reflection takes place upon the two long sides, which are in apposition at a small angle, which admits of adjustment in the plane of the section shown, the lower termination always remaining in contact. The light through the objective which impringes upon *a* is, that part of it which enters the prism, refracted to the left, so that it meets with the reflecting surface *b*. Suffering total reflection it emerges from the surface *c*, where from the necessary identity of the immergent and emergent angles, it is refracted to the right, so as exactly to compensate for its previous refraction to the left. This implies that the upper and lower angles of the prism are equal.

In the instrument before you, these equal angles are 45°. The ray of light in pursuing the paths *a*, *b*, *c*, suffers a minute chromatic dispersion; inasmuch as, by the refraction and dispersion at *a*, the red, violet, etc., will be found somewhat separated at *c*. Thereafter, in traveling in the direction *c d* to the ocular, the red and the violet will

move in parallel paths, so that no further dispersion will occur. Upon a close scrutiny into this matter, I find that it does not practically lessen the sharpness of definition, unless an eye-piece of unusually high power be made use of. The minimum limit of angular definition, perceptible by the human eye, is about 45 seconds of a degree (45".) The extreme dispersion occasioned by the prism as above, may be kept handsomely within this limit. This can be shown, both by calculation and experimental demonstration. By making the equal angles of the prism near 85 ° or 86 °, so that the immergence and the emergence shall be at right angles to the glass planes, this theoretical dispersion can be avoided. But, practically, in this case, the usefulness of the prism would be destroyed, by the interference of light directly transmitted through without reflection. Prisms with equal angles of 60 ° will probably be found as appropriate as any.

It would be inappropriate to consume much of your time, in explaining the mechanical details of this instrument. The following sketch will assist you to comprehend the essential peculiarities of a plain, firm, comparatively simple stand, with all the most important adjustments.

Fig. III.

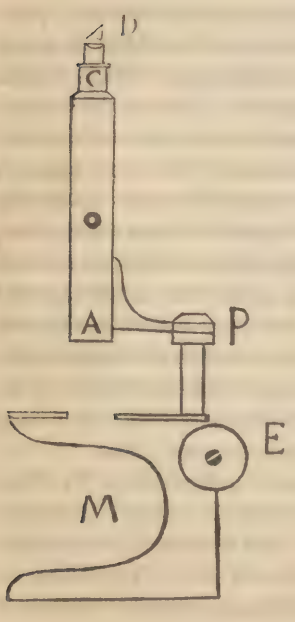


Fig. IV.

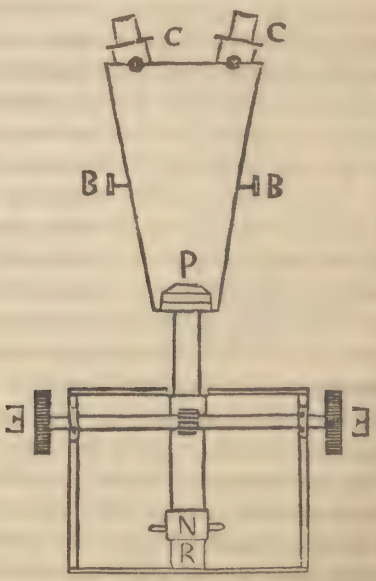


Figure 3 represents a side view of the instrument. The stage is immovable, being firmly supported, so as not to spring sensibly under considerable and sudden pressure, it extends six by four inches. The optical parts are supported by a stout triangular gun-metal bar, bearing rack work, and moving up and down, by a pinion terminating in large milled heads, one of which is shown at E. For the convenience of changing objectives, the arm carrying the optical apparatus, has at P nearly a half revolution, so as to carry it off the stage. The prisms are at the bottom of a brass box at A. One of the oculars is seen as fitting into an adjustable tube C. A small rectangular, or equilateral prism is so mounted in a brass cap, as to be slipped at pleasure over the eye-glass. This little prism is adjustable in the plane of the drawing, on an axis transverse to this plane, so as to erect the image seen, and at the same time, allow of its being viewed at any inclination between vertically and horizontally, which may be convenient to the observer. It will be seen that the prism at A has the effect of erecting the image in one plane; while the small prism at D can be placed so as to erect it in the plane precisely transverse. Thus the movements upon the stage will be seen through the instrument to be natural or erect; a condition essential to the convenient manipulation or dissection of a microscopic object. M represents the position of a concave mirror, or other apparatus for illuminating transparent objects. Two small mirrors will be found more satisfactory than one large one; as the operator can then easily secure a good light to each eye, which is sometimes difficult with a single mirror.

Figure 4 exhibits a back view: the common letters, or letters common to both, referring to the same parts as in figure 3. Thus C C are the adjustable tubes into which the oculars fit. These tubes are hung upon axes, so that their inclination to each other may be varied; and the whole arrangement slides at pleasure, horizontally, in order to adapt the distance to the eyes of different observers. B B are the milled heads of screws, for the adjustment of the inclination of the prisms, as explained in connection with figure 2. R is a brass tube surrounding the box in which plays the triangular gun-metal supporting bar, before explained. Concentric with R, and moveable thereon is N, a short brass tube carrying the illuminating apparatus.

Let the observer using the instrument carefully illuminate the object to be seen, than after adapting the line of vision to the natural requirements of the pair of eyes, duly alligning and superposing the corresponding images, and carrying them into the corresponding parts of the two circles of light as defined by the diaphrams of the oculars; and

lastly, regulating the focal position of the object from the objective, all of which can be readily accomplished by the various adjustments; let him now place two good eyes of equal power, in proper position near the eye-glasses, and a magnificent field will present itself to his sight. He seems to look through a circular window or port hole, say two feet off, and a foot in diameter, a foot or two beyond which, his microscopic objects, perhaps, seemingly hung in mid air, stand out in all the boldness and perfection of relief, and definiteness of position in width and depth, which he has been accustomed to realize without glasses in the natural objects around him.

It does not appear to him that any glass or other artificial medium is interposed between his eyes and the objects seen. The vision fatigues him no more than does a landscape, or the inspection of the implements and objects upon the table before him. A drop of water teeming with algæ and living infusoria, looked into in this way for the first time, impresses upon the beholder, even though he be a veteran microscopist, a profound sense of the sublimity of nature in her lesser spheres; and a vivid consciousness of beholding the microscopic world in a new and palpable condition.

By varying the inclination to each other, of the luminous pencils entering each eye, the objects can be made to appear definitely nearer or further off, at pleasure. In these cases the parallax, or apparent angle subtended by an object remains constant, while the apparent size, varies of course with the apparent distance. Thus, a mite of a wheel animalcule, one hundredth of an inch long will, perhaps, appear to be a foot off and as large as a mouse; but bring the prisms nearer together, and tilt the oculars to correspond, and the image waxes marvelously immense, and taking a position, perhaps, apparently more than a hundred feet distant, the being too small to be seen with naked eyes, vies with the great whale of the ocean in size; wearing an aspect more awful to behold than the savage beasts of an African forest, and clearly exhibiting a complex transparent structure, more unique and wonderful than the mind of man can well conceive.

This instrument with it firm stand, broad stage and erect images, is pre-eminently adapted for use, in prosecuting minute dissections, or unraveling of minute structure of any kind. Opake objects may be illuminated by the bulls-eye condenser; and transparent objects by two concave mirrors, aided by two diaphragms or screens, or one large concave mirror and two screens. At night, two candles may be used conveniently with one mirror. To illuminate for the higher powers, a single achromatic condenser suffices.

Almost any model or form of monocular compound microscope extant, can be modified to become binocular, on the principles here explained, in connection with figure 2. In one respect, it would be convenient to adopt the trunnion mounting of Spencer; the whole instrument might be tilted, so as to use conveniently the camera lucida in drawing. This would detract from the firmness and simplicity of structure so essential in a dissecting microscope, and add materially to the cost, a circumstance of importance to some who might wish to possess it.

As it is, the instrument can be readily braced up at an angle near 45°, at which angle Natchet's camera lucida, works in drawing, with perfect satisfaction. If the same object be drawn as seen through each ocular respectively, a difference between the two drawings is perceptible, similar to that between match stereoscope pictures; so that if these two drawing be viewed, each with the appropriate eye, the natural relief of the object is reproduced. I have already suggested in Silliman's Journal, the propriety of publishing such drawings, appropriately placed upon the paper, in illustration of natural history and histology.

VII.—YELLOW FEVER IN MISSISSIPPI.

RODNEY, MISS., October 3, 1853.

Dr. A. Hester:

DEAR SIR: On the 29th ult., I addressed you a letter,* informing you of the appearance of yellow fever, on several plantations, in this neighborhood, since which time cases have continued to occur; but in no instance has the disease attacked any except those who have been exposed by their attention to the sick. Every possible precaution has been taken to prevent the disease spreading, and thus far the means used have been effectual. I verily believe, if the infectious nature of this fever had not been observed soon after its appearance in the neighborhood, it would have spread extensively. It was first pronounced bilious fever, then dengue, and it was not discovered to be yellow fever until many had been exposed to it, and contracted the disease.

I cannot see how any one familiar with yellow fever can maintain its identity with bilious fever. It is a disease differing, as I conceive, essen-

* We did not receive the letter alluded to.—ED.

tially from the hepatic variety or modification of remitting fever; the yellow appearance of the eyes and skin, not being dependant on, or necessarily connected with hepatic derangement, but an altered condition of the blood. The eyes, in some cases, present a greenish yellow hue, similar to what is observed sometimes in chlorosis. This appearance has been common in negroes. The cause of yellow fever may effect this lesion of the blood without giving rise to any febrile symptoms whatever, it being followed by prostration and an enfeebled circulation, the result of defective innervation. Yellow fever may engraft itself on any existing indisposition, as intermitting and remitting fever, etc., and in such cases the operation of the special cause may not be detected until the appearance of hemorrhage or black vomit. Indeed, in some instances, so stealthily does it progress in its work of death, that the patient himself may be unconscious of the mischief it is working in his system, until he finds himself fully within its grasp. Although local inflammation may occur in the course of yellow fever, I do not consider it essentially an inflammatory disease, but the febrile commotion seems to me as I stated in my former letter, attributable to cerebro-spinal irritation; and when coma and delirium come on, in the latter stages of the disease, they are probably referable to the deleterious action of the altered blood upon the brain.

You may expect to hear from me again, on this subject, as I have time to write.

In haste, respectfully,

WM. G. WILLIAMS, M.D.

VIII.—YELLOW FEVER AT GRAND LAKE.

Arkansas, Sept. 12, 1853.

Dr. A. Hester:

Dear Sir—I propose to give some account of a few cases of fever which have occurred in my practice recently, as you solicit reports of the symptoms and treatment of endemic and epidemic diseases, from correspondents.

The town of Grand Lake is situated on the bank of the Mississippi river, one hundred miles above Vicksburg, by water, between the river and a lake, from which the town takes its name. This lake, in consequence of timber falling into it, etc., has a very filthy and marshy margin.

The country, two or three miles back, is subject to inundation from the river, in high water. Fevers of almost every variety abound in this region; the most common of which is the intermittent fever.

On the 20th of August of this year, the steamer Bunker Hill, from New Orleans, landed at the town to wood, and put off a passenger, and in the meantime buried a man who had died with the yellow fever. Had at the same time several cases on board.

I was called on the 24th, at 12 o'clock, to see the passenger. He came from Natchez, on the Bunker Hill. Had been sick two or three days, Says he was taken with a chill, followed by fever; severe pains in the back and limbs; nausea and vomiting; tongue coated with a yellowish white fur; had taken blue mass at the outset, and continued taking quinine until the present time. When called, found he had fever, hot and dry skin, pulse 120, tongue dry, brown and chapped or fissured; somewhat delirious; extreme thirst; bowels constipated; stomach very irritable and tender on pressure; vomited blood; respiration hurried, eyes red and watery.

Treatment—Gave him immediately 10 grains quinine, 15 grains calomel, half grain sulph. morph.; mustard sinapisms to epigastrium.

At night—Skin still dry and hot, gave him 20 grains quinine, half grain morphine, 10 grains calomel.. (Heroic!—Ed.)

Second day, morning.—Found his bowels had been opened during the night; skin and tongue moist; pulse reduced; stomach not so irritable, being little delirious, no pain in the head; rested well. Gave him 20 grains quinine, opium 2 grains; lemonade and ice for drink.

Night—Skin and tongue still moist, pulse reduced, had been perspiring freely during the day. Some irritability of stomach—attempted to take some nourishment, said he was hungry. Very little if any delirium, no pain in the head; a little deaf. Gave 20 grains quinine, half grain morphine, calomel 10 grains; cantharides to epigastrium.

Third day, (27th,) *morning.*—Had been up and exposed himself during the night. In attempting to put on his clothes, had fallen on the floor and bruised his hip a little; skin and tongue slightly moist and cool; skin had a yellowish tinge—was before very fair. (The yellow was more perceptible at the lower part of the abdomen.) Epistaxis—some

irritability of stomach. Gave 10 grains quinine, half grain sulph. morphia, acetate of lead 2 grains.

At noon—Skin still moist, epigastrium very tender on pressure; skin of a deeper yellow or orange color; nose still bleeding occasionally. Lost but little blood, however. *At 3 o'clock*, black vomit; shortly after discharged a dark-colored matter from the bowels, and afterwards had hemorrhage from the bowels; at 6 was raving with delirium, and at 7 was dead.

This is, as near as I can remember, the symptoms and treatment. I gave him, occasionally, other unimportant remedies, etc.; but I took no notes at the time, and cannot remember.

On the 26th, there were three other persons residing in the town, taken with precisely the same symptoms. They were promptly relievd by large doses of calomel and quinine.

Previous to the arrival of this steamer, there had not been a single case attended with these symptoms. I attended many cases of fever and would have known if there had been such. I am satisfied, that the first case mentioned, would have been relieved by the 20 grain doses of quinime, if it had not been for exposure and want of proper nursing.

About the same time as the attack of the last three cases mentioned, there was another similarly attacked on the river, two miles above. Was at the landing while the steamer was lying there. I did not attend him. It was at first diagnosed bilious fever; skin became yellow; tongue black; stomach very irritable; vomiting constantly. The attending physician said he had black vomit, and discharged a dark-colored matter from the bowels. His skin afterwards became clear, and he seemed for awhile better, but his stomach again became irritable, and continued so in defiance of all treatment. After lying for sometime, he died.

I think all these cases could be distinctly traced to the Bunker Hill. Some of those attacked were on board of her, while she was here.

I give you these facts, which you can use as you think proper.

Very respectully,

D. NATHANIEL JONES, M.D.

Part Second.

EXCERPTA.

I.—*Annals of Micrology.*

BY ROBERT D. LYONS, M. D.

Continued from page 219.

DEFINITE MORPHIC ELEMENTS.

Elements of Blood: Origin and Destination of the Blood-corpuscles.—Bennett* has investigated this highly interesting and important problem, about the solution of which two opinions have chiefly prevailed; one being, that the colored corpuscles are formed from the colorless (by direct transformation, Paget;) the other theory maintaining, that while such may be the cases in fishes, reptiles and birds, in mammals the colored disc is merely the liberated nucleus of the colorless corpuscles (Wharton Jones.) From his own researches Bennett inclines to the latter view; the paper contains the results of several observations connected with his researches on leucocythemia, which appear to him sufficient to establish this opinion. The following are his conclusions:

1. That the blood-corpuscles of vertebrate animals are originally formed in the lymphatic glandular system, and that the great majority of them, on joining the circulation, become colored in a manner that is not yet explained. Hence, the blood may be considered as a secretion from the lymphatic glands, although in the higher animals that secretion only becomes fully formed after it has received color by exposure to oxygen in the lungs.

2. That in mammalia the lymphatic glandular system is composed of the spleen, thymus, thyroid, supra-renal, pituitary, pineal, and lymphatic glands.

3. That in fishes, reptiles and birds, the colored blood corpuscles are nucleated cells, originating in these glands; but that in mammalia they are free nuclei, sometimes derived as such from the glands, at others developed within colorless cells.

4. That in certain hypertrophies of the lymphatic glands, their cell-elements

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* Edinburg Monthly Journal, March, 1852.

are multiplied to an unusual extent, and under such circumstances find their way into the blood, and constitute an increase in the number of its colorless cells. This is leucocythemia.

5. That the solution of the blood-corpuscles, conjoined with the effete matter derived from the secondary digestion of the tissues, which is not converted into albumen, constitutes blood-fibrin.

Whatever importance may be attached to these conclusions as regards the adult, it must be borne in mind that blood-corpuscles are formed under conditions in which there can be no connexion shown with glands of any kind, as in the ovum; and again, as we learn from M. Lecanu's researches, that while fibrin is abundant in the serum, it exists only in the *envelopes* of the globules.

Micro-chemistry of Blood.—M. Lecanu has presented to the Institute a memoir on the blood,† in which he takes up the solution of some highly important questions—viz., the origin of the fibrine; the separation of the globules from the other constituents, and the determination of the chemical constitution of the globules. Having satisfied himself that a concentrated solution of sulphate of soda, which prevents the precipitation of fibrine, is without action on the globules, he received a quantity of blood into a solution of sulphate of soda, at a temperature of 12°, and marking 12° Baume. The mixture was then filtered; the globules remained on the filter, while the serum passed through; from the latter, on the addition of eight or nine times its volume of water, the fibrine was precipitated in gelatinous filaments, scarcely a trace of it remaining in the filtered liquor. As it is subsequently shown that the globules contain but very little fibrine, and that only in their envelopes, it follows that this substance is contained in the serum chiefly. In order to obtain the globules perfectly free from serum, it is only necessary to allow them to remain on the filter, and wash them with the saturated solution of soda. When obtained thus, M. Lecanu finds that they consist of not less than eight different substances—1. Hæmatosine; 2. Globuline; 3. A very small quantity of albumen; 4. A fibrinous matter, constituting their envelope; 5. An animal extractive matter soluble in ether and alcohol; 6. A fatty matter; 7. Various salts, amongst which are chlorides, phosphates, and alkaline carbonates; and 8. Water, which holds all these matters, with the exception of the envelope, in solution. Water, it is well known, breaks up the globules, leaving their envelopes isolated, and dissolving their contents; by boiling the solution, the globuline, hæmatosine, and albumen, are coagulated. Hæmatosine is soluble in alcohol and ether at ordinary temperature, giving to the solution a beautiful red color of blood, and by spontaneous evaporation forming small lamellæ of a metallic lustre, and an amethyst-color, exactly like the red silver of mineralogists. M. Lecanu believes in the presence of iron in the blood, but does not express himself definitely as to its particular mode of combination. (He suggests with regard to hæmatosine, that there is reason to think that it would be an excellent substitute for the combinations of iron exhibited in chlorosis and other affections. The difficulty of procuring it in sufficient quantity is, however, considerable; the largest quantity he obtained being about 30 grains, from somewhat more than one pound and a half of ox blood.) With regard to albumen in the globules, the commissioners appear to think that as it is in such very small quantity in these little bodies, swimming in a highly albuminous fluid, its presence in them may be due to absorption or endosmose. The following contrast between the two chief constituents of the blood is highly interesting and valuable: "It results," says the commissioners, "from these observations, that the animal matters which compose the serum are essentially different from those which compose the blood-corpuscles. The serum contains

† Read to l'Academic des Sciences (de l'Institut,) July 5, 1852; reported on by MM. Thenard, Dumas, and Andral.

but albumen and fibrine; no globuline—no hæmatosine; the globules, on the contrary, contain hæmatosine and globuline, with a fibrinous matter, but no fibrine, and only a little albumen."

Nucleolated Red Corpuscles in Blood.—Mr. George Busk* has recently met with an example of this very rare condition. In his Memoir on the Blood, in the "Philosophical Transactions" (1845,) Mr. Wharton Jones states that the bodies are common in the blood of the horse and elephant; but they appear to have occurred to his observation but once, and that doubtfully, in the blood of man. Mr. Busk's observation was confirmed by Mr. Huxley; only one corpuscle was seen, but in a very distinct and clearly defined manner. The nuclear portion of the corpuscle was rather smaller than most of the free blood discs, but not so small as some of them, nor, apparently, much, if at all, below the mean size. On proceedings being taken to make an accurate measurement of it, it disappeared; no others could be found on prolonged investigation of the same blood, which was taken, about an hour after breakfast, from a young and vigorous man.

Blood-Corpuscles Nucleus.—Harting† advocates the existence of a nucleus in the human blood-corpuscle. When treated with a solution of corrosive sublimate, the corpuscles become remarkably altered in form; they contract about one-tenth of their size, assume a spherical shape, and at the border a small, generally round or oblong body passes out. This he considers to be the nucleus. This distinguished observer further remarks, that in the blood of the same animal much difference will be found in the comportment of the corpuscles to a solution of given strength, some being more and some less affected, apparently according to the manner in which they are presented to the reagent.

Absolute quantity of blood corpuscles.—Quantitative Microscopical and Chemical analysis of the blood-corpuscles and blood-fluids.— Under this head a memoir has appeared from the pen of Vierordt,‡ who proposes to estimate the quantity of the blood-corpuscles in a given volume of blood by actual numeration of them under certain conditions. The mode of procedure is as follows: a small quantity of blood is taken up by a capillary tube of 0·1^{mm} diameter, and uniform in size throughout. The length of the blood column is measured under a low magnifying power, and this quantity multiplied by the known diameter of the tube gives the total volume of the column. The blood is now allowed to run from the tube on a glass slide, and the last particles are washed from the tube with a solution of gum or albumen, with which the whole is now mixed, and uniformly spread on a glass slide in narrow striæ, three to four inches long. With a finely pointed glass tube the corpuscles are distributed with as much uniformity as possible. The blood-streaks are then successively brought under the divisions of an ocular micrometer, and the number of corpuscles in each carefully counted. The smaller the diameter of the capillary tube, and the volume of blood employed, the less difficult will be the process of reckoning. Where we only wish for approximative results, a measured volume of blood may be mixed with a known quantity of a menstruum, such as gum or albumen solution, and a microscopic quantity of the mixture can be submitted to the microscope for the purpose of counting the corpuscles. In a subsequent communication‖ the author gives the results of nine measurements made with extreme care and delicacy of manipulation. The capillary tubes employed were 0·8295^{mm} to 0·8327^{mm} in diameter, the quantity of blood operated on not more than one five-hundreth of a cubic millimetre. The mean of the nine calculations gave, in a cubic millimetre, 5,174,400 blood corpuscles; in

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* Microscopical Journal, No. 2, Jan. 1853, p. 145.

† Schmidt's Jahrbucher, No. 8, p. 147, 1852. ‡ Ibid, No. 4, p. 1, 1852.

‖ Schmidt's Jahrbrucher, No. 8, p. 147, 1852.

a cubic line 59,396,100 corpuscles; the maximum in the cubic millimetre. 5,818,700, the minimum 4,597,800. These measurements were made during the winter, and when the author was suffering from indisposition. The process of counting is extremely laborious, Vierordt's idea of quickness being, that in summer and by good light a single enumeration may be completed in a week! The method of estimating the total quantity of blood-corpuscles in the body, by multiplying the number contained in a given volume, and ascertained by this process, into the total volume of the blood in the body, is open to several objections, and contains more than one source of fallacy. Thus the corpuscles vary much in diameter. (Lehmann has found great differences in the blood of the porta and the liver.) The several acts of micrometry and enumeration are of exceeding difficulty and liable to many errors. But in addition to all these difficulties, which have been pointed out by Funke, in his report on the subject in the Jahrbucher (*loc. cit.*) we apprehend that the labor and truly iron patience required to carry out even a single numeration will be an almost fatal obstacle to its employment. We indulge hopes, however, that some simplification of the process may be devised, whereby its practical application to the blood analysis may be, in a great measure, facilitated. In estimating the relative as well as the actual quantity of the red corpuscles in various states of disease, such a method would be invaluable in clinical research. In a critical review of a work on leucocythemia,* without being acquainted with the researches of Vierordt, we had ourselves called attention to this great desideratum. Professor Bennett thinks the best method of estimating the relative proportion of the two kinds of corpuscles is to observe the spaces or meshes left between the rolls or aggregations of yellow corpuscles. In reference to this subject we ventured to make the following suggestions: "For the purpose of a more accurate estimate, we would suggest the use of an ocular micrometer divided into squares of any convenient dimensions, with the aid of which we may ascertain, in a number of examples of normal blood, what is the natural numerical proportion between the red and white corpuscles, by counting both as they are placed under the square in two or three opposite portions of the field selected indifferently; the average result of a few computations of this kind, we are of opinion, would be found not to be far from the truth. A similar proceeding could then be had recourse to, to estimate the proportion between the numbers of the white and red corpuscles in suspected cases of leucocythemia. Until some such process be brought into use generally, it is manifest that we can learn but little from such vague statements as are usually appended to reports of cases, "one-third as numerous," "greatly increased," etc., etc.† We do not venture to be too sanguine about the success of this process, but we have some hopes that it will be found useful.

Hepatic venous blood (human.)—Lehmann has observed peculiarities in the hepatic blood of the horse, and Funke‡ has described the same characters as existing in that of the human liver. The red corpuscles are found to be somewhat smaller than the normal average, the central depression generally only slightly marked, and in many imperceptible; the corpuscles towards the borders of the field appear somewhat pointed or lenticular, but rarely two and never more than three are grouped like coins. Besides the colored numerous colorless cells are to be seen of various sizes, some very large some single, some, especially the smaller ones, grouped together in twos

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* See Bennett on Leucocythemia, reviewed in the Dublin Quarterly Journal August, 1852, p. 204. † Dublin Quarterly Journal, loc cit.

‡ See Funke's Atlas, Plate IX, figs. 5 and 6; and letter-press descriptive thereof. page 26.

threes, or greater quantity. They are, for the most part very pale, their surface only slightly granular; some completely resemble transparent vesicles, in which an excentric round nucleus is visible. In a considerable number fine dark highly refractive points are to be observed. This observation was made on blood from one of the large hepatic veins in an old woman five hours after death. From the spleen of the same individual, the

Splenic venous blood was examined by Funke. It contained elements similar to those described in the venous splenic blood of the horse, viz: small lenticular colored cells, exhibiting only here and there the appearance of a central depression, and seldom united in *rolls*. The intermediate colorless corpuseles were of different sizes, generally small, partly single and isolated, partly united into large round or irregular heaps by an enveloping fine molecular mass. They are generally pale, but clearly granular, seldom exhibit a nucleous without the application of re-agents; but on the addition of acetic acid the majority display a single nucleus. He has also observed, as in the splenic venous blood of the horse, large, round, or oblong structure, having the appearance of coagula, and containing in their interior colorless blood-corpuscles and nuclei, and in a few cases, colored corpuscles. He has not been able to establish the existence of a cell-membrane, and therefore doubts that these are to be regarded as "blood-corpuscles holding cells." Granular cells were also visible.

*Blood-corpuscles-holding Cells; Pigment-corpuscle-holding Cells.**—A very considerable amount of labor has been expended in the investigation of these structures by several microscopical observers, and it is much to be regretted with a great absence of uniformity in the results arrived at. Since the first discovery of the former bodies by Remak, two opinions chiefly have been entertained with regard to them. Kolliker (who describes them as round masses of blood-corpuscles which collect together, and subsequently acquire an envelop and nucleus) considers them as a stage in the solution of the blood-corpuscles; while, on the other hand, Gerlach, Weber, and others, regard them as formative elements. Remak, who gives the results of a very extensive inquiry, considers that both these theories are erroneous. In order to ascertain, if possible, the conditions under which these microscopic coagula occur, he examined a large number of animals, fishes, frogs, birds, and mammalia (dogs, swine, sheep and oxen,) in not one of which did he meet with a single example of these structures. From March to July, under different conditions of temperature, he examined eight perches, killed in different ways, with a similar negative result. At last, in the middle of July, he succeeded in finding them in a perch, but not until three-quarters of an hour after death, when the blood of the spleen presented the cells in question; though both it and other specimens of blood were unsuccessfully examined by vivisection. In other cases he was unsuccessful in his search for the cells, though made at a short period after death, and also a day subsequently. The following are Remak's conclusions from his own researches. The belief in the existence of blood-corpuscle-holding cells, under normal conditions in the spleen, liver, and other organs in the vertebrata, man inclusive, has arisen from mistaking for such structures, pigment-corpuscle-holding cells, and also round coagula enclosing blood-corpuscles. Neither the spleen or any other organ can be considered as the locus of formation or of destruction of the blood-cells, so far as any observation

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* Muller's Archives, No. 2, p. 115, 1852. An article on this subject, by Mr. Wharton Jones, chiefly devoted to an exposition of Kolliker's views, appeared in the number of this journal for January, 1853; and as we now consider only those points not therein reviewed, we must refer our readers to it for further notice of these investigations.

yet may go. Pigment-corpuscle-holding cells occur so frequently in many full-grown fish and amphibia, that they must be regarded as normal elements of the blood. Investigations on the larvæ of frogs show, that when deprived of motion and nourishment, pigment corpuscles are formed of the fat-corpuscles in the cells of the liver and spleen; and that during hybernation in the full-grown animal, a remarkable increase of the pigment formation takes place in the liver, at the expense of the fat. Lastly, blood-corpuscles holding fibrinous coagula form after death in the blood vessels of the spleen and the kidneys in the tench (and probably also in other vertebrate animals.) The formation appears to stand in relation to the late occurrence of coagulation in the blood of the abdominal veins; but there is as yet, according to this author, no ground for believing that they occur during life in the vessels of the spleen or kidneys, or that they pass into the pigment-corpuscle-holding constituents of these organs.

Crystalization of Blood.—Funke* has prosecuted his researches on the crystalization of the blood, and has ascertained *that all normal human venous blood may be crystalized.* This crystalization is brought about simply by the action of distilled water. A drop of blood is placed on a slide (the phenomena will be better observed when the blood has been allowed to rest for a day; but the author states, that it can be produced in a fresh drop obtained from the hand by a needle,) and after it has rested a few moments, a drop of distilled water is added, and the whole covered with a thin glass; in a short time, when the preparation is fairly coagulated regular red-colored crystals of different sizes and forms begin to appear; they are partly needles and columns, partly prisms, and partly rhomboid tables. The color varies from a pale red to a deep crimsoned-red, the best examples of which are to be seen in the regular tetrahedra described by Lehmann and Kunde in the venous blood from the neck of the guinea-pig; these Funcke states to be identical with the "albuminous crystals" of Reichert. Kunde has repeated the observations of Funke, and with similar results. Robin and Verdeil† have observed colorless tetrahedra in colorless blood-serum. These authors further observe, that many of the salts of the blood exhibit crystals of various forms, and have the faculty, during their passage into a crystaline state, of carrying with them any coloring matter which may be in their vicinity; and it is in this manner that they explain the formation of the colored crystals of Virchow (hæmatoid crystals,) and also those described by Funke. On the other hand, Funke, Kunde, Virchow, Reichert, and Lehmann, consider that a protein compound enters into their formation. Parkes has described and figured various forms of crystals as forming in putrefying human blood, with and without the addition of water.‡ A memoir by Virchow on animal crystaline coloring materials may be consulted with advantage.‖ We have ourselves observed granular bodies and prismatic crystals in blood taken from the arm, twenty-six hours after being drawn.

PERMANENT TISSUES.

Dr. James Drummond◊ has instituted a series of inquiries into the mode of development of the tissues in the mammalian embryo. His researches em-

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* Schmidt's Jahrbucher, No IX, p. 1. See also the very beautifully colored lithographs, pl IX, of his Atlas of Pathological Chemistry. For his former papers see Henle and Pfeufer, B. ix; also Dublin Journal of Medicine, May, 1852.

† Traite de Chimie Anatomique, vol. iii, *p.* 436.

‡ Medical Times and Gazette, June, 1852.

‖ Verhandlungen der Phys. Med. Ges. Wurzburg, vol. ii, p. 303, 1852. See also paper by Saunderson, in Edinburgh Monthly Journal, 1851-'52.

◊ Edinburgh Monthly Journal, October, 1852.

brace the consideration of, 1st, the morphological changes which the tissues undergo in the process of their formations; and 2d, the chemical changes which the histogenetic or protein compounds undergo during this process. The author very justly observes, that the latter series of changes have not been sufficiently studied. He considers that with each morphological change we find a corresponding change in the chemical constitution of the structure; and, moreover, that whenever we find two or more tissues presenting the same morphological characters at any stage of their development, they have at this stage the same chemical constitution, however much they may differ in this respect afterwards. The memoir is chiefly devoted to

Morphology of white fibrous tissues, in its different stages of development. In a portion of blastema from the deep layer of the integument of an embryo calf, about two inches long, Dr. Drummond found—1st. Embryonic corpuscles in large numbers; they presented a rounded shape, granular surface, and measured from one three-thousandth to one two-thousand-five-hundredth of an inch in diameter; they were slightly affected by acetic acid. 2d. The same bodies, with a number of minute granules deposited around them. 3d. Others, with the granular matter deposited in them so as to give a spindle shape. 4th. The last-named bodies, with the granular matter matted together, so as to form a solid body. He concludes that Schwann's view with regard to the disappearance of the nucleus is incorrect, this body being visible in white fibrous tissue throughout the entire period of its existence in fœtal as well as in adult tissue.

Corresponding with the several phases of development just enumerated, the author of this paper has observed a passage from albuminous to other protein and gelatine-yielding compounds. The granules exhibit the following reactions.

They swell up and become gelatinous on the addition of weak acetic, tartaric, citric, and hydrochloric acids, by which, after a time, they are completely dissolved. They are insoluble in nitrate of potash. They are colored orange-yellow by tincture of iodine; and greenish-yellow by chromic acid. Millon's solution of hyponitrate of mercury in nitric acid causes them to assume a blood-red color. These re-agents have similar effects on plain and striated muscular fibre, as well as on the compounds of fibrine, and hence he concludes that the granules are identical with fibrine. At this stage, no gelatine exists in the white fibrous tissue. In the second stage, the reactions are the same. In the third stage, when the fibres are fully formed, gelatine can be extracted from them by boiling. Dr. Drummond recognises a second mode of formation of fibrous tissue, in which the blastema at once splits up into fibres. His researches are to be continued, and we anticipate many interesting results.

A memoir has been published by Engel, on the growth and development of animal cells and fibres, and the character of their nuclei.*

Bone, Cartilage, and Connecting-tissue Corpuscles—identity of these elements.—Virchow* has succeeded in isolating the bone-corpuscles and their canaliculi from the intermediate substance, by maceration in hydrochloric acid. Dry fragments, as well as fresh moist specula from the medullary canal and thin layers from the cortical substance, were macerated in concentrated hydrochloric acid, either directly or after being boiled for some time. As the result of the action of this acid there remained only a pale, homogeneous, lightly striated mass, in which no corpuscles could be observed. He therefore concludes, that the bone corpuscles as well as the bone-canals must

* Sitzungsbericht d. Kais. Akad. d. Wiss., vol. vii. Part 1, p 57. Vienna, 1851.
† Verhand. d. Phys. Med. Ges. Wurzburg, vol. ii, p. 150, 1852.

be regarded as parts having a different chemical composition from the osseous basement structure. Donders and Kolliker had already obtained results somewhat similar. The former had observed, that on macerating bone for a considerable time in dilute hydrochloric acid, and then placing it in potash for five hours, and lastly in water, the osseous tissue dissolved, but the corpuscles remained, and nuclei, some of which were surrounded with cells, became visible in them. Kolliker likewise observed round or oblong nuclei in the corpuscles of bones similarly treated, but he doubts the existence of cells.

Branched and anastomosing cells, similar to the corpuscles of bone had been found by observers in the cartilage of the invertebrata (by Bergmann in Loligo, Quekett in Sepia-Officinalis.) Virchow and Quekett had also described them in Enchondrona. More recently, Virchow has ascertained that they exist in normal cartilage. This author is convinced that the so-called cartilage-corpuscles are actual cells, lying either in a cavity in the basement-structure, or in a cell-cavity with a wall of double outline, and that they possess a membrane, granular contents, and a nucleus often provided with nucleoli. Near the line of ossification in growing cartilage, as well as in the young callus cartilage of fractures, these cells are extremely large, clear. and round; while in the neighborhood of joints they are very small, pressed together, and dark. On the addition of water they shrink, and sometimes form such peculiar branched bodies, that they may be readily mistaken for branched cells. The best examples of the transition from round or oval cartilage-cells to those of stellate form, are to be found in situations where the fibrous passes into the hyaline cartilage, as in the invertebral substance. Here, in a section, all the stages may be observed between the hyaline cartilage, with round or oval perfect cells, and the fibrous, with its oblong, caudate, club-shaped, and stellate elements; boiling, or treatment with acetic acid. will render these appearances more distinct, by making the fibrous structure more transparent. It is exceedingly difficult to demonstrate the presence of actual cells in the small cavities of cartilage near the surface; the membrane of the cell appears as if it had melted into the intercellular substance, leaving only cell contents and a nucleus. Virchow states, however; that by continued action of acetic aid, or by maceration in hydrochloric acid, the entire cell, with contents and nucleus, becomes visible in the cavity; but without these preliminary steps, erroneous impressions may be received as to the relation of the cell to the intercellular substance.

Areolar, or Connecting-tissue Corpuscles.—Henle described interstitial and enveloping nucleated fibres in areolar tissue—the first coursing along in the interspaces or on the borders; the second, spirally coiled round the parcel. Both these forms he considered to take their origin in cytoblasts, or nuclei, which became prolonged and grew endways. He studied their development partly in fully formed tissue, partly in embryonal; in the latter he found, at first, in a homogeneous substance, nuclei thickly pressed together, and arranged in longitudinal rows, which subsequently, by elongation and growing together, constituted nucleated fibres. This exposition was generally received, until Reichert stated that there was nothing determinately known about the formation of these "spiral fibres." Schwann described the embryonal areolar tissue as a gelatinous homogeneous mass, which is dissolved by boiling, leaving behind cells, which now become apparent, not being acted on by heat. The cells are partly round, partly caudate; the homogeneous substance is regarded by Schwann as a blastema, out of which the cells are formed. Reichert, on the contrary, regards it as purely an intercellular substance, from which the latter connecting tissue is directly formed. Originally only cells exist, between which there subsequently appears an intermediate substance, with which their envelope becomes blended; the complete as well as the young connecting tissue being homogeneous. He denies altogether the existence

of fibres, and of caudate or stellate bells. Virchow agrees with Reichert as to the persistence of the gelatinous intercellular substance, and its transformation into proper connecting tissue; but by careful treatment with re-agents he has obtained further results. By boiling, by treatment with acetic acid, by hardening in pyroligenous acid, and by maceration in concentrated hydrochloric acid, he has found that in the connecting tissue, as in bone and cartilage, the intercellular substance may be separated from the cells which exhibit the greatest resistance to these re-agents, and in this respect constitute a special group. In connecting tissue thus treated, cells will be obtained which are either isolated or anastomosing; round, oval, caudate, stellate or branched, interstitial or enveloping; *thus, then, the bundles of the connecting tissue are only striæ of the intercellular substance separated by the cells.* These cells possess generally one, seldom two, nuclei; in transverse sections the nucleus appears round; in longitudinal sections, oblong or pointed; they may also have a spiral appearance. He has not seen any example of the direct anastomosis or union of these cells, though their prolongations, or branches, freely unite. Cell-contents are rarely to be met with, with the exception of a few fat granules which lie near the nucleus. The prolongation of the cells are sometimes large, sometimes exceedingly fine threads; the longer the finer.

[*To be continued.*

II.—*Annals of Physiology.*

BY HENRY GRAY, F.R.S.

Demonstrator of Anatomy at St. George's Hospital, and Surgeon to the St. George's and St. James' Dispensary.

In commencing a series of systematic reviews of the progress of physiological inquiry, the writer deems it necessary to state briefly the plan he intends to follow. His aim will be, not to occupy space by noticing every physiological paper, whatever may be its value, but to make such a selection of the materials before him, that in a short space every important fact in the progress of physiology may be presented to his readers. It is not intended either to include systematic treatises, which are more properly analyzed in the review department, or to repeat statements which have already appeared in the reviews. The "Annals of Physiology" will appear every six months, alternately with the "Annals of Micrology;" and, as far as possible, it is the intention of the author, in future numbers, to test by independent experiments the statements made in the papers under review. He has to apologize for the short notice some important papers have received from him on the present occasion; but the limited space which could be assigned to him must be his excuse.

BLOOD.

Dr. Thomas Williams* shows, in the invertebrate animals, that there exists two distinct nutritious fluids, dissimilar in their anatomical relations, and in their chemical and vital compositions.

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* On the Blood Proper and Chylaqueous Fluid of Invertebrate Animals. By Thomas Williams, M.D. (Philosophical Transactions, part ii, 1853.)

Below the Echinodermata the blood proper is wholly supplanted by the chylaqueous fluid; above the Annelida, this latter fluid, in the adult animal, is superseded by the true blood; whilst in the Echinodermata and Annelida these two systems of nutrient fluid exist, bearing always to each other in the same individual the same quantitative proportion.

The author also shows, that in the *containing system of the blood proper*, excepting the Echinodermata, there is an entire absence of an internal lining of vibratile cilia, the contents being propelled by the contraction of the muscular parieties; whilst, on the contrary, *the containing system of the chylaqueous fluid* is circulated by ciliary vibration.

The main differences between the chylaqueous fluid and the true blood appear to be these:

1st. The former is contained in channels, the walls of which are lined with vibratile cilia, by means of which the fluid is circulated.

2d. In the Zoophytes and Medusæ this fluid is distinguished from the blood proper by the fact of its having mixed with it sea water, in large quantities, more or less directly, and with which it vitally assimilates.

3d. In all classes in which this fluid exists, the corpuscles vary in size with the variations in the size of the body of the animal. In this respect they are diametrically opposite to the corpuscles of the true blood in the vertebrate animals, the corpuscles of which bear no proportion to the size of the animal from which they are taken.

The author commences his researches on this fluid in the lowest classes in the animal scale.

In the *Sponge*, the chylaqueous fluid is contained partly in the interior, and in part between the cells of the gelatinous cortex. It is composed of a mixture of *salt water and albumen.*

In all *Polypes* it is principally composed of sea water, which, in passing through the stomach, is mixed with the secretions of this organ, and then enters the visceral cavity. In the Tubularia indivisa it is charged with minute granules, consisting of solid spherules from the solidification of albumen.

In the *Molluscoid Polypes* this fluid presents a higher organic composition than in the former class. It is true blood in its composition and function; its cells are corpusculated, many bear globules of oleine, although comprising several individual forms of cell, which are constant in their microscopic characters in the same species.

In the *Meduse* the chylaqueous fluid is contained in a system of canals, which are in direct communication with the stomach, and which are lined with a vibratile epithelium. The contained fluid is composed of corpuscles which never contain a distinct nucleus, suspended in a fluid consisting chiefly of salt water, presenting in the Rhizostomidæ a yellowish hue, which has its seat in the fluid; in the Vilella it is bluish. The floating cells, which contain secondary oleaginous cells, and a molecular base, exhibit great irritability, the minute molecules being mutually repulsive.

In the *Echinodermata*, instead of the three distinct systems of fluid which physiologists usually describe—1st, that of the general cavity of the body; 2d, that of the feet and water canals; and that, 3dly, of the blood proper—the author is led to believe that they constitute a system of separate cavities containing a similar fluid, from the fluid contents of the blood-vascular system being chemically and morphologically identicaly with those of the water-vascular system, and with those again of the visceral cavity; the blood-vascular system not being an independent and closed system of conduits, but is imperfect in its peripheral portions, whilst the internal lining of its channels is ciliated; a peculiarity which separates the blood-system of this class from that of every other in which it is known to exist.

The Blood Proper, in the Echinidæ and Asteriadæ is colorless, containing irregular organized particles, which cannot be distinguished from those found in the peritoneal fluid or in the water-vascular system of the same individual. In the Sipunculidæ the corpuscles in this fluid present a pink tinge; each corpuscle is flat, irregularly oblong, and contains a small bright refractive nucleus; in some cells a second may be seen. The color is dissolved in the fluid between the nucleus and involucrum. The blood proper is contained in the Uraster in a circular vessel, with radial trunks, the walls of which are everywhere lined with ciliated epithelium. In the Sipunculidan genera there is only one vessel, extending back from the œsophageal ring, no capillary system being traceable at its end. In these animals the blood proper has acquired scarcely any distinctive and independent characters, the system of channels in which it moves being so rudimentarily organized as to receive its contents directly from the fluid occupying the visceral cavity; the blood-vascular apparatus, developed only in its central segments, is designed to concentrate the nutritive force of the chylaqueous fluid upon the more important viscera: the nutrition of the peripheral structures—such as the muscular, calcareous, and integumentary—being sustained under the agency of the chylaqueous fluid.

The Chylaqueous fluid in the Crinoideæ, Asteriadæ, and Echiniadæ, is far more voluminous than the blood proper. In the Sipunculidæ it has assumed a greater relative development. In the Holothuridan genera it exhibits the most advanced condition under which it is known to exist in the echinodermal series. In all echinoderms it is contained in the peritoneal cavity and its hollow prolongations, the boundaries of which are universally ciliated. These prolongations constitute the true respiratory organs, exposing the chylaqueous fluid to the external air. This fluid, which is of a pure water-like appearance, is charged with cells which are less organized and fewer in number than those of the chylaqueous fluid of the superior genera. In the Echinus it contains flattened corpuscles, the largest of which are provided with an involucrum bearing particles of a limpid oleine, the involucrum projecting out like a cilium, and when these are numerous it is easy to mistake such an appearance for the characteristic of a sperm-cell. It is really due *to the fibrinous contents coagulating in lines* on escaping. The fluid tested by nitric acid contains albumen. In the Sipunculidæ the fluid is opalescent, the great bulk being salt water holding in suspension numerous flattened irregular oval cells, of a pink hue, like those of the blood proper. In the Asterias rubens, the corpuscles in the water-vascular system, and the contents of the digestive cæca, are identical with those of the peritoneal fluid. For these reasons the author concludes, that the bulk of the fluid contained in the peritoneal cavity of the Asterias and in the Echinus, is derived from that which enters through the mouth into the digestive cæca, in which the first phase of the digestive process is performed; its subsequent changes, by which it is raised to a higher grade of organic composition, occurs during its sojourn in the peritoneal space into which it passes by exomosis from the digestive cæca penetrates the hollow axis of all the membraneous processes of the shell which constitute the true organs of breathing, and where it experiences the change of oxygenation, conveys the results of this change to the blood proper, and replenishes the water system or ambulacral feet.

In the *Entozoa*, the blood-proper system is very inferiorily developed, and the blood itself is colorless, and perfectly fluid, holding no cells in suspension.

In the Trematoid and Nematoid Entozoa, the space intervening between the intestine and the integument is filled with the chylaqueous fluid, remarkable for its viscidity, and the molecule-like size of its corpuscles.

In the Cestoid Entozoa, where the elementary canal is intimately adherent to the integument, the chylaqueous fluid, which is much reduced in volume, is contained in the recesses of the alimentary canal. In those cases where the fluid exists external to the digestive canal, it is limpid and non-corpuscular

In other cases, where the chylaqueous fluid entirely disappears, it is compensated for, as in the earthworm and leech, by a corresponding greater development of the true blood system.

In the *Annelida*, the chylaqueous fluid which is contained in the peritoneal cavity is of specific gravity, 1.032 to 1.034, sea water being 1.028. On standing, a coagulum is formed, which proves the presence of fibrin; albumen also exists, as seen by the addition of nitric acid. The contained corpuscles, which differ in different species, are usually of an orbicular form, bearing a nucleus, and filled with minute granules. It is colorless, excepting in the Clymene, Arenicordia, and Glycera alba, where the colorless fluid contains blood-red corpuscles.

Contrary to the statement of Milne Edwards and Wharton Jones, the author states, that the red blood of the Annelida contains *no corpuscles of any description*, it being a limpid fluid, variously colored in different species, owing probably, to the salts contained in it—the red to iron, the green to copper, and in those where the fluid is colorless, the author supposes these salts may exist under colorless combinations.

In the *Embryonic condition* of the Myriapoda, Insecta, Arachnida, and Crustacea, the circulating fluid presents all the characters of the chylaqueous system. On the contrary, in their mature state, the blood-proper system is the only one observed, the chylaqueous fluid and the blood proper having in no instance a contemporaneous existence in the same individual. In all the articulated animals, the corpuscles of the true blood conform to one type of structure and figure; the blood proper of insects is colorless, and charged with colorless floating-cells, impelled by a dorsal vessel. There is a nucleus in each cell, surrounded by minute, pellucid, very slight refracting granules, which, on bursting, fibrillate.

The author also differs from Mr. Wharton Jones as regards the structure and form of the blood-corpuscles in the Crustacea, stating that the corpuscles in the blood occur in three discernible varieties: 1st, simple non-granular, non-nucleated, pellucid, spherical globules; 2d, more or less orbicular bodies, of which the bright nucleus is prominently visible, and a mass of slightly-refractive molecules; 3d, the fact, characteristic of all blood-cells falling under the denomination of the articulate type, of the apparent suppression of the cell-capsule.

In the *Arachnida*, the corpuscles of the true blood occur under the character of minutely granular bodies, varying between the spindle-shaped and orbicular in figure, differing from those in the Crustacea in the position and invisibility of the nucleus, which is seated in the centre of the body, and therefore indetectable, because surrounded by molecules, which are the counterpart of those formerly described in the blood-cells of the Crustacea.

In the *Mollusca* there is but one system of fluids, which unites in itself the separate characteristic of the blood-proper system and the chylaqueous, and is in every physiological property intermediate between that of the vertebrate animal and the chylaqueous fluid of the Annelid. It is colorless, like dilute milk, and more coagulable than any kind of chylaqueous fluid, containing corpuscles, the amount of which varies in different orders.

Development of the Blood-globules.—Moleschott* has lately been conducting a series of observations on the splenic and cardiac blood of frogs after excision of the liver. The first effect of excision of the liver is a striking diminution in the quantity of the blood, inducing a sort of chlorosis. The colorless corpuscles are much increased in relative quantity—the proportion in the cardiac blood being (average of many observations) 1 white to 2.24 colored;

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* Mueller's Archiv., 1853, 73.

while in healthy frogs it is 1 to 8. In the blood of the liver, the proportion was 1 to 5.88. The same diminution of the colored corpuscles, after ablation of the liver, was observed also in the abdominal blood and in that of the "fat body." In the spleen, the quantity of the colored corpuscles was found reduced by more than half, so that, in consequence of the smaller number (only about one-sixth) of colored corpuscles naturally present, the quantity of colorless corpuscles under these circumstances exceed that of the colored.

From these experiments, it follows that the Liver is an organ in which the conversion of colorless into colored corpuscles goes on to a great extent.

Frogs deprived of the spleen show a slight increase in the relative proportion of the colored to the colorless corpuscles. Frogs deprived of both liver and spleen present, in proportion to the colorless, four times fewer colored corpuscles than in the natural state.

In the *conversion of colorless cells into colored*, the author states that the nuclei separate into two or three smaller ones, and these into granules; the granules become colored, and dissolve; and thus colored cells without nuclei are produced. At the same time, the round form of the colorless is gradually converted into the eliptical one of the colored. This change of form takes place sometimes before, sometimes after, the cleavage of the nucleus.

Chemical Composition of Blood.—Magendie* has lately instituted a series of experiments on the blood of four horses, differently fed, but no distinct results were arrived at, the quantity of all the principal elements of the blood varying irregularly. The most constant phenomenon was, that the blood-corpuscles and fibrin were in inverse proportion to each other.

Enderlin† from a series of researches which he has lately made, has shown that cholic acid, in combination with soda (choleate of soda,) is a normal element of the blood, but, under ordinary circumstances, is soon eliminated from it. The alcoholic extract of dried blood, tested with Baryta water and tribasic acetate of lead, shows similar reactions to the extract of dried bile. Its ash contains much carbonate of soda, which is a product of the combustion of choleate of soda, together with tribasic phosphate of soda and sulphates. It also answers to the test with sulphuric acid.

Temperature of the Blood.—The results of former investigators on the difference in temperature between arterial and venous blood, have shown that there is usually a difference of about .5° C. in favor of arterial blood, comparing generally the jugular and the carotid, or the opposite ventricles of the heart. A series of extensive and very accurate researches made by Liebig‡ son of the renowned Munich Professor, have shown, on the contrary, that venous blood is warmer than arterial under all circumstances, the average of the maxima and minima being ·70° to ·19° C. The experiments which were made on the living or recently-killed animals were conducted in the following way: A dog was killed, and a ligature tied tightly round the neck, to prevent the lungs from collapsing. In this way, as the pulsations of the heart lasted a short time longer, some of the blood would be arterialized by the air in the lungs, and some arterialized blood would therefore always be found in the left side of the heart. The thorax was then opened, by as small an opening as possible, the aorta tied at its bend, and the vena cava superior close to the auricle; then the dog being raised, and the ligatures drawn upwards, in order to make the opening in the vessels the highest point, thermometers were passed into the two ventricles down the artery and vein. After the introduction of

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* Ueber den Ein flues der Nahrungsmittel auf das Blut. (Schmidt's Jahr., i. 1853.)

† Enderlin: Chlosaures Natron im Blute. (Schmidt's Jahr., i. 1853.)

‡ Liebig Georg; Ueber die Temperaturunterschiede des Venœsen und arteriellen Blutes. Giessen, 1853.

the thermometers, cotton-wool was placed in the opening of the thorax, in order to prevent any real difference between the temperature of the blood occurring from the difference in the thickness of the two ventricles. In the experiments conducted on living animals, thermometers were introduced by the carotid artery and jugular vein in the heart.

The conclusions that may be drawn from these experiments may be divided into three heads:

1st. The difference of temperature between the two kinds of bloods.

2d. The temperature at different parts of the same system.

3d. The changes of temperature at one and the same level of the arterial or venous system.

1st. As to the difference of temperature between the two kinds of blood. In the experiments on the recently-dead animal, the temperature of the blood in the right ventricle was found once higher, and twice equal, to that of the left. The temperature of the blood in the ascending cava was found ·72 ° C. higher than in the carotid. In the experiments on the living animal, in which the temperature was constant, that of the venous blood was found constantly higher than that of the arterial by .07 ° to ·19 ° C. on an average of the minima and maxima.

2dly, As to the differences in temperature of the venous blood at different parts of the circulation. The temperature of the blood in the large vessels coming from the head and extremities, rises as we approach the vena cava inferior, in which the blood reaches its highest temperature. This change does not reach a high value for small distances in the vena cava superior, but is rapid in the auricule, where the mixture of abdominal blood is greater. A similar rise is observed in the iliac vein. The temperature of the venous blood of the extremities is far lower than that of the vena cava inferior, of the right heart. The differences in the arterial blood are far smaller; they are not even appreciable at less than 6 cn. from the heart.

3dly. As to the changes of the temperature at one and the same level of the arterial or venous system. The temperature in the veins varies regularly in inspiration. This is observable in the larger venous trunks of the chest and belly, except the vena cava inferior. In the superior cava, the rise takes place at the end of inspiration, and is at its lowest point after inspiration.

Maximum.	Minimum.	Maximum.	Minium.
Inspiration—	Expiration—	Inspiration—	Expiration—Inspiration.

The oscillations in regular breathing are from ·07 ° to ·10 ° C. These oscillations are thus explained. In inspiration the pressure of the diaphragm and abdominal viscera causes an increased flow of blood from the vena cava inferior into the right ventricle, which overpowers the stream from the vena cava superior· while in the inspiration, the flow of blood occurs towards the vena cava inferior, and its exit is impeded. The exit of the blood from the vena cava inferior is also impeded, but not to so great an extent. Now the blood of the inferior cava being warmer than that of the superior, it follows that the blood in the right ventricle will be found warmest just after inspiration. Their maxima and minima will be found at different periods of inspiration, in different parts of the circulation, according to the period at which the stream of blood reached them.

VASCULAR SYSTEM.

On the Structure of the Endocardium.—The researches of Luschka on the structure of the endocardium add but little to the account already given by Thiele, Purkinje, and Racuschel. He describes the epithelium of the endocardium as consisting of two forms of plates. 1st. Lancet-shaped cells placed close together, with sharply-defined nuclei, and one or two nucleoli. The

second, which is more seldom met with, is composed of irregularly polygonal cells, like the epithelium of most serous membranes. The epithelium is sometimes met with detached. This degenerates into fat-corpuscles, whereby it is hindred from being deposited by passage into the smaller vessels. Under the epithelium are longitudinal fibres, not (like the fibrous coat of the vessels) united by interstial substance, but running quite isolated, in a straight direction, frequently bifurcating and crossing each other at acute angles. The layer under that of the longitudinal fibres is analogous to the contractile coat of the vessels; it is composed of simple elastic fibres, and of other fibrillæ united into nets, the interspaces being filled up with a thin, fenestrated, structureless lamella, which in the endocardium of the auricle, is granular and non-nucleated. This layer, though everywhere very thin, is thicker near the insertion of the valves. Lastly, beneath the elastic coat is the cellular coat, analogous to the "tunica adventitia" of the vessels; it is the conductor of the vessels, and may easily be separated as an independent membrane. He gives the following description of the arrangement of the vessels between the layers of the endocardium on the valves. In the auriculo-ventricular valves they run between the two layers of the endocardium; they are separated at the insertion of the valves by a thick cellular layer, and at their edge by a thin one; the vessels are most numerous where the valve is thickest, and they ramify towards its free edge. The results of the author's researches on the arrangement of the blood vessels between the layers of the valve, have led him to conclude, that the exudations or fibrinous vegetation observed on the surface of the valves in endocarditis, is dependent on a hyperæmic condition of these vessels, accompanied by exudation of lymph from them, and do not arise as fibrinous precipitations from the blood flowing through the heart as Simon and others have supposed.*

[With regard to the vacularity of the valves of the human heart, we have been completely unable to verify the statement of Luschka. In a minute injection of the heart of a young child which we lately made, we could trace no blood-vessels between the layers of the valves; the capillary vessels of the muscular substance of the heart stopped at the point where the valves are attached, having a loop-like termination.]

Nega† has added some contributions to our knowledge of the function of the auriculo-ventricular valves of the heart, their tones and sounds, and the signification thereof. His conclusions are founded on five vivisections of sheep and calves, on experiments on the human heart after death, and on clinical experience. He believes, first, that the closure of the auriculo-ventricular valves is not induced by the contraction of the ventricules, but by that of the auricles. To prove this, (on the dead heart,) he fills the ventricles through the venous sinuses with water, slowly, until the flaps of the valve are raised, and their still floating edges made tightly tense, then he injects rapidly a stream through a small syringe, pointed directly towards the valvular orifice. This induces such a tension and closure of the valves, that the heart may be turned over without the water running out. In life, the contraction of the auricle acts in the same way as the syringe; thus the closure of the valves occurs at the end of the auricular, and before the ventricular contraction.

The second point he has assured himself of by vivisection. The tense valvular flaps are drawn down by the contraction of the papillary muscles, which, according to Kurschner, almost disappear into the substance of the

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* Luschka: Archiv. fuer Pathol. Anat., iv. 2.

† Nega: Beitrage zur Kenntniss der Function der Atrioventricularkappen des Herzens. (Schmidt's Jahr., i. 1853.)

heart during the systole. Thus the valve acts not only as a valve to the auricular opening, but as a forcing-pump on the ventricular, and a suction-pump on the auricular side. Further, it may be concluded from this that the first sound of the heart is caused, not by the closure of the valves (for these have closed before the beginning of the ventricular systole,) but by the tension of the valves and their tendinous fibres, caused, on the one side, by contraction of the musculi papillares, and on the other, by the hydrostatic pressure of the blood on the flaps.

These conclusions, which he announces in opposition to those of Skoda, do not seem to affect the diagnosis of heart affections.

Muscularity of the Valve which closes the Foramen Ovale, and Cause of the Closure of the Foramen after Birth.—In a series of preparations lately made by Dr. Peacock, is shown the valve which closes the foramen ovale, which presents a peculiarity of structure which he thinks powerfully contributes to the permanent adhesion of this membrane, and the consequent completion of the auricular septum. The muscular character of this valve was first pointed out by Senac, but denied by Haller, who remarked that its structure was purely fibro-cellular, the presence of muscular fibres being accidental and unusual. The researches of Dr. Peacock have enabled him to refute the assertion of Haller and establish the truth of Senac's doctrine. Heretofore, the explanation afforded of the closure of the foramen ovale was purely mechanical. After birth it is said the pressure of the blood in each auricle becomes equal, and no excess of force existing on either side of the foramen ovale, the valve is kept in contact with the edges of that aperture, to which, in process of time, it becomes solidly united. The author, on the contrary, believes that muscular action is called into force for the purpose of bringing the valve into contact with the margin of the foramen ovale, and a series of examinations have shown him, that the drawing up of the cornua and fold of the valve above the isthmus takes place by the action of muscular fibres, derived from the walls of the left auricle, assisted by its general dilatation; whilst the approximation of the cornua of the fold takes place by the contraction of either muscular or contractile fibres. Without muscular fibre it is difficult to explain the closure of the orifice, for after birth the pressure of the blood in both auricles is not so certainly equalized, for there are instances in which the walls of the left auricle are scarcely stronger than those of the right, while the cavity of the right auricle remains the larger; and hence the superiority of force on the right side of the valve exists, which, but for some counteracting agent, would drive the flapping membrane into the left auricle, and prevent the completion of the septum.

Hypertrophy of the Nerves of the Heart in Hypertrophy of the Heart.—Cloatta* has repeated Dr. Lee's dissections of the nerves of the heart with similar results, but thinks that the flattened swellings which the nerves present in calves and oxen, at the places where they cross the vessels, are not ganglia.

In order to discover whether Dr. Lee was right in his description of the increased size of the nerves in hypertrophied hearts, he dissected them in a case of the kind, and found, as Dr. Lee has figured, the nervous cords much enlarged, the gangliform swellings well marked, and the nerves of the left side more developed (as they are in the natural state) than those of the right. Whether this excess in the size of the nervous cords depended on increase in the nervous substance or hypertrophy of the fibrous tissue, he cannot decide.

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* Hypertrophie der Herznerven bei Hypertrophie der Herz substanz. Von A Cloatta. (Virchow's Archiv., 1853, Band v. 2.

On some Forces which influence the Speed of the Circulation.—Hering's† experiments on the forces which influence the circulation of the blood, are a continuation of those made as early as 1828, published in Tiedemann and Treviranus's "Zeitschrift fur Physioligie." He then showed (and these experiments he has confirmed) that the time required for the passage of a solution of ferrocyanate of potash, which is mixed with the blood, from one jugular vein (through the right side of the heart, the pulmonary circulation, left cavities of the heart, and general capillary circulation) to the jugular vein of the opposite side, varies from twenty to thirty seconds.

To these experiments it has been objected, that the rapidity of the circulation is increased by the circumstances of the experiment—viz: by opening the jugular vein in which the salt is detected at the same period as that in which the solution is injected into the blood. Under these circumstances the author performed a series of experiments.

[A.] *Experiments on the influence of opening the two jugular veins on the rapidity of the circulation.*—These consists in comparing the time at which the salt is detected under the above circumstances, and when the blood is *not drawn from the opposite jugular till the salt may be expected to be found.* He concludes that the acceleration cannot be more than five seconds.

[B.] *In order to find the time occupied in passing through the systemic capillaries.*—He opens the metatarsal artery on one foot and the corresponding vein on the other, and notes the difference in the time at which the solution can be detected in them. He finds the time consumed to be, *at most*, five seconds.

[C.] *On the influence of phlebotomy on the rapidity of the circulation.*—The same experiment as the last mentioned was performed before and after bleedings of from eight to ten pounds. From these he concludes, that moderate bleeding has no appreciable effect on the rate of the circulation; but if so great as to weaken the animal considerably, the circulation is retarded to a variable extent.

[D.] *As to the influence of the rapidity of the pulse on circulation.*—He finds that increased rapidity of the pulse has no constant influence on the time of circulation.

[E.] *On the influence of the frequency of respiration on the rapidity of the circulation.*—To test this without including the influence of the pulse, he chooses horses suffering under two diseases, in which the respiration is affected while the pulse remains natural—viz: tetanus and broken wind, in both of which the respirations are increased in number. He finds the rate of the circulation not materially affected, even when the respiration was from four to seven times the natural frequency.

In one case of very slow respiration he found the circulation quickened, in another the reverse.

On the Velocity and Pressure of the Blood in the Pulmonary Artery and Vein.—The results to which Beutner† arrives from his investigations are as follows:

The average height to which the column was raised in a tube inserted into the *pulmonary artery* was in a dog 17.6^{mm} 29·6^{mm} in cats, 12·07^{mm} in rabbits (measured by the "kymographion.") The pressure in the left pulmonary artery, as compared with that in the carotids, he finds at 1·3 in dogs to 1·5 in cats. The actual value seem to vary in each individual case.

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* Versuche ueber einige Momente, die auf die Sschnelligkeit des Blutlaufs Einfluss haben. Von E Hering. (Vieroedt. Archiv. 1843.)

† Schmidt, Jahr. ii, 1853.

The average height to which the column was raised in a tube inserted into the pulmonary vein of a cat (only one experiment) gave a value of $10{\cdot}5^{mm}$.

The following are the results which he announces:

1. The capillaries of the lung have a very small lateral pressure to restrain.

3. They oppose very slight resistance to the velocity of the blood—the difference of speed in the artery and vein being 9^{mm}.

RESPIRATION.

Donders,* in a series of observations on the movement of the lungs and heart in respiration, made on living rabbits or on the dead subject, has shown that the expansion of the lungs takes place principally from above downwards, and from behind forwards; the apex and posterior part of the lungs may be regarded as fixed points. In ordinary respiration the lungs do not descend below the sixth or seventh rib. In forced inspiration they may reach the eleventh. The movement from behind forwards is most important: the edges of the lungs come into contact in front during forced inspiration, covering the pericardium so that the dull sound of the heart on percussion is quite lost.

With regard to the movement of the heart during respiration, the author has shown that during expiration the heart lies in the anterior part of the chest. In inspiration, on the contrary, the heart falls back, and the folded part of the pericardium lies against the lungs; this change of position is due to the forward movement of thorac parietes The impulse of the heart is produced by the contact of its apex with the walls of the chest, and partly to the shock communicated to the parietes of the chest by the change of shape of the heart. Kiwish found that a needle passed in exactly at the place where the impulse is most distinct, hits not the apex of the heart, but some point in the right ventricle. The apex of the heart lies loosely against the intercostal space in diastole, and is pressed against it in the systole. This is a consequence of the direction of the axis of the heart, which, during the diastole, forms an obtuse angle, directed backwards with the plane of its base, which is then elliptical, and in contraction becomes vertical to its base, which become circular. The change in the diameter of the heart from before backwards may give an impulse to the chest, but cannot raise the finger. That the impulse is proiduced by the systole is easily proved by observing the movement of an empty heart, or one containing any substance put into it. The movement of the apex downwards which takes place in systole, has no influence in producing the impulse. [*To be Continued.*

III.—*Injection of Nitrate of Silver into the Nose, in Chronic Opthalmia.*

Dr. Paul has derived most excellent effects from injections of nitrate of silver, thrown up the nose, in opthalmia. He first introduced this practice in 1850, into the Breslau Hospital. since which the practice has been imitated by Tavignot, and other distinguished occulists of Paris. Does it act by revulsion or sympathy? Perhaps, both; at all events, it may be safely tried in obstinate cases. [*Revue de Therapeutic.* 1853.

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* Die Bewegung der Lungen und des Herzen bei der Respiration. (Zeitschrift fuer Rat. Med., von Henle und Pfeufer, 1852, Heft. 1.)

IV.—*Spirits of Turpentine in Syphilitic Iritis.*

Some of the European surgeons have been making trials with the spirits of turpentine in syphilitic iritis, and with wonderfully happy results, if their reports are entitled to confidence. Among the number, M. M. Helbert and Hœaing stand most prominent, as advocates of this treatment.

These gentlemen report a case of syphilitic iritis, which resisted first, mercury, then the iodide of potash; but, finally, gave way under repeated small doses of the essence of turpentine. They gave it in 6 grammes, mixed with 45 grammes of honey, in two days, in coffee spoonful doses. Does the medicine act specifically upon the poison, or does it cure in such cases, by its revulsive effects upon the primæ viæ?

[*Revue de Therapeutique*,

V.—*Sanitary Condition of England, in* 1852.

The total mortality of England and Wales, during the year 1852, amounted to 407,938 deaths, which is beyond the average of former years—except 1847 and 1849. In these years the mortality exceeded any known, owing to the great prevalence of typhus, influenza and cholera; when the mortality for 1847 reached 423,304, and 1849, 440,839 deaths are reported. The prevalent diseases were cholera, typhus fever, scarlatina, rubeola, pertussis, variola, diarrhœa, etc,

Remarkable fluctuations have been observed in the mortality in the various districts, for different years—being *above* the average in some, and *below* in others—caused chiefly by the greater prevalence of some particular epidemic or endemic disease.

The mortality in London alone was for 1847, 60,442 persons; in 1848, 57,628; in 1849, the cholera year, 64,432; in 1850, 48,579; in 1851, 55,354; and in 1852, the deaths from all causes were 54,213.

VI.—*Resection of the Xyphoid Cartilage, for an Affection of the Stomach. Cure.*

A singular operation, performed by Dr. Linoli, and reported in the *Annali Universali di Medicina*, from which it has been copied into the July No. of the *Revue de Therapeutique Medico-Chirurg.*, deserves some notice at our hands as a journalist. We, therefore, translate the following facts, from the last-named Journal, relative to the unique surgical feat:

In February of 1851, Dr. Linoli was requested to see a young man 22 years of age, who had suffered for a long time with obstinate cardialgia. He had lost much flesh—strength—and seemed to be rapidly declining, from what was supposed to be scirrhous of the pylorus. The patient was attacked with pain and vomiting soon after eating, if he attempted to ascend a hill or a flight of steps; whereas, if he reclined, he remained free from pain. M. Linoli noted that the point of the epigastrium, upon which the slightest pressure produced intense pain, exactly covered the point of the xyphoid cartilage. At the same time, he discovered that the xyphoid cartilage, terminating in an obtuse angle, pressed upon the stomach. Pressure upon this point produced intense pain and violent vomiting.

M. Linoli states that he had enjoyed the rare fortune of examining in the cadaver, three cases of this species of deformity, all of which had suffered during life, with symptoms very like those which afflicted the young man in question. The recollection of these three cases, and the absence of phenomena indicating organic lesion of the stomach, led M. Linoli to believe that the cardialgia, in this case, proceeded from an introflexion of the xyphoid cartilage. He, therefore, determined to resect this portion of the sternum, and the patient consenting, he performed it on the 4th February, 1851.

By an incision, M. Linoli exposed the retracted portion of the xyphoid cartilage; he opened the peritoneum, penetrated the cavity of the abdomen, and having introduced his finger, he felt at the extremity of the sternum, a sort of chrochet, against which the stomach pressed when filled with food. With a blunt-pointed bistoury, he divided the appendix at the point where it retracted. Two arteries required the ligature, and with two ligatures the wound was closed.

No serious accident followed the operation. He had slight fever and some meterorism, which were overcome by a small bleeding. On the fourth day, the dressings were removed, and on the eighteenth day the wound was entirely healed. The cardialgia and other stomach symptoms entirely disappeared, and eight months afterwards, this young man enjoyed perfect health.

VII.—*On the mode in which Death is produced by Chloroform.*

The July No., of 1853, of the *Journal des Connais, Medico-Chirurg.*, has an able report by M. Robert, on the effects of Chloroform; the lesions found after death, and also an attempt to explain the manner in which it produces death, in certain cases. We shall only notice some of the lesions which seem to characterise the fatal effects of this extraordinary anæsthetic.

Exteriorily, the body is generally slightly discolored and the limbs rigid.

Head—The sinuses of the dura-mater is sometimes nearly empty, as also the veins which are distributed over the surface of the brain. The cerebral substance is quite firm, and when sliced, but little blood escapes. The ventricles contained but a small quantity of serosity.

Chest—The trachea was not unusually discolored; and there was no froth in the bronchiæ. The lungs were gorged with blood throughout their whole extent, with a considerable increase of volume. Exteriorily, they presented some brownish ecchymotic spots. When cut in different directions, they were unfiltrated with a considerable quantity of black blood, diffluent in some places, but coagulated in others; much resembling apoplectic points. Bubbles of air also escaped, when cut into; but there was no emphysema. The heart—a little larger than natural, was excessively flaccid. The left cavities were empty—the right contained some clots.

Abdomen—The stomach, distended with gas, was entirely free from alimentary substances. The liver was slightly enlarged, and of a deep color. When cut into, a large quantity of black blood escaped from the incisions. Both the spleen and kidneys were loaded with the same dark fluid. Such were the lesions found in one case; we shall notice others.

A woman, aged 32 years, of excellent general health, had a tooth extracted by a dentist while under the influence of chloroform, and soon after expired.

Twenty-four hours afterwards the body was examined, and the following lesions were observed:

No signs of putrefaction. When the head was opened, a large quantity of frothy blood escaped; the vessels of the membranes were gorged with blood, containing bubbles of air, of considerable size. The cerebral substance was injected. The blood of the internal carotid and the vertebral artery likewise contained bubbles of air. The trachea and bronchiæ were injected; the lungs of a bluish red color, at its two inferior thirds, were filled with red blood, of a dark color—frothy and free from emphysema. The heart was natural in size and in its usual place; it was, however, soft and flaccid. The coronary vessels were gorged with blood, containing numerous bubbles of air; the auricles were distended; not with blood, but with air. There were no traces of blood in the right auricle, nor in the two ventricles,—the left held a small teaspoonful only,—the pulmonary artery, the aorta, two cavas at their junction with the heart, were empty. The blood of the inferior vena cava contained bubbles of air. Chemical experiments detected chloroform in the blood.

The result of these, together with other post-mortem examinations, lead the author to the following conclusions:

1st. That chloroform may cause death almost instantaneously, like certain kinds of poisons, which act with great rapidity.

2d. That death appears to be caused, generally, by the sudden cessation of the movements of the heart—by a genuine syncope.

3d. That in a great number of the facts noted up to the present time, death does not appear to have been produced by the excessive quantity of the anæsthetic used, but rather in consequence of a peculiar predisposition of the organism, utterly unknown in its nature. This predisposition may be developed in a moment—and in such a manner, that persons who may have been previously subjected to its influence, with safety, may perish at another time, while under its influence.

4th. That up to this time our science does not possess any means of detecting these peculiar idiosyncrasies. The use of chloroform is contra-indicated in individuals whose central organs, both of innervation—of the circulation and of respiration are diseased; those who are naturally or accidentally disposed to syncopes—those, finally, who have been much enfeebled by hemorrhages, etc.

VIII.—*English mode of treating Yellow Fever.*

Dr. Cummins, surgeon on board the "Medway," in a series of articles, lately published in the London Lancet, gives the following as his method of treating the Yellow Fever. He gives 20 grains of quinine every second hour, until four doses have been taken, when, if deafness has commenced, which it probably has by this time, he pauses until it begins to subside, which it usually does in about four hours. He then gives ten grains of quinine every second hour, and continues it according to the indications given by the headache, pains in the back, congestion, and muscular prostration, as well as the state of the secretions; but he never, he says, trusts much to the improvement in the pulse or the tongue. This is the routine treatment he generally adopts to fulfill the primary and most important indication; but the patient must be watched closely, as the dose has often to be increased or diminished.

Note.—This is certainly pushing the quinine practice *ad extremas*—up to, if not beyond, the limits prescribed by the greatest quininists in New Orleans. Eighty grains of quinine in eight hours must be regarded as heroic practice in the South, where this method of treating fevers first originated. Dr. Cummins did not tell us what success attended this bold use of quinine; but in charity, we must conclude it was quite satisfactory—more so we hope than it has proved to be in New Orleans, in the epidemic of 1853. The advocates for large doses of quinine, in the cure of yellow fever, have certainly greatly diminished in numbers, if not in respectability, since the breaking out of the epidemic this season; for it so often failed to check the tendency to a fatal termination, that many of our most respectable physicians began, about the *acmé* of the epidemic, to withhold, in a great measure, the free use of this salt—some to omit it altogether in a number of cases.

More of this elsewhere.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*On Diseases of the Liver.* By George Budd, M. D., F. R. S., Professor of Medicine, in King's College, London, etc. 1853.

The important functions which are assigned to the liver in our complex machinery, and the various changes which it undergoes in disease, have made works specially devoted to its study and elucidation particularly acceptable to the profession; the more so surrounded, as it is, by numerous impediments and difficulties to correct diagnosis of its structural changes; touch may, indeed, sometimes be rendered available in estimating its bulk, but percussion fails to enlighten us upon the changes of its texture, so that in distinguishing diseases of the liver, we have, in a great measure, to rely upon the signs of functional disturbance which, if we except jaundice, are at best equivocal. To those residing in hot climates and Southern latitudes, which constantly predispose to biliary derangement, with a train of serious and fatal maladies, the work of Dr. Budd will be especially acceptable, and his plain, yet lucid style invite the reader to a careful consideration of the several divisions of his book.

For those who may not have the book itself at hand, some of the opinions and views of the author may, with due brevity, be advantageously embraced in our proposed notice.

The following are the divisions of the book:

On Congestion of the Liver.

On Inflammatory Diseases of the Liver.

On Diseases which result from Fatty Nutrition of the Liver, or Faulty Secretion.

On Diseases which result from some growth foreign to the natural structure.

On Jaundice.

The author having considered the structure of the liver, and brought it in a visible manner before the reader, by numerous wood cuts, exhibiting the portal and hepatic veins, the capillary vessels and nucleated cells, the writer remarks under this last head, that it is not in the liver only that the cells perform this office, for it seems established as a general law, that all true secretion, whether in animals or in plants, is effected by the agency of cells; that "however complex the structure of the secreting organ, these nucleated cells are its readily operative part." In each secreting organ, the secreting cells have a peculiar form, or to withdraw from the blood, the secretion proper to the part.

On examining the nucleated cells of the liver under the microscope, we see that most of them inclose small spheroidal globules, which are recognized by their dark outline or high refractive power, to be globules of oil or fat. In ordinary livers the fat globules are small, but in persons who die of phthisis and those especially who have been fed upon fatty substances, they are so numerous and large as greatly to increase the volume of the liver.

The coloring matters of the bile are also contained in these nucleated cells, as evidenced in persons who die jaundiced, large quantities having been found collected round the nuclei, and interspersed through the cells.

In a medical point of view the coloring matters of the bile are highly important, speedily indicating, when not duly eliminated, defective secretion of the liver; tinging the complexion, and imparting deeper color to the urine.

The principle of the chemical influence of the air upon the coloring matter of the leaves of plants, as well as upon the coloring matters of the bile of different animals, is ingeniously expressed by the writer in these words.

"The coloring matter of bile have different tints of green, yellow, and brown in different animals. It has been shown by Berzelius, that the green coloring matter of ox bile, of which the yellow coloring matter is probably a modification, is identical with chlorophyl, the green coloring matter of plants. Its exact composition has not been determined; but it contains a large proportion of carbon and hydrogen, and from seven to nine per cent. of nitrogen.

"The brown bile-pigment of other animals seems to be also a modification of the same substance. When exposed to the air, it absorbs oxygen, and becomes of a dark green. Nitric acid, also, by its oxidizing influence, renders it of a dark green, which soon, however, passes into various shades of red. A similar change of color is produced by similar influences in the leaves of plants. The various shades of yellow, brown, and pink, which the leaves present in autumn, are all owing to the action of oxygen on their coloring matter when, at the appointed time of their decay, they can no longer resist the chemical influence of the air."

Although it is amongst the most important purposes of the liver to secrete bile, and to maintain the purity of the blood by depriving it of foreign matters; its uses, observes Dr. Budd, are not confined to the changes which it immediately produces in the portal blood. The fact that the bile is poured into the intestinal canal, so near to its upper end, is sufficient to show that it is not a merely excrementitious fluid; but that, when it has arrived in the intestine, it has important offices to serve. These offices are related to the function of digestion on the one hand, and (according to Liebig) to that of respiration on the other.

It is the opinion of the author, that the part which bile plays in the process of digestion has been much overrated, notwithstanding, that formerly its principal use was supposed to be for the completion of digestion. Experiments have now demonstrated that for this purpose the gastrice juice is all sufficient, except for the assimilation of fat upon which it produces no action. On this head the following language is employed by the writer:

"It appears then that all the staminal principles of the food may be digested or rendered fit for absorption without the aid of the bile—a fact, indeed, sufficiently established, as regards man, by the observed effects of permanent closure of the common gall-duct. In a future chapter, cases will be related in which the common gall-duct was completely and permanently closed by a gall-stone, so that no bile could flow into the intestine; yet in spite of the complete absence of bile in the intestine, and of the deep and permanent jaundice which results from this condition, the body was tolerably well nourished for more than twelve months; clearly showing that all the staminal principles of the food must have been digested and absorbed."

Such cases are quite as convincing as the results of direct experiments, and show conclusively that all the staminal principles of the food may be digested and absorbed without the aid of the bile. But it does

not follow from this that the bile is of no use in digestion; for the very cases in question show that permanent closure of the common gall-duct destroys life in the end, and generally in little more than twelve months, by causing a gradual impairment of nutrition.

The estimated amount of bile daily secreted is from 17 to 24 ounces, depending greatly, of course, upon the health of the party. To the neglect of an abundant supply of fresh air, exercise and food of an unirritating quality, may unquestionably be traced the frequency of bilious disorders in hot climates, as well as "the greater frequency of bilious disorders in middle life, when men begin to take less exercise, and their respiration less active, while on the other hand, the tendency to indulge at the table but too often increases.

The first morbid action of the liver, of which the author speaks, is congestion; the efficient means of removing such a state, when dependent upon an unhealthy state of the blood, or in the hot stage of fever, is stated to be quinine and other antidotes to marsh poison.

The connection between abscess of the liver and dysentery is considered at some length in the book before us, the frequent association of the two diseases in the same individual, having led to conflicting opinions amongst authors regarding the primary diseased organ.

Dr. Cheyne having discovered in the majority of his dissections, in speaking of the dysentery of Ireland, that the liver was apparently sound.

Dr. Abercrombie, although he frequently found ulceration of the intestines, in cases of abscess of the liver, is disposed to consider the association of the two diseases accidental; and remarks in his work on diseases of the stomach, that dysentery is often accompanied by diseases of neighboring organs, especially the liver, in which are found in some cases, abscesses, and in the protracted cases chronic induration.

Annesley, in India, where diseases of the liver and of the intestines are frequent, found it exceedingly difficult, if not impossible, to decide in which organ disease had priority, yet in some cases he inclined to the opinion, that "abscess of the liver was not only consequent on the dysentery, but caused by it."

To a similar conclusion Dr. Budd has arrived. The opinion that abscess of the liver is a sequence of dysentery, is strengthened by the fact that the lower extremity of the ileum is frequently found diseased as well as the large intestine. "In India, derangements of the liver, consisting in excessive and perhaps vitiated secretion of bile, and inflammation of the gall-ducts, are very common; the consequence it

would seem, of the heat of the climate and the free living, in which the English in India indulge."

With regard to the treatment of suppurative inflammation of the liver, *active* treatment will be of value only before an abscess exists, and the administration of mercury after the ascertained existence of such a state, would be worse than useless, because positively injurious. Annesly observes, there can be no doubt that the system will not be brought under the full operation of mercury, or that ptyalism will not follow on the most energetic employment of this substance, where abscess exists. The wisest course, then, is, I believe, says Dr. Budd, merely to regulate the bowels by rhubarb, or rhubarb and aloes; to recommend habits of strict temperance; and when the circumstances of the patient permit, residence in a mild climate and other measures that tend to improve the general health. If the complexion be sallow or dusky, the nitro-muriatic acid, as recommended by practioners of India, will often be productive of benefit. Whenever there is reason to infer, from increase of pain and fever, that fresh inflammation is set up within the cypt, and that the abscess is growing larger, blood should be taken from the side by leeches or cupping, or a blister should be applied there.

It is a common practice in India to thrust an exploring needle into the liver to detect the presence of abscess, and a cure is sometimes affected in this way.

The proportion of recoveries of hepatic abscess seem to be as great when the abscess has opened into the lung, or the bowel, as when it has made its way through the side.

After treating at some length adhesive inflammation and fatty degeneration of the liver, the author passes to the consideration of functional disorders of that organ as exhibited in the excessive and defective secretion of bile, and which, although less destructive of life than the class of diseases which have been under consideration, are the fruitful source of serious inconvenience and grave results.

The quantity of the bile, like that of the urine, may vary much without giving rise to serious inconvenience, depending greatly upon climate and the observance of hygienic regulations. A prudent dietic course frequently proving all sufficient for the correction of an excessive secretion of bile, and the removal of consequent irritation.

"The readiness," observes the writer, "with which these attacks are removed, often makes people regard them lightly; but they are not unimportant, as evidence of disorders, which, aggravated by time and by continuance in the habits under which they have arisen, may end

in some organic disease, or in the total failure of those assimilating processes on which nutrition depends.

During the attacks, signal relief is produced by a dose of calomel, or blue pill, followed by saline purgatives. If there should be pain or tenderness in the region of the liver, and the patient can well bear it, blood should be taken by leeches, or by cupping. These measures are generally sufficient for the time, but they do not strike at the root of the evil. Exemption from future attacks, and from the manifold and greater evils to which these disorders may lead as age advances, can only be procured by a change of habits. One of our objects, in directing this, should be to increase the amount of oxygen inspired, and thus to consume in respiration, or burn off, materials that would otherwise be left to excrete. The means most efficacious for this purpose are sea voyages, riding, or other exercise in the open air, well-ventilated rooms, early rising, the cold or shower bath, etc. Too much indulgence in sleep, which so much reduces the activity of both respiration and circulation, must be especially injurious.

Another object of still greater importance, should be to limit, in the food, the supply of those materials—such as spirituous liquors, butter, cream, fat, sugar—which contribute directly to form bile, or which increase the quantity indirectly, by serving as fuel for respiration. Some of those aliments—as cream aud porter, for instance—seem to be not only pernicious in this way, but, also, by directly embarrassing the secreting function of the liver." Defective secretion, which, by Dr. Prout, has been ascribed "to excess of acid in some part of the intestinal canal;" and in which view of the subject the author concurs. Most cases of this kind may be traced to disordered digestion; an avoidance of drastic purgatives, and the use of magnesia, bismuth, or chalk, will generally relieve frequent and distressing headache, as its accompanying symptom.

In chapter the fourth, the origin of cancerous tumors and other foreign growths of the liver is diseased. "The most important member of this class," says Dr. Budd, "is *cancer*, which is more frequent in the liver than in any other organ. Indeed, no serious organic disease of the substance of the liver is, in this country, at least among the people who have never drank hard—so frequent as cancer. In some instances, the liver is the only organ affected with cancer, or is the organ in which cancer originates; but far oftener, the formation of cancerous tumors in it is consequent on cancer of some other parts, especially the stomach and the breast." And, again, "the most remarkable property of cancer—a property which often influences the condition of the patient

more than any damage the disease does to the part in which it first appears—is its power of dissemination. This varies much in degree, according to the variety of cancer and the part of the body in which it originates." Cancer is often propogated by inoculation, as well as by the injection of cancerous matter into veins, from one animal to another, and thence disseminated in the venous current. "So many instances have occurred of cancer of the penis, in men whose wives had cancer of the womb, that many physicians have been led to believe that the disease, in these instances, was propagated by contagion."

Upon the whole, the author is forced to the conclusion, that the cancer depends less on the general state of nutrition, and more on accidental conditions affecting the particular parts than some other diseases—for instance, consumption and scrofula—which likewise result from faulty nutrition. It is not hereditary in the same degree, and it seldom originates, as the last-named diseases do at the same time, or nearly at the same time, in fellow organs, on the two sides of the body. It occurs also in persons who are plethoric and seemingly robust.

The remaining portion of the book is devoted to the consideration of hydatid tumors of the liver and to jaundice; the former, as is asserted by other authors, are considered more common in the liver than in any other organ.

In regarding the question, as to the nature of hydatid tumors, and how they originate, Dr. Budd has only coincided with many able pathologists, in the opinion that these cysts "are the dwelling-place of those microscopic animalculæs to which Rudolphi gave the name of echinococcus, from the cylinder of books which surrounds the head. The echinococcus is a transparent, colorless creature, somewhat egg-shaped, and presenting under the microscope, a distinct double outline. (A representation is here given.) The creature is studded with globular bodies, which, from their refracting light have, under the microscope, a strong, dark outline and a bright centre. In some hydatids, the echinococci are not seen as white grains on the inside of the cyst, and are hardly discoverable by the naked eye, but they are readily seen when a portion of the cyst is looked at through the microscope." Hydatid tumors in the lung differ little from those in the liver, they have been known to exist in the lower lobes of the lung, when there were none in the liver, or in any other organ. Hydatid tumors of the spleen, though rarely found in its substance, are often associated with those of the liver and the mesentery.

The author reports numerous cases of hydatid tumors in the liver, in which recovery took place; a case of this kind was reported in a medi-

cal journal some months ago, the patient being a man, and recovery following an operation.

A short chapter upon jaundice completes this valuable volume. A volume worthy to be placed in every medical library, and which will, doubtless, enhance the reputation of its author.

The work is embellished with four beautifully colored plates, the drawings for which, were taken from preparations in the museum of King's College.

Steel, on Camp street, has the book for sale. G. T. B.

New Orleans, Sept., 1853.

II.—*Abrégé de Pathologie Medico-Chirurgical ou Resumé Analytic de Medicine, et de Chirurgiæ.* Par M. E. Triquet, Docteur en Medicine, Ancien Interne à Hopital de Tours, etc. Paris, 1852. 2 vols.

Works like the one before us, become necessary for the active practitioner, who is too much occupied to read carefully one half that flows from the medical press now a days. To abridge knowledge, to condense the numerous facts and observations which are scattered over the vast field of medical science, is at once a herculian task for the author, and a profitable speculation for the publisher.

Books to be read now, must be small in size and direct in their object and aim—they must contain many facts in a few words, and much reasoning in a small compass; otherwise few, very few will take the time or the patience to read them. "Pathology Abridged and Theraputics Condensed," would take much better than voluminous Encyclopedias or huge Dictionaries on the same subjects.

The world, at least the greater portion of it, rather looks to the conclusions at which men of science and learning arrive, than study the steps by which they may have reached such conclusions. It is much easier says the world, to cut than untie the gordian knot.

It would be impossible to review a book of this character and size in a periodical like this; so many and various are the subjects discussed, that we could scarcely enumerate them in the limited space allotted for this department.

We may state, *en gros*, that the work represents the present views and opinions of the French Medical School, in Pathology and Therapeutics.

III.—*Transactions of the Tennessee State Medical Society. Nashville*, 1853.

This society held its 24th Annual Session, at Nashville, in May, 1853. Dr. Watson presided over the meeting and delivered an address, after the Society had been organized for business, on the *Retrospective, Perspective and Prospective Views of Medicine.*

The address, like similar efforts on like occasions, is a well turned and, if not a polished, is at least, a highly creditable lecture. He goes back to the dark ages, to the mythological era of medicine, when demi-gods paid homage to the sons of Æsculapius; and when the wise physician skilled our wounds to heal, excited the envy and admiration of mankind. We cannot follow Dr. Watson in his rapid, though graphic sketch of the ups and downs of medicine, in its primitive state; we pass on to the latter and more practical part of his address, wherein he speaks of our wants—of the diseases which mock our art, and for which other and more successful means are to be yet discovered to stay their fatal tendency. On this point he says—

"We want a remedy for consumption and cancer, and more reliable ones for many other diseases. The question arises, shall such ever be discovered? does the world contain them? or are any artificial combinations capable of producing them? I think we shall soon be able to give an answer to the foregoing questions: for if the world contains the means, or if a combination of different things be capable of producing them, they may yet be discovered. By the discovery of a more effectual remedy for scrofula, we may learn how to protect persons against phthisis. But does the pathology of these diseases admit of any relief. Can tubercles and cancerous degenerations, with all their structural derangements, be ever cured by clinical medicines? Judging from the past and present, I fear not, indeed; and more attention will, in all probability, hereafter be given to the means of preventing than of curing them. We shall learn to seize upon their early indications by signs we know not now, and by the use of proper measures, protect patients from further progress of such maladies. Some modern Jenner may yet give the world an antidote against these diseases. It is highly probable that in a short time we will have able scientific men engaged in the treatment of such affections; when we shall have a separate branch of medicine for every organ—as separate and respectable as surgery and midwifery are in our day. Hence we may soon look out beyond occulists, dentists, lithotomists, for pneumatologists, gastrologists, hepatologists, spermatologists, and in short, a name and a doctor for every part of the system, whose care and duty it shall be to treat the diseases of one particular organ in an *open, scientific way.* The men who are now engaged in a quackish practice of this kind will have to give way to able scientific men, as did the barbers and bath-men, and slaves of old in surgery,

and as did silly women and ignorant pretenders in midwifery. The obstetrician and surgeon is not more distinct now, than will be pneumatologists, gastrologists, etc., in a coming day. And when great minds shall be engaged according to professional interest, zeal and emulation, in such investigations, we may expect all that can be achieved by man, aided as he will be by strong collateral lights on every hand. Medicine does not now, as in former times, pertain to one or two countries, but to the whole world. All scientific knowledge is fast being equalized throughout the world. The world will have no more dark ages. All will be ages of inconceivable light. We may say of the sun of science, in degree, as was said of the Son of Righteousness, it will arise over the whole earth with healing in its wings. Nation shall no longer learn the rudiments of science from nation, but all shall be taught, from the least to the greatest, the things that promote man's good HERE. 'The farthest verge of the green earth,' shall rejoice in the light of science, and by it, 'the most distant barbarous climes' shall be softened down; and all the nations of the earth shall become ONE in the great light which is now pervading the whole face of the earth, and a strong intellectual union be thereby established among all mankind.

We believe Dr. Watson's predictions may yet be in part fulfilled, aleast, we observe in the public and professional mind, a tendency to specialities in the study and treatment of disease. Both the profession and the public will derive marked advantages from it, if wisely carried out; the first will acquire more skill, and consequently succeed better in the diagnosis and treatment of disease; the second will derive all the advantage that can flow from such knowledge and skill in our profession.

After Dr. Watson's Address, we have a lengthy "*Report on the Adulteration of Medicines, Chemicals, Drugs, etc.*," by Dr. R. O. Currey, of Nashville. This is an important paper and will, we hope, exercise a salutary influence in the preparation of medicines, drugs, etc. He reviews the different articles of the Materia Medica which are usually adulterated, and points out the tests by which they may be detected.

We shall notice a few of the articles in common use.

"*Balsam Copaiva.*—M. Guibourt, acting under the authority of a commission from 'Ecole de Pharmacie de Paris,' after examining a variety of samples of this oleo-resinous balsam, asserts that the four following properties indicate a balsam that is certainly pure: 1. Entirely soluble in 2 parts of absolute alcohol. 2. Forming at 60 F. a transparent mixture, with two-fifths of its weight of a strong solution of ammonia. 3. Solidifying with one-sixteenth of its weight of calcined magnesia. 4. Producing a dry and brittle resin after prolonged ebullition with water.

Its usual impurities are castor oil and turpentine. The presence of this fixed oil will prevent its solubility, according to the first property; neither will

the resin left after ebullition harden, but will remain soft. A drop on paper, volatilized by heat, leaves a translucent spot, if pure, but if impure, this spot will be surrounded by a fatty areola. Castor oil will also prevent the transparency of the mixture with the solution of ammonia. The turpentine is detected by its odor."

"*Ipecacuanha* root is composed of two parts, the cortex and woody fibre in the proportion of 4 to 1. The virtues of the root reside eclusively in the bark, the woody fibre being inert. Hence, it may be seen that when the root is ground up, regardless of these distinctions, one-fifth of its active properties is destroyed, consequently the choicest powdered ipecac is labelled *cortex sine ligno*. The root is annulated, rendering its recognition easy. Hence it is in its preparations and powder that it admit of adulteration. Liquorice root is frequently ground up to a large extent with it, and again, the peculiar powder called 'powder of post,' is also added to it. This, of course, would weaken its emetic properties, to restore which, its adulterators resort to a certain proportion of tartar emetic. With the microscope we can detect the different starch granules of these several powders—while sulphuretted hydrogen will produce an orange precipitate, the golden sulphuret of antimony, if tartar emetic be present. The presence of foreign substances will also affect its action with certain chemical agents.

Blue mass—pilular hydrargyri, if of the officinal strength, should contain one-third mercury. While, therefore, it frequently falls far short of this standard, it is also found to contain a strange medley of impurities, as compensating agents. The following analysis, made by Prof. Reid, of New York, upon a sample of imported blue mass, presents these filthy substitutions in a striking manner. In one hundred grains, there were of—

Mercury,	7½
Earthy Clay,	27
Prussian Blue,	1½
Lard,	2
Soluble saccharine matter,	34
Insoluble organic "	12
Water,	16

Pure blue mass should contain 33½ grains mercury in 100. Here we have only 7½—nearly four-fifths less than there should be. Sulphate of mercury is also found in it, and when so, it renders the mass highly injurious. This impurity arises from the gross error of endeavoring to heighten the color of the conserve of roses by means of sulphuric acid. Consequently, on mixing the ingredients into a mass, a chemical action takes place—the deep blue color of the mass partakes of a golden tinge, from the formation of the sulphate of mercury. Its presence may be suspected when this particular tinge is found, but readily ascertained after washing, by means of the baryta test. The organic earths will be left after incineration of the mass, and Prussian blue by striking an ink with sulphate of iron"

Iodide of potassium.—The principal adulterations of this valuable medicine are carbonate of potassa and chloride of sodium. Iodide of potassium is very soluble in water, and in five times its weight of alcohol—is *slightly* deliquescent and of a pungent taste. Carbonate of potassa is insoluble in alcohol—is very deliquescent, and consequently its presence will cause the iodide to assume a pasty, semi-fluid condition. It will also effervesce with acids, and redden tumeric paper. Either of the chlorides may be detected by the silver test. The quantity of iodine present may be ascertained by separating it from the potassium by heat, condensing the scales and weighing either of the ingredients separately. In a sample analyzed by Dr. Christison, he found 74 parts of carbonate of potassa, 16 of water, and only 9 of the iodine.

The *Sulphate of Quinine* is now frequently adulterated says, Dr Currey, with a new principle called *quinidine*, which is easily obtained and is wholly inert. The test for detecting this fraud is as follows: To ten grains of quinine add 10 drops of diluted sulphuric acid and 15 drops of pure water, in a strong test-tube, fitted tightly with a cork, and then apply gentle heat to hasten the solution. When cooled, add to the mixture 50 drops of sulphuric ether, with 20 drops of spirits ammonia, then close and shake the mixture. The tube must now be closed and shaken for some time. If there be no cinchonine or quinidine in the salt, it will be completely dissolved; but if impurities be present, they will rest on the surface, when the contact of the two layers of clear liquid takes place. But, if more than a tenth of cinchonine or quinidine be present, an insoluble precipitate will be found interposed between the two fluids. If this be quinidine, it will be dissolved on the addition of ether, while the cinchonine will remain unaffected.

We deemed it of some importance to point out, as above, the method of detecting two of the most common articles with which the sulphate of quinine is adulterated. To the general practitioner in the South, a pure article of quinine is of the last importance; and, he should, for that reason, be made acquainted with tests of its purity or impurity.

The sources of Adulterated Medicines are two-fold, foreign importation and home manufactures—the former has been in a great degree remedied, by the appointment from Government, of competent individuals, at our large importing cities, who have rejected and driven from our warehouses, thousands of pounds of drugs and medicines, utterly unfit for the purposes for which they were intended.

The trade and the profession have already been greatly benefitted by this surveillance; and both will be likely to advance still further the interests of the public by discouraging and putting down the too common practice of manufacturing spurious drugs and medicines, at home. In a former number we published a valuable paper on the subject of

adulterated drugs from the pen of Mr. Bolton, of this city; it is to that essay, we would refer our readers for further details and important suggestion on this subject.

Dr. Curry closes his report on the subject by pointing out the surest means of correcting some of the evils which flow from ignorance of chemical and pharmaceutical science. On this subject, he says with truth.

"Pharmaceutical education is another important means for securing this end. It should be regarded as essential for the business of dispensing medicines. In this respect, other countries are far in advance of our own. In Sweden, no youth can enter as an apprentice in an apothecary's shop, who is under fifteen years of age, and who has not gone through a school course of history, geography, mathematics, Latin and modern languages. In England, to practice pharmacy requires a diploma from a college of pharmacy. But in our own country, the monied qualification is the only requisite; the profits of the business being the *sine qua non* with four-fifths of those who engage it. But we confidently hope that a brighter day is dawning. The science of pharmacy is beginning to be regarded as a twin sister to the science of medicine, and that whatever affects the one, equally affects the other. This arises from the attempt now being made to dissever the two professions, and place each upon its proper basis, yet as auxiliary to the advancement of each other. The colleges of pharmacy of New York and Philadelphia, have nobly struggled for years, to elevate their science to its proper position, and are now beginning to reap the reward of their labors. In all of our large cities pharmaceutical associations are being formed, and during the last years, by the united action of all such associations throughout the country, the National Pharmaceutical Association was organized. It is gratifying to your committee, engaged now as he is, exclusively in promoting this department of honorable medicine, though claiming paternity from the household of Æsculapius, to bring to the notice of his medical brethren, the efforts of pharmaceutists to elevate their profession to its proper position, by requiring higher qualifications for membership in their associations than merely the ability to conduct ably a drug establishment. The pharmaceutist stands between the physician and his patient. As the compounder of his prescriptions, the physician's success is, to a certain degree, in his hands. Such a responsible position requires a thorough pharmaceutical as well as practical training. It seems to be an inevitable result that there should be a distinction made between the apothecary and the druggist—the former standing between the physician and the latter. He, the apothecary, would be accountable for all medicines compounded and dispensed, and patronage should be extended to no one unless he had a certificate of graduation from some college of pharmacy.

General chemistry, theoretical and practical pharmacy, materia medica and medical botany, are all essential for the proper practice of pharmacy—years of practical training in a drug store cannot be substituted for this course of

instruction. It is true, our assistants may become expert as salesmen, and be able to pronounce an opinion respecting the character of the ordinary medicines; but the daily routine of duties in our establishments is too imperative to allow of any systematic or regular instruction, and hence, especially in our western country, there is a deficiency of instruction in pharmacy. We hope, however, that the day is not far distant when the college of pharmacy will take its place along side of the medical hall, or when such advantages will be given to students of pharmacy in our medical schools, by the establishment of professorships of theoretical and practical pharmacy, as will induce them to qualify themselves properly for their responsible duties. Such professorship *might* be instituted as collateral, not as indispensable for every member of the institution. There is no doubt but that many would avail themselves of such advantages in connection with the courses on chemistry and materia medica. Pharmaceutical education is, therefore, another important means for remedying this evil.

Foreign adulterated and deteriorated medicines being declared contraband by our national Congress, it devolves upon our State Legislatures to devise some means by which home adulterations may be arrested. More, however, is to be gained by popular opinion, and by pharmaceutical and chemical education, than by any other means. Our Legislatures may pass laws, inflicting heavy penalties upon all who may adulterate, or knowingly sell adulterated or deteriorated medicines, but who is to be judge in such cases?

"Regarded in any light, the subject of this report is one of great interest. It involves more than the preservation of our estates—the honor of man is implicated in it—his life, and the consequent happiness and welfare of families. Was every medicine strictly pure, and every physician fully competent to his duty, the honorable science of medicine would not be an uncertain profession. Quacks and their nostrums would no longer find a resting place, for they would be regarded as monsters

'Of such frightful mien,
That to be hated needed only to be seen.'"

Dr. C. is entitled to the thanks of the whole profession for the very able manner in which he has discharged his duty as chairman of the Committee on the "*Aldulteration of Medicines.*"

A case of *Cerebral Disease* is reported by Dr. Ransom, of some importance. We prefer to give it in the language of the author, as any abridgement of its details would diminish its practical value.

"August 9, 1852. Saw in consultation with Dr. S———, Julia H., aged about ten years.

About the 20th of last month, after complaining for a day or two of headache, she was very suddenly attacked with partial paralysis of the left side of the body. She soon had high fever, with violent pain of the head, and some delirium. By mercurial cathartics, refrigerant diaphoretics and local depletion, she was partially relieved, but again grew worse. About eight days

ago she began to have clonic and tonic spasms of the muscles, generally, which have continued paroxysmally to this time. Some of the muscles of the lower limbs are permanently contracted. The left leg is rigidly extended, while the right is closely drawn to the body, and she seems to suffer excruciating pain when any attempt is made to change their position. The right pupil is natural, while the left is widely dilated. Has not been able to articulate since the 6th; swallows with much difficulty; skin hot and dry; pulse 120 and rather small; tongue coated yellow; bowels sluggish; is slightly ptyalized.

Treatment.—A blister to the whole occiput, and ammonia liniment to the spine; to take half oz. of castor oil, and afterwards one gr. of calomel three times a day, or as often as may be necessary to keep up ptyalism. A teaspoonful of spirits of nitre every two hours.

11th. Less fever; much the same otherwise. Blister drawn well, spine red and sore; bowels moved only once. Ordered purgative enema; continue calomel and add camphor to spirits of nitre.

13th. Paroxysms or spasms not quite so frequent or long continued; castor oil and injections have failed to move the bowels; ptyalism increasing. Suspend the calomel, and give half a drop of croton oil in half oz. of castor oil, every two hours, until bowels are moved, assisting it with enema.

15th. After taking four doses of the purgative, the bowels were freely moved. More quiet; very little clonic spasm now, but the tonic contractions remain the same. Left side is completely powerless. There is now such an acute, morbid sensibility of the surface of the limbs, that she cannot bear the contact of the slightest covering. Re-apply the blister, extending it over the cervical spine.

18th. No fever; swallows better; bowels easily moved. Has incontinence of urine. Dress the blister with mercurial ointment, and keep the bowels open with oil.

21st. Improving in general health. The paroxysms of spasm have entirely subsided, but the limbs remain contracted and paralyzed as before. Re-apply blister, and give bichloride of mercury three times a day, (one-twentieth gr.)

25th. The morbid sensibility of the surface of the limbs is so far diminished as to allow of their being covered. Has a bleeding sore on the sacrum; incontinence still continues. Continue the mercury and 10 drops of tincture of cantharides, three times a day.

Sept. 1st. General health tolerably good; she recovered her speech imperfectly on the 27th; can now protrude the tongue, the left side of which is palsied; is relieved of the incontinence; sore on the sacrum soon healed after removing the pressure. Continue same treatment, and iodine and mercurial ointment to the old blistered surface.

6th. General health good; is gaining flesh rapidly, but the improvement in paralysis and muscular contraction very slow. I should have mentioned before that there has as yet been no want of sensibility of any part of the surface. Take in addition to the bichloride of mercury, one-twentieth gr. of strychnine, three times a day.

14th. After taking the strychnine two or three days, it produced a trembling and wildness, that induced the parents to discontinue it, but since then, the improvement has gone on much more rapidly than before. The right leg can now be extended. Continue it in one-twenty-fifth gr. doses. Friction to the spine and limbs.

Nov. 1st. The patient has gradually acquired tolerably good use of all the limbs, but the left foot remains drawn backwards so as to impede her walking. The left pupil has not yet contracted to its usual size. Discontinue medicine except laxatives, and keep up the frictions.

March 30, 1853. Patient has been apparently well for three months. Anything causing an afflux of blood to the head, as being too long in a stooping position, causes the left pupil to dilate.

This case is interesting, as one exhibiting at once the various effects of a morbid condition, in a different degree, of opposite sides of some portion of the nervous centres, as well as affording additional evidence of the value of mercury in the active stage of such affections, and the efficacy of strychnine in restoring nervous energy. Although the manner of attack would lead us to suspect hemorrhagic extravasation, yet the prominent muscular contractions and other symptoms, seem to point out inflammatory softening as the true pathological cause."

Dr. Park, of Franklin, reports a new mode of operating for *fistula in ano*, with a new instrument; but as we have found the old method all sufficient, we shall not make room for the particulars. We pass on to notice a case of "*false aneurism*," read before the society by Dr. Knight, of Rutherford County.

The gentleman (we condense the case.—Ed.) was a dissipated man, and had been suffering for 20 years with an aneurismal tumor, on his left leg. It was situated midway between his knee and ankle, posteriorly, and had been caused by a musket ball. Until seen by the Doctor, he had experienced but little pain or inconvenience. The tumor had increased in size, and was tender on pressure. The skin over the tumor presented a gangrenous appearance; pulse 130 and feeble. No sleep for several nights. No aneurismal thrill could be detected; formerly it pulsated, but not at this time.

Treatment.—Pus being detected, the tumor was lanced, and it was discharged along with blood; it had partly coagulated. By this he was greatly relieved, but became sick and feverish. The limb was too tender to bear the bandage. It was dressed with charcoal poultices, and placed on an inclined plain. Took mild cathartics and anodynes to secure some rest. For four or five days had high fever, feeble pulse. Tumor continued to discharge pus and blood. For the next four days he grew more feeble. In this state of the parts, and knowing his

bad habits, the Doctor, after consulting with other practitioners, determined to amputate the limb. To this the patient objected, and finally so far recovered, as to resume his usual occupation as a wood-cutter, on the Mississippi river.

Dr. Bowlin reports to the Society, the following unique case of *Fallopian Pregnancy*, which we shall give entire.

"I have just returned, accompanied by Dr. D. H. Johnson, from an examination of Mrs. Wiggs, from whom I have learned the following history. She was born of healthy parents, was robust and of a good constitution—menstruated at the age of fourteen, and continued regular until the age of twenty-two, at which time she married. In October, ten months after her marriage, she was attacked with excruciating pain (supposed to be of labor) across the loins, hips and lower part of the abdomen, which was only partially relieved, for twenty-four hours, by copious bleeding.

The pain then increasing, an old and very respectable midwife was then sent for, who failed to deliver her; Dr. Wm. Norton was immediately summoned. Dr. Norton confirmed the opinion of the midwife, viz: that her time was not out. Another old and experienced midwife was sent for, who gave it as her opinion, that Mrs. W. was not in the family-way at all, and that there was something in the uterus, she knew not what. Mrs. W. continued very unwell from October 25, 1830, to February, 1831, during two months of which time, she was unable to lie down at all, but was compelled to remain in a sitting posture, because of the severe pains, together with an alarming and constant discharge from the uterus, which was dark colored, very offensive to the smell, and evidently saturated with flesh. February 28, 1831, the disease having now assumed an alarming aspect, Dr. Beckton, of Rutherford county, was called in, who, after making the necessary examination, decided that it was a clear case of Fallopian Pregnancy, and the only one he ever saw of the kind. Dr. Beckton possessing no means of relief, left her case to the powers of nature. Soon after this she began to improve, the enlargement of the abdomen began gradually to subside, the pain in the loins, hips and lower part of the belly became less severe, the discharge from the uterus began to diminish, and was less offensive. Her health continued to improve until menstruation was again regularly established, which continued until interrupted by pregnancy.

In July, 1832, Mrs. W. was delivered of a fine healthy child; since which time she has had four living children, and has miscarried once. There were thirteen years between her last. Twenty-two years have now elapsed since this singular occurrence. Mrs. W. is forty-five years of age, and enjoys a tolerable degree of health, with the exception of pains more or less severe, nearly all the time, in the right iliac region, where the bones of the child are deposited, and may be plainly felt by external examination. I have carefully examined Mrs. Wiggs, and there cannot be a reasonable doubt as to the nature of the case. It must be Fallopian Pregnancy.

Having met with but one similar case on record, and that was by Dr. Baird I believe, I have selected this case to report to the Medical Society of Tennessee, hoping it would be acceptable to the members of our honorable profession, and furnish another well attested case of Fallopian Pregnancy to the medical history of our country."

The following are the officers of the Society:

"Dr. Felix Robertson, Nashville, President.
" E. B. Haskins, Clarksville, Vice-President.
" J. W. King, Nashville, Recording Secretary.
" R. C. Foster, Nashville, Corresponding Secretary.
" W. P. Jones, Nashville, Treasurer.

The Society meets again next November.

IV.—*American Pharmaceutical Association.*

This Association met in Boston on the 24th of August 1853, and organized for the transaction of business.

The report of the Executive Committee for the past year was read by Prof. Procter, the Chairman. (We omit a part of this report, as not likely to interest the readers of the Journal, and quote only the following, which we recommend to every member of the Profession):—Ed.

1st. To increase the future usefulness of the Association, it is proposed that a system of local secretaryships be adopted, which in the beginning may be limited to the large cities and chief towns, and so distributed over the several States, as to enable the Corresponding Secretary and the Executive Committee to obtain and distribute information in an effectual manner. At first, it is proposed to appoint the local secretaries from among pharmaceutists known to reside in the localities chosen; and afterwards, if more agreeable to the body at large, in each place, the name of a person for secretary may be suggested by his brethren as their medium of communication, to be confirmed at a meeting of the Association. We believe that there are qualified persons who have the good of the profession at heart in most of the cities, and that such an organization would greatly increase the power of the Association in carrying out its disinterested measures for the advancement of pharmacy.

2d. It is recommended that the collection and arrangement of the Statistics of pharmacy in the United States, be committed to a special committee, properly authorized to act for the Association, in such manner as shall most effectually and speedily obtain a list of the reputable druggists and pharmaceutists in each place; ascertain the degree to which medicine and pharmacy are separ-

ated ; the condition of dispensing pharmacy : the condition and progress of preparative or manufacturing pharmacy ; the extent to which the apprenticeship system prevails, and wheather any attention is given to furnish apprentices with proper books for the study of their business ; and finally, whether there is any disposition to organize local societies.

3d. It is suggested that the subject of pharmaceutical education be entrusted to a special committee at the commencement of the session, that they may have time to prepare an address to the pharmaceutists of the whole country, which shall enter into the practical difficulties which oppose the attainment of pharmaceutic knowledge by assistants and apprentices, point them out, and encourage the brethren to extend reasonable aid to those in their service, both by personal interest. and by providing books and the opportunity to use them ; — and also shall recommend attention to the proper preparatory education and mental fitness of apprentices for the responsible offices involved in their duties.

4th. It is proposed the *idea* of universally adopting a single definite name, which shall indicate the qualification for preparing and dispensing drugs and medicines, as possessed by thorough-bred chemists and druggists, or apothecaries, be considered, and its propriety decided on. The word "Physician' indicates a person educated to practice medicine in all its branches ; why should not 'Pharmacian' define a qualified practitioner of pharmacy ?

5th. It is recommended that the Association shall earnestly advocate the *extensive issue* of a *cheap* and accurate edition of the United States Pharmacopœia ; say at the price of seventy-five cents or one dollar. Of the large number of persons who, in the country, lay claim to the names of apothecaries and physicians, a great proportion have never seen the Pharmacopœia as a separate and distinct work, a fact easily understood, when it is stated that but 1500 copies are published in ten years ! In many localities the U. S. Dispensatory is considered to be the Pharmacopœia. Physicians are constantly prescribing medicines under unofficinal names, and apothecaries making officinal preparations by foreign formulæ, because in the dispensary the receipes are all commingled in such a manner as to lead to confusion. This would be prevented, and a greater uniformity of practice created, by making the Pharmacopœia, with its clearly defined recipes, the guide at the counter, and in the laboratory, for the officinal preparations.

6. We believe that the action of the Association should not be limited to the practical, the ethical, and the educational interests of the profession,— should not stop within the limits of self-improvement. American pharmaceutists owe a large debt to their brethren in Europe, for a constant influx of knowledge, the result of their past and present investigations in pharmacy and its accessory sciences; and the time has fully arrived, when, as Americans, they should feel bound to render a larger return than individual efforts have heretofore accomplished. The pharmaceutical societies of Europe, offer annually, prizes for the determination of questions deeply interwoven with the practice or science of pharmacy, and thus elicit valuable accessions to existing knowledge. We believe that the Association is now competent to

adopt a course of this kind, as an incitement to dormant ability, and to awaken laudable ambition. At first, if the idea is adopted, the prize should be limited in value, except as a symbolic of reward for honorable and successful enterprise.

To make the proposition clearer, a few questions appropriate for such objects will be instanced.

a. It is conceded that Digitalis of American growth is less active and efficient as an arterial sedative and diuretic than that of English origin. Is this deterioration due to the less abundant formation of *digitalin;* to its modification; or to any other definable cause?

b. What are the impediments, if any exist, to the free cultivation of Colchicum autumnale in the United States, so as to preserve its power unimpaired; and is it true that the recent cormus is more active than the same carefully dried, and if so, why?

c. Do Hyoscyamus and Belladonna, grown in the United States, contain the active principles in the same proportions as the European plants?

d. Spigelia is admitted to possess positive anthelmintic power. Does this power reside in a distinct, well-defined principle, capable itself of producing the effects of Spigelia; if so, isolate and describe it?

e. The best essay on extemporaneous pharmacy, which shall treat of the incompatible combinations most usually prescribed, the best manner of avoiding them, and the most efficient methods of proceeding in effecting the union of substances that are physically incompatible, as emulsions, certain liniments, certain pill ingredients, etc.

f. For the best essay on the identification of volatile oils when mixed, their preservation, and the actual effects of light and air on them, under the ordinary circumstances that they are kept in the shop, so as to decide the question whether all of them, or only a part, should be kept in the dark, to prevent change?

g. For an essay which shall develop the commercial history of all drugs indigenous to the United States, as senega, spigelia, serpentaria, etc., as regards the manner and places of their collection for the supply of commerce, the annual amount collected, and the channels through which they enter general commerce.

h. For the best essay on the construction and material of pharmaceutical apparatus, including that for evaporation, distillation and solution more especially, as regards economy, convenience and effectiveness; with a view to the ordinary wants of a thorough pharmaceutist.

Such are a few of the questions which might be offered. The nature and value of the prizes, and the local or general invitation to the competition, will require mature consideration, should the idea be adopted.

7th. An efficient committee might be appointed to inquire into the nature, extent, and locality of *home adulterations*, and propose a remedy.

8th. Whether it would be advisable to appoint a committee to consider the subject of state and municipal laws in relation to controlling the trade in

drugs and medicines, and whether such laws would be likely to prove salutary without oppressing the well-disposed druggist and pharmaceutist.

On the indiscriminate sale of poisons, as now tolerated throughout the country, the committee previously selected for this purpose, made the subjoined able report. Let all read and profit by it:

"The Committee to whom was referred 'the subject of the indiscriminate sale of poisons, as now conducted by apothecaries, druggists and others, as regards the practicability of effecting some useful reform in the present state of the traffic,' Report, that they have been engaged since the time of their appointment in endeavoring to collect information relating to the subject, and in considering it in view of the object of the Association; yet they have been but partially successful. In the course of their inquiry the measures adopted by European legislators, naturally claimed attention, and what they now have to offer will include a notice of the measures legalized in Europe, the condition of the traffic in this country, and suggestions tending to regulate the sale of *popular* poisons in this country.

In Prussia, and perhaps in Germany generally, the law requires the apothecary to keep poisons in a closet, under lock and key, and not to sell them but under certain conditions, to persons free from suspicion. The poisonous substance, be it arsenic or other, must be enclosed in a box, tied, sealed and inscribed with the German or French name, and the Latin name; and marked with a *Death's head or three Crosses.* It is also necessary, in some of the German States, for the purchaser to give a receipt, declaring the name and quantity of the poison, that it was dispensed according to regulations, and that the seller is exonerated from all blame for its misuse.

In France, the law designates the substances considered poisons in view of the Government, which are required to be kept under lock and key, much to the annoyance of the Pharmaciens. These substances are *Hydrocyanic Acid*, the *poisonous vegetable alkaloids* and their *salts*, *Arsenic* and *its preparations*, *Belladonna* and *its extracts and tincture*, *Cantharides* in substance or extract, *Chloroform*, *Hemlock*, and its extract and tincture, *Cyanide of Mercury*, *Cyanide of Potassium*, *Digitalis*, its extract and tincture, *Tartar Emetic*, *Hyoscyamus*, its extract and tincture, *Nicotine*, *Nitrate of Mercury*, *Opium* and its extract, *Phosphorus*, *Ergot of Rye*, *Stramonium* extract and tincture, and *Corrosive Sublimate.*

In Great Britain, the country most analogous to our own in the character of its population, and the legal and economical usages that exist, the sale of poisons until recently was completely unrestrained by law; except, perhaps, a few municipal regulations. In the latter part of 1849 the subject of the loose manner in which the sale of poisons was conducted, and the frequent ill results that followed, was brought to the attention of the House of Commons by the Provincial Medical and Surgical Association, praying that no druggist be allowed to sell arsenic without a license, under penalty; that *no person* be allowed to sell small quantities of arsenic unless combined with some dis-

tinctive coloring material, that every purchaser must have a witness, and that every vendor should keep a strict record. The petitioners stated that, of the fatal cases of poisoning, *one-third* were from arsenic, and that in 1837-8 these cases amounted to 185!

Pending the action of Parliament, the subject was referred to a Committee by the Council of the Parmaceutical Society, who, as a preparatory step, issued a circular of inquiries to 1600 members, over England and Scotland, querying whether the parties sold arsenic; under what regulations, if any, for what objects and to what classes of persons, what trades employ it, whether general dealers sell it, would it do to prohibit its retail sale, how is it dispensed what number of accidents and whether these occur from its use by agriculturists?

The Committee reported that a majority of the *Chemists* and Druggists require witnesses in selling arsenic, label the inner and outer wrappers, and some color it. The classes of persons who buy arsenic are colorists and chemical manufacturers, candle-makers, farmers, flock-masters, veterinary surgeons shipwrights, glass manufacturers and dyers, in large quantities; and braziers' whitesmiths, bird-stuffers, gamekeepers, gardeners, grooms, whitewashers, painters, protechnists, rat-catchers, and housekeepers of all grades, for vermin, in small quantities. They ascertained that arsenic was employed most extensively through the agricultural districts, both for *sheep-dipping* and for *steeping wheat*. About 40 lbs. of arsenic are required for every 1000 sheep to kill vermin. Of 728 answers, 509 advocated prohibition; yet the numerous legitimate uses of the poison render its sale necessary. One large farmer had killed in a year more than 40,000 rats. The Committee arrived at the follo w ing conclusions, viz:

1st. That with regular chemists and druggists, proper precautions are taken and few accidents occur.

2d. That the unrestricted sale of poisons, in general, by *unqualfied persons*, is the great source of danger.

3d. And that the total prohibition of the retail sale of arsenic is impracticable and inconsistent with the requirements of legitimate trade.

Subsequently, on the 5th of June, 1851, Parliament enacted a law requiring

1st. That all arsenic sales shall be witnessed by a third party when the purchaser is unknown to the vendor.

2d. That all arsenic sales shall be registered in a book, in a specific form.

3d. That not less than ten lbs. of arsenic shall be sold unless colored by soot or indigo, unless for a specific purpose in the arts, under a penalty of $100.

4th. That the Act shall not apply to arsenic used medicinally, or to the intercourse between wholesale and retail dealers.

5th. And that the word 'arsenic' includes all preparations of arsenious and arsenic acids and other colorless poisonous preparations of arsenic.

The Act was confined to arsenic because it was the most generally known and most accessible poison, and because restrictive legislation on the whole list of poisons was looked upon as impossible. The poison is sold in all quan-

tities, from a pennyworth up, at petty stores, and by general dealers in England, and the Arsenic Act, without depriving of their right to sell, compels them to do it as above.

To the correspondence they have instituted, your Committee have received answers from parts of Pennsylvania, Vermont, New Hampshire, Maryland, the District of Columbia, Virginia, North Carolina, Georgia, Florida, Mississippi, Louisiana, Tennessee, Missouri, Indiana, Ohio and California.

It appears that but little State Legislation has taken place in regard to the sale of poisons. In Ohio an Act somewhat similar to the English 'Arsenic Act' was passed soon after the latter, requiring the poison to be mixed with soot or indigo before being retailed, yet but little regard is had to it in practice.

[The following is a copy of the Ohio law, taken from the Cincinnati Report.

Sec. 1. *Be it enacted by the General Assembly of the State of Ohio*, That it shall not hereafter be lawful for any apothecary, druggist, or other person in this State, to sell or give away any article belonging to the class of medicines, usually denominated poisons, except in compliance with the restrictions contained in this act.

Sec. 2. That every apothecary, druggist, or other person who shall sell or give away, except upon the prescription of a physician, any article or articles of medicine belonging to the class usually known as poisons, shall be required:

1st. To register, in a book kept for the purpose, the name, age, sex, and color of the person obtaining such poison.

2d. The quantity sold.

3d. The purpose for which it is required.

4th. The day and date on which it was obtained.

5th The name and place of abode of the person for whom the article is intended.

6th. To carefully mark the word 'poison' upon the label or wrapper of each package.

7th. To neither sell nor give away any article of poison to minors of either sex.

Sec. 3. That no apothecary, druggist, or other person, shall be permitted to sell or give away any quantity of arsenic less than one pound, without first mixing either soot or indigo therewith, in proportion of one ounce of soot or half an ounce of indigo, to the pound of arsenic.

Sec. 4. That any person offending against the provisions of this act, shall be deemed guilty of a misdemeanor, and, upon conviction thereof, shall be fined in any sum not less than twenty, nor more than two hundred dollars, at the discretion of any court of competent jurisdiction.

Sec. 5. This act to take effect and be in force, from and after its passage.—*Exec. Comm.*]

In New Hampshire, a State law exists requiring 'Every apothecary, druggist, or *other person*, who shall sell any arsenic, corrosive sublimate, nux vomica, strychnia, or prussic acid, shall make a record of such sale, in a book kept

for that purpose, specifying the kind and quantity of the article sold, and the time when, and the name of the person to whom such a sale is made, which record shall be open to all persons who may wish to examine the same.'

The other sections exempt physicians' prescriptions, and provide a penalty of $100, for the violation of the first section. Mr. Edward H. Parker, of Concord, N.H., in giving this information states, that the law is almost, if not entirely, ineffectual, and that not more than *one* in five of the druggists pretend to keep such a record, and some are not even aware of its existence. The effect has been to confine the sale of poisons to the druggist, as 'grocers and shopkeepers rarely, if ever, retail arsenic or other poisons specified in this law.'

[The following law of the State of New York is derived from the report of the N. Y. College of Pharmacy, on statistics. 'Every apothecary, druggist, or other persons, who shall sell or deliver any arsenic, corrossive sublimate prussic acid, or any other substance or liquid usually denominated *Poisonous* without having the word poison written or printed upon a label attached to the phial, box or parcel in which the same is sold; or who shall sell and deliver any tartar emetic, without having the true name thereof written or printed upon a label attached to the phial, box or parcel containing the same, shall upon conviction be adjudged guilty of a misdemeanor, and shall be punished by a fine not exceeding one hundred dollars.' *Exec. Comm.*]

Through Mr. Peck, of Bennington, it appears that no law bearing on the sale of poisons, exists in Vermont. The regular druggists take generally the proper precautions, but at nearly all of the little stores in the villages throughout the state, arsenic, opium, and even *strychnia* are sold without being labelled.

In the large cities, the better class of druggists and apothecaries are exceedingly careful in the sale of all poisons; many refuse to sell arsenic at all except in medicine, and strychnia and poisons of that kind are refused without a prescription, except in special cases, where the applicant is well known and the purpose obvious to the vendor. Yet it cannot be denied that many others, while careful to label poisons, are not sufficiently discriminative in their sale. It has become usual in many places to employ corrosive sublimate as a *bug poison*. Many druggists require that the purchaser shall bring a bottle that can be properly labelled—some are willing to sell the poison in substance, and risk its subsequent appropriate use. In view of the abundant employment of this poison in families, often put in the hands of servants—it is surprising that so few accidents occur with it.

Our inquiries from correspondents in the South and South West, exhibit that no State law exist restricting the sale of poisons, to persons not suspicious yet there is an universal practice of refusing arsenic and other poisons to the black population, unless they bring a satisfactory order from employers or owners. In middle Florida, 'opium, morphia, strychnia, nux vomica and arsenic' can be procured from the general storekeepers without difficulty, and the practice of keeping poisons for destroying vermin and animals, by the country storekeeper, is very common throughout the whole Western, Southern and Middle States Perhaps the chief demand for arsenic from country stores is by by farmers and millers as a *ratsbane*. The tastelessness and effective

ness of this poison renders it superior to all others for this purpose, and hence its employment in mills and barns, where it has too often occasioned accidents to horses and to poultry; to the latter from the sweepings of the mill floor, sold commonly for the purpose of feeding such stock. Several valuable horses were destroyed in this way near Blandensburg, Maryland, a few months since.

Our correspondent at Sacramento, California, Mr. G. L. Simmons, states, "that large quantities of all kinds of poisons are sold by grocers as well as by druggists. The larger part is used for the destruction of animal life. The regular sales of *strychnia* must be immense. Its more speedy effects than most other poisons is liked by the "Ranche-men," who are the principal customers. Arsenic, is also sold, but chiefly as *ratsbane*. Since the year 1849, no case of poisoning by arsenic has come to my knowledge."

We have reason to believe that a large proportion of the strychnia made, is used to poison wolves and other carnivorous wild animals in the newly settled territories where the population is sparse. It is generally admitted, that the sale of arsenic by druggists in the Atlantic cities has increased many fold within ten years. The quantity is vastly too great for use as a poison, and we believe the demand is chiefly attributable to the requirements of new branches of manufactures, and, perhaps, by wool growers. In reference to strychnia, the increased use of which is directly attributable to the pioneers and hunters of our extensive and rapidly peopling territories, both for the purposes of the fur trade, and protection from the dangerous carnivora, it may be stated that from the best data the committee can arrive at, between 5,000 and 6,000 ounces are manufactured annually in this country, from about 120,000 pounds of nux vomica, besides what is imported; and that one manufacturer of Philadelphia, in the year ending June 3, made 1840 ounces from about 40,000 lbs. of that drug.

As regards cases of suicide, the poison most frequently chosen is laudanum or opium, not only because it can be readily obtained without suspicion, but because the suffering is avoided. The immense increase in the consumption of opium and its preparations, is a subject that deeply concerns the well wisher of society. Their substitution for alcoholic liquids is but too frequent. The Committee have not entered into this branch of their inquiry, however, and have not obtained any facts to communicate.

In making any suggestions with a view to remedying the evils appertaining to the trade in poisons, the committee feel the necessity of keeping in sight the habits and peculiarities of the people who are consumers, and do not believe that the stringent measures adopted in Europe are calculated to work well in this country. The absolute free trade which now exists, and its general use as a *ratsbane*, exhibits a remarkable carefulness in the use of arsenic, in so far as fatal accidents are concerned. We believe that by far the larger portion of apothecaries and retail druggists are careful in labelling this poison, and observe some discrimination in its sale. As a class, they are better fitted by their knowledge and judgement to guard against mal-uses then general dealers. We are, therefore, prepared to recommend to the Association that the State Legislatures, who have not enacted laws on this subject, be petitioned to

pass laws in their several jurisdictions, confining the sale of arsenic, corrosive sublimate, opium, strychnia and other poisons popularly known as such, for destroying life, to druggists, apothecaries and physicians, who shall keep an accurate record of such sales. That such sales shall not be made to minors or servants, unless properly authorized by a responsible person ; that all packages or bottles shall be distinctly labelled with the name of the poison, and the word "poison ! ! " or a death's head symbol, conspicuously printed, and that any sale of poisons followed by accident, in which these precautions shall not have been observed, be considered a misdemeanour punishable by legal process,

And to further recommend that the druggists and apothecaries of the United States, do voluntarily adopt a system of precautions in the sale of poisons, both for their own sake and that of the community, in view of the probable non-action of the legislative bodies.

They believe that the community can be needfully supplied, even in the rural district, from their physician, who necessarily keeps medicines, or from the apothecary in the nearest town, and a check would thus be given to the facility of obtaining the poison, by the consequences of neglecting the legal precautions.

William Procter, Jr.,
S R. Philbrick,
Alex. Duval,
Geo. D. Coggeshall,
Committee.

In reply to the question : "how many Apothecaries and Druggists are there in each of the principle towns and cities of the United States," propounded by the American Pharmaceutical Association, the Cincinnati College of Pharmacy replies as follows :

In Ohio, the whole number is 534. In the following cities and towns, as follows : Cincinnati, 91 ; Cleaveland, 16 ; Chilicothe, 5 ; Columbus, 7 ; Dayton, 10 ; Zanesville, 4 ; Steubenville, 4.

2d. What organization exists in the several States, and what is the number of their members as compared with the number of Druggists and Apothecaries in the localities which they include ?

There is, as far as we are informed, only one regularly organized Association of Pharmaceutists in the State, the Cincinnati College of Pharmacy, which has thirty members, or one in three of the Druggists and Apothecaries of Cincinnati.

3d. How far is the business of dispensing medicines separated from the office of prescribing ?

Almost universally in the city of Cincinnati, and in the State, but to a limited extent.

4th. Have you any information in regard to the practice of our art, and the professional character of its practitioners in different localities, likely to be of advantage to the Association in promoting the objects in view ?

In the practice of our art there is evinced a disposition by our druggists to

supply themselves with a better quality of medicines generally. And in the preparations by some, a more strict adherence to the formula of the United States Pharmacopœia, with a use of proper weight, than was the practice a few years ago. Still, we regret to say that our national standard is not as closely adhered to as it should be.

We would also state that there has been brought to our market a number of sophisticated and spurious articles. A few have come under our own observation, which we shall mention, viz: glauber salts, crystallized in small crystals, resembling those of Epsom salts, and sold as English Epsom salts; sulph. lime, precipitated carbonate of lime; opium, containing 15 to 20 per cent. of pebbles, shot, &c., some of which weighed from one to two drachms; iodide of potass containing from 15 to 20 per cent. of impurities; also, pulv. cream tartar, largely adulterated with alum, sulph. potass, &c. The past year, large quantities of the exfoliated bark of the platanus Occidentalis (false sycamore) have been shipped from this port, probably for adulterating other more costly medicines, A better article could not well be selected, it being perfectly inert, inodorous, tasteless and cheap.

5th. Are there any State laws for the protection of the interests of the profession of pharmacy, for the suppression of empyricism, or in reference to the sale of Poisons?

We have no State laws governing in the least the vending of patent or secret medicines, or for the suppression of empyricism. In the winter of 1851–52, a law was enacted by the State Legislature, to prevent the promiscuous sale of poisons, a copy of which is herewith submitted. [See page 10] As far as we are aware, it has been inoperative, owing to the provisions of the law being to general.

Ww. B. Chapman, *President.*

A. M. Stephens, *Recording Secretary.*

The New York College of Pharmacy, replies to the same queries in the language below:

The committee appointed by the College of Pharmacy, of the City of New York, to aid the Executive Committee, of the American Pharmaceutical Association, in obtaining statistics of the state of Pharmacy, in the United States, in accordance with a resolution passed by the National Pharmaceutical Convention, at a Meeting held in Philadelphia, Oct. 6, 1852, beg leave to submit the following answers, to the questions there proposed, as applied to the City of New York.

1st. According to the canvass of the City, in May, of the present year, for the publication, of the business directory, the city contains 273 apothecaries, 51 wholesale drug houses, and 29 analytical and manufacturing chemists, making about 353, engaged in the manufacture and sale of medicines.

2nd. There are two Pharmaceutical Associations at present existing in this city. The College of Pharmacy, and a German Society, of which the committee have not received any particulars.

At a meeting of the druggists and apothecaries, of the City of New York, held on the 18th March, 1829, a draft of a constitution was presented by a committee, who were appointed for that purpose at a preliminary meeting, for the organization of a society to promote pharmaceutical science. The proposed Constitution was approved, and at a subsequent meeting held on the 25th of the same month, it was signed by upwards of 30 members ; and the first officers of the College of Pharmacy of the City of New York were then elected. The Constitution provided that, 'any person now engaged in business as a druggist or apothecary in the city and county of New York, may become a member of this Institution, at or before the stated meeting in June next, by signing the Constitution, &c.,' also that 'no person hereafter engaging in such business shall be admitted as a member, unless he has been regularly educated as a druggist or apothecary, &c.'

In 1831 the College was incorporated by an act of the Legislature.

The College, with the view of providing for the education of future apothecaries, established, in 1829, a school of Pharmacy, and appointed professors of chemistry, and materia medica and pharmacy ; the school continues in successful operation. Lectures have been delivered annually, to classes averaging from 25 to 30 students each, and about 60 candidates, after fulfilling the requisitions of the College (see article 4, section 1, By-laws) have received its diploma. In 1850 botanical lectures were introduced with decided benefit to the pupils.

Alterations have been made from time to time, affecting the eligibility to membership in the College.

In consideration of the number of highly intelligent and respectable druggists and apothecaries emigrating to this city from places where Schools of Pharmacy have not been established, the College in 1849 deemed it expedient to open its doors to their admission, by amending its By-Laws, (see Art. III, Sec I, of the By-Laws.) Experience has confirmed the policy of the change ; members elected under it, who otherwise would have been excluded, have rendered the College valuable service and counsel.

The College at present enrolls 53 members, bearing the proportion of about 16 per cent. to the whole number of druggists, apothecaries and chemists, in this city. New York is strictly a commercial city, and it is difficult to engage the attention of many who cannot calculate the result directly in dollars and cents ; but under all circumstances, the College may be considered in a prosperous condition.

In 1852 a monthly Journal was established by authority and under the supervision of the College, for the promulgation of pharmaceutical knowledge ; its first number was issued in January of last year; the encouragement for its continuance increases.

3d. The practice of dispensing and prescribing medicines are considered very distinct branches, and are generally so conducted ; there are, however, many small shops in retired parts of the city, resorted to by the poorer classes, in which the physician and apothecary are identical. They are usually

held by young physicians of slender means, as a support and a stepping stone to practice, until they can live without them, then as heir-looms they are handed down to the more needy junior, and thus transmitted from generation to generation.

4th. In this, as in all large cities, every profession presents a great diversity of characters, the particulars of which the Committee do not consider to be of any practical value to the Association."

The American Parmaceutical Society, as now organized, is destined to achieve much good, for our common profession and the public welfare. For proof of this, we refer to the preceding extracts from the proceedings of the recent meeting of the Association. We shall not fail to lay before our readers, the valuable labors of this eminently useful body of scientific men. We hope they will not tire in well doing, but push forward in the track already marked out for the advancement of human knowledge and the best interests of humanity.

V.—*Dr. Armor's Prize Essay, read before the Ohio State Medical Society, at Dayton.*

The text which Dr. Armor has chosen to place at the head of this Essay, might give the reader the impression that he was about to be favored with an erratic discourse. Something so "liberal" in profession, that in the merging of "sects" and the heterogenious collection of scattered opinions, it were difficult to present sound doctrine and forcible argument; such opinion, however, would be doing vast injustice to the essayist, who, in aiming at a "prize," is entitled to the *quantum meruit* for his labor.

But to the text: "I profess a liberal medicine: I am neither of the old sects nor new, but follow wherever they cultivate truth."

Taking this, the maxim of Klenius, the writer sets forth upon his pathological inquiries into the changed conditions of the blood, but the subject being too vast to be intelligibly handled in an essay, the matter for consideration is reduced to the "Zymotic theory of essential fevers and (some) other disordered condition of the blood; together with an appendix on medical theories and vital statistics." In directing attention to the admixture of foreign matters in idiopathic forms of fever, Dr. Armor says:

"In the Essential or Idiopathic forms of fever, it is evident that change has been induced in the blood by the admixture of *foreign matters.* The proof of this consist in the fact; 1st. That diseases analogous to those fevers have been induced by injecting putrid matter into the veins of animals; 2d. These fevers are readily produced by the introduction of animal poison into the blood, as in the case of small-pox, measles etc; 3d. These poisons are known to operate through the medium of the air, by thus gaining access to the blood through the lungs; 4th. The non-contagious fevers, such as Intermittents and Remittents, are universally admitted to depend upon a poisoned or changed condition of the atmosphere; 5th. Actual observation establishes the fact that the blood *is* altered in all Essential or Idiopathic fevers."

From the foregoing statement, the Doctor does not seek to establish the principle of the *identity* of fevers, and gives as his *rationale* of the chemical, physical and vital changes which take place in the properties of the blood, upon the introduction of foreign matters, the different action of different poisons upon the human constitution.

"Urea," says he, "and its compounds, if retained in the blood, affect the brain and nervous system, and are apt to give rise to a low grade of inflammation in serous and sero-fibrous tissues; while mucous structures will suffer but little. But the small-pox virus spends its force upon mucous and cutaneous structures, and leaves, unharmed, the serous and fibrous structures.

All Essential fevers should be regarded as distinct in species, *according to the circumstance of the primary sedative impression.* This is the only true and rational classification of fevers."

The doctrine assumed there is, that each specific measure has a peculiar and distinct law of development; upon the essential nature of these the author does not design to speculate. Mercury, if uncontrolled in its action, will excite inflammation of the salivary glands; arsenic will act upon mucous structures, and ergot upon the uterus; whilst the poison of small-pox may spend its virulence upon dermoid structures; and certain fevers, consequent upon miasmatic poisons may result in the destruction of intestinal glands.

The question is asked in reference to the Zymotic theory of fevers, "is there any evidence to show, that the introduction of putrid matter into the animal system, does give rise to effects which are at all comparable with those of fever?" The question is answered by Leibig, Magendie and Armstrong, in the following manner:

"'That subjects in anatomical theatres frequently pass into a state of decomposition, which is communicated to the blood of the living body.' And the fact observed by Magendie, that putrefying blood, brain, eggs, etc., laid on recent wounds, cause vomiting, lassitude and death, after a longer and shorter

interval, has never, as yet, been contradicted. Numerous experiments have demonstrated that putrid matter injected into the blood of healthy animals, will give rise to a set of symptoms which are very analagous to Typhus. 'If a small portion of putrid matter,' says Dr. Armstrong, 'be accidentally introduced into the blood during dissection, or if the experiment be made upon the lower animals, it produces fever, having exactly the characters of Typhus under its continued form, and *no individual could confidently pronounce that it differed from it.*"

And, again, we quote the words of Leibig:

"A universal observation that the origin of epidemic diseases is often to be traced to the putrefaction of large quantities of animal and vegetable matters; that miasmatic diseases are endemic in places where the decomposition of organic matter is constantly taking place, as in marshy and moist localities, that they are developed epidemically under the same circumstances after inundations; also in places where a large number of people are crowded together with insufficient ventilation, as in ships, prisons, and beseiged places."

In further illustration of this point under consideration, the experiments of Henry Lee, Esq., "On the obliteration of varicose veins, and the sources of danger involved in that operation," as given in the *Med. Times and Gazette*, for January, 1853, might be adduced. The chief source of danger in this operation is the introduction into the general circulation of the decomposed fibrine of the blood, or some purulent secretion, which may be introduced into it; symptoms of typhoid fever having set in, and in some instances proving fatal in two or three days, as was the case about fifty years ago, in a private patient of Sir E. Home, after having performed the operation of tying the saphena vein.

Mr. Lee injected into the jugular vein of a donkey, one ounce of putrid fibrine of the blood, with the same quantity of water, and the animal, after four hours extreme suffering, died.

Severe symptoms and death have been induced in animals by injecting into their veins, with the blood of persons laboring under small-pox.

In the above cases we have instances of extrinsic causes of disease, deliterious substances acting injuriously upon the blood, rendering it unfit for respiration, and death ensuing as a consequence; and to which Dr. Armor has given the name of factitious fevers. There are many poisons, acting intrinsically, which may be carried off by the most important emctories, the kidneys; thus, by the experiments of Orfield we find, "that the pernicious effects of small and repeated doses of arsenic could be readily averted in animals, by giving them, at the same time, a diuretic medicine."

The altered condition of the blood is made manifest, says Dr. Armor, by general febrile phenomena, and he regards it as an important truth, that in all essential or idiopathic fevers, changes of the solids depend on previous alterations (quantitive and qualitative) of the blood, and gives his assent to the doctrine "that nature is ever active in her recuperative powers, and is, after all, the best and wisest of physicians."

Some opinions upon medical theories and vital statistics, under which last head the lengthened term of human life within the last half century, as shown by the report of the Registrar General of England, and the statistics of America, are set forth for the consideration of the reader—and rejoicing in this prolongation, the author eloquently concludes his essay in the following language:

"Is it not true, then, that medicine is the first of the progressive arts; and not first only, but incomparably above and beyond all others in the priceless benefits it has bestowed on man? Yet who has risen up to give it public thanks for its herculean labors? Who has proposed to commemorate the vast achievement of prolonging the years of the life of man more than one-fourth their former average, throughout civilized Europe and America, in the short period of half a century?

When a great canal or railroad is completed, the air is rent with clamors. Men's voices are inadequate to express their joy, and cannons thunder forth their glad congratulations. Orators speak of 'the marriage of mighty waters;' and men, as they meet in the street, say, the great work is accomplished. Well, is it not better thus?—for what celebration can adequately commemorate these triumphs of medicine! What monument can typify their greatness? Yet we have a right to demand a fair estimate of the value of our profession to society, and an honest acknowledgment of what it has done for the well being of man. Grant us this, and, by the blessing of God, we will raise our own monument; it shall be the armies of living men our hands can rescue from the grave."

G. T. B.

October 14*th*, 1853.

Part Fourth.

MISCELLANEOUS MEDICAL INTELLIGENCE.

I.—*Yellow Fever, in Mississippi.*

RODNEY, MI., Sept. 29, 1853.

Dr. Hester:

DEAR SIR: I regret to have to say that yellow fever has made its appearance on several plantations in this neighborhood, and is undoubtedly spreading by contagion. I trust that the experience of this year will convince you of the portability and infectious nature of this disease. I was strongly impressed with this belief from my observation of the disease in 1843, and what I saw in 1847, served but to confirm me in this opinion. Sufficient evidence, I think, can be advanced this season to establish the fact beyond a reasonable doubt. I will, as soon as I have leisure, furnish you with the results of my observation of the disease here. Yellow fever seems to be characterized by a greater amount of gastric irritation in the cases I have had than formerly, and in many instances, presents in the onset appearances not usually met with earlier than the third or fourth day, as the yellow appearance of the eyes and complexion, which I am disposed to attribute to an altered condition of the serum of the blood.

The organic nervous system seems to bear the brunt of the disease, which consists, essentially, in cerebro-spinal irritation. In bad cases there is a lesion of the blood, which loses its plasticity, in consequence of which the solids become affected, the vessels which open upon the mucous surface and skin become patulous, and hemorrhage results as a consequence of this condition of things.

I consider it a disease prone to run a definite course, requiring six days to

pass through its phases. These are some of the conclusions I have come to respecting it; but have not time at present to furnish you the data upon which my opinions are founded.

I am, respectfully, yours, etc.,

WM. G. WILLIAMS, M.D.

II.—*Subnitrate of Bismuth in Cholera Infantum.*

The following treatment of *Cholera Infantum*, has been found very successful, by some of the French physicians:

Fomentations or warm cataplasms to abdomen; hot applications to the extremities; gum water and this mixture:

℞	Subnitrate of Bismuth,	1 part.	
	Gum Dragon,	1 "	
	Lettuce Water,	120 "	
	Simple Syrup,	30 "	M.

Dose.—Half a table-spoonful every half hour. This simple mixture acts with decided promptness; suspends the vomiting; changes the color of the stools, from a thin whitish appearance to a dark bilious hue; reduces excitement, and allays all dangerous symptoms.

(*Ed. N. O. Med. Jour.*)

III.—*A Doctor's Charity.*

The famous Villeneuve, a renowned member of the Academy of Medicine, of Paris, died recently, and left instructions in his will, to the effect, that his funeral should be conducted without pomp, and as cheap as possible, and that his body should be transported to the cemetery in a common hearse, and there interred in a poor man's vault. He did not stop here; but, likewise ordered his executor to distribute among the poor of his parish, the amount of money which would have been requisite to have given him a splendid funeral cortege, such as is due a member of the Academy.

IV.—"*Inhumanity of Physicians.*"

"Specimens have been given, in some of the papers, of the unfeeling manner in which patients have been treated at New Orleans, in the midst of the present pestilence, by their medical attendants. If the vulgarity, profanity, and want of sympathy, attributed to some of them, is true, they deserve the infamous

notoriety that is likely to accompany them in after life. There is no apology for a physician being ill-bred, ungentlemanly or unkind, in this age of Christian refinement and morality."

[*Boston Medical and Surgical Journal, Oct.*. 1853.

We are not a little astonished to find the above paragraph seriously put forth in our Boston cotemporary. Surely the herculian labor (more than three-fourths of which were gratuitous) performed by the medical men of New Orleans, in the late epidemic, should screen them from the idle attacks of a newspaper scribbler, who, for some personal pique, ventured to traduce one of the profession, but who never had the courage to mention names. If such silly attacks, which are dispised and treated with contempt in this city, upon the profession, are to be received and believed, we are indeed in a sad plight, and deserve the commiseration of the profession.

From the medical men of this city, as a body, patients, whether rich or poor, received nothing but kindness, unremitting attention and sympathy. Our Boston cotemporary seems much better acquainted with the profanity, not to say inhumanity, of the physicians of New Orleans, than we, who have resided 14 or 15 years among them; and we have for the first time heard them gravely charged with "vulgarity, profanity," etc.

We venture to say, our Boston cotemporary might learn to judge more justly of the profession in New Orleans, were he to follow them in their god-like labors through an epidemic.

When we tell our cotemporary, that the profession of this city, treated about 20,000 *poor* persons, sick of the epidemic, through which we have just past—surely, he will be a little more merciful to us, who have sinned rather in forgetting our own interest, and attending to the health and welfare of those who do not even thank us for sympathy.

We repeat, we are surprized and mortified to see a respectable Medical Journal, give publicity to such unfounded and ill-tempered charges of a newspaper paragraphist. We deserve a better fate than this, after the risk—the toils, and the anxiety through which we have passed.

(*Ed. N. O. Med. Journal.*)

V.—*Prospectus of the Georgia Journal of Medical Sciences.*

Uuder this title, a new Monthly Medical Journal is soon to be published, at Savannah, Georgia. As soon as issued and received, we shall take pleasure in calling the attention of the profession to its pages.

VI.—*The Peninsular Journal of Medicine and the Collateral Sciences*

Is a new Monthly Periodical, published in Michigan, by E. Andrews, M.D. It is respectable, both in appearance and matter.

VII.—*Conservatism in Surgery.*

A sensible writer, in a late number of the *New York Medical Gazette*, throws out some just criticisms upon the mainia which now prevails in this country, for surgical operations. In concluding his strictures upon this subject, he observes—

"The following *surgical rules* are deduced from the principles of the science, as held and taught by every standard authority, and are habitually violated in certain hospitals, and sometimes in private practice, by men ambitious to be mere operators, viz:

1st. No operation by possibility endangering life, is ever to be performed for the removal of a mere deformity or inconvenience.

2d. Amputation of a limb, except when necessary for the preservation of life, is unjustifiable on the part of the surgeon, even though the patient may presumptiously be willing to incur the risk. And so of any other hazardous operation.

3d. No dangerous or capital operation should ever be undertaken when there is a reasonable doubt whether the patient may not die on the table; nor unless there is a strong probability that it will prolong life, if he survive the operation itself.

4th. If the patient is dying from an incurable malady, or constitutional disease, no operation is justifiable, lest it hasten the fatal result.

5th. If the removal of a cancer ot tumor will not give rational promise that the patient will live longer with, than without the operation; or if the deformity will be greater after the operation, if successful; then no operation is justifiable.

6th. Where there is a probability that the use of the knife may hasten the development of the same disease in other organ of the body, as in malignant maladies of the constitution, to perform any operation is highly criminal.

These and other similar axioms are founded on the professional and moral obligations of every surgeon, and are hence inculcated among the principles of surgery by the best and highest authorities recognized in Europe and America. And yet, we see them every day disregarded and practically condemned by men calling themselves surgeons, who disgrace both the science and the art at the expense of human life, and this too often by 'deeds of blood,' 'enough to make the cheek of darkness pale.'

In a future article we may cite the surgical authorities whence these axioms derive their sanction, but for the present it must suffice to allude to a few examples, to which we could add a multitude, occcurring within our own observation, of the unwarrantable and criminal recklessness of human life, evinced by these mere operators, for surgeons they are not. As principles, not men, are aimed at, no designation of those concerned will be made.

1st. *Amputation of limbs*, involving danger to life, and many of them fatal, have been performed for the removal of mere inconvenience and deformity, viz: for *club foot*, *crooked legs*, *deformed hanas*, and even for limbs disabled by *paralysis*, etc. The pretext for such surgery, and the defence when it was fatal, was that the patient took the responsibility by consenting.

2d. Operations for removal of ovarian and other tumors, reducible hernia, etc., have been performed upon persons in good health, and who might have lived to old age if let alone, and often with immediate fatal results; when in some cases, it was discovered after death, that the patient was not suffering under the disease for which the operation had been performed, a mistake having been made in diagnosis.

3d. Operations have been frequently performed with fatal result, when the patients were at the time in articulo mortis, from tubercular consumption, old

age, etc., merely for the sake of showing the operation, and the eclat of its performance.

4th. Amputations or excisions have been performed, or attempted for necrosed bones and sore legs, even when syphilis was the cause, and this when no joint was involved, and when the probabilities of cure by medical treatment was so great, that but for the morbid anxiety to cut, no operation would have been thought of. Such operations have been repeatedly fatal.

5th. Similar operations have been performed upon cancers, fungus hœmatodes, and other malignant diseases, with the result of immediately aggravating the vice of the constitution, and hastening death, even after experienced surgeons had predicted this result from any operation."

VIII.—*Prize Essays of the American Medical Association.*

At a meeting of the Association, held at New York, in May, 1853, the undersigned were appointed a Committee to receive voluntary communications on medical subjects, and to award two prizes of $100 each, to the authors of the best two essays.

Each communication must be accompanied by a sealed packet containing the name of the author, which will be opened only in the case of the successful competitors. Unsuccessful communications will be returned on application, after June 1st, 1854.

Communications must be addressed, post-paid, to the Chairman of the Committee, Dr. Charles A. Pope, 123 Locust-street, St. Louis, Mo., on or before the 30th of March, 1854.

Charles A. Pope, M. D., Thomas Rayburn, M. D., John S. Moore, M. D., John B. Johnson, M. D., A. Litton, M. D., *Committee.*

CHARLES A. POPE, M. D., *Chairman.*

☞ The medical press of the United States are requested to copy the above notice. [*New York Medical Gazette.*

IX.—*Dysentery and its treatment—Lead Poisoning and its treatment—Paralysis and its treatment, in the New York Hospital.*—*N. Y. Times.*

During the six weeks, from the first of August to the middle of September, thirty cases of dysentery in different stages and of different degrees of severity, and some neglected before applying for admission, have been received into the hospital, or have originated in the house, in patients ill there from other diseases; and of this number 17 have been discharged cured, 2 have left the hospital, and one has died; 10 still remaining under treatment.

The case which proved fatal, occurred in a patient convalescing from typhus fever, who was attacked while in the house, and who neglected to report his condition to the house physician for two or three days, and very soon sank down into a state of collapse and died. The plan of treatment most usually pursued was as follows: when, on admission, the discharges were either bloody, or a mixture of blood and mucus, or shreddy, and there was an absence of fœcal matter, three grains of calomel and one grain of opium were given, to be

repeated in three hours, followed in three hours more by a dose of castor oil. This generally brought away fœcal stools. Opium was then directed in doses of one grain every two, four, or six hours, according to the frequency of the discharges and the severity of the pain. If fœcal matter ceased to appear in the stools for twenty-four hours, the pills and oil were repeated in the same way. Acetate of lead was given in a very few cases only, and in moderate doses; and, in a few instances only, opiate injections, prepared usually with sulphate of morphine; when the stools were fœcal on admission, opium alone was administered. These means, with a carefully regulated, farinaceous diet, rest, etc., have sufficed to bring about the fortunate result above mentioned, and in a very fair average period of time. A tendency to sink into collapse, indicated by sudden prostration, cold extremities, shriveled skin, contracted countenance, etc., attended at time by rice-water dejections and vomiting, has been a not unfrequent occurrence, and has rendered it necessary to watch the cases very closely, and to meet such cases promptly and actively, by means of the alcoholic vapor bath, sinapisms, stimulants, etc.

The number of cases of poisoning by lead, received at the hospital during the past season, has been quite large. Some of them have been traced to the influence of lead-paint as a cause, either in painters or in those who have remained in rooms newly painted; but, in many cases, the mode in which the poison was introduced into the system could not be detected. In cases of constipation, strychnia in doses of one-sixteenth of a grain, three times a day, has been given, and continued until the bowels were opened, or until the specific effects of the drug were produced. Other means have been used to assist its action; and evacuations from the bowels have been produced in some cases by croton oil, and, in other cases, by turpentine enemeta, when croton oil has failed. The strychnia is thought to obviate the tendency to costiveness which follows the use of drastic cathartics. One patient was permanently cured of the constipation by strychnia alone. The blue line about the gum has been recognized in every instance; and in one case there was a patch of deep-blue color, of the size of a shilling-piece, on the membrane lining the lower lip. All the cases of this disease, after the urgent symptoms are relieved, are now treated with the iodide of potassium, in doses of five grains three times a day, gradually increasing to ten grains, according to the plan proposed by M. Melsens, the results of whose researches have been repeatedly verified by finding lead in the urine after the exhibition of this remedy, when none could be detected in the urine of the same individual before taking it.

We were struck with the number of cases of different forms of paralysis in the wards, exclusive of those produced by lead. One case of paraplegia, the first symptoms of which were felt on the 3d of July last, is improving under the use of acetate of strychnine, in doses of one-fiftieth of a grain three times a day, as proposed by Dr. Marshall Hall. Several other cases of paralysis are also under the use of this form of strychnine; and it is an interesting fact, that in one case of paraplegia, produced by excessive venereal indulgence, more decided effects were produced by this preparation, even in the minute doses in which it was used, than had followed the exhibition of the sulphate of strychnine in doses of one sixteenth of a grain at the same interval, for 18 or 20 days. In one acute case of paralysis affecting both the upper and lower extremities, in a man 24 years of age, of intemperate habits, produced by exposure to cold, the disease extended to the base of the brain, and produced death.

Two cases of saccharine diabetes are also under treatment, one of four months' standing, and the other of about three. The former patient passed thirteen quarts of urine the first twenty-four hours after his admission, of a specific gravity of 1039, and had all the symptoms of the disease well marked. He is now under the use of the phosphate of iron, in doses of ten grains, three times daily, and a diet of animal food, with a moderate share of bread, and

Dover's powder and warm baths. The other patient is under the use of Dover's powder, with a milk diet. Ice is found the most grateful as well as the most effectual means of allaying the excessive thirst which is known to be connected with this disease. Sugar have been found in the urine of borh, by different tests."

X.—*An Inquiry into some of the relations between Menstruation, Conception, and Lactation; and the influence of lactation in causing Abortion; founded upon an analysis of the histories of one hundred women.*

A valuable paper, in which the above topics were fully discussed, was lately read before the Medical Society of London, by Dr. Robert Barnes, from which the following deduction is drawn:

First. Lactation exercises a considerable influence in preventing menstruation and conception.

Secondly. This influence appears to be marked and constant in some women, and to exist but feebly in others.

Thirdly. The influence of lactation in averting menstruation or conception, cannot for the most part be kept up longer than twelve months.

Fourthly. There is a close relation between the occurence of menstruation during suckling, and conception; viz: when menstruation appears during suckling, conception is very likely to follow.

Fifthly. When pregnancy takes place during suckling, and suckling is continued, abortion is very apt to follow.

Sixthly. The chief causes of the abortions brought about during suckling, are the revolt of the ovaria and uterus, evinced by the return of the menstrual nisus; and the deterioration of the mother's blood; to which must be added, superinduced disease of the ovum.

Seventhly. The practical conclusion that weaning should be enjoined, not only whensoever pregnancy takes place, but also whensoever menstruation returns." [*Lancet.*

XI.—*Physicians of the "Present Age."*

To Norwood, the elegant contributor to that popular monthly, "*The Ladies' Pearl,*" we are indebted for the following beautiful description of the Doctor's mission:

"Macaulay tells us that the common laborer, in our day, who is so unfortunate as to have his skull or his limbs broken, *is more scientifically and skillfully treated* by an ordinary village physician that may be called to dress his wounds, *than the King could have been*, by the best surgeon in the days of Charles II. The fact speaks well for the advancement of medical science during the two centuries past, and every candid, intelligent physician will bear witness to the truth of the statement. The tables of mortality show us, as one of the results of this improvement, (assisted also, doubtless, by better living,) that the average duration of human life has greatly increased in the same period; while the small-pox, that was wont, in former times, to depopulate whole cities and districts, is rendered harmless by vaccination. The Doctor's mission is a noble one. It is to combat disease and subdue it. Guided by the lights of his science, he can expel disease from the humau system, raise the

afflicted from a bed of pain, re-establish the violated laws of health, and plant new roses on the pallid cheek where ghastly death has sought to fix his horrid seal. With what delight do we welcome him when those who are near and dear to us require his aid! With what pleasure do we recollect each word of hope he utters! How anxiously we wait to hear his opinion of the diseases and the remedy. And how delighted we are to see the beloved invalid up again. Affection has no greater joy in store for us. All honor, then, to medical science, which has done so much, and promises still more for suffering humanity; and a double meed of praise to those noble spirits, its worthy ministers, who labor for its advancement, and by their discoveries and skill alleviate the sufferings, prolong the lives, and improve the condition of our race."

Some of the New Orleans *prints* think differently; but as doctors are permitted to disagree, so may editors and writers differ in their estimate of the medical profession. *De gustibus non. Vide, Delta, 20th October.*

(*Ed. N. O. Med. and Sur. Jour.*)

XII.—*Anæsthesia in Midwifery and Fatal Effects of Anæsthetic Agents.*

The undersigned was appointed by the American Medical Association to report on the above mentioned subjects at it next session in St. Louis.

He therefore respectfully urges his medical brethren to make extensive and close observations on anæsthesia in midwifery, and also to analyze carefully all alleged cases of death from the use of anæsthetic agents, and to forward the results to him before February 1st, 1854.

The latter cases must be those only occurring within the present year of the association.

JAMES BOLTON, M.D., *Richmond, Va.*

XIII.—*Treatment of Epidemic Typhoid Fever, in the Parisian Hospitals—Post Mortem appearance—Statistics, etc*

The correspondent of the *Charleston Medical Journal*, writing from Paris, among other interesting statements, has the following, on the treatment, etc., of Typhoid Fever.

(*Ed. N. O. Med. Journal*)

As the subject of typhoid fever has been much discussed of late at the South, and in the pages of this Journal, it may not be thought useless to consider the following autopsy which I witnessed at La Charite, a few days since. It is instructive, as indicating the characteristic march of the disease in an individual in whose system it was not modified by medicines. The subject, a man of about 33 years of age, of good conformation when he entered the hospital, presented every favorable condition save the existence of the endemic; this had seized upon him as it had upon most of those who had within the last year or two moved from the country to Paris, and who did not live under favorable hygienic conditions. He had been bled, but absolutely no medicines were taken, with the exception of *tisans* and mucilaginous drinks. Notwithstanding the entire lining membrane of his *stomach* was intensely red, and exhibited all the traces of severe inflammation; there was some exudation of plastic lymph, and the particles of coloring matter seemed infiltrated beneath the mucous coat; when scraped off with a sharp bistoury, the surface beneath was white.

Thirty of Payer's glands were enlarged and inflamed, decreasing in size from the commencement of the colon, until they disappeared in the small intestines. The largest were about an inch and a quarter in diameter, with elevated edges, presenting, as M. Briquet remarked, precisely the appearance of carcinoma; the surface of those situated most inferiorly exhibited the yellow summit, indicating the commencement of gangrene. No other portion of the intestinal canal was diseased, if we exclude the above enlarged mesenteric glands. The *spleen* was four times the ordinary size, though it did not appear that the deceased had suffered from intermittent fever. The cavities of the *heart* contained some fibrinous deposits, and its muscular tissues were soft. The lungs showed previous inflammation of the bronchia, and the lower lobes were still engorged; these did not compromise the life of the individual, as quite a large portion was spongy and elastic. The consistence of the *brain* was firm, no fluid was found in the ventricles, but there was venous engorgement on its surface. The *liver* and other organs remained quite natural.

Four cases of Asiatic Cholera appeared at Hotel Dieu during the month of January—so said the journals and the physicians! I heard M. Louis observe, a few mornings since, that the diarrhœa or cholorine, which had since existed to a certain extent, had not respected any class of the patients, not excepting those with typhoid fever. Before leaving the subject of this latter affection, which is now on the decline, I append a few statistical items which I have selected and re-arranged from a mass of valuable matter contained in the *Moniteur*. It may afford data for comparative estimates in the United States.

On the first of March there were 1442 sick of the disease, 4344 having been the maximum of those sick in the Hospitals. At the same time the number of the sick, generally, has descended from 6735 to 6618. From the 19th to 27th of February, (9 days, 131 had died of 1253 sick of typhoid in hospitals, or about 1 in 100—a proportion less than has characterized any previous epidemic. The winter has been a very wet one, the Seine having been higher than for many years past, and snow having fallen frequently within the last month.

During the year 1852, there were 29,873 deaths in Paris. 29,664 had the relations of sex, age and disease indicated. Of these 16,220 were males, and 14,444 females. In 1851 there were 29,709 deaths; 15,110 males, 14,699 females. The most fatal period of life was under three months; the least, between six and eight years. Among the 29,644 deaths, maladies of the chest, digestive apparatus, and Typhoid Fever occurred most frequently. Of Pulmonary Pthisis, there died 2073 males, 2038 females; of Pneumonia, 1279 males, 1346 females; of Pulmonary Catarrh, 849 males, 955 females; of Enteritis, 1895 males, 1365 females; of Typhoid Fever, 603 males, 503 females. The temperature was quite elevated during the whole year, though there were few atmospheric variations. Of the 29,873, 10,157 were in hospitals, remainder *a domicile*, a word which may well be retained to mark those sick in the city, but not in the medical establishments. In 1829, when statistical researches commenced to attract greater attention, there were 17,101 deaths, *a domicile*, and 9884 in the hospitals, the population then being 776,241. In 1832, the numbers rose to 30,843 dead *a domicile*, 15,481 in hospitals. This increase is accounted for by the presence of Asiatic Cholera. 1814, the year of the war, was the most fatal, there being 24,197 deaths, the severes epidemic of Typhoid ever experienced in Paris, contributed to swell the number of victims. The most melancholy reminiscences are said even now to attach to this sad epoch, which elicit a great deal of generous sympathy.

Peninsular Journal of Medicine.

XIV.—*On the influence of Noxious Effluvia in the origin and propagation of Epidemic Diseases.*

Mr. Grainger, when on this subject, uses the following :

Influence of Human Effluvia.—According to my own opportunities of observation, the most injurious of all the causes operating in the diffusion of epidemic diseases, are the effluvia proceeding from the human body, and especially from the lungs and the skin. The special deleterious agent consists of the effete and, as it has been proved experimentally, highly putrescent organic matter, mingled with the expired air. That it is, when reintroduced into the living body, liable to be highly injurious, may be inferred from the fact of the careful provision made by nature for its incessant elimination from the system. That it is small in amount, is no objection to the intensity of its action ; for to the physiologist it is well known that a minute quantity of a powerful agent—the putrid matter introduced on the point of a needle in the inspection of a dead body—a single drop of concentrated prussic acid placed in the mouth of an animal—is sufficient to destroy life. It is in overcrowded bedrooms, in unventilated schools, workhouse, dormitories, &c., that this effete matter taints the air, and, entering the blood, poisons the system. That the remarkable diminution in the amount of carbonic acid evolved from the lungs, where persons, as in crowded and unventilated apartments, breathe an impure atmosphere, acts in such cases injuriously, admits of no doubt ; but the evil, *quoad* the development of fever, scarlatina, cholera, &c., depends on the organic, and not on the chemical products of respiration. As one indication of this, it may be explained that it is possible, under certain circumstances, to observe the action of the former when separated from the latter. As soon as the expired air quits the body, the matters of which it consists have a tendency to separate ; and as regards the two substances under consideration, the carbonic acid mixes with the atmosphere on the principle of diffusion ; whilst the animal excretion, no longer held in solution by the colder external air, is deposited, and particularly clings to woollen articles, as bedding and clothes, which last, as it is well known to medical men, clergy, and others, will often retain for hours, or even longer, a foul smell from this cause alone. When this matter, from neglect, is allowed to accumulate, it will affect the health. An instance of this was mentioned to me by the surgeon of a large pauper school, where the health of the boys was decidedly improved by substituting, for the usual dress, clothes capable of being readily washed.

It is, however, familiar to all practitioners, that human effluvia especially exhibit their poisonous influence when either multitudes of human beings are crowded together, or where a smaller number are placed in confined and unventilated sleeping places. Many instances of the influence thus excited on all kinds of epidemic disease have come under my notice ; but only a few illustrative examples can here be adduced. In making these selections, it will be my object to present instances which, as far as possible, display the operation of some one individual agent ; for when, as usually happens among the poor, a multitude of unfavorable conditions are present, it is extremely difficult to define and demonstrate the deleterious agency of each.

Half-Yearly Abstract of the Medical Sciences, 1853.

XV.—*On the predisposing Causes of Epidemics.*

BY DR. CARPENTER, F. R. S.

(Medico-Chir. Rev., Jan. 1853, pp. 175-177.)

The generally-recognized predisposing causes of zymotic diseases, are all reducible to one of three categories.

(1) Those, namely, which tend to introduce into the system decomposing matter that has been generated in some external source;—(2) Those which occasion an increased production of decomposing matter in the system itself;—and (3) Those which obstruct the elimination of the decomposing matter normally or excessively generated within the system, or abnormally introduced into it from without.

1. Under the first head will rank putrescent food, water contaminated by sewerage or other decomposing matter, and air charged with mismatic emanations.

2. Under the second, any unusual source of degeneration of the tissues within the body, such as presents itself in the puerperal state, after severe injuries, or as a consequence of excessive muscular exertion.

3. Under the third, an insufficient supply of air, a high external temperature (which slackens the respiratory process,) and ingestion of alcohol.

We cannot assign a definite place to starvation, until we more clearly know the cause of the retention of the decomposing matter within the body.

Any one of these causes will tend to produce *an accumulation of disintegrating azotized compounds, in a state of change, in the circulating current;* and this is precisely the condition which, on the fermentation-theory, will afford the greatest readiness to the development of any zymotic poison in the system, whatever the specific nature of that poison may be. But as each of these causes will act separately, their potency is vastly augmented when they act concurrently; as when the puerperal patient, whose special liability arises from the source of disintegrating matter within the system, is also subjected to putrescent emanations from some external source, and is pent-up in a heated atmosphere, very insufficiently renewed. Let any poison, capable of engendering puerperal fever, be applied to a female thus circumstanced, and our readers will not have much doubt about the fatality of the result. Or, again, if to the influence of the fatigue and exposure of a long march in a tropical country, be added that of overcrowding in tents or barracks, and on that be superimposed the ingestion of putrescent water, or of alcoholic liquors in excessive amount—and if the individuals, thus primed, be exposed to the poison of cholera, dysentery, or fever,—the experience of our Indian army tells a fearful tale of the consequences of such a combination.

Looking at the question, then, in the first place, as one of experience simply, we believe that no one who duly weighs the evidence, can hesitate to admit the facts on which our superstructure is based. Looking, secondly, to the immediate consequences of each of the causes we have enumerated, we believe that no physiologist can hesitate in admitting the possibility, to say the least, of the *rationale* which we have assigned for every one. And when we find that each of these causes, without a single exception, tends to induce *one and the same condition of blood*, the conclusion seems to us almost irresistible, that this condition of the blood,—in which it is charged with decomposing organic compounds, is that which is the immediate source of its liability to be affected by the introduction of zymotic poisons. All this is a matter of direct *induction;* and the only hypothetical part of the doctrine, is the occurrence of anything like fermentation as a consequence of that agency. Here we consider that our doctrine affords to the fermentation-theory a support of which it stands greatly in need; for if experience demonstrate, that zymotic poisons have little morbific power over blood which is exposed to them in a state of purity, but

immediately exert their malign influence when the blood is unduly charged with organic matter prone to change, the *modus operandi* of the poison acquires so remarkable an additional feature of resemblance to that of "ferments," that the essential correspondence of the two seems to us almost indubitable.

If it be inquired, what is the practical bearing of this discussion, we at once reply that, if our view be correct, it would be possible to extinguish the greater number of epidemic diseases, however intense or abundant may be the atmospheric or other agencies which constitute their potential causes, by preserving the blood of every individual in that state of unfermentibility (if we may coin such a word,) which shall effectually prevent these poisons from finding the conditions of their development within the body; this end being to be attained, on the one hand, by preventing (so far as may be possible) any unusual production of fermentible matter in or out of the body; and on the other, by promoting its removal when it is inevitably generated (as in the puerperal state,) through the respiratory process, which ought to be favored as much as possible, not merely by a free supply of air, but by the reduction of that air to the lowest temperature at which the condition of the patient will allow it to be safely inhaled. We have no new measures to propose; but we do think, that by more clearly specifying than has heretofore been thought possible, the *rationale* of those which experience has demonstrated the value, we may promote their employment, not only with greater positiveness and consistency, on the part of the profession, than has yet been manifested by the majority of them, but also to a far wider extent than that to which they have hitherto been carried out.

Half-Yearly Abstract of the Medical Sciences.

XVI.—*Congenital Contraction of the Intestinal Canal.*

BY S. L. ANDREWS, M. D.,

In a private letter from my friend, Dr. Baldwin, of Lahaina, Sandwich Islands, I have an interesting account of a case of congenital contraction of the intestinal canal. As Dr B. has given me the case more in detail than is needful in your Journal, I have abridged it for your use. The child, a fine-looking, plump female, weight 8¾ lbs., was born Dec. 5th, 1838. The first indication of anything abnormal was the rejection of a little sweetened water, given a few hours after birth. On the following morning castor oil was rejected with bilious vomiting. A judicious use of cathartics, including suppository and enemata, the latter sometimes administered through a gum-elastic catheter introduced several inches into the rectum, failed to produce any adequate evacuation of the bowels. Castor oil and other cathartics, and sometimes enemeta, only excited vomiting, usually bilious. At length, the contents of the intestines, in a very offensive state, were thrown off by vomiting. All that was passed, per anum, was fragments of hardened meconium, shaped to the intestines, and amounting to several inches in length. The last fragment tapered to a point at its upper extremity. Death at the 13th.

Diagnosis, contraction of the intestine, which was confirmed by the autopsy.

The rectum and colon were about half the natural size, or perhaps a little more, except a portion in the middle of the arch, where it was reduced to about half the diameter of that on each side of it. The coecum was natural, but for twelve inches above it the intestine was small indeed, not larger than the narrowest tape, and the canal too narrow to admit anything solid; the

next six inches, proceeding towards the stomach, was very narrow, but contained a few small pieces of hardened meconium. Eighteen inches above this was larger, but crowded with viscid meconium. The remainder of the intestine to the stomach was twice the natural size. The gall-bladder was large and full. The stomach and upper part of the intestine was filled with a liquid appearing like a mixture of bile and milk. The child had nursed until the last day.

The father of the child, an efficient and devoted missionary under the American Board, has disproportionately short limbs, both upper and lower. He is also afflicted with exostosis. A sister is afflicted in the same manner, and some of the children of both brother and sister have the same morbid state of the bones.—*Peninsular Journal of Medicine and the Collateral Sciences.*

XVII—*The actual Cautery in Diseases of the Uterus.*

[The following interesting account of the first surgeon of the Emperor of France, in an important class of diseases, is part of a letter from Paris, written by W. E. Johnston, W. D., and addressed to the editor of the New York Medical Times,]

The most striking feature, however, in M. Jobert's service at Hotel Dieu, is the bold manner in which he attacks the malignant diseases of the uterus. — This is a specialty which he has made his own. Every Monday morning is now almost exclusively devoted to the study of this class of maladies. Besides those confined to beds in the wards, fifteen or twenty out-door patients present themselves every morning, making a large and useful "special" clinic.

A large majority of these cases is composed of malignant diseases of the neck of the uterus. To all such he applies once a week the actual cautery, and the relief which this therapeutic means affords is not only striking, but in its final results astonishing. To M. Jobert the profession owe the revival of this heroic, and, in his hands, successful remedy, in France. Many were the contests which the surgeon of Hotel Dieu had to sustain in its introduction; but he has overcome all opposition by that best of all arguments, the results which follow his treatment.

The number of cases which M. Jobert has now under treatment is about fifty; and in this number one sees every variety of uterine disease, from the simple granulations with slight engorgements, through the various stages of superficial and profound ulceration, terminating the ragged, sloughing, encephaloid disease, with a surface sometimes two inches in diameter. These cases are submitted to the separate inspection of the persons who follow M. Jobert's course, and offer a fine opportunity to become familiar with this class of maladies. The largest proportion of the cases are of the encephaloid character

To a case of simple engorgement of the neck with granulations, and a discharge more or less copious, M. Jobert applies the nitrate of silver, in substance. To superficial ulcerations with a white tenacious discharge and engorgement, what might be called, as regards intensity, the middle variety of disease, he applies the mercurial nitric acid, on a probang of cotton, pure and in small quantity, quickly followed by a small jet of water, lightly thrown on. He dips the probang into the acid, then squeezes it almost dry, and touches the disease but for a moment. He frequently, where the cavity of the neck of the

uterus is diseased, open, and full of mucus, pushes the probang up half an inch, turns it round and draws it out. It will be seen that, altogether, the amount of the medicament applied is very trifling ; but many witnesses can testify to the gradual and uniform relief which this treatment affords. In the more advanced forms of disease, where sloughing has commenced, and the true malignancy of the disease is at once recognized, he invariably uses the hot iron.

The most common form of disease which presents itself, as already mentioned, is the encaphaloid. In some instances he removes the neck with the knife before he commences the cauterizations. Sometimes the ulcer is a saucer-shaped cavity ; sometimes it presents a flat surface covered with a mass of *bourgeons*, and emitting an offensive, sanguineo-purulent discharge ; again it occupies but one lip, which points forward in a huge mass like a fungus ; and again it is found burrowing a deep hole into the substance of the womb enormously ; while the most persistent form of the disease is found in those cases where there is a large, nodulated, ulcerated surface, which constantly bleeds. The patients run from 23 to 60 years of age.

To these patients M. Jobert never gives any medicine. He depends entirely on the power of the hot irons. He orders simply, after all cauterizations, two injections daily of ptisan de guimauve (marsh-mallow.) He uses a large, plain, ivory speculum in nearly every case. Three sizes of irons are used : the smallest is about the size of the little finger, and is used to penetrate the simple saucer-shaped ulcers, yet small, and those which penetrate deeper, like fistula; while the largest size is an inch and a half diameter, two inches long [in the bulb] and slightly oval at the end The length of time he holds the hot iron on the diseased part is from four to eight seconds—scarcely more than a touch. The rules which guide him are, if the womb beyond the diseased surface offers no resistance on pressure with the iron, that is, if it seem not engorged or scirrhused beyond the ulcerated surface, the indication is that the disease is not profound, and he touches but lightly. If, on the other hand, evidence to the contrary exists, he not only holds the iron longer, but pushes firmly upon it, so as to carry the burning surface deeply into the diseased structure. The small iron he uses but once, the larger one sometimes two or three times in quick succession, holding it out long enough to have an injection of cold water thrown into the speculum, so as to cool it off—a very necessary precaution ; for if it be held to long, it will heat the speculum sufficiently to burn the walls of the vagina. These operations are performed every seven or fourteen days, and it is not all uncommon to see cases decidedly carcinomatous yield after the twentieth or thirtieth cauterization. At least they are discharged, with the neck of the womb, or what is left of it, soft and pliable, and with the diseased surface cicatrized, pale, and free from discharge, other than a sweaty exudation of trifling importance. As might be expected, there are many old standing and bad cases in woman of depraved constitutions, who are not cured ; but the relief which the cauterizations afford even these patients, is so marked and so prompt, especially in diminishing the discharge and pain, that it is not uncommon to see them come time after time a long way to the hospital, after they know there is no hope for them, to have the hot iron applied.

A fact which will seem curious to persous who have not seen the operation, is, that the application of the *fer rouge* gives no pain whatever. All the horrors of the operation consist in the sight of the red iron, the hissing noise, the smoke, and the suffocating smell of burning flesh. I have seen M. Jobert apply the iron more than four hundred times, and I have the first time yet to hear a woman cry from the pain of the burning ; while they almost invariably cry at the introduction and withdrawal of the speculum. It must be confessed, however, that this surgeon is non to delicate in the use of the latter instrument.

Why does not the *fer rouge* produce pain ; and why does not the burning pro-

duce inflammation and disagreeable *sequelæ*, as in other parts of the body? — M. Jobert maintains that the neck of the uterus possesses a peculiar arrangement of fibers, which precludes it from carrying the burning influence of a red-hot iron to parts beyond that immediately in contact with the iron, even if applied to a perfectly healthy uterus; and contrary to doctrines taught by some, he believes that the neck of the womb is too feebly supplied with nervous filaments to carry readily the deleterious impression of the iron.

These are the doctrines and this the practice of M. Jobert, in the treatment of the class of diseases spoken of; and he has succeeded beyond the expectations of all his *confreres*. I was informed by this gentleman lately, and I take pleasure in making it known, that he is now, by the aid of M. Gaillard, one of his *internes*, preparing an ample work for publication, on the diseases of the uterus and the results of his peculiar modes of practice. The book will meet a high appreciation in this part of the world.

M. Jobert also applies the hot iron to obstinate chronic diseases in other parts of the body. Recently, he treated an interesting case of salivary fistula in this way. The case was that of a healthy young woman from the country, who, after a severe attack of typhoid fever three years ago, was left with an obstruction of Steno's duct on each side, with fistulous openings just behind the branch of the inferior maxillary and close under the lobe of the ear. All surgeons know the disappointments which frequently attend the treatment of these cases. These disappointments had been verified in this case. M. Jobert thrust the red iron into each fistula, and waited a week without noticing any amelioration. He then resorted to his old practice—compression, with cauterizations of nitrate of silver. These cauterizations were practised twice and three times per week, and the compression was uninterrupted. Under this treatment, the fistulas healed in two months. M. Jobert attributes most of the cure to the latter treatment; but from the sudden destruction of the diseased tissues which surrounded thickly the fistulous orifices, there is no doubt but that the first thorough burning was the principal cause of the hasty cure.

Boston Medical and Surgical Journal.

XVIII.—*On a means of arresting instantaneously Cholera Cramps in the Limbs.*

BY M. GUYON.

(Nouvelle Encyclogr. Oct. 1852.)

In a recent communication to the Académie des Sciences at Paris, M. Guyon states that these spasms may be immediately arrested by firm and steady extension of the cramped muscles. In the case of the leg he directs the heel to be held in one hand and the foot bent towards the shin; in that of the arm the fingers are to be straightened, and the hand carried towards the dorsum of the wrist. [*Abstract of the Medical Sciences.*

XIX.—*Army Medical Board.*

An order from the War Department constitutes an Army Medical Board for the examination of Assistant Surgeons for promotion, and of candidates

for appointment in the medical staff of the army. The Board will convene in the city of New York on the first day of December next, and will probably continue in session three or four weeks. Applications must be addressed to the Secretary of War; must state the age and residence of the applicant, and must be accompanied by respectable testimonials of his possessing the moral and physical qualifications requisite for filling creditably, and for performing ably, the arduous and active duties of an officer of the medical staff.

Medical Examiner.

XX.—*Yale Medical College.*

Drs. Henry Bronson and Benjamin Silliman, Jr., have been appointed successors of Professors E. Ives and Benjamin Silliman, in the Medical Department of Yale College; the former in the Department of Materia Medica, and the latter in that of Chemistry.

Ibid.

XXI.—*Health of Memphis.*

The summers of 1851 and 1852 were characterized by a much larger amount of mortality than has been usual in our general healthy site; this coincidence of two concecutive unhealthy seasons gave rise to gloomy forebodings on the part of some of our citizens and physicians, lest the causes of this mortality might be permanently associated with our locality, and the mortuary returns of these two years be considered as representing the average sanitary condition of Memphis.

The time has at last arrived at which we are enabled to congratulate our goodly city upon retrieving her character for salubrity—that portion of the year having now passed by during which the heaviest mortality of the two last years occurred. That we may not be supposed to be speaking from vague impressions, we give the figures derived from our Sexton's reports, from which the comparative mortality of the three years can be estimated at a glance:

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1851 -	22	26	37	37	70	145	86	61	73	72	38	50
1852 -	38	32	30	35	55	119	104	58	67	64	56	46
1853 -	51	27	36	25	36	33	41	36				

The number of interments for the three years, down to the end of Agust, will be seen from the above statement to have been as follows:

Interments to end of August,	1851	-	-	481
" " "	1852	-	-	471
" " "	1853	-	-	285

But the vast improvement of the present year will be better appreciated if we take the last four months of this period in each year, that being the period during which the mortality was so unduly increased during the former two years.

Deaths from May to August, (inclusive)	1851	-	-	362
" " " " "	1852	-	-	336
" " " " "	1853	-	-	146

(*Memphis Medical Recorder.*)

The New-Orleans Medical and Surgical Journal.

Vol. X.] NEW-ORLEANS, NOVEMBER 1, 1853. [No. 3.

HEALTH, MORTALITY, Etc.

When our September No. went to press, the city was still in the grasp of the epidemic; but soon after that time, it began slowly to decline, and on the 13th of October, the Board of Health unanimously adopted the following resolution:

"*Resolved*, That in the opinion of this Board, the Yellow Fever no longer exists in New Orleans as an epidemic."

Even prior to the adoption and publication of the above, the fever had ceased to be regarded as epidemic by those who watched its progress and gradual decline. Thus, then, it would appear, that the popular and oft-expressed belief, that a frost was absolutely required to put a period to—to arrest and extinguish the epidemic, has been falsified by the events of this season. The epidemic, as such, is at an end—the Board of Health has so declared it; and, yet, there has been no frost—no freeze—and only a few days of cool, north wind.

Nor is this the first season that the epidemic has subsided in advance of a killing frost; it obeys certain laws—laws as fixed and immutable as those which govern the growth, development and decay of organized matter. In the execution of such laws the rise and fall of the thermometer can exert only a limited—a temporary influence—may retard or hasten the march of epidemics. Look to Havana, Vera Cruz, and other localities, where the yellow fever is indigenous, and where the mercury, in the thermometer, never sinks to the freezing point, and yet in these cities, the yellow fever, after having run its course, obeyed the laws which must everywhere control it, dies away; and, finally, disappears in the latter part of summer or the first of autumn, to return with renewed virulence the succeeding spring, run its destined course, and again subside as before. Then let there be no more deferring of hope until frost; let us study and understand the laws which govern our epidemics, if we cannot banish them from our cities.

If this error in the public mind could be eradicated, it would be relieved of much anxiety and apprehension regarding the termination of our epidemic fevers; but the conviction that a frost is requisite to destroy the yellow fever is so firmly fixed in the popular, no less than the professional mind, that we fear no array of facts nor process of reasoning can divert it from this long established belief.

We mistake a coincidence for a consequence; the subsidence of fever and the appearance of frost can have no connection as cause and effect. We have seen that the disease dies away before, and re-appears after, a frost; for we distinctly recollect seeing a case of black vomit in the Charity Hospital, on the 25th of December, of 1847. But we are treading on dangerous ground—we are opposed to the public prints of the day, and the convictions of three-

fourths of our citizens, and we, therefore, leave this subject for future experience and observation to determine.

The epidemic, whatever may be the laws by which it has been governed, revealed certain striking peculiarities, widely different from any witnessed on almost any former occasion. In the first place, it was more fatal and ran its course more rapidly when once fully developed, than in almost any of our previous epidemics. The head symptoms were unusually severe, while the stomach manifested a peculiar irritability, sometimes difficult to control, and often ending in black vomit. Heretofore, suppression of urine was a very common and unfavorable symptom in the latter stages of the disease; this season, however, the kidneys continued, in a majority of cases, to perform their functions up to the last moments of our fatal cases. Whence this difference? Is it not reasonable to suppose that the peculiar poison of the disease expends its force rather upon one organ than another in different epidemics? At one season the brain, at another the stomach, and at a third the kidneys, seem to bear the *onus* respectively of the diseased action. In all seasons and at all times the great nervous centres seem to experience some profound change—some sudden departure from the normal state; hence, the sighing, the sense of oppression and of weight about the great gangliæ; hence the intense neuralgic pains in the head, back, limbs, etc., in the earliest stages of the disease. Again, the large doses of quinine, as far as our experience extends, did not succeed as well in this as in the epidemic of '41 and '47; from this fact, may we not justly deduce another striking proof of an essential difference in the epidemics themselves. Another feature of the epidemic of 1853, stamping it as different from any previous one is the power to propagate itself from one subject to another. In a word, it seemed to be *infectious*, it spread from the city to the various villages and towns contiguous to the navigable streams communicating with the city of New Orleans. The poison of the disease, the *materies morbi*—whatever its nature and form, seemed to be more active—more virulent, and seized upon the unacclimated wherever and whenever they exposed themselves to its influence; or shall we suppose that the poison itself was not more active in reality; but different and distinct from that which predominated in our previous epidemics. We must therefore contend, either for a greater intensity or a marked discrepancy in the poisons of the epidemics of '53, and '47—assuming the constitution of the atmosphere to be the same for the two seasons.

We shall leave these questions for solution to those more conversant with meteorological phenomena; and confine ourselves to the facts of the epidemic. We have already admitted the transmissibility of this disease; we have seen that it was propogated from this city as a *focus of infection* for hundreds of miles in Louisiana, Arkansas, Texas, Mississippi, Alabama and Florida, retaining many, and at some points, all the grave symptoms which characterized the disease when rife in New Orleans. The constitution of the atmosphere certainly favored the transmission of the poison, from one point to another; but we have other facts to prove, that in several instances, the fever perished

with those who conveyed it to particular localities; and again, it made its appearance on plantations and in communities which had had no intercourse with diseased persons or infected districts. Thus, then, we have evidence both *for* and *against* the transmissibility and infectiousness of the disease; it therefore remains to determine on which side of the question the weight of evidence stands. The necessity of quarantine regulations which has recently excited so much interest in the community, may be determined by sifting to the bottom the evidence and facts regarding the infectious or non-transmissibility of yellow fever. It will devolve on the Sanitary Commission recently appointed by the Board of Health, to investigate the nature of this fever, to settle this question. In the meantime, we shall await with much impatience the result of its deliberations. Since the fever disappeared, our city has been unusually free of disease, as will be apparent from the subjoined mortuary statistics.

QUININE IN YELLOW FEVER.

New Orleans, October, 1853.

Dr. Hester:

Dear Sir: In the September No. of your valuable Journal, I find an article under the above caption, in which you say, "That large doses of this salt cannot be relied upon in the early stages of the attack," and again you say, "in our previous epidemics of yellow fever, the quinine practice succeeded best; but it is generally conceded, as far as we could ascertain, that this season it failed in a majority of cases, to sustain its previous high reputation, as a powerful curative agent."

In regard to the term *larger* (when applied to doses of quinine.) There has of late years been such a diversity of opinion, especially among Southern writers, that the term has become altogether indefinite and inexpressive so far as quantity is concerned. I can recollect the time when what might be now called a small dose would have been by the faculty termed absolutely poisonous.

Upon inquiry, I find that in 1847, the physicians here used this salt in doses ranging from 30 to 80 grains, and in one or two instances I heard of 100 grains being administered.

Now, if your observations apply alone to the *larger* dose last mentioned, I fully agree with you, and will go even futher, and say without fear of question, that it was positively injurious. But, if (as it might be supposed) it applies to the administration of quinine generally, in yellow fever, then I must demur to your conclusion.

For the reason that in the early part of the attack, I found it above all things valuable and by far the most effective agent I possessed, but not in larger doses (of 47,) I generally prescribed it in from 8 to 15 grains every 3 hours, thus giving from 30 to 60 grains in the first 12 hours after the attack.

Commencing the exhibition of it immediately on the accession of the fever if possible, but in no case waiting for a *remission*, and often finding it necessary to repeat the prescription the following day, and as a general rule continuing it until its specific effects were shown in the head, and a white line evident upon the gums; (as if they were covered with a thin coat of white paint,) I think I can safely say that I have lost but two or three cases when these effects were produced during the first 24 hours after seeing the case, (and of course charging these upon negligence of those nursing) *I would not like to appear singular, by admitting I had lost a case, and have no excuse for the same,*) generally in those cases where the fever was fully formed, I gave the first and second doses larger than I intended the succeeding ones to be. In conjunction, of course, with hot mustard foot bath, diaphoretics, cupping over mastoid or nucha, etc., and later in the epidemic, generally preceding the quinine, with an active emetic of ipecac, lobelia, and sanguinaria, which, followed by the foot-baths, I found to have an excellent effect, and sometimes I gave 8 to 12 grains mass. hydrag., with the first dose of quinine, often I found when the skin was dry and the tongue becoming dry and coated, the exhibition of 10 grains of quinine would soften the skin and produce a moist state of the tongue, much to be desired. In the early stage of the epidemic I made but little use of purgatives or laxatives, depending on enemas to keep the bowels open, but later I found a free evacuation of the bowels by medicine born to advantage. In fact, during the early part of the epidemic I found no plan succeed as well as it did afterwards. Should we suffer again from a similar epidemic, I should expect (from the effect I saw produced in the few cases I used these) much benefit from Tinc. Aconiti and Tincture Verat-Veride; especially in those cases of intense arterial action.

In giving expression to the above views in regard to the use of quinine, I am actuated by the wish to elicit from the profession generally, their opinion in regard to the use and value of quinine in yellow fever, and would respectfully suggest, that in mentioning the dose used, that the number of grains should be mentioned.

Yours, truly, J. C. BATCHELOR, M.D.

Note—From 20 to 30 grains of quinine are regared by us, and we believe the mass of physicians, of this city, as large and sedative doses; and we felt so confident that we should be so understood, that it was not thought necessary to specify the exact dose in grains, We repeat, notwithstanding the experience of our intelligent correspondent, the "quinine practice," as now generally understood, did not succeed to the satisfaction of those who pursued it in the late epidemic, and which was found so successful in the fever of '47. Ten or fifteen grains of quinine, in the first stage of the fever, acted well and favorably in our hands; and we ventured often to repeat this dose in particular cases, with abatement of the pains and febrile symptoms. Some inquiry among some of the best practitioners of this city, has but served to strengthen the suggestion thrown out hastily, on the subject of quinine in yellow fever, in our September No.. Large doses of quinine (20 to 30 grains, repeated every

2 or 3 hours,) was found so depressing to the heart and arteries, and so injurious to the nervous centres, in the late epidemic, that stimulants would sometimes fail to sustain the vital forces and bring about a salutary reaction. Indeed, in some instances it acted, in such doses and thus repeated, as a powerful sedative poison; and hence, some prudence and judgment are required to wield this powerful medicine to advantage in our epidemics. We do not, of course, condemn the free and prudent use of this salt in our fevers; but we do protest against its indiscriminate administration in such monstrous doses.—Ed.

TO SUBSCRIBERS.

We are now in the middle of our 10th Volume; and, although, our terms are *invariably in advance for the volume*, yet, we are sorry to find so many of our subscribers in arrears to the Journal. We feel assured, that none are willing to receive the Journal at the expense, both of our time and means; yet such is the case with those who do not remit to us the little amounts due the Journal. We do hope this suggestion may direct the attention of many of our subscribers to the *unpaid* accounts which we mailed to them, at the commencement of this volume. Enclose to our address and at our risk.

Editor and Proprietor.

YELLOW FEVER IN ST. JOHN THE BAPTIST, LA.

New Orleans, Oct. 20, 1853.

Dr. Hester:

Permit me to furnish you with some facts in regard to the epidemic, which has reigned for nearly two months in the parish of St. John the Baptist. I passed four days in this parish, I then visited about one hundred and fifty cases of fever, accompanied by Dr. Fortineau, and I can assure you that the sickness which has caused so many victims in St. John the Baptist, is exactly the same as the one which has made such ravages in New Orleans. The symptoms are the same, the march is the same, the gravity is the same, the modes of it are the same, and so is the termination in death or recovery.

The first case occurred in the lower part of the parish, in a small house, situated a few acres from the river. The person attacked was a lady who resided there. She had not visited the city, and she had not been in contact with any one attacked with yellow fever. So you see, sir, the explosion was spontaneous, there was no contagion, nor anything resembling it. This lady died, and two or three days afterwards a new case appeared in a house situated but a few acres higher, at the river, than the house, where died the first case. Seven or eight cases originated successively in this last residence, and nearly all were fatal. Thence the disease spread from one to another, and was not long before it invaded the entire village, for we may call by this name a collection of little huts which occupy the space of about one mile.

Dr. Fortineau observed that the disease attacked almost invariably those persons or relations who came from a healthy place to give assistance to their friends; that is to say, those who went suddenly into the focus of the infection. An isolated case broke out on the opposite side of the river, without being followed by any other. He observed likewise that the disease was very mild among the negroes, strictly speaking; he saw among them a good many who, after having the black vomit, got well with wonderful facility.

The fever begins generally with a chill of greater or less severity, it is accompanied with violent headache and pain in the back, sensibility of the stomach, nausea, vomitings, and acrid and burning heat of the skin. The face is red, the eyes injected, the tongue covered with a thick fur in the middle, red on the edges and at the point. The gums soon became congestive; in a good many cases they became covered with a thick whitish pellicle which collects in the form of a cream-like substance and is easily scraped off by the finger. The febrile condition, do what you will, lasts three or four days. Some convalesce rapidly, in others appear that terrible train of serious phenomena with which you are acquainted, such as hamorrhage from the mouth and bladder, black vomit, yellowness, &c.

I do not pretend to say, sir, that the yellow fever exists alone in the parish of St. John the Baptist; we see there also simple intermittent fever which yields easily to the sulphate of quinine. Often times it is even easy in the beginning to establish the differential diagnosis. In the cases of intermittent fever, the tongue is uniformly furred, without being red at the point nor at the edges; the patients do not feel that burning or arid heat of the skin, and we do not observe that congestion of the face, nor that vivid injection of the eyes. Some of the inhabitants of the place assured me that the winds have blown from the East, coming consequently from New Orleans, during the three last weeks which preceded the breaking out of the epidemic. They also observed something unusual in the heat of the sun. After being exposed to it some time, they returned home with pain and heaviness of the head, and some were obliged to make use of an umbrella, contrary to their usual custom.

I limit here, sir, the particulars I possess in regard to the epidemic prevailing in the parish of St. John the Baptist. I might enter into many other details did I not fear to occupy in your estimable Journal a space which might be better filled. Accept, Mr. Editor, &c.,

CHARLES DELERY, D. M. P.

SOME FURTHER OBSERVATIONS ON THE PREVENTION OF YELLOW FEVER.

VICKSBURG, Oct. 21, 1853.

Dr. Hester:

DEAR SIR.—Since I wrote the article for your Journal in September last, on "*Prevention of Yellow Fever*," some additional facts have come to light confirming the views therein taken.

I was not quite correct in stating that the first case occurred on the 28th of May. Dr. Moses M. Dowler asserts that he had a decided case on the 6th of May, in the person of a German, on Gormley's Basin—a shingle-maker, laboring in the swamp, and entirely disconnected from the shipping. The man's wife also died. But from the facts brought to light by the Investigating Committee, it appears that the first cases of yellow fever in that part of the city where the disease became epidemic, were from the ship Northumberland, direct from Liverpool, with four hundred emigrants on board. One died with black vomit on the way up the river. The ship arrived on the 9th of May, three days after Dowler's case—and soon after her arrival, some five or six cases of yellow fever, among her emigrant passengers, occurred. In the article I sent to your Journal I took the position that yellow fever is a species of typhus, belonging to the same genus as the jail, camp and ship fever. The jail, camp and ship fevers are however to be communicable in a confined atmosphere. Four hundred passengers crowded together in a single ship, in passing through the tropics caused the fever, which originated among them, to assume the yellow fever or black vomit type—and its being virtually a ship fever, made it more communicable than yellow fever generally is.

I was in error in saying that no cases had occurred in the jail. Since I wrote the article, I have visited the jail, conversed with the officers, and had the books examined. What I stated was strictly true in regard to half the jail, the northern half, occupied by women and children. No case of yellow fever has occurred among them—about fifty in number, and generally unacclimated. In the other part of the jail 22 cases are reported as having happened and six deaths—two attended with black vomit. The first case occurred in a man committed on the 5th of July—he was from the lower infected district. At his trial, on the 27th of July, he was attacked with the yellow fever. Yet only 22, out of about 160 prisoners, took the disease, although many, besides the twenty-two, were unacclimated. Only one more death occurrrd in the jail from the 2d of June to the 4th of October, 1853, than happened in the same time in 1852.

The epidemic, therefore, was not epidemic in the jail—nor has it been all over the city. I mentioned that the back part of the city, next to the swamp, had in a great degree escaped. In fact, there is very good reason to believe, that as an epidemic, it did not extend below Poydras street from the upper focus of infection, and did not come above Esplanade from the lower focus. In one Irish family, in the heart of the old square, consisting of eighteen persons, who came to this country together, only three were attacked with the disease. The others were saved from an attack by ventilating the house, by breaking certain doors down which had been nailed up. When the cold weather set in these doors were again closed, and soon two other cases occurred, *only* a few days ago. I have also visited Pass Christian, Natchez, Vicksburg, and other places, since I wrote the article for your Journal. At Pass Christian the disease has not been epidemic, yet a great many cases have happened in spots as it were. Where the patients were shut up, nearly the whole family were

affected—but when the sick room was properly ventilated, the disease did not spread. A number of cases were carried to Proctorville, on Lake Borgne, at the end of the Mexican Gulf Railroad, and several families, for the want of houses, camped out on the borders of the lake. Yet, in that airy place, in an open prairie, on the banks of the lake, not a single case of yellow fever was communicated from the sick to the well—proving conclusively that the disease is not communicable in a well ventilated atmosphere. It is only communicable in closely shut up rooms. It originated on board of the ship Northumberland from the confinement of a great number of persons in a hot, filthy, unventilated atmosphere.

The same causes which produced it on shipboard, were in full activity where the emigrant population were huddled together in filth and poverty, in those parts of the city which the epidemic scourged so severely.

Respectfully, yours, &c.,

SAMUEL A. CARTWRIGHT.

CHOLERA.

This plague is again spreading in Europe, and is now returning to our shores by the same route which it traversed in its march of death, in 1847-8 and 9. The last accounts announce its appearance in England—even in Liverpool—which is but a few days sail from our shores. We may soon expect it to pay us an unwelcome visit; and we predict that before the lapse of 12 months, this dreaded pestilence will be down upon us like a tornado—carrying death and desolation in its train. Let us then put our house in order, and prepare for its reception; let us not be caught as in the late epidemic, yellow fever, with folded arms, and flattering ourselves that we have already been scourged enough, to appease the wrath of high heaven. By anticipating, we may often mitigate, if we cannot obviate evils; this is markedly so in relation to hygienic precaution and sanitary measures. Both experience and observation, confirmed by scientific men, prove that cholera finds a resting place—a starting point, in the damp, low, confined, and crowded portions of our large cities. Here is its chosen seat—its permanent quarters, from which it radiates to all parts of our great towns, destroying in a few days, perhaps a few hours, a large portion of a populous district.

Let such places, such spots of infection be explored, and the causes calculated to generate and propagate diseases be removed, both by a free use of water, ventilation, and the liberal employment of some powerful disinfectant.

The cholera, we learn, is committing terrible ravages in Yucatan and many parts of Mexico; and we may expect it on our shores at an early day. One epidemic frequently succeeds another; the yellow fever, the cholera, and the latter the former. Let the authorities look to it. We warn them in time. Soon the struggle will commence, we fear, with another, a second epidemic—more terrible than the last.

YELLOW FEVER IN ALABAMA.

The following letter has been addressed to us by one of the first physicians of Alabama, who requests us to name the disease which will be found fully described below. The description of the symptoms which generally characterize the yellow fever, is so true to nature, in this case, that we could not resist giving it publicity, even at the risk of offending the talented author.

We need hardly say, that hundreds of such cases, as here described by Dr. Boling, presented themselves to our physicians in the late epidemic. Indeed, nothing was wanting, [but death and a post-mortem,] to complete the full picture of the case.

(*Ed. N. O. Med. Journal*)

MONTGOMERY, ALA., Oct. 13, 1853.

Dr. Hester:

DEAR SIR—Please excuse the liberty I take, in asking your opinion as to the nature or name of the disease, in the following described case. As a medical practitioner of New Orleans, your opportunities for observing such cases, or cases somewhat resembling it, have probably been numerous; and as the Editor of a Medical Journal, your attention may have been directed to distinctions, to shades of difference, that might possibly escape the notice of others.

Mrs. F., a German woman, aged about 38, was taken early in the morning of the 21st of September, with high fever, preceded by a very slight sensation of chilliness. I saw her first on the evening of the 22d, and was told that her fever had continued without abatement since the morning before. Her pulse was 120, or rather more, her skin hot and dry, thirst moderate, eyes red, very great pain in the head, back and limbs, with a feeling of soreness over the surface generally. I prescribed for her 36 grains of quinine, 20 of calomel, and half a grain of morphine, to be mixed and divided into three parts, one of which was ordered to be administered every 4 hours. A hot mustard foot-bath was also directed. Her fever continued high during the greater part of the night, though in the morning I found her pulse down to 100, the skin moist, and the head-ache, etc., diminished.

Her pulse, during the day fell to 96, and by the next day was reduced to 86. During the entire period of her sickness it was moderately full—firm at first, and at no time, I think, could be called decidedly soft. Her treatment during the next three days, was small doses of calomel and morphine, about three times in the 24 hours.

On the evening of the 24th, her skin was slightly yellow, and on the morning of thh 25th, she was deeply jaundiced. The color deepened, and for seven or eight days, the shade of her skin, instead of yellow, was rather a copper color. After this, the color became less deep—but slowly—and she is at the present time quite yellow. During the few days that her color remained of the deeper shade, her pulse was from 80 to 84; she lay in a somnolent stupid condition; though occasionally slightly delirious, and always forgetful—

she would generally, at the moment she was roused, give a correct answer; but her eyelids would immediately begin to sink, and she would again fall back into the somnolent state described.

On the morning of the 26th, she had slight hemorrhage from the nose, and during the entire day; as also, all day on the 27th, there was a constant oozing of blood from the gums. It did not take place from any sore or ulcerated spot, but from the entire margin of the gums, as far as could be conveniently examined. The quantity lost was about a pint, I would say, during the two days. The hemorrhage from the gums was as bad, if not worse, on the morning of the 28th; but ceased during the course of the day—the patient having miscarried about 10 o'clock, in the fifth month of pregnancy.

On the 29th, some slight mercurial sores appeared on the inside of the lower lip; the mouth in a few days became quite sore; but no salivary discharge occurred till about the 6th Oct., when it suddenly became quite profuse.

Although, for several days, while seemingly at her worst, the patient complained of nausea at times, or rather "a bad, heavy feeling" in the stomach; she vomited but once. On the morning of the 4th of October, I was told that she had vomited once during the night On asking what she had thrown up, her husband answered, "little, except the medicine," he supposed, as she threw up soon after taking it. On the breast of her gown was a splotch about as large as the palm of one's hand, stained of a light brown color, with numerous very dark brown, or blackish spots or specks upon it, as if produced by floculi; which her husband said was produced by part of the matter, vomited, falling on her dress. These black spots together, would have covered one-third or one-fourth of the stained surface. During her sickness, she at no time took anything, either as drink, diet or medicine, of a dark color; and the medicine she was taking at the time she vomited; and the only medicine she had taken for several days, was a solution of creosote in wine and water.

I will further remark, that at the time a form of disease was prevailing, to a limited extent in the place—supposed by the physicians of the place, generally, to be yellow fever, though denied or doubted by a few.

I have given every symptom that seems to me in any way essential—none of note occurred that has not been stated. The patient is, and has been slowly improving, for ten or twelve days.

What would the case be called in New Orleans; if not yellow fever, what was it? or, rather, what symptoms were lacking to make it such?

Very respectfully, W. M. BOLING.

TYPHOID FEVER AND ITS TREATMENT IN ALABAMA.

FARRIORVILLE, Alabama.

The medical topography of my field of operations is, to sum it up in a few words, a country location, three miles north of Conecuh river, which is but a large creek, with an average of near half a mile in width of swamp that is

inundated by every freshet, but which nearly all becomes perfectly arid in a dry time.

Within two miles to the eastward there is a smaller creek on which there are four mills, the ponds of which become nearly dry sometimes. The face of the country immediately in this neighborhood, and for several miles to the S. W., is moderately undulating; fertile oak and hickory land, about half of which is under cultivation by farmers, mostly in easy circumstances. This land contains a considerable portion of lime, and the sub-stratum is a limy clay, at no great depth for the most part. Some of our water is "freestone," but most of it contains lime and other salts. East of the Mill Creek is level pine woods interspersed with muddy branches and gallberry sloughs. To the north there is a belt of rolling sandy pine woods, intervening between us and the prairie mud of Macon and Montgomery counties.

We have had an unusually warm summer here, and April, May and June were exceedingly dry. July was seasonable, and August pretty wet. Most of September was dry, though we had several days cool rains from the N. E. During most of the last two weeks the thermometer has ranged from 55° to 75°; and, although it has been two or three degrees higher this year than it has before since I have been here (near four years,) it has not been above 120 Fahrenheit in the shade. Our latitude is about 32. The year 1850 was neither wet nor dry, and temperature moderate, and it was decidedly healthy. Last year, and the first three months of this, was very wet, and it was more healthy. In 1851 the summer was dry and warmer, and the autumn dry and cooler than usual, and catarrhal affections prevailed considerably early in autumn, so extensive indeed as to be considered an epidemic; and later we suffered some from what we considered *typhoid fever*. The present year was remarkably healthy up to July, when we began to have some miasmatic fevers. During the last three months we have had more fever than during the three previous years. Our fevers are chiefly intermittent or distinctly remittent, and I consider them all of miasmatic origin; but my neighboring physicians have to treat a good deal of what they call the typhoid fever. lasting from fifteen to fifty days, and frequently proving fatal. Now I practice in the same localities, and frequently in the same families where they meet with these tedious typhoid cases, and I have not a fever case to last over eight days, where I have seen it early nor lost a fever case that I have treated from the commencement, for the last two years, and I think I have treated as many fever cases as any one in this region of country. When I am asked why it is that I have none of those tedious cases of typhoid fever, I reply that I always cut them short before they turn to typhoid fever; but it may be merely a strange coincidence that I meet with no such cases while it is prevailing all around me, and frequently occurring right in my field of labor when treated by others. I have seen several of those typhoid cases which presented all the symptoms of that disease, according to the best authorities, but I have seen quite a number of cases that seems to me precisely similar to those at the outset, and occurred in and about the same localities, and under the same circumstances, which yielded in a few days to

my plan of treatment. The principal difference between my treatment and that of those typhoid doctors, consists in the quantity of quinine we use. When I am called to a case where the exacerbation, so to speak, has continued without a remission for some time, I reduce the febrile excitement, usually with Norwood's tr. veratrum viridi, the lancet, or antimony, pro re nata, with some otder adjuvants, mercury, opium, camphor, terebinth, &c., as the symptoms seem to indicate. I then give from gr. v. to 30 grs. quinine, and repeat three or four times, at intervals of two to four hours, according to the gravity of the case.

I am not entirely alone in giving quinine with a bold hand in those continued fevers. There are a few of our professional brethren in this part of the country, who are not afraid to give grs. xx or grs. xxx of quinine under certain circumstances, and I believe they have not much more typhoid fever to treat than myself. A case in point is my friend Dr. F., of Macon county. He has been especially annoyed with tedious typhoid cases; and remarkable for his sparing use of this invaluable drug, holding the doctrine that all that he could do was to watch for collateral symptoms, and guard them through their tedious courses, until some six weeks ago, having lost six or seven of his own family, besides several other cases, within a year, with this typhoid fever, he expressed some surprise that I had met with none of those severe cases, when I told him that I had no doubt some of his cases had perished for want of quinine, and prevailed with him to give my plan a trial. A few days ago he told me he had got to giving quinine by the spoonful, and that he "smashed the fever right out" wherever he goes. He still regards it as typhoid fever, but says if he had known as well how to manage it a year ago as he now does, he would be worth several thousand dollars more than he is.

As for the correctness of the doctrine of the abortive treatment of typhoid fever, this deponent saith not; but this I do think, that either we have no typhoid fever in this region of country else Durand and Fenner are about right, and will prove great benefactors to mankind.

Yours, respectfully,

G. W. OUTTER.

THE SANITARY COMMISSION.

This body, recently appointed by the Board of Health, to inquire into the origin, causes, character, etc., of the late epidemic, is composed of Drs. Axson, Barton, Riddell, Simonds and McNeil. They have been for some time occupied with the duties assigned them—taking the testimony of physicians and others, who had watched the origin and progress of the epidemic. From the labors of this scientific "Commission," we expect a flood of light will be thrown upon this truly vexed question.

Members of this Commission will shortly visit the various points, towns, etc., to which the fever has been known to spread—with a view to collect all the

authentic facts in relation to its mode of propagation or its transmissibility, its type, and such other information as may tend to elucidate its peculiarities. We trust the physicians who have seen and treated this disease at the various localities where it has prevailed will, when waited upon by the members of the Sanitary Commission, cheerfully communicate the results of their experience and observation, in order that the public mind may be fully enlightened on the subject, and prepared to adopt such sanitary measures as shall secure, for the future, those who have been scourged by the epidemic this season, against the terrible ravages of the fever, a second invasion.

Circulars propounding interrogatories to physicians, will be early forwarded to the various towns, villages, etc., where the epidemic has prevailed; to which answers are solicited: Below, we shall give an outline of the subjects on which the Sanitary Commission seeks information:

"Please furnish to the Sanitary Com mission of New Orleans, any information you may possesss with regard to the following subjects, adding such other particulars, as you deem useful.

1. *With regard to the locality, concerning which you can report.*—The name of the locality; its limits and boundaries; the surface, soil [state whether sandy, clayey or calcarious; what kind of drinking water is used in your neighborhood, specifying whether well, cistern or spring water, and whether free-stone or limestone; there (has or has not) been recently extensive clearing of lands]in the vicinity, or disturbance of the soil from the digging of wells or canals, making levees, improving roads, draining or paving of streets or any other upturning of the soil; state its position with regard to rivers, bayous swamps, marshes, stagnant lakes or pools of water, &c.; state its condition as to drainage, does the water run off freeely or does it accumulate, and if so how near your place?

2. *As regards the meteorology of your locality.*—Please furnish if practicable a detailed statement of the meteorological observations of your neighborhood for the entire year; if this cannot be obtained state as nearly as possible the condition of the weather as to dampness or dryness, the temperature whether hot or cold, whether very hot in the sun, or cool in the shade, the prevalence of rains and fogs, the electrical state of the atmosphere as evinced by the occurrence of thunder, lightning &c. and the prevalence and direction of winds during the existence of the fever, and for a month or two previous.

3. *Please state if you have observed anything remarkable in the Animal or Vegetable Kingdoms, prior to, or during the epidemic, such as the blighting of fruit, the inordinate prevalence of flies, mosquitoes, etc., the death of animals or the unusual occurrence of mould, stating its color.*

4. *Give an approximate estimate of the population of your town or place previous to the commencement of the epidemic.*

Whites, over 10 years, Males; do., Females; do. Both; Whites under 10 years; total whites; of whom are natives of the place; of whom are natives of the United States; of whom are natives of Foreign Countries; stating of what countries: number of Colored.

5. *Fill up the following blanks of deaths from Yellow Fever.*—Whites over 10 years, Males; do. Females; do. Both; do. under 10 years; Total Whites; of whom are natives of the place; of the United States; of Foreign Countries, stating of what Countries; number of Colored.

6. *Furnish the same information with regard to the cases of Yellow Fever.*—Whites over 10 years, Males; do. Females; do. Both; do. under 10 years; —Total Whites. Of whom are natives of the place; of the United States; of Foreign Countries; stating of what countries: number of Colored.

7. *Early Cases.*—Give the date at which your first case of yellow fever occurred, with as many particulars thereof as possible. Give the same as far as practicable of the next ten, fifteen or twenty cases. Had any of these cases been in the locality where yellow fever was prevailing? Are any believed by you to have arisen from the handling of goods, clothing &c., or from direct intercourse with other cases of the disease? Do you know of any case which appeared to have originated spontaneously without even the suspiciou of intercourse with other cases of the disease? If you can trace the sperad of the early cases of the disease from house to house, or from person to person, or their relations to any local cause of disease, such as vicinity to streams, ponds or swamps, or the direction of the wind—please do so.

8. *Social condition.*—What classes of your population, with reference to their personal and social habits, whether temperate or intemperate, occupying isolated dwellings, or crowded lodgings, have suffered most from this disease, both with regard to attacks and mortality?

9. *Character of the Epidemic.*—Give the prominent symptoms, progress, duration and termination of the cases occurring under your observation. In what proportion of the cases was there Black Vomit? In what Yellowness of Skin? In what Hemorrhage? Did other types of fever prevail at the same time, or did all assume the type or peculiarity of the prevailing epidemic?

Assuming the propagation of the disease from exposure either to an infected atmosphere, to personal communication with the sick, or contact with goods or clothing, either of the sick or transmitted from a locality considered infected, what time intervened between said exposure and the appearance of premonitory symptoms, and also the development of the disease? Do you regard the Epidemic as true yellow fever? Have you ever seen this disease before? If you have, state where, and when? Please state the whole number of cases of black vomit which you have seen; Also number of recoveries therafter. State the number of cases alleged to be the second or third attacks, and the evidence thereof. State as nearly as possible the number of persons attendant on the sick, or otherwise exposed to its possible causes and liable thereto from never having had it, who have entirely escaped during the Epidemic. On what days did deaths usually occur? on the 3rd, 5th, 7th, &c.

MEDICAL DEPARTMENT OF THE UNIVERSITY OF LOUISIANA.

In the second week of November, will commence the Lectures in the Medical Department of the University of Louisiana, with prospects and under auspices highly encouraging to the friends of the University. With an experienced and well tried Faculty—with spacious and choice Lecture Rooms—with material pursuits, and an immense Charity Institution for Clinical teaching at the disposal of the Professors—nothing is wanting to make this the first medical school in the South for the acquisition of practical medicine.

THE EPIDEMIC.

Total number of deaths by yellow fever and other diseases, from May 26th till date—continued from our last number:

Week ending.	*Total.*	*Yellow Fever.*	*Other Dis.*	*Not stated.*
May 26 to 31	- 110	3	97	10
June -	- 666	40	581	45
July - -	- 2077	1406	559	112
August 26 -	- 5460	4574	582	304
Total	- 8313	6023	1819	471

Week ending.	Total.	Yellow Fever.	Other Dis.	Not stated.
August 27	187	155	25	7
28	169	131	26	12
29	143	120	15	8
30	139	114	14	11
32	137	95	27	15
September 1	119	96	16	7
2	133	103	23	7
3	116	87	20	9
4	110	90	15	5
5	98	71	26	1
6	95	65	25	5
7	70	48	17	5
8	—	—	—	—
9	64	43	17	4
10	80	56	19	5
11	68	43	20	5
12	65	33	15	7
13	47	26	19	2
14	45	32	10	3
15	52	32	19	1
16	51	31	19	1
17	47	24	21	2
18	47	28	16	3
19	34	21	11	2
20	49	27	14	8
21	38	16	18	4
22	34	16	14	4
23	28	9	12	7
24	34	10	22	2
25	35	15	19	1
26	42	15	23	4
27	33	12	19	2
28				
29	26	13	10	3
30	31	8	19	4
October 1	26	7	15	4
2	33	11	17	5
3	19	8	11	
4	23	4	14	5
5	29	10	15	4
6	29	11	17	1
7	19	4	12	3
8	32	10	16	6
9	30	4	21	5
10	24	5	18	1
11	13	5	8	
12	19	8	8	3
13	17	2	14	1
14	27	9	17	1
15	15	5	8	2
16	19	4	12	3
17	23	4	19	
18	22	5	17	
19	26	9	15	2
20	14	2	10	2
21	27	10	14	3
22	17	2	12	3
Total,	11,252	7,847	2,714	691

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1853.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich

WEEKLY. — 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days	Quantity of RAIN — Inches.
	Max.	Min	Range.	Max.	Min.	Range.				
Aug 26	91 .00	74 .00	17 .00	30 .15	30 .12	0. 03	NE.	2 ¼	5	0.855
Sept. 2	87 .00	75 .00	12 .00	30 .07	29 .98	0. 09	NW.	2 ½	6	2 170
" 9	91 .00	74 00	17 .00	30 .15	30 .10	0. 05	SE.	2 ½	6	1.150
" 15	92 .06	59 .00	33 .00	30 .19	30 .08	0. 11	SW.	2.	1	0.350
" 22	86 .00	63 .00	23 .00	30 .18	30 .07	0. 11	NE.	1 ¾	0	0.000
" 30	85 .05	65 .00	20 .00	30 .17	30 .12	0. 05	N.	2.	0	0.000
Oct. 7	84 .05	70 .00	14 .00	30 .14	30 .07	0. 07	NE.	2 ¼	0	0 000
" 14	80 .00	65 .00	15 .00	30 .10	29 .19	0 .20	NNW.	2.	2	.095
" 22	82 .00	60 .00	22 .00	30 .18	29 .90	0 .40	NW.	1 ¾	4	0.000

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For August and Sept mber, 1853.

	SEX.	AUGUST.	SEPT.
ADMISSIONS -	Males	1298	654 -
Do. - -	Females	642	220
		——1940	—— 847
DISCHARGES -	Males	729	444
Do. - -	Females	413	177
		——1142	—— 621
DEATHS - -	Males	792	217
Do. - -	Females	227	45 -
		——1019	—— 252
BIRTHS - -	Males	4	4
Do. - -	Females	5	4
STILL-BORN -		4	1
		—— 13	—— 9

H. VANDERLINDEN, Clerk.

ERRATA.

The article on "*Continued Fever*," by Dr. Gordon, of Alabama, published in our last number, contains the following typographical errors, which the author requests us to correct: On page 146, line 29th, for "oddities" read "crudities"—same page, line 33, insert "and" after causes—next page, 147, line 38, instead of small t insert T, (capital) and let a period instead of comma precede it—on page 148, line 18, insert "so" after were—on page 210, line 6, insert a comma after control, and read "aggravates" for aggravate; (this alters the sense of the whole passage)—same page, line 9, read "3" for "$\frac{3}{}$"—same page, line 18, read "8" for "8th"

THE PRACTICE OF MEDICINE.
WHAT IS IT?

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

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

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

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

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

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

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

The Report of the U. S. Examiner says:

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

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

consequently worse than worthless for medicinal purposes ; or whether it is not, in fact, composed entirely of the latter worthless variety ? or can say, by looking at powdered rhubarb, whether it is of that prime quality which affords from sixty to seventy per cent. of soluble matter, and some twelve per cent. of pure resin, or whether it is an article produced from the decayed and worthless root, (the color and smell having been heightened by artificial means,) which affords not to exceed fifteen per cent. of soluble matter and no resin at all ?

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

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

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

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

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

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

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

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

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

It is well known among dealers, and yet not generally known by the profession and the public, that pure and genuine medicines, particularly pure powdered drugs, from the first quality of gums and roots are scarcely procurable in this country, and therefore physicians often prescribe medicines to meet certain indications in the disease of the patient, without obtaining the desired and expected beneficial result. To enumerate the articles of adulterated medicines that are daily sold in market would be to name almost the entire list of the materia medica. From the finer and more important chemicals and pharmaceutical preparations, such as Morphine, Quinine, Hydriodate Potass, calomel, Blue Pill, &c. &c., down to the most common, and those of daily use, such as Cream Tartar, Rhubarb, Ipecac, &c., the adulterations are so adroitly made, that (without analyzation) even the closest inspection will fail to detect them. Quinine is often found largely adulterated (in some instances more than half) with mannite and other substances. Blue Mass and Calomel have been found much more than half adulteration. A gentleman at one time connected with an extensive manufacturing establishment, informed us, that just before he left England, the factory turned out more than four thousand pounds of Blue Pill, containing Barytes, instead of Mercury ; and it was all destined for the American market.

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

CIRCULAR.

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

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

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

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

PARTICULAR NOTICE.

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

proofs or the difference between our 'Extra Powders' and the ordinary powders of the trade.

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

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

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

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

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

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

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

"EXTRA POWDERS."

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

Bottles and Cans at Cost,

When put up in Quarter and Half Pound Bottles additional ten cts. per lb.

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

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

Pure Chemicals Prepared at our Laboratory.

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

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

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

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

Extract of Belladona
, Butternut
, Buchu
, Bloodroot
, Boneset
, Conium
, Chamomile
, Colocynth, Ordinary
, Colocynth, Opt.
, Dandelion, Alcoholic
, , Inspissated
Extract of Digitalis
, Gentian, opt.
, , ordinary
, Hyoscyamus
, Hops
, Jalap
, Lupuline
, Mandrake
, Nux Vomica
, Opium
, Pinkroot
Extract of Quassia
, Rhubarb
, Sarsaparilla Simplex
, , Compound
, Para , Alcoholic
, Hon. , Alcoholic
, Stramonium
, , Seeds
, English Valerian
, Dutch ,

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

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

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

These Extracts together with our pure Chemicals and Extra Powders have received the unqualified approbation of various Medical Associations, and of Physicians and Apothecaries and have elicited many complimentary letters on their good qualities. The following token of approbation is from the Ohio Medical Convention:

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

And also from the following eminent Practitioners:

PHILIP SCHIEFFELIN, ESQ.:

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

Wishing you the success you so fully deserve.

I remain yours, very truly, H. P. DEWEES.

Seamen's Retreat, Hospital, Staten Island, June 21st, 1850.

Messrs P. Schieffelin & Co.,

Gentlemen,—Having used your Drugs and Chemicals in this Institution, as well as in private practice, for the last twelve years, it affords me much pleasure to bear testimony as to their quality and purity; for without these, no physician, be he ever so skilful, can calculate the result to his patients.

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

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

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

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

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

Our arrangements and facilities are such, that we can offer inducements to dealers, which must influence all, who not only like to have a fair equivalent

THE

NEW-ORLEANS

MEDICAL AND SURGICAL

JOURNAL.

JANUARY, 1854.

A. HESTER, M. D.,

EDITOR AND PROPRIETOR.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*

N. O. Charity Hospital.

NEW-ORLEANS:
PRINTED BY JOSEPH COHN, 12 ST. PETER STREET.
1854.

TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

All communications should be addressed to the Editor of the New Orleans Medical and Surgical Journal, and they will receive prompt attention.

Papers for publication have been received from Dr. W. P. Reese, of Selma, Ala.; Dr. T. A. Cook, Washington La.; Dr. J. S. Davis, Salem,Miss.; Dr. E. McAllister, Port Gibson, Miss.; Dr. J. B. Hacker, Plaquemine, La.; Dr. J.U. Ball, St. Francisville, La.; Dr. E. F. P. Alexander, Neutonia, Miss.; Dr. J. C. Nott, of Mobile; Dr. W. J. Tuck, of Memphis, Tenn., Dr. J. Garril, of Apalachicola, Fa.

Since our last, we have received the following books, circulars, pamphlets, etc., etc., for review:

A practical treatise on the diseases of children. By Francis Condie, M. D. Secretary of the College of Physicians, Member of the American Philosophical Society, etc. Fourth edition, revised and augmented. Philadelphia, Blanchard & Lea, 1853. (From the publishers.)

Chemistry and Metallurgy as applied to the study and practice of Dental Surgery. By A. Snowden Piggot, M. D., late Professor of Anacomy and Physiology in the Washington University of Baltimore; with numerous illustrations. Philadelphia, Lindsay & Blakiston, 1854. (From the publishers.)

A Text-Book of Anatomy, and guide of dissections, for the use of students of medicine and dental surgery. By Washington R. Handy, M. D., Prof. of Anatomy and Physiology in the Baltimore College of Dental Surgery; late Professor of Anatomy and Operative Surgery in the Washington University, Baltimore. With 264 illustrations. Philadelphia, Lindsay & Blakiston, 1854. (From the publishers.)

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England, by James Paget, F. R. S., lately Professor of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Phy-

siology at St. Bartholomew's Hospital. Hypertrophy, Atrophy, Repair, Inflammation, Mortification, Specific Diseases and Tumors. Philadel, phia, Lindsay & Blakiston, 1854. (From the publishers.)

On the Etiology, Pathology and Treatment of Fibro-Bronchitis and Rheumatic Pneumonia. By Thomas H. Buckler, M. D., formerly Physician to the Baltimore Almshouse Infirmary. Philadelphia, Blanchard & Lea,1853. (From the publishers.)

Medical Communications of the Massachusetts Medical Society. Vol. 5. Boston. (From Edward James.)

Report of the Standing Committee on Surgery, read before the Kentucky State Medical Society, October, 1853. By Joshua B. Flint, Professor of Surgery in the Kentucky School of Medicine. (From the author.)

Hospital Hygiene, Illustrated. By John H. Griscom, M. D. From the Transactions of the New York Academy of Medicine. New York, 1853. (From the author.)

Essay on the Sudden Coma of Typhus and Typhoid Fevers and Typhoid Pneumonia. With Illustrative Cases. By J. Lewis Smith, M. D. New York, 1853. (From the author.)

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JANUARY, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

I—ON THE TREATMENT OF PNEUMONIA.

BY S. AMES, M.D., MONTGOMERY, ALA.

It is now a good many years since certain defects in the ordinary treatment of pneumonia, by means chiefly of mercury, emetic tartar, and blood-letting first attracted my attention. The dissections which I then made, showed that some of my patients, dying between the sixth and tenth days, had a smaller aggregate of disease of the lungs than others who recovered had manifested, at any time in the attack, by physical signs. In connexion with this fact, of little consequence in itself, two other things were noticed: *First*, that the fatal cases, having less disease of the lungs, were attended with certain complications and a new set of symptoms, which seemed very materially to influence the progress and result of the attack; and, *secondly*, that these complications had a certain relation to the treatment. The complications were an ileo-colitis, with its attendant symptoms; a dry and red tongue, tympanitic abdomen, and diarrhœic or dysenteric stools; sometimes succeeded shortly after its advent, sometimes accompanied from the beginning by an affection of the liver and brain, giving rise to jaundice, delirium and coma. The relation of these conditions of disease to the treatment was supposed to be evinced by several circumstances. It was observed that those cases in which the treatment was begun early

resulted less favorably, as a general rule, other things being nearly equal, than those in which the treatment was begun later, and consequently was less protracted; that in the former the complications were more common, and, when not fatal, the attack was more obstinate and followed by a slower convalescence; so often indeed were these accidents presented in this apparent connexion, as to induce a good deal of doubt about the propriety of beginning the treatment early in the attack, the temptation being rather to defer it until the approach of one of those critical days on which this disease naturally inclines to terminate favorably. And, lastly, it was observed that serious affections of the gastro-intestinal mucous membrane never occurred in the beginning of an attack, nor indeed at any time in the progress of it, before any treatment was begun. Later experience has satisfied me that, however common may be the evidences of some degree of irritation in some part of the digestive canal, an active inflammation is naturally an exceedingly rare complication of pneumonia.

Thus, the facts seemed very obviously to lead to the inference that these complications were produced by the deleterious agency of the remedies, or some one of them employed in the treatment. At the same time the nature of these new conditions of disease, in connection with the well known toxicological properties of the medicines, while it served to confirm the former inference, pointed to the mercury and antimony as the only agents concerned in producing them. Further observations, however, seemed to be required, in order to determine the relative importance of these agents in bringing about these results; whether one only was concerned, or their joint action was required. Without entering into the details of this part of the inquiry, further, than to state that the method employed was that of occasionally leaving out of the treatment one or other remedy, it will suffice for the object in view to give the positive results obtained after some years appropriated to the investigation. It seems that while either mercury or antimony are capable of superinducing these forms of disease when administered in pneumonia, the accidents arising from the one are less frequent and somewhat different from those arising from the other; the latter, however, being equally formidable when they do occur. An ileitis, or gastro-enteritis is most common, and is the usual result of poisoning by tartar emetic. Mercury, on the other hand, may induce an inflammatory state of the intestinal mucous membrane, less frequently, if ever, involving that of the stomach, and more frequently that of the larger bowels, and occasionally, it may be, about the same time the liver and brain. When the two medicines are given together

the resulting complications are apt to involve all the structures mentioned, and, it may be added, are more likely to occur.

It is, perhaps, unnecessary to say that these complications were proved to be of the most formidable nature, always aggravating the pulmonary disease, and rendering it less amenable to treatment, they not unfrequently led to a fatal termination, when death most probably would not have occurred from the pulmonary disease alone.

Finding that the relation of cause and effect between these medicines, and the complications existed, there still remained a further inquiry respecting certain facts connected with these agents and their effects, which the mere existence of this relation did not account for. The facts are, that although the symptoms of these complications are little else than the expression of the usual toxicological effects of mercury and antimony on the gastro-intestinal mucous membrane, and on the functions of the liver, when given in small doses and long continued, they have, notwithstanding, been observed to occur much more frequently in pneumonia, in this climate than in some other climates; and, in this climate, much more frequently in pneumonia than in some other diseases. Now it is plain, that if the toxicological action of these two agents were alone concerned, this irregularity in their operation could not happen. There is then some other cause in operation besides the mere poisonous properties of the remedies. The immediate subject of inquiry is as to the nature of this cause? The properties of the agents and the nature of their effects lead us to seek it in the condition of the alimentary canal and of the liver. And here we are reminded, that the effects, very similar to those which take place in pneumonia, have been, perhaps, more frequently observed to take place in our endemic periodical fevers; and hence it may be inferred with great probability, that whatever may be the cause of the frequent development of the toxicological action of these medicines in the one, is also the cause of the same effects in the other. Now, it is well known that a state of irritation of the intestinal, or gastro-intestinal mucous membrane, and of functional excitement in the liver, are almost uniform conditions of our endemic disease. These conditions are indeed so general, that we may very properly conclude their production to be a law of the remote causes, whatever may be their nature, of the most important of our acute endemic diseases. Sometimes these conditions are openly manifested, while at others they are latent, existing rather as a tendency to take on inflammation in the membrane and functional excitement in the liver, when the former is acted on by irritative injestæ, whether of food or medicine, or the latter by its peculiar

stimulant—mercury. It is on account of the condition of these organs in our endemic diseases, that tartar emetic has never become established as a remedy in any of them except pneumonia, and in fact, also, that mercury has been for a great length of time slowly falling out of use, particularly in periodical fevers, when its deleterious effects are most obviously and most frequently displayed.

That pneumonia falls within the operation of this causative influence might be inferred from the similarity of the effects of these two irritant poisons in this and in other endemic fevers; and we might, I think, rest on this inference as on an assured fact, if there were no direct evidence to support it. The induction, however, is supported, nay verified, by some direct evidence in the primary symptoms of this disease. While it is true, that the gastro-intestinal irritation is not so often shown primarily by prominent signs, such as nausea, epigastric tenderness and diarrhœa in pneumonia as in periodical fevers, yet there is still sufficient evidence of its actual presence, though, as it were, latent, in the state of the tongue, and the uniform susceptibility of the bowels to the action of purgatives; and so too in regard to the evidences of over action of the liver, the signs of it are generally obscure in pneumonia, hardly at all manifested, unless vomiting or purging are provoked by medicines; but at times there are cases of pneumonia, in which the evidences of a bilious diathesis are very prominent, and in all, this diathesis is more or less manifest, either by the primary symptoms, or by the immediate action of emetic or cathartic medicines. But I need not dwell on this point, the facts being, no doubt, familiar enough to the reader. Assuming, then, from all that has been said in this connection; in the first place, that the production of a certain state of irritation in the digestive tube and liver, obvious or obscure is a law of the cause or causes of our acute endemic diseases; and in the next place, that pneumonia is no more exempt from the operation of this law than other endemic fevers, we are enabled to refer the more frequent occurrence of injurious effects from these medicines, and their greater activity, in a Southern climate, to the state of irritation so induced as their immediate antecedent, or cause, and thus to arrive at a satisfactory explanation of a number of well ascertained facts which otherwise seem inexplicable. The difficulty of tracing the frequency of these effects to its proper source hitherto, has consisted in two things; first, in the frequent obscurity of the signs indicating a proneness in the organs affected to take on inflammation, or a high degree of functional activity; and, secondly, in viewing the facts in regard to pneumonia in an aspect of isolation, and not in connection with similar facts common to several

varieties of endemic disease, and, as we conclude, referrable to the same cause.

We take up now the subject of *blood-letting*, which for the sake of perspecuity, as well as on account of its distinct nature and effects, required to be treated of separately. The objections that I have noticed in the course of my experience, to its employment to any great extent in pneumonia, have been presented in two aspects. In the first, a considerable mitigation of the symptoms has been obtained, which, lasting but a few hours, has been followed by a reaction in which the disease has passed beyond the point of severity it had previously attained; that is to say, the pulse became in the reaction more full, frequent, and sometimes harder; the respiration increased in frequency, while the restlessness and general feeling of malaise were aggravated. Along with these outward signs, there was commonly a more or less evident extension of the limits of the diseased parts. In this manner, the effect of one bleeding was to render the indications for another more urgent, and if repeated, it was again followed by temporary relief and an ultimate aggravation; results which I have known to follow repeated diurnal or semi-diurnal bleedings until the near approach of a fatal issue arrested the treatment. Such effects from bleeding, there is reason to believe, are not confined to any particular climate or locality. Of this, any one, I think, may satisfy himself, who will study carefully, in reference to this matter, the published clinics of the European hospitals, where he can hardly fail to find examples of this kind.

These results, however, have not there been attributed to losses of blood, but to other circumstances connected with the natural course of the disease. And here the question may be asked, how has it been ascertained that the latter opinion is not the correct one, or that the real error does not lie in attributing to bleeding what properly belongs to the disease itself? The answer is, and it will equally apply to what has been advanced in regard to the deleterious agency of other remedies, that this question has been submitted to that kind of test which logicians call the method of difference; that is, the circumstances of the disease being as nearly as possible the same, the supposed cause, namely, the bleeding, has been withdrawn from the treatment and the result noted. This test so certain ordinarly, is not so in this instance, unless greatly extended as regards time, and the number of cases observed. If the symptoms attributed to bleeding were uniform attendants in the cases bled, the case would be different, for then it would only be necessary to set aside the remedy in one, or at most a few cases and note what followed. But being only an occasional, though not an

unfrequent occurrence, it is impossible to say in one, or in a few cases, that the absence of the symptoms had any necessary connection with the suspension of the remedy. Hence arises a necessity, in order to arrive at any great accuracy in the conclusion, to apply the test, if successful in a few, to a great number of cases, and through a series of years. The test has been so applied; and the observations of many years have satisfied me, that sudden and violent changes for the worse, of the kind here spoken of, do not occur in the progress of pneumonia, unless bleeding constitutes an essential part of the treatment. Perhaps it would be more accurate, instead of stating the conclusion in such general terms, to say that under my observation they have not occurred in one hundred and thirty-two case, not bled.*

In the second of these aspects, no mitigation of the symptoms followed the bleeding, the immediate effect being to quicken the pulse and to enfeeble it; and, though there might be a subsidence of pain, the condition of the patient was in other respects altered for the worse. I take for illustration a single example of this kind of change from my note book now before me. A young man of good constitution, and before this attack in good health, was bled at 12 M,, on the fourth day of September, 1843, being the third day of his illness, to twelve ounces. While the blood was running the pulse rose to 120, becoming small and soft; no faintness; half an hour afterwards, pulse 132; at 6 P.M., pulse 125. The disease occupied the lower and middle lobes of the right lung; a part only of the diseased portion having passed into the second stage.

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* Some weeks after writing this and the preceding paragragh, I met by accident, and so far as I remember, for the first time, with an extract from Lænnec, which fully confirms my opinions in regard to the effects of bleeding in pneumonia. It is contained in Mr. Guthrie's tenth lecture, "On some of the more important points in Surgery," published in the June No. of the Lon don Lancet, for this year (1853.) The most valuable remark of Lænnec, says Mr. Guthrie, on its use, (*tartar emetic*) is, "that by bleeding we almost always obtain a diminution of the fever, of the oppression, and of the blood expectoration, so as to lead the patients, and the attendants to believe that recovery is about to take place; after a few hours, however, the unfavorable symptoms return with fresh vigor; and the same scene is renewed often five or six times after as many venesections. On the other hand, I can state that I have never witnessed these renewed attacks under the use of tartar emetic."

The common opinion, however, in regard to the cause of these changes is, I believe, correctly stated in the text, and I have not therefore, thought it best to alter it.

The unfavorable effects of this remedy have not appeared to be governed by any specific circumstances that can be appreciated in individual instances, so as to enable one to determine before hand the probability of a good or bad effect from it, independently of those general rules which apply to all climates, and to all diseases in which the remedy is employed.

But if the condition on which such effects immediately depend are inappreciable, or undiscovered, the more remote causes are not perhaps altogether so. The general experience of physicians is, that the loss of much blood is not so well borne, nor its curative influence so favorably exerted in this as in Northern climates. If this be generally true, of which I think there cannot be much doubt, there seems to be two causes in operation which make it especially true of pneumonia. The first concerns that class of our population most liable to its attacks, namely, the blacks, whose nervous and muscular tone, or force, is more easily acted on by depressing influences than that of the whites; requiring more animal food to sustain it in health, and giving way more readily to the impressions of cold, or fatigue, and, in disease, to any kind of active depletion. The second concerns a peculiarity in the disease itself common to both races. The peculiarity consists in this, that the disease hardly ever lingers in the first stage, but presses on to the second with a degree of rapidity which, while it constitutes the rule with us, is the exception in colder latitudes. It is not uncommon, for example, to notice the rust colored sputæ, with a well marked dullness, bronchial respiration, and bronchophony in some part of the diseased structure, sometimes over a large part of it, within the first twenty-four hours. On one occasion, I saw the characteristic sputa at the very beginning of the attack, even before the cold stage which ushered it in had passed off. It is much more common than otherwise, to find the physical signs of the second stage fairly developed on the second day. And thus it happens, the greater number of first visits being made on the second day, that when the physician come to prescribe, he finds his patient in that stage in which the propriety of bleeding at all, is held by many to be more than doubtful, and by all to be much less effective for good.

Whatever may be the influence of these causes, in modifying the effects of bleeding in this climate, and among a slave population, certain it is, that any very obvious and permanent, as well as immediate benefit, is seldom obtained from it in this disease, while it is sometimes obviously and immediately injurious. Hence, it has come to be very sparingly employed by much the greater number of physicians of expe-

rience in this section of the country, and many dispense with it altogether; while among those who bleed most, not one, I suppose, could be found who ever entertains a thought of following out the *coup sur coup* plan, which Bouillard, it would seem, found so effective in France.

That either, or all of these potent remedies were frequently beneficial, I was not permitted to doubt. That they were often extremely deleterious in the ordinary way of using them, seemed to me to admit of as little doubt. The latter conclusion led to, and, I think, justified, (a matter, however, which I leave to the casuists in medical ethics) the institution of a series of observation in order to determine, first, the possibility of so employing them as to obviate their injurious effects, and afterward, failing in this, the practicability of finding efficient substitutes less liable to those contingencies, which, according to Dr. Boling,* referring, however, chiefly to antimony, are quite as formidable and fatal as the disease itself. It is not necessary to the object of this explanatory introduction to follow up in detail the successive steps, running through a number of years, by which slowly and even reluctantly, first one and then another of these old and familiar remedies were laid aside and others substituted. Suffice it to say, that after the conclusion was come to that a change of remedies was necessary, neither of the established ones was rejected suddenly or capriciously. The first steps in the experimental inquiry encouraging me to proceed, blood-letting and mercury came to be used only in certain circumstances, (the former, when the breathing was not only frequent but embarrassed by excessive pain, the latter, after the acuteness of the febrile symptoms had subsided, and the physical signs persisting, the disease threatened to become chronic,) and afterwards were dispensed with altogether. It is now about seven years since I have drawn blood in any mode, or prescribed mercury in any form or dose, in the treatment of this affection. Tartar emetic, by far the most valuable remedy of the three, was continued longer, because it was found on trial that its poisonous effects could generally, though not always, be

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*"Among the cases of pneumonia which we have treated with tartar emetic principally, we do not hesitate to say, that half as many deaths have occurred in consequence of gastro-enteritis—induced seemingly by the remedy—supervening during the progress of the disease, or at the moment of apparent convalescence, as from the primary disease itself."—*N. O. Medical and Surgical Journal, vol. 5, p. 291.*

avoided by giving it in large doses, repeated at long intervals, (two grains every third or fourth hour, dissolved in at least two ounces of water) and suspending it during eight or twelve hours at night. This, however, with the others was finally laid aside, giving place to a contra-stimulant more prompt and efficient in its action, and at the same time divested of all the deleterious qualities of the other.

The treatment of pneumonia, then, which was finally settled down on somewhat more than four years ago, and since, with some slight modifications, steadily pursued, consists in discarding the three principal remedies in common use, and substituting others in their stead, after the following manner:

On visiting, for the first time, a person of adult age having pneumonia, in the first or second stage, pleuro-pneumonia, or pneumo-bronchitis, I make the following prescriptions:

℞	Tinct. Aconitum Napellus (saturated)	gtt. xii.
	Quinine Sulph. vel. Ferro-Cyan.,	gr. xxxvi.
	Morphia Sulph.,	gr. i.
	M. ft. pil. xii.	
℞	Solution of Phosphorus,	gtt. xvi.
	Water,	℥ iv. M

Of the first, two pills are directed to be taken every third or fourth hour, usually every fourth, each dose being preceded one or two hours by a teaspoonful of the phosphorus mixture. If an anodyne be required in addition to that contained in the pills, a quarter of a grain of morphine is given at bedtime. If the disease is in the first stage, the beginning of the second, or, after the second stage is fully developed, if there be much pain, not yielding permanently to anodynes, a large blister is directed to be applied over the seat of the disease. Such is the outline; the details will be given in speaking of the remedies separately.

The preparation of Aconite used, is a saturated alcoholic tincture, made by percolating through a pound of the bruised root alcohol enough to make a pint of tincture. This obtains, if the root be of the right species, is unmixed, and not too dry or too long gathered, a stronger tincture than that of Dr. Fleming, of London; whose valuable paper on the therapeutic and toxicological effects of this drug first suggested to me its employment as a substitute for bleeding and antimony. The dose advised, two drops, may be considered a medium dose, when made up into pills according to the prescription, or a full dose if given in water. I speak here as well as in what follows of the tincture made from the best specimens of the root. But as our druggists hardly ever

get two successive parcels of equal strength, the dose requires sometimes to be raised to two and a half or three drops. This difference in activity is partly owing to a difference in the varieties, of which there are several, but also in part to the mode of cultivation, and the length of time it has been gathered, and the amount of exsiccation it has undergone. I am not familiar enough with the physical qualities of the several varieties of the A. Napellus, to furnish the means of deciding, by an examination, of a given specimen, what degree of effect is to be expected from its administration; the effect in kind, I believe, is the same, or nearly so, of every variety of this species. It is desirable, however, to obtain some guide in this respect, and though none may be afforded by the root itself, there is a mode of testing the activity of the tincture, which serves a very good purpose in selecting it for use. The best tincture, diluted in the proportion of an ounce of water in sixteen drops, taken into the mouth in a small quantity, produces a burning in the tongue and lips, with a feeling of tingling and numbness, and a loss of taste; the sensations lasting from two to eight hours. Diluted with twice this proportion of water the same effects follow, though less actively and durably. This test can be depended on only to a certain extent,—an article requiring, for instance, three drops for a full dose, could hardly be distinguished in this way from one requiring only two. One is able, however, to decide by it at once, between a good and a bad preparation. If a few drops of the tincture, diluted as first mentioned, produces no burning, no tingling, when applied to the tongue, the specimen should be rejected without hesitation; and so also of the weaker dilution, if a teaspoonful be taken into the mouth and retained there but a moment. Other things being the same, the root making a dark-colored tincture, is not so active as that which gives it nearly the hue of Madeira wine. A still more effective test may be found in a few tentative doses. If in a trial of this kind, the tincture has first been tasted, and found to produce the effects described, the experimental dose should not exceed, at first, two drops. No danger, certainly, is to be apprehended from a dose two or three times as great, but the effects of two drops are sometimes very disagreeable and even painful.

The curative influence of this medicine, though by no means dependent on doses sufficient to produce any poisonous effects, is, nevertheless, the more promptly exerted in proportion as the latter are developed within certain limits. It is desirable, therefore, to give enough, or to repeat the dose often enough to induce some nausea, or slight vomiting, particularly in the first stage of pneumonia. I believe, it is not needful to go farther, as a general rule, in order to get the best effects of the

remedy, without, at the same time, harassing the patient with its sickening influence. On some occasions, however, when the attack wears an unusually threatening aspect a more decisive impression may be required; and in such a case, it is better to repeat the dose more frequently, rather than enlarge it much. Under ordinary circumstances, then, let us say the development of the toxicological action of the remedy, to any great extent, is not desirable; for if the dose be too large or too often repeated, its effects may become exceedingly distressing to the patient, and alarming also to him and his friends. In such instances there occurs, more or less suddenly, a feeling of great prostration of strength and of sinking; coldness, palor, and profuse sweating of the skin; pretty constant, though not painful nausea; frequent efforts to vomit; purging; a slow, feeble, and thready pulse; and sighing respiration; to these symptoms there are added, a dryness, or rather the feeling of dryness, and constriction of the throat; burning, tingling and numbness in the mouth, and numbness in the skin of the hands and feet, and frequently over other parts of the surface. These symptoms I have witnessed, to the full extent described, in two instances, both adults, from a single dose of three drops, given in water. Hence, I have adopted the custom of beginning with a dose of two drops, in the pill form, and one and a half drops in water, increasing the latter to two drops in the second dose, and still more in the third, if required. The poisonous effects from the largest dose spoken of, even if very actively developed, are only to be dreaded on account of inconvenience to the patient; they always pass off in a few hours, never exceeding eight, without leaving behind anything injurious or unpleasant.

Children bear somewhat larger doses, in proportion to age, than adults. A child, six or eight months old, can generally take one-fourth of a drop without inconvenience, and one of twelve or eighteen months will frequently bear a third or half a drop; having reference in all that I have said, of the dose for adults and children, to its repetition, except in cases of unusual violence, at intervals of not less than three hours; but in no circumstances ought the intervals to be less than two hours; in children, I am not sure that it ought ever to be repeated so often. Ordinarily, its repetition once in four hours is sufficient.

The best effects of this remedy in pneumonia, like bleeding, is exerted in the first stage, or that of capillary repletion. After the second stage is completed, throughout the greater part of the inflamed structure, though not at all doubtful as a remedy, nor, indeed, any the less efficient or certain in its curative action than before, the latter is usually less promptly exhibited as regards both the rational and the physical

signs. As a substitute for bleeding, it seems to possess several other advantages. While it reduces the force and frequency of the pulse with greater certainty, though somewhat less speedily, its action in this respect may be kept up for any length of time required, without fear of present or subsequent injury from it of any kind; if suspended, there is no tendency to any violent reaction in the circulation, nor, indeed, to any speedy febrile reaction at all, the pulse coming up to the natural standard, after having been brought belows it, very slowly. Hence, if it be thought desirable for any reason to suspend its administration during the night, no fears need be entertained of finding the pulse materially accelerated the following morning. It may be added, in concluding this part of the subject, that, convalescence is never retarded through the influence of this remedy, and that unlike bleeding, it is safe, as well as efficient, in all circumstances of the acute disease, if used with but ordinary caution. If any serious harm results from it, the fault, I will venture to say, will lie with the physician and not with the remedy.

Phosphorus, it seems, from the accounts given of it in the books, is not a new remedy in inflammatory affections. It is said to have been employed in several diseases of this class, and among the rest, in pneumonia, but precisely under what circumstances, and with what success I have not been able to learn. It is generally acknowleged to be a remedial agent of great power, and available in the treatment of a great variety of morbid affections. It appears to have gone out of use on account of the dangerous and even fatal consequences resulting from its employment in the large doses usually recommended. It is proper to say, that I am indebted to the representations of its value by my friend, Dr. James Berney of this city, for my employment of this remedy in diseases of the lungs.

The solution referred to in the prescription is a saturated solution in anhydrous alcohol, diluted by nine additional parts of alcohol. This diluted solution is preferred for several reasons: It is not liable to undergo waste or change from exposure to the air like the stronger solution, which gives out a vapour of phosphorous every time the vial containing it is opened, this vapour, combining with oxygen of the atmosphere, forms hypo-phosphoric acid, a part of which being absorbed into the solution, may, to some extent, alter its medicinal as well as its chemical qualities. The weaker solution mixes better with water, and the dose for children as well as adults is more easily regulated with it. This last consideration, it will be seen presently, is of great importance. The dose recommended is the smallest that I am in the habit of using,

and is the one I prefer after many trials of larger ones. Authors recommend the oily solutions to be given in doses of from two to ten drops. Each drop containing about the one-hundred-and-fiftieth of a grain, the dose is found to vary from the seventy-fifth to the fifteenth of a grain. In these doses, small as they may seem, it is spoken of in many instances, as a dangerous and uncontrolable remedy, and cautions against mischief from it are everywhere numerous, urgent, and impressive. Dr. Chapman, referring to doses of a sixteenth of a grain, says—"Whatever may have been the degree of its utility, this appears to be fully balanced by the hazardous nature of the medicine, and the positive mischief which is acknowledged to result from it. Even in the very small doses of the prescription above, though always safe, and generally free from any unpleasant consequences, it occasionally produces some very sensible effects on the head and stomach. Given in what I suppose to be the minimum dose of authors, the seventy-fifth of a grain, these effects, according to my experience, are not only frequently produced, but with such additional activity as to require the suspension of its administration, and the dose on renewing it to be greatly reduced. The same thing is occasionally true of doses of one-fourth the quantity. Dr. John McLester, Dr. Taylor, Dr. Hinkle and Dr. Oliver, all of whom have witnessed its effects, in doses varying from half a drop to two drops of the saturated solution, (from five to twenty drops of the diluted solution,) concur with me in the opinion, that the medicine cannot be continued in the smallest quantity just mentioned for any great length of time without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat and a feeling of oppression at the epigastrium; and that in the larger quantity, though a single dose, or perhaps a few doses, may be given with impunity, it cannot be continued for any great length of time with ordinary, or at least with a proper exercise of prudence. If therefore we suppose, as is probable, that anhydrous alcohol will hold in solution as much phosphorus as the æthereal or other oils, it is seen that the minimum dose of authors is much too large for ordinary use in diseases that require a frequent repetition of remedies, and that much watchfulness is required to render it even safe. So far in regard to its activity merely; but in estimating the proper dose, several other things require to be taken into consideration, having reference to certain peculiarities in its operation: First, the eccentricity of its action as a poison; thus while it is said on good authority to have been given, at times, in doses of several grains without doing serious mischief; at other times less than one-tenth of a grain (six milligrammes) has been known to prove

fatal.* Secondly, its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome, or even dangerous after several repetitions, at intervals of three or four hours; this quality was developed in one instance by repeating it, in a dose of two drops of the strong alcoholic solution, three times, at intervals of twenty-four hours. Thirdly, unlike most other therapeutic agents, its medicinal and its toxicological action are, in a certain degree of the development of the latter, antagonistic; so that in proportion as its toxicological powers are brought into active exercise, so are its medicinal virtues diminished, and thus it is found that its curative effect is not in the ratio of the quantity administered, except within much narrower limits than are prescribed for other poisonous remedies. It may be concluded, therefore, from all that has been said, that if it be desirable, as in this instance it certainly is, to obtain the curative, without danger of developing the poisonous properties of this agent, the first object can be very surely attained by giving it in nearly the doses recommended, small and even minute as they certainly appear, when the activity of the poison is not fully appreciated, while the evil can be hardly avoided by giving it in much larger ones.

In dwelling so long on this subject of the proper medicinal dose of phosphorus, I have been influenced by the double conviction, in the first place, of its great value as a therapeutic agent in other diseases, as well as in the one now under consideration; and, in the next place, of the facility of doing mischief, by a careless or improper use of it. There is another reason: Both the nature of the remedy, as such, and the limits of safety in giving it, have hitherto been greatly misunderstood, and consequently greatly misrepresented; a matter which I shall have occasion to refer to again presently.

If the medicinal qualities of aconite adapt it more especially to the first stage of pneumonia, so, it may be said, those of phosphorus recommend it more particularly in the second and third stages. Given alone in the first stage, it is occasionally effectual in arresting the further progress of the attack, but cannot be depended on for this purpose

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* *Cazenave.* But this author thinks that in all cases in which such large doses have been given without harm, the article had undergone some change in its chemical state, which rendered it inert. "Si l'on a pu dire qu'il a été administre avec innociuité à la dose de 3, 4, 5 et 6 decigrammes, on doit croire que, dans ces cas, il y avait décomposition et change dans son êtat chemique." My own experience of its effects most certainly leads to the same conclusion.

with nearly the same confidence as aconite alone. More frequently it does not prevent the second stage from forming, and decided signs of amendment are commonly deferred to the fifth or sixth day. While either phosphorus or aconite, within my experience, is more efficient in either the first or second stage than any other single remedy, it is nevertheless desirable to obtain their combined action, as being more efficient than either separately. In regard to the value of phosphorus in the third stage, I can speak only from a very limited observation, and that chiefly in cases under the treatment of others; but so far as this goes, the result has been highly favorable.

In concluding what I have had to say of phosphorus, I wish to add a few words concerning its therapeutic qualities and what I conceive to be the mode of its operation. In doing so, I hope to place this article in class with those articles of the Materia Medica with whose qualities it is most closely assimilated, and where, consequently, it properly belongs.

Phosphorus certainly acts as an expectorant, with great promptness and efficiency in pneumonia and bronchitis, in some forms of asthma, and in the bronchitis of asthmatic subjects. It is also an effective remedy in irritation about the neck of the bladder; in chronic or subacute inflammation of the inner membrane of the urethra and bladder; and also, as a diuretic in dropsy. Its action on the lungs seems, from its effects, to be directed especially to the minute bronchial tubes, and the air cells; and in inflammation, more especially to the capillary vessels than to the heart. In all this we recognize a kind of speciality in its operation, which likens it to a number of medicines, such as mercury, cantharides, digitalis, and others, which seem in the same manner to act on certain organs, parts of organs, or secretions, in preference to others. But we may, I think, proceed a step further, and inquire into the mode of its curative action.

It is well known that under certain circumstances the remedies for inflammation are required to be of a stimulating nature, not merely locally, but generally, and may be required to be of the most active and diffusible kind. It may be added, that in one point of view nearly all the most efficient remedies for inflammation are stimulants. The pathology of inflammation explains this seeming paradox. The phenomena of inflammation are now known to be derived from an engorgement, or "repletion in excess" of the capillary vessels carrying blood; the repletion being itself dependent on a deficiency in the organic, contractile force, which in health propels the blood, in part at least, through these vessels. This force is to a considerable extent

regulated (not imparted) by the nervous influence carried along the nerves distributed on the vessels; consequently the organic force, under influences derived from the nervous system may be impaired, or aggravated, or possibly otherwise disturbed by causes acting either generally through the nervous centres, or locally on the nervous filaments themselves. Now, in this view of the proximate cause of inflammation, (engorgement of the capillaries) its immediate antecedent being a deficiency of contractile force in the capillaries, remedies for inflammation ought to be stimulants, at least in their local action on the part inflamed. This is indeed true of nearly all the so called contra-stimulants, and would be true of all remedies for inflammation, if the organic force of the capillary vessels were the only force concerned in circulating the blood. Taking, however, the contractile power of the heart into consideration, we have to add to the list some remedies, active and efficient ones too in some circumstances, which are in no sense stimulant. These act exclusively to reduce the injecting force of the heart; and these alone, therefore, in a pathological classification of remedies for inflammation, can be ranked as pure sedatives. The list of this class is small; blood-letting being its best representative, and veratrum viride, and digitalis, probably next in rank. Leaving these out of consideration, because the received views of the pathology of inflammation being admitted, their operation must be indirect, and merely adjunctive to the others, we may, for the purpose of better understanding their relation to this branch of pathology, divide the others into three classes.

The first that I shall mention belong to the diffusible stimulants. Their operation is on the nervous system generally, increasing by this means the energy of the action of the heart, and of the secretions, and at the same time augmenting the power of voluntary muscular contraction. They have no especial local action on the capillary vessels; or if any, they tend rather to relax them. They are applicable only in those states of the system, where local inflammation co-exists with a depression of vital power, a deficient action of the heart, and of the nervous force of the capillaries; the organic force of the latter remaining normal, at least in its capacity to act. They are injurious in all cases of inflammation where the injecting force of the heart is equal to the propulsion of the blood through the capillaries in their normal state; the vis a tergo imparted by them in such cases increasing the repletion, while the agents themselves exert no compensating effect on the local organic force of the capillaries. In this respect they differ from

the class next to be spoken of. The best representative of this class is alcohol.

Those of the second class are also medicines which stimulate the nervous system generally, and through it the heart's action also, but moderately, but at the same time have an especial action on the organic force of the capillary vessels. Thus the local nervous power, stimulated through the nervous centres, and the stimulus of these medicines to the organic force of the capillaries co-operate in doubly compensating for the disadvantage of the slight additional injecting force of the heart imparted by them. This class includes a great number of individuals, and may be subdivided into several groups, as, first, tonics, quasi stimulants, such as mercury, iodine, colchicum, and others; secondly, narcotics; thirdly, the pure tonics; and lastly, some of the astringents.

The third and last class consists of such medicines as combine the properties of a sedative to the heart's action, and of a stimulant to the contractile force of the capillaries. These properties make them, as they have proved to be in practice, especially applicable to and efficient in acute inflammation and fevers; though applicable in all cases whether chronic or acute, in which the vital power, and the force of the heart's action are equal to, or above the standard of health. In this class may be placed in the order of what I conceive to be their relative value in acute inflammatory affections generally; first *aconite;* secondly, *antimony;* thirdly, *phosphorus;* fourthly, *quinine*. *Aconite* takes precedence of all others, because, so far as my experience goes, besides its greater efficiency, its application does not require to be limited by any peculiarities in its operation, nor by the character of the organ affected. It is proper to add, in connection with this last remark, that my experience in its use is limited to inflammation of the brain and its meninges, of the throat, of the lungs and pleura, peritoneum, intestinal mucous membrane, whether attended with dysentery or diarrhœa, rheumatism, chronic and acute, rheumatic gout, erysipelas, acute cornitis and conjunctivitis. *Antimony* comes next in order, because so far as any thing is known of phosphorus, the former has the advantage of being available in a greater variety of inflammatory affections, although it is believed to be much less efficient in those to which both have been successfully applied. *Phosphorus* is put in this class solely because of my own experience, and that of a few others, of its immediate sedative, or contra-stimulant influence on the general circulation, when given in a dose large enough to produce any sensible influence of any kind on the action of the heart, but still not large enough to excite inflammation or

a high state of irritation of the stomach and bowels. Its sedative or contra-stimulant, is its medicinal or therapeutic effect. Its poisonous effect is the reverse of this, namely, highly stimulant by reason of the local inflammation it excites. In this way is brought about the antagonism between its effects in large and small doses. There is a point at which it ceases to be medicinal or sedative, and becomes poisonous or stimulant. Thus it is not possible to produce by it the extreme depression which follows large doses of aconite; for when the dose is enlarged for this purpose beyond a certain point, a new and opposite action is immediately set up, by which the power is lost or merged in the local inflammation and its concomitant influence on the nervous system and the general circulation. In pneumonia it is not often that the quantity in which I usually prescribe it exerts any immediate influence in reducing the frequency or force of the pulse. Its action is more slowly developed in this respect than either of the other remedies included in this division, but at the same time its remedial influence is more certainly and uniformly obtained than that of antimony or quinine and hardly less certainly than that of aconite.*.

The reader is aware, no doubt, that the properties here ascribed to phosphorus do not accord with those ascribed to it by the profession generally. It is said by all those who have published an opinion about it, so far as I know, to have no other therapeutic qualities than those of a diffusible stimulant of the most active kind; and I suppose the impressions of nearly all others who have given any attention to the matter, accord with what has been published about it. Perhaps the only exceptions are to be found among a few professional gentlemen near me, who, guided alone by their own experience, concur with me on this point. And here, in alluding to this discrepancy, I wish to say, that I am fully sensible of the responsibility of uttering as a new medical fact, that which is opposed to the standard authorities in medicine, and to the established opinions of the great mass of the profession.

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*Two young gentlemen, my personal as well as professional friends, have been recently engaged in some experiments to test the effects of phosphorus on persons in health, they themselves being the subjects of the experiments. These gentlemen (Dr. Pollard and Dr. Oliver) found that a single dose of two drops of the saturated alcoholic solution invariably reduced the force and frequency of the pulse. The changes in frequency ranged, in the number of pulsations to the minute, between eight and twelve beats. A change was perceptible within about twenty minutes, which reached its maximum in from an hour to an hour and a half.

Certain, however, of the correctness of my own observations, to say nothing of those of my professional friends just alluded to, whose capacity to observe and truthfulness do not, to my conception, admit of a shadow of doubt, I have no hesitation in stating the result of those observations, being satisfied that a more widely extended experience can but the more certainly correct the common error. This common error, as I have unhesitatingly assumed it to be, is a remarkable one, and deserves, I think, something more than a passing allusion to it. It is remarkable in this, that relating to a matter of fact, viz., the medical properties of a remedy; one of pure observation, or at least, one which could truly be derived only from observation, it had its origin in a community of scientific observers, obtained universal credence among them, and has held its place there through several generations unquestioned; in this time a great deal has been written about it, and of course a great deal observed, such is the inference, and thus it has all the time been liable to instantaneous correction, without having been corrected. But is the mistake really one of observation as it appears to be? Let us see if the medical history of phosphorus does not afford some explanation less discreditable to medical experience, and to medical authorities?

And first as to the way it was introduced. No one, it seems to me, can read much of what has been written on phosphorus without coming to the conclusion, that its medical action had not been studied with the caution required by the nature of the agent, nor with the care due to the successful introduction into practice of any new remedy. On the contrary, it appears to have been rather suddenly introduced, soon after its discovery, not on account of any observations, accidental or otherwise, of its medicinal virtues, but rather from a priori considerations connected with its ascertained poisonous properties, and its inflammable nature as a chemical agent. As a poison it is a stimulant; as a chemical agent it is highly inflammable. The reader will remember that the principle of phlogistion, (to inflame) existing in dead matter, and that of inflammation in living matter, were held to be identical. Now as phosphorus contained more of this principle than any other substance in nature, and observing its effects as a poison, it was natural enough to suppose it to be an active stimulant under all circumstances, capable of imparting its *phlogistion* to the living body, and thus to excite an active *phlogosis*, or inflammation, local as well as general; it was in fact inferred, a priori, both from its combustible or inflammable nature, and from the symptoms induced by poisoning with it, to be a powerful *phlogistic* agent. Such, in brief, seems to have

been the reasoning; and the conclusion thus obtained could not fail to be confirmed by the experience obtained by the mode in which it was administered; and thus what is known of it has come down to us with a prestige of authority which in appearance is hardly questionable, and still confirmed, no doubt, by later and later experience to this time. In truth it could not be otherwise, so long as physicians acted under the weight of subsequent authority, which supports the original opinion of its properties. Being prescribed, or recommended, chiefly, perhaps only, in those states of disease which required or was supposed to require active diffusible stimulants, the quantities necessary for exciting this condition were given. The consequences were deplorable; but at the same time, the fault seems to have been, not in observing wrongly, but in binding down the observations to a hasty and altogether untenable hypothesis. Latterly, since the article last went out of use, the opinions of the older authors appear to have been blindly copied by their successors; or if their truth has been tested, it has been under the same influences, with the same object, and in the same doses. On the whole, therefore, I think it may be safely concluded that the true properties of phosphorus have never been put to the test of unbiassed observation, by any considerable number of physicians.

In view of the dark episode in medical history exhibited in the use of this medicine, we are not permitted, therefore, to wonder that the hopes excited by the hypothetical notions entertained of its qualities were never realized, nor that its use was speedily abandoned after a first trial; nor for a moment to regret that, taken up again and again with renewed energy and hope, but on the same insufficient and deceptive grounds, it was again and again abandoned as an unruly and dangerous remedy. The whole of the brief history of its use and abandonment is indeed highly instructive; furnishing, as it does, an example of a pure a priori and rational practice carried out more speedily and palpably than any other on record to its legitimate results; first unmeasured injury to human health and life; then the abandonment of the remedy (or treatment), and finally the explosion of the hypothesis which afforded the deductive authority for its employment.

Quinine constitutes a very important part of the treatment of pneumonia in this climate. If it be sometimes inefficient, it is also at times indispensable. In an affection simply inflammatory, as I suppose pneumonia usually is in cold climates, and often is in this, though never a tonic or stimulant in any dose, its sedative influence is too feeble to be available for much good; and if such cases could always be distinguished, quinine might very well be dispensed with in them. On the

other hand, when a malarious taint is an obvious complication, or when the pulmonic disease seems, as it were, engrafted on an intermittent fever, no other remedy can be so confidently depended on. But this taint may exist, while the signs of it are so masked as to be detected with great difficulty. The excess of fibrine in the blood, the violence of the local inflammation, and the exalted state of the innervation, may overcome the tendency of the cause of periodical fever to manifest itself in the usual way. To this it may be added, that in this latitude, whenever intermittent fever is endemic, there is more or less of periodical disease in every season of the year, assuming most frequently the form of fever, but often of other forms of periodical disease. Residents of such localities are, in fact, all the time subjected to the influences out of which proceed a class of diseases, *a quinquina*, as the French have it, which are amenable to treatment by the bark and its preparations. Hence, it becomes a safe rule to begin the treatment of pneumonia in malarious districts (so called) by making quinine a component part of it. My custom therefore is, to give it in the manner above specified, until its peculiar effects on the head become very well marked, and if the signs of amendment are not then satisfactory, or such as may be properly attributed to the quinine, to discontinue it, and give the aconite in water.

Blisters are also important adjuncts in the treatment of pneumonia. Systematic writers, drawing their experience from places north of 36 degrees of north latitude, in this country, and in Europe, are almost unanimous in proscribing blisters before the violence of the inflammation is subdued by bleeding, or has worn itself out. Here, however, in pneumonia, as well as in other inflammatory diseases, this rule is reversed. Blisters are found to be most beneficial applied in the first stage of pneumonia, and the earlier the better. So applied, they never seem to produce any general irritation, are prompt in relieving pain, and, there is reason to believe, assist in resolving the local inflammation.

Morphine (or opium) besides its occasional use as an anodyne merely, is introduced in the plan of treatment in order to effect two special objects. One of these is to prevent any irritation of the bowels, or to remove it if present. We have seen that the tendency of the disease is to take on this kind of complication, which, when it occurs, always aggravates the danger of an attack, as well by its unfavorable reaction on the primary affection, as by the presence of disease in two vital organs instead of one. The predisposition to an enteric complication is sometimes so strong as to be developed into active disease

from very slight causes ; even quinine, slightly irritative as it is, may produce this effect. Hence the advantage of combining the opiate with it, and of so timing the doses of the opiate as that its quieting effect on the bowels may be continuous. One of the effects of this mode of administering the opiate is, to secure a constipated state of the bowels, which I think is always desirable. The rule being to keep the bowels quiet, cathartic or even aperient medicines, form no part of the treatment. If a diarrhœa happen to complicate the attack from the beginning, the treatment for the primary affection is usually found sufficient to remove it very soon ; otherwise I interpose a few doses of the acetate of lead and opium.

The other object is to prevent inflammation of the pleura. This complication, at least in an active state, is by no means common in the beginning of an attack, although a predisposition to it is very common. Thus, while my notes show but one instance of a pleuritis marked by physical signs in sixty-eight cases, the predisposition to it, evinced, by acute pain, was evident in nearly the whole number. It is not, therefore, as I conclude, at the beginning, that a pleuritis occurs in pneumonia, but some time afterwards in the course of an attack. So strong, however, is the predisposition, and so frequently is it developed under the usual treatment, that very few dissections are made after death from pneumonia, which do not exhibit the evidences of it. This is, indeed, so much the case, that Andral proposed to prefix the word *pleuro* to pneumonia, for its common name. I find also that other authors come very nearly to the conclusion of Andral, that pleuritis is an essential part, or an invariable complication of pneumonia. Nevertheless, the fact is that the physical signs of pleurisy are very seldom exhibited in the beginning of an attack. A pleuritic pain, however, is seldom absent. This pain is, for the most part, in the first stage, (perhaps always in the absence of the physical signs of pleurisy) a pleurodynia merely. This may depend on a repletion of the capillaries, not yet amounting to inflammation, or at least not active enough to produce the products of inflammation, but enough so to excite a painful irritation in the nerves ; or, which I think more probable, the relation may be reversed ; the capillary repletion, and the consequent development of an active inflammation, being dependent on a precedent irritation of the nerves. However this may be, the presence of acute pain is a sign of the predisposition, and the severity of the one is ordinarily the measure of the strength of the other ; while on the other hand, the removal of the pain may be depended on, as a general rule, as the evidence of the removal of the predisposition also. That is to say, speak-

ing only from my own experience, whatever will remove the pleurodynia permanently, its cause going with it, will also remove the disposition to pleuritis Hence, I have been disposed to look on the pain of pneumonia as a most important object to be cared for, independently of the suffering from it, and to regard opiates as a most important adjunct in the treatment; if for this reason alone, that it is more efficient in removing the pleurodynia of pneumonia than any thing else in common use.

By the judicious use of opiates, then, we expect with confidence to ward off two sources of difficulty in the treatment, and of danger in the result. Occasionally, however, the additional aid of a blister may be required; when this is the case, the blister should be applied, not over the seat of the pain, for this may be in the side not otherwise affected,* but over the seat of the inflammation of the pulmonary parenchyma.

Of the advantages of opiates in alleviating the feeling of restlessness and malaise, in mitigating a violent and harassing cough, in quieting an oppressed and hurried breathing, and in procuring timely sleep, in this disease, I need not speak, inasmuch as the name of opium is associated in the minds of physicians with all such kindly, and, to a certain extent, curative influences. The fears so often expressed by systematic writers in regard to the use of opiates before the acute febrile excitement is subdued, and the apprehension of their aggravating the inflammation by checking expectoration, seem to me altogether unfounded; except, indeed, as regards the latter, under the following circumstances:

It sometimes happens that, late in the attack, usually after the seventh day, a copious expectoration sets in, which is evidently a secretion from the larger bronchial tubes; at least it resembles such a secretion, and not that which previously took place, being white, frothy, and only slightly glutinous; it is discharged in considerable quantities by an almost incessant cough, which is accompanied by a loud and coarse mucous rale, and a feeling of suffocation; at the same time the face is pale and anxious, and the pulse small and quick. I have for some years looked upon this state of things as denoting a favorable crisis, and experience has taught me that it is not altogether safe to interfere with it by opiates at or near the beginning of it. After a few hours the expectoration may be safely stopped, and then is not likely to return in excess, which it will do after the effect of the anodyne has worn off, if checked too soon. In all other circumstances, opium is, within

* See note to case 50.

my experience, a safe, highly useful and pleasant adjunct, the more valuable and efficient when the inflammation is most acute.

The results of the treatment here recommended are shown in the annexed table, which is a record of all the cases of pneumonia treated by me during the last four years, including also all that were treated by Dr. John McLester while associated with me in practice in 1849 and 1850. In noting the cases, the rule was followed of excluding all cases of disease of the lungs in which the diagnosis of pneumonia could not be clearly made out, as well as those in which the pulmonic inflammation was not the primary and predominant disease. For example: Those cases which we occasionally meet with among children, in which the predominant disease being bronchitis, some inflammation of the pulmonary parenchyma may be suspected from the rapidity of the breathing, and some peculiarities in the expression of the countenance and in the character of the cough; but the physical signs of this affection are absent, and there is no visible expectoration to guide us in the diagnosis; somewhat similar cases occurring among adults, in which, as in children, we may suspect the existence of pneumonia, chiefly from the frequency of breathing, but there are neither the physical signs nor the characteristic sputa to justify it; the pneumonia, if present, being probably limited to a small part of the deep seated structure of the lung, or to small points scattered through it. Two cases of this kind, cases primarily of acute bronchitis, have come under my observation, one of which proved fatal; no opportunity having been presented for a post mortem examination. And lastly, cases of typhoid fever, in the course of which pneumonia has supervened. In these cases I have been in the habit of treating the pneumonia in the same way as when idiopathic, and it has been common to see this complication yield happily to the treatment, while the primary affection held on its course.

It is to be observed, also, in further explanation of the statistics, that the beginning of the attack is fixed at the time of the first febrile movement, accompanied by cough and pain, or a well marked soreness in the chest; and the termination at the time when all treatment was discontinued. When the treatment, or any part of it, was continued on the day of the last visit, this day is included in the estimate of the duration of the attack, whatever may have been the extent of improvement in the disease; when, on the other hand, the last visit has been one of observation and inquiry merely, it has not been included in the estimate of time. M. Louis dates the termination of an attack from the time the patient begins to take food; three days, at least, according to

STATISTICS OF PNEUMONIA, AND THE RESULTS OF TREATMENT.

Name.	Number.	Age.	Male.	Female.	White.	Black.	Date of Attack.	Date of First Visit.	Discharged.	Deaths.	Days of Treatment.	Duration of Disease.	Seat of Disease.	First Stage.	Second Stage.	Remarks.
1849.																
Westcott,	1	4m's	1			1	Apr. 25	April 29	May 2		4	8	Upper lobe of left lung, anteriorly.		1	
Pitkins,	2	5y's	2		1		May 7	May 7	" 9		3	3	Right lung, lower lobe.	1		
Larkin,	3	3		1		2	June 7	June 10	June 15		5	8	Left lung, lower lobe,		2	
Amason,	4	26	3			3	Aug. 5	Aug. 8	Aug. 8		3	4	Right lung, lower and middle lobes.	2		
Freeman,	5	4	4			4	Sept. 2	Sept. 3	Sept. 7		5	6	Right lung, lower lobe.		3	
Wood,	6	22		2	2		Oct. 24	Oct. 25	Nov. 10		17	18	Double, clavicular and sub-clav. region of both lungs.		4	Pleuritis in right clavicular and sub-clavicular region, the loudest friction sound being under the clavicle. Parturition six weeks before the attack, pale, sallow and anasarcous. Bellows murmur in right side of heart, which persisted after recovery. Had been subject for several years to occasional attacks of rheumatism.
Graham,	7	36	5			5	" 31	Nov. 1	" 12		12	13	Right lungs, lower and middle lobes.		5	Delirium tremens after fourth day.
Vandeveer,	8	3	6			6	Nov. 1	" 3	" 9		6	8	Right lung, lower lobe.		6	
Wood,	9	22		3	3		Dec. 16	Dec. 16	Dec. 25		10	10	Left lung, entire.	3		
Stewart,	10	3	7			7	" 25	" 29	Jan'ry 2		5	8	Left lung, upper lobe, posteriorly.		7	
1850.																
Whiting,	11	58	8			8	Jan. 1	Jan. 2	" 10		10	11	Right lung, upper lobe.		8	Had been drinking to excess some days before the attack.
Larkin,	12	3		4		9	" 3	" 3	" 5		3	3	Right lung, upper lobe, posteriorly.	4		
Allen, Alfred,	13	15	9			10	" 15	" 23	" 25		3	11	Left lung, entire.		9	
Allen, Anthony,	14	14	10			11	" 19	" 23	" 25		3	7	Right lung, middle and lower lobe.		10	
Westcott,	15	20		5	4		" 25	" 26	" 28		3	4	Left lung, clavicular and sub-clav. region.		11	
Hobbie,	16	55		6	5		Feb. 5	Feb. 6	Feb. 16		10	11	Right lung, middle and lower lobes.		12	
Westcott,	17	20		7	6		" 9	" 12	" 15		4	7	Right lung, sub-clavicular region.		13	
Chisholm,	18	28		8	7		" 10	" 10	" 20		10	10	Right lung, lower lobe.		14	
Taylor, Tom,	19	48	11			12	" 11	" 13	" 22		10	12	Right lung, entire.		15	Preceded by cholera seven days—succeeded by hepatitis and jaundice.
Taylor, Rose,	20	20		9		13	"						Right lung, lower and middle lobe.		16	
Taylor, Jack,	21	35	12			14	"						Right lung, lower lobe.		17	
Figh,	22	40	13			15	" 13	" 15	" 18		4	6	Right lung, clavicular and sub-clav. region.		18	
Freeman,	23	50	14			16	" 14	" 16	" 20		5	7	Right lung, lower lobe.		19	
Allen,	24	45	15			17	March 5	March 11	March 17		7	13	Right lung, upper and middle lobes.		20	
Hayne,	25	40	16			18	" 12	" 17	" 20		3	8	Double, lower lobe of left and lower, and middle of right.		21	
Laprade,	26	54		10	8		" 14	" 16	" 20		5	7	Right lung, upper lobe.		22	
Fair,	27	4	17			19	" 16	" 19	" 25		6	9	Right lung, lower lobe.	5		
Gilmer,	28	26	18			20	" 18	" 20	" 24		4	6	Right lung, lower lobe.		23	
Whittaker,	29	33		11		21	" 25	" 27	April 6		10	12	Double, lower lobe of both lungs.		24	
Thorington,	30	30		12		22	April, 1	April 2	" 7		6	7	Right lung, lower lobe.		25	
Taylor,	31	44	19			23	" 11	" 13	" 15		3	5	Right lung, lower lobe.		26	
Pitkin,	32	2		13	9		" 8	" 10	" 12		2	4	Left lung, lower lobe.	6		
Pryor,	33	60	20			24	Oct. 28	Oct. 29	Nov. 3		6	7	Right lung, lower lobe.		27	
Freeman,	34	29		14		25	Nov. 25	Nov. 26	Dec. 4		8	9	Right lung, lower and middle lobes.		28	
1851.																Four months pregnant, miscarried 30th November, 6th day.
Bardwell,*	35	32	21			26	Jan'ry 9	Jan'ry 14	Jan'ry 16	1	3	8	Double, lower and middle lobes of right and lower lobe of left.		29	
Wilkinson,	36	26	22			27	" 27	" 28	Feb. 5		9	10	Right lung, lower lobe.		30	Treated the first five days by a steam doctor.
Randell,	37	22	23			28	April. 2	April 3	April 7		4	5	Right lung, lower lobe.		31	
Harris,	38	6	24			29	Sept. 16	Sept. 17	Sept. 23		7	8	Right lung, upper lobe, posteriorly.		32	
Eckels,	39	47		15	10		Oct. 20	Oct. 22	Oct. 30		7	10	Right lung, upper lobe.		33	Preceded three days by severe cephalalgia.
Pitkin,	40	10		16	11		Nov. 14	Nov. 16	Nov. 21		5	7	Left lung, lower lobe.			
Ogbourne,	41	27	25			30	Dec. 10	Dec. 11	Dec. 17		6	7	Right lung, entire.		35	Rotten egg sputa—highest range of pulse 130, resp. 65†
Lewis,	42	20	26			31	" 20	" 21	" 30		10	11	Right lung, clavicular and sub-clav. regions.		36	
1852.																
Harwell,	43	26	27			32	Jan'ry 8	Jan'ry 9	Jan'ry 18		9	10	Left lung, lower lobe.		37	
Hall, Henry,	44	34	28			33	" 15	" 16	" 22		6	7	Left lung, lower lobe.		38	
Hall, Samuel,	45	17	29			34	" 15	" 16	" 22		6	7	Left lung, lower lobe.		39	
Hall, Phœbe,	46	26		17		35	" 19	" 19	" 28		10	10	Double, sub-clav. of right and lower lobe of left.	7		Six months pregnant, miscarried on the fourth day—highest range of pulse 136, of resp. 74.
Hall, Amy,	47	49		18		36	" 20	" 21	" 28		8	9	Double, upper lobe of right and lower of left.		40	Bronchitis of both lungs—highest range
Hall, Charles,	48	32	30			37	" 21	" 22	" 27		5	6	Right lung, lower lobe.		41	of pulse 126, of resp. 64.
Westcoll,	49	2	31			38	" 31	Feb. 3	Feb. 7		5	8	Left lung, entire.		42	
Shaver,	50	9		19	12		Feb. 24	" 25	March 2		7	8	Left lung, upper lobe.		43	Very acute pleurodynia on right side, no pain on left—highest range of pulse 130, of resp. 52.
Chisholm,	51	3		20		39	March 8	March 10	" 16		6	8	Left lung, lower lobe.		44	Highest range of pulse 160, of resp. 70.
Jones,	52	7		21		40	" 20	" 25	April 4		10	15	Double, lower lobe of both lungs and middle lobe of right.		45	Highest range of pulse 146, of resp. 84—bronchitis of both lungs.
Randolph,	53	14	32		13		April. 12	April 13	" 20		8	9	Right lung, lower lobe.	8	46	Highest range of pulse 120, of resp. 40.
Goode,	54	4	33			41	" 14	" 17	" 24		8	11	Right lung, lower lobe.		47	" " " 152, " 56.
Westcott,	55	7	34		14		May 8	May 9	May 14		6	7	Left lung, lower lobe.			" " " 132, " 54.
Farley,	56	3	35			42	" 27	" 29	June 18		21	23	Right lung, lower and middle lobes.		48	" " " 140, " 64.
Ogbourne,	57	16	36			43	Aug. 4	Aug. 9	Aug. 12		4	9	Left lung, entire.		49	" " " 132, " 65.
Noble,	58	8m's	37		15		Oct. 5	Oct. 6	Oct. 12		6	7	Left lung, upper third of lower lobe.	9		
Sayne,	59	12		22	16		" 23	" 26	Nov. 2		7	10	Right lung, lower lobe.		50	" " " 124, " 66.
Freeman,	60	27	38			44	Nov. 4	Nov. 6	" 14		9	11	Left lung, lower lobe.		51	" " " 108, " 66.
Updegraff,	61	17	39		17		Dec. 15	Dec. 16	Dec. 20		4	5	Left lung, lower lobe.		52	" " " 130, " 48.—rotten egg sputa changed to rust-colored on the day of disease.†
Hassell,	62	24	40			45	" 17	" 18	" 21		4	5	Right lung, upper and middle lobes.			Highest range of pulse 96, of resp. 32.
Jones,	63	36	41			46	" 26	" 27	Jan'ry 4		9	10	Left lung, lower lobe.	10	53	" " " 100, " 32.
1853.																
Coxe,	64	46	42			47	Jan'ry 4	Jan'ry 7	" 13		8	10	Right lung, lower lobe.		54	" " " 120, " 38.
Randolph,	65	22	43		18		" 23	" 24	" 25		2	3	Left lung, lower lobe.	11		" " " 94, " 32.
Figh,	66	16	44			48	April 17	April 20	April 25		6	9	Left lung, whole of upper lobe.		55	" " " 104, " 48.
Coxe,	67	12	45		19		" 21	" 23	" 27		4	6	Double, whole of right lung and lower lobe of left.	12		" " " 144 " 56.
McKane,*	68	1	46			49	May 27	May 28	June 3	2	7	8	Double, whole of left lung, a lobular portion of upper, and upper third of lower lobe of right lung.		56	" " " 180, " 106.

* Some circumstances attending these two fatal cases required to be noticed, in order to render the table a true exposé of the effects of the treatment.

The first, (case 35) was treated by my associate, Dr. John McLester. I did not see the man, and consequently know but little more of the circumstances connected with the attack than is contained in the brief statement in the table, viz: that the patient had double pneumonia, and had been five days under treatment on the Thompsonian plan; that is, by active stimulants. To this I am able to add, that Dr. McLester's first visit was made sometime in the night of the fifth day, and that the patient died early in the morning of the seventh day, having been under treatment by Dr. McLester not more than thirty-six hours.

Of the second, (case 68) the following is a brief history. The child, one year old, playing on the edge of a terrace, ten feet high, fell over the edge and rolled to the bottom. Being in the charge of very young black children, no information could be obtained of the immediate effects of the fall. The next day he was attacked with convulsions, followed by high fever and a cough. It was in the afternoon of the next day, the second of his illness, that I first saw him. He was then comatose, with an ardent fever, cough, and hurried respiration, and the physical signs of pneumonia, as noted above. The head symptoms continued with little variation until the seventh day, but the pulmonic symptoms had, in the meantime, very much improved, the pulse having fallen from 180 to 124, and the respiration from 70 to 40 in the minute. There was at this time considerable remission in the head symptoms also; but in the afternoon of this day he grew stupid again, and by night, having become profoundly comatose, he was seized with convulsions, which continued, with short intervals, until he died. No post mortem examination was made. From the time the convulsions returned, the affection of the lungs, indicated by the physical signs, remained nearly stationary; the pulse, however, ran up to 180, and the respiration as stated to 106.

† So named from its resemblance in color, consistence and smell to rotten egg. This kind of sputa begun to change to the usual rust color, and to decrease in quantity on the third day of the disease; on the fourth day the change was completed. The matter was discharged in considerable quantity at a time, I suppose about half an ounce, and very frequently. The breath partook of the fœtor of the matter expectorated.

M. Grisolle, after the cessation of fever. M. Bouillaud, on the other hand, dates the termination from the time the fever had been decidedly mitigated, and other signs of convalescence had appeared, although the characteristic sputa, as well as some fever, remained—"à l'époque où les signes caracteristique de la pneumonie et le mouvement fébrile ont presque entièrement disparrée." I have followed neither. While I have put the termination further off than M. Bouillaud, waiting until the fever and the characteristic sputa had, not almost, but altogether disappeared, and the tendency to health was so well marked as to give unequivocal assurance of the safety of the patient, I have not, with M. Louis, supposed the disease to exist on account of the remaining dulness and crepitation, after all other signs had ceased, along with the fever; for after this, the dulness and crepitus ought, in my opinion, to be viewed as evidence, not of present, but of recent disease—the crepitus being in fact a sign only of convalescence.

The stage of the disease is represented as it was exhibited at the first visit.

II.—VESICO-VAGINAL FISTULA—ULCERATION OF THE OS TINCÆ—LEUCORRHŒA—CURE.

BY H. J. HOLMES, M. D., MISS.

Mrs. V. aged 26, of sanguine nervous temperament, the mother of two children, visited my infirmary on the 20th April last, for vesico-vaginal fistula and uterine disease. After a careful examination of the uterus and bladder, I detected a large ulcer seatedaround the os tincæ, embracing both labia of the uterus and uterine canal with leucorrhœa and engorgement—with the bladder, a transverse fistula three-fourths of an inch in length, occupying the bas fond and a portion of the urethra through which the urine had escaped for eleven years past.

A partial history and its treatment will comprise what I have to say of this case. At the age of fourteen this lady was married to a very worthy and respectable planter of the State of Louisiana, who proved to be one of the most indulgent and devoted husbands, as the sequel will show. Eighteen months after marriage she was confined with her first child, which proved to be a very tedious and protracted labor, continuing near three days and nights, during which time she was attended by Dr. ——, of the parish of ——, who it was said by the husband and lady to have shown but little skill in the management of the case; which finally resulted in the birth of a still-born infant. Extensive inflammation supervened, from which immense sloughing ensued of the walls of the vagina—finally healed by several cicatrices and closing the entrance to the vagina, which barely admitted a small finger at any time afterwards. Her periods did not return for two years; then made their appearance at intervals of two and three months for the space of four years—during which time they were very painful and scanty—and each time attended with very severe hysterical paroxysms, which continued from one to twenty-four hours. At or about the expiration of five years from the birth of her first child, an effort was made to cohabit, and although an entrance could not be effected, yet conception occurred. Having passed nearly through gestation, she was placed under the care of Dr. P., of L., who made several efforts to enlarge the entrance by frequent incisions through these hard cicatrices, but with partial success only; labor came on and continued four days, during which time frequent incisions were made through the ostincæ; in the meantime the efforts of the womb being very great, and with the use of instruments, delivery was at last effected of a fine healthy daughter. From this she recovered very slowly, but perhaps as well as could be expected under the circumstances.

The great injury she sustained in her first delivery, rendered her

exceedingly nervous and susceptible to these paroxysms, and from the slightest cause she could be thrown into one of them, from which, to all appearance, she could scarcely revive; and to guard against which, every means were resorted to by her attentive physician and indulgent husband. This case had been submitted to the care of several eminent surgeons as well as many other eminent practitioners, none of whom had been able to afford relief—and in this helpless and deplorable condition the case was presented to my notice. Being cognizant of this fact, I required three days to form and express an opinion. Perhaps while here, I had better give a more accurate description of the uterus and vagina, than that which I have already mentioned.

1st. A large ulcer seated in the neck around the os tincæ and extending into the canal.

2d. A very considerable engorgement of the neck and body.

3d. Neck hard and exceedingly tender upon touch.

4th. Leucorrhœa of a straw color adhering to the ulcer and uterine canal.

5th. Vagina sufficiently large and capacious; about three inches and a half in depth, from large old cicatrices, commencing about one inch within the vagina, half to three quarters of an inch apart and running backward and upward, meeting at a point where the neck of the uterus passed through the walls of the vagina, and had the appearance of being puckered or drawn together by a strong band.

6th. The perineum, labia majora, nympha, clitoris and urethra, all natural in appearance and in situ.

A very important matter with me at this time was to ascertain whether or not the womb could be probed, canal dilated and cavity reached; with this view I set to work with my metallic probes, varying in size, and on the third day I ascertained that I could pass the probes very well a distance of five inches with less pain than I had expected. All doubt having been removed in my being able to treat the womb thoroughly, I ventured to promise to cure both the uterine disease and the vesico-vaginal fistula. This opinion, though cautiously given, was viewed by herself and friends with a good deal of doubt.

Treatment.—My whole time for the first three months was devoted to the treatment of the womb and her general health.

1st. By drawing blood regularly from the neck of the uterus for two weeks.

2d. By dilating the uterine canal, which I found to be seven inches in length.

3d. Applying the nitrate of silver to the ulcerated surface; within the canal and uterine cavity with the *Porte Caustique*, and with the

solid nitrate of silver in a quill to the ulceration around the os tincæ and neck of the womb. This was repeated every fifth or sixth day, according to circumstances. The pain attending the use of the caustic was very severe, often ending in a severe paroxysm; to relieve which morphine and brandy were given internally and by enemata, warm fomentations to the abdomen, warm bricks to the feet, and sinapisms to the extremities. At the expiration of twenty-four or thirty-six hours, she would become quite comfortable, perhaps sitting up or walking about. These means improved the appearance of the ulcer, reduced the engorgements and improved her general health very decidedly. The severe paroxysms to which she had been accustomed having ceased for one month, together with the improvement in her general health, I concluded to embrace this favorable opportunity to operate for the vesico-vaginal fistula, and with her consent, invited my friends, Drs. J. J. Pugh and Wm. H. Thomson, to assist me. At the hour of 11, A. M., on the first day of August, in a large and spacious room, I proceeded to operate by placing her in a bed upon her knees and elbows before a large window. The lever speculum was introduced and held by Dr. Thomson, at the same time separating the right glutei muscles with fingers extending to the labia majora; the left side was attended to by Dr. Pugh; then by pulling the nates upward and outward and lifting the perenium, stretching the sphincter and raising up the recto-vaginal septum, the vesico-vaginal fistula was brought fairly to view. A slightly curved tenaculum served to hold it steady, while I inserted a sharp pointed bistoury one eighth of an inch from its margin, and then by a circular sweep I endeavored to remove this much all around. But the difficulty of carrying it on a straight line was often interfered with by the knife cutting through the margin into the fistula; however, by renewing my hold with the tenaculum, I succeeded after a time in removing a sufficient slip to answer my purpose. But little hemorrhage occurred during the operation, and that little removed often by one of my assistants, which enabled me to operate faster than I had expected. Having suceeeded in this part of the operation to my satisfaction, I next passed a spear-pointed needle around with a silk ligature passing it a quarter of an inch anterior to the margin; then withdrawing the needle and threading it again, I passed it through the upper margin at the same distance. This I effected much more easily than I expected by holding a piece of sponge attached to a small wire, which held it firm and solid. The needle was again withdrawn, leaving the thread, as intended; a second and third was carried, making three sutures in all. A small silver wire was then attached to each silk ligature, and carried in succession through the punctures I had made with the assist-

ance of a blunt hook, which was made to answer the purpose of the crescent shaped fork. The distal ends of the wire were withdrawn from the vagina sufficient for us to pass their ends through a small leaden bar, one and a quarter inch in length and one sixteenth of an inch in diameter, previously perforated with an awl; upon the ends of each I pressed a small shot, which had also been previously flattened and perforated, and upon which the ends of the wire were turned over so as to form a knob; the proximal ends of the wires were then pulled so as to bring the bar of lead above the fistula; another bar of lead of the same size and length was also perforated, and the ends of the wires passed through and pushed up with a blunt hook; by this means the raw edges of the fistula were brought directly in contact and secured by perforated shots being compressed on each wire, and the ends of the wire cut close to each shot with a pair of bone forceps. The operation being completed, a large dose of morphine was given, and the lady put to bed with a catheter two and a half inches in length in the urethra and bladder.

She complained more of the position in which she had been placed than the pain attending the operation. The first night and second day was passed quite comfortably; on the third morning I removed the catheter and washed away the slight secretion which had occurred up to that time. Every morning up to the tenth day this was attended to regularly, at which time I removed the bars of lead by clipping the ends of the wires without difficulty, and found the fistula healed perfectly. The catheter was worn for several days afterwards, when it was removed, and an effort made to pass the urine into a chamber. On my return to her room, I at once discovered her countenance radiant with joy, and the first word that greeted me was—"that there was now some pleasure in living, doctor; I find that I can retain my urine for any length of time and pass it at pleasure into a chamber, a thing I have not been able to do for eleven years past." It is here proper to remark, that I kept this lady in a horizontal position, on a light diet, and bowels confined, so as to give the parts an opportunity of healing by the first intention.

One month having passed since the operation, and finding that she had entire control over her urine, I commenced again the use of the caustic to the external surface of the neck and uterine cavity, at intervals of six and eight days for two months; at the expiration of which time there being no ulceration, leucorrhœa or engorgement, I discharged the case as cured.

It is due however to remark, that this lady has been a confirmed

dyspeptic, and the least imprudence on her part in eating, especially a piece of ham, a peach, or a fried fritter, she might expect with a degree of certainty that it would be followed by a severe spell of vomiting, and finally end in one of her worst paroxysms.

These paroxysms, she has often remarked, seemed to be a family disease; three or four married sisters are alike subject to them, and can be produced from any slight cause.

In conclusion, I must respectfully add, that I am mainly indebted to Dr. J. Marion Sims, of Montgomery, Ala., for a pamphlet on the treatment of vesico-vaginal fistula. In my operation I was guided principally by his remarks upon this disease; and to him all praise should be given for the great light he has thrown upon this hitherto obscure and intractable disease.

Oct. 28, 1853.

III.—THEORY OF MOLECULAR FORCES—EXPLANATORY OF THE GASEOUS, LIQUID AND SOLID CONDITIONS OF MATTER:

Read before the American Association for the Advancement of Science, Cleveland, Aug. 2, 1853.

BY J. L. RIDDELL, M. D.,
Prof. Chem. Med. Dep. Univer. La., New Orleans.

The following is a condensed abstract of the paper presented to the Association under the above title:

1. No system of philosophy can be true, which does not recognise the necessary continuity and equivalency of cause and effect.

2. All hypotheses are false which preclude rational explanation.

3. Fundamentally involving few and simple data, the problem of nature is one of infinite complexity.

4. The problem of nature has its geometrical projection in space, in the forms of material organization, aggregation and position.

5. The phenomena of nature must occur in a rational manner.

6. Natural laws must rest on a physical basis.

7. Matter and motion must be indestructible.

8. Beyond its passivity to motion or rest, matter cannot possess inherent qualities.

9. Force and power are strictly equivalent to, and identical with, momentum, which is *matter moving*.

10. Such attractions between individual molecules or bodies, as vary in intensity reciprocally as the square of the intervening distance, must be generally borrowed from the omni-directive impulses, which pursue their endless rectilineal paths, in the different coextensive systems of refined media, with which boundless space has ever been furnished.

11. The crude hypothesis, that the more simple molecules, like those of oxygen, water, etc., are usually revolving on axes with intense rapidity, enables us to form a conception of the possible cause, of what may be called polaric attraction and polaric repulsion. Similar, but more gross attractions and repulsions, arise from rotating solid disks in the air; when, if free to move, the disks arrange themselves in relation to each other, with coincident axes; each rotating disk acquiring an equatorial ring of repulsion, and centrally transverse thereto, two opposite poles of attraction. The experiment with the disks is easily made, and the origin of the polaric forces which manifest themselves, plain and easy to be fully comprehended. Thence by parity of reason, some notion may be formed of the probable mechanism, by which polaric molecular forces are exercised, crystalization and chemical action occasioned, and solidity or molecular fixedness produced and maintained; the passive agency of media more refined than air, being considered as instrumental.

12. The different media, as air, water and solids, transmit at all times, by the mutual impact of molecules, lines of molecular momenta in all possible directions, with a velocity normal to each case; say, in air near 1140 feet per second.

13. Molecular momentum is partly identical with heat.

14. We have a complex and partial apprehension of molecular momentum, in our sense of temperature.

15. We appreciate momentary wave-like disturbances of molecular momentum, recurring at appreciably short regular intervals as the tone of sound.

16. Repulsion among gasseous particles arises from their mutual impact, occurring in the transmission of molecular momentum.

17. The force of collisionary molecular repulsion in a gas, varies of necessity, in reference to a constant transverse plane as

$$\frac{1}{X^3}$$

X standing for the variable intermolecular distances.

18. The molecular forces in fluids are best expressed algebraically,

by reference to a plane regarded as constant in size, and transverse to the direction of the forces under consideration.

19. If between two molecules or particles of a fluid, at variable distance (X) from each other, the force of attraction vary as

$$\frac{1}{X^2}$$

between the molecules on opposite sidesof the constant plane, the attraction varies as

$$\frac{1}{X^4}$$

20. Owing *possibly*, in part to the variable amount of refined material envelope, appropriated at different mutual distances by each molecule; and in part *probably*, to the influence of polaric repulsion, which as explained, (11), would of necessity extend further from the molecular centres than polaric attraction; owing perhaps to these causes, the molecular attraction in fluids appears by observation, to vary, say within the exponential limits 1 to 4,

$$\frac{1}{X} \quad \text{to} \quad \frac{1}{X^4}$$

according to a continuously varying law; which law, by assuming z to be some direct and continuous function of X, may be thus expressed,

$$\frac{1}{X^z}$$

21. The equation theoretically expressive of the molecular equilibrium, existing among the particles of fluids, (liquids and gases) in reference to a constant plane, must have the following form:

$$\frac{1}{X^z} + \mathrm{P} - \frac{r}{X^3} = 0$$

Here X = the variable intermolecular distance.

z = a direct continuous function of X.

r = rate, or co-efficient of collisionary repulsion, dependent for its value on the temperature.

P = extrinsic pressure, as for example the weight of pressure o the atmosphere.

Assuming $P = 0$, r at any constant value as unity; and for convenience, assuming $z = X$*, the equation

$$\frac{1}{X^z} + P - \frac{r}{X^3} = 0, \text{ becomes } \frac{1}{X^x} - \frac{1}{X^3} = 0;$$

then as X varies from $X = 0$ to $X = \infty$, there will occur, among others, the several maximum and minimum values of

$$\frac{1}{X^x} - \frac{1}{X^3}$$

corresponding in a general and remarkable manner to what is experimentally demonstrable, respecting all the natural phases of balance or preponderance of the molecular forces, in liquids and gases.

Thus when

$X = 0$,	$\frac{1}{X^x} - \frac{1}{X^3}$	$=$ max. $= -\infty$,	Repulsion.	A.
$X = 1$,	"	" $=$ min. $=$ 0,	Balance,	B.
$X = 1.87$	"	" $=$ max. $= +$ Finite.	Attraction.	C.
$X = 3$,	"	" $=$ min. $=$ 0,	Balance.	D.
$X = 3.95$	"	" max. $= -$ Finite.	Repulsion.	E.

A. The molecular relations implied by value A, $X = 0$, are imaginary and impossible, since two or more molecules of the same order, cannot be supposed to occupy simultaneously the same space. From this, however, we can form some notion of the insurmountable barrier to indefinite compression in liquids.

B. With the value of $X = 1$, we have a molecular position of stable equilibrium, such as exists in the liquid condition of matter; exemplified by water, mercury, alcohol, etc.

C. In the case of the value of $X =$ near 1.87, we have the intermolecular distance, at which the greatest amount of separative force would be required, in the operation of causing a rupture of the continuity of a liquid mass. This is also the natural transformation point, between the liquid and gaseous condition of matter; whenever such

* The result is essentially the same, as to the succession of maximum and minimum values, with or without this assumption; but the special finite value of X, at which the maxima and minima occur, would be somewhat different.

transformation is caused by increase or decrease of collisionary repulsion.

Taking the varying intermolecular distance (X) as an abscissa, and the corresponding molecular forces lineally expressed as ordinates, so as to project the curves of attraction and repulsion, it will be seen tha these curves cross each other, normally, at $X = 1$, and $X = 3$; now by increasing the amount of repulsion, as by the application of heat to a liquid, these crossing points will approach each other, so that at $X = 1.87$, the curves will be tangent to each other. The condition of the liquid will now be one of unstable equilibrium, for by the slightest in crease, the force of repulsion predominates, and the liquid begins to assume the condition of a gas.

Since the limit of height to which liquids will ascend in capillary tubes depends upon the maximum limit of cohesive force subsisting between the uppermost row of liquid particles, and the particles below, it follows that the intermolecular distance between the upper and the subjacent row must correspond to $X = 1.87$.

D. The condition $X = 3$, marks the normal limit between the same substance as a liquid and as a gas.

E. The condition $X = 3.95$, is that wherein the repulsive force of a compressed gaseous body is a maximum; being the maximum limit of pressure required for condensing a gas to the liquid form. At this point, the repulsive force is nearly three and a half times greater than the force of attraction. At greater distances, such as we find exemplified in the molecular constitution of the atmosphere, the diminished force of repulsion alone manifests itself, while attraction, observing a more rapid law of decrease

$$\frac{1}{X^4}$$

becomes wholly inappreciable.

SUMMARY.

Solids. The molecules constituting solids, are held in a fixed relation to each other by polaric force. (11.)

Liquids. In liquids, the molecules are in effect held equidistant from each other, (X), by virtue of a balance between the force of molecular attraction, which in reference to an invariable plane, (18), varies as

$$\frac{1}{X^4}$$

(19), usually conjoined with a subordinate influence, external pressure, acting *centripetally*; and impingent or collisionary repulsion, which varies with the temperature and also varies as

$$\frac{1}{X^3}$$

(17,) conjoined with polaric repulsion, (11) acting *centrifugally*. Considering the polaric repulsion as added to the molecular attraction, then between the limits $z = 1$ and $z = 4$, the resultant attraction varies as

$$\frac{1}{X^z}$$

z standing for some direct and continuous function of X. (20.)

Gases. In gases, the molecules occupy also equal and equidistant spaces. External pressure mainly, and molecular attraction, varying as

$$\frac{1}{X^4}$$

subordinately and for the most part inappreciably, act as the *centri petal* forces; while impingent molecular repulsion, varying with the temperature and as

$$\frac{1}{X^3}$$

acts *centrifugally*.

IV.—OBSERVATIONS ON THE NATURE AND TREATMENT OF YELLOW FEVER.

(*Translated from the French of* ———, BY THE EDITOR.)

In some of our former numbers we published portions of an excellent work, written by an able and learned French Naval Surgeon, on Yellow Fever. The work (very rare) from which we made the translation, has been misplaced, and the author's name forgotten; this will not make the facts and observations, which he has collected from personal observation in the Gulf—at Havana, Vera Cruz, etc., the less valuable and interesting to our readers, especially in view of what

transpired the past season. We continue these translations from time to time, without any very scrupulous regard to connection or date. The facts will prove useful, we hope, to many who read the Journal, and feel anxious to learn the peculiarities of the disease in other regions than our own. At the time the author gathered the materials out of which his work was made, he was full Surgeon on board the French frigate "Herminie."

PRECAUTIONARY MEASURES.

1st. To avoid exposure to the sun and rain; placing one's self in a current of air, sleeping during the night in a place exposed to the dew; the guard should converse, promenade and sing, rather than remain sitting, rather than give themselves rest, from inaction and silence, to sleep which they are unable to resist.

2d. For the night guard, woolen clothing should be substituted for the light dressing of the day.

3d. If men become wet from a shower of rain, they should as soon as possible change their linen, keep in motion, and give themselves up to constant exercise, which prevents chilliness and its dangereus effects.

4th. It is important, when men with wet clothing undress themselves, that they should leave their effects in the battery and descend undressed into the forecastle, in order to avoid depositing there the elements of humidity. For the same purpose, it is proper to place there after each squall, stoves lit up with dry fuel, and cut into small pieces, as the heat of the furnace is inadequate to dessicate this part of the frigate.

5th. Lastly, the influence of the *moral* on the health of a ship's crew, being a fact incontestible, it hence becomes highly necessary to engage marines in some kind of occupation, and when they have no duties to perform, light amusements serve to enliven the spirits and divert the mind. The various exercises on board, but never too protracted—the dance in the evening, gaiety, etc., within certain limits, become excellent preservatives against epidemic diseases.

I owe my acknowledgments to the officers, for the pains which they took to enforce the strict observance of these measures, as ordered by the Commandant; thus during the winter season the crew had no communication with Havana, so that in fact no excess was committed; the introduction of fruits on board the vessel, and strong spirituous liquors, were rendered impossible, in consequence of the vigorous surveillance; finally, from 10 in the morning up to 3 of the evening, all labor was suspended.

SECOND TABLE—VERA CRUZ.

Grade and employment of the men on board.	*No. on board.*	*No. of sick on board.*	*Proportion of sick in the hundred.*	OBSERVATIONS.
Chirurgeons.	4	4	100.00	In this table I have separated from each other the topmen, assistant cooks, deck sailors and rowers; in this I have been guided by the lists on board, but I should observe that during the epidemic this distinction was not practicable; every man in good health mounted the top, rowed in the small boats, in a word, obeyed all the demands of the ship's service; hence we must not calculate rigidly from cyphers placed opposite the employments.
Superintendent of Infir'y	12	10	83.33	
Domestics	11	9	81.81	
Topmen	52	42	80.76	
2d mates & deck sailors	259	202	77.83	
Cooks and assis't cooks	9	7	77.77	
Rowers	113	81	71.63	
Bakers	3	2	66.66	
Officers and Eléves	14	9	64.28	
Caliers	11	7	63.65	
Cambusiers	8	5	62.50	
Blacksmiths	2	1	50.00	
Masters	7	3	42.85	
Total	505	382	75.64	

We perceive by glancing over the above table, that the proportion of the sick varied greatly, according to the nature of the employment on board. The highest cyphers, firstly, undoubtedly appertained to the surgeons; secondly, to the superintendents of the infirmary; and lastly, to the domestics, all of whom were in attendance upon the sick officers.

Here is, without doubt, a series of facts favorable to the admission of contagion; but I will remark that contagion is not requisite to explain this result; the surgeons, first on the list, and in the second place, all those who paid attention to the sick, experience physical fatigue far superior to that of all the rest of the crew, and were on this account alone much more disposed to contract a disease, to the causes of which all were exposed.

It is thus that among the commanding officers, the only one who died was the capitaine d'armes, whose duties, always laborious, were ten times more so during the epidemic. Lastly, a strong proof that fatigue contributes greatly to the development of Yellow Fever, is the comparatively small number of the caliers, of the cambusiers, and of the first in command; during the months of August and September all labor was suspended on board, and the provisions of two thirds of the equip-

age were distributed by the superintendents of the infirmary, instead of the victualler.

On comparing this second table with the first, (published in a former number) we discover that the effective force of the equipage was much greater at Vera Cruz than it was at Havana; this is owing to the fact, that a supplement of forty sailors were sent to us from the Antilles, to supply the place of those who had died, and of such as had been compelled to return to France for the recovery of their health. We should likewise remark, that the total number of Yellow Fever cases surpassed the number of men on board; the changes which took place on board of the frigate in part explain this result, but it was due chiefly to relapses, as I shall hereafter prove.

Symptoms.—When the Yellow Fever broke out on the Herminie, at Havana, almost all the sick were sent to the Maison de Santé of M Bélot; there I watched attentively the progress of the disease. I wrote down, at the bedside of the patients, all the clinical observations. I watched carefully both the immediate and consecutive effects of the therapeutic agents; finally, I myself made a post mortem inspection of the majority of those who succumbed, and I assisted in those performed by MM. Bélot, Legrana and Chaumel. I give these particulars in order to justify the facts which I shall cite, because I have been induced to record in this memoir many remarkable cases, presented by subjects who did not belong to the crew of the frigate.

To develop the symptoms of Yellow Fever, I shall adopt the classification of M. Bélot, and this will afford an opportunity to appreciate the relative frequency, and consequently, real value of the four types which this physician has established; I shall only change their numerical order, for the purpose of reasoning, to include the most common form, namely, that which requires the fullest explanation.

First Type—*Acute Gastritis.*

This type of Yellow Fever usually declares itself after a copious repast, an excess in eating fruits, or drinking strong liquors; when it takes place, the stomach is always under the influence of violent excitation, and it reacts suddenly upon the entire organism. This is the type which is characterized by the absence of all precursory symptoms. An individual enjoying good health eats with appetite; suddenly experiences weight in the stomach; a sense of nausea is felt and is promptly succeeded by vomiting of alimentary matters; the vomiting continues after the stomach is entirely relieved of its embarrassment. As soon

as the nausea has made its appearance, the epigastric pain has progressively augmented, then comes cephalalgia, lumber pains, and suffering in the limbs; the countenance is *vultueuse*, the ears of a deep red color, the conjunctiva injected, the skin becoming hot and entirely dry, the pulse quick, accelerated, frequent, and sometimes remarkably hard; in some cases it is tense and seems small; but the cœliac artery always produces distinct pulsation at the epigastric centre.

The hand, when placed flat upon the epigastrium, detects them very distinctly. The tongue promptly sympathises with the stomach; it is loaded with a thick mucous coat, its borders become red, it soon assumes a dry appearance, and it is particularly in this form of the disease that it becomes unequal, and as rough as the surface of a lime. The anxiety of the patient rapidly increases. Such is the state of the patient during the first hours of the invasion of this type, which runs a rapid course; happily, to compensate for such a risk, it is also that over which we can triumph with the greatest ease, when we begin the treatment at an early period. Let us suppose that nothing is done, and let us trace the development of the disease. The vomiting continues, and the sensibility of the gastric mucous membrane is so much increased that the contractions of the stomach, already very painful, become excited by the slightest cause. The liquid rejected by the vomiting deserves the particular attention of the physician; it assumes successively different colors; sometimes whitish mucosities with a yellow bilious tinge, again the brown red of pure blood, and finally the deep black of the genuine *vomito-negro*. Then the urinary secretion is suppressed or diminished in quantity; in this latter case it presents alterations in the product secreted; thus the urine becomes troubled, highly colored, and often, when a cure takes place, it assumes a black color, inducing us to believe that a true humoral crisis is effected by the kidneys. If the patient does not yet succumb, and the disease becomes more and more aggravated, the vomitings succeed each other rapidly, and are renewed by a single spoonful of any kind of liquid; the countenance is altered, the eyes sunken; the icterus, which has already commenced, rapidly spreads over the whole surface of the body; the pulse is weak, becomes unequal; the extremities become cold, and the patient is hurried into the agonies of death without exhibiting, most frequently, the slightest disturbance of the intellectual faculties.

The symptoms of the first type present a striking analogy to those of the second; besides, the disease first invading the stomach, very often attacks the small intestines, insomuch that the two types then become confounded. That which distinguishes more particularly the first

is, the suddenness of the invasion, and the great rapidity of its progress; it however demands the same treatment as the second form.

Second Observation.—*First Type of M. Bélot.* Heinrich Radmacher, a youngster attached to the Julius Edwards, entered the Maison de Santé of M. Belot on the evening of the 6th September, and died the 9th. The youth, aged fifteen years, was indisposed from the evening until the morning; he had vomited frequently; had experienced violent pain of the head and loins; and, instead of entering the hospital immediately, he had taken some medicine, the nature of which I was unable to ascertain.

6th September, 18—. Face injected; eyes brilliant; conjunctiva red; acute cephalalgia; general pains, particularly in the lumbar region; skin dry and hot; pulse full, hard and frequent; repeated vomiting of bilious matters; constipation; suppression of urine; tongue red and dry; great thirst; peculiar odor of the breath; pulsations at the epigastrium extremely violent. Bleeding in both feet, ad deliquim animi. Discharge of urine during the syncope; no stool; no nausea. As soon as consciousness returned, eight cups to the epigastrium, four ad nucham; purgative lavement; sinapisms to the feet; proscrip tion of all food and drink.

7th Sept. He passed a tranquil night; he vomited but once. At the morning visit the pulse was less strong; the skin generally moist; the pain less intense; the tongue still red along its borders; the finger, when placed at the base of the tongue, excited nausea; however, the general condition of the patient was quite satisfactory. The evening lavement having produced only a small evacuation, it was repeated; three cups to the epigastrium; cold applications to the head. At the evening visit a fatal change was observed. The vomitings returned at twelve, and were renewed every quarter of an hour; at each attempt he rejected scarcely a spoonful of liquid; at first yellowish and bilious, but which, in the evening, assumed a brownish tinge; at the same time the face was altered; the eyes were sunken; the skin had a dirty aspect; the pulse was small; the skin cold on the extremities. Sinapisms to the feet; blisters to the legs; compound camphorated julep.

8th Sept. The vomiting continued, and the black matter was now perfectly distinct; life seemed on the point of being extinguished; the pulse at the radius was scarcely perceptible, and the cœliac artery still beats with energy. The blisters had produced only a slight rubifacient effect; we re-applied them; we continued the potion. But the evils progressively increased, and the patient expired between 8 and 9 o'clock at night.

Autopsy. In the cranium and chest nothing. Abdomen—The peritoneum was healthy; so were the liver, the kidneys, the bladder, and the spleen. The stomach contained nearly one third of a pound of black vomit; the mucous membrane was softened down to a pulpy consistency; it was detached by a slight pressure of the finger, and presented a black color exactly similar to the grumous matter which floats in the liquids contained in the stomach. Near the piloric extremity we found true gangrenous spots, the largest of which was from six to eight lines in diameter, the smallest as large as a grain of millet. Under these spots, which involved the whole thickness of the mucous membrane, were seen the sub-mucous cellular and muscular tissues, which were very highly injected. This injection was continued into the duodenum and as far as the commencement of the small intestine; but we could neither discover black vomit nor gangrenous spots in these intestines. The end of the small intestine and the beginning of the large, were in their normal state.

A case identical in every respect occurred in the person of a boy named Aulion, (Louis François) aged fifteen years, a youngster on board the French brig Leopold, who entered the 11th of September and died the 15th. The symptoms, including the black vomit, and the pathological alterations found after death, were all exactly similar. The only difference was, that the cause of the disease in this case was not arrested for a single moment; the violence of the symptoms continually increaeed up to the hour of death.

THIRD OBSERVATION.—*Same Type.*

Lefèvre, a domestic, aged 31 years, of a good constitution, was for fourteen days in attendance upon a sick officer, when, on the 5th of September, after dinner, he was seized suddenly, without any premonitory symptoms, with copious vomiting, at first of undigested alimentary substances, afterwards of bilious matter, acrid and bitter. After the vomiting, which occurred five or six times in the course of an hour, Lefèvre was conveyed to the Maison de Santé.

September 5. Nausea; acute epigastric pain; tongue now whitish in the middle and surrounded with a red circle; pulse strong and frequent; skin burning; face and conjunctiva highly sujected; ears of a deep red color; thirst; cœliac pulsations; cephalalgia and lumbar pains. Copious bleeding from the teeth, followed by syncope and a free evacuation from the bowels.

When the patient recovered himself, he said that he was entirely relieved; he had no pain either in the head, the abdomen, or the kidneys

nausea no longer existed ; the thirst itself had abated ; yet, as a measure of prudence, twelve scarified cups were applied ; eight to the epigastrium, four ad nucham; sinapisms were applied to the legs; drinks prohibited.

Sept. 6. Spent a good night; perspiration abundant. At seven in the morning the skin was cool ; the pulse natural ; the bitterness and acidity of the mouth had disappeared ; in a word, convalescence was established. A severe regimen, a few lavements, and repose, completed the cure. Lefèvre went out the 16th September.

The 25th he returned to his affairs at Havana, performed some duties in the city, fatigued himself, and was suddenly seized with colic, with a slight cephalalgia and pain in the limbs. Dieted for two days ; some emollient lavements, and two sinapised pediluvia, sufficed to dissipate all the symptoms of this relapse. Since this time his health has been good.

At Havana, in 1837, out of three hundred and twenty-three cases of Yellow Fever, which I witnessed, I saw only fourteen of this number, which could be referred to this first type.

At Sacrificios, in 1838, it was still more rare ; in four men only did the disease make its first appearance by vomiting, without premonitory symptoms. In three of them the skin remained cool and moist ; the cephalalgia was so slight, and the epigastric pain, when the alimentary vomiting had ceased, yielded so promptly to a single application of cups, that I have not included these men in the list of Yellow Fever cases. I have then only had but one case of the first type.

Fourth Type. *Meningo-Cephalo-Gastritis.*

I have never seen, either in the Island of Cuba, or in Mexico, an example of this form of the disease. I do not doubt its existence, since M. Bélot has reported many observations of this kind ; but the experience of two consecutive epidemics authorises me to affirm, that this type is at least the rarest of all. We should not, however, allow the consequence of this opinion to carry us too far. I do not pretend to say that the encephalon and its envelop always escape morbid reflection. In the majority of men whom I have lost, the delirium manifested itself two or three hours before death, sometimes sooner, and fully testified the part which the brain or the arachnoid played in the latter stages of the disease.

To express myself more clearly, I will assert that I have never seen the Yellow Fever progress in the order indicated in the beginning of

this paragraph ; the encephalon first becomes affected in the development of this affection.

THIRD TYPE.—*Colo-entero-Gastritis.*

This type is infinitely more rare than the second ; at Havana, the only observation which can be presented is the following :

FOURTH OBSERNATION.—*Third Type.*

M. F———, aged 55 years, of a biloso-sanguine constitution, with much *embonpoint*, entered the 23d of August, and died the 26th. During the nights of the 22d and 23d, very painful colics ; great disengagement of gas by the mouth ; five liquid stools.

August 23*d*—Morning. General pain in the abdomen, sense of stricture, mouth clammy, tongue foul and covered with a yellowish coat; breath inodorous; pains in all the limbs ; in the lumbar spine ; cephalalgia ; pulse frequent, small, hard and contracted ; no pulsations in the cœliac vessels ; skin dry and hot; urine turbid, thick, producing a sense of smarting in its passage through the urethra. Sixteen scarified cups to the abdomen, to the back-head, and to the dorsal spine ; so little blood followed the scarifications of the abdomen, that the operation was repeated; we obtained nearly a pound of blood. The skin continued hot ; the pulse did not fall ; three liquid stools of a greenish yellow color and of an infectious odor.

Forty leeches to the anus ; vapor bath ; a free discharge of blood. Better; a little moisture on the skin ; cephalalgia dissipated ; pulse still remained frequent, continued state of somnolency, sinapisms to the legs.

August 24. Night disturbed ; sleep interrupted by reveries ; stools three, of yellowish brown color, and of an extremely offensive odor. Pain in the course of the transverse colon and in the region of the cœcum ; skin dry and hot ; pulse frequent ; tongue of a yellowish white color ; dull pain in the kidneys ; a sensation of uneasiness in the limbs ; cephalalgia; oppression. Ten cups in the direction of the transverse colon ; apply 30 leeches, ten by ten, to the anus, at intervals of two or three hours. Cold applications to the head and abdomen ; a potion with nitre and camphor. Slight amelioration.

August 25. Disturbed night ; somnolency ; skin hot and dry ; heaviness of the head ; one stool almost black ; pulse quick and frequent, 95 pulsations ; persistence of the other symptoms. Forty leeches to the anus, at two applications ; continued the cold applications ; the same potion with the addition of one grain of emetic. At noon same

state, sinapisms volants to be applied successively, to the feet, to the thighs and over the abdomen.

August 26. Delirium through the night; no stool; in the morning tongue dry and yellowish; the delirium continued at intervals; pulse small, contracted and frequent; one hundred and ten pulsations per minute; skin hot and dry; the patient replies correctly to the questions addressed him; he then emerges from his somnolent condition, but to relapse again into the same state, as soon as we cease to interrogate him. Blisters to the legs; nitre and camphor; two ounces of the Ol. Ricini.

At two o'clock of the morning, cold sweat; pulse growing feeble; jactitation; slight icteric tinge of the sclerotica. Twenty grains of Protoc. Hydrarg. He succumbed at 11 o'clock.

Autopsy. Icterus well marked. The abdomen only could be opened. The stomach and duodenum sound; small intestine slightly injected at its superior three fourths; the redness augments in proportion as we approach the cœcum, and it was at this point that we found the greatest lesions; the cœcum and the colon contained a large quantity of brownish yellow matter, in the midst of which were suspended floculi of a perfectly black color, similar to coffee grounds; the intestine when washed presented a vivid color, with some brown striæ along the mucous membrane. The liver had the yellow color indicated. The kidneys and the bladder were in their normal state.

At Sacrificios, in about twenty men, the Yellow Fever broke out with dysenteric diarrhœa. The better to comprehend the progress of these particular cases, I shall quote the following example, furnished by a man named Simon. As the sailor had had the Yellow Fever on two occasions at Sacrificios, I avail myself of this opportunity to report both cases, in order that we may be able to compare with each other the two series of symptoms, and thus be convinced of the identity of the disease in both these two cases.

Fifth Observation.

Simon, a sailor, belonging to company No. 123, a rower, 26 years of age, of a bilioso-sanguine temperament.

Second Type.

April 11, 1838. During the night and day. At three in the evening face and conjunctiva much injected; violent cephalalgia; the patient could scarcely open his eyes; acute pains in the loins and in all his limbs. Tongue white in the centre, red along its borders; character-

istic odor of the breath ; skin very hot and dry ; pulse hard, full and frequent ; epigastric pulsations. Venesection, six cups to epigastrium three ad nucham ; purgative lavement ; sinapisms to the feet in the evening. Three pounds of blood were drawn by the lancet without producing either syncope, vomiting, catharsis, or a discharge of urine. The patient had passed no urine since the day previous.

April 12. No sleep during the course of the night ; skin dry and hot ; pulse not so hard ; pulsation in the cœliac arteries very active ; the cephalalgia and lumbar pain had diminished but little ; the face was constantly red ; one bilious stool during the night ; urine scanty and highly colored. Eight cups to the epigastrium, four ad nucham ; two emollient lavements.

April 13. No sleep ; great jactitation through the night ; the skin was much hotter and more dry than on last evening ; pulse hard and vibrating ; the pulsations of the cœliac artery were perceptible at some distance ; there exists at the scrobiculis-cordis a well marked depression, in the centre of which the recti muscles were distinctly seen; the cephalalgia and lumbago had increased in violence ; the patient was unable to keep one position a single moment. Eighteen ounces of blood from the arm ; fifteen leeches to the anus ; emollient lavement ; blisters to the legs ; cold applications to the head and to the epigastrium. This second bleeding was followed by syncope, by a bilious stool, by nausea, and a free flow of perspiration ; the patient urinated half a glassful ; prompt relief.

At noon amelioration continued.

April 14. One hour's sleep in the night ; the skin was soft, much cooler ; the pulse had fallen ; the cephalalgia not so intense ; the pain of the kidneys still continued to annoy the patient; the urine was more abundant, but always highly colored ; two copious liquid stools during the night ; the epigastric pulsations almost entirely disappeared ; the blisters drew well. Diet; emol. lave. suppurative dressing for the blistered part ; cold application to head and abdomen.

April 15. Skin still a little hot, pulse quick and frequent, yet he did not complain of any pain ; the tongue loaded with a thick adhesive coat ; odor of the breath disagreeable. Diet ; cold water dressing for blister ; emollient lavement.

April 16. Skin hot ; pulse small, contracted and frequent ; eyes sunken ; pulsation at epigastrium very distinct ; urine more abundant, not so high colored ; prostration. Diet ; ptisan of barley, two spoons-

ful; a potion of nitre and camphor, one spoonful every hour; cold water.

April 18. Sensible improvement; from this day the patient progressively advanced in improvement.

Third Type.

August 16, 1838. Violent colicy pains in the night; a sensation or obstruction in the course of the transverse colon; pressure painful; from eight to ten liquid stools, with griping; tongue white, moist, and red at the point; pulse but little developed; skin hot, but moist with perspiration; no nausea; some wandering pains in the limbs. Ptisan of rice with four drops of laudanum; demi-lavement of starch. Three o'clock in the evening; he had also had five stools from twelve to three o'clock; then the face red, injection of the conjunctiva; acute cephalalgia above the orbits; pain in the epigastrium, in the lumbar spine, in the joints of the lower extremities; skin dry and burning hot; pulse hard, full and vibrating; energetic pulsations of the cœliac trunk—the tongue white and moist; characteristic odor; obvious depression at the epigastrium; the urine suppressed since the morning. Syncopal bleeding, eight cups to the epigastrium, four ad nuch., demi-lavement again; in the evening sinapisms to the feet. The loss of thirty-five ounces of blood produced complete syncope, vomiting twice, one operation of the bowels, a free discharge of very turbid urine, and an abundant perspiration. The relief was instantaneous.

August 17. The fever had greatly diminished; the pains were less acute; both pulse and skin better; no stool. Diet, acidulated gargarism; emollient lavement.

August 23. Convalescence declared itself on the 24th; a little fever and some agitation without any pain. I interrogated the patient and I learned that he had not been to stool for many days; pressure over the epigastrium produced pain; a few cups entirely dissipated it, an emollient failing to operate, an injection of salt and water produced a consistent and copious stool, and since then the case has been uninterrupted. The patient's exit is dated the 8th of September. The 3d of October he had the scurvy, but convalescence was protracted and it was not until the 25th of May that he resumed his labor.

Reflections. If we compare carefully the symptoms of the 11th of April with those of the 16th of August, as developed at three o'clock in the evening, we shall easily recognize their identity. The progress of the disease was suddenly arrested in the second case by the synco-

pal bleeding ; in the first case, syncope did not take place, and the evils were prolonged. But this is only a difference in the course, and not in the nature of the disease. The observations of the 16th of August are only distinguished from those of the 11th of April, by the diarrhœa which ushered in the attack. But this diarrhœa ceased spontaneously, so to speak, in the course of ten or twelve hours, and it was only when it had been arrested that the pathognomonic signs of yellow fever manifested themselves in reality. Does the symptom, which constitutes the beginning of the vomito, authorize the creation of a type? I believe not, for the treatment is absolutely the same as in the second type ; only that it is not necessary, in consequence of the frequent stools, to prescribe a purgative lavement ; moreover, remark on this subject, that I have always seen the yellow fever yield more readily when it had commenced by a diarrhœa, just as often as it happened that convalescence follows closely copious and repeated alvine dejections.

I could cite the cases of Bernard, Boules, Callet, Deroff, Rersalvo, Croisier, Degall, the quartermaster, &c., but they are exactly similar to that of Simon ; the disease in all commenced with a diarrhœa of some hours' duration, which in all cases ceased of itself. Again, being enlightened by clinical observation, when a sailor came up, complaining of diarrhœa, I was content, for twenty-four hours, to make him drink rice water. If the diarrhœa was only a *prodrome* of yellow fever, the latter manifested itself in the course of the day, and I had not interrupted a favorable critical effort ; but if, on the contrary, it was a prelude to a dysentery, I had not lost much by deferring for a single day the treatment of this affection.

It now remains for me to demonstrate that the case of Simon, and similar ones, ought to be referred to the second type. Thus, in three hundred and twenty-three cases, at Havana, and in three hundred and eighty-two at Sacrificios, I have really seen but a single example of the third type. This form of the disease is then still more rare than *acute gastritis*, and in fact there remains only the second type, which includes in itself all the cases of yellow fever, with the fewest possible number of exceptions.

It is well understood that these conclusions are only applicable to the facts which I have observed, and which I now give ; for I cannot too often repeat, the yellow fever may, according to localities and certain circumstances, modify its power of development ; in a word, the history of an epidemic is very far from being the veritable history of all other epidemics of the same form.

It is evident, moreover, that in the yellow fever, as well as in many other diseases, nature sports with our methodical classifications, and sometimes presents to us, united in a single individual, the greater part of the arbitrary divisions which we have admitted. Under this head the following observation seems to me interesting, because the morbid lesion assails successively different organs of the body.

OBSERVATION.

Coubart, a fourrier of the 129d company, aged twenty years, of a nervoso-sanguine constitution, entered the 15th of September, 1837, died the 16th November following. At his entry into the Maison de Santé, this man appeared but slightly affected. Although he presented all the symptoms of the yellow fever, as they were not intense, and as the skin was moist, he was not bled; cups, leeches to the anus, purgative lavements, and a rigid diet, soon triumphed over the disease. Still convalescence was not fully established or confirmed.

Coubart sometimes experienced a little malaise of short duration; it is true that he had, on the internal faci of the *preputium*, venereal ulcers, which had produced phymosis, and after operating on it, a very high degree of inflammation had determined a fever for some days. However, he finally reached a satisfactory state, and he was on the point of returning to his duties, when on the eighth day of November he relapsed.

November 8. Febrile movement, heaviness of the head, wandering pain during the night; at the morning visit, abundant bilious vomitings; pain in the right hypochondrium; cephalalgia, a sense of uneasiness in the lumbar region, pulse frequent and well developed, skin dry and hot; thirst, tongue loaded with a whitish coat. Absolute diet; twelve cups to the abdomen; four ad nucham; two emollient lavements.

9*th.* Aggravation of symptoms; vomitings more frequent; injection of the conjunctiva, which presents a slight yellow tinge. A potion of camphor and nitre; purgative lavement; in the evening hot pediluvia. Syncope during the footbath.

10*th.* Restless night; frequent vomitings; six stools during the night; six cups to the abdomen; twenty leeches to the anus; two lavements of starch; cold applications to abdomen.

11*th.* In the morning some perspiration, pulse more regular; the vomiting was arrested; one operation on the bowels; the icterus, which since the 9th continued to increase, was complete. In the eve-

ning the patient expressed much anxiety in relation to his condition. The pupil was dilated ; urine natural. Diet—half cup of tea, to be taken in the course of the day ; suspended the potion. Evening, two blisters to the legs.

12*th.* Delirium during the night ; great anxiety ; a little more calm in the morning. Pain in the epigastric and hypochondriac regions. We repeated the application of the leeches and continued the cold fomentations. He has neither been purged nor vomited ; urine flowed freely ; the blisters suppurated finely.

13*th.* Same state. Besides, the tongue was surrounded by a bright rose colored circle ; in the centre, some reddish points begin to develop themselves, which predicted a buccal hemorrhage. The patient complained that his sight was a little obscured ; he required some time to recognize those persons who approached his bedside; he spoke of death, which he seemed to fear greatly. Same prescription ; two other blisters to the thighs.

14*th.* Acute pain in the direction of the transverse colon ; dryness of the skin ; pulse frequent and contracted ; a number of black fetid stools ; the red circle bordering the tongue disappeared ; extreme prostration; the patient had some slight wandering of the mind, yet he replied correctly to the questions addressed him. A suppurative dressing of the blistered surface. An anodyne draught ; emollient lavements. Evening, blister to the neck.

15*th.* The state of the patient was aggravated, the urine flowed less freely ; frequent stools of dark matters mixed with blood ; a number of discharges of almost pure blood, until the close of the day. He expired in the night.

Autopsy. Icterus, arachnoiditis, encephalic structure sound and consistent ; nothing in the thorax.

Abdomen. Stomach and small intestine so much contracted, that the latter is very little larger than a writing quill ; walls thickened, mucous membrane of a reddish gray color, doubled on itself, and forming longitudinal folds in the stomach, but circular and oblique in the intestine, these folds were of a black color, and evidently softened. The large intestine was distended and dark on its exterior surface ; its walls were attenuated ; the whole of its calibre, from the cœcum to the rectum, was filled with an immense quantity of black, viscous and decomposed blood, exhaling an odor of putrefaction. The parenchyma of the liver was of a dull yellow ; the biliary vesicle contained but little bile ; the bladder was full of urine ; the kidneys sound.

In this observation, the progress of the disease appears to me to offer much interest. First, it is the stomach which suffers; afterwards the liver and small intestine; then the tongue assumes an hemorrhagic appearance, but it fails to take place, and the patient succumbs to a true intestinal hemorrhage. The symptoms, during life, correspond precisely with the lesions after death; we find traces of the disease in the stomach and small intestine; but the greatest lesions exist in the large intestine. The abnormal condition of the liver corresponds with the pain of the hypochondrium; the arachnitis with the delirium. This single example presents, when united, in the short space of eight days, most of the symptoms belonging to the fourth type.

We may remark, to anticipate, the identity which exists between the coagulated, black, viscous blood which the large intestine contains, and that which we find to constitute the intermuscular effusions.

Second Type.—*Entero-hepato-gastric.*

This form of Yellow Fever, the most common of all, differs from the first type, not only from its seat, which is much the most extensive, but moreover, from the existence of precursory symptoms.

It seems that in the first there had been a sudden poisoning, so quick and overwhelming is the attack; in the second, a gradual introduction of a *tonic* agent, which progressively modifies the organism, until, accumulated in sufficient quantities, it unexpectedly develops the disease.

Prodromes. The prodromes of Yellow Fever are general or pathognomonic; it is unnecessary for me to treat of the first. The second are, a peculiar sensation of uneasiness in the lumbar region, a cephalalgia most frequently supra-orbital, slight dimness of sight, which is almost never wanting; an odor *sui generis*, exhaled by the mouth, and with which the tongue seems highly impregnated; this last symptom is almost infallible; every individual who presents it, whatever may be, in other respects, his apparent health, is threatened *du vomito.*

Symptoms. After one or more days, this general malaise is dissipated, or rather each of the symptoms is exasperated, and the Yellow Fever declares itself. The actual invasion is often marked by a chill, to which succeeds a burning heat; at other times the heat is primitive. There is a feeling of heaviness, and sometimes of acute pains in the joints, which make it impossible to maintain the erect position. The lumbar region suffers from similar pains. The cephalalgia is oppressive; the face is animated, and so red as to remind us of eruptive fe-

vers; the conjunctivæ are strongly injected, the tearful eye can scarcely bear the impression of light. The tongue grows red and soon becomes dry; the breath of the patient is characteristic; the skin dry and burning hot, or covered with a slight degree of moisture; the epigastric pain is very severe and generally extends to the right hypochondrium; the pulsations of the cœliac artery are very distinct; it is rare that any nausea occurs during the first day; the thirst is intense; the urine suppressed, or at least diminished in quantity; constipation often exists; the pulse is strong, full, hard and frequent; a sense of anxiety is stamped upon the patient's countenance.

These symptoms continue for a period of time which varies from one to four days; but if the disease is abandoned to its own course, and sometimes in spite of treatment, from the second day of their appearance these phenomena assume a new character of gravity. Then the tongue is covered with a coat whose consistence and color present a thousand varieties; nausea and then vomiting supervene; the face is discolored and assumes an air of dejection; icterus is developed; it begins in the conjunctiva, and finally spreads over the entire surface of the body; the circulation is reduced; the respiration becomes embarrassed; the gums, the lips, the tongue and the nose discharge black liquid blood; the parotids swell; petechial eruptions, and sometimes gangrenous spots appear on the body or limbs. Death soon releases the sufferer.

But this simple enumeration of symptoms is by no means sufficient; many of them are of the utmost importance, and consequently demand special attention.

A. *Icterus.* Icterus constitutes a phenomenon too prominent for authors to have forgotten to mention it; on the contrary, they have dwelt at length on this point; this symptom has likewise given its name to the disease which I am describing. Yet, we repeat, it is a sign which is very often wanting, and I shall have the same remark to record constantly in reference to the black vomit, so that Yellow Fever exists without yellowness, and without black vomit. This proves, to my mind, that the disease to which we have given these two names is not a local affection, as these nomenclatures would have us believe; if the morbid changes were confined to the stomach and to the liver, the effects produced should always be the same, and the black vomit as well as the icterus should never be absent. If, on the contrary, the lesion which constitutes the Yellow Fever was generalized, and distributed at the same time, to one or more apparatuses, we might readily con-

ceive that some of its phenomena of outward expression were not fully developed, without, on that account, their absence giving room to the smallest rational presumption against the identity of the disease.

When, in spite of the gravity of the Yellow Fever, the icterus is not present during life, it is almost always established after death; the exceptions to this general rule have been extremely rare indeed, both at Havana and at Sacrificios.

The time at which the yellowness makes its appearance in the course of the disease, is, according to the observations of M. Bélot, a prognostic sign of great certainty. When icterus establishes itself towards the third or the fourth day, death is almost inevitable; towards the fifth or the sixth, the chances are rather against than in favor of a case; lastly, when it is not developed until after the seventh day from the invasion, the patient is almost sure to be saved. I have been enabled to verify the justness of this remark, and I will cite, among many examples, the two following:

1st. The steersman Lohier entered the 8th of September, and died on the 17th; after the second day a considerable icterus covered the entire body.

2d. A man named Victor, a sailor on the packet boat of Bordeaux, No 4. This man being seriously attacked, manifested moreover great disquietude; the eighth day of his disease, when the yellowness commenced, the icteric tinge was very manifest, and convalescence was soon established.

Is the icterus owing to an effusion of blood—to an ecchymosis, or is it purely bilious in its character?

On the one hand, when the yellowness develops itself during life, we are almost certain to find, after death, the color of the liver altered; the biliary vesicle is often distended with a great quantity of black and thick bile, as though this fluid had ceased for a long time to flow into the duodenum; there may be then a re-absortion and a transportation of bile into the torrent of the circulation; or perhaps in consequence of the interruption of the functions of the liver, the materials which are furnished for the secretory action of this organ are retained in the blood.

On the other hand, the blood is altered in the Yellow Fever, and becomes everywhere infiltrated with great facility. It is then probable that the yellowness borrows its characteristic qualities from this two-fold etiology.

B. *Black Vomit.* At Havana and at Vera Crux, I have witnessed but a small number of cases of black vomit. The frigate Iphigene,

which was anchored near us at Sacrificios, and which has had much greater losses than we to deplore, presented in more than forty deaths, only three or four cases of black vomit.

To what cause is the rarity of this symptom to be attributed? I believe that it is easy to indicate it. The marines of ships of war promptly come forward, on the slightest indisposition, to claim the assistance of the physician; the disease is combatted as soon as declared, and if a treatment as energetic as that which constitutes the method of M. Bélot does not always rescue the patient from death, it at least suffices, when timely adopted, to prevent a rapid sanguino-congestion upon the digestive tube, and thus to spare the patient the horrors of black vomit. The material which constitutes this fluid being blood, the result of a profound lesion of the mucous membrane, whose secretions are mingled with the altered blood, escape either by exudation or rupture from its vessels; it is necessary for this to be accomplished, either that the inflammation should be acute, or that a poison of great activity should be brought in contact with the walls of the stomach or the digestive tube. But, since the miasms which produce Yellow Fever possess a medium of transmission in the torrent of the circulation, it is evident that by diminishing the influx of the blood towards the organs of digestion, we remove at once the two primary causes of black vomit.

It often happens that in an autopsy we find the black matter in great quantities in the stomach and intestines, when in consequence of the absence of all nausea and any peculiar appearance of the tongue, we would not have suspected its existence; it is at the close of the disease, at a moment in which the effusion takes place, when the interior organic dissolution precedes the complete abolition of the vital forces; the sensibility of the stomach is exhausted, its contractility destroyed, and its sympathies extinguished. I have often remarked this at Havana, and I am persuaded that in the majority of patients who died at Vera Cruz, the black matter existed in the stomach. Unfortunately, I have not been able to satisfy myself of the fact; we know that in the midst of an epidemic which attacked so many individuals at the same time, it was impossible to make autopsies; besides, on board, where seclusion was impracticable, a necropsy performed in public would have produced on the *morale* of the crew the most pernicious effects.

C. *Exhalation of blood from the mucous membrane.* When the Yellow Fever, instead of improving under the influence of therapeutic means, continues to grow worse, it may happen that the mucous membrane of the nasal passages and of the mouth shall suffer black and liquid

blood to escape from their vessels. The fluidity and deep color of the blood deserve to be taken into consideration, and to recall to mind the exhalation which takes place in the stomach; the hemorrhage is rarely so great as to become a cause of uneasiness; sometimes, however, it tends to hasten death. When this hemorrhage is abundant, a short time after the invasion of the Yellow Fever, it may be regarded as a salutary crisis; it is not so when it occurs at a later period of the disease; yet, a man named Rivoal presents a striking exception in this particular. On the 13th of November, 1837, the sixth day of the disease, this sailor complained of a spitting of blood; an examination displayed a tongue surrounded by a red border, but smooth on the surface; the central part exhibited inequalities, owing to the papilæ, which were erect, conical, of a red brown, and which permitted a small drop of blood to exude; the lips were cracked, as from the action of cold, and these small solutions of confinuity give rise to the discharge of a dark liquid blood. This hemorrhage continued four days, caused the patient to lose from 20 to 30 ounces of blood, and was followed by complete recovery.

V.—REMARKS ON THE NATURE AND TREATMENT OF TYPHOID FEVER.

Dr. A. Hester:

The following thoughts are respectfully submitted to your consideration, if perchance they should contain any thing worthy of it. Notwithstanding it is my lot to be cast far from the seat of scientific research, yet (thanks to the medical press) have the genial rays of the sun of science penetrated the dark vista, and I have read with the most profound pleasure many of the articles contained in your Journal. There is one subject in which I have taken the greatest interest—Typhoid Fever. Can this disease be cut short in its forming stage?—can it be arrested, or must it be allowed to pursue a regular course to its termination, as small-pox, scarletina, measles, etc.? Although this important question has been ably and extensively discussed, there are some points which seem to have been almost entirely overlooked. I believe it is an admitted fact, that diseases, of whatever name or order, are influenced by, and partake, to some extent, of the nature of the most prevalent diseases of the particular locality in which they occur; and

more especially is this the case when that influence is of miasmatic origin.

Pneumonia, for instance, being purely an inflammatory disease, under ordinary circumstances requires no other than a depletive course of treatment to arrest it, whereas when it assumes a typhoid form, which is frequently the case when this disease is prevalent, we must use tonics and stimulants, and if complicated with the pathological element of periodicity, we find it indispensable, in order to counteract this influence, to use quinine, though it may evidently act as a stimulant, and be positively contra-indicated by every other symptom. No reasonable man will say that we use it here for its sedative effect, or because we believe it to exert any specific influence over purely inflammatory diseases. We know that two specific, distinct diseases may, and do frequently complicate each other, (gonorrhœa and syphilis, for instance) and in the treatment of such cases we must use the specific remedies applicable to each.

Now I ask, may not this view of the subject serve greatly to facilitate the investigation of this vexed question—the therapeutical value of the sulphate of quinine in the treatment of Typhoid Fever. Why is it that this war in the profession is so much more rife here in the South than amongst our Northern brethren? I think the answer is plain—simply because this morbific cause, called miasma, is more generally diffused through our Southern country than is the case in the North. Far be it from me to advocate the theory that quinine is worthless, nay even poisonous, (as some have done) in Typhoid Fever; it would be injustice, when I feel quite sure that to it I am indebted for the life of many friends, who would have been swept off by this disease but for the prudent use of this drug. On the other hand, I am well assured that great men are liable to err on the opposite extreme. There can be no doubt but that Typhoid Fever, as it occurs in the South, is more frequently than otherwise modified to some extent by this miasmatic influence, which being the case, necessarily points to the use of the antidote to this poison, and he who obstinately refuses to administer it, under such circumstances, because he believes the disease, when uncomplicated, is not controllable by it, is either an empiric or legally culpable. Cases have occurred under my own observation, in which I have removed the tendency to periodicity, and yet the typhoid symptoms still continued.

Why is it that the strongest advocates of the abortive treatment frequently fail in their expectations? Is it not because they have a case of purely, specific, uncomplicated Typhoid disease? Why is it that

many of us have so often most happily succeeded in cutting short a disease which presented almost every symptom of Typhoid Fever? Is it not because we have mistaken cases of bilious fever for typhoid, the distinction between which is not always so apparent upon a superficial examination, more particularly if there is a decided inclination to diagnose a case of typhoid.

I am aware that I may be here met by the theory of the convertibility of all fevers, and the unity of cause in their production, which being the fact, would of course lead us to the use of the same remedy in their treatment. A few interrogatories to the advocates of this theory may serve to substantiate the views already advanced. If typhoid and intermittent fevers are of common origin, produced by the same cause, why is it in some localities, where Typhoid Fever is prevalent, intermittents and remittents are almost unknown. Is it not probable that there would be at least some comparison in the relative frequency of their occurrence? Until the fact is established that Typhoid Fever never exists except in malarious districts, or in those who have been exposed to this influence, it must ever remain a distinct disease, produced by a distinct cause, to say nothing of the pathognomonic lesion of Peyer's glands, and the great weight of authority in favor of this distinction. (See Bartlett on Fevers.)

To my mind it is evident, that the great reason why many have fallen into this error is, the modifying influence miasmatic poison is capable of exerting over the progress of this disease, and the difficulty with which it is sometimes distinguished from bilious fever, particularly in the incipient stage. Again, it is very evident that intermittent and remittent fevers may assume what is usually termed a typhoid type, if not properly treated; but this is far from being a case of true Typhoid Fever; such cases might be cited as evidence of the convertibility of the two diseases, but this argument loses its force when we remember that they occur only in miasmatic districts, whereas Typhoid Fever is not confined to such localities.

The above remarks are founded entirely on my own experience. I have observed this disease where Typhoid Fever was of more frequent occurrence than all others combined, and where intermittents scarcely ever appeared. I am now observing it in a miasmatic district, where typhoid is of very rare occurrence, and intermittent and remittent fevers prevail extensively. I have used quinine in typhoid cases, as recommended by Dr. Fenner and others, and I have never seen any deleteri-

ous effects from it; on the contrary, it often reduces the frequency of the pulse, and produces a moisture of the skin, which is always a desirable object, though I cannot say that I have ever succeeded in cutting short the disease.

The veratrum viride in my hands has been of inestimable value.

Very respectfully,

L. Y. GREEN, M. D.

Mississippi, November 23, 1853.

VI.—PATHOLOGY AND TREATMENT OF APOPLEXY.

A Letter from Dr. Cornelius S. Baker, of Bucks County, Pa., addressed to and answered by Samuel A. Cartwright, M. D., New Orleans.

Dear Sir—When I last wrote to you, I supposed that in all probability our correspondence was terminated, since my ends were satisfactorily accomplished, and beyond my expectations a series of valuable papers from your pen have been obtained, suggested in part, perhaps, by my interrogations. But the paper published in the Boston Journal for October 6, on Apoplexy, &c., has, by an irresistible impulse, driven me again to my pen, so that it "is not I," but the spirit of the essay, that is the occasion. I know of no other medical practitioner who would venture the assertion that "for 25 years I have been in the habit of curing Apoplexy almost as readily as intermittent fever," and know truly that it would never have dropped from your pen without due consideration! It is *this* that surprises me.

I have long regarded the theory and distinctions of the books as fallacious, and their conclusions as liable to lead to erroneous practice—at the same time I distrusted my own views and waited for further observations. In the first place, *it is not true*, that the flushed face, turgid veins, and other marks of cerebral congestion, are the first indications of an apoplectic paroxysm; on the contrary, they are the *second* stage of the paroxysm, and only a result of the preceding, at least so I have regarded them. The first stage is characterized by a death-like pallor and a shrunken countenance; vacant swimming of the eye within its orbit, and an idiotic or demented expression of the countenance. These phenomena are well attested in the cases that have fallen under my notice, in which an opportunity for observation was afforded. This

first stage is only of momentary duration, yet it is as well defined and as characteristic as that which follows, and which, as I stated before I regard as only a consequence or result of the former. The first may indicate a suspension, interruption, or obliteration of mental power in the brain or nervous system ; or just that condition, if more local, which we would denominate Palsy, which I am strongly inclined to believe to be one and the same, so far as both may be regarded as diseases of function, with the former ; the one being *local* and the other *general*. Of course, both Palsy and Apoplexy may, and often do exist, in consequence of organic lesions, alterations of structure, effusions, morbid growths, &c. ; with these we now have nothing to do, since it will scarcely be questioned but that they as often, and perhaps more commonly exist as mere functional diseases, and as such not susceptible of being demonstrated by the scalpel.

If the preceding views are correct, then a reason is afforded for the sudden effusion of the countenance, congestion of the brain, &c., which so uniformly attend an attack of this disease. The brain, no longer able to defend itself, or to resist the action of the heart, and removed from the influence of atmospheric pressure, by its bony encasement, creates a vacuum and invites the flow of blood to itself as it is propelled from the heart. How far some of the cases that show organic lesions are a mere result of the foregoing condition, is an interesting enquiry.

There are yet two other phenomena to be noticed, constantly attending upon Apoplexy and other spasmodic diseases, which very materially enhance their danger, and in no inconsiderable degree embarrass the treatment, viz : the spasmodic closure of the larynx and convulsive action of the respiratory muscles, and a copious excretion of tough, viscid phlegm from the mucous tissues, expressed, in a word, by an increased mucosity. Account for the phenomena as we may, the conditions uniformly exist ; they may, and possibly are, like the congestion, *mere* results, and if for the laryngismus, &c., we accept Marshall Hall's "Excito-Motory" theory, we are still in the dark as to the sudden filling up of the trachea and bronchia with such a superabundant amount of mucus. In the earlier part of my practice this condition was forced upon my attention while in attendance upon protracted cases of hemiphlegia. Patients who had hitherto been free from all suspicion of pulmonic disease, will very soon after an attack of paralysis begin to excrete and throw off large quantities of tough phlegm from the bronchial surface, very much resembling the increased mucosity incident upon the relaxation of the mucous tissue consequent upon sen-

ility; and in one instance that now occurs to my mind, the discharge was so great, and its debilitating influence so apparent, as in other circumstances to suggest the idea of "hasty-consumption." This condition, so closely analogous to that observable in Apoplexy and other spasmodic diseases, has led me to infer a closer connexion between the mucous and nervous systems than is generally recognized, and to regard this copious excretion of mucus as a result from the loss or suspension of nervous power in the brain. I am fully aware that the term "loss of nervous power" is in itself an opprobrium; in our present state of knowledge it is an ultimate fact, and as such we employ it.

The existence of the above conditions, in embarrassing the respiratory act, and in retaining carbonic acid within the lungs, not only of themselves are adequate to the production of asphyxia, but are probably efficient agents in preventing or retarding any recuperative effort on the part of the brain itself, and thus perpetuates the condition on which all the others are dependent, and thereby very materially lessens the prospect of recovery.

Such, in a few words, have long been my views of the nature and conditions of phenomena observable in Apoplexy; but to adopt a corresponding treatment, without being reasonably certain as to their correctness, would savor more of the effrontery of the charlatan than the caution of an enquirer after truth. Fortunately for my own comfort, in a diffuse country practice, the story is generally told before our agency can be expected, for either weal or woe; at least it has so happened to me.

In a recent case of paralysis, involving only the hand and forearm, but attended with vertigo, dimness of sight, and other symptoms involving the brain, I had recourse to the lancet, but with no benefit whatever. I have (according to custom) employed it in paralysis, epilepsy, and other spasmodic affections, with like results, save in one case, in which I am satisfied its effects were decidedly pernicious. From all this I have been led to infer, that the lancet was not "the all" in Apoplexy, nor the first. Emetics promise the most, and according to my view, more completely cover the *whole* ground than any one class of agents; but this "congestion," like the ghost of Macbeth, ever stares me in the face. I am pleased with the irritant emetic you have suggested, and have no doubt but it is the "very thing." The hot water is all of a piece with the foregoing; but surely there can be "no fear" of congestion, with all its train of ills "before your eyes," or you

would not have ventured the ordeal. Would "Granville's Lotion" be a substitute, in your estimation ?

The ordinary course of nature, in throwing off the influence of "concussion," has strongly impressed me in favor of emetics; often the very first indication of returning consciousness is denoted by a free emesis, and when the act is consummated, the patient often seems to wake as from a profound sleep. So uniformly has this occurred, that I have no hesitancy in such cases of availing myself of the benefit of a prompt emetic in bringing on reaction, in cases where they can be administered. I think Charles Bell says, the landlady who administered a glass of hot punch to a patient laboring under "concussion of the brain," is, rather than the surgeon who bleeds him, "the better surgeon of the two!" I never was quite satisfied with either, but would look to an *irritant*, rather than either a stimulant (in the true sense of the term) or a depletant.

There is yet another difficulty. Suppose a diseased condition of the arterial coats to exist, and of the existence of which we can never be certain, if upon the sudden invasion of Apoplexy and the consequent violent influx of blood upon the brain, a rupture of the arterial coats should occur—would blood-letting, in this passive condition of the brain have a greater tendency to lessen the amount extravasated, rather than an irritant emetic, by arousing it from its torpor, and thus better regulate the circulation? Or, is the one mode safe, and the latter hazardous? I doubt the efficacy of any treatment in the generality of such cases, yet the books seem to hold out the idea that an "apoplectic-clat" of a former period has been found, snugly stowed away in some vacant corner of the brain.

The foregoing, though greatly extended beyond what I at first designed, is but an introduction to the immediate object I had in view, and which might, after the manner of the ladies, about as well be embodied in the postscript, viz: to solicit from you, if practicable, *the paper published in the New Orleans Journal*, referred to in the essay under consideration.*

I know not that my suggestions are of any value; yet such as they are you will receive as an expression of my high regard for your services in our profession. Should you feel disposed to notice any of the queries that presented themselves in the course of writing, will you, in addition, inform me as to your *mode of insufflating* the lungs, as in the case of young Ely? I do not think that we have at the North any con-

* Dr. Baker refers to my paper on Apoplexy published in this Journal some two years ago.

venient apparatus, beyond a common bellows and stomach tube. I mentioned the case of Ely to Professor Mutter, whose first inquiry was, as to the mode and apparatus; from this I infer the correctness of my conclusion. If we are to reap such glorious fruits from Hæmatokinety, and I scarcely entertain a doubt on this subject, it becomes a matter of importance that a convenient instrument should be at our command, and more seems requisite than merely to force atmospheric air into the bronchial tubes; it seems to me important that the action of a suction pump should be conjoined, to remove the effete air that may already be locked up in the ultimate air cells of the pulmonic tissue. In cases of asphyxia, I should think it desirable to pass the nozzle of the tube within and beyond the glottis; this would not be difficult with an ordinary gum elastic stomach tube, yet it prevents the free return of the air thus forced in, and consequently it is objectionable, at least to allow its remaining for any considerable time.

I remain, my dear sir,

With great respect, yours, &c.,

CORNL. S. BAKER.

SAML. A. CARTWRIGHT, New Orleans.

THE ANSWER

To Dr. Baker's Inquiries, with some additional comments on the Pathology and Treatment of the Apoplectic forms of Disease.

Prefatory Remarks.

I deem it proper to observe that Dr. Baker's letter was not intended for publication. It is one among several very interesting letters I received from that gentleman, and I have ventured to publish it without having obtained his permission to do so. It is too valuable to remain private property; and when the late Dr. Hester called on me for its publication in the New Orleans Medical and Surgical Journal, I could not refuse without doing injustice to the profession, to whom it rightfully belongs. I am sure if Dr. Baker was as well satisfied as I am of the good it is calculated to do, he would not permit modesty or any personal consideration to bar the claims of science to it. I should not have ventured to have sent it to the press, without his permission, if, not only myself but others to whom I submitted it had not have regarded it as a production entitled to rank among the highest, and of which any physician, whether of city or country, might justly be proud. Soon after it was sent to the press, and before I had prepared some remarks upon it by way of reply, which I had promised to accompany it, the profession

and this city met with a deplorable loss in the unexpected and sudden death of Dr. Hester, the able editor of the New Orleans Medical and Surgical Journal. Supposing that the publication of the present number of the Journal would be postponed, I neglected to prepare the reply until the printers began to set up the letter, and now have to write as they print, with constant demands on my time from pressing professional business. This must be my apology for not being able to make what I have to say more worthy of the subject of which it treats. Fortunately any failure on my part will not impair the value of Dr. Baker's communication, as it opens several new doors to the investigation of an old subject of great practical importance, which increases, instead of diminishing in interest, with the lapse of ages. Should I fail to stumble on any thing convincing or useful by entering in thereat, the way will still be open for others better qualified to make explorations on those important points to which he has called attention—nothing doubting but that they will find most of the apoplectic forms of disease, now so formidable under the treatment recommended by Watson and other text books of the day, not only as obedient to medicine as intermittent fever, but a good portion of them, including convulsions in children, mere gastric irritations, bad colds, or intermittent and remittent fevers in disguise.

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Dear Sir:

The remark that "for twenty-five years I have been in the habit of curing Apoplexy almost as readily as intermittent fever," was made to Dr. Jackson of Boston, Professor of Pathological Anatomy in the Medical College of Harvard University, and I deemed it unnecessary to add, that Apoplexy from structural disease of any important organ, as the heart or brain, was excepted. But I must admit that, including the most of the cases in that numerous class of diseases attended with coma from functional derangement, the assertion was a bold one, as it announced the attainment of an object, supposed to be a great way off, but which the progress of the sciences has put within the reach of practitioners of very moderate attainments. I include under the term the various apoplectic forms of fever, convulsions and epileptic fits in children and adults, the different phases of asphyxia, sun stroke and congestive maladies, where the sentient system is more or less suddenly stricken with torpitude—mental or corporeal, or both—general or partial—which are not the effects of inflammation, mechanical violence, or the wasting of the vital energies from pre-existing disease. I would not be understood as saying, that all the various forms of apoplectic affections, embraced in this definition, when depending only on func-

tional derangement, are almost as readily cured as intermittent fever, but I contend that the great majority of them are, when promptly succored by science, not blinded by an erroneous theory, or trammelled by dogmas drawn from a hypothetical physiology. Some of them, I admit, are from necessity fatal, owing more, perhaps, to the shortness of time given to act, than to any thing in their nature. Asphyxia, for instance, from the inhalation of carbonic acid gas, apoplexia fortissima and that form of coup de soleil, which Dr. Dowler, in an unpublished work, calls *solar asphyxia*, often occurring in this warm latitude and extinguishing life in thirty minutes, and altogether different from the coup de soleil of more northern latitudes, which he calls *solar exhaustion*, are incurable from their falling into an incurable state before medical aid can be obtained. Whereas that form of coup de seleil, properly called *solar exhaustion*, is even more quickly cured by the proper remedies, than a common intermittent fever, and is almost as fatal as solar asphyxia if improperly treated. Laudanum, brandy, cold affusions and rest in a horizontal position, will quickly cure conp de soleil produced from mere solar exhaustion. It is, in fact, a mere fainting, the face being pale and the skin cold, or not above the natural temperature. In solar asphyxia, the face is flushed and the skin burning hot to the touch. Dr. Dowler by actual experiment, has found the temperature as high as 112 degrees of Fah., the sphinctres relaxed, and mind and body utterly insensible to impressions. In cases of this kind, post mortem examinations furnished no evidences sufficient to account for the symptoms, or fatal termination within the cranium; but sanguineous infiltration was so abundant in the substance of the lungs, that instead of the usual crispy feel under the knife, they cut, says Dr. Dowler, like a mass of jelly. I mention these facts, not to anticipate Dr. Dowler's intended work, but to give interest to it, if I could, and to hasten the publication of his vast and highly important contributions to science. They demonstrate the role the brain plays in apoplexy, attended with general and complete insensibility to pain or impressions of any kind, is only secondary—that it is not properly a disease of the brain. The assumption that it is a disease of the brain, or nervous system, is founded on an erroneous hypothesis and is the main cause of the apoplectic forms of disease being less amenable to treatment than remittent or intermittent fevers. The latter diseases are about as often intractable, at present, as Apoplexy would be under a judicious practice founded on observation and experience, instead of those vain hypotheses drawn from a speculative physiology, which decoyed the medical

teachers of the last century from the pathway of true science marked out by Hippocrates. Most diseases have since been brought into the light of the inductive sciences, but those of the apoplectic kind still remain afar off in that dark hypothetical ground where the speculative philosophy of the last century placed them—the pathology of Apoplexy resting upon a presumed obstruction of a hypothetical sensorial power manufactured in the brain, and its treatment founded upon that hypothetical pathology as a law given to direct the physician what to do. It left him but two things to do—to bleed and purge or to stimulate. One to remove a supposed pressure on the brain, causing the imaginary nervous fluid or power to be obstructed in its passage through the nerves, and the other to stimulate the nervous system to convey it or to force a passage for it. It made but little odds in the rules of practice what the disease was, causing the coma, insensibility, or convulsions, the theory would keep the attention of the practitioner in spite of himself fixed upon the single point of adopting measures to remove an imaginary obstruction of an imaginary nervous fluid or sensorial influence, supposed to be manufactured in the brain and transmitted through the nerves. It may have been dentition, an overloaded stomach, the recession of a cutaneous eruption, or many other morbid states as easily remedied as an intermittent fever, or it may have been the paroxysm of an intermittent fever itself causing the convulsion or coma; yet whatever it may have been, it was all referred to a unity of cause requiring unity of treatment in diseases and pathological conditions of the system as different as possible. No wonder that the apoplectic forms of disease have proved to be more formidable and fatal under such a routine practice, directed by but one idea and that erroneous, than they were in ancient times. Even at the present day the closer the text books are followed the more fatal are such complaints; the best read doctors, fresh from the best schools, having the worst success. The announcement I made to Professor Jackson, that they may be almost as readily cured as an intermittent fever, will cease to startle the profession as soon as the inductive sciences have been brought to bear upon them, and every vestige of that hypothetical pathology discarded which has so long directed their treatment.

Since I received your letter I was called in consultation to a negro man with coma and convulsions, whom I found under the secundem artem treatment to relieve the brain of a supposed congestion, interrupting the transmission of a supposed sensorial power to the rest

of the system, on a pathology to that effect, though expressed in different words. Bleeding, purging, cold applications, blistering, etc., being the means tried, without success. I advised hot applications to the head, stomach and bowels, by cloths wrung out of hot water, and a mixture of quinine, camphor, capsicum, calomel and laudanum, to be forced in his mouth from time to time, until he swallowed a good dose. The attending physician thought such a treatment would kill the negro, and called in another counsellor. They followed the text books most faithfully, the patient growing worse and worse all the time, until the poor negro was thought to be utterly hopeless. At length to satisfy all parties, and reconciling the conscience by the reflection that nothing could hurt a dying man, totally insensible to pain, it was concluded to apply the hot cloths and to give the hot medicine. At first it appeared to strangle him, but on being turned on his side, the throat and fauces became disembarrassed of much tough phlegm, which the hot medicine had loosened sufficiently to be ejected, together with it, from the mouth. More was put into his mouth from time to time, until he swallowed a good dose,—the power of deglutition, before lost, returning as the tough phlegm, obstructing the respiration, was thrown off by the secretory action so powerful an apophlegmatic excited; soon after which, to the amazement of the bystanders, he came to his senses and recovered. The account of what transpired I received from the master himself, who was astonished and delighted at the wonderful cure. It was not so wonderful, as the case was nothing more than an intermittent fever under the disguise of coma and convulsions. The sexton's book lost the report of another case of apoplexy, or congestion, or inflammation of the brain, which would, no doubt, have been added to it. King James died with an intermittent fever. The physicians of the present day would not have let him die with a disease under that name; they would have fancied it something else. Intermittent fever still continues to kill a great many people in this enlightened age and to put out their eyes, but it does it under the disguise of some undefined congestion, inflammation or neuralgic amalgam. A gentleman, among the most distinguished men of the United States, was suddenly attacked in the night with a violent inflammation, as he supposed, of the eyes, attended with the most intolerable and excruciating pain. There was great intolerance to light, so much so that it seemed almost impossible to exclude the painful impression it made by trying to shut it out by darkening the room and covering the eyes. In addition to the pain in the head and eyes, there was a buzzing sensation in his ears, which he

compared to that produced by quinine. Indeed he thought his constitution had been greatly impaired by his having, in former times, taken "too much of that pernicious drug," and its effects still remaining in the system, was supposed to be the cause of the buzzing he then felt in his ears. The veins of his face and head were turgid, the arteries beat violently—the integuments of the head and face were burning hot to the touch. The pulse at the wrist weak and irregular, the extremities cold, and there was a weak, faint, sinking sensation at the epigastrium. He informed me, that the year before he had been attacked with a similar inflammation, which, after confining him in a room, hung with black, for more than three months in great torture, finally disappeared; leaving the sight of one eye permanently impaired without any perceptible disorganization or blemish in the organ itself. Although I loved this patient well, not only as a man, but for his noble achievements in his country's service, yet after contemplating his case a few moments in deep grief, I was suddenly filled with extreme pleasure when whispered to by inductive Science, that on that very day she would display her power and make him whole. According to her bidding I went to work. His head and the lids of his eyes were washed in chloroform, and diaper cloths, seven folds thick, wrung out of water hotter than the hands could bear (without cooling them with ice preparatory to the wringing,) were applied all around the whole head, including the eyes, and renewed every ten or fifteen minutes. At the same time two grains of opium and ten grains of the sulphate of quinine, with a tea spoonful of the wine of the seed of colchicum, were given at a dose and repeated every three hours. The relief afforded was almost instantaneous. It was morning, and before the sun set he was nearly entirely relieved of the pain, the buzzing in the ears had gone off, and with it the insupportable sinking sensation at the epigastrium. After having taken twelve grains of solid opium, sixty of quinine, and half an ounce of colchicum wine, I gave him a dose of castor oil to move the bowels and to procure sleep. After it operated he slept, and the quinine, colchicum and opium were occasionally given through the next day to prevent a relapse; after that, the opium was omitted and one or two doses a day of the quinine and colchicum until he regained his strength and appetite. The eyes were let alone—nothing was applied to them, leaving them to take care of themselves after having dislodged the misplaced intermittent fever which had fallen so suddenly upon them. They did take care of themselves, and in a few days not a vestige of the inflammation or turgescency could be seen, and the sight was improved

instead of having been weakened by the attack, and the patient got in better health than he had been in for a long time previously. His was about as much a case of inflammation of the eyes as the negro's was a congestion of the brain. The disease of both was essentially the same, but choosing different organs to manifest its effects; the organs themselves—the brain in the one and the eyes in the other—being neither at fault, but perfectly passive, having nothing to do with causing or curing it. Lest you might think it was the previous depletion, almost to death, which prepared the negro for the cure, I have given the eye case, where a similar quinine, opium and hot water treatment, without any depletion or preparation whatever, had the same marked and almost instantaneous effect. But was not such a treatment a hazardable experiment? It was no experiment at all.* I had often seen the same kind of treatment, in similar morbid states of the system, have the same happy effect in saving life, as well as eyes, from destruction. It would have been hazarding a fearful experiment to suppose that nature would be untrue to herself and to have trusted either patient to any treatment she had in such cases repeatedly pronounced against. Resin Bowie, brother of the inventor of the knife that bears his name, with a similar affection of his eyes, coming on suddenly in the night at his hotel in Philadelphia, was made so furious with the pain that he broke the bedstead to pieces. Dr. Chapman, I think, was sent for. Bleeding, leeching, cupping, purgatives, cold applications, blistering, a severe regimen, lotions and nitrate of silver to the eyes, all availed nothing—the sight of one eye was totally lost, and not long afterwards a second attack nearly entirely destroyed the sight of the other. His athletic frame and great muscular strength gave him the appearance of blind Sampson. The history of his, and other like cases, added to my previous experience, convinced me, that in such cases of misplaced intermittents, whether called inflammation, neuralgia, or any other name, antiphlogistics are not much more favorable to the eyes than Sampson of old found the Philistines to be. I found them to be Philistines in the case of Mr. Warren's eyes, as far back as November, 1826. On looking now at the notes of his case, I read that after copious and repeated bleedings from the arm, free purgation with mercurial and saline cathartics, cold applications, a liberal use of antimonials and local bloodletting, and finally boldly opening the temporal artery, that instead of having given the smallest relief to the pain in the eye chiefly affected,

* Hip. Dr. Flatibus. Sect. 7 & 12: "We relieve such diseases by applying hot applications externally."—At § 12. Apoplexies are specially named and referred to the same cause — § 7.

another pain, equally intolerable, sprung up at the end of the second day, in one of his ears. It was proceeded, as in the case above mentioned, by a sensation as if wind was breaking into his ears. He was promptly cured and his eyes saved by four grains of quinine (a large dose in those days,) every hour and three grains of opium every three hours. In September, the next year, I was called to a negro boy of Mr. R. L. Smith's, in an apoplectic fit. He was comatose, but not perfectly insensible. Loud calls, close to his ear, would rouse him for a second or two, when he would fall back into coma. Mustard and external stimulants to the skin were not felt at all. He had fallen into this condition while a dose of calomel and jalap was operating. That it was not faintness, was proved by the turgescency of the blood-vessels of the face and head and the burning heat of that part of his body, while the extremities were cold. His pulse was 160 in a minute,—precluding all idea of bleeding. I ascertained that four days previously, after bathing in Lake Concordia, he had a slight chill, succeeded by fever, not remarkable for any other thing than a great determination of blood to the head, for which evacuating medicines were used. It returned more violently the next day and the day after, and on the fourth day the apoplectic fit took the place of the paroxysm. Diagnosis—misplaced intermittent affecting ths brain. Treatment—the same as for the misplaced intermittent affecting Mr. Warren's eye the preceding year. Four grains of quinine every hour was, however, first tried. A few doses reduced the pulse to 130, but finding the extremities growing colder, instead of warmer, two and a half grains of opium and a dose of calomel were conjoined with it. The next day, under this treatment, the patient regained his senses. The quinine was continued at longer intervals, the pulse coming down all the time, approaching more nearly the healthy standard, and on the third day after the fit the patient was well.

During one day I prescribed for six cases of coma or profound insensibility of body and mind, among the grown negroes at Mr. P. M. Lapice's White Hall plantation, several cases of fits among the children, like epileptic fits; three cases of congestive fever, and a number of cases of remittent and intermittent fever. A large sloo or shallow pond of water, near the houses, had dried up about the time of the occurrence of these cases. No better proof could be afforded of the vanity of nosological definitions and their inutility in a practical point of view. Here were upwards of 150 negroes, all living in the same kind of houses, engaged in the same occupations, eating at the same table, drinking the same water, wearing the same kind of clothing, and breath-

ing the same atmosphere infected by the drying up of the pond, attacked nearly about the same time with some of the most common, as well as the most formidable diseases to be found in the Nosology. Theoretical classifiers had drawn very marked distinctions between them, placing some in the brain and nervous system, others in the sanguineous or hepatic system; some as fevers, others as congestions or inflammations of this organ or that, requiring special treatment to be directed against the organ which their fancy blamed with being the cause of the mischief. But Nature declared she knew no such distinctions, not only from the cases on that plantation, but elsewhere in the same neighborhood; where filthy pools of water had begun to dry up. Some fell down in the field apoplectic without any warning. Others took a chill and in a day or two the fit took the place of the chill; but in most cases the comatose affection occurred during the exacerbation of the fever; and the epilepsy in the children was started by any irritating cause, as that of teething or the operation of drastic medicine. Produced by unity of cause, they required unity of treatment. What was found to cure one would cure all, notwithstanding the difference in the symptoms. They were all cured by the same treatment, a little varied in kind, to suit the circumstances of each particular case. Thus when the air tubes were choked with mucus, rattling in the throat, preventing the free ingress of air, a little mustard and salt, or some apophlegmatic, as capsicum with assafœtida and ipecac, forced into the throat, caused its disgorgement, let in the air and restored the power of deglutition; after which the apoplectic cases were treated precisely as the reigning disease of the season, only more actively, with larger doses. The mortality was frightful throughout the country where the books were followed and the treatment, in the comatose form of the disease, was directed to the removal of some imaginary pressure on the brain stopping the transmission of some hypothetical fluid, power or influence, supposed to be manufactured in the brain and transmitted through the nerves. Whereas, when the specific remedy for the disease itself was freely given, no matter what the protean forms it had assumed, the cure was almost equally certain and speedy, in one form as in another. When the season changed and the cold weather set in, intermittents themselves often resisted the specific treatment, requiring bloodletting or evacuents to prepare the system for them; so did the apoplectic forms of disease then occurring. The stoppage of the insensible perspiration, by exposure to cold, gave the mucous surfaces more to do, and showed its effects by influenzas, pneumonias and comatose affections of the brain. What would cure one was generally found to be a good remedy

for all the diseases prevailing at the same time. The apoplectic forms nearly always required an apophlegmatic or pungent substances, combined with nauseating or emetic medicines, to disembarrass the throat and air tubes of that excessive secretion of phlegm, you have noticed in such cases, and to let in the air freely to the blood in the lungs. This being done, whatever would cure the prevailing disease, whether quinine, the lancet, purgatives, antimonials or sudorifics, would assuredly cure the apoplexy. Hence, when I penned the lines to Prof. Jackson, "that for 25 years I had been in the habit of curing apoplexy almost as readily as intermittent fever," you were right in supposing that I would not have made such a startling assertion without having some good grounds for it. From what I have said, you will perceive that I am no believer at all in the existence of such a thing as idiopathic apoplexy or a disease of the brain, interrupting a supposed nervous influence, fluid or power transmitted through the nervous system, and I carry the war into Africa by contending that the practice, founded upon that hypothesis, is the cause of the greater fatality of apoplectic and comatose affections—the last half century than they were previously—when treated chiefly by pungent emetic medicines, and like the reigning malady of the time being. It is admitted that a disease of the brain may be the primary cause of muscular rigidity or insensibility, so may a disease of the toe or the finger, a splinter under the nail for instance, or a tooth coming through. It is admitted that there may be effusions of blood or serum in the brain, but these are mere effects and not causes, as they are often found in other parts of the body in those who have died apoplectic, with no morbid appearances whatever in the brain. In 1792, Young America, in the person of Dr. Phillip Syng Physick, bearded the great British reformer, Prof. Cullen, in the city of Edinburgh, on this very point, by proving, "that in cases of apoplexy with effusion, the presence of the fluid cannot be considered as the cause of the apoplectic symptoms." (See his Inaugural Thesis.) But the American youth, announcing the truth, was not listened to, with all Europe against him, as it overturned the very foundation of that theory which decoyed the medical profession from that solid ground, observation and experience, that Hippocrates had enjoined on them never to leave under any pretence whatever. They left it, however, and followed a theory which, so far from showing them how to cure apoplectic affections, has led, not into a comedy, but into such a tragedy of errors from that time until this, that yourself and many members of the profession are startled at the announcement, that such affections are curable like intermittents and other diseases. But, unfortunately, it is to

be feared that much the greatest number are too deep in error, too much like Ephraim, joined to their idols, to be startled by anything—not even by the facts announced by the celebrated Marshall Hall, who proved conclusively, that so far from plethora and pressure on the brain, interrupting the manufacture or transmission of a nervous fluid or sensorial influence, being the essential cause of apoplexy, that the disease very often occurs from anæmia and inanition; dyspepsia, gout and rheumatism, or anything that vitiates or impoverishes the blood; excessive bleeding from the arm will produce it; so will Bright's disease of the kidneys; also fright, fear, joy, anger, etc. A great error lies in confounding mechanical apoplexy, produced by external violence, a depressed bone or a clot of blood acting as a foreign body on the brain, with the disease or rather those morbid phenomena called apoplexy, and applying the theory of the former to the latter. When the inapplicability of pressure as an essential cause of the phenomena classed as apoplexy is proved, they save their theory by a quibble on the word, restricting its meaning to those cases only where a depressed bone, clots of blood, or serous or sanguinous effusions are found on post mortem examinations, although the phenomena may have been the same where no such causes are found to exist. Dr. Physick and a great many others have found such causes to exist without the patient, during life, having been afflicted with any apoplectic symptoms whatever, as in the case of Mr. Webster, where a false membrane was found in the brain and other morbid appearances, sufficient to have given him apoplexy or to have made him an idiot, if the prevailing doctrine in regard to the brain and nerves be true. A great while ago, Dr. Elliott, now of Lake Providence, and myself, had a case of apoplexy supervening on a fall on the head, which did not deprive the patient in the first instance of the power of muscular motion, but not long afterwards he became apoplectic, and had strong convulsions. There was no fracture. I trepanned him, and removed a large clot of blood lying on the brain, and although the man had been insensible for some days, without any use of his limbs; the operation was no sooner over than he got up from the table and walked about, and was perfectly restored to health, except the wound. It seemed to be clear enough that the clot of blood produced the apoplexy, but it is not equally clear that it produced it by pressing on the brain. It may have acted simply as an irritant or foreign body on a tender organ, as a mote in the eye, affecting the general system, and not by virtue of any direct pressure. A tooth coming through a resisting gum often causes similar symptoms. A melon seed in the trachea produced even more alarming apoplectic symptoms in a little boy, whose case I reported in the

New England Journal of Medicine and Surgery, vol. 14, page 136, for the year 1826. It is there stated that "he fell to the ground apparently dead, being breathless and black in the face, as if suffocated." I cut into the larynx and took the seed away, and in a short time he was well. In this case the apoplectic symptoms were caused by a foreign body interrupting the respiratory process—producing a species of asphyxia. Asphyxia and apoplexy are essentially the same in their anatomical characters. Asphyxia was not understood until Kay explained it. The blood stagnates in the capillaries of the lungs, in consequence of its failing to undergo the change from venous to arterial, and the left heart ceases to act for want of blood to act upon. Cullen considered asphyxia from mephitic air, narcotic poisons, cold, concussion and certain passions of the mind, as a veritable apoplexy, and supposed it was occasioned by such agents "directly destroying the mobility of the nervous power." (Practice Physic, MCXV.) His hypothesis in regard to asphyxia has been disproved by Kay; yet the same hypothesis is virtually at the bottom of the theory and practice in apoplexy at the present day. Asphyxia is an apoplexy where the heart ceases to act for the want of blood; but anything, as Cullen has truly said, is calculated to produce it, "that produces a more difficult transmission of blood through the vessels of the lungs," as that "interrupts the free return of the venous blood from the vessels of the head." It differs from asphyxia only in degree. It differs from palsy by its affecting the whole of the powers of sense and motion, while palsy affects only a part. Epilepsy is nothing but apoplexy in an intermitting form. All these affections are essentially the same, differing only in degree. Whatever stops or retards the transmission of blood through the lungs produces this affection. When the causes, interrupting the passage of blood through the lungs are temporary, the disease assumes the form of epilepsy. The blood not being able to pass through the lungs, in due time, produces a fullness in the vessels returning the venous blood from the head. Hence, "the turgid veins and other marks of cerebral congestion" are secondary, not primary symptoms, as you truly say, "they are preceded by death-like palor and a shrunken countenance." As a natural consequence of the turgidity of the venous system, from the retardation or obstruction in the transmission of the blood through the lungs, the mucous surfaces pour out a more abundant secretion. This is properly only an effort of nature to relieve the turgid veins of a portion of the fluid which distends them. But in her attempt to relieve herself in this way, as far as the bronchial tubes are concerned, nature commits suicide by filling them with tough, viscid phlegm, making them

more impervious to the air, and preventing the transmutation of the venous into arterial blood. Hence, the necessity of removing, by emetics, the super-abundant mucosities, poured out into the air passages, in order that the air may be able to permeate them and vivify the venous blood and to facilitate its transmission through the lungs. The motive power of the blood being itself derived from the air, whatever removes the obstructions to its entrance into the air cells must, from necessity, facilitate the cure. In my essay on apoplexy, published some few years ago, I endeavored to point out the uses of the membrane lining the nasal fossæ, sphenoidal and ethmoidal cells, the frontal sinuses and the antrum maxillare. I proved that it is, properly speaking, an anatomical contrivance to relieve the brain of its surplus fluids. In children, the office performed by this membrane is more important than in the adult, owing to the larger comparative size of the head, requiring this safety valve for the excess of fluids determined to the brain. In apoplexy, irritating substances applied directly to this membrane, by exciting a copious excretion of mucous, is the quickest and most effectual method of relieving the plethora of the cerebral vessels, amounting in fact to the most direct local depletion that can possibly be adopted. The experience of two thousand years has proved the virtues of volatiles to the nose and errhines directly applied to the membrane. They not only produce a copious secretion of mucosity, but equalize the circulation and invigorate the languishing vital energies. About the close of the last century they were condemned on theoretical grounds. Although experience proved them to be very valuable in cephalalgia, and in affections of the eyes, ears and teeth, attended with a plethora of the veins returning the blood from the head—and unattended with any increased impulsion or corresponding fullness in the arteries. Increased determination of arterial blood to the head may cause phrenitis or inflammation, but not apoplexy. Anything diminishing the impulse of the blood to the head, as Abercrombie truly says, may cause the latter affection, which is the very opposite of inflammation, as it consists in a sluggish or impeded motion of the venous blood. The bloated and puffed appearance of the face in the apoplectic forms of disease, so well marked in puerperal convulsions, is owing to an exudation into the cellular tissue, caused by the slow motion of the blood in the engorged veins. This cellular infiltration is an effect not a cause of apoplexy, so are the extravasations, whether sanguinous or serous, often found in the brain, mere effects not causes. All the antecedents and concomitants of apoplexy point it out as a pathological condition of the system, produced by a diminution of the vital forces, from a retardation or impedi-

ment in the transmission of the blood through the lungs; in a word a partial asphyxia. The fullness of pulse, sometimes observed, is a deception arising from the slow breathing. In the milder cases, where the blood passes freely and in full volume through the lungs, only slower, the left heart struggles the harder to unload itself. It is a good symptom instead of a bad one. Instead of bleeding to reduce the vigor of the heart, that remedy is only useful in diminishing the amount of venous blood, enabling the balance to pass more freely through the lungs. Hence, when judiciously used it increases the force of the arterial circulation instead of diminishing it. To bleed until the force of the pulse is reduced, is fatal in apoplexy. The operation is mostly useful in those cases where the lungs are so overloaded with black blood as to prevent its transmission into the pulmonary veins and on to the left ventricle. Hence, in that species of asphyxia from retrocession of the measles, attended with lividity of the face, opening a vein will enable the blood to pass through the lungs, and to give the patient a pulse who previously had little or no pulse. I have repeatedly followed Sydenham, and saved life in the lividity occurring in measles after the patient was pulseless or nearly so by bloodletting. An emetic of mustard and ipecac should go with the bleeding, and a mercurial cathartic follow it. Also, sprinkling the face with cold water, and pouring a little on the extremities or rubbing them with ice. They are always cold in such cases, and cold is much more effectual in warming them than hot applications. But you are deterred from the use of emetics, volatile and acrid substances applied to the mucous membrane lining the mouth, nose and fauces, and especially substances calculated to cause sternutation, by "*this congestion*," which, "like the ghost of Macbeth stares you in the face." Suffolk's description of the body of Henry VI (Shakspeare's Henry VI, second part, act 3d,) will effectually exorcise that ghost. It paints that "*congestion*" to the life, and points to the cause of it. The cause of it is asphyxia, or a want of a proper oxigenation of the blood. Irritant emetics, by removing the mucosities obstructing the bronchial tubes, are the most effectual means of removing the congestion by facilitating the metamorphosis of venous into arterial blood, and by exciting secretory action in that extensive mucous lining of the nasal fossæ, sphenoidal and ethmoidal cells, directly depletes the engorged vessels of the brain. But there is still another depletory measure, the application of many folds of cloths, wrung out of hot water, applied all around the head. The hot applications around the head produce a copious sweat and tend to unload the blood vessels of the head, and to remove the cerebral congestion. They also stimulate the venous sys-

tem to push forward the sluggish congested blood towards the right auricle. Granville's lotion, you perceive, would not answer as a substitute. But is not all this a mere practice founded on a vague theory? So far from it, it is an old practice, which, after having proved its efficacy from the time of Hippocrates, has recently been displaced, and has gone out of use in consequence of a vague theory, introduced into medicine by Cullen, towards the close of the last century, referring apoplexy to a want of mobility in a hypothetical nervous fluid, supposed to be produced by some imaginary pressure on the brain. Sydenham, Fothergill, Pitcairne, Selle, and the most distinguished physicians, anterior to Cullen, used emetics and apophlegmattes in the treatment of apoplectic affections. Van Helmont declares that they are the infallible secret of curing such affections. It was the common practice, anterior to Cullen, for physicians to use horse-radish, mustard seed, spirits of ammonia and acrid volatiles to stimulate the schneiderian membrane to secretion, and to cause sneezing, as methods of cure in apoplexy. Cullen condemned such substances purely on hypothetical grounds. Berselius was in the habit of rubbing the tongue and mouth with the volatile oil of amber to relieve the congested vessels of the head, by causing a copious flow of mucosities from the membrane lining the mouth, tongue, fauces and posterior nares. Paul, of Egineta, removed the obstructions in the bronchial tubes, preventing the ingress of air into the lungs, by a feather dipped in oil, made more stimulating by the addition of castor, opoponax and such substances, passed down the throat. When such means fail to promote the transmission of the blood through the lungs, I have recommended and practiced artificial insufflation. In an article I published, sometime ago, in the Boston Medical and Surgical Journal, on Chloroform, I recommended artificial respiration as the most certain curative measure for apoplexy or asphyxia, caused by the inhalation of that agent. It excited no attention at the time, but I am happy to perceive that the practice is beginning to be adopted, and has already saved many lives. In the case of Dr. Ely's child, and in those cases where I have used it in restoring suspended animation, no instrument was employed, the air being blown into the lungs by the mouth of the operator. Hunter's double bellows would no doubt act quicker and better. But in such cases we have no time to loose in looking up instruments. The air can be thrown into the lungs from mouth to mouth, without loss of time. In bringing dead alligators to life, I used the common fire bellows; I exposed their lungs to view and watched what was going on. The phenomena, I saw, told me plainly that the flesh derived its life directly from the blood, and that

the blood derived its life and motion directly from the air. The brain and nerves are only a finer species of flesh, consisting, like the muscles, of two elements—the one vascular and the other fibrous. In order to arrive at the correct theory and practice in apoplexy, it is necessary to begin by reading and believing, in its full literal sense, the 14th verse of the 17th chapter of Leviticus.

Very respectfully, your obedient serv't,

SAMUEL A. CARTWRIGHT.

To Dr. Cornelius S. Baker,
Richboro', Bucks County, Pa.

VII.—ON THE MOTIVE POWER OF THE BLOOD.

BY ALBERT WELLES ELY, M. D.

In the November number of this Journal, for 1852, it will be recollected I published a detailed exposition and refutation of Dr. Samuel A. Cartwright's strange and unsupported theory, to which he has given the sesquipedalian and somewhat uneuphonous name of "*Hæmatokinety.*" In the September number, for 1853, Dr. Cartwright replies to my last article, after taking nearly a year to reflect upon it. He begins by excusing himself for remaining so long in a brown study on the snbject, and for concocting new means of resuscitating poor asphyxiated "Hæmatokinety," by saying that he did not reply to my article "because all those well versed in Natural History would perceive that it needed no answer, his data being incorrect; and it was perfectly useless to get into a controversy with those who had not studied the subject.* Dr. Ely had got hold of an incorrect translation of Cuvier's works, and turned the same against the new doctrine with tremendous effect in the eyes of all those unacquainted with Comparative Anatomy, and who had never seen the Napoleon of Naturalists in any other than an awkward English dress."

This is indeed a very singular and summary way of disposing of all the facts and arguments contained in my last article, in which I proved, by facts, and by quotations from standard authors on Natural History,

* The whole of this controversy about "hœmatokinety," must have convinced many, that it is Dr. Cartwright who has "not studied the subject;" for if he had, properly, he would not have committed so many blunders in natural history.

that all of Dr. Cartwright's positions were without foundation, and overthrew every one of them by citations from Cuvier and his commentators; from Dr. T. W. Harris, an eminent naturalist of Cambridge; from the latest French authorities, such as H. Milne Edwards, M. Deschayes, Lanarck, De Behn, Strauss and others, from whom I gave extracts in French, except in the single case of Cuvier, whose *Règne Animale* was only accessible to me in an "English dress," published in London, by a body of eminent naturalists, in 1840. The extract which I made from that work completely overthrows Dr. Cartwright's position, "that it is known to naturalists, that of the twelve thousand species of fishes, not one has any muscular organ answering to the left ventricle of mammals"—a position from which Dr. Cartwright very erroneously concluded that "Hæmatokinety" must be true. The extract in question was the following: "The blood is brought to the gills by the heart, which thus answers to the right ventricle of warm blooded animals; and from the gills it is sent to an arterial trunk lying immediately upon the under side of the back-bone, which trunk is the left or systemic ventricle of the heart, and sends the blood throughout the body of the fish." Cuvier's *Animal Kingdom*, p. 290, London edition, 1840.

Dr. Cartwright meets this quotation, so fatal to his theory, with the simple *assertion*, that it is "an incorrect translation of Cuvier's works." Such assertion would do very well, provided that he had accompanied it with some *proof* of its being an incorrect translation. But the proof is wanting, and the quotation will therefore have to stand, until Dr. Cartwright can rebut it with something better than bare assertion.

Dr. Cartwright quotes the following passage from Cuvier's *Histoire Naturelle des Poissons*, as though it contradicted the passage above, which I quoted from Cuvier's *Règne Animale*. Speaking of fishes, Cuvier says:

"Leur caractere propre consist en ce que leur circulation branchiale a seule à sa base un appareil musculaire, au ventricule droit des animaux dont nous venons de parler, et qu'il n'y a rien de semblable à base de système de la circulation de corps; c'est à dire que les analogues de l'oreillette et du ventricule droit leur manquent entièrement, et que les veines branchiales s'y changent aux artères sans être enveloppées des muscles."

In this passage Cuvier means simply to say, that the hearts of fishes are single, having only one auricle and one ventricle, and those on the *right* side only. Nobody denies this; nor does it conflict with the passage which I quoted above, from the *Règne Animale*, which also ad-

mits that the hearts of fishes are single; but adds, that the "arterial trunk lying immediately upon the under side of the back-bone, performs the office of the left ventricle, which is wanting.

After a long narrative of his alligator experiments, Dr. Cartwright observes, "The burden of Dr. Ely's objections to the new doctrine was chiefly founded upon the doubts of Dr. Dowler and others, in regard to the experiments of 1852—doubts which have been entirely removed by the experiments of 1853. This passage is incorrect in every particular. I do not know that Dr. Dowler ever had any doubts on the subject. On the contrary, I believe that he has always regarded "Hæmatokinety" as the greatest and most miserable of all modern humbugs. I have had many conversations with that distinguished physiologist on the subject, and he has always, so far from expressing doubts, distinctly declared that all these experiments prove nothing in reference to Dr. Cartwright's "Hæmatokinety." Nor has he pursued his vivisections with any reference to this ridiculous and unsupported theory of "Hæmatokinety." He has made a better use of his time. He has instituted vivisections in reference to the functions of the nervous system, and has arrived at many very important results.

As to my having founded my objections to "Hæmatokinety" on "the doubts of Dr. Dowler and others, in regard to the experiments of 1852," I distinctly declare that I have founded nothing upon doubts regarding any subject. Doubts are not a proper basis for any thing. I neither deal in doubts nor assertions. Dr. Cartwright appears to have a very poor memory; for in my last article, in this Journal for September, 1852, in speaking of the question regarding the tying a ligature about an alligator's trachea, my words were these: "We do not, however, attach any importance to the question, as regards its bearing upon Mrs. Willard's theory. Admitting that the ligation of the trachea will speedily kill the alligator, that fact would merely prove that the animal, like all others, cannot live without air; and not, as Dr. Cartwright supposes, that air, inhaled into the lungs, causes the blood to circulate."

The sum and substance of the whole of Dr. Cartwright's argument derived from experiments on alligators is this: The blood ceased to move as soon as atmospheric air was excluded from the lungs, and while its supposed chief motive power was hard at work;" *ergo*, "the contraction and expansion of the heart cannot be the chief motive power which circulates the blood." We think it will be difficult for any one to discover the connection between his premise and his conclusion. Dr. Cartwright's logic is as miserable as his theory. We have before refuted this argument, and we will only add, that in the

above euthymeme, the premise is false. The blood does not cease to move as soon as atmospheric air is exhaled; a short space of time must elapse; and the blood finally ceases to move because, not being oxygenated, it ceases to stimulute the heart into action. So that the heart, and consequently the circulation, does not cease its action for want of air, but for want of a proper stimulus. The air only *fits* the blood for circulation; but Dr. Cartwright, by a sort of Irish blunder, has "got the cart before the horse," and makes the air circulate the blood.

Dr. Cartwright continues: "Instead, therefore, of entering into a controversy with him, I refer him to those whose doubts deceived him." I beg to inform Dr. C. that I was deceived by the doubts of no one; and that this appears to me like a very ingenious way, on the part of Dr. C., to try to crawl out of a controversy.

We will close this paper, by adding what we have recently written in the columns of the National Intelligencer of the 22d of November:

Dr. Cartwright, who has undertaken the difficult task of proving that the circulation of the blood in animals is not caused chiefly by the contractions of the heart, but by a certain power inherent in the atmospheric air we inhale, (which power he calls "Hæmatokinety," contends that this strange theory of the circulation is fully established by the *fact* that there are "twelve thousand species of fishes which have no heart or other muscular organ at the base of the systemic circulation to propel the blood." This *fact* of Dr. Cartwright, regarding the twelve thousand species of fishes, we do not at all admit; nor do we know that any distinguished naturalist admits it as a fact, although Dr. Cartwright affirms triumphantly that "it is proved by the twenty-four volumes of the Natural History of Fishes, written by Cuvier himself; and not only by him, but by every other distinquished writer on the subject." It would be exceedingly interesting to naturalists if Dr. Cartwright would cite the names ef those "distinguished writers" on the subject who assert that there are twelve thousand species of fishes that have no apparatus for circulating the arterial blood. For ourselves, we humbly confess that we do not know who they are; but as to Cuvier, whom he includes among the number, we can assert possitively that he does not maintain the *fact* as Dr. Cartwright calls it, that there are twelve thousand species of fishes that have no muscular apparatus for propelling the arterial blood throughout the body.

Dr. Cartwright must have read Cuvier's works very carelessly, or he would not have fallen into the error of supposing that the writings of that great naturalist afforded any support to the strange idea that the heart

of animals does not circulate the arterial blood. If he will take the trouble to read the following passage in Cuvier's *Leçons d'Anatomie Comparée*, he will readily discover that Cuvier lends no support to "haematokinity ;" on the contrary, that he maintains that the single heart alone of fishes is sufficient to propel the arterial blood throughout the body :

" Lorsqu'il n'y a qu'un seul coeur," says Cuvier, "il faut que celui des deux systèmes artériels qui en manque épreuve encore l'influence du coeur unique, et que le sang y conserve son mouvement, après s'être filtré au travers de toutes les subdivisions do systeme pourvu de ce coeur ; ou bien qu'il faut qu ce système artériel sans coeur agisse assez par lui-même sur le sang pour le pousser, par le contraction successive de toutes ses parties, dans toutes ses subdivisions, et de celles-ci dans les veines, ou bien enfin, que les deux actions s'entr'aident, et c'est cette derniere idée qui nous semble le vraie. Le sturgeon, par exemple, nous donne une preuve évidente de la continuation de l'action du coeur pulmonaire sur le système aortique. A peine les veines du poumon s'y sont elles réunies pour former l'aorte, que celle-ci s'enfonce dans un canal cartilagineux qui lui est fourni par le corps des vertèbres. Elle semble s'y dépouiller entièrement de ses tuniques, et le sang y coule dans un tuyau à parois absolument immobiles ; c'est des trous de ce tuyau, ou canal cartilagineux, que sortent les branches arterielles qui se rendent aux parties. Le sang ne peut évidemment entre dans ces branches, qù en vertu de l'impulsion qu'il a recue primitivement du coeur et des artères pulmonaires."—Cuvier, *Leçons d'Anatomie Comparée*, tome 6, pp. 354–5.

From this extract Cuvier's views of the circulation of the arterial blood in fishes are very evident. He maintains that although the hearts of fishes are what are called single—that is, having but one auricle and one ventricle—still this single heart does actually propel the arterial blood through the body. What, then, becomes of Dr. Cartwright's assertion that Cuvier and other distinguished naturalists admit that there are twelve thousand species of fishes that have no muscular apparatus, no heart, for propelling the arterial blood through the body ?

But lest Dr. Cartwright may still remain sceptical on the subject of Cuvier's opinions, we will give him another short extract from that great naturalist :

"Dans beaucoup d'autres poisons les parois de la grosse artère sont adherentes, en partie, dans le demicanal osseux qui contient cette artère. Il faut donc que *l'impulsion imprimée aux sang artériel par le coeur se conserve à travers les branches, jusqu'au moins dans les troncs principaux des artères du corps;* voilà pourquoi la base de l'artère pulmonaire es distincte du reste par sa dilitation et par ses fortes parois, en partie musculaire. C'est, pour ainsi dire, un second ventricule, placé audevant du premier, et dont l'action augmente plus ou moins l'impulsion imprimée au sang par celui-ci. On le remarque même dans les *Batraciens*."—Cuvier, *Leçons d'Anatomie Comparée*, *p.* 355.

Dr. Cartwright calls Cuvier the greatest of modern naturalists, and seems to rely implicitly on all that he says. He will therefore be

obliged to admit that Cuvier completely falsifies the assertion, that fishes have no muscular apparatus for circulating the arterial blood. Dr. Cartwright ought to read Cuvier more carefully; but he is so much absorbed in his favorite theory of "*haematokinity*" that the truths of that great naturalist entirely escape him.

The great error that Dr. Cartwright has fallen into is that there must necessarily be always a left auricle and a left ventricle, similar and similarly situated to that of mammals, in order to the circulation of the arterial blood; but the above extracts from Cuvier clearly show that the left auricle and ventricle may be dispensed with, in fishes. The hearts of fishes, although called single, have in fact two ventricles, and one auricle, the second ventricle being placed after the first to give additional force to the blood. It is a distinctly muscular organ, and nearly as large as the first ventricle.

To Dr. Cartwright's assertion that fishes have no muscular organ for propelling the arterial blood, I replied in a paper in the New Orleans Medical Journal by quoting the following from the London translation of Cuvier's *Règne Animale*, not then having at hand the Paris edition. Cuvier says:

"The blood is brought to the gills by the heart, which thus answers to the right ventricle of warm-blooded animals; and from the gills it is sent to an arterial trunk lying immediately upon the under side of the backbone, *which trunk is the left or systemic ventricle of the heart, and sends the blood throughout the body of the fish.*"—Cuvier's *Animal Kingdom*, p. 290, London edition, 1840.

After pondering over this complete refutation of his assertion for nearly a year, Dr. Cartwright at last comes out in the New Orleans Medical and Surgical Journal for September, 1853, and asserts that the above extract is not a correct translation of Cuvier. He says:

"Dr. Ely had got hold of an incorrect translation of Cuvier's works, and turned the same against the new doctrine with tremendous effect in the eyes of all those unacquainted with Comparative Anatomy, and who had never seen the Napoleon of naturalists in any other than an awkward English dress."

We are greatly surprised at this broad assertion of Dr. Cartwright, which he accompanies with no proof whatever; and we are half inclined to think that it is Dr. Cartwright himself who has "never seen the Napoleon of naturalists in any other than an awkward English dress." It is quite certain that he has never read him very carefully, for if he had, he would have seen the following:

"Ce sang, après avoir respiré, se rend dans un tronc artèrial située sous l'epine du dos, et qui *faisant fonction du ventricule gauche*, l'envoie par toute le corps, d'où il revient au coeur par les veines."—Cuvier, *Règne Animale*, p. 4, Paris, 1840.

What will Dr. Cartwright say to this? And what becomes of his pompous assertion that the London translation af Cuvier, made by a body of English *savans*, is only "the Napoleon of naturalist in an awkward English dress."

After so many blunders made by Dr. Cartwright in relation to Cuvier's works, we are almost disposed to believe that his knowledge of French is as imperfect as that of his Hebrew, which he has been in the habit of quoting in the Boston Medical Journal; and which, we are credibly informed, was furnished him by a distinguished Hebrew scholar in New Orleans, the Doctor being not even familiar with the Hebrew alphabet. Of this, however, we may be mistaken. We would not judge him too harshly. But it is a little singular that being, as he pretends, so familiar with Cuvier's works, he should have entirely overlooked the passages which we have quoted, and the more so because one of the passages is in the very chapter which he quotes, and almost in juxtaposition with his own extract.

Dr. Cartwright, in his reply to my last paper in the New Orleans Medical and Surgical Journal says:

"The expression in the English translation of Cuvier, '*which trunk is the left, or systemic ventricle of the heart, and sends the blood throughout the body of the fish,*' was evidently intended by the translator to be understood in a metaphorical sense."

This is the first time we ever heard of fishes being described in metaphors. Who would ever think of looking for metaphors in a grave and elaborate anatomical description of fishes? What would naturalists think of a man who should undertake a work on itchthyology, giving all his descriptions in metaphors? Metaphors would be as foreign to the subject as quotations from Shakspeare or Hudibras. Dr. Cartwright must be driven to an extremity indeed to be obliged to attempt to invalidate my quotation from Cnvier on the circulation of fishes by asserting that it was metaphorical. Will not Dr. Cartwright favor the scientific world with a commentary on the anatomical metaphors of Cuvier? Such a work would doubtless contribute to the improvement of the style of scientific writers, and to the severe exactness of scientific descriptions.

Dr. Cartwright is equally unfortunate in entomology. He has asserted that insects have no heart, or any thing corresponding to it; that "the dorsal vessel is a heart only in a metaphorical sense," and that their circulation depends entirely upon "haematokinity." We have cited the most eminent authors, and completely refuted all of this misconception of his, regarding insects, but still he reiterates his assertions.

We must beg to be excused from examining his statements further on entomology, until he produces something like facts and arguments in support of his positions. Science is built up on observations and experiments; and we apprehend that Dr. Cartwright will have to resort to these before "haematokinity" will be able to make further progress, as all observations and experiments thus far made go directly against it.

Part Second.

EXCERPTA.

I.—*Opium in Irritable and Anæmic States of the Brain in Fever.*

BY HUMPHRY SANDWITH, M.D.

"The employment of opiates in cerebral affections," says Dr. Holland, "is a question of much interest and various difficulty;" and "there is a great scope for farther research on this subject, as on all that relates to disorders of the brain, and a strong presumption that opium is capable here of larger and more beneficial application than has yet been given to it." His subsequent remarks, in the same article, "On the Use of Opiates," embrace, but are not restricted to, its use in fever, may be consulted with advantage. Meanwhile, the profession owe Dr. Latham a large debt of gratitude for his masterly sketch of those irritable and anæmic states of the brain in fever, which demand the cautious use of this powerful narcotic. His brief but comprehensive paper on the subject, published twenty years ago, is still a beacon to guide us in the path, which his observant genius first irradiated.

The class of cases of purely irritable states of the brain is to be discriminated, as Dr. Latham shows, less by any *series* of symptoms flowing from the brain, than from the *single* symptom of a state of protracted wakefulness. Nor is the wakefulness pathognomonic *per se*, but to warrant the use of opium, it must occur in combination with an irritable state of the nervous system, induced either by depressing moral agencies, or by the physically exhausting one of alcohol. The fever may be mild, and "exhibit a *sort of contrast* with the existing affection of the brain; or it may correspond in severity with the sensorial disturbance up to a certain point, and then the symptoms referable to the brain outrun the febrile phenomena. In the latter case, though the vascular over-action may have been kept in check by general or local bleeding, still the sensorial disturbance progresses. "As other symptoms are relieved, the delirium is even aggravated." We are thus presented with two forms of irritable brain in fever,—the one being marked by simple wakefulness with no other cerebral symptom, and the other by wakefulness coupled with symptoms of high sensorial excitement. Both varieties are "incident only to those, whose habits and mode of living have been calculated to do an abiding injury to the nervous system, and who have been long actually suffering from such injury."

The same acute observer, however, recognizes another variety of sensorial disturbance in fever, which is obviously associated with anæmia. We shall quote his words:

"Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first; and *large depletion* has been required to subdue it and to guard particular organs, and especially the brain, from injury. Under such treatment, all has gone on successfully, and the patient has reached the point of convalescence, with a soft pulse, a cleaning tongue, no pain, and refreshing sleep for two or three days; when suddenly (the tongue, the pulse, and all other circumstances continuing the same) some strangeness of manner has arisen, and then the wildest delirium, and then the unrestrained passage of the evacuations. I have known the transition from such a state of *convalescence* to such a state of peril, take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours by a moderate dose of opium. This is a rare form of disease, but one in which, when it does occur, opium is eminently indicated."

Now here was an anæmic condition of the brain, but not to so frightful an extent as in the case to which I beg leave now to call the reader's attention; and I may add, that its very extent suggested, not "a moderate dose of opium," but the liberal use of the remedy.

Case—Mrs. T., a rather delicate woman, about thirty years of age, fell into fever during the second week of November, 1848, when near the close of the third month of pregnancy. Her abode was in the vicinity of open and offensive ditches; and it was soon evident that the attack would prove severe. The case ran on nearly three weeks, with symptoms of grave and increasing disturbance of the sensorium, and other indications of low typhoid fever. By careful management, indeed, had she not been pregnant, the disease might probably have evinced no symptoms of more than usual danger. On the 2d day of December, the irritation of the great nervous centres palpably interfered with gestation, and abortion was the speedy consequence. The process, unhappily, was accompanied with a very profuse hemorrhage, which ceased indeed with the expulsion of the fetus and placenta, but which in twelve hours had brought the patient into imminent peril. Along with continued sensorial disturbance, there were the signs of incipient collapse, as manifested by a rapid and fluttering pulse, unequally diffused animal heat, and laborious respiration. Moderate stimulation by wine had been latterly allowed; but it now became necessary both to give ammonia, and augment the dose of alcohol. The extreme restlessness, subsultus, and other alarming symptoms demanded, however, a cordial, on which more reliance could be placed than even on the stimulus of brandy. Life, in short, was now in extreme peril. Calling to mind the marvellous examples of the virtues of opium in uterine hemorrhage *without fever*, as recorded by Dr Stuart in the *Medico-Chirurgical Transactions*, as well as its power to sustain life in the fearful struggles of angina pectoris, and reasoning from the analogy of its virtues *in fever*, when the circulation is at the same time depressed by antimony, as also in delirium tremens, when excessive and habitual intoxication may be presumed to have exhausted the vital energies of the brain, I determined on an attempt to steady the heart, restore the capillary circulation, and calm the irritation of the nervous centres by a full dose of opium. Mr. Millin, the surgeon with whom I was attending in consultation, fully concurred with me in these views. We accordingly gave a draught containing a hundred drops of laudanum. The effect justified our most sanguine hopes. Sleep ensued, the circulation rallied; every symptom of cerebral irritation subsided, fever broke up, and convalescence was speedily established.

The present short practical paper contemplates chiefly that condition of the brain in fever, in which an anæmic state of its vessels warrants us in availing ourselves of the stimulant properties of opium, with a view to maintain the equilibrium of the cerebral circulation. In such cases the use of the drug is salutary, chiefly as it favors an amount of congestion essential to healthy sleep. Not that its effects on vascular structure are its merits; for it is fair

to argue with Dr Holland, that as "narcotic substances have effects, locally applied, on nervous sensibility," so also "there can be little doubt, that in sleep it is the same singular influence, extended more widely over this part of organization, and reaching through the cerebral parts of it, the higher faculties of our being." But in anæmic conditions of the brain in fever, the use of opium seems to be salutary, we repeat, chiefly as it favors an amount of congestion essential to healthy sleep. "It is certain, that the state of sleep and coma frequently graduate into each other, in such way as to show that the proximate physical conditions are nearly the same in both; for "one degree of pressure seems essential to perfect and uniform sleep, while a greater degree, without other alteration of State, assumes more or less the character of disease." While such changes in the circulation of the head are obviously concerned in influencing the functions of consciousness and volition, it is equally manifest that an anæmic state of the vessels is precisely the one calculated to disarm the narcotic of its dangerous properties. But for that anæmic state, the cerebral capillaries might be goaded on by a full dose of opium into fatal coma. The necessity of a healthy amount of congestion and consequent pressure, however essential to the production of sleep, may be inferred also from considerations regarding the nutrition of the organ. "The sleep of animals," Dr. Carpenter tells us, "consists not in a state of diminished energy of the *nutritive* functions, but in the cessation of the *sensorial* activity, dependent upon a suspension of the functional power of certain parts of the nervous system, during which there is reason to believe that the nutritive and reparative operations of those organs go on with even augmented rapidity." The value of sleep in this view, and the importance of a healthy circulation of the materials for nutrition, considering the wasting effects of sensorial hyperactivity in fever, are self-evident.

On witnessing such striking displays of the remedial virtues of opium, as in the case related above, one is fain to break out with Sydenham "in praise of the great God, the giver of all good things, who has granted to the human race, as a comfort in their afflictions, no medicine of the value of opium, either in regard to the number of diseases that it can control, or its efficacy in extirpating them!" After all, most of its value depends on the descrimination with which it is prescribed. When injudiciously administered, as in sthenic cerebral excitement or in the improper arrest of diarrhœa in certain states of fever, it has been observed to produce phrenitis, epilepsy, and coma; so also, when indiscriminately prescribed in delirium tremens, its employment has occasionally been followed by apoplexy. Great and marvellous, therefore, as are the virtues of opium in a variety of diseases, and admirable as are its soothing qualities in several of the forms of cerebral disorder in fever itself, yet let no man venture to prescribe it for the latter (whether in large or small doses) in the dark or at random. It is a sharp-edged tool, and of such fearful potency, that, if it fulfills not a curative intention, it will probably destroy life. There is no instance, in the whole range of practical medicine, more imperatively demanding a sure diagnosis. In short, our warrant to prescribe it hinges on our ability to ascertain precisely that condition of the brain which alone will admit of its safe employment.

It was a rare sagacity which led Dr. Graves to employ tartar-emetic in combination with opium in those cases both of idiopathic fever and delirium tremens, in which the narcotic would probably stimulate to over-congestion of the brain, but for the depressing action of the mineral on the heart and capillary circulation. The complex practice finds its parallel in the analogous operation of opium in states of anæmia in fever.

Medical science is more advantaged, perhaps, by defining the circumstances to which our known remedies are applicable, than by exploring the resources of nature for others. Not only is great discrimination necessary in deciding on the cases which demand the use of opium, but also in regulating the doses

adapted to each variety. Little can be added to Dr. Latham's admirable directions. Our rule of conduct indeed is suggested by the degree of sensorial excitement. "Simple wakefulness may be gently lulled to sleep by a few drops of laudanum, but wild delirium requires to be mastered and (as it were) forced into repose by a much larger dose." Opium, however, goes much farther; and, therefore, a much less dose is required in quelling asthenic sensorial disorder, combined with fever, than when it exists alone. Five minims in the milder cases, and twenty in the graver, may be considered minimum and maximum doses in fever. Much, after all, must be left to our vigilance in watching its effects, and to our discretion in judging when to desist, and when and in what doses to repeat the remedy. There are yet other "cases where the indications for the employment of opium are *doubtful*." We shall describe the variety of sensorial disturbance in fever in Dr. Latham's own words:

"Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is a *certain jerk* in the pulse, so that we cannot help suspecting that the blood vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man; but I have also seen the same quantity produce a fatal coma, from which he has never been roused."

Dr. Latham's recommendation, therefore, is in so dubious a case, and to avoid "striking a heavy blow in the dark," to administer a small dose at intervals of one hour or two," so as to stop short of actual mischief at the first glimpse of its approach, or be led by a plain earnest of benefit to push the remedy to its full and consummate effect." Judicious as is this advice, we cannot but think, that on the mode of treatment of cases, in which, on Dr. Latham's own showing, "the indications for the employment of opium are doubtful," any practice which aims at once to keep down vascular action and soothe the nervous system, is a real improvement. And such, we need scarcely add, is the complex method of Dr. Graves.

It only remains that we should revert to the *anæmic* type of sensorial disturbance in fever, in that milder variety of it which, as described by Dr. Latham, we placed in contrast with a much graver form, "a moderate dose of opium," he tells us, suffices to change a state of peril to one of convalescence. But his remarks do not contemplate so frightful a form of anæmia in fever as that of which we have given an example, and which might result equally from a large intestinal, nasal, or bronchial hemorrhage, as from uterine He speaks of heroic doses of opium only "in extreme cases of delirium tremens," while twenty minims of the tincture, he asserts, are quite sufficient for the purpose of subduing "the very same symptoms, carried to the greatest extremity, *when combined with fever*." But while "a moderate dose" suffices to relieve the less urgent anæmic forms of the disease, the case related above as having occurred to my own observation, seems to sanction a bolder practice in the perilous cases of an extremely ex-sanguine condition of the brain in fever, coupled with the exhaustion resulting from protracted sensorial excitement.

(*Rankings Abstract.*)

II.—*The Blood—its Chemistry, Physiology, and Pathology.*

BY THOMAS WILLIAMS, M.D.

The fluids and solids in all biochemical inquiries should rightly be studied in conjunction. They interblend, subtantially and dynamically, with such intimacy, that to indicate a partitional boundary were to divorce what nature

has united. They reciprocally cede and accept. They mutually act and react. Fluidity and solidity are the first conditions of all organized matter. The aeriform state has nowhere a single illustration. Every gas must become a fluid as the fundamental term of union with the elements of the living body. The fluid is the active condition; the solid, the passive. The latter is sedentary; the former, locomotive. The former is the scene of rapid moleculer transformation; the latter is distinguished for its stability; between these two extremes there exist manifold intermediate conditions. In the category of the nutritive fluids, unmixed fluidity is not known; solids are introduced. Fluids are not self-productive; they require either the direct or the catalytic agency of solids. The fluids even of the vegetable organism bear floating corpuscles. In the vegetable economy, it has hitherto been falsely supposed that the cells of the fixed tissues traversed by the fluids effect that change which, in the instance of the animal fluids, is accomplished by the self-borne corpuscles. The formative capacity of a fluid, in the abstract, is limited to a low standard. Albumen cannot mount into the condition of fibrine in virtue of any self-originated and self-sustained inherent molecular activity; the agency of a third body must be interposed. This is the "doctrine of the schools."

The blood is a wondrous epitome of fluids and solids. In complexity of composition it has no parallel. He who would resolve it into its components, must in himself unite the qualifications of the chemist and the microscopist, the anatomist and the physiologist. The chemistry of organized beings has now assumed the exalted rank of a rich and varied science. The microscope, at first a costly toy, has grown into its inseparable handmaid. Micro-chemistry is opening upon the mind like a vast panorama, opulent in wonders and boundless in limits. The microscope conducts the eye to the confines only of visible form; the formless eludes its scrutiny; it is, emphatically, an instrument fitted to define the geometric properties of bodies. Into the world of fluids it cannot penetrate. Fluidity is a condition of matter which it cannot appreciate. Of solids, it takes cognizance only of the state of quiescence; it does not reveal the "forces" impelling them to activity; it is an apparatus of inquiry into the statistics, not the dynamics, of matter. In micro-chemical inquiries, its peculiar value consists in empowering the eye to read and register the changes of form and color wrought by test-fluids upon solids: the knotted tangle of a multiplex compound is thus gracefully unwound; it is in this elegant manner that mind arrives at a knowledge of the chemical constitution of bodies—even those so minute in mathematical dimensions as to occupy the remote frontier-line which divides the fluid from the solid state, the extreme verge of morphological substance.

From the arena of zoochemical research, in these unsuperstitious days, all substanceless divinities must he exterminated. The era of the *anima mundi* has become historic. The "essence of life," *vis vitæ*, *vital principle* (Prichard); *nisus formativus* (Blumenbach); *organic force* (Muller): *organic agent* (Prout); *vis medicatrix naturæ* (Hoffman and Cullen) now number amid the effete jargon of an extinct nomenclature. The "illuminating orb" of a new mental life has arisen above the edge of the eastern horizon. "Abstract terms" are as luring as "golden calves; they demand the homage of believers. The "*principle* of motion," the "*principle* of gravitation," the "*principle* of attraction," are the relics of an entombed terminology. The "*life*" of the blood is an ontological expression which belongs to a former phase of science. No *divinæ particula auræ* swims in this fluid. Modern philosophy displays a tendency to refer all phenomena to the *laws* of matter—*directions* of action first implanted by the Deity. A "law," in its ontological sense, is a *deus ex machina;* it sounds as though endowed with an existence distinct from the body —as something superadded to the organism—like magnetism to iron, heat and light to a luminous body, *sic.* The word "*principle*" is characteristic of a less advanced state of science; it should *now* be used only as final letters of

the alphabet are employed by the algebraist—to denote an unknown element, which, when thus indicated, is more conveniently analyzed. It is customary, even now, to speak, of the "principle"—the agent of heat, light, electricity, magnetism, gravitation, etc.—as signalizing the severally undetermined causes of familiar phenomena. When these phenomena, in the progress of science, fall under a more just interpretation, they come to be referred to the primary properties of matter; they then may be deduced by demonstrative reasoning, like geometric theorems from the postulates on which they are founded. But in the science of physiology the term "law" has been employed in a less justifiable acceptation; a "principle," a "force" has been personified, and invested with a spontaneity of action. As an expression descriptive of the "conditions," the "circumstances" under which the actions and reactions of the material elements of the organism originate and proceed, it may be tolerated as a convenient phrase; the physiologist, however, idolatrously animates it with a creative and directive power, in virtue of which it acts upon matter removing its particles from the pale of physical and chemical laws, transforming them into organized tissue, endowing these tissues with novel properties, prompting them to action, opposing resistance to the injurious influences, defining the cessation of these acts of the organic body as synonymous with the departure of the "vital principle." A "law" ought not to be defined as altogether resident in the mind of the observer. The mind only links certain acts into a certain order of occcurrence; this *order of occurrence* is coeval with matter; it moves, "displays its forces," exerts its energy," only in a given direction. This *mode* of occurrence is the *law;* it is the final impress of the will of Him who made matter; it is an ultimate fact beyond which philosophy cannot penetrate. The task of the organic chemist, then, lies not in a search after an *ignis fatus*, a *vital principle;* he is required only to define the terms of events, the conditions of phenomena, as they occur in the living organism. *Words* must be used to indicate events. The exigencies of language will oblige the use of such expressions as vital force, affinity, property, &c. It will however be now understood within what latitude such expressions are to be limited.

Two new ideas have recently taken birth in science. The mutual convertibility of the "forces" is an accepted doctrine, and the transmutability of matter [Dumas, Faraday] is no longer held as an alchemic extravaganza. The facts of allotropism, established by the genius of Schönbein, and the late prophetic speculations of M. Dumas on the chemical electrical, and mathematical *progression* traceable through the properties of isomeric or conformable bodies, awaken in the mind of the modern chemist a reverence for the disentombed manes of alchemy. The transmutable bodies group themselves in nature in triads, or ternary series, thus: chlorine, bromine, and iodine; sulphur, selenium, and tellurium; calcium, strontium, and barium; lithium, sodium, and pottassium. The members of these triads, severally, are capable of replacing one another in chemical compounds. "When three bodies, having qualities precisely similar, though not identical, are arranged in succession of their chemical powers, there will be also a successive arrangement of mathematical powers, indicated by the respective atomic numbers of the substances, and amenable to every mathematical law. "That this symmetry of chemical with mathematical function points to the possibility of 'transmutation, is unquestionable, yet not transmutation in the sense of the old alchemical philosophy." Chemists see no manifestations of a tendency of being able to convert lead into silver, or silver into gold. These metals are not chemically *conformable;* one cannot take place of another by substitution; they do not form an isomeric group.

The preceding illustrations are drawn from inorganic bodies. Chemists have long believed that certain organic compounds display, in their properties, a close resemblance to metals. Of this kind are the three organic radicals—

C2 H2 O
C4 H5 O
C6 H7 O

which may be regarded as the three several oxides of an isomeric triad, bearing analogy to those already adverted to in the inorganic world. With reference to these radicals [emitting the oxygen] it is found, as in the case of the inorgauic triads, that the sum of the atomic weights of the extreme bodies, divided by two, is equal to that of the intermediate body.

The discovery of allotropism has bereft these speculations of impossible extravagance. The allotropic modifications of which sulphur, phosphorus, oxygen, and carbon are susceptible, suggest the thought that the countless array of organic compounds, which now bewilder the chemist, may prove only modified forms of one unchanging radical. Like those of isomerism, the phenomena of allotropism inspire the zoochemist with new hopes.

Solidified albumen and fibrine are allotropic conditions of liquid albumen and fibrine. Physically the former differ remarkably from the latter, chemically they are identical. "There was a time when the doctrine, which supposed the convertibility of metals, was opposed to known analogies: it is now no longer opposed to them, but only some stages beyond their present development." [Faraday.] These discoveries have been accomplished, not by the magic touch of the philosopher's stone, but by the touch of genius.

Coincidently with these recondite speculations as to the transmutability of *matter*, have been projected novel thoughts as to the "mutual correlation and convertibility of '*forces.*'" [Grove.] Heat, light, electricity, magnetism, motion, etc., are severally inter-producible "forces." [Faraday.] "Vital action," cell-growth, nerve-force, muscular action, *are the* physical imponderable forces modified in manifestation by passage through an organic material substratum. [Carpenter.] Heat becomes vital force by passing *from* without *into* the egg. Such expressions imply the locomotion of an entity. Against the vague use of this hypothesis-involving language, the student of organic chemistry should jealously guard. He must be warned once more, lest he confound the *ego* with the *non ego*—the conditional with the absolute and unconditioned. All general ideas are born of abstraction. They should be permitted in science only as intellectual guides. The "forces" of matter are the "properties" of matter. The quality cannot be separated from the body. The word "property" in chemistry should be accepted in a phenomenal, not entological sense. The dynamics of matter cannot be studied apart from the statistics. Though, however, the student be warned against the idolatrous worship of all bodiless essences; though it be affirmed that the physical philosopher is in pursuit, not of fictions, but facts, not of subjective creations, but objective realities, it must be understood that the intellectual "aid to pursuit" to be drawn from an intelligently conceived theory is not to be contemned. A "theory" is not an illusive palace raised by fancy; it assists in two ways; it guides the mind in the marshalling of novel phenomena under known analogies—and it dictates method, suggests conditions, devises experiments. It ought not to trammel the freedom of research, nor circumscribe the range of just speculation. It is a mental light, an intellectual instrument of inquiry. It is the bright path projected by forethought into tracts of research yet unsurveyed by the eye of science, and uncatechized by experiment.

"Chance" may have imparted motion to the classic chandelier in the Cathedral of Pisa; but if chance also suggested to Galileo the laws of the pendulum, it must have belonged to that multitudinous order of casualties by which ideas are ordinarily propagated in fit and fertile minds. Theoretic provisions had qualified the mind of the philosopher to evolve an immortal principle out of an accidental event, to generalize a "law" on the basis of an accident. "Facts," uninterpreted by "theory," are inert, incoherent verities.

A *vital fluid* is different in no single particular from any cosmical compound

fluid. The constituent elements of an organic fluid, completely interblended in the living organism, manifest the same laws and the same properties with those distinctive of them while yet within the region of inorganic matter. In the body they acquire no *new* properties. The *old* are only modified, intensified, neutralized, coordinated with, or subordinated to, "conditions" of action known nowhere but in the living body. An organic substance may be composed of five different elements. Each element may be present in a great number of equivalents explains the complexity of organic compounds:—thus,

	Carbon. Atoms.	Hydrogen. Atoms.	Nitrogen. Atoms.	Sulphur. Atoms.	Oxygen. Atoms.	Total. Atoms.
Albumen,	216	169	27	2	68	482
Caseine,	288	288	36	2	90	544
Fibrine,	216	169	27	2	68	482

A "force" exerted in *one* direction—i. e., between *two* elements—unites them in the closest and strongest manner. The result is a compound which is gfted in the highest degree with the power to resist the causes of perturbation. To this compound if one more element be added, the uniting force is reduced to one-third—it is now exerted in three different directions; if a fourth constituent be added, each line of force will be four times less than the sum of the power by which the first two elements were joined. If this reasoning be prosecuted to the limit of 500 atoms, it admits of no doubt that the power will remain the same in *nature*, though multiplied in *directions* of action. The centre of force remains unchanged, though the radii therefrom may diverge to numerous points of an embracing circumference. Where, then, is the limit to be inscribed between vital and cosmic chemistry, between organic and non-organic affinity? Is it at 8, or 10, or 20, or 500 equivalents? Is it at the fifth element, or the fiftieth? Is it at carbonic oxide (C_2O_2=28) and cyanogen (C_2N=26,) or at starch [C12 H10 O10] and sugar [C12 H11 O11?] Where between oxalic acid and those composite products containing sulphur and phosphorus, can the chemist mark the bound beyond which the physical force does not ascend? In the formation of chemical compounds, "organic principles," it is quite certain that the supposed line cannot be drawn which is said to divide physical from vital affinities. Thus, then, the instability, the ready decomposableness, of organic compounds is a property which flows from the multiplied directions in which the uniting power operates upon the constituent atoms. In an atom of sugar the attraction radiates in 36 different lines—in one of olive oil, in many hundreds. But this denotes merely a difference of *number* in the combining elements. Is there no difference of *kind*, of arrangement? Without adding or withdrawing any element, we may conceive the 36 simple atoms of which the atom of sugar consists to be arranged in a thousand different ways; with every alteration in the position of any single atom of the 36, the compound atom ceases to be an atom of sugar, since the properties belonging to it change with every alteration in the manner of the arrangement of its constituent atoms. It will be subsequently proved that whatever may be the groupings of elementary atoms, no *new* force will be required to explain their altered disposition. There is, however, in the study of organic structures, a point at which the evidences of a *new* power are encountered. It is in accordance with analogy to suppose that albumen, fibrine, gelatine, etc., owe their formation to the play of purely chemical affinities. The power which moulds a cell, configurates a fibre, shapes the tubularity of a vessel, transcends the conceptions of the pure chemist. It cannot be experimentally catechized, it cannot be imitated by any artificial arrangement. It is a force *sui generis*. The corpuscles of the blood assume the discal figure, not mechanically, in virtue of a rotary or elliptic motion like the planets in the orrery. In the latter case the *figure* may be predicated from a calculation of the attractive and centrifugal forces. The oblate spheroid is a *necessary* figure. In the attempt to explain the *form* of organized solids, all mathematical and

mechanical conceptions utterly fail. Wherefore the discoidal shape of the blood-cell? why the elliptical in the fish? *This is the vital force;* chemistry compounds the materials, vitality chisels them into form. Chemistry unites the elementary atoms into composite groups; vitality utilizes these compound groups, and no other, in the fabrication of the solids. The cells are the only parts of the fluids which, in this sense, exhibit the evidence of a vital force. The hematosine and globulin and cell-walls are chemical products. "Vitality" gives to these products a special arrangement; but no other "principles" could be shaped, even by the vital force, in corpuscles. It requires conditions of action as well as every other force. There is, therefore, some mysterious relation between the *shape* and the *substance*, between the *material* and the *form*. The cells of the blood, though the constructive principle were provided, can be formed nowhere but in the blood—no other locality offers the required conditions. Muscular fibre can only be formed out of previously prepared compounds, and *then* only in a particular place. Thus obviously governed by "conditions," the *vital* force, as much as any other force, becomes a legitimate object of investigation. Allied intimately to the chemical force, it discovers a mechanical and mathematical mode of operation. In this inquiry the difficulty has hitherto proved insurmountable, of ascribing effects to their true causes—of explaining the share which belongs to each "force" in the arrangements which constitute life. *Med. Chir. Review.*

III.—*On the Rational Treatment of Spasmodic Affections.*

We earnestly direct the attention of the reader to the following practical observations, based on physiology and pathology, on the therapeutics of spasmodic affections, from *Contributions to Rational Medicine*, by Dr. A. Wood, of Edinburg. (*Ed. N. O. Med. and Sur. Jour.*)

1st. The importance of great attention to a proper supply of nutriment and of air in all circumstances, where either hereditary tendency or other circumstances are likely to develop convulsive diseases. Trismus nascentium is epidemic in the West Indies, from the absurd way in which infants are there treated. The same disease was banished from the Dublin Lying-in Hospital by proper attention to ventilation and cleanliness.

Laryngismus stridulus is never so satisfactorily treated as by change of air. The mortality of tetanus in our naval stations in the West Indies has been very much reduced, mainly, according to Dr. Dickson, by improved hygiene. —(*Med. Chir. Trans.* vol. vii. p. 765.)

2d. Let us divest our minds of the idea that it is necessary to treat the fit in any of these diseases. It is only a part of a train of morbid phenomena, and though the part most striking to the bystander, ought not to make the same impression on the intelligent physician. No one now thinks of treating the fit either in epilepsy or chorea, and why should we think it necessary in tetanus or hydrophobia?

3d. Is it not worthy of consideration, whether the obstinate constipation in tetanus and lead poisoning may not be a spasm of the muscular coat of the intestines analogous to that of the voluntary muscles, and like it, not to be overcome by brute force, (that is purgatives?) Certain it is that in lead colic, the finger, introduced into the rectum, is held by the sphincter as by a vice, and pressed tightly upon by the gut, and that this pressure returns at each paroxysm. Certain it is also, that the retraction and hardnes of the abdomen,

associated by Merat with that internal constriction, is found also to exist in tetanus, though I am not aware that any one has ever explored the internal state of the bowel in that disease. It may be necessary to remove the morbid secretions in the bowels, though every intelligent physician will have to balance the amount of irritation produced by their presence with that caused by the drastic purgatives necessary for their removal. It is to treat symptoms, to attack it alone ; and when we succeed in overcoming the disease, the bowels will spontaneously relieve themselves. Tralles found opium succeed in a case of ileus where purgatives had failed. I have seen the same. Lentillus has confirmed this ; and in a severe form of colic, Bonn became convinced by experience that it was the most powerful remedy.

4th. The most efficient and the most frequent agents in the production of these diseases are sedatives. Bloodletting is a most powerful sedative, and if carried to any extent in a healthy person, produces convulsions. Is bloodletting, then, a suitable remedy in these affections ? In chorea it was formerly practised, and is now abandoned from the injurious effects which it produced. In delirium tremens and hysteria, its use has also been given up. In epilepsy it is rarely used by intelligent physicians, except to meet the requirements of secondary affections; and if we still retain its employment in the more convulsive diseases, it is probably only because their rapid course and frequently fatal issue makes it very difficult for us to ascertain the effect of any treatment.

5th. In one class of convulsive diseases, bloodletting seems, in the present state of our knowledge, to be indispensable. Where urea exists in the blood and produces convulsions, it must either be expelled or counteracted. We scarcely know how to accomplish the latter indication, and therefore are driven back on the former ; but even while seeking by bloodletting to get rid of the urea which is mixed with that fluid, we must never forget that it "both acts on the nervous system as a narcotic poison and impoverishes the blood, inducing degeneration of the tissues," and that therefore, while we take blood to remove the poison, we must do our utmost to replace the nutriment which we are unavoidably compelled to abstract along with it.

6th. Our views on this subject would become more definite and precise, could we avoid imagining that spasm implies augmented strength. It is not easy for us, when seeing the violent agitation of the system which prevails, to divest our minds of the idea of great power being developed, but the same remarks apply to mania, in which disease, thanks to the exertions of Dr. W. A. F. Browne, general depletion, tartar emetic, brisk cathartics, and ice to the shaved scalp, are no longer in such vogue as they once were.

If such are the objections to the routine practice, what course ought to be pursued ? It is easier to point out error than to demonstrate truth. But I venture to suggest—

7th. That sedatives should be cautiously used. Chloroform and cold affusions have each proved fatal in delirium tremens.

8th. That every effort should be made to put into the system as much nourishment as it is capable of beneficially employing.

9th. But if there is any faith to be placed in antipathic treatment, it is to stimulants we must not trust.

10th. It is worth observing, that most of those chemical agents which produce convulsions, acting, as has been already said, in large doses as sedatives, do as small doses act as stimulants.

11th. We find also, that where the minor spasms, as cramp, have been excited by irritation of the peripheral distribution of one class of nerves, as those of the mucous lining of the bowels, they are often relieved by irritation of the peripheral distribution of another class of nerves, as by friction on the skin.

12th. The cerebral functions, more especially volition and sensation, being

much in abeyance, any stimuli, whether mental or physical, by which they can be excited, should be freely given. It was on this principle that Boerhaave prevented the recurrence of epileptic attacks, by directing a red hot iron to be applied to any who might be seized.

13th. The extreme sensitiveness to all irritations which exist whenever the spinal predominates over the cerebral system, suggests the propriety of enforcing the most absolute quiet, and preventing the access of all bodily and mental stimuli. In tetanus and hydrophobia, the creaking of a shoe, the slamming of a door, the sight or even thought of water, or the gentlest zephyr playing on the surface of the body, excites a fit. Hence Armstrong tells us, that in tetanus those patients recover best who get little active treatment, but are nursed, as it were, through the fit.

14th. And if sources of irritation ought to be prevented, those actually existing ought to be removed, but never by irritants which are more powerful than themselves. This surely gives sufficient latitude to the most devoted admirer of the gum lancet, the bolus, or the bag and pipe, while it suggests to him a little caution in these somewhat coarse, though most popular remedies.

15th. And lastly, if I have not succeeded in pointing out any one remedy, which stimulates the cerebral without exciting the spinal system, it is because few, if any such, are known. It is a subject of investigation well worth attention. And if no particular plan of treatment has been announced, which can be unhesitatingly recommended, I am perfectly satisfied if I have created any doubt as to the course which at present is too unhesitatingly followed. I think at least, something has been done to show how much more constitutional these affections are than is usually supposed, and thus open up new plans for prevention, if not for cure. And, if in acute mania, in puerperal insanity, in delirium tremens, in chorea, in laryngismus, and may I add in epilepsy also, empiricism or the influence of authority has induced us to abandon antiphlogistic treatment, and to adopt stimulant and soothing remedies, I do not despair of a sounder pathology soon prevailing in regard to this whole class of affections, and of the discovery yet being made, that they are much more amenable to treatment than has hitherto been supposed.

(*Ranking's Half Yearly Abstract Medical Sciences.*)

IV.—*Uterine Contractions excited by Terebinthenate Injections.*

Late French journals speak of turpentine injections as a valuable means of stimulating the uterus to increased contractions. It has been used in this way to bring about, with a medical view, premature delivery, and to hasten natural labor at full term, when the uterus failed to expel its contents. Over ergot it posseses several advantages; it does not, like this medicine, excite vomiting, whilst it acts with more certainty and greater celerity. When the os is sufficiently dilated, and the head fairly presenting, we may with perfect safety administer turpentine enemas, combined with a suitable proportion of mucilage, with every assurance of hastening the expulsion of the fœtus. To produce artificial delivery, turpentine enemas should be preferred to the usual violent mechanical means resorted to in such cases.

V.—*Rennet as a remedy in Diabetes.*

Some experiments have recently been made by Dr. Gray and others, with rennet in the treatment of diabetes. Several cases are reported in a late No. of Ranking's Abstract, in which the free use of this article had produced the most marked beneficial effects. It is given in tea spoonful doses after each meal, until the flow of urine is diminished and the sugar entirely disappears in this fluid. In the meantime, a suitable course of diet is instituted, as auxiliary to the cure. From the testimony adduced, with the cases reported, we have every reason to entertain a favorable opinion of the remedy proposed.

VI.—*Occlusion of the Vagina.*

Dr. Storer read the following case before the Boston Society for Medical Improvement, sent to him by Dr. Brainard of Chicago, and published in the American Journal.

July 7, 1852, I visited Mrs. ———, a young married woman, aged 19 years, in the central part of Wisconsin, on account of an obstruction of the vagina.

On attempting to pass the finger into the vagina, it was arrested at its very orifice. Immediately behind the carunculæ myrtiformes there was a firm cicatrix, which entirely shut up the passage, there being a transverse band from side to side, both above and below which there was a slight depression scarcely half an inch deep. On introducing a catheter into the bladder, and the finger into the rectum, the septum seemed thin, and as thick as the natural septum of the bladder and vagina. The uterus was felt distended, filling up the cavity of the pelvis, and rising so as to be felt in front two inches above the pubis. She was constantly affected with severe expulsive pains.

This young woman, who was but 19 years of age, and presented appearances of not fully developed womanhood, was married on the 8th of October, 1851. About the 20th of November following, she was attacked with great pain, hemorrhage, profuse discharge of pus and mucus. There was constitutional disturbance of the severest kind, and her life was despaired of. There followed a discharge of shreds and sloughs, horribly offensive, which continued several weeks. This attack was taken for an abortion, but without good reason. It was simply a violent case of acute vaginitis, from too frequent sexual connection with the sexual organs imperfectly developed. She informed me, in answer to inquiries, that coition was very painful, and became more and more so.

Pains, indicating the return of the menstrual period, occurred February 1, 1852, and recurred twice at four weeks' interval without discharge. From that time the pains were continuous up to the time the operation was performed, the uterus gradually enlarging.

Operation July 8, 1852. The patient being placed in a suitable position, an incision was made from before backwards through the band before described extending the diameter of the vagina. The sides being separated by the finger, it was found that the mucous membrane of the rectum could be separated from that of the bladder with the point of the finger, without great violence. When a band of tissue resisted, it was divided with a blunt-pointed bistoury. The separation was freely made to the uterus.

Here, however, instead of finding the os uteri, nothing but a smooth round tumor presented itself. The finger was carried over its surface, and the surface denuded for a space at least two and a half inches in diameter. Finding no mouth, I determined to make a puncture at a point where a slight elevation was felt, and where it was presumed the os had been situated before it sloughed away. Accordingly, I made an incision in the form of two thirds of a circle an inch in diameter, raising up a piece attached towards the anterior part. There was a copious discharge of the menstrual blood. The wall of the uterus was thick, and gave the sensation of fibres in cutting. Passing the finger within, a slight depression was found opposite the little tubercle noticed externally, seeming to confirm the idea that this was a vestige of the neck.

After treatment, a tent, or pessary, one and a half inches in diameter, made of pieces of sponge strung together upon a stick, and covered with oiled silk, was passed into the artificial vagina. The point was sufficiently small to press into the opening of the uterus, and the whole long enough to press externally. The case was entrusted to the care of Dr. Robert W. Earll, to whom and to Dr. Axtell, I was indebted for assistance.

Not a bad symptom occurred; no swellings and but little inflammation. August 25 she was able to walk from her room without difficulty.

Four months after the operation, November 8, she was well, doing the work of the house and able to ride several miles. The cicatrization was perfect, at least that was Dr. Earll's opinion. The menses have occurred regularly without pain, excepting the first, August 20, which was painful. She is unwilling to dispense with the pessary, as she experiences a sensation of falling of the womb without it, even when lying. Sexual connection is not painful, and there is no tendency to contraction.

May 30, 1853, she continued in the same satisfactory condition.

VII.—*Sugar of Milk an article of Food in Consumption and other Pulmonary Diseases.*

BY JAMES TURNBULL, M. D.

Those who are familiar with Liebig's works are aware that he established several years ago the fact, that all the various substances used as food, belong to one of two classes; the azotised or plastic, which form the tissues of the body, and replace the worn out tissues; and the non-azotized or combustive, which furnish food for the lungs, supporting respiration and animal heat. Now it is a curious fact, which he also established, that though the fibrin, albumen, and casein, which constitute the chief of the first or azotized class of alimentary substances, exist in vegetables as well as animal food, animals have not the power of forming in their own bodies any of those azotized alimentary principles. They are primarily derived from the vegetable kingdom; and the digestive organs of animals have no power of producing them, but merely of assimilating what has been already formed by plants, or previously drawn from the vegetable kingdom, from some other animal. These views met with considerable opposition when they were first advanced; but their correctness is now generally admitted, and there is no essential difference in the chemical composition of fibrine procured from vegetables and that obtained from the flesh or the blood of an animal, or between vegetable and animal albumen or casein.

Of the other class of alimentary substances—the non-azotized—the chief use of which is to supply food for respiration and the support of animal heat,

the principal are starch, sugar, oil or fat, and alcoholic liquors. These unite with the oxygen absorbed at the lungs, and are the chief sources of the carbonic acid and watery vapor given off by these organs. They are in fact burnt by a process of slow combustion, which is the great source of the high temperature of animals.

It appeared to me, that as this function of the lungs must necessarily be more or less impeded in all pulmonary diseases and as cod-liver oil had been found so beneficial in that particular disease, consumption, advantage would be gained by selecting from this, the non-azotized, or combustive class of alimentary substances, such of them as would have the greatest tendency to unite readily with the oxigen absorbed at the lungs—and thus, in the disabled condition of these organs, to facilitate the performance of their functions.

I was thus led to inquire which of the non-azotized or combustive class of alimentary articles are most readily digested, and have the greatest affinity for oxygen. Sugar of milk is an article belonging to this class of aliments, which possesses these properties in a high degree, and is deserving of more attention than it has yet received as an article of food. I shall therefore state a few facts respecting it, which seem to me sufficiently interesting to be worthy being brought under the notice of the profession.

There are three principal varieties of sugar—cane sugar, milk sugar, and grape sugar. They are closely allied in composition, though they differ considerably in chemical properties. All kinds of milk contain sugar of milk; but it is worthy of notice that asses' milk, which has always had a greater reputation than any other kind, as an article of food in consumption and other pulmonary diseases, contains the largest proportion, relative to the caseous and oleaginous principles, of any kind of milk. Whey, which consists almost entirely of sugar of milk, has also been found a useful article of diet in consumptive cases.

When we inquire into the chemical properties of milk sugar, we also find that it has so strong an attraction for oxygen, that when dissolved with an alkali, it has the power of reducing more or less completely some of the metallic oxides. It is readily absorbed into the blood, which being an alkaline fluid containing oxide of iron, furnishes the necessary conditions for its oxygenation. Besides this, its composition is such, that it must be readily converted into carbonic acid and water. There is only one other point in relation to its fitness to supply material for respiration, which I shall at present notice. It is the fact, originally pointed out by Liebig, and now admitted by physiologists, that one of the great offices of the liver is the preparation of combustive material for the respiratory process. This is a point which has not been sufficiently kept in view by medical men; but it is one of great practical interest when we consider, that the function of the lungs and that of the liver are so intimately connected and mutually dependent, that derangement of the secreting function of the latter must necessarily interfere with the former, and may not improbably be one of the chief causes of a tubercular state of the blood. The liver prepares the combustive materials for respiration; and of this there are two sources, one being the worn out tissues of the body, the hydrocarbonaceous part of which forms bile, and being re-absorbed, is consumed at the lungs; the other the saccharine and fatty matters of the food, which are consumed in a similar way. It would seem, however, that the liver has not only the power of preparing the latter, but also of forming saccharine at least, if not oleaginous matters, from the blood. A defect in this power may be one of the great causes of tubercular diseases; and if we can, by giving a ready formed oil, which is stored up at certain times in the liver of the codfish, rectify to a great extent any defect in its action, so far at least as the oleaginous material for respiration is concerned, there is good reason to expect that still more may

be gained by giving, in a ready formed state, the other combustive material, the saccharine.

The facts I have brought forward have led me to use sugar of milk in the treatment of consumption; and as I have seen benefit from its use, I wish to recommend it as an article of food deserving of more attention in the treatment of this disease than it has yet received. I believe also that they embrace an important principle, applicable to the dietetic treatment of other diseases.

(*Assoc. Medical Journal, June* 24, 1853.)

VIII.—*Nympho-Maniacal Hysteria.*

M. Sandras defines hysteria "an habitual nervous condition, in which occur, at longer or shorter intervals, paroxysms characterised by a peculiar sensation of strangulation, by oppression of the respiration, more or less acute pain in the head, and by clonic convulsions in all or nearly all parts of the body." This rapid description, the only definition possible, omits a double phenomenon, which is very common in hysteria, and of which a patient in M. Grisolle's ward at La Pitié, presents a remarkable example. We refer to anæsthesia (absence of tactile sensibility) and analgesia (absense of sensibility to pain.) However rude the pressure exercised upon different portions of her body, she does not perceive it. A feather may be introduced into the nostrils or pharynx without exciting the slightest sensation. A pin may be passed through the fold of the skin without producing pain. This woman is therefore affected with anæsthesia and analgesia, but not in every point; a very circumscribed portion, the septum of the nasal fossæ is still sensible, and it is sufficient to pinch this between the fingers to occasion a convulsive paroxysm.

Every sensation may be decomposed into three elements, which are, the impression, the transmission of the impression, and the perception. The impression and its transmission are not indispensable, inconceivable as this appears at first. Do not those who have been subjected to amputations experience sensations referable to the limbs which they have lost—and are not sensations reduced to perceptions in hallucinations? In the particular case of which we are speaking, in hysterical anæsthesia and analgesia, which of the three elements of sensation are wanting, or are they all absent?

The patient of M. Grisolle is interesting in another point of view; and here again we have to signalize an omission in M. Sandras' article on hysteria. It does not mention the libidinous, erotic, or nympho-maniacal form of hysteria; a form which is nevertheless common. Not only is M. Sandras silent in regard to the erotic form of hysteria, but he denies that hysterical women "are more disposed than others to take an active part in sexual intercourse;" and he adds, that the contrary rather is true.

In M. Grisolle's patient the erotic form is very marked. This woman's first action, when her paroxysms come on, is to seize upon the nearest man, and she moves the pelvis in a voluptuous manner throughout the whole duration of the attack. At the visit, when she is surrounded by the medical staff, her desires are seen to become excited whenever she is spoken to. This is not nymphomania, for the hysterical paroxysms are perfectly characteristic. On that score it is sufficient to say that she is considered hysterical by such an observer as M. Grisolle. It will be easily understood, that under such circumstances no attempt has been made to ascertain the condition of the sensibility of the genital organs.

M. Grisolle saw, some years ago, a case of hysteria in a very distinguished

woman of fashion, thirty-five years of age, the widow of a man, who, having become the victim of satyriasis, subjected her to the rudest assaults. It cannot be doubted that the privation of sexual intercourse, succeeding such great abuse, was the cause of the hysteria in this case, and for this reason. This lady soon married a robust man, and under the influence of marriage the hysteria was dissipated.

Relatively to the treatment of this strange neurosis, the case we present is likely to modify the opinion of M. Sandras, who expresses himself in these terms: "I have never witnessed a case in which the enjoyment, or even the abuse, of venereal pleasures, have produced a diminution or cure of hysteria."

The seat, or rather the starting point of hysteria, is very obscure, and is the subject of much dispute; the majority of physicians and philosophers have placed it in the womb. Vain attempt at localization! Is it not true that hysteria has occurred in a woman without a womb, and also in men?

M. Grisolle has seen one example of hysteria in a woman in whom the uterus was absent. The anatomical fact was established by M. Chassaignac and himself. It was easy to be satisfied in regard to it by introducing a sound into the bladder and a finger into the rectum. On either side could be felt two small bodies supposed to be ovaries. The hymen was intact, and behind it, instead of a vagina, there existed an opening of scarcely more than a line in depth. The external genital organs were normal The mammæ very large. Strangely enough, hysteria assumed the strongly characterized erotic form in this unfortunate (à cause de son extreme lubricité) was greatly sought after by the nurses of the hospital. Is it not clear, after this case alone, that the cause of hysteria is not in the uterus, neither in venereal appetite?

Where then does it reside? We say simply, without entering into considerations which would carry us too far, that it varies, probably, in different cases; but, that wherever it may be, the morbid action, even if eccentric, inevitably rebounds upon the nervous centres, and from these irradiate the influences which produce the multiplied neuropothic phenomena which constitute the fantastic symptomology of hysteria.

As to venereal desire, it arises in the central nervous system, in a point which we do not now seek to determine (apparently in the cerebellum), but which is affected, on the one hand, by the excitement of the imagination, and on the other, by physiological or morbid excitement of the external organs of generation.

Is hysteria diathesic, that is to say, connected with a general condition of the economy, or does it consist in an organopathy limited to the nervous system? This is still a problem unsolved by the sagacity of pathologists.

(*Gazette des Hôpitaux.*)

IX.—*Fistula in Ano, treated by Iodine Injections.*

BY M. BOINET.

At a meeting of the Institute, of August 1st, M. Boinet read a memoir, designed to demonstrate the efficacy of injections of iodine in the radical cure of fistula in ano, whatever their form, extent, or complications. Seven cases are detailed, which offer examples of almost every variety of fistula—complete, blind, or incomplete fistula, deep fistula, with loss of substance of the intestine, and fistula in tuberculous subjects. These observations tend to prove that iodine injections may be advantageously employed in all cases of fistula, but especially in those in which the method by incision is dangerous or ineffectual,

such, for example, as extend deeply, or occur in phthisical patients, or depend upon some alteration of the ischium, the coccyx, or sacrum.

The advantages of iodine injections over the ordinary method, consist, in obtaining a cure more easily, and in a shorter time, in avoiding pain and the danger of hemorrhage, and in permitting the patients to continue at their usual avocations.

The following are the conclusions of the memoir:

1. Iodine injections, properly administered, can cure radically all cases of fistula, whether complete or incomplete, simple or complicated.
2. They cure them more promptly than the method by incision commonly employed, and with less danger.
3. They produce no pain, and are easily practised.
4. They permit patients to follow their occupations, and relieve them from daily painful dressings.
5. They are applicable to all cases, and especially to those in which incision or excision are dangerous or impossible.
6. They do no harm, even if they are ineffectual, and do not prevent the subsequent use of the knife. It is therefore rational to employ them before having recourse to a cutting instrument (*Ib.*)

X.—*Structure and Function of the Spleen.*

There are still numerous opinions almost constantly being advanced on the structure and function of this complicated organ. Buk supposes that the colorless corpuscles of blood which are to be changed into the colored ones, are formed in it. This takes place by the passage out of the twigs of arteries ramifying on the Malpighian vesicles of an organizable lymph, which thus gets into the lymphatics. (He believes the Malpighian corpuscles to communicate with the lymphatics.) Here the first developed elements of the blood, the colorless corpuscles, are formed; part being transferred to the lymphatic vessels, and part to the veins. Thus the venous blood contains an important component not found in the arterial.

Tigri, in a paper reprinted from an Italian medical journal, and written in order to vindicate his claim to priority over Assor and Kolliner, in their researches on the function of the spleen, re-asserts, as the results of his microscopic and other investigations on the spleen of men and animals, the following conclusions:

The spleen is an organ which nature has destined to preside over the material composition of the blood. It receives into its vessels blood loaded with solid matters for elimination; these are the used up epithelial cells and red globules, which are assimilated in it, and reduced into new principles of nutriment.

1. The anatomical elements of the spleen are blood and lymphatic vessels, to which are united the Malpighian corpuscles, the fibrous structure, the microscopic web, and the splenic fluid.
2. That it is not credible that the vessels of the spleen, looking at their size, as compared with that of the organ, are destined only for its nutrition.
3. This is confirmed by observing that in other organs in which the blood has to undergo a modification, there exists two orders of different vessels, i. e., the pulmonary and bronchial arteries for the lung, the hepatic artery and portal vein, for the liver.
4. Similarly, these two orders of vessels must be recognized in the spleen;

the first comprises the nutrient vessels; the second, those which carry into the venous system the blood loaded with eliminable materials.

5. The special conformation of the splenic venous canal of ruminants, visible from the point of their entrance into the organ, has reference not only to the form of the canal, but also to the structure of its walls.

6. To the form, which is cylindrical, but irregular, from hollows and projections, to which he gives the name of splenic productions.

7. To the structure, inasmuch as the parietes of the veins are formed by the red substance of the organ, together with a most subtle and transparent membrane, which divides it from immediate contact with the blood.

8. This membrane, organized like that of the capillaries, performs the office of a filter, and gives passage to the red globules of the blood, which are rendered inactive as well as the epithelial bodies.

9. This structure, so visible in the large venous trunks of the spleen of ruminants, is verified also in that of the horse, pig, and lastly in the human spleen.

10. The communication between the arteries and veins of the second category, by the intervention of a capillary system, is effected by channels so ample, as to permit the easy passage of bodies as large as one third of a millimetre.

11. The spleen pulp otherwise is not a dense liquid, but an assemblage of fusiform nucleated cells, involuted or folded on themselves, isolated nuclei, and red blood globules, which elements are contained in a most delicate areolar web.

12. The presence of the Malpighian bodies is undoubted.

13. The structure of the spleen presents no resemblance to the cavernous bodies.

14. The microscopic web, with its areolæ, is in direct communication with the venous cavity, by the porosities of the stratum limiting the isolated or confluent splenic productions.

15. The same web is in communication with the lymphatics.

16. The epithelial bodies, detached from the walls of the vascular system, and mixed with the circulating blood, are brought by the artery into the splenic tissue, in which there is every reason to believe that they are arrested.

17. The same happens to the worn out blood globules.

(*British and Foreign Med. Chir. Review.*)

XI.—*Diabetes Mellitus not Incurable.*

M. Schutzenberger, Professor of Clinical Medicine at the Faculty of Strasbourg, has just published a case, which would tend to show that diabetes mellitus may be in some degree arrested, and the patients restored to comparative health. After detailing the case at length, he comes to the following conclusions:

1. Glucosuria is not an incurable disease, and although the tendency to relapse is certain, it is possible, by perseverance, not only to remove the sugar from the urine, but also to bring the patients so far as to bear a varied diet, in which feculent substances may enter without causing a relapse.

2. The amount of glucose excreted by patients is sensibly proportionate to the quantity of feculent matter added to the food, and it is possible to discover

errors of diet by the increase of the glucose in the urine. The quantity of urine is equal to that of the fluid ingested, and the latter also proportionate to the amount of the feculent substances taken into the economy.

3. Diet forms the principal part of the treatment of glucosuria; the food ought chiefly to consist of milk, fatty substances, butter, oil, eggs and meat.

4. The entire banishment of feculents seems necessary to make the sugar disappear completely.

5. Small quantities of bread, viz: about three ounces per diem, are usually well borne, and do not excite a fresh secretion of sugar when once it has disappeared.

6. The powers of assimilation increase gradually, and it is possible, by means of chemical analysis, to ascertain the extent to which the peculiar diet and medicine ought to be carried.

7. The effect of the diet is powerfully assisted by the use of certain therapeutic agents, and especially by opium, in increasing doses, and by alkaline drinks. In this affection the tolerance of the opium is very great.

8. It is certain that the glucose is formed in the alimentary canal, and that the absorbents generally take it up, so that solid motions come to contain no sugar.

9. Purgatives may cause a diminution of glucose in the urine, as more or less of the sugar which would have passed into the urine is carried away by the liquid stools.

The author does not enter into any theoretical discussion touching glocosuria, as his object was merely to call attention to a series of interesting and instructive facts.

(*Lancet, August 6th*, 1853.)

XII.—*Ligature of the Femoral Vein.*

BY M. ROUX.

At a meeting of the Surgical Society of Paris, on the 27th of July, M. Roux asked leave to communicate an observation which had recently occurred in his practice.

A patient had been operated on twenty-eight days previously for a voluminous tumor, occupying the whole of the inguinal region, and descending into the scrotum.

The tumor was composed of various elements, but contained no cancer cells; it had relapsed five times.

It had been removed four times by Chelius; it first appeared eight years ago; the last operation was done ten months since. The constitution was not impaired.

After having dissected the tumor superficially, when it became necesssary to separate the deep seated portions, the surgeon avoided the artery, which it was necessary to dissect for a considerable extent, but it was not possible to avoid the crural vein, and this was opened.

I thought it best, said M. Roux, to place a ligature above and below the wound. The wound was above the junction of the saphena.

At the moment I applied the ligature, the limb became livid and cold—but, on the following day, the coloration was less, and the temperature was normal.

On the third and fourth days a considerable œdema supervened, invading the

whole of the limb quite up to the hip. A few days afterwards erysipelas came on, and passed through its regular stages; and then an abscess formed on the side of the foot. This was opened and readily healed up. At present the patient is in a very satisfactory condition.

The operation lasted two hours; the patient was under the influence of ether the whole of this period.

The wound has nearly cicatrized.

This fact, said M. Roux, in conclusion, appears to me most important. Perhaps I was the first who called the attention of surgeons to the danger of wounding the femoral vein above the saphena. A priori this should be considered a most serious lesion; nevertheless, in this case, perhaps unique, we see that the accidents were altogether insignificant.

(*Gazette des Hôpitaux.*)

XIII.—*Furunculus.*

M. Nelaton (Gazette des Hop.) observes, that the development of furuncles may be always arrested by keeping the part covered with a linen compress, which has been dipped in concentrated alcohol. This must be accurately applied to the part, and care taken to keep it constantly moist, so that evaporation may be constantly taking place from its surface.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on operative Ophthalmic Surgery.* By H. Haynes Walton, Fellow of the Royal College of Surgeons, England, Surgeon to the Central London Opthalmic Hospital, etc., etc., etc.

In his preface, Mr. Walton says: "As an untried writer, I cannot be otherwise than anxious for the success of this work. I have spared no pains in my attempt to make it of practical value, and I have laid under contribution all that has come within my reach or my knowledge, whether English or Foreign on the subject of Ophthalmic Pathology and Surgery."

In addition to the vast amount of book-knowledge to which Mr. Walton has free access, he also brings to the task the results of his own personal experience and observation, which must have been very great, from his holding for a long time the post of Surgeon to the *Central London Ophthalmic Hospital.* Thus much then for the auspices under which this work was elaborated by the author.

The first chapter is devoted to the consideration of the "*Use of Chloroform in Ophthalmic Surgery.*" He pronounces Chloroform one of the great improvements in the Surgery of our times; and rejoices that its benefits have been extended to operations on the eye. By its tranquilizing effects upon the patient's mind it prepares him for those

delicate operations on the eye, which not only require a steady hand in the operator, but likewise, demand on the part of the patient the most perfect repose of the parts. In operations of this sort upon children, the use of Chloroform is especially indispensable,—for without it the struggles of the child may frustrate all the foresight and dexterity of the most consummate operator.

Mr. W. awards the honor of the first discovery of an Anæsthetic to this country, to the "*New World*," as he expresses it. Of the glad tidings, he says:

"This discovery, fraught with so much interest to the suffering part of mankind, spread with unusual rapidity over the world, and in an incredibly short period, ether has been inhaled in the most remote quarters of the globe. He then goes on to give a short sketch of this great discovery, mentions the parties who justly claim the credit, those also, who have pointed out the precautions which are required in its administration, and particularly singles out Mr. Snow, who stands pre-eminent for his zeal and successful efforts in this important question.

In the *Second Chapter*, he describes the various instruments required in operating on the eye, furnishing plates, both full and accurate; but we refer to the text for particulars under this head.

The *Third Chapter* discusses the *injuries from mechanical or chemical* agents. Among these are most common and serious burns and scalds, which end in deformity and permanent injury to the parts on account of contractions which are apt to result from the thinness and looseness of the structures concerned in the parts adjacent to and surrounding the eye.

It requires therefore particular care and constant attention to arrest and limit as far as possible the suppurations which are so liable to follow burns and scalds of the lids. For this purpose, the author speaks in high terms of the application of cotton wool; it shuts out the air and absorbs the morbid secretions. This application is preferable to cold water dressings, as it can be easily removed and as often reapplied.

If the *cutis vera* be deeply involved, no treatment that can be instituted will obviate the resulting contraction and deformity.

If the injury be superficial, sprinkling the part with some simple powder, or penciling it with the solution of the nitrate of silver, or covering the whole with gold beater's skin or gum, should be preferred to collodion which is too painful to be borne. For *ecchymosis* about the eyes he discards leeches, denounces punctures, and only in

particular cases advises cold or warm sedative lotions, as the feelings of the patient may require.

In speaking of the different states of the constitution as influencing and modifying the products of inflammatory action, Mr. Walton quotes the late Mr. Dalrymple "*on the rapid organization of lymph in cachexia,*" and the latter gentleman tried to ascertain if effusions of the organizable materials of the blood become vitalized by the production of new vessels more readily and sooner in cachexia, than in robust states of the constitution. After a number of careful observations on this point, he thus sums up his convictions :

"The conclusion he arrives at is, that in those who are enfeebled and depressed, effusions from the capillary vessels are more speedily and completely organized, with vessels capable of being permeated by minute injections, than in the more vigorous and plethoric, in whom inflammation is more acute in the outset, and passes through more speedy and determined stages. The greater tendency to the effusion and organization of fibrine on the surface of the iris in syphilitic cases, than in those of idiopathic iritis is noticed ; and the remark is made, that there will be no difficulty in admitting that the specific cases occur, at least in London, in far greater proportion, in enfeebled constitutions, and in those debilitated by excess and irregular habits, or the mal-administration of mercury for the primary disease.

A well-marked case of syphilitic iritis, peculiarly valuable and well worthy of perusal, is given in support of this view, and further evidence is adduced of the rapid organization of lymph in other instances of disease where life has been nearly extinct."

Mercury in diseases of the eye, is considered by the author to be highly beneficial; at the same time he acknowledges that, in these, as in many other diseases, it has been much abused. This salt does not always, says he, control inflammatory affections of the eye, for he has seen irritis attack persons who were laboring under the effects of mercury, for syphilitic diseases. In one case, at St. Bartholemew's Hospital, the iritis notwithstanding the patient was thoroughly salivated, proceeded so far in spite of treatment that the eye was totally lost.

We have seen iritis develope itself in syphilitic subjects whilst fully under the influence of mercury ; in such cases, the iodide of potash the mecurials being suspended, acted favorably, aided by counter irritation and revulsives.

Mr. Walton tells us that in his practice he is in the habit of using mercury in very small doses. He gives the *hydrargium cum creta*, in two and three grain doses, combined with hyosciamus or conium, as the state of the bowels may require. This combination is effectual, mild in its action, and answers all the purposes of an alterative and antiphlogistic. He regards salivation as a poisonous influence on the

system, and thinks all the benefit desirable may be accomplished short of ptyolism. Our author positively condemns salivation as necessary to cure syphilitic iritis; it is rarely necessary to push the mercury to this extent in this affection.

Mr. W. advises the artificial dilatation of the pupil whenever there is a tendency of the iris to adhere to the capsule of the lens. To effect this, he prefers the sulphate of atropa to the belladonna, in the proportion of two or three grains to the ounce of water, of which a drop or two applied three times a day, on the conjunctiva of the outer part of the lower lid, will produce all the effects of dilatation. An ointment, made of one grain of the atropa to a drachm of lard, may be applied to the lid with excellent effect.

This work condemns, except in extreme cases, the evacuation by puncture of the aqueous humor in inflammation of the eye ball, as recommended and practised by Mr. Wardrop. Both Mr. Lawrence and Dr. Jacob reject the practice, whilst Mr. Tyrrell is a strong advocate for the operation, particularly when the size of the chamber was sensibly augmented, and when the globe was very tense and tender, associated with much ophthalmia or sclerotitis.

The operation is thus described:

"Should chloroform not be used, considerable difficulty will be found in exposing and steadying the globe. When the patient is insensible, the operation is very simple. The humor should be let out with the smallest possible puncture, with a needle, one rather broader than the straight solution needle, or with the point of a cataract or iris knife. The operator stands behind the patient, who may be sitting or lying down, and proceeds as if about to operate for cataract, by raising the lid with the forefinger, fixing the globe with the point of that, and of the middle finger, while an assistant depresses the lower lid. The globe thus steadied, the point of the instrument is introduced close to the edge of the cornea, and when fairly through the cornea, turned on its axis to make the wound gape, when the aqueous humor will flow out."

The accumulation of pus in the anterior chamber of the eye, should not be evacuated by puncture, except in rare instances; with active purgation and counter-irritation behind the ears, the matter may be removed by absorption, and the sight restored. Mr. W. is not a warm advocate for blisters either *behind* the *ears*, or to the *temples*; but we must acknowledge that we have seen Dr. Stone of this city apply them with excellent effect in chronic ophthalmia in our great Charity Hospital.

Wounds of the eye, produced by blows, sometimes cause effusion of blood in the chambers; but such effused blood may be readily absorbed by the use of cathartic medicines and cold lotions to the organ.

Mr. W. never ventures to evacuate the blood by puncturing the cornea; he has never been disappointed in seeing it absorbed.

The crystalline lens may be displaced—thrown forward by blows on the eye or head, and if the capsule be unbroken, it should be at once removed by operation, as the chances for its absorption are exceedingly small indeed.

Hernia of the iris produced by its protrusion through a wound of the cornea, may be reduced sometimes by dropping a solution of the *sulphate of atropa* on the conjunctiva. Our author mentions an instance of this kind. He moreover hints of the practicability of enlarging the corneal wound, the patient being chloroformised the while, and thus relieve the strangulated or strictured iris and restore it to its natural position. It would require great skill and expertness on the part of the surgeon, and absolute repose of the patient, to execute this delicate little operation.

A common eye-probe may be used to produce a prolapsed iris, when not too closely grasped by the cornea.

The application of caustic to the protruding iris is recommended by good authority; and we have seen it used in the Charity Hospital of this city with good effect in similar cases.

Some practical observations then follow on the injuries to the eye by chemical and mechanical agents; except in certain cases, but little can be done to neutralize the destructive effects of chemical agents when accidentally brought in contact with the eye; for the most we must be content to combat the resulting inflammation or ulceration. Our author advises a thorough exploration of the eye, and particularly the duplications of the conjunctiva, in cases where we have reason to suspect the presence of any foreign substance—such as sand—small pieces of iron, &c. When we are unable to detect them with the eye, and yet, from the sensation of the patient, suspect their presence, a stream of pure water must be driven by a small syringe over the globe of the eye. In this way, we once relieved a female whose eye was charged with a number of small irregular pieces of glass, which accidentally got into the eye by having her head thrust violently by a brutal husband through a pane of window glass.

Foreign bodies sometimes are projected with force and are driven into the cornea or sclerotica, giving rise to much pain—sometimes inflammation and other troublesome symptoms. These should be removed as early as practicable, when all pain &c. will disappear. Several examples are given by the author, and among these are the following:

"The following case of the impaction of metallic particles occurred in the practice of my friend Mr. Browne, of Belfast, and illustrates many practical points. The entire surface of the cornea, and the greater portion of the conjunctiva scleriotica, were literally paved by fine particles of iron. A young man in an iron foundry was drilling a hole in the cylinder case of a steam engine; he stooped down to observe his progress, and holding a lighted candle, there was an explosion of some gas that had collected between the cylinder and case. The eye was scorched, and the particles from the drilling thrown on its surface. The cornea was scraped of its epithelium, and the particles of iron removed to an extent that saved the eye, and rendered it useful. Deformity, however, remained from the stains on the cornea, and the presence of some of the iron in the conjunctiva, from which but a small quantity of the filings was extracted. Some of the metal was even under the conjunctiva."

"In the sixth volume of the 'Dublin Medical Press,' we also find an account of a nail in the eye: Mary Barron, admitted on the evening of the 29th May, 1841, into the Richmond Surgical Hospital, stated, says the reporter, that about two hours previously, when shaking a carpet, a small nail, which had been concealed in it, was jerked against the centre of her right eye. The shock it caused was very great, and slight nausea and general weakness followed. A nail supposed to have done the injury, was found in the carpet. Very shortly afterwards she observed, on her apron, a small glairy substance, which from her replies to questions respecting it, was probably the crystalline lens. The palpebræ of the right eye were much inflamed and swollen, the chemosis being so extensive as almost to cover the cornea, which presented a depressed appearance with a slight oozing of blood. Not the slightest trace of anything like a foreign body in the eye could be discovered. Up to the 15th of June there was scarcely any alteration in the state of the eye, notwithstanding the vigorous antiphlogistic measures which had been pursued; and on the evening of this day the eyeball was found greatly enlarged, very painful, and, apparently, hopelessly disorganized. Under these circumstances Dr. O'Beirne thought it advisable to make a free opening into the cornea; the incision was followed by a considerable discharge of purulent matter mixed with serum, and the patient expressed herself relieved. Whilst making this incision, Dr. O'Beirne felt the point of the lancet grate against some hard resisting body, and at the moment stated his decided opinion to the class, that there was some foreign body in the eye. He thought proper, however, to desist from any further examination on this day; but two days after, having by a careful examination satisfied himself of the presence of a foreign substance, he proceeded to extract it with a pair of long-limbed forceps, and readily succeeded in drawing from the eye a small black nail, of about three-quarters of an inch in length, having a broad and flattened head very nearly a quarter of an inch in diameter. It is a singular fact, that the point of the nail was in front. Immediately on the withdrawal of the irritating body, the distressing symptoms ceased."

Certain structures of the eye, like other parts of the system, may become ossified,—for instance, the capsule of the lens is sometimes ossified—when it acts as a foreign body and requires removal. The choroid and the hyaloid membranes; the retina and all the textures of the eye have been found more or less ossified, and calcareous concretions within the eye have been observed. We find the following examples of this kind of degeneration:

"From a man who was blinded by lightning forty years before death. The choroid membrane contains several small thin plates of bone at its posterior part; the lens is absorbed, and its capsule ossified.

The eye of a blind man in which large plates of earthy matter or bone are formed in parts of the retina and on its inner surface. There is a second specimen similar to this.

Parts of an eye dried; the lens, converted into a mass of white compact earthy substance, nearly retains its natural size and form; some irregular portions of earthy substance, extend also from it into the vitreous humor.

The sclerotica has been found partially ossified. Mr. Middlemore alludes to the *post-mortem* examination of an idiot boy, at St. Bartholomew's, where ossification of the greater part of the globe of each eye existed.

In the museum of St. Bartholomew's Hospital, there are sections of the eye of an adult, showing that the retina has disappeared and its place become occupied by a thick layer of dense osseous substance.

A remarkable case of the impaction of a grain of duck-shot in the optic nerve, where it lodged for six years and six months, is recorded in the "London Medical Gazette," for 1834, vol. xiii. The shot entered the eye at the inner side of its surface, near the cornea. Occasional and intense pain, for four years and a half, and the serious disturbance of functions of the other eye, induced the patient, contrary to medical advice, to have the body sought for. The lens, which is said to have been partly bony, partly calcareous, was removed with the hope of affording relief, but without benefit; pain continued, and the sight of the other eye being endangered, an attempt to find the shot was made, but unsuccesfully. The sufferer now determined to have the eye extirpated, and the shot was found impacted, as the report says, in that part of the optic nerve which expands and forms the retina. The right eye was daily getting into health when the last communication was sent to Dr. Butter, the operator."

A long chapter is devoted to affections of the eyelids; but as these may be treated according to the common principles of surgery, we shall not stop to notice any special points discussed. We refer to the work itself for ample details.

Chapter Sixth treats of "*affections of the Puncta—the Canaliculi and the lachrymal tube.*"

From the minuteness of these structures, they are seldom the seat of disease, although they may suffer from mechanical injury—contractions from ulcerations proceeding from burns. To give the reader a distinct idea of the excellent views laid down by the author on the subject, embraced under the above head, would require more space than we devote to this subject; we therefore again refer to the text for full details.

Caries of the orbit next demands the consideration of the author; but we confess we have not read his observations on this point.

Chapter Eighth goes at great length into the examination and treatment of *nævi materni*—a pathological point, which we had imagined appertained to surgery proper.

We must here close this notice, with the recommendation to our subscribers to procure the work and read it. The plates are well executed and the text neat and well printed. T. L. White, No. 105 Canal street, keeps the work for sale. A. H.

II.—*Dr. Hooper's Vade Mecum: or a Manual of the Principles and Practice of Physic*, by Wm. A. Gry, M. D., Cantab, author of Medical Jurisprudence, 1853.

We have heretofore spoken in condemnatory terms of "Manuals, Vade Mecums," and their congeners; in the present instance however, we must except from this class, the compendium of Dr. Hooper's work by Mr. Guy. In Great Britain, the work has already passed through many editions with additions and emendations; and at this time it is very popular with all classes of practitioners in the old world.

As a treatise on the practice of physic, we know of few books better calculated to guide the doubting, or give confidence to the wavering in perplexing and trying emergencies than the compendium before us.

No theoretical doctrine or mooted question occupies a place in the pages of the work, it presents only those well ascertained and leading facts of the science, which are absolutely required of every general practitioner who undertakes to cure diseases.

The Book is arranged under the following distinct heads:

1st. The characteristic symptoms by which diseases are known.

2d. The causes from which they most frequently have their origin.

3d. The circumstances that more especially point out the difference between diseases which resemble one another.

4th. The signs which influence the judgment in forming a prognosis of their event.

5th. That mode of treatment, which in the present improved mode of treatment is deemed most appropriate and which experience has sanctioned.

At the close of the volume, a select collection of useful Formulæ are given which renders the work more valuable. White, 105 Canal street, has the book for sale. A. H.

III.—*The Practice of Surgery.* By James Miller, F.R.S., F.R.C.S.E., Surgeon in Ordinary to the Queen of Scotland, etc. Third American from the Second Edinburgh edition, with additions by Sargent. Illustrated with 319 engravings. 1853. Philadelphia.

We have from the first been a warm admirer of the writings of Mr. Miller, because he is sound in doctrine, simple and graphic in style, and practical in his aim. In proof of this, read the first chapter on "Oper-

ations," in which he vindicates surgery from the opprobrious charges which have been leveled at it, because it achieved good through blood and much suffering. Thanks to chloroform, this latter objection can be no longer urged with reason; whilst the former is terrible only to the child and the highly nervous. Within the last quarter of a century the knife has been resorted to only in extreme cases; the bloody bistoury and reeking knife have been superceded by the more mild and persuasive means so profusely scattered throughout the materia medica. The progressive spirit of the age has been conspicuously shown in the surgical department of our profession. *First*, in a better knowlege of the mechanical means requisite to correct the deformities of the physical system. *Second*, in a more thorough acquaintance with the diagnosis and treatment of disease.

Surgery, in the strict sense of the term, has been narrowed down to a mere appendage of the healing art; and Dr. Physick long since remarked that the merely *operative* part of surgery in any one of our large cities would not support one medical man in a respectable style. If this was true 20 years since, it must throw a damper upon the enthusiasm of those who aspire at the present day for fame as operative surgeons.

Mr. Miller's work is progressive in tone and matter; it teaches the science of surgery as well as the art. He is conservative, because he is enlightened on the general principles of medicine, and is therefore the best guide that the general practitioner can follow. It is, in a word, the best work of the day on the principles and practice of surgery. We would advise all who wish to understand the "*healing art*," to secure a copy of this truly useful work.

J. B. Steel, 60 Camp street, has copies for sale. A. H.

IV.—*A Treatise on the Venereal Disease.* By John Hunter, F.R.S. With copious additions, by Dr. Philip Ricord, Surgeon of the Hospital *du Midi*, Paris, &c., edited with notes by Freeman J. Bumpstead, M. D., Physician to the North Western Dispensary, New York.

Ricord and Hunter! names which stand in such bold relief before the Medical world, and whose writings have enriched the pages of many a book, whether the chosen theme has been the fluids or the solids of our *vile compound* of "*mud and blood*," whether in a disquisi-

tion upon the blood alone, or in treatises, or most *ravishing* letters upon syphilis, need not the aid of some humble plea to point out their excellencies, if at times, (since to err is human) a vein of *gentle* satire might pass upon their foibles. The present treatise on venereal disease is by John Hunter, with copious additions by Ricord; the notes are by F. I. Bumpstead, M.D., of New York.

There are some important points, upon which the two names which we have noted just now with exclamation, have widely differed: and first upon Gonorrhœa, we quote from Hunter.

"It sometimes happens that the parts which become irritated first, get well, while another part of the same surface receives the irritation, which continues the disease, as happens when it shifts from the glands to the urethra."

From this circumstance of all Gonorrhœas ceasing without medical help, I should doubt very much the possibility of a person getting a fresh Gonorrhœa while he has that disease, or of his increasing the same by the application of fresh matter of its own kind.

From all this it is reasonable to suppose that such a surface of an animal body is not capable of being irritated by its own matter; nor is it capable of being irritated beyond a certain time; and, therefore, if fresh venereal matter were continued to be applied to the urethra of a man having a Gonorrhœa, that it would go off just as soon as if no such application had been made, and get as soon well as if great pains had been taken to wash its own matter away. The same reasoning holds good in chancres.

Mr. Hunter carries the point still further, and asserts "that the parts become less susceptible of the venereal irritation; and that not only a Gonorrhœa cannot be continued by the application of either its own or fresh matter, but that a man cannot get a fresh Gonorrhœa, or a chancre, if he applies fresh venereal matter to the parts when the cure is nearly completed, and continues the application ever after, or at least at such intervals as are within the effect of habit. I can conceive that in time the parts may become so habituated to its application as to be insensible to it: for by a constant application the parts would never be allowed to forget its irritation, or rather never become unaccustomed to it, and therefore this supply of fresh matter could not affect the parts so as to renew the disease till they first recovered their original and natural state, and then would be capable of being affected again."

The above sentences embrace the opinion of Mr. Hunter on this head. With due reverence to the authority, doubt might naturally

arise as to the correctness of the doctrine; and first, is it a fact that all Gonorrhœas cease without medical help? If so, how comes it that the urethral discharge continues after the inflammation has subsided, or until the patient, worn out by distress and annoyance consequent upon the irritation produced by such discharge, seeks medical aid as a *dernier* resort? And if, as it is asserted by Mr. Hunter, "the venereal matter formed in a Gonorrhœa does not assist in keeping up that Gonorrhœa," of what avail is it that we order careful ablution for the removal of "matter" from this and other surfaces upon which it is formed? The objection is reconciled by Mr. Hunter in the following words: "that no matter of whatever kind can produce any effect on the part that forms it." Yet may we not again urge, that the *stimulus* of the discharge increases the inflammation? With the view of arresting the discharge we employ astringent agents in the abortive treatment, a decrease of inflammation being generally consequent thereon.

Opposed to the opinions of Hunter, as already stated, we have those of Ricord in these words: "The more numerous attacks of Gonorrhœa have been, the more easily the disease is contracted anew; and the succeeding affection is developed with more ease and rapidity in proportion to the traces remaining of the previous attack.

It is well known that the most common cause of relapse, especially in Gonorrhœa, is the too hasty repetition of sexual intercourse before the cure is perfect, and even before convalescence commences."

The different modes recommended at the present day, for the varieties of stricture, by their respective advocates, and the annoyance, (not to mention the more serious consequences) resulting from the malady, may, perhaps, have induced Mr. Hunter to allot many pages of his book to the discussion of this subject.

Authorities have generally agreed to assign (not to say solely) the cause of stricture, to Gonorrhœa; of course perineal injuries (which frequently give rise to the most serious strictures) must always be excepted.

Mr. Hunter, however, goes so far as to question if stricture ever arise from gonorrhœa; yet keeping in mind the fact, that any source of irritation to the uretha, if sufficiently long continued, will produce stricture, and bearing still farther in mind the fact, that a gonorrhœal discharge does produce urethral irritation, so that its walls, at certain parts, are contracted, as is evidenced in the forked stream of the urine; we have, it would seem, a refutation of the opinion of Mr. Hunter; even were we to admit, (and which circumstance does not admit of doubt,) that

the urine itself may be so changed from its normal standard as to be a cause of irritation, so much so as to produce stricture. Upon this head Mr. Babington holds the following words: "Many well authenticated facts disprove the common prejudice which attributes stricture invariably to gonorrhœa or to the use of injections. But when the author (alluding to Mr. Hunter) goes so far as to question whether it ever arises from these causes, his opinion is contradicted both by reason and by experience."

Passing over the primary and simplest manner of treating stricture, by dilatation with the sound or bougie, (and which observation and experience furnish abundant proof, is very frequently palliative rather than curative,) we recur to Mr. Hunter on the application of caustic to strictures, who, according to his annotator was not the first to employ this method of treatment; Alphonso Ferri and Ambroise Paré having employed cauterization at an earlier date.

Mr. Hunter observes, "about the year 1752, I attended a chimney-sweeper laboring under a stricture. He was the first patient I ever had under this disease; not finding that I gained any advantage, after six months' trial with the bougie, I conceived that I might be able to destroy the stricture by escharotics; and my first attempt was with red precipitate. "I applied to the end of a bougie some salve, and then dipped it into red precipitate." The mode of thus applying an escharotic, which, to effect any good, must reach the strictured part, will best account for its failure; and as might have been anticipated, Mr. Hunter continues, "I found that it brought on considerable inflammation all along the inside of the passage," The subsequent plan which he adopted, a rude porte-caustic of the present day, soon had the desired effect.

Mr. Hunter, prior to his death, improved upon his original plan of applying caustic; yet upon the whole, he seems not to have been well pleased with the result, for his concluding opinion is, that, "caustic has no advantage over bougies in respect to the permanent cure of the malady. Experience has fully proved that after a stricture has been removed by caustic, bougies are still necessary, and that unless they are occasionally passed, it is almost certain to recur."

Ricord recommends cauterization under two varieties of circumstances, which may be briefly expressed as follows: "Whenever the stricture still permits the passage of the urine, but resists the introduction of every other instrument;" and again, "when dilatation is without effect, or is too slow, or when the stricture remains stationary, after having yielded to a certain degree."

The same writer, in conclusion of this subject, remarks: "It must

be confessed that some strictures will not yield to dilatation, and are aggravated by caustic. Such are, in a large number of cases, those which depend on cicatrices, bands, callosities, inodular indurations, and fibrous degenerations. Here, however, art still offers to true practitioners resources which all speculative theories cannot gainsay; for in these refractory strictures, we may, though difficulties still confessedly remain, oppose the action of cutting instruments, of which Hunter does not speak, though they were employed before his time."

We will now take up another subject or two, not so fully treated of in the book under consideration. The intimate relation, by sympathy or otherwise, which some of the diseases of the bladder bear to the whole of the urinary apparatus; and the serious and sometimes fatal consequences which ensue from diseases of that viscus, (the bladder) might reasonably have lead the reader to hope for, and to expect something more than a summary dismissal of this branch of the author's writings; especially when we consider his marked ability, and his deservedly high reputation.

One of the most painfully distressing and annoying (not to mention those of graver character, which demand the most prompt interposition of the surgeon) diseases of the bladder, is extreme irritability, not caused by obstructions to the passage of the urine, and which in the course of time so completely exhausts the nervous system, that in a greatly emaciated state the patient succumbs.

Mr. Hunter, in the brief allusion which he has here made to this source of disease says: "the symptoms of this disease are very similar to those arising from obstructions to the passage of the urine in the urethra, but with this difference, that in the present disease the urine flows readily, because the urethra obeys the summons and relaxes; however, there is often considerable straining, after the water is all voided, arising from the muscular coat of the bladder still continuing its contractions."

Regarding irritability of the bladder most frequently to arise, (next to stone and tumors forming on the inside, in which event obstructions to the passage of the urine would almost infallibly occur,) from an altered quality of the urine, whether it be in an excess of acids or alkalies, or in a deposit of mucous with needle-shaped crystals, and knowing that the urethra will very generally take on an irritable state from the same cause, which irritation, (granting its continuance a sufficient length of time,) will produce stricture; we must again call in question the correctness of the statement of Mr. Hunter, as given at page 234, "that in the present disease the urine flows readily, because the urethra obeys

the summons and relaxes," (!) when in fact, the only *summons* to which the urethra will *readily* respond, is upon the application of a bougie!

Truly does Mr. Hunter observe in reference to stone, etc., in the bladder, as well as from irritability arising from other causes, "in such cases the straining is violent, for the cause still remains, which continues to give the stimulus of something to be expelled, and the bladder continues to contract till tired, as in the case of simple irritability, and then there is a respite for a time; but this respite is of short duration, for the urine is soon accumulated." As the best means of procuring temporary relief, when the irritability of the bladder is not dependent on stone, or other local affection, Mr. Hunter recommends opium, and to be most effectual, given in the form of clyster, or "a blister applied to the perineum, the lower part of the small of the back, or upper part of the sacrum if more convenient." To these might have been added the application of leeches to the perineum, or some sedative and anodyne ointment, as the extract of belladonna, or hyoscyamus, blended with some camphor pomade, and applied to the same part; but those who have been frequently called on to treat these painful affections, cannot but have marked the too common failure of each, and all these appliances, to accomplish any more than, as Mr. Hunter justly observes, "temporary relief."

In turning to the close of the book, we quote a curious passage or two from Mr. Hunter, without comment, under the head "of diseases supposed to be venereal, produced by transplanted teeth."

Since the operation of transplanting teeth has been practised in London, some cases have occurred in which the venereal infection has been supposed to be communicated in this way, and they have been treated accordingly; nor has the method of cure tended to weaken the suspicion; yet when all the circumstances attending them, both in the mode of catching the disease, and in the cure, when they have been treated as venereal are considered, there is something in them all which is not exactly similar to the usual appearance of the venereal disease when caught in the common way; especially, too, when it is considered that some of the cases were not treated as venereal, and yet were cured; and, therefore, the cure of the others, which appeared to be from mercury, are not clear proofs of their having been venereal." Mr. Hunter then cites several cases which came under his care, the time, in which to accomplish a cure, varied from a few weeks to about three years.

He had great difficulty in arriving at a definite opinion on this subject:

indeed, his last remarks rather withhold, than give such. "If we take some of the above cases," (alluding to those which he has given) "and consider them as they at first appeared, we shall almost pronounce them to have been venereal. If we take the others, we shall pronounce them absolutely not to be venereal. And if we consider every circumstance relating to those, probably, venereal, we shall, as far as reasoning goes, conclude that they were not venereal.

I cannot conclude without intimating that undescribed diseases, resembling the venereal, are very numerous; and that what I have said is rather to be considered as hints for others to prosecute this inquiry farther, than as a complete account of the subject."

The work before us is embellished with several plates, contains much valuable reading, and the names with which we have prefixed our comments, will be the certain indices of its sale.

It is to be found at White's, Canal street.

G. T. B.

New Orleans, November 28, 1853.

Part Fourth.

MISCELLANEOUS MEDICAL INTELLIGENCE.

I.—*Yellow or Malignant Bilious Fever, in the vicinity of South-street Wharf, Philadelphia*, 1853. *Read before the College of Physicians, Aug. 3 and 7*, 1853. *With an Appendix.*

BY WILSON JEWELL, M.D.

This is a most interesting pamphlet of 40 pages, extracted from the *Quarterly Summary of the Transactions of the College of Physicians.* If our limits would permit, we would insert the whole of it, as the subject is one of thrilling interest; but as it is, we can only make room for the most material parts of the narrative, omitting the fully reported cases and all comment upon the treatment. The respected author is evidently in a quandary as to whether the disease originated in Philadelphia or was introduced from Cuba, but states the *facts and attendant circumstances* so fully and candidly as to give the reader a fair opportunity to form his own opinion. This is exactly as it should be; our opinions may pass for what they are worth, but our statement of facts should be as full and authentic as possible. It takes patient and persevering labor to get up facts, and that labor is not always performed by those who undertake to give accounts of epidemics. But let us see what Dr. Jewell says about the yellow fever of Philadelphia.

"During the past month (July) our usually healthy city was thrown into a state of great excitement, from a suspicion that yellow fever, with its fearful concomitants, threatened once more, after an absence of the third of a century, to find a 'local habitation and a name' in our midst.

A careful investigation into the circumstances giving rise to this alarm, has resulted in the development of the following facts, having a direct bearing upon the origin and history of this much dreaded visitation:

On the 25th of June last, the bark Mandarin, Captain R. N. Campbell, sailed from Cienfuegos, Cuba, for this port, all in good health, with a cargo of sugar, molasses and cigars. Her crew consisted of twelve men. On arriving at the

Lazaretto, July 12, after a passage of seventeen days, she was visited by the officers at the station, and, on oath, the captain reported 'cases of small-pox and fever' at Cienfuegos when he left. That he had lost two of his crew on the passage with fever. The statement of the Lazaretto physician is, that 'the crew, numbering ten souls, were examined and proved to be of good health; notwithstanding this, it was considered prudent that the bark should be detained until thoroughly ventilated, cleansed, and fumigated; the bedding and clothing of the deceased sailors were destroyed, the vessel whitewashed and fumigated in every part with chloride of lime, the bedding of the crew aired, and their clothing washed; she was detained an entire day; and, before being allowed to proceed to the city, all on board were separately and minutely examined; all hands were on duty, and apparently free from disease. The captain spent a portion of the day on shore, and before being admitted up, declared on oath that 'all on board were in good health,' and that no sickness, except that resulting in the death of the two seamen, had occurred during the voyage."

On the evening of the 13th, the Mandarin reached the city, and came to at South-street wharf. On the 16th she was hauled up to the lower side of the first pier below Lombard-street, where she discharged her cargo. The crew having been previously discharged, the captain and mate remained by her, sleeping on board. On Wednesday morning, July 20, seven days after her arrival, she dropped down to the lower side of the first pier above Almond-street, (her several positions being designated in the accompanying diagram,) where she remained until the 26th, when she was removed by order of tha Board of Health to the cove below the Navy Yard, from whence, on the 28th, she was remanded to the Lazaretto, in order to undergo a more rigid and thorough purification.

There was no development of disease of a malignant type in the vicinity of where this vessel lay, as far as has been ascertained (and the strictest inquiry has been instituted by Dr. Gilbert, the Port Physician, to whose politeness we are indebted for many of the facts here recorded,) either before or during the time of discharging her cargo, and it is still to be made known that any of her sailors, or any of the laborers employed in removing her cargo, have since been sick.

There appears to have been no cause for alarm until the cargo was out of the vessel, when it was noticed that a very offensive smell proceeded from her hold. After she dropped down to the pier at Almond-street wharf, on Wednesday morning, the 20th, the stench became intolerable, rspecially whenever the pumps agitated the bilge water, contained under the limber planks or flooring of the hold.

The first case of suspicious fever which occurred in the neighborhood was on Tuesday, the 19th of July, the day before the Mandarin left her position at Lombard-street for Almond-street wharf—Joseph Sharp, a young man, eighteen years of age, who drove a furniture car, and whose stand was on the upper side of South-street wharf. This case proved fatal on the 26th, seven days after the inception of the disease. He died at the corner of Almond and Swanson streets.

The next case was that of Captain George Robinson, of the British brig Effort, which vessel lay in the Lombard-street dock, next above the pier where the Mandarin discharged. Captain R. slept on board his brig, but took his meals at the Champion House, kept by Mr. Charles Koehler, near the N. W. corner of South-street and Delaware Avenue. He sickened on Wednesday, the 20th, and died at the Champion House, on Saturday, the 23d, at 3 P.M.

On the evening of the same day, the 20th, Mr. Koehler's son, aged nineteen, a ship-carpenter, who passed daily along the wharf in going to and from his business, took sick, and died on the 27th, at his father's, the Champion House, where he boarded.

G. W. Kerkeslager, who kept the Red Bank Ferry House, on the S. W. corner of South-street and the Delaware or Wharf street, was the next victim. He was taken on the afternoon of Wednesday, the 20th, and died on the 25th. His wife, Mrs. Kerkeslager, was taken on the same evening, the 20th, and died on the 26th.

Charles Burrows, the second mate of the bark Mazeppa, lying on the north side of the pier, first below Lombard-street, where the Mandarin discharged her cargo, sickened on Wednesday evening, the 20th,* was removed to the Pennsylvania Hospital on the 22d, and died the same night.

Frederick H. Kellog, mate of the Mandarin, was taken sick on Thursday, the 21st. On the 23d, was removed to the Pennsylvania Hospital, and died the following day.

The next one attacked was Fanny Martin, a maid-servant at Mr. Koehler's, Champion House. She sickened on Thursday, the 21st, and died on the evening of Tuesday, the 26th. On the same day, another son of Mr. Koehler, aged seventeen, and a daughter aged nine, both took sick of the fever, but recovered in a few days.

Honora Stanton, residing at No. 16 Little Water-street, above South, was taken sick on Friday, the 22d, and died on the 27th.

Silas Green, a laboring man, at No. 21 Little Water street, took sick on Saturday, the 23d. This man came from the country on Wednesday, where he had been for several weeks. On Thursday and Friday evenings he had visited the avenue, and sat for an hour or more on the wharf at Almond-street, where the Mandarin lay. He was not sensible of any bad smell. Recovered.

Capt. R. N. Campbell, of the Mandarin, took sick on Friday, the 23d, and was removed to Mr. Clement's Hotel, Delaware Avenue, between South and Lombard streets. Recovered.

On the 25th, John Shellcott, steward, and John White, mate of the brig Effort, already alluded to, were taken sick on board that vessel, lying at Lombard-street dock. On the 27th, they were removed to the Pennsylvania Hospital, where John Shellcott died on the 30th; but John White recovered.

Captain David Murray, of the brig Reform, lying on the north side of the pier above Almond-street wharf, where the Mandarin lay last (see diagram,) was taken sick on the 26th, and on the 29th entered the Pennsylvania Hospital. On the 1st inst. he was doing well. Recovered.

James Markley, another of the crew of the Effort, was taken sick on the 27th, removed to the hospital on the 29th, and August 1st was reported doing well. Recovered.

A man about fifty years of age died on the night of the 27th of July, at No. 8 Little Water street, said to have been with yellow fever; but, upon strict inquiry, there is not a doubt but that his death was from *typhoid* fever, caused by intemperance and exposure.

Between the 19th and the 27th of the month (July,) there were other cases of sickness in the the neighborhood of South-street and the Avenue, but those above enumerated are all the deaths that are known to have taken place, and all the cases of disease, as far as could be ascertained that bore any resemblance to fever of a malignant grade.

The following are the pathognomonic symptoms exhibited in several of the case of yellow fever and furnished to Dr. Gilbert and the writer, by the physicians in attendance.

Joseph Sharp, aged 18, sickened 19th, died 26th July. Fever without re-

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* The account given by Burrows, at the hospital, was that he sickened on the 17th, but the above date was procured by Dr. Gilbert, at his boarding-house, and is no doubt correct.

mission until the end of third day; then prostration, without reaction; sallow skin and eyes.

Captain Geo. Robinson, of brig Effort, taken with fever 20th; died 23d. Fever without intermission for thirty-six hours, then prostration without reaction; black vomit, hemorrhage from mucous surfaces, and bronzed color of skin.

G. W. Kerkeslager, sickened 20th, died 25. Excruciating pain in loins; fever without remission for three days, when pulse became natural and sank; black vomit, and sallowness of conjunctiva.

Wife of last case sickened two hours after, and died twenty hours after her husband. Symptoms the same, except black vomit.

J. D. Koehler, aged 19, sickened 20th, died 27th. Pain in loins; no remission of fever until end of third day; yellow skin, black vomit, hemorrhage from mucous membranes.

Fanny Martin at Koehler's sickened 22d, died 26th. Symptoms same as last case, except the yellow skin.

J. M. G. Koehler, aged 17, taken 21st; and Pauline, daughter, aged 9, taken same evening. Febrile excitement of the same character as the others; abatement on third day, followed by reaction and recovery.

Case of Honora Stanton. Could not discover any symptoms of yellow fever in the account of this case as given by the husband, family and neighbors, nor did her body after death present any symptoms favorable to such a view. The certificate of her physician' Dr. Gegan, said *bilious fever*, although the doctor insisted upon it that she had the black vomit.

Silas Green, taken on the 23d. Fever without intermission for several days, with vomiting of bilious matter, and considerable gastric distress; intense pain in the head, back and limbs; fever abated on the 26th, and recovery was rapid.

Captain R. N. Campbell was attacked on the 23d. Symptoms were accompanied by headache; fever without remission for several days; nausea, without vomiting; was convaslescent on the 27th.

We are not advised that the treatment in the above cases differed particularly from the usual method pursued in our ordinary bilious fevers, unless by the early introduction of decided doses of quinia in several of them.

Of the case treated in the hospital, Dr. Gerhard has furnished the following very brief but emphatic report:

"*Dear Doctor:* I inclose you a list of cases of *yellow fever* admitted into the hospital; two of them were from vessels from Cuba, the others were from the English brig Effort, lying near the Mandarin, and one from the Reform, lying in the same neighborhood.

Two of the fatal cases came into the hospital dying; one had black vomit, the other had the same black secretion contained in his stomach. The steward of the Effort had abundant black vomit in the last twelve hours of his life. These cases have all been genuine *yellow fever*, bearing no resemblance to any other variety of febrile diseases.

The examination after death, in the three cases, gave us nothing but the usual lesions; that is, the peculiar yellowish hue of the liver, coupled, generally, with a little gastritis and a slight enlargement of the spleen.

All of them had a yellowish hue, and one was exceedingly jaundiced. The treatment we adopted was quinia in large doses, together with cupping in some cases.

Striking out the two cases that came in dying, we have lost one out of four."

Before the arrival of the Mandarin, and up to the 19th of July, the day on which the first case of fever occurred, the vicinity of South-street and the wharf, as well as the entire city, enjoyed its usual degree of health. Certain it is, that no epidemic was prevalent. For the week ending July 9, the deaths

in the city and liberties were 229, and only one death from fever of a bilious type. For the week ending July 16, there were 206 deaths, including one from intermittent fever. For the week ending July 16, there were 206 deaths, including one from intermittent fever. For the week ending July 23, there were 218 deaths, of which four were from fever. Three of these were in children; one was recorded fever, one bilious, one congestive, and one remittent.

As yet, no one with whom we have conversed has ventured to intimate a doubt as to the agency of some uncommon and virulent poison, diffused through the atmosphere, as the pestilential cause of the malignant or yellow fever, a few cases of which have made their appearance recently in the vicinity of South-street wharf.

The essential character and origin of this poison may afford an opportunity for the speculative inquirer to indulge anew, either in an effort to demolish some favorite, though long established, yet not the less false theory of the origin of malaria; or to build upon the ruins of theories, once accredited as ingenious and popular, some more modern system of causation, which, as we advance in medical science, may be destined to meet the fate of those which have preceded it, however elaborately and industriously they may have been exemplified and sustained.

But, while we leave the discussion of this subject to others, it will not, we conceive, be questioned, when all the facts are clearly and minutely examined into, that this poison, whatever may be the nature of its character, must in part be ascribed to a morbid effluvia generated under the limber planks, in the hold of the bark Mandarin, from the putrescent state of her bilge-water.

Upon the first glance at the Manderin, and the history of her voyage previous to her arrival at Philadelphia, the advocates for a contagious germ for yellow fever, or, in other words, a principle emanating from the sick, and capable of being conveyed from one person to another, as the focus for the fever which has threatened our city, may imagine they have discovered another instance in support of their favorite theory. This, however, we are persuaded can hardly be the case, although we are desirous that a careful review of the facts connected with this ill-fated vessel should speak for itself.

The Mandarin left Cienfuegos on the 25th of June, with a healthy crew. No *epidemic* was prevailing there when she sailed, although the captain, on oath, admits that a "few cases of small-pox and fever" did exist. He states that his crew lived on board the bark while in port, anchored off the town, were seldom on shore, and as far as his knowledge extended, none of them had visited among the sick.

The captain admits that the hold of his vessel had often been in a foul condition, as all vessels were that carried cargoes of sugar and molasses. Eight days out from Cienfuegos, July 3, one of the seaman sickened with fever, and died on the 7th. On the ninth day at sea, July 4, another took sick with fever, and died on the 9th, being the fifth day of his illness. This last man was thrown overboard after the vessel was within the Capes of the Delaware.

On the 12th of July, seventeen days from the time of leaving Cienfuegos, she hoisted a whiff on approaching the Lazaretto, for a visit from the doctor. She remained at the station one day, to undergo a certain amount of cleansing, as a precautionary measure only, there being at the time no sickness on board.

On the 13th, she was permitted up, and at Lombard street wharf proceeded to unload. The seaman were discharged, and, up to this date, we are yet to learn of any one of the eight having had any sickness; nor can we ascertain that any of those who worked on board, during the time of her discharging cargo, have either died or sickened.

Not a case of fever, supposed to have had its origin from the malaria arising out of the foul condition of the bilge-water of the Mandarin, occurred

until the 19th of July, six days after her arrival, and not subsequently to the cargo being discharged.

All the cases of fever known to have taken place up to this date, August 3, seventeen in number, and in the vicinity of the Mandarin, occurred between the 19th and 27th of July. Of these, eleven, nearly two-thirds died.

The poison appeared to be most active between the 19th and 22d. During this interval, twelve cases were reported, and of these ten died.

Of the seventeen cases recorded, we have no direct evidence of the existence of black vomit but in eight, and all of these perished.

On the 20th of July, the Mandarin was warped down to Almond-street wharf, below South-street, about three hundred yards from her berth at Lombard-street.

All the cases that have occurred up to this date, either resided in, or did business in the vicinity of South-street wharf, nor is it known that any case originated below, or south of where the vessel had been last moored, nor has any case come within our knowledge north of Lombard-street.

The prevailing winds during the week ending the 27th, varied from north-north-east to south-east and south, while the average gauge of the thermometer was 79½°.

From all the facts above recited, we may be warranted in drawing the following conclusions:

1. That no disease of a malignant type was prevailing in our city previous to the arrival of the Mandarin.
2. That none of the seamen discharged from the Mandarin have sickened.
3. That none of the laborers employed in unloading the Mandarin have taken the disease.
4. That the fever did not develop itself until after the cargo was discharged; when, it is believed, the noxious emanations which had been latent in the hold, under the limbers of the vessel, had an opportunity (acted upon by certain exciting causes, as heat and moisture,) to disseminate itself, and, coming into contact with other elements of decomposition existing on shore and in the docks, soon poisoned the atmosphere of the immediate neighborhood where the bark lay moored.
5. That in no instance can the disease be traced to any individual, except among those who either visited or resided in the immediate vicinity of South and Lombard-street wharves.
6. In no case has the disease been communicated to any persons visiting, or engaged in attendance upon the sick.
7. Up to this period, not a single instance can be met with, having its origin to the south of where the Mandarin lay last.

[*Read September* 7, 1853.]

Since our communication of August 3, read before the College, on the subject of yellow fever in this city, we have come into possession of some additional particulars, and a number of new cases that are well authenticated, and beg leave to submit them to your consideration.

On the 26th of July, as already stated in our first communication, the bark Mandarin, by order of the Board of Health, was taken in charge by the health officer, Wm. McGlensey, Esq., who put on board of her a crew of five men, including the watchman. These men remained on board from the 26th up to the 29th of July, when she was safely anchored inside the island of Little Tinicum, opposite the Lazaretto, and placed in charge of the quarantine master. Neither the health officer nor any of his men, nor did the watchman who remained on board until after her purification and returned to the city, experience an hour's sickness.

While at the Lazaretto, where she remained from the 29th of July to the 2d of August, several of her limber planks were removed, but no mud or

other collection of filth was found concealed there. She was then scuttled and filled with water, which remained in her twenty-four hours. The holes were then plugged up, and the water pumped out. After this operation, she was thoroughly whitewashed, and in every part of her chloride of lime was freely distributed. During this process, the quarantine master, with several of the barge-men at the station, were on board and at work in the hold, cabin, and other parts of the vessel for several hours during each day—the weather excessively hot, without any inconvenience to their health.

On her return to the city, the 3d of August, she was moored at Noble street wharf, Northern Liberties, where she remained until near the close of the month; then was removed to the screw dock, Kensington, for repairs; and, on the 6th of the present month, was taking in cargo between Arch and Race streets, preparatory to a voyage for New Orleans.

No sickness whatever has followed in her track, since she left Almond street wharf, on the 26th of last July.

The population within the district where the disease first appeared, and where it seems to have been almost altogether confined, is by no means crowded. Many of the houses, however, were filthy in the extreme, with dirty yards, and full and foul privies.

The docks along the Delaware line, between Lombard and Almond streets, as usual, contain large accumulations of offensive mud and other filth.

Upon the authority of a highly respectable shipwright, who, in his official capacity, very carefully examined the Mandarin, we learn that her pumps are so constructed as to render it impossible to remove all the water in her hold. Being a tight vessel, the bilge-water remaining in her will smell in a very few days after pumping her clean. His language is, 'If you draw those pumps every five minutes in the day, there must remain in her twelve inches of water. My opinion is, that the pumps do not go down low enough; they do not go down within twelve inches of the keel, whereas they ought to be at least eight inches lower.'

We present this fact in evidence of the impure state of the hold of the Mandarin, six days after she was pumped clean at the Lazaretto.

In addition, however, to the prevalence of the morbific atmosphere which we have clearly shown developed itself on board the Mandarin—but not until her cargo was discharged—and which so sensibly affected individuals on approaching her when she lay at Almond street wharf, we must not for a moment conceal the existing causes in the immediate vicinity of South-street wharf, sufficient to justify the supposition of their agency, in the development of disease of a malignant type, when subjected to a high thermometrical influence which prevailed throughout the months of June and July. Not the least mischievous of these causes in the production of an unhealthy atmosphere, was the outlet of the sewer into the dock at South-street ferry, belching forth continually putrid masses of animal and vegetable filth, accumulating around its mouth, and exposed at low water to the rays of the sun, exhaling streams of unwholesome and poisonous gases into the surrounding air. Besides this agent, there was a most foul wharf at the upper side of South-street; a filthy avenue, between Lombard and South streets, without any properly constructed surface drainage; numerous damp and confined cellars, subject to an overflow by the ebbing and flowing of the tide-water of the Delaware; and various minor causes that might properly be added to the above category, fruitful in the production of atmospherical changes injurious to health.

In summing up, however, there is one prominent feature in the chain of our narrative that must not be passed by without notice, viz:

No yellow fever existed in our city until six days after the arrival of the Mandarin; that it broke out immediately abreast of the wharf where she first hauled to, and, although there were existing causes in the vicinity—on shore—for the production of disease, there were 'plague spots' in other parts of our city, remote from South street wharf, where, had the question been asked, we

should have unhesitatingly located the first appearance of fever of a malignant type, independent of the suspected existence of a foreign focus of infection, competent to exercise its morbific influence on an atmosphere already tainted.

In presenting the above, it is proper to say that we are influenced solely by a desire to arrive at the true cause for the origin of the yellow fever in our city. That we are no blind adherent to any favorite theory for the spontaneous or domestic origin of yellow fever, nor yet an uncompromising opponent of those who advocate the doctrine of a contagious principle, capable of being carried about from place to place, and, under a train of favorable circumstances, productive of disease. An honest inquirer after truth, we would disguise no fact, that might tend in any way to elucidate a question so intricate, that for years it has been controverted by the ablest pens in our profession, and yet remains a mystery.

In our first account of this disease, we narrated briefly the history of seventeen cases, bringing them down to July 27, and gave the pathognomonic symptoms of several of them as far as they could be obtained. Since then, we have collected twenty-seven additional cases, that may be relied upon, and presenting in all respects, the evidences of genuine yellow fever.

The accompanying table embraces the entire list, in their numerical order, since July 19, including the name, age, sex, place of birth, where the disease was contracted, the date of attack, where attended, by whom, the duration of the disease, those that were accompanied with black vomit, those that died, and those that recovered.

From this table we learn that 9 of the cases were under 20 years of age; 18 between 20 and 30; 10 between 30 and 40; and 7 over 40.

Twenty five were males and nineteen females. Of the whole number, twenty were born in Ireland, four in Germany, five in England, and fifteen in the United States.

Thirty-seven of these cases may be traced directly to the infected district. In four, the origin is doubtful, and in three, no clue could be had as to where the disease was contracted.

Of the forty-four cases, thirty-four died. The mean duration of the disease in those who died was four days; not including, however, Dr. Jackson's case, No. 23, and that of James Genton, No. 30. The former was protracted until the fourteenth, and the latter to the twelfth day.

In twenty-six of the forty-four cases recorded, there occurred the peculiar dark-colored 'coffee-grounds' ejection from the stomach, known as black vomit.* This substance, when placed within the field of the microscope, exhibited the true blood-corpuscles, denoting its sanguineous character, with the exception of that in Ellen Parr's case, No. 35, examined by Professor Gilbert, whose testimony disproves its identity with blood, and consequently throws a doubt on the genuineness of the case. This patient recovered, and was one of the two instances of recovery where the black vomit was said to have occurred.

In eleven of the thirty-four deaths a *post-mortem* was made; in all of which the yellow or ochre-colored liver was detected in whole or in part. In all of them the 'coffee-grounds' fluid, or melænic blood, was found, either in the stomach or intestines, with other evidences of a pernicious form of fever.

The mortality, according to the table, has been fearful; equal to 80 per cent., or 5 to one on recoveries.

APPENDIX.

In addition to the 44 cases of yellow fever enumerated in the preceding articles, as read before the College of Physicians, we have, after considerable labor, gathered the statistics of 126 more; bringing the report up to October 7, 1853, the date of the occurrence of the last case. Thus making in all 170

* In the seventh case, there was no black vomit before death, but it was found in the stomach at the *post-mortem*.

cases of yellow fever in Philadelphia from July 19 to October 17, inclusive, a period of 80 days.

Of these 128 died ; equivalent to 1 in every 1.42 hundreths, or 75 per cent.

One hundred and forty-seven of all the cases may be traced to a direct intercourse with the infected locality. Twenty-two are of doubtful or unknown origin ; and one was contracted at the Lazaretto, while at work on board the ship Caledonia Brander, from New Orleans.*

One hundred and eighteen were treated in private practice,twenty-four at the Pennsylvania Hospital, eighteen at the Blockley Hospital, seven at the Bush Hill or City Hospital, and three at St. Joseph's.

Ninety, equal to 53 per cent of the cases, were accompanied with black vomit ; all of which died, with the exception of four. The genuineness of the discharge in one of these four cases has been doubted, as stated before. As the discharge of black vomit from the stomach of a yellow fever patient has generally been considered a fatal symptom, we should always be inclined to question the character of the matter vomited where the patients recover, who are said to have had black vomit, no matter how high the authority from which they emanate, unless it had first undergone a careful examination under the microscope, and then found to contain blood-corpuscles.

The greatest number of cases that occurred in any one day was ten ; this happened on the 11th and also on the 19th of September.

The duration of the cases that proved fatal was from two to twenty-four days; the mean period was a fraction less than six days.

Ninety-three of all the cases that occurred were among males, and seventy-seven in females. That period of life which appeared most susceptible of the fever was between 20 and 30 ; one-third of all the cases happened in this decade.

Out of the whole number of cases recorded, 68 were born in the United States, 62 in Ireland, 19 in Germany, 18 in England, 1 in Scotland, 1 in France and 1 in Spain.

In no instance have we learned that the fever has been communicated to others by those laboring under the disease. At the Pennsylvania Hospital the yellow fever cases were placed in the wards promiscuously, with other patients, without any attempt to prevent intercourse between them ; and not an individual, either among the patients, nurses or visitors, contracted the disease. The same immunity was observed at the Blockley, St. Joseph's and City Hospitals. In private practice, although many of the cases were treated away from the infected locality, where they contracted the fever, we have not been able to hear, after the strictest inquiry, that the disease spread itself in any case beyond the individuals infected, although there was unrestrained intercourse between them and their friends. An evidence of the non-contagiousness of yellow fever, or in other words, that the poison which produces yellow fever is incapable of being generated in the body of the sick, so as to produce a similar disease in those attending upon them.

We have been unable to calculate the proportion of cases, or even of deaths, to the population, from yellow fever, in the infected district. The information required to make an accurate estimate not being reliable, all we can offer on this subject is, that the population involved was by no means a crowded one. The number of resident inhabitants in that locality, where the first 19 cases happened, bounded by the south side of South street, the west side of Little Water street, Lombard street, and the Delaware front, did not exceed 100. The remaining portion of the infected district to which the fever seemed to be confined, and to which we have ascribed limits, extending from Union street on the north to Queen street on the south, Second street on the west, and the Delaware front on the east, did not by any means contain an excess of population.

* This vessel had yellow fever on board when she arrived.

The inference therefore is, not only, as we have already shown, that the deaths to cases have been large, but that the cases to the population, as well as the deaths to the population residing within the above limits, have been proportionately large.

While the fever in general has been confined to the limits above named, it is evident that a few cases of genuine yellow fever have appeared beyond these boundaries, between which and the infected locality no direct communication could be traced. Showing either that isolated cases of malignant fever may occur in our city when the mean of the thermometer is above 76°, or that an epidemic influence was extending its treacherous march to remote and varied sections of our metropolis.

We must omit a beautiful map of the infected district and a comprehensive table of the cases. F.

The New Orleans Medical and Surgical Journal.

VOL. X.] NEW-ORLEANS, JANUARY 1, 1854. [No. 4.

TO THE SUBSCRIBERS OF THE JOURNAL.

The undersigned most willingly completes the unfinished labors of his lamented friend, Dr. Hester, in bringing out this number of the Journal. He regrets the delay in its appearance that has occurred; but begs leave to assure the subscribers that on account of numerous and pressing engagements, it could not be avoided. In regard to the continuance of the Journal, he is authorised to say there will be no interruption to its progress, and its business affairs will be conducted in the same manner as heretofore. It is not yet determined who is to be its future permanent Editor,but, until finally disposed of, the subscribers may rest assured that it will appear regularly at its appointed times under the direction of the undersigned. Those wishing to remit money may do so as usual, and new subscribers may forward their names and payments in advance, which will be properly attended to.

The undersigned would most respectfully remind the many subscribers who are still in arrears with their payments, that they cannot in a better manner testify their regard for the late editor, who struggled so perseveringly to maintain this Journal, than by promptly remitting the amounts they owe, for the benefit of *her* who is left desolate by his untimely death. Communications may be addressed as usual to the Editor of the New Orleans Medical and Surgical Journal.

E. D. FENNER.

DEATH OF DR. HESTER.

Died, of Epidemic Cholera, on the morning of the 1st December, 1853, Dr. ABNER HESTER, aged about 40 years, late Editor and sole Proprietor of this Journal.

In the course of a somewhat eventful and checkered life, the writer has but seldom performed so melancholy a task as that which now devolves upon him. When the ties of friendship which have been knit for years by close and confidential intimacy are severed *during life*, it never fails to cause a pang in the sensitive bosom; yet, if there be no violation of *honor* in the breach, there ever lingers a hope that it may be healed at some time; but when those ties are severed by *death* and the grave closes upon the dearest objects of affection, how sad and gloomy is the void that is left and how melancholy the workings of the mind ever clinging to the memories of departed worth !

The eloquent divine who attended the funeral of Dr. Hester made something

like the following touching remarks: "My friends, I have come here this evening, not alone in my official capacity, to perform the last sad service to a fellow mortal, but *to bury* a *friend*—one whom I have known since his boyhood—who was my school fellow, and gained my affectionate regard at a period of life when the affections are warmest and most pure." Our own acquaintance with the deceased does not extend so far back, but we may say that for the last twelve years he has been our most intimate friend and companion. We have struggled together in adversity and enjoyed together such pleasures as we were able to find along the rugged walks of life—we have worked together in the great cause of medical science and literature, which enchained alike our ambition and our energies, and we both had the gratification to see our efforts crowned with some degree of success. Now he comes to an untimely end—snatched away at the meridian of life and the full development of his fine mental powers. By the force of his talents and indomitable energy, unaided by wealthy friends, whose influence his proud spirit scorned to court, he made himself one of the first physicians of New Orleans, and if he had lived ten years longer, would have amassed a splendid fortune.

As none of Dr. Hester's relatives reside in this city, we are indebted to some of his old acquaintance for the following memoranda of his early life. We learn from one of his most intimate friends, who has known him from his boyhood that he was born in Mecklenburg county, Virginia, but that his parents emigrated to the West when he was but a child, and settled themselves in Montgomery county, Tennessee, near the Kentucky line, where he was brought up on a farm. His parents were of that unostentatious but independent class of farmers constituting the best portion of the population in the Western States, whose chief ambition is to educate and elevate their children above the sphere in which they themselves have moved. Indeed this is the class from which the most talented and energetic men of our country have sprung. Born to independence and equality in point of moral position with all around them, yet sufficiently cramped by the *res angustæ domi* to protect them from the enervating influences of luxury and indolence, they soon feel the soul-stirring impulses of pride and ambition, and even with limited education, are sent forth into the world armed with an energy and firmness of purpose calculated to overcome whatever obstacles they may meet in their future careers.

At an early age Dr. Hester displayed talents and ambition which induced his parents to bestow what assistance they intended to give him, in the way of a *good education.* At about sixteen years of age he was sent to Cumberland College, situated in Princeton , Kentucky; an institution which at that time had an able faculty and was in a flourishing condition. It was under the special direction of the Cumberland Presbyterians, and was conducted on the manual labor principle; that is, the time of the student was occupied partly in mental application and partly in laboring on the farm. The Rev. Wm. A. Scott, now one of the most distinguished Ministers of the Gospel in this city was at college with Dr. Hester, and knew him intimately. Dr. Scott says he was a good student, very generally esteemed, and prided himself on being well prepared for his recitations. After graduating at this college he went into the

office of Dr. Walter H. Drane of Clarksville, Tennessee, for the purpose of studying medicine. Dr. Drane at that time commanded a large practice and was considered one of the ablest physicians in the State. After going through the usual preliminary course of reading, Dr. Hester went on to Philadelphia, where he attended two courses of lectures in the University of Pennsylvania, and graduated in the spring of 1837. On returning home, he remained but a short time before concluding to come further South, and selected the flourishing village of Holly Springs, in North Mississippi, as a place of residence. Here he practised his profession with much credit to himself until the fall of 1839, when, finding the field too small for his ambition, he resolved to leave it and plunge at once into the great emporium of the South-West, New Orleans. His fortunes were now desperate, having spent the last of his patrimony in acquiring his profession and as yet made but little by the practice. It was certainly a bold step for a young man without money or acquaintance to come and settle himself in this extravagant city. Inspired, however, with confidence in his own abilities, he boldly trusted to fortune for an opportunity to display them, though his daily bread depended on the cast. The difficulties he encountered were perhaps even greater than he had anticipated; yet by close attention to his meagerly furnished office and the observance of the most rigid economy, he managed to keep himself afloat. His genteel outward appearance and ever cheerful countenance doubtless led many to suppose he was living in affluence, whilst he was actually denied the comforts of life.

We came to New Orleans on a visit in the winter of 1840—41, and then made the acquaintance of the deceased. In the winter of 1841—42 we settled ourself in the city, and since that time have been intimately associated with him. We found him oppressed with poverty, but full of energy and hope. Being in somewhat similar circumstances myself at that time, a fellow-feeling generated a mutual attachment between us, which was only disturbed by the intervention of death.

In 1843 Dr. Hester was so much discouraged by his slow progress in practice that he thought seriously of again changing his residence and seeking his fortune in a foreign land. He was persuaded by a sea-captain whom he happened to attend professionally, to go to Merida in Central America, and had actually packed up his clothes ready for starting, and waited only for the captain to call for him. He was disappointed—the captain did not call, and thus he was forced to renew his efforts for a living in New Orleans. If he had left here at that time his subsequent career would probably have been altogether different.

In January, 1844, Dr. Hester and myself, both then in rather desperate circumstances, embarked in an enterprize which has proven to be of no trifling importance to the Medical Profession throughout the Southern States; we allude to the establishment of this Journal. In the first volume of my *Southern Medical Reports*, published in 1850, we drew up a brief sketch of the origin and progress of this Journal which, we trust, will not be deemed inappropriate to the present occasion. It is as follows:

"This work was commenced by its present editor, and the editor of these

reports in 1844, and is the oldest of the Southern journals now in operation. Happening to be thrown together in the city of New Orleans and finding our fortunes alike desperate, "a fellow-feeling" gave rise to an intimacy between us which it is hoped will endure through life. Without money, with but few acquaintances, and dependent on a precarious practice which barely afforded the most economical support, we determined to project the hazardous adventure of a Southern medical journal and trust to the liberality of the medical profession for its support. The field was ample, rich, and entirely unoccupied, but it was difficult to see how the experiment could succeed without having *one cent of capital* to start on. We actually had the Prospectus printed *on a credit*, one of our booksellers being willing to go that far at all hazards, and we paid the bill, *eleven dollars*, out of the first spare money we had. The Prospectus being out and distributed throughout the country, we were fairly committed to bring out the work, but as yet, could find no person willing to undertake the publication. All we had to give was our own labor, which was cheerfully offered, but something more substantial was required. We appealed to the booksellers, to the proprietors of the city newspapers, and finally, to the Medical College and leading physicians of the city, for a guaranty of five hundred dollars, but all to no purpose. The enterprize was *conceived in poverty*, and *poverty brought it forth!* At this stage of our gestation, we had the good luck to come across a poor *French printer*, who had *a handful of type and nothing to do*. Him we persuaded, by means of *flattering promises*, to bring out the first number; and thus the New Orleans Medical Journal saw the light! Each number made out to pay its own way, but left no surplus on hand. In this manner we struggled through the first volume and were entering upon the second with prospects somewhat improved, when an unexpected rival appeared in the field. The Professors of the Louisiana Medical College issued a prospectus announcing the early appearance of a new Medical Journal from their school. An union was effected between the two, and the late Professors Harrison and Carpenter joined us in the publication of the New Orleans Medical and Surgical Journal. In 1848 we voluntarily withdrew from the Journal, and in less than two years, Drs. Carpenter and Harrison were removed by the hand of death, leaving the present worthy editor "alone in his glory." And nobly has he devoted himself to the discharge of the heavy duties devolving upon him. It affords us much pleasure to say, that this Journal is now in a very prosperous condition, having a list of subscribers numbering about one thousand, and constantly increasing. It has given a decided impetus to the cultivation of medical science in the South, thus fulfilling the highest object of its original founders. Its circulation is confined chiefly to the Southern States, though some of its contributions, particularly thoso of Dr. Bennet Dowler, have attracted much attention both at the North and in Europe. We trust this historical notice will not be unacceptable to the reader, as it adds another to the numerous instances on record, of the triumph of energy and perseverance over obstacles apparently insurmountable. We heartily wish the editor and his work all manner of success."

From that time the Journal has continued to progress with brightening prospects until it has become one of the most repectable in the Union, and if well conducted in future, will doubtless prove a lasting monument to Dr. Hester's memory.

But let us notice some other events in his life. In the spring of 1846 war was declared between the United States and Mexico, and under a pressing emergency, General Taylor called upon Louisiana for three Regiments of Volunteers. The call was promptly responded to, and Dr. Hester was appointed Surgeon to one of these Regiments. From that time we may date the

commencement of his rise as a practitioner in this city. For seven years he had persevered in his efforts to get into practice, but had only succeeded so far as to make a bare support. His face was familiar to hundreds of his fellow-citizens, but few of them had ventured to employ him. With a proud aspect of independence he walked the streets of New Orleans, relying upon his merit alone and scorning to stoop to any means of getting practice that were at all derogatory to the dignity of the Profession. When he went into the army he was brought in contact with a regiment of his fellow-citizens who were forced to discover his merits as a physician, and they were astonished to find them of a superior order. His regiment was only in service about three months, but it was a trying time for new recruits who were suddenly removed from the comforts and luxuries of city life to the hardships of the camp. There was a good deal of sickness amongst them, but scarcely a man was lost. When he returned to the city he resumed the practice of his profession under much brighter auspices. He had now a *regiment of friends* around him who had formed a high estimate of his professional abilities and were disposed to do all they could to promote his advancement. The result was that he soon got into a lucrative practice which he ever afterwards sustained with signal ability. Soon after his return from Mexico he was elected Secretary to the Board of Health, with a salary of six hundred dollars per annum, which was of immense advantage to him by keeping his name constantly before the people in the publications of the Board. In 1847 there was a great epidemic of yellow fever, in which Dr. Hester did an extensive and lucrative practice. He then became really independent and triumphed at last over all the obstacles which had so long opposed his progress. He was not able to support a horse and carriage till 1846—indeed, he could hardly afford to ride in the omnibus or car previous to that time; consequently he had to practice almost entirely on foot.

In 1848 Dr. Hester was appointed by Governor Johnson a member of the Board of Medical Examiners for the Eastern District of Louisiana, and continued in that office until the Board was abolished in 1852.

He was appointed Visiting Physician to the Charity Hospital upon numerous occasions, and served as Visiting Surgeon during one term.

On the re-organization of the Board of Health last summer he was elected Port Physician, which office he held till his death—in fact, the office expired the very day he died.

He was one of the founders of the Medico-Chirurgical Society, of which the late Dr. Luzenberg was President, and which was suffered to expire with him. He was a member of the Physico-Medical Society when he died, and his memory has been duly honored in that Society by the passage of the complimentary resolutions that follow.

At the annual meeting of the Louisiana State Medical Society held in March last, Dr. Hester was elected one of the Vice Presidents, and held that office at the time of his death.

Such is a brief summary of the offices and honors conferred upon Dr. Hes-

ter, all of which were deservedly bestowed and faithfully discharged. As Editor of this Journal from its foundation, he deserves much credit for his persevering efforts to sustain it through all its difficulties, and they have been by no means inconsiderable. By his urbanity and conciliating disposition he ingratiated himself into the favor of his subscribers, most of whom display a deep interest in the success of the Journal.

Dr. Hester's sudden and unexpected death caused quite a panic in the city. He was seen practising his profession in different parts of the city till past six o'clock in the evening; by three o'clock the same night he was a corpse, and on the following morning his friends were invited to attend his funeral. The consternation thus produced by the death of one so generally known and esteemed may readily be conceived. A number of cases of Cholera had already occurred and caused considerable alarm in the city; but the death of Dr. Hester at the St. Charles Hotel created a shock which was felt far and wide. About two weeks previous to this attack he had one which would undoubtedly have taken him off, but for the prompt and efficient medical aid he obtained. From that time he looked badly, as if he had not fully recovered, but it seemed even that serious warning could not put him properly on his guard. He knew he had a predisposition to bowel-complaint which always endangered his life when Cholera was in the atmosphere, yet he could not be prevailed on to be prudent or use proper precaution against this insidious disease. In the last number of this Journal he predicted the re-appearance of Cholera in this city before a great while, little thinking perhaps that it was quite so near, and that he himself would be numbered amongst its victims. If he had used the precautions which he recommended to others, we cannot doubt that he would be with us still; but there really seems to be something in the nature of this disease calculated to deceive the sufferer and lead him on under a false sense of security into the very jaws of death. We see this not only amongst the gnorant, but often with the most intelligent. But it is vain now to recount the irretrievable errors of the past, unless it may serve to warn some future sufferer of the danger of trifling with this insidious foe. No fatal disease gives such fair warning of its approaches as Cholera, and there is none more manageable in its early stages, yet we see thousands upon thousands falling beneath its deadly grasp.

It remains to say a few words respecting the professional abilities of the deceased, before closing this imperfect sketch. It will be conceded by all who knew him that he was a physician of rare skill and judgment, of ample resources, bold and prompt in action, and untiring in his attention to the sick. But few physicians in this city command the confidence of their employers to a greater extent than Dr. Hester did. He was ever willing to consult with his honorable brethren when desired, but most of his patients were content to trust their lives to his skill and judgment. He was remarkable for his fine personal appearance and the urbanity of his manners. He had the happy faculty of ingratiating himself with his acquaintance and gaining their affection. But few men had warmer or more devoted friends than he, as was evinced by the large and respectable cortege that attended his funeral. We see in his life an illus-

tration of the triumph of talent and perseverence over great obstacles. He came here a stranger oppressed with poverty; he died possessed of a very handsome competence, beloved and regretted by a large circle of admiring friends.

A few short months ago he led to the altar a beautiful bride, upon whom he had concentrated the whole force of his affections; he leaves her independent, it is true; but naught to compensate his own irreparable loss. By his death the Medical Profession has lost one of its most useful members. For ourself, we must be permitted to say we have lost an intimate friend and companion whose place we may scarcely expect ever to have filled.

Vale! longum, longum vale! E. D. F.

We insert some of the notices of Dr. Hester's death that appeared in the daily papers. There were equally complimentary notices in the other city journals, which are not now at hand.

From the N. O. Evening Delta, December 1.

Death of Dr. Hester.—We regret exceedingly to announce the death this morning of Dr. Hester, editor of the Medical and Surgical Journal, and the New Orleans Port Physician. He was a man of high and deserved reputation in his profession, an experienced and skilful practitioner, and a writer of much ability and acuteness. It is somewhat strange that he was the first to announce the approach of the disease to which he has fallen a victim. His prophecy (the "sunset of life gave him mystical lore," though his sun has set early) can be found in the last number of his periodical. His premature death is a source of regret to all who knew his estimable character, and a serious loss to the medical profession of this city. We have only space to announce the melancholy fact and to wish his soul the peace of the righteous.

From the N. O. Daily Crescent, Dec. 2.

Dr. A. Hester.—The death of this gentleman at an early hour yesterday was not less mournful than unexpected to his numerous acquaintances and friends. There were few abler members of his profession than the subject of this notice, and none whose death would have been more feelingly regretted. Dr. Hester was Physician of the Port and Editor of the New Orleans Medical and Surgical Journal. He was an able and polished writer, and in his profession few of his age had attained greater skill. Doing a large practice, and enjoying the confidence of the community as a successful practitioner, his loss will be deeply felt. To the members of his family we tender our sympathies in their unspeakable bereavement.

From the Daily Picayune, Dec. 2.

The remains of the late Dr. Hester were yesterday followed to the grave by a large concourse of mourners. His sudden death, in the full maturity of his powers, when the promises of future usefulness were so bright, has caused a deep sensation among the medical profession and with the public. An ardent devotee of science, and a man of study as well as of observation, he held a high rank among his brethren, and was much esteemed by the public. It is but a few months since he married a charming lady of this city, for whose bereavement, in this overwhelming affliction, the expressions of sympathy are universal.

The annual supper of the Medical Society, which was appointed to take place on Saturday evening next, will, in consequence of Dr. Hester's death, be postponed to some later period, to be selected hereafter.

The following notice was taken of his death in the Physico-Medical Society:

Anniversary of the Physico-Medical Society of New Orleans.

This Society held its annual meeting in the Medical College on Saturday evening, December 3d, the President, Dr. Axson, in the chair. After listening to an interesting address from Dr. C. R. Nutt, the orator for the occasion, the following gentlemen were elected officers of the Society for the ensuing year:

President, Dr. R. Bein. Vice Presidents, Drs. P. B. McKelvey and S. Choppin. Corresponding Secretary, Dr. T. S. Clapp. Recording Secretary, Dr. D. McGibbon. Treasurer, Dr. G. T. Browning. Curator, Dr. Boyer. Orator, Dr. A. Mercier.

The customary anniversary festival of the Society was indefinitely postponed on account of the recent death of Dr. Hester, and the following preamble and resolutions adopted unanimously:

Whereas, it has pleased Almighty God, in his inscrutable providence, to remove from amongst us in the person of the late Dr. Abner Hester, one of the most talented, honorable and useful members of the medical profession in this city; and

Whereas, the Physico-Medical Society recognizes in the sudden and unexpected death of Dr. Hester, the loss of one of its worthiest members, and a gentleman whose labors in behalf of medical science and literature have been productive of most beneficial results, and entitle his memory to be cherished with respect aud esteem by the medical profession of this country in all time to come; therefore,

Resolved, as the sense of the Physico-Medical Society, that the untimely death of Dr. Hester is deeply regretted by every one of its members, as a loss not only to us, but to the whole community in which he lived.

Resolved, That the sympathies of this Society be communicated to his bereaved wife and distant relatives, through the medium of this preamble and resolutions, to be forwarded to them by the Corresponding Secretary.

A true copy from the records.

D. MacGibbon, M. D., Recording Sec'y.

HEALTH OF THE CITY.

In the last number of this Journal Dr. Hester announced the subsidence of the great epidemic which had so severely scourged our unfortunate city, but at the same time predicted the reappearance of Epidemic Cholera at an early day. This prediction has been fulfilled, and is rendered more impressive from he fact that the Prophet has fallen a victim to the anticipated scourge. From our last date, Yellow Fever continued steadily to decline until it has now so nearly disappeared that we only occasionally meet with an isolated case. On the 19th of December the writer discharged his last case of this disease at the

Charity Hospital—a young man who came over from Ireland about the middle of October and went to work on the levee, fourteen miles above the city. He was there seized with a violent fever, and on the 11th December, the 5th day of sickness, was brought to the Hospital. He was then turning yellow and had slight hemorrhage from the nose and gums. He soon convalesced and was discharged cured. We know of no case in the Hospital or any other place at this time.

The re-appearance of Cholera in our city, after it was thought to have vanished entirely, caused much alarm and has seriously injured the business of the season. The injury, however, would not have been so great, but for the exaggerated reports that went abroad in all directions. We will briefly review some of the prominent facts in connection with the late Cholera at this place.

On the 21st of October, in one of our wards at the Charity Hospital, we found a case of Cholera in complete *collapse*. He had arrived from St. Louis the day previous, and had suffered from diarrhœa several days. On the 22d October we found another case in the same state. This man had just come down the river from Evansville, Ind. Both said they had heard of no Cholera where they had been.

On the 29th of October the newspapers contained notices of the introduction of a considerable amount of Cholera amongst the European immigrants arriving at the Quarantine Station, New York, but nothing of the kind was observed at this port. Still it created uneasiness and we expected to have the disease introduced in a short time. Before this took place, however, Dr. Hester himself was the subject of a well-marked attack on the 12th of November. There may have been other cases about this time. On the 19th of November the following announcement of the approach of Cholera by sea appeared in the *Picayune* newspaper:

> The Cholera.—A despatch from the South West Pass, dated yesterday at 2 P. M., announces the arrival below of the British ship Blanche, from Liverpool, with four hundred passengers. She has lost nine passengers during the voyage, by cholera, and one was buried yesterday after her arrival.
>
> This need not, however, create any alarm. The Board of Health are providently supplied with the means now at Forts Jackson and St. Philip for receiving and taking care of at least six hundred persons; so that if due diligence be observed, and we have every ground to believe that no effort will be wanting, the cholera imported into the Balize will be no nearer New Orleans than it was at Liverpool. Agents have been despatched by the Board of Health to see to this matter at once.

On the 20th we found a case in complete *collapse* at the Charity Hospital. The man said he had been in the city *fourteen days* and was last from the interior of Texas. The same day Dr. Hester, Port Physician, examined a vessel just arrived from Europe, which had lost nine emigrants with Cholera on the voyage, but had no sickness at that time. This was probably the Blanche, which anchored off Slaughter-house Point on the opposite side of the river. On the 21st, there were several cases at the Charity Hospital and the Touro Infirmary. At this time and for a week or two previous the weather was warm and wet, and the streets so dirty as to attract comment in the newspapers. On

the morning of the 22d we found that twelve cases of Cholera had been admitted into the Charity Hospital within the previous 24 hours, and cases were heard of all over the city excepting the 4th District. From this time the disease increased so rapidly that the weekly report of the Board of Health of the 27th November contained 129 deaths from Cholera. On the evening of the 30th, Dr. Hester was attacked a second time and died in less than nine hours after he took his bed. This melancholy event caused quite a panic in the city. The next weekly report from the cemeteries contained 214 deaths from Cholera. On the 8th December there was a sudden change of weather—turned very cold and windy. This had a marked effect on the progress of Cholera. The next weekly report showed a diminution of deaths by Cholera of nearly one half—*one hundred and eleven.* This was on the 11th December. The report of the 18th showed only 37 deaths from Cholera—and that of the 25th only *five;* three of which occurred at the Charity Hospital.

Such is the history of this brief and limited epidemic. It must have sprung up spontaneously amongst us, for surely no one will contend from the facts presented, that the disease was imported from Europe this time, whatever may have been the fact in December, 1848. We have heard of two deaths on the river near Princeton, some 500 miles above here, but none anywhere else. How long we shall continue to be occasionally visited by this scourge remains to be seen.

It is somewhat remarkable that for six years past we have had a warm and wet spell of weather about the last of November or first of December, which has invariably been attended by an outbreak of Cholera, and a change of weather has invariably been the signal for its disappearance. The reader may determine for himself what relation these two facts bear towards each other. Although Epidemic Cholera may not arise from *local causes alone*, there is no fact better established by observation in different parts of the world than that *this disease appears first, rages worst and prevails longest* in localities otherwise unhealthy, and the filthiest portions of those cities, towns and neighborhoods which happen to lie across its pathway in its mysterious progress over the world.

The city of New Orleans is again without a Board of Health, the last having expired by limitation on the 1st of December; so, there is at present no special organization or police department to which is entrusted the important business of removing the many local causes of disease that exist here or making any sort of effort to improve the health of the city. If this state of things continues, we are doomed to suffer the dreadful consequences which invariably await *neglect of duty;* but how awful the responsibility that rests upon those who have charge of the city government! If the best sanitary measures that could be advised were at once put in force, it would probably require five or six years to develop its beneficial effects; but to this we must come at last, if we are ever to be delivered from pestilence; and therefore, the sooner we begin the good work, the better. We need not enlarge upon this topic, as our remarks will be read by but few of our fellow-citizens. What we have said we consider due to the medical history of the time.

Our city is remarkably healthy at this time.

CITY MORTALITY,

For the 10 *weeks ending Jan.* 1*st*, 1854.

		Total.	*Yel. Fever.*	*Cholera.*	*Other Dis.*	*Not Stat.*
October	30	193	26	——	148	19
November	6	79	9	——	56	14
"	13	115	5	——	95	15
"	20	110	8	——	86	16
"	27	261	3	129	102	27
December	4	372	——	214	141	17
"	11	255	——	111	127	17
"	18	153	——	36	107	10
"	25	*91	1	5	68	17
1854.						
January	1	118	——	9	102	7
		1747	52	504	1031	159

MORTALITY FOR THE YEAR 1853.

It will appear from the reports published in this Journal, which are not precisely correct, that the total mortality of the city for the year has been 15,085.

From Yellow Fever, 7,899

From Cholera, 607

This is the greatest mortality that ever occurred in New Orleans in a single year. May we never witness the like again!

* Not quite full.

REPORT OF THE HOWARD ASSOCIATION.

The transactions of this celebrated Association in the most memorable epidemic that ever visited our ill-fated city, makes a neat pamphlet of 28 pages, three fourths of which are taken up with the names of the contributors from all parts of the Union. It presents the most magnificent display of active benevolence, not only in dollars and cents, but also gratuitous labor for the benefit of the afflicted by fever perhaps ever witnessed. Contributions came in from cities, towns, villages, neighborhoods and individuals in nearly every quarter of the Union, and in sums varying from above *fifty-eight thousand dollars*, down to *one dollar*—the latter, perhaps, like the *widow's mite*, displaying as much *true charity* as the former. Our city being the first afflicted by the pestilence, many cities and towns in this region promptly forwarded most liberal contributions, though doomed too soon to require a helping hand themselves. By the time the pestilence reached them it had abated here, and the Howards flew to their relief with experienced physicians, nurses and money.

The following extract from their report gives a summary of their receipts, disbursements and labors.

"It will be seen by reference to the above statements that the total receipts of our treasury since the 14th July, on which day we regularly organized for action in view of the impending epidemic, have amounted to $228,927 46; and that the sum expended from the same date is $159,190 32, which, together with an approximated estimate of $3500 for outstanding debts, is $162,690 32; leaving a balance this day of $66,237 14; of which balance $30,000 is invested in mortgage on real estate, bearing 8 per cent interest per annum, and 30,000 specially deposited in bank, subject to draft after 15 days notice, and bearing 4 per cent interest per annum.

The total number of cases of yellow fever attended to in this city, as per returns of the members, is 11,088, to which we might add several thousands of cases in the surrounding country, where the offices of the association have been extended, and where, in many instances, in addition to the means forwarded, our members have attended in person.

It will also be borne in mind that we have given special relief in this city to the indigent, whose sufferings, either directly or indirectly, grew out of the fell pestilence that stalked among us, and thus, whilst our expenditures may seem large at the first glance, they will appear moderate when compared with the number to whom we have afforded comfort during their sickness—sustenance in their convalescence. The stranger, the poor, the widow and the orphan have all been cared for. In practice, the association ever since its formation in 1837, has adopted as its motto the noble sentiment, '*I am a man—whatever concerns mankind concerns me!*"'

It is with painful regret that we feel impelled by a sense of duty to our profession to point out *one dark blank* that mars this otherwise most interesting document—it contains not one word of thanks, not a single expression of gratitude to the generous, charitable and self-sacrificing physicians who worked day and night and followed wherever the Howards pointed out the abodes of sickness and distress—not even an expression of regret for those noble fellows who died in their service without any expectation of pecuniary reward—who labored as hard as they did, and in far greater danger, for these were mostly unacclimated. When the Howards proclaimed the presence of the Epidemic and their readiness for action, they at the same time called on the medical profession for aid in their benevolent services to the poor. This call was responded to by about *forty physicians;* of whom some four or five fell victims to the pestilence. They surely were worthy of a passing notice in the report of the Association. Now, it is well known that the medical profession does more gratuitous service for the afflicted than any vocation in the world, but the people of this goodly city seem to think it is not only our duty to attend the poor *gratis*, but actually make us pay a tax for the privilege. There is injustice in this, and it ought to be corrected.

We highly appreciate the noble benevolence of the Howards, but they should not forget their worthy coadjurors in the glorious work of doing good.

HISTORY OF THE GREAT EPIDEMIC.

The undersigned has prepared a full and minute history of the late Epidemic Yellow Fever of this city, which is now in press and will be ready for delivery about the 18th instant. Part of it will appear in neat pamphlet form and part in muslin binding. Dr. Hester, before death, had ordered a sufficient quantity of this work to be added as an *Extra* to the present number of the Journal, but, unfortunately, it cannot be gotten out in time. The writer happened to have in one of his wards the first case of black vomit that occurred at the Charity Hospital, and commenced at that time noting facts and circumstances with the view to making this history as minute and authentic as possible. The work will be about 75 pages, and will be furnished on the following terms:

Pamphlet, single copy,	50 cents.
" 100 copies,	one third less.
Bound in muslin, single copy,	$1
" " 100 copies,	one third less.

Address E. D. FENNER, M. D.,
No. 5 Carondelet street, New Orleans.

RESPECT FOR THE DEAD.

We most cheerfully insert the following communication, displaying as it does proper respect for departed worth:

At a meeting of Physicians of Port Gibson, Claiborne county, Miss., Nov. 24th, 1853,

It was resolved, that the members of this meeting wear the usual badge of mourning for thirty days as a mark of respect for the memory of our late friend and brother, Dr. D. D. Irwin, who died of yellow fever in this town, September 26th, 1853; and that this resolution be published in the American Journal of Medical Sciences, Philadelphia, and the Medical and Surgical Journal, New Orleans.

Signed, R. W. HARPER, President.
W. McN. RUSSELL, Secretary.

DEATH FROM CHLOROFORM.

A young female, whose great toe was about to be amputated by one of the visiting surgeons of the Charity Hospital, suddenly expired while under the influence of chloroform. It was some time before she could be brought fully under the effects of the anæsthetic; she finally, however, became completely insensible, and before the operation was concluded, she sank and rapidly expired in spite of the most strenuous and judicious efforts of several medical men present. The usual precautions were used in its administration, and no censure can justly be attached to the surgeon or his assistants for the untimely result of the case.

A post-mortem was made by the Professor of Physiology in the University of Louisiana, and all the organs were found to be perfectly healthy. A. H.

DR. J. MARION SIMS.

It is with much pleasure we learn that this talented and skilful surgeon has completely regained his health and settled himself permanently in the city of New York, where he has established an *Institution for diseases and injuries incidental to Parturition.* We congratulate the great Metropolis upon this acquisition to her able medical faculty, and sincerely hope she may duly appreciate his superior professional abilities and his real worth as an exemplary citizen. Dr. Sims' brilliant surgical operations in the city of Montgomery, Alabama, where he has resided for the last ten or twelve years, and his writings in the leading Medical Journal of this country have gained him a high and well-merited reputation. Driven from his beloved Southern home by the unfriendliness of the climate, we trust he may find at the North not only a larger field for the display of his talents, but also a clime that will prolong his usefulness for many years to come. He has devoted special attention to those injuries and accidents which too often befall the unfortunate *parturient female*, such as Vesico-vaginal Fistula, Rupture of Perineum, etc.—injuries which entail upon woman incessant pain, the most loathsome inconvenience, and have hitherto almost defied the skill of the profession. The great improvements he has made in the treatment of these diseases require only to be more generally known to ensure him a world-wide fame. In the central position he has now taken, we really think he may render almost as valuable services to the medical profession as to the unfortunate females who may come under his care, for the most of us do but little more than palliate the sufferings which he by his ingenious appliances very often permanently cures. We therefore think it the duty of the profession to send all such patients to Dr. Sims, who will certainly give them the best treatment that has as yet been devised. We are pleased to see he has republished his interesting paper "*on the treatment of Vesico-vaginal Fistula,*" which first appeared in the *American Journal of the Medical Sciences*, for January, 1852. His present residence is at No. 79 Madison Avenue, New York, where he may be addressed or applied to at any time.

LORD PALMERSTON ON FASTING AND PRAYER.

We take the following admirable letter with its editorial preface from one of our city newspapers, the *Orleanean.* Its religion and philosophy too are worthy the great statesman who dictated it.

Lord Palmerston, although having no identification with our citizens, and remotely situate from our latitude, would appear, notwithstanding, to have our people, our authorities and our city in his mind's eye, as clearly as though he had resided amongst us, when replying to the Presbytery of Edinburgh, who addressed him a letter, inquiring whether the British government intended to appoint a national fast on account of the prevalence of cholera. We annex it:

WHITEHALL, Oct. 19, 1853.

Sir—I am directed by Viscount Palmerston to acknowledge the receipt of your letter of the 15th instant, requesting on behalf of the Presbytery of Edinburgh to be informed whether it is proposed to appoint a day of national fast on account of the visitation of the cholera, and to state that there can be no doubt that manifestations of humble resignation to the Divine will and sincere acknowledgment of human unworthiness are never more appropriate than when it has pleased Providence to afflict mankind with some severe visitation; but it does not appear to Lord Palmerston that a national fast would be suitable to the circumstances of the present moment.

The Maker of the universe has established certain laws of nature for the planet in which we live, and the weal and woe of mankind depends upon the observance or neglect of those laws. One of those laws connects health with the absence of those gaseous exhalations which proceed from over crowded human beings, or from decomposing substances, whether animal or vegetable; and these same laws render sickness the almost inevitable consequence of exposure to those noxious influences. But it has at the same time pleased Providence to place it within the power of man to make such arrangements as will prevent or disperse such exhalations, so as to render them harmless; and it is the duty of man to attend to those laws of nature, and to exert the faculties which Providence has thus given to man for his welfare.

The recent visitation of cholera, which has for the moment been mercifully checked, is an awful warning given to the people of this realm, that they have too much neglected their duty in this respect, and that those persons with whom it rested to purify towns and cities, and to prevent and remove the causes of diseases, have not been sufficiently active in regard to such matters.

Lord Palmerston would therefore suggest, that the best course which the people of this country can pursue, to deserve that the further progress of the cholera should be stayed, will be to employ the interval that will elapse between the present time and the beginning of the next spring, in planning and executing measures by which those portions of their towns and cities which are inhabited by the poorest classes, and which, from the nature of things most need purification and improvement, may be freed from those causes and sources of contagion which, if allowed to remain, will infallibly breed pestilence, and be fruitful in death, in spite of all the prayers and fastings of an united but inactive nation. When man has done his utmost for his own safety, then is the time to invoke the blessing of heaven to give effect to his exertions.
I am, sir, your obedient servant, HENRY FITZROY.

To Rev. W. H. Gray, Moderator, Edinburgh Presbytery.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1853.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich

WEEKLY. — 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Oct, 27	74 .00	49 .00	25 .00	30 .30	29 .80	50. 0	E.	2 .¾	4	1.645
Nov. 5	79 .00	52 .00	27 .00	30 .40	30 .05	35. 0	SE.	2 .¼	0	0 000
" 12	77 .00	48 .00	29 .00	30 .30	30 .10	20. 0	SW.	2 .	2	0.035
" 19	80 .00	54 .00	26 .00	30 .27	30 .10	17. 0	SE.	1 .¾	0	0.000
" 26	79. 00	59 .00	20 .00	30 .23	30 .10	13. 0	E.	2. ¼	2	0.450
Dec 3	72 .00	52 .00	20 00	30 .20	30 .05	15. 0	NW.	3.	3	6·560
" 10	74 .00	40 .00	34 .00	30 .30	30 .00	30. 0	SW.	2 .¾	0	0 000
" 17	69 .00	50 .00	19 .00	30 .10	29 .70	40 .0	NNW.	3 .¼	2	2 620
" 24	64 .00	33. 00	31 .00	30 .27	30 .00	27 .0	E.	3 .½	5	5.930
" 31	66. 00	39 .00	27 .00	30 .15	29 .70	45 .0	NE.	2 .¾	3	0.055

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For October, November and December, 1853.

	SEX.	OCTOBER.	NOVEM.	DECEM.
ADMISSIONS -	Males	691	720	809
Do. - -	Females	179	203	190
		870	923	999
DISCHARGES -	Males	506	514	677
Do. - -	Females	159	162	144
		665	676	721
DEATHS - -	Males	165	126	130
Do. - -	Females	30	32	41
		195	158	170
BIRTHS - -		0		
Do. - -		0		
STILL-BORN -		0		

ANNUAL REPORT FOR 1853.

Total Admissions,	Males,	10,126	
	Females,	3,633	—13,759
Discharges,	Males,	7,971	
	Females,	2,762	—10,733
Deaths,	Males,	2,426	
	Females,	738	— 3,164
Remaining January 1s, 1854,		508	
Births during the year,		159	

REMARKS—It will be seen that the number of patients admitted into this Hospital is smaller than usual. This proceeds from the fact that the Hospital was greatly relieved by the Howard Association, which established four Infirmaries during the great epidemic. Four others were established by the Board of Health.

THE

NEW-ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

MARCH, 1854.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia, Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresp. Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c., &c.

EDITOR'S OFFICE : NO. 30 CAMP STREET;

Residence : Tchoupitoulas Street, between Louisa & Poyfarre Streets.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*

N. O. Charity Hospital.

NEW-ORLEANS:
PRINTED BY JOSEPH COHN, 12 ST. PETER STREET.
1854.

TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

All communications should be addressed to the Editor of the New Orleans Medical and Surgical Journal, and they will receive prompt attention.

Papers for publication have been received from Dr. W. Taylor of Talladega, Ala.; Dr. M. Troy of Cahaba, Ala.; Dr. S. S. Grier of Adams County, Miss., and Dr. J. S. Davis of Salem, Miss.

Since our last we have received the following books, pamphlets, etc., for review:

A Treatise on Diseases of the Eye. By W. Lawrence, F. R. S. A new edition by Isaac Hays, M. D., Surgeon to Well's Hospital. Philadelphia. Blanchard & Lea, 1854. (From the publishers, T. L. White.)

The Medical Formulary. By B. Ellis, M. D., etc.. 10th edition, revised by R. P. Thomas, M. D., Prof. Mat. Med., etc. Blanchard & Lea. (From the publishers.)

Dental Chemistry. By A. S. Piggott, M. D., etc. Lindsay & Blakiston, Philadelphia. (From the publishers.)

A Text Book of Anatomy, and Guide in Dissections. By Washington R. Handy, M. D., etc. Lindsay & Blakiston, Philadelphia, 1854. (From the publishers.)

On the Use and Abuse of Alcoholic Liquors in Health and Disease. By W. B. Carpenter, M. D. Philadelphia, Blanchard & Lea, 1853. (Prize Essay.) (From the publishers.)

An Inquiry into the Nature of Typhoidal Fevers, based upon a consideration of their history and pathology. By Henry F. Campbell, M. D. Philadelphia, 1853. (From the author.)

Professional Letters from Europe, written during the summer of 1852. By P.

.F Eve, M. D., Professor of Surgery in the Nashville University. (From the author.)

A Case Book, to be used at the bed-side. By G. F. Cooper, M. D., of Savannah, Geo. (This is a well-arranged Blank Book, very convenient for keeping Notes of interesting cases. Every physician should have one. *Ed.*)

We have received all our exchanges regularly, with the addition of the American Medical Monthly. New York : Edited by Edward H. Parker, and conducted by Horace Green and six other physicians. (A very neat journal.)

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MARCH, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

I.—THE EPIDEMIC YELLOW FEVER OF MOBILE IN 1853.

Communicated to the Sanitary Commission of New Orleans.

BY J. C. NOTT, M. D.

GENTLEMEN :

I herein transmit to you such facts as I have been able to collect, bearing on the Epidemic Yellow Fever which prevailed in and around the city of Mobile during the summer and autumn of 1853.

The disease this season has pursued such an unusual course, as to bring under discussion again the long neglected idea of *contagion*, which I, in common with most members of the profession, had regarded as obsolete. However it may be explained, the fact is none the less certain, that the disease has extended not only to all the little settlements within five or six miles of the city, but to Citronelle, the present terminus of the Ohio Railroad, thirty-three miles from town ; and to the various towns on the rivers tributary to our Bay as far as steamboats have gone and no farther—to Montgomery and Demopolis, for example, to say nothing of many intermediate points.

The first cases of Yellow Fever which occurred in Mobile, it is conceded on all hands, were imported from New Orleans on board the barque *Miltiades*; and for the following facts I am indebted to Doctor Walkly and Mr. Cox, one of our most respectable stevedores—Doctor Walkly's information was derived from the captain of the barque and the second mate of the steamer Daniel Pratt, which acted as lighter to her.

The Miltiades sailed from Portland, Maine, to New Orleans, where she lost several of her crew with Yellow Fever; from thence she came to Mobile Bay and anchored below Dog River Bar, some fifteen or twenty miles below town, on the 11th July; and on the 13th Peter Johnson, one of the crew, was sent to our Marine Hospital in the back part of the city, one mile from the wharves, where he died with black vomit. Dr. Lopez, surgeon of this hospital, informs me that this man entered on the 11th instead of the 13th, in *articulo mortis*, and that he had been sick at sea five days with Yellow Fever.

On the 14th, three days after the arrival of the vessel, the stevedores went on board to load her with cotton for Liverpool. One of them, John Johnson, was taken down with Yellow Fever on the 19th or 20th, and was brought to town on the steamer Daniel Pratt, and placed in the "Sailor's Home," where he died with black vomit on the 25th. On the 25th four others were brought up from the vessel sick by the same steamer. One was taken to No. 9 Government street; one to Franklin street, below Eslava and another went to the hospital.

On the 1st of August the second engineer of the Daniel Pratt was taken down with the same disease and recovered. Dr. Levert saw a stevedore, David Nichols, with Yellow Fever, from the same vessel, on the 27th July.

These, as far as I can learn, include all the cases from this vessel. There were, however, other imported cases, preceding the appearance of the disease among our citizens, as the following facts will show; and these, like the former, cannot be questioned.

On the record of our "*City* Hospital" the following entries are made of Yellow Fever cases: July 23d one; 25th two; 26th three—all of whom were laborers that had fled from the Epidemic in New Orleans, and were either sick on arrival or taken soon after. It may be worthy of remark *en passant*, that I was informed by the Sisters of Charity that the disease did not spread among the inmates of this hospital until some time after, when it had become epidemic throughout the city.

After diligent inquiry among the physicians, the first case I can trace

among our citizens who had no communication with the Miltiades, was Mr. McDowell, a patient of Dr. Levert; he slept at Hollywood, a watering place on the opposite side of the Bay, and came to town every day on the steamer Junior; he sickened on the 31st of July, and recovered.

A few days after this, rumor was busily at work, and cases were talked of in distant parts of the town, having no connection with each other. On the 18th I made a memorandum in my notebook, to the effect, that up to that date, from the best information, there had been in the town about thirty cases. I inquired among the physicians as to their dates and localities and could trace no connection amoug the cases; they seem to have been sown broad-cast over a mile square. I kept, as is my custom, the range of the thermometer, the winds and rain, from the 1st of May until frost, and could see nothing in the season to account for disease. May, June and July were temperate, showery, pleasant and remarkably exempt from all febrile diseases. Nor was there any thing in the type of diseases to foreshadow Yellow Fever. Yet I predicted, a month before its appearance, with great confidence, that we should have a terrible epidemic in Mobile, and simply from the fact that I had never known the disease early in the season to attack Vera Cruz, West India Islands and New Orleans, without completing the circuit of the Gulf. I expected unusual virulence, because this had been its character every where it had gone, and I shall be greatly deceived if the same disease does not attack cities on the Atlantic next season, and particularly Philadelphia. The germ is sleeping, but not dead.

It should be remarked that our corporate authorities had shown unusual activity in cleansing our city, and long before the appearance of the disease every thing had been done which foresight and prudence could do, to ward off the scourge.

The foregoing statement includes, as far as I know, all the essential facts connected with the late epidemic in the city. I now propose to give what information I have gathered relative to its extension from this point to others around the city and along the rivers.

"*Spring Hill*" is part of a sandy, pine hill region, west of Mobile, 150 feet above tide water, and six miles distant from the wharves of the city; it has been a summer retreat for many years, is watered by excellent springs, and heretofore has been considered exempt from Yellow Fever, or any form of malarious disease. This settlement covers about three fourths of a mile square, with the virgin pine forest still

standing, and includes about thirty families, together with St. Joseph's College, which contains about 200 resident pupils. The epidemic commenced its ravages at Spring Hill about the 5th of September, and we shall give the history of its progress.

On the 12th of August, just about the time the Yellow Fever began to assume the epidemic form in Mobile, and one month after the first imported case, I was called to see a young gentleman, Mr. Alfred Murray, with a well marked attack of the disease, at a boarding house in Mobile, on St. Louis street, near St. Joseph, and on the 14th had him removed on a bed to the house of his brother-in-law, Mr. Wheeler, on Spring Hill, about the centre of the settlement. He recovered, and twenty days after he entered the house, 5th September, two of Mr. Wheeler's children were attacked with the epidemic, and about two weeks after two other children were attacked; three had black vomit and two died.

On the 22d August Mr. Stramler moved his family from town to Spring Hill, and occupied the house of John B. Toulmin; on the 27th he carried out a negro woman sick with intermittent fever, who died on the 31st, under circumstances which I need not detail, but I have every reason to believe she did not have Yellow Fever.

Mr. Greer moved with his family to the same house on the 29th from town, carrying a daughter convalescing from Yellow Fever; another daughter sickened on the 8th; three of Mrs. Flemming's children, in the same house, on the 10th; and Mrs. John Greer on the next day; Mrs. Flemming on the 15th, and John H. Greer two or three days after. This house is about 300 yards north-west of Mr. Wheeler's.

My father-in-law, Col. Deas, lived on a lot about 100 yards north of the last named house, and his household, white and black, consisted of sixty persons. On the 7th September one of his negro women was attacked on an adjoining lot; on the 8th his daughter-in-law, Mrs. John Deas, and on the 9th Mrs. Brown, his daughter—each being in a different enclosure and 100 yards from each other. The disease then spread rapidly through the families of the three adjoining premises, attacking whites and blacks indiscriminately. Fifty-four were attacked out of the sixty, and in 14 days the whole tale was told—five whites, two mulattoes and one black were dead with black vomit, and the rest were convalescent. One half of the whites attacked died, and I had never in twenty-five year's practice witnessed such a scene, among a class of people well lodged, in clean, well ventilated apartments and surrounded

by every possible comfort, and this, too, on a high, barren sand hill, nearly six miles from the city.

Cases existed simultaneously at Wm. Stewart's, Mr. Wheeler's and Mr. Purvis's and Toulmin's houses, widely separated from each other; and in the latter part of September and through October, the disease visited the houses of Capt. Stein, McMillan, Rev. Mr. Knapp, Mrs. George, Dubose's, John Battle's and some others. The disease skipped about in an extraordinary manner; some houses escaped entirely, some had but one or two cases. I could see no connection between the houses or inmates to explain the order of attack. There was scarcely a fatal case among those attacked after the 16th September—not more than two or three.

The great majority of the subjects on Spring Hill had had no communication with the city for many weeks, and it is worthy of note that the disease had attacked most of the country between the Hill and town before it reached the Hill, though some neighborhoods, as the Nunnery, and around it as far as Hubbell's, escaped. As far as I can learn, the disease did not spread among the country population beyond Spring Hill, which is sparse.

Citronelle. This is the name of a village which has sprung up in the last twelve months, 33 miles from Mobile, at the present terminus of the Mobile and Ohio Rail Road. It is situated on a beautiful plateau of pine land, about 400 feet above tide water, and has been considered, like all these pine hills at the South, perfectly healthy.

The following is an extract from a letter of Dr. James S. Gaines, a a most promising and estimable young gentleman, who witnessed the facts. This letter was dated 4th October, 1853, and published in the Mobile Advertiser of the 6th:

"The local population of Citronelle is 250; adding the boarders at the hotels and different boarding houses, say 100, it will make our population about 350. This estimate of the population does not include over 100 hands in the immediate vicinity of Citronelle. I have seen and treated since the 16th of August 53 cases of Yellow Fever; 13 out of this number have died. There have been seven other deaths since this date; they were not seen by me, but from what I could learn, five out of this number were from Yellow Fever; making the total number of deaths since the 16th of August up to date, 20. That will just make an average of 1 death to 17 of the population. The first case that I was satisfied of its originating here occurred on the 11th of September, since which time there have been several clear cases

and within the last ten days the number has been increasing, some of them of a very malignant type. I have no idea that the disease could have originated here, had it not been for the frequent communication between this point and Mobile; and it is not singular that it should have done so, when we reflect that the baggage cars are almost air-tight when closed, running from Mobile to this point in two hours."

The Doctor, unhappily, did not live to tell the whole tale—he himself fell a victim to the disease soon after this date. Many more of the population died, and 16 out of 18 of the employées on the Rail Road, besides many laborers. There are no data for accurate statistics, but from what I can learn, something like a fourth or a fifth of the population along the road from Mobile to Citronelle died. According to Dr. Gaines' statement, there was just a month between the first case imported into Mobile and the first at Citronelle.

The Dog River Cotton Factory is situated south-west of Mobile about five miles, and has within its enclosure of some 20 or 30 acres about 300 operatives, including their families. The houses are built in a hollow square and form a complete village. Mr. Charles Wattleworth, one of the most efficient and intelligent officers of the establishment, under date of 21st November, writes the following reply to certain queries:

"Dear Sir—In answer to yours of yesterday, I send you the following account of the deaths and recoveries from Yellow Fever in our immediate neighborhood.

The first case we had was a man that had been to New Orleans; he was taken ill on the 18th August, (two days after his return) and died on the 22d—a man that waited on him died about the same time.

The next cases that occurred were about the 1st September; they were about six in number, and the parties had been in the habit of going frequently to town.

The first cases that appeared here among parties that had been in no way connected with the city, or with the sick, occurred on the 9th October; there were five cases on the evening of that day, and about the 13th there were five more. Other cases have occurred since that time and there are three sick at present (21st November) one of which is not expected to live.

The whole number that died with Yellow Fever up to date is twenty-three, and forty-six have recovered.

Yours, &c., Chas. Wattleworth."

What is called St. Stephen's road goes off from Mobile in a northwest direction, and is so densely populated for five miles as to present much the appearance of a continuous village. My friend, Dr. E. P Gaines, lives about four miles from town on this road, had ample opportunities for investigating the epidemic, and to him I am indebted for the following facts:

The following cases all occurred from two to four miles from town, on the St. Stephen's road, or in other words, between the Creek and Gen. Toulmin's residence. August 23d, two cases; 24th, one; 30th two. September 1st, one. These were all contracted in town.

The following were the first originating in the country: September 4th, one; 7th, one; 9th, one; 11th, two; 12th, two; 16th, one—Miss Wilson, the first death with black vomit, and from this date the disease became decidedly epidemic.

Dr. Gaines thinks the disease contagious, and narrated to me some instances which are difficult to explain on any other ground. The disease extended out in this direction some ten miles, into the neighborhood of William Cleveland.

Heretofore in Mobile the colored population, except in 1819, have escaped Yellow Fever; this year they have been as generally attacked as the whites, but with less fatality; there have been at least 50 deaths among them this season from Yellow Fever, and the mulattoes have suffered more than blacks.

Children, who heretofore have been little liable, this year have been generally and violently attacked. No acclimation, short of an attack of Yellow Fever, has served this year as a protection; not only many who have lived here fifteen or twenty years and passed through several epidemics untouched, but grown up natives, and even those advanced in life, have been fatally attacked. There were very few secoud attacks. I saw but one clear case.

It is remarkable that not only some neighborhoods around the town escaped, within three or four miles, but many houses in town. Mrs. McKnight, a milliner, lived in Claiborne street, between Dauphin and St. Francis, and she with eleven unacclimated girls escaped entirely. Other examples of the same kind occurred.

Elevation seemed to have no influence over it. The Battle House, a large and superb new hotel, had just been completed and occupied but a few months; it was as clean as any building could be and as well ventilated. The female Irish servants slept in the fifth story, and the males in the basement. They were nearly all attacked and about one

half died. The cleanest parts and best residences in the city suffered as much as the small buildings in filthy alleys.

On the opposite side of the Bay, while many cases occurred at isolated houses and some sixty deaths between the village and Point Clear, yet Freeman's and the Point Clear Hotels, having more than one hundred regular boarders each, escaped almost entirely, though cases were brought to them from Mobile and New Orleans.

Contagion. Under this head, according to my view, two distinct questions have been confounded, viz., the *contagiousness* and the *transportability* of a disease. A disease may not be contagious, in the proper acceptation of the term, that is, communicable from one human body to another, like small-pox; and still it does not follow that the germ or *materies morbi* may not be transported from one place to another in a vessel or baggage car, and there be propagated.

With regard to the *contagiousness* of Yellow Fever, my mind is still undecided, nor is my conviction yet complete with regard to its *transportability.* In the epidemics of Yellow Fever which I have witnessed on former occasions, 1837—'9—'42—'43—'47, the evidence seemed to be decidedly against contagion, while in 1853 the facts have been so conflicting as to leave me still in doubt, though my leaning is rather in favor of the contagiousness of this epidemic.

The reader need not be told how completely we are in the dark with regard to the laws by which epidemic diseases are propagated, to say nothing of their obscure origin. It is a common opinion that the decomposition of animal or vegetable substances may and do produce certain gaseous emanations which rise into the air, are diffused through it, and thus produce Yellow Fever; but this theory will not bear a moment's examination. If a gas, the cause of Yellow Fever must obey the laws of gases, and be very soon diffused, by changing currents of wind, all over a city, from a given point. Yellow Fever, on the contrary, is extremely erratic in its course. It prevailed this season in Mobile for more than two months as an epidemic, and attacking new houses every day in different parts of the city; houses on opposite sides of the street, or beside each other, were attacked at intervals of several weeks, and many houses escaped entirely, or had but one or two cases, in the very heart of the city. If the cause was in a gaseous form, how could it thus skip from house to house in town, and travel in the same erratic way for miles around the town? It is a curious fact that Montgomery, Demopolis and Spring Hill were attacked about the same time, viz., between the 1st and 15th of September, while Selma

and Dog River factory were not attacked till about the 8th of October. Spring Hill and Dog River factory are within five miles of Mobile, while the other points are 200. Intermediate points, like the houses in town beside each other, were attacked at irregular intervals.

The above facts would seem to disprove the idea that the cause of Yellow Fever exists as a gaseous emanation, and we must seek some more plausible hypothesis. An examination of the facts tends more to show that the cause exists in an organic form and possesses the power of propagation and progression by organic laws. The *transportability* of Yellow Fever, to say the least, rests upon much more stable support than its contagiousness, for however conflicting the minor details may be, the broad fact stands out that the disease was not only a traveling disease, but traveled to those points on the Gulf of Mexico frequented by vessels and railroads, and *no further*. When on former years Yellow Fever visited Vera Cruz, the West India Islands and New Orleans early in the summer, it has almost invariably extended along the coast of Mississippi, Alabama and Florida. So in 1853, after it had marched from Rio to New Orleans, I predicted with certainty that it would continue its march around the Gulf; and although we had had a cool, showery, pleasant summer in Mobile, and extraordinary sanitary precautions had been taken, I advised my friends to fly, and was called an alarmist.

It has been, too, the invariable habit of Yellow Fever, when it has visited Mobile, to commence first in the city, and not to attack the surrounding country for several weeks. Why, if it depends upon an atmospheric cause, should it not attack the settlements around for five miles, as soon as the town?

It is a fact worthy of note that the Yellow Fever this season has visited every point on the Lake where the New Orleans boats have touched, while Portersville, where they did not touch, has escaped—Biloxi, Pass Christian, Pascagoula, &c., have all been attacked. At Portersville, where several hundred people were assembled, aud about 150 in one enclosure, no cases occurred, though five imported cases were brought in, nursed by different persons, and two died with black vomit. These facts I have from Dr. J. W. Moore, a very intelligent gentleman who lives at Portersville, and saw every case of sickness that occurred there.

Other facts favor the transportability of the germ or *materies morbi*. It is admitted that a vessel may go from an infected to an uninfected

port, carry the *materies morbi* with her, and that persons at the latter port may go on board the infected vessel, take the disease and die with it; hundreds of examples of this kind have occurred, and the barque Miltiades, alluded to above, is a case in point. The stevedores of Mobile, as did two men from the steamer Daniel Pratt, which lay alongside of her, took Yellow Fever from her. It is by no means an unreasonable idea to suppose that the *materies morbi* may have been transmitted to the Daniel Pratt, that was carrying freight to her for some days and by her brought to the city.

It is also a fact perfectly well established, that Yellow Fever has in many instances started in an alley or other point in a city, and gradually extended itself through the whole or part of a city; this has occurred twice in my day in Mobile—1842 and 1843—each year taking several weeks to travel half across the city, and each year prevailing in different parts of the city. In '42 the disease commenced in the southern part of the city and spread over one half; and in '43 it commenced in the extreme north and covered the part of the city untouched the previous year. This fact and others lead me strongly to believe that Philadelphia will be scourged next summer, and probably other Atlantic cities.

It is notorious that Yellow Fever has repeatedly spread from a point in Philadelphia and New York. So slow has been its progress that they have fenced it in, and in some days after, discovering that the disease was progressing, they have moved the fence to keep pace with it. A very reliable old gentleman, who was a member of the Board of Health in New York in 1822 or '23, when Yellow Fever prevailed, told me that by actual calculation it travelled forty feet a day on that occasion.

If, then, the *materies morbi* of Yellow Fever can be transported in the hold of a vessel from one port to another; and if it can be propagated from a single point in a city throughout that city, why may the disease not make its point of departure an infected vessel lying at a wharf, as well as an infected alley, or other point of land? As far as reasoning goes, I confess I can see no difference, and the spread of Yellow Fever in 1793 from a vessel in Philadelphia, and numerous other examples since, would seem strongly to favor the idea that a city may, under certain unknown circumstances, receive and propagate the *materies morbi* of Yellow Fever from an infected vessel. It is true that infected vessels have often arrived in ports without communicating the disease, but the same may be said of small-pox and other strictly contagious diseases—a negative does not disprove a positive fact.

Nor can the admission of the occasional importation of Yellow Fever into New Orleans or Mobile conflict with the fact, that sporadic cases or epidemics may spring up from germs which have been long slumbering in these cities. The facts do not conflict.

A doubt was long ago started as to the indigenous origin of Yellow Fever in America. Many have contended that it is an imported African disease, and I confess that my mind is by no means free from doubt on this point. Cholera, small-pox, measles and scarlet fever are all Asiatic diseases, all imported into Europe since the Crusades, and into America since the conquest. So recent is scarlet fever in this country that Dr. Rush remarked, fifty years ago, that the disease was so rare that one physician would not be likely to see it more than once in his lifetime! It was never known as far South as the Carolinas before about 1830, and yet how common has it become. These diseases have all the habits here which they had in their original country; they lie dormant for a time and then wake up to their work of destruction; they travel from place to place in the most erratic manner, by laws impenetrable to us. Some may be transported by contagion, others not; some may be transported both by epidemic laws and by contagion. Scarle fever, for example, may break out and prevail as an epidemic without its origin being traced, or it may be transported by contagion.

Some five years ago I published an article in the New Orleans Medical and Surgical Journal, to show that the *animalcular* hypothesis explained better the erratic habits of Yellow Fever than any other, and every day's experience and reflection since have strengthened those views; but I will not here repeat them. I am fully aware of the numerous and ingenious objections which have been urged, and among others those in the recent paper of Prof. Leidy, in which he pronounces the idea "absurd."

I am not disposed to open the discussion at present, but must be permitted to say, that ingenious and philosophical as are the experiments of Prof. Leidy, they are wholly inconclusive to my mind. Prof. Agassiz, whose authority will be allowed in any scientific assembly, regards all microscopic observations heretofore made in this department as so defective, that he informs me he has not assigned the infusoria a place in his classification of the animal kingdom. Microscopic observations are yet but in their infancy, and in reaching the causes of disease it is as far behind reality as we know chemistry to be.

In reasoning from analogy, the "Insect hypothesis" of Sir Henry Holland explains best the habits of certain epidemic diseases, and it is he part of true philosophy to abandon such theories as the old malarial

one, which is in accordance with no known laws, and to explore in a direction towards which rational hypothesis points. Prof. Leidy says "none of the well known animalculæ are poisonous. At various times I have purposely swallowed large draughts of water containing myriads of *Monas*, *Vibrio*, &c., &c., without ever having perceived any subsequent effect." He might have swallowed the poison of the Viper with the same impunity. By what various means the poison of insects or animalculæ might be communicated through the air or directly to individuals, we know not. During the past summer I knew a lady of very nervous temperament to be kept for weeks in a nervous, nauseated state, from the effluvia of certain insects on trees in the yard, while no one else perceived it, or was affected by it; she did not recover until the season for the insects had passed over. Here is a perfect analogy to the *Rhus Vernix* and other vegetable substances alluded to by Prof. eidy ,as capable of poisoning the air. Similar analogies abound.

Table of interments in the Mobile Cemeteries during the Yellow Fever Epidemic from 1*st August to* 1*st November*, 1853.

		Deaths			Deaths			Deaths
August	1	6	September	1	40	October	1	8
	2	3		2	45		2	9
	3	4		3	29		3	6
	4	2		4	31		4	15
	5	5		5	36		5	9
	6	2		6	39		6	4
	7	2		7	30		7	9
	8	2		8	44		8	5
	9	3		9	33		9	6
	10	4		10	28		10	7
	11	4		11	31		11	4
	12	11		12	30		12	8
	13	6		13	32		13	6
	14	7		14	26		14	6
	15	9		15	40		15	8
	16	8		16	25		16	5
	17	10		17	31		17	11
	18	12		18	21		18	8
	19	15		19	18		19	9
	20	11		20	21		20	11
	21	8		21	18		21	8
	22	11		22	21		22	7
	23	12		23	14		23	6
	24	12		24	20		24	4
	25	20		25	13		25	5
	26	15		26	18		26	6
	27	26		27	17		27	3
	28	28		28	8		28	4
	29	39		29	9		29	5
	30	15		30	12		30	6
	31	25					31	6
		337			780			214
								780
								337
								1331

The epidemic had so exhausted itself by the 26th October that the Medical Board announced it was at an end, and discontinued their daily reports; scattering cases, however, continued to occur throughout the months of November and December, and I find on examining the records of the Sextons, that 25 deaths in November and 15 in December are placed to the Yellow Fever list. The last death was on the 16th of December, but other cases, not fatal, occurred later.

The above table includes deaths from *all causes*, and we possess no data by which we can classify with accuracy the different diseases for those months, but we can approximate the number of deaths from Yellow Fever alone sufficiently near for all practical purposes. The aggregate from *all causes* during the three months was 1331, and those informed on the subject will allow that 15 deaths a week, or 60 a month, would cover the mortality at this season of the year from all other causes than fever, and particularly during the prevalence of an epidemic. According to this estimate, the three epidemic months would give an aggregate of 180 deaths from causes exclusive of Yellow Fever. The facts may be tabulated as follows:

Deaths during August, September and October from all causes, - -	1331
" for same period from other causes than Yellow Fever, - -	180
" " " " Yellow Fever alone, - - - - - - - -	1151
" during November and December from Yellow Fever alone, - -	40
Total of Yellow Fever from 1st August to 16th December, - - - -	1191

About fifty of the deaths from Yellow Fever were among the colored population, and this class was almost as universally attacked as the whites, which shows a degree of malignity unknown in Mobile since 1819, when the disease attacked creoles, negroes and Indians.

The winter population of Mobile is at present about 25,000, of which at least one third were absent during the epidemic; some of the latter remained in the vicinity, and many went to the interior or other States. It should however be borne in mind that our city cemeteries are the repositories of most of the dead for several miles around the city, as well as for the steamboats; and that our bills of mortality may therefore exhibit a larger per centage on our population than truth would justify. But mitigate the facts as we may, 1331 deaths in 90 days is a terrible mortality, and had the population remained in the city, I see no reason to doubt that the white portion would have been more than decimated. Certain it is, that in many villages along the Gulf States, where the number of inhabitants could be closely approximated, and where none were "acclimated," this fearful epidemic committed ravages far beyond decimation.

Mobile, December 18, 1853.

II—AN INQUIRY, ANALOGICAL AND EXPERIMENTAL, INTO THE DIFFERENT ELECTRICAL CONDITIONS OF ARTERIAL AND VENOUS BLOOD.

BY JOHN GARRIE, M. D., OF APALACHICOLA, FLORIDA.

The existence of organized bodies depends upon two properties of matter—the one peculiar to it, and called the vital, the other common to all matter, and known as the physical. The concurrent action of both is generally recognized as indispensable to our idea of life in its ordinary acceptation; yet each is manifestly antagonistic of the other. Under the exclusive control of the former, organized structure would retain whatever form and composition it received at its creation. As in the seed of a plant, the ovum of a bird, or an hibernating animal protected from the action of physical stimuli, it would maintain an inert and dormant vitality, unrecognizable by our senses, for an indefinite period—it would be immortal. Placed under the sole influence of physical forces, it would instantly undergo numerous modifications in the composition and condition of its materials, which would be subversive of its distinctive character; death and decay would be cotemporaneous with conferred existence. Applied to man, the union of the two properties is necessary to the maintenance of that beautiful, and in its results, conservative law which consigns his body, after a certain degree of maturity, and the fulfilment of duties of high importance in the scheme of creation, to temporary decomposition and destruction.

The phenomena of organic matter have always been objects of profound interest to men of science. Their investigation has long and ardently engaged the attention of ingenious minds; and a clear and just solution of their nature has ever been looked upon as more important than the most successful inquiry into any other department of knowledge. Agreeing, generally, in viewing them as under the control and direction of both vital and physical forces, physiologists have yet differed widely as to the degree of influence that should be assigned to each. The leading tendency among them at the present day is to view the functions of life as mainly dependent upon physical laws. Indeed, by a few, vitality is deemed a mere hypothesis—"a chimera of the dark ages"—supported neither by analogy nor experiment, and introduced into scientific discussion to hide the ignorance of pretending philosophers. Life, in their view, is merely some undiscovered physical or electrical condition of the material particles constituting the substance in which it exists. All the supposed proofs of its separate existence are consid-

ered only figurative representations of ideas incapable of being plainly expressed because not susceptible of being clearly understood; and therefore substituted to render, without the labor of explanation, its effects cognizable by our faculties. They are considered as retarding progress, and keeping physiology in the rear of all other sciences. Their only effect, like the mirage of the desert, is delusion; they show us deceitful views of refreshing lakes and shady forests, while the consequent disappointment discourages us on our march to the true fountains and groves of science.

In the prevailing spirit of modern science to simplify and bring under the cognizance of the senses all natural causes, attempts have been made to reduce the phenomena of life to the common laws of mechanical force; but the more general view of material physiologists is, that with these are united "all the peculiarities of chemical forces, and of the not less wonderful cause, which we regard as the ultimate origin of electrical phenomena."*

But the existence of a vital principle, superadded to the physical forces, and, distinct from any known physical property, operating as the source of growth and reproduction of organic bodies, is generally recognized by physiologists. That the influence of this force has been too widely extended and too implicitly submitted to, has been also generally suspected. The idea, inseparable at the dawn of science from every examination of the living organism, of a vital principle distinct from the organism itself, is scarcely admitted by any intelligent modern physiologist, yet many still consider that vital activity has nothing in common with the causes which produce motion or change of form and structure in organic matter. And the fullest examination of the peculiar properties of living beings shows that they are influenced by forces which no present knowledge of the action of physical and chemical agents will explain. Indeed, all our observations and experiments confirm the view that the nature of life can only be investigated with a prospect of success when many of its manifestations are regarded as resulting from the agency of forces as distinct from those of physics and chemistry as these are from each other.† Invoking the aid of physical forces, vitality still maintains its independence; and while it subjects the former to its paramount power, it never permits them to usurp its natural dominion.

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* Liebig's Animal Chemistry, p. 63, Philadelphia edition.

† Carpenter's Principles of Human Physiology, p. 219, American edition.

As belief is essential to the acquisition of knowledge, so incredulity is necessary to that discrimination which constitutes its accuracy; and every doubt, which, leading to the examination, tends to detect the falseness of a reputed fact, tends also to promote science. If applied to the received dogmas of physiological science, its effect is to remove properties of animals from the dominion of vital to that of physical laws; it tends to bring the mysterious principle of vitality itself more within the compass of human comprehension. An object of this and a subsequent communication will be to show that one function, at least, of the animal economy—that of the often discussed one of the capillary systemic circulation—is plainly susceptible of such a removal.

That living beings are endowed with the general properties of all natural bodies, is too evidently in accordance with the analogies of nature and the conclusions of experimental science, to be disputed. Even in the more recondite forms of caloric, light, molecular attraction and repulsion, and chemical affinity, the influence of physical forces over the structure and functions of organic matter is universally recognized. Although differing from vital laws, yet their analogies are sufficiently numerous to show that there is an affinity between them, which is fully equal to the purpose of producing a modification of each other's effects. The separate and combined action of both sets of forces—their concurrence and opposition—their analogy and antagonism—appear to human comprehension inextricably blended, and certainly present complex problems well calculated to rouse the genius and engage the labors of philosophers in their solution. The physiologist who reduces either within its natural and true boundaries, effects an object of high interest to mankind; or, falling short of demonstrating such a result, proves a new analogy, antagonism, difference or concurrence between them, renders a valuable service to science.

Enquiries into the nature of new properties of bodies show such a similarity and conformity in all nature's operations, that the employment of comparison and hypothesis in assigning analogous functions to analogous causes, is fully justified. This, indeed, forms a rule of logic from the practical application of which no science is fully free, and is indispensable to the successful prosecution of physiology. Notwithstanding the endeavor and pretension of physiologists to adhere rigidly to the Baconian system of induction, we perceive that much of our knowledge of the laws, or most general principles of organic structure, is intimately connected with and dependent upon comparison and analogy. Without the valuable indications which these principles of reasoning afford, how

much should we know of comparative anatomy and pathology, or even therapeutics?

In every science there are axioms that require no proof, and there are operations of laws, which, from their being above the reach of experiment, admit of no demonstration, and yet are equally entitled to belief. That invaluable but inexplicable property of the human mind, common sense, compels it to receive many unprovable, and even incomprehensible ideas, because they carry an evidence of truth which necessitates their admission. In the department of knowledge treating of the nature of the vital forces, how many propositions are there which can be proved experimentally? And yet we readily admit there are very many, deduced from analogical reasoning and hypothesis, that are not only probably, but absolutely true. In the germination of the grains of wheat, exhumed after a burial of thousands of years in the catacombs of Thebes, we have evidence that vitality may exist in a dormant state for an indefinite period, and we may infer that under the same circumstances it would exist for ever; but it is obviously impossible to prove it. Even in the generally exact and demonstrative science of mathematics, we are incapable of proving experimentally, or even logically, beyond the possibility of doubt, many of its most elementary positions. How can we prove that those which assume a continuance throughout all space, or for eternity, are true?

Hypothesis, as affording a rational means of attempting a description of instinctive ideas, and of giving form and expression to self-evident, though not demonstrable propositions, is admissible in scientific enquiries. As an agent by which we are enabled to concentrate our views of phenomena, and direct our investigations to rational experiment, it is a requisite aid to almost every advance in knowledge. A discovery, not preceded by an hypothesis, is an accident; and, however valuable, confers no merit on the discoverer. But an hypothesis founded upon the analogies which facts in a cognate science, and enlightened reasoning afford, is not only admissable and meritorious, but indispensable to all our attempts to complete the boundaries of any science in which few discoveries have been made and defined by the senses. Though it may not admit of direct experimental proofs, yet if it is founded npon known analogies, and accounts for phenomena with a probable adaptation of means to ends, it is entitled to the favorable consideration of philosophers.

The attempts of physiologists to withdraw the phenomena of life from the dominion of purely conjectural vital laws, and bring them under that of the more demonstrative principles of physics, required hypothe-

sis, and have been both impeded and aided by its use. In the early efforts, the mind was bewildered in the vain endeavor to trace the mazes of doubt in which the whole subject of physiology was involved, and suggested hypotheses which were as wild and fruitless as any they were put forth to supersede, until the light afforded by the fortunate detection of one fallacious principle, induced the proper examination of another, and thus led us along the path of discovery. Very soon after the attention of mankind was generally directed to the connection of the physical forces with the functions of life, the belief became common, and the expression of it floated alike on the lips of the philosopher and the sciolist, that electricity was generated in the animal organism, and was a necessary property of organic existence. But having no other basis than simple conjecture, it failed to command the general approval of philosophers. And unaided by experiment, or the parallelism afforded by the cognate sciences, to verify or render probable its reality, the conviction or conjecture cannot be considered as contributing any thing to the extension of knowledge, "and as the induction of enthusiasm, rather than any solid reason, is not to be admired for its wisdom." But, as illustrating an instinctive idea of truth, and as an evidence of the general persuasion of the existence of an important natural law, it urges speculative minds to attempt its discovery in the legitimate path of experiment, and is therefore not to be contemned as a mere guess.

Experimental researches in physico-chemical science have evolved the laws and properties of this "ubiquitous, imponderable, exhaustless energy, power, spirit, substance—*quocunque nomine gaudes*—called electricity. Throughout the material universe we find that every change in matter, whether of form or place, develops it in one form or another. A state of electrical tension is produced whenever a body is submitted to any kind of mechanical motion; it is set free by every chemical change of composition; it is developed by every disturbance of temperature; it results from every variety of substance placed in galvanic or magnetic arrangements; and lastly, it is displayed unequivocally in all its known forms by certain animals. Electricity is vaguely believed by many to be the source of all natural motion; the general cause of animal and vegetable life, and its decay; the source of increase and growth from that of the inorganic crystal, and the most imperfect plant or minute insect, up to a tree or a whale; and under a different name, as the power whence springs the revolutions of satellites around planets, and planets around suns.

The analogies afforded by physical science, and physiological experiment, so far as it has been made, show that electricity is the probable,

perhaps the undoubted cause of muscular contractility and expansion, the various capillary circulations, nutrition, secretion, the generation and exhaustion of animal life. No physiological fact is more certain than that it is capable of performing, at least vicariously, many of the functions considered as dependent on vital principles. And certainly, if we consider its laws in connection with the various composition of the living structure, it may well be doubted whether any bodies in nature or art unite more of the conditions necessary to produce a subversion of the balance, or a restoration of the equilibrium of electricity. The whole animal body exhibits in its incessant composition and decomposition the appearances commonly assigned to electro-chemical force; while all the phenomena constituting the inexplicable power called life, manifest effects which may rationally be considered as resembling a series of electrical actions and reactions.

The connection of electricity, established by experiment, with parts of the animal structure, may very properly be extended by analogy to the whole. The subject divides itself into several branches, partly corresponding with the progress of our knowledge of the facts that have been gradually developed, and partly depending upon the divisions into which it has been made by the science of natural phylosophy.

Static electricity, as evinced by the spontaneous projection of sparks from the skins of the lower animals, and sometimes of man, appears to have been observed, though recorded in obscure and scattered facts, and often under the influence of an imagination which magnified and exaggerated them into supernatural appearances, at a very early period of philosophical history. As the result of friction, in particular states of the weather, it is among the best attested electrical facts of modern days. Saussure, Larrey, and other writers, detail instances where electricity in this form has attained an intensity that placed animals of the quadruped class, and even man, in rivalry with the torpedo, or the common electrical machine.

Galvanism, when first discovered, was supposed to be exclusively an animal electricity; and though it has been found a property common to every kind of matter, yet its evolution must be regarded as a prominent quality of the animal structure. Phenomena analogous to those developed by the ordinary forms of galvanic arrangement, are readily made manifest, by combinations of different kinds of animal matter, made into a circuit by means of a metallic conductor. Experiments with such an instrument have produced the most wonderful physiological effects, but as they scarcely afford any support to the position that there is an independent electricity of the living body, it is not necessary

to repeat a description of them. Aldini, the nephew of Galvani, believing that the nervous fluid itself was identical with, or developed the galvanic fluid, performed some remarkable experiments in which very considerable muscular contractions were excited without the intervention of any metal; and thus was the first to demonstrate the independence of electricity of inorganic matter. Under the influence that this simple but novel experiment produced, Galvani, Aldini and most of the men of science of that period assumed that there was a proper animal electricity inherent in the living body; and the hypothetical agent which for many ages, under the name of the nervous fluid, had reigned as the vital principle, now gave way to electricity, through which "the decrees of the understanding and the dictates of the will were conveyed from the organ of the brain to the obedient member of the body."* But this opinion, however attractive from its simplicity, was too crude and too feebly supported by facts to command the assent of all the contemporary physiologists, electricians and chemists, and was generally repudiated in the succeeding age. Volta, and after him Davy, offered as the true explanation, that the electrical excitement was due to the mutual contact of two dissimilar substances; or rather to that decomposition of electricity which takes place on the combustion of any three kinds of matter.

Recently, Matteucci, improving upon the experiments of the early electrical physiologists as well as upon his own, has proved the correctness of Galvani's hypothesis, by demonstrating the generation of electrical currents in the muscular system of the living body. By the happy combination of the rigorous method of physical experiment with the close reasoning of physiological research, he has fully established the important fact of the existence of an electric current, directed in its development from the deep to the superficial parts of the muscles. Contrary to the deductions of Volta and other experimenters, he has determined that this electricity pervades every muscle in every species of animal which has been made the subject of examination. He has also shown that the intensity of the current is greater in the warm than in the cold blooded animals.†

The question whether there is an identity between the magnetic forces and any of the phenomena presented by the animal body, has received some examination from men of science, and has frequently been answered in the affirmative. But in general the demonstrations of this identity, though more numerous than those in regard to the resemblance

* Lardner's Popular Lectures on Science, vol. 1, p. 363.

† Lectures on Physical Phenomena of Living Beings, p. 178.

between galvanism and animal electricity, can scarcely be considered either so conclusive or satisfactory. Experiments from which obvious and uniform effects were expected, have been attended with results, from which the most vague and discordant conclusions have been drawn. Thus Pouillet, Buland,* Prevost, David,† Beraudi,‡ assert, as the result of numerous, varied and careful experiments, that a needle introduced into a nerve of a living animal becomes magnetic. On the other hand, Person,§ though believing in the electrical properties of the nerves, was unable to detect any electrical effect from them upon the needle of the galvanometer. And after him, Matteucci, experimenting in his precise and critical manner, on the large crural nerve of a horse, says, "I must confess, that whenever the experiment was well made, I never obtained evident and constant traces of the electric current," by a deflection of the galvanometer from irritation excited in the nerve. How far this failure may be dependent upon the small quantity or low intensity of a current, and the inadequacy of our means and mode of experimenting to develop it, we do not know; but considerations on these subjects will be hereafter presented to the reader from which he can draw his own conclusions. In the present state of our knowledge, it is at least doubtful whether electricity or any other fluid is transmitted along the nerves.

Although the proof of the existence of electro-magnetic currents in the nerves is of difficult or doubtful attainment, yet the action of the muscles in inducing this effect is by no means equivocal; for, independent of direct experiments showing the fact, it is well ascertained that needles, after being used in acupuncturation, show magnetic power.

Thermal electricity being the result of every disturbance in the equal flow of heat, must necessarily exist as a property of matter so subject to changes of temperature, and so capable of transmitting their effects as the animal structure. So vast is the quantity of electricity of this peculiar form developed by animals, that with a pile, carefully made of a large number of the elements usually employed for manifesting its effects, united to a galvanometer, the warmth of the hand may be made sensible at the distance of thirty feet. Indeed so conspicuous is this species of electricity, or so sensible and delicate a measurer of it is the instrument

* Ranking's Abstract, vol. 1, p. 244.

† Cyclopedia of Practical Medicine, vol. 2, p. 291.

‡ American Journal Medical Sciences, vol. 9. p. 483. Lectures, op. cit. p. 259.

§ Dunglison's Philosophy, 7th edition, vol. 1, p. 119.

referred to, that by its means the comparative warmth of different insects may be ascertained.*

The most full and complete evidences of free electricity in living animals are to be found in certain species of fish, the best known of which are the Raia-Torpedo, and the Gymnotus-Electricus. In these fish the identity of animal with common electricity is proved, not only by the similarity of the effects upon the feelings produced by their shocks, but also by the most striking resemblance between their other effects and those of the various kinds of electricity. From the former, Blainville and Flourens obtained magnetic deflection ; Dr. John Davy magnetized the needle, and was able to effect electro-chemical decomposition of common salt, nitrate of silver and super-acetate of lead ; and more recently Linari and Matteucci gave produced the spark. But the Gymnotus in the hands of Farraday has more readily, and in a more striking manner, all the proofs required of the identity of its power with that of common electricity.†

In the preceding cursory view of the connection and identity of electricity derived from the animal structure with that from purely inorganic sources, we have advanced nothing that is not universally recognized as true by philosophers, and easily demonstrated to the senses. But it is not acknowledged, nor do the facts now mentioned prove that electricity is a special property of the living organism, (at least of warm blooded animals) and still less that it directs or influences the functions. Even if the experiments adduced be admitted as affording evidence of electrical action on the solids and semi-solids, they cannot be considered as proving any operation on the different circulations, nutrition, the secretions, the production of animal heat, or any of the functions that are referred to vital principles as their cause. To prove a functional power in electricity, it is necessary that it should be made apparent as a distinctive property of organic fluids. That appearances are presented by the fluids of the animal system that can only be rationally assigned to the influence of the forces of attraction and repulsion, is acknowledged ; but a polarization of these forces, or a reciprocal action of each upon the other, rendered distinct to the eye of observation, is required to demonstrate that they are the effects of electricity, and this has not yet been effected in a conclusive or satisfactory manner. It is true that M. Donne, a physiologist favorably known to medical men as a microscopist, and who, in this instance, appears to be a patient in-

* Noad's Lectures on Electricity, page 241.

† Ibid, page 248.

vestigator of animal electricity, has presented to the public some highly interesting statements, which tend to show that the polarization of animal fluids has been accomplished. If the experiments he has reported have been carefully observed, and there is no mistake of cause, electromagnetic force is proved to exist as a function of the glandular system, and secretion is resolved into a mere play of electric affinities. The principle results he obtained show that the external cutaneous and internal mucous membrane represent, through their secretions, the two poles of a galvanic circle, the electrical effects of which are appreciable by the galvanometer. He affirms that on placing one of the conductors of the instrument in connection with the mucous membrane of the mouth, and the other in contact with the skin, the magnetic needle deviated fifteen, twenty and even thirty degrees, according to its sensibility; and the direction of its deviation indicated that the internal mucous, or alkaline membrane, took negative and the cutaneous or acid membrane positive electricity. He further asserts that between the acid stomach, and the alkaline liver, extremely powerful electrical currents are formed.* The only probable objection to the conclusions of M. Donne appears to be, that the different electrical effects he obtained may be attributed to the decomposition of salts, and the action of acids, after they had been thrown off from the system as mere excretory inorganic matter upon the wires† of the galvanometer, rather than as the action of living fluids.

Having these ample evidences of electrical manifestations in organic structure, the hypothesis of different electrical states in arterial and venous blood readily suggests itself; and the object of the remainder of this essay will be to show by analogical and inductive reasoning that it is founded on probability, and to demonstrate by experiment its certainty.

The blood is known as the common material from which all the tissues of the animal body are built; as the grand source of stimulus to the nervous and secretory functions; as the fountain of vitality, and the fluid with which, of all parts of our body, we associate most intimately and indissolubly our idea of life. On account of the very important part which it performs in every function of the animal economy, it is necessary for the object in view that notice should be taken of every one of its supposed properties, and its real ones ascertained and examined.

* Dunglison's Physiology, vol. 2, p. 257.

† London Medico-Chirurgical Review, 1837.

Upon pure physiological considerations we should infer a difference of electrical excitement between arterial and venous blood. The former is recognized as the real and exclusive source from which all parts of the body are formed and developed, while the latter is only the receptacle of such products of the metamorphosis of the tissues as are not carried off by the secretions. The very great difference of office assigned to each fluid would alone indicate that the arterial blood must be more eminently charged with the forces of attraction than the venous. We find, too, that in the execution of the glandular functions of the organism, peculiar elements of the blood, tending to alter its electrical properties, are dismissed from the circulation. Thus the kidneys, in forming urine, separate directly from arterial blood a large proportion of nitrogen—an electric high in the scale of negatives—and, of course, the blood, returned to the circulatory system by the renal veins, cannot possibly be in the same electrical condition as that in the renal arteries. But independent of such a fact, it is difficult to suppose that "the different transmissions and transformations of the arterial blood, by which its constituents are converted into fat, muscular fibre, substance of the brain and nerves, hair, bones," etc., can take place without the presence and exertion of those forces, in their appropriate quantities, which accompany all changes in organic matter; or that venous blood, having no similar duties to perform, can be charged in an equal degree with electricity.

The general physical properties of the blood show a marked difference between its arterial and venous varieties; and indicate, upon known principles, a higher electro-positive state in the former than in the latter. In this part of our enquiry little assistance can be derived from the investigations of the anatomist; but that little is eminently favorable to our view. The structure of the blood, divested of the fanciful properties with which the enthusiasm of microscopists has endowed it, seems, equally with that of the solids, to present the conditions necessary for developing electricity. According to the most generally recognized opinions it is composed essentially of cells or vesicular globules, enclosing a dense, high colored liquid of obviously a different chemical composition, and floating in a liquid, which is less consistent, and of course also of a different chemical composition. There are here not only the physical elements of matter necessary to every simple galvanic circle, but the form and arrangement of them most favorable to the polarization and transmission of electricity. All the examinations of the blood indicate that the arterial is richer in globules than the venous,

and hence it may be inferred that it is endowed with a higher vitality and a greater quantity of electricity.

Among the physical differences of the blood contained in the two sets of vessels, the arterial is found to yield a fuller, firmer coagulum, and therefore it must contain a less quantity of the more aqueous serum than the venous; which is an indication of a larger quantity of globules and electricity generating matter.

The color of the two kinds of blood is very different; that of the arterial being in the higher order of animals, of a richer and vermillion redness, dependent alike upon a larger proportion of globules and a surcharge of the electro-negative principle—oxygen.

Although the relative temperature of arterial and venous blood, simple as the determination of the question would seem to be, has not been ascertained to the satisfaction of all physiologists, yet the weight of authority seems to be in favor of the opinion that the temperature of the former is higher than that of the latter. Independent of observation with the thermometer, we might infer such a result from the absorption and solution of oxygen by the arterial, and the setting free of carbonic acid by venous blood—upon the simple physical principles that condensation produces heat and expansion generates cold. Conjoined with the fact that the specific heat of arterial blood is also somewhat greater than the venous, we may rationally infer that the former has, from this cause, a higher electrical tension, and that a current of electricity always flows towards the latter.

Mechanical friction is well known as the most simple and general means of putting electricity into action; and this effect takes place whether it be between solids, or solids and fluids. If mercury, after being subjected to the friction of agitation in a glass tube be presented to an electroscope, it will give distinct signs of positive electricity. Mr. Farraday has shown, in his observations on the source of electricity in the hydro-electric machine, that the forcible ejection of a liquid through small tubes, or orifices, may be made the most powerful source, in both quantity and intensity, of statical electricity.

In the structure of the arteries of the systemic circulation we see a nice adaptation of means not only to the generation or evolution of electricity from the blood, but also to its preservation or insulation. "These vessels are flexible, elastic tubes, principally composed of membraneous matter, formed into distinct layers, and comprising what are called the coats of the arteries. Of these coats anatomists usually describe two, as possessing a sufficiently determinate structure to be easily distinguished from each other; the outer one partaking more of the cellular

structure, and therefore called the cellular coat; and an inner membrane, white, firm and smooth, possessing more of the physical properties of tendon. Between these membraneous coats is situated a stratum of transverse fibres, which have been termed the muscular coat."*

The chemical composition and anatomical arrangement of these coats suggest an obvious and beautiful design for checking that tendency to equilibrium which is so marked a property of electricity, and for retaining it in the blood, in furtherance of any functional duties it may have to perform. Electricians appear to consider the animal system as nearly equally divided into conductors and non-conductors of electricity; the aqueous fluids and muscles being considered as belonging to the former class; the oils and all adipose matter to the latter. Indeed, the structure of the whole animal system seems, among its other objects, to be admirably adapted and intended for the retention and economical dispensation of the energizing fluid. All the organs of the body, whose functions are considered essential to secretion or reproduction are found protected to a greater or less extent by non-conducting investments. The position of the blood in this relation does not seem to have been defined or examined, but fulfilling in its systemic circulation the great duties of nutrition, secretion and the vital functions, probably connected with the evolution of electricity, we should readily suppose it would be well provided with non-conducting envelopes, for moderating that tendency to equilibrium which would otherwise take place too rapidly. Experiment fully confirms this view of the electrical properties of arteries. The aorta of an ox was found, even in its recent state, to be a non-conductor of electricity, high in the scale of insulators—classing apparently above parchment or leather. The arteries are therefore good electrics, because the properties of bodies which render them good insulators, render them also good electrics.

"In the arteries there is a very great degree of friction—of the blood globules against the arteries—of the arteries contracting round the blood like an obstacle—and of the particles of blood amongst each other, by the confused and vertical manner in which they are propelled."† On the other hand, the greater size of the veins and the consequent slowness with which the blood flows in them would lead us to suppose that venous blood must be nearly free from friction; while, from the thin and dense texture of the venous coats, they must be incapable of insulating electricity. We may, therefore, reasonably conclude that

* Bostock's Physiology, vol. 1, p. 271.

† Haller's Physiology.

the projection and transmission of the arterial blood through the tortuous, non-conducting and electric walls of tubes, presenting before their termination a very small calibre, must generate a portion of that electricity which we believe is necessary to the animal system. At least we must infer that, from the difference in the motion of the blood and the properties of the vessels, it must be in a higher state of excitation in the arteries than in the veins.

The next analogical confirmation of our hypothesis is to be found in the chemical constitutions of arterial and venous blood, and the well known law of electricity that bodies differing in chemical composition exercise different electrical relations towards each other; it being considered that that body, whether simple or compound, which is most highly charged with electro-negative principles or elements, has always a tendency to give out positive electricity to bodies presenting the opposite, or a different electrical relation.

The determination of the chemical composition of organic structure, is always a difficult task, and subject to much greater diversity of result than that of inorganic matter. This is fully exemplified in the attempts to analyze the blood; for no two agree in all respects with each other. Considering the striking difference between the physical qualities of arterial and venous blood, it excites some surprise that chemical analyses should show so great a uniformity as is found in the composition of their organic elements. In the absence of an apparent or easily ascertained diversity in the chemical constitution of the two kinds of blood, we recognize a strong objection to our electrical hypothesis; but, besides that organic elements do not form the whole of the blood, a minute and careful examination shows that there is a sufficient difference in composition to enable it to possess electricity enough for any duty it has to perform. All the experiments and discoveries in animal electricity (except those in electrical fishes) evince that it exists in a state of exceedingly low intensity—corresponding with the slight difference in chemical composition of the animal tissues. Nature, in this instance, as in her usual practice, works out the end she aims at gradually, silently, imperceptibly, and with the greatest economy of means; yet with these means she always obtains the most complete as well as the most magnificent results. In particular cases, the small quantity of the forces she employs in her operations is compensated by the greatness of their intensity; but more commonly she supplies deficiency of intensity by vastness of quantity. Unlike the narrow resources of art, she can make up for limited power by a rapid renewal of her ope-

rations; for low intensity by great quantity of force: and for small space by endless convolutions of surface, or infinite divisions of matter. In the circulation of the blood through the capillaries of the systemic system, it is divided into an innumerable multitude of minute streamlets, each so small, as, perhaps, to admit but a single layer of its corpuscles; and in these, therefore, the surface which is placed in contact with their walls is so enormously extended, that the degree of friction and exposure to electrical and chemical attraction must be almost beyond calculation. Hence, it is possible to understand how an electric force, adequate even to perform all the functions of the animal body, might be obtained from an electrical excitement of very low intensity.

According to M. Lucanu, who may be regarded as among the highest authorities on this subject, the immense number of substances blood contains, are found in both kinds and in nearly equal quantities. Beyond the physical appearances already mentioned, arterial seems to vary from venous blood only in the more decided character of its peculiar odor, the greater abundance of globules, fibrine and fixed material, a diminished proportion of carbon and a higher one of oxygen in combination.* In the quantity of albumen, and of extractive, saline and fatty matter, both kinds of blood are about equal.

But it is in the comparative quantities of the inorganic, mineral and ærial constituents of the blood—particularly iron, oxygen and carbonic acid—combined and free, that the two kinds of blood chiefly differ. Respecting the manner in which oxygen—the quantity and electrical properties of which render it far the most important in the present consideration—is combined with the blood and conveyed to the peripheral system, there are two theories prevalent among physiological chemists —those of Liebig and Mulder. Liebig refers the change of properties that venous blood undergoes in becoming arterial solely to the absorption of oxygen by the iron in the corpuscles;† while Mulder maintains that it is entirely owing to the oxidation of proteine compounds.‡ There is reason to conclude from the experiments of M. Denis, as well as the researches of Mulder, in regard to the difference between the fibrine of arterial and venous blood, that oxygen derived from the inspired air enters into actual combination with this element. Doctor

* Gibert on the Blood, page 12.

† Animal Chemistry, page 55.

‡ Ranking's Abstract, vol. 1, page 287.

Carpenter* adds, that the same may be probably true of other constituents of the blood; and therefore the theory of Mulder is probably ths more correct one. But the operations comprised in both theories may be carried on; for while arterial blood undoubtedly contains more assimilated oxygen, and is more highly animalized than the venous, "the red corpuscles are the chief carriers of the free gases to be interchanged between the capillaries of the several systems of the vessels." The truth and accuracy of either theory, however, is not absolutely essential to the attainment of the present object. The experiments and deductions on which the conclusions of both are founded show the important fact, that there is more of the electro-negative element, oxygen, in arterial than venous blood.

The difference in the quantities of the free oxygen and carbonic acid in the blood is of higher consequence than that of the combined, in an enquiry into the comparative electricity of its two varieties. In the investigation of this subject we are aided by the recent and splendid researches of Magnus on the function of respiration. He has made a great many experiments with a view to elucidate the nature of arterialization and the relation of the gases to the process; and they are deemed so accurate and deserving of confidence, that they are quoted with approbation in most late treatises on Physiology. They were not, as from the nature of gases they could not be, entirely accordant with each other; but a minute examination of them by Gay-Lussac, with the object of refuting the conclusions of Magnus, and a repetition or extension of part of them by Majendie, afford corroboration of the correctness of his views on respiration.† In consequence of the objections of Gay-Lussac, these experiments have been repeated and extended by Magnus himself and their accuracy confirmed; they have also been repeated by Prof. Shultz of Berlin, and by Doctors Stevens and R. E. Rogers, of this country, with a general verification of the results.‡ A table deduced from Magnus' experiments represents the average absolute quantities of free gases in the blood of horses and calves as follows, viz:

	Cubic Centimetres.	
For 100 parts of arterial blood	10.4276 of gases composed of	6.4967 carbonic acid. 2.4178 oxygen. 1.5131 azote.
For 100 parts of venous blood.	7.6825 of gases composed of	5.5041 carbonic acid. 1.1703 oxygen. 1.0081 azote.

* Human Physiology, page 588.

† Ranking's Abstract, vol. 1, p. 295.

‡ Dunglison's Physiology, vol. 2, p. 112.

This view of the subject presents results which are of the highest interest to our theory of the electrical relation of arterial to venous blood. They show that—

First. There exists in the arterial blood a larger quantity of gas than in venous blood.

Second. The quantity of oxygen found in arterial blood is double that which exists in venous blood.

Third. The ratio between the oxygen and carbonic acid is from one third to almost one half in arterial blood, while it is only from one fourth to even one fifth in venous blood.*†

The importance of these beautiful experiments in explaining the function of respiration cannot be doubted; nor are they less valuable as corroborative of the view of a higher state of electrical energy in arterial than venous blood. Oxygen and carbonic acid gases are negative electrics of a very high order; and, according to recognized principles of electricity, they must cause any substance containing them in one quantity to transmit a current of positive electricity to another containing a less quantity, on the completion of a galvanic circle. Minutely diffused and circulating rapidly with the blood throughout the animal system, and constantly receiving new accessions from the atmosphere, they must necessarily, in their contact and affinities with the tissues, and their course to the venous system, evolve very large quantities of electricity. Viewed in both their physiological and electrical relations, there can be little doubt that the changes in the blood, which are effected by the function of nutrition, have reference, in great part, to the proportions of these gases which the arterial and venous systems hold in solution, and the quantity and kind of electricity they evolve.

It is, perhaps, not the least of the recommendations of this hypothesis, that it proves an adaptation to and supplies a direct and positive want of organic structure. This fitness of means to ends is an invariable characteristic of the Deity's works; and the highest degree of deducible evidence of its existence is indispensable to the support of every hypothesis.

In explaining the phenomena of the organic functions, it has been customary to commence with the proofs of digestion and terminate with

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* Note. Though the absolute quantities of oxygen and carbonic acid are greater in arterial than in venous blood, yet the relative quantity of the latter to the whole amount of the gases is greater in venous blood; it being in arterial 62.3 and in venous blood 71.6 per cent.

† Matteucci. Lectures on the Physical Phenomena of Living Beings, p. 127.

that of nutrition. After the chyle is seen to be formed, its course in connection with the lymph and venous blood, is traced to the heart, thence to the lungs, and thence, after an examination of the changes by which the whole is animalized and vitalized, it is retraced to the heart. The arterial blood is then followed through the arteries till it is lost in the capillary tissues, and finally resigned to the inexplicable laws by which the tissues are made to appropriate the parts suitable to their purposes, or the glands to separate the molecules required for secretion. The remainder of the blood and the residue of the tissues are considered as then, in some unknown way, transferred to or absorbed by the veins and changed into venous blood, in order to renew the cycle of an ever varying but continuous existence. In the details of this elaborate prosecution and enquiry the possible causes of the various effects are minutely examined, but the examination has never resulted in satisfactory conclusions. A broken series of causes, tending rather to render mysterious the nature of the powers by which the individual is preserved, continued and renewed, than to furnish an adequate explanation of the effects, has been adopted. The idea of a power received with the ærial inorganic elements from the atmosphere, which, by mixing with and vivifying the blood, is, in connection with other known physical causes, sufficient to produce the whole series of phenomena, has been either disregarded or slightly and partially examined. Though often surmised or hinted at, the possibility of demonstrating its existence seems never to have been suggested.

The ordinary changes in the properties of matter from inorganic to organic are the most wonderful, as they are the greatest that take place in nature. But organic existence presents manifestations of a power—at one time normal, at another abnormal; sometimes increased, sometimes diminished in energy—which seems to be superadded to the ordinary evidences of vitality. This power is seen to be exerted in the embryo to develop it into the perfect-formed and new-born animal; and is farther continued till it attains the powers and magnitude of maturity. It exists in the reproductive power by which the inferior animals renew lost parts—as the snail its head, the newt its legs, or the crab its claws. It is evinced in its most extraordinary form in the power of parents to procreate and transmit their own form and features to a numerous offspring. But it also exists in the morbid growths of animal structure, constituting hypertrophy and tumors; and must be considered as operating in the opposite effects of atrophy and attenuation. All these effects of this extraordinary power, on the hypothesis that life de-

pends exclusively upon a vital principle, must be attributed to a redundancy of vitality—a superfluity of life, or a deficiency. Upon this view life must be supposed to increase or diminish with every change in the volume and form of a living being. With our most enlarged views of the nature of vitality can such attributes be considered possible? and under the more generally received idea of its unity and immateriality, are they not too absurd to be logically assumed?

Every view that we are able to take of the recondite and mysterious phenomena of the functions, demands that the agents through which they act should be endowed with very energetic and very peculiar properties. Electricity is the only known power which presents the least probability of a capacity to work such extraordinary results. In assuming that this force exists in arterial blood in a greater quantity and of a higher tension than in venous, or indeed any other structure, we assume a power which supplies the link, hitherto wanting, in the chain forming the connecting bond between the organic functions, and fully adequate to explain them. The general properties and analogies we have assigned to it we deem sufficient, on recognized principles of philosophy, to explain the nutritive functions; and if we add to them its power of indefinite accumulation, condensation and expansion in the whole or part of a structure like that of the animal body, we may be able to understand how those extraordinary appearances, which, on the vital hypothesis, require a superabundance of life, may be produced. If, then, we have proved by argumental evidence that electricity exists in an exalted condition in arterial blood; and more especially if we shall hereafter demonstrate the fact experimentally, we shall have proved the existence of a cause adequate to produce the effects we have assigned to it.

(*To be continued.*)

III.—AN ACCOUNT OF THE YELLOW FEVER WHICH PREVAILED IN THE TOWN OF WASHINGTON, LA.

In the latter part of the Summer and Fall of 1853, *with Remarks.*

BY T. A. COOKE, M. D.

I propose to give an account of the Epidemic Fever which prevailed in the town of Washington during a portion of the Summer and Fal. of this year; and in noticing its origin, type, symptoms and treatment, shall occasionally indulge in such remarks as the subject will necessa-

rily suggest. It is in truth one which has acquired a fearful interest in the history of Louisiana, and indeed of the whole South.

We hear of its introduction into New Orleans in the early part of summer ; in the month of July it assumes an unparalleled severity ; its march is onward in defiance of all opposition ; it consumes the aliment which a long period of security had accumulated in too ample abundance ; and then it slowly departs, lingering in its footsteps, and still proclaiming its irresistibleness in the daily additions it makes to the long list of the dead.

But to the city of New Orleans its ravages are not limited. The disease is transmitted to all the towns and villages in the State ; indeed to all the towns in the Southern States, with few exceptions, having communication with that city of pestilence and of death ; and from the towns it is even carried to the large neighboring plantations.

The effect of a fatal pestilence in towns or the country cannot well be conceived by those who have not witnessed it. It spreads alarm ; the people are panic-stricken ; and every death adds to the consternation, which sweeps over the land. Soon all direct communication is arrested between the country and the unhappy town ; intercourse even in the country is restricted ; every house becomes a barricaded castle ; then ensues a disruption of the bonds which hold men together, and for the time society is dissolved. Such a calamity as a fatal epidemic in the country is not easily forgotten. Years cannot efface the recollection of the disastrous effects of such a visitation. In the country the dead were known to all, and the remembrance of virtue and of merit is not buried with the mortal remains of the dead. But the city, after such a visitation marking its progress with desolation and misery, sweeping into the untimely grave whole families, is like the sea, more tranquil after the tempest has subsided and the surges cease to roll. The public mind, instead of being aroused by scenes the most fearful —intense beyond the reach of the imagination—pictured only in reality, falls back into the coldest apathy, and the past is forgotten.

With regard to the origin of this disease, it is to be remarked that there was a weekly communication between the town and Orleans up to the 15th of August, the date of the last arrival of a steamboat from the city. Every trip the boat brought various kinds of merchandize for the merchants of our town and for the people in the country. In the last trip she brought back an individual, who, though suffering with fever on his arrival, went his way to his home in the country, and expired on the second or third day with all the symptoms of yellow fe-

ver. By the trip of the boat before the last, four individuals, free persons of color, after the death in the city of their mother, a brother and a sister, of the prevailing epidemic, with their baggage returned to the town, from whence a year ago the family had removed to New Orleans. Soon after their arrival one of these persons had an attack of fever, but it was not of such severity as to attract particular attention. The members of the family in whose midst these people from the city were received, were evidently seized with great fear and anxiety, as is proven by the contradictory statements which were made by them relative to the baggage, etc., brought by these people, and by the fact that a trunk remained unopened for many days. At length the brother of Simon Lepp, the proprietor, determined to open this trunk, and the clerk of the store relates that the smell from the articles it contained was so offensive as to sicken him, and to cause him for a time to desist. All the articles were finally taken out and exposed to the air in the back yard; and it appears that an individual from the country, named Miller, passed an hour or two exposed to the effluvia from these clothes thus exposed. This man, the proprietor, the clerk and brother of the last, and a free man of color living in the next house, were all taken sick within a day or two of each other. Miller and Lepp's brother died after about three day's illness; the other three got well. The clerk had a violent attack. Lepp was not long sick, and Sterling, the colored man, had a mild attack.

About this time other cases occurred. A child of Lelah, free colored woman, who had received into her house the girl from New Orleans a few days after her arrival and seizure with fever, fell sick; and here it may be remarked that soon every member of the family, seven in number, one after the other was violently attacked. A child of Sterling, before his recovery, was seized with fever; and in rapid succession the wife, three remaining children and two slaves were attacked. Mrs. King, living at a distance from the point of infection, passed several hours in the immediate neighborhood; very speedily she sickened and died. All the members of the family in whose house she died, fell sick in rapid succession. Several individuals attended the funeral of Mrs. K.; one of them, it is reported, died with black vomit. Two others I know were attacked and recovered. In another part of the town, at this early period, a case occurred, Mrs. Jenkins, who no doubt contracted the disease in daily recent visits she received from a female member of Lepp's family. Among the first cases reported was that of A. Folain, who, nevertheless, had had the disease many years ago. He is the owner of a large ware-

house and receiver of goods from the city. And also in the country the case of Mr. Valensart was reported; a case undoubtedly of a violent attack of fever. This gentleman had been often in town, but in what manner he contracted his disease I have not been able to learn. These cases occurred in the practice of Dr. Tallis, a gentleman of high reputation here. Mr. Valensart came to town several weeks after he first fell sick, contracted the epidemic fever, and in three days died.

In a short time after the occurrence of these first cases, not only individuals and whole families living in the neihhborhood of the house into which the poison of yellow fever, in a concentrated form, or in quantity unusually abundant, or most potent in its procreative energies, was directly imported from New Orleans, were soon attacked with a fever, distinct in its symptoms, malignant in its type, sui generis, wearing an unmistakeable livery, but a great many individuals living in different parts of the town, but in the habit of daily communication with the infected region, also fell sick with the same fever, and thus in a brief period the fever reigned all over the town, and ere long the number of the sick at one time was reckoned at two hundred—nearly one half of the whole population then remaining.

Louis Lambert, a most estimable youth, replaced the clerk of Lepp, immediately after the attack of the latter; in a few days his whole system, apparently saturated with the morbific cause of the fever, yielded to the disease. He was with difficulty on the second day removed to his home in the country. Medical aid was fruitless. Black vomit closed the scene in almost a few hours. His father, mother, brother and two little sisters were in town several times during his brief clerkship. They all and alone of the family fell sick; four were severely attacked; all distinctly presented strong symptoms of the epidemic influence. As said, none but these, of this large family, who contracted the disease in town, took it. They were visited by a large number of persons with impunity.

V. Goubert attended the funeral of Mrs. King; he had a mild attack. but a severe jaundice retarded his recovery. He lived on an adjoining plantation containing some sixty souls; none contracted the disease from him.

About ten members of her family from the country visited Mrs. Sterling on her death-bed. On their return home—and some remained but a few hours in town—her father, mother, a sister and two brothers and two servants fell sick. They were all mulattoes except the slaves. To this house in the country, immediately after the death of his wife, Ster-

ling, his four children and two slaves were removed. He and one child were already sick; the other three and the two slaves were speedily attacked.

There were ten patients in one small house in the country at one and the same time, and as many as four in one close room. Under these circumstances, not only several persons who had been in town, but all the attendants on the sick who had not been in town, escaped the disease, which was in this family of a most fatal type. It would be a superfluous but easy task to cite numerous examples similar to the above.

In the years 1837, '39, '42 and '53, about fifty persons contracted the yellow fever in Opelousas and Washington, the disease only prevailing in the last place in 1853, and only in Opelousas in the other three years; they died almost always with black vomit at their country houses, and in no one instance did any attendant on the sick catch the disease.

As far as I have been able to learn, no doubt exists this year that this fever has been transmitted directly or indirectly from New Orleans to every place in which it has prevailed as an epidemic; and I believe that the opponents of the importation of it into New Orleans must admit that there was this year, at least, a remarkable coincidence between the actual importation of its morbific principle and the actual occurrence of the disease; and it may be here remarked, inasmuch as in hundreds of certified instances which have occurred from year to year up to this time all over the country, and embracing also New Orleans, this pestilence can be legitimately referred to an imported cause; that in the absence of overt facts proving its importation, a strong probability of this importation should be regarded as satisfactory as if proof positive existed. In the country towns the fact of importation can be, and is always verified; and another admitted fact—indeed a necessary consequence of the preceding one—is, that it never prevails in the towns unless it previously exists as an epidemic in New Orleans.

With regard to the manner in which the morbific cause is conveyed, no doubt exists that it may be effected through the media not only of sick persons, but of fomites—that is, through every description of merchandize. The only difference between the two modes of producing the disesse is, that fomites convey the actual morbific cause, and consequently will, when accumulated in sufficient quantity, produce the disease in individuals predisposed to it, under any circumstances and every where; whereas the effluvium from sick persons is perfectly innocuous,

unless the surrounding medium be so constituted or modified, as by its addition to assume the morbific character necessary to produce the disease, or to afford sufficient food for the development and perhaps rapid reproduction, of the germs of the disease emanating from the sick body. An infected atmosphere may thus originate from a sick person, and the bed-clothes used, as well as from the actual materies morbi imported through the medium of fomites; and hence arises much confusion of ideas in relation to what is called contagion and infection. The facts in proof of the importation of the disease through the medium of such persons, and of merchandize, are as numerous as the leaves on the trees, at least in the opinion of most country people destitute of prejudice, or a taste for metaphysical disquisitions. For my part, I sincerely believe that if the origin of the disease in New Orleans could always be accurately ascertained, it would be found to consist invariably either in the infection of the sick, or of merchandize from an infected port, or in the actual infection imprisoned in the holds of vessels coming from such ports.

I will here introduce a fact, familiar to the old people of Opelousas, which, though now a matter of tradition, is as well authenticated as any other whatever. About the year 1826 Mr. L. Louaillier, a merchant of Opelousas, imported late in the fall from the city, during the prevalence of yellow fever, some merchandize. Three individuals, who were present on opening some of these goods, contracted a violent fever; one died, and all were said by an experienced physician to have had yellow fever. In 1828 the same individual, with M. Lazaretti, another merchant, imported goods from the city during the prevalence of the fever. Mr. Louallier, ever regretting the error he committed in 1826, for he believed in the importability of the poison or cause of yellow fever,—opened his boxes and bales, and ventilated his goods for three days before taking them to town, and no disease followed from contact with them; whereas of four individuals present on opening the goods of Lazaretti, brought directly to his store in Opelousas, three died with black vomit, and the fourth narrowly escaped with his life.

And facts are not wanting to prove that this disease can be excluded from the towns, even nearly in the same latitude with New Orleans. Natchez was always protected when her quarantine was vigorously enforced; and this year the exemption of Franklin, by her quarantine, and also of Opelousas by her sanitary regulations, are worthy of particular attention. These quarantines, especially that of Franklin, were enforced vi et armis; indeed with a rigor commensurate in severity

with the evil to be warded off, and for the successful execution of them every praise is justly due to the people and the public authorities. The disease was directly transmitted from the city to the small villages of Pattersonville and of Centreville, just below Franklin, with her population exceeding 2000. It was imported into the town of Washington, six miles only from Opelousas. It came also directly from the city, and burst forth with a fatal violence in the town of Lafayette. Franklin and Opelousas, protected by their quarantines against the invasion of a foreign enemy, worse than fire and sword, alone are exempt. The history of the origin of the disease in all the interior towns of the State, affords satisfactory evidence to every one that the cause is always imported from some other place in which the disease existed. Other conditions are also indispensably necessary for the production of an epidemic; to wit, a more or less crowded population, and the previous existence of summer heat for a certain length of time. A fourth condition, equally indispensable, is a peculiar condition or constitution of the atmosphere; certain strange meteorological conditions of the earth and air. Dr. Rush has said "an atmosphere charged with miasmatic effluvia, or pestilential exhalations."

Last year (1852) a man arrived in this town about the middle of September; immediately after leaving the city, three days previously he was seized with violent fever. Dr. Heard was called to him on his arrival, and on his first visit sent for me in consultation. It was a case of pure yellow fever, which from neglect and bad treatment had run a rapid course; death in a few hours ensued. He died in nearly the centre of the town, in a hotel containing a large unacclimated family and numerous boarders. He was well nursed and seen by many people. From this individual no disease spread. There were present apparently all the circumstances required for its propagation. There only existed this year a higher degree of temperature, and more rains than last year, two circumstances not very material, for yellow fever prevails not only in warm and wet, but in cool and dry summers. Looking to the facts that have been mentioned, we are forced to the conclusion, that in 1852 that peculiar atmospheric constitution favorable to the increase and diffusion of the cause of the disease, nay, indispensable to its development, did not exist. During last year it did not grow even in New Orleans into an epidemic; but 72* deaths, I believe, were reported. For several years previously no epidemic had occurred in the city; and no one can doubt that last year the city was full of material

* 456 deaths from yellow fever in New Orleans in 1852.—Ed.

for the disease to prey upon, if the atmospheric condition of its existence had been fully established. Exemption from epidemics for several years had lulled strangers and emigrants into a false security; and this year, unfortunately trusting to the same false hope, the number of unacclimated persons had greatly increased. If we deny the doctrine of contagion, and admit as necessary to the production of the disease the importation of a foreign cause—two conclusions to my mind as clear as any medical facts whatever—it inevitably follows that the poison, or cause of yellow fever, after its introduction, must, before it can engender an epidemic, find pabulum for its development, increase and extension; or by combining with, and vitiating the peculiarly modified atmosphere it meets with, produce a potent cause of disease.

There are many interesting facts which show that the cause of the disease may for a long time be confined to limited districts, and unless contracted by individuals visiting the infected region, and communicated to other districts, its extension will be very slow; but when it once gets a foothold in an atmosphere favorable to its increase and multiplication, its progress, though it may be slow, is certain; nor can any thing arrest it but freezing weather.

Unlike last year, not only in New Orleans, but in every town and village in the State, and even amongt the negroes on many large estates, this year a constitution of the atmosphere most propitious to the devolopment of the morbific cause existed. Three summer months, as by some considered indispensable, had not elapsed before the disease in the city assumed the character of an epidemic. In every village and town of the whole South, a single germ of the pestilence had but to be cast, and universal flight alone could prevent its rapid development and a ruthless decimation of the inhabitants.

The prevalent opinion amongst medical men, and particularly in former years amongst the faculty of New Orleans, was opposed to the importability of the morbific cause of this disease into cities. It is by many said to arise sporadically; that is, as the result of local causes, which are generated without the aid of any foreign cause whatever. That in Mobile, New Orleans and elsewhere, cases do often occur, even in the winter months, is a circumstance, which, instead of proving its indigenous origin, rather in my opinion shows the reverse. These cases are of rare occurrence; they appear to me rather to reveal the fact, that in these commercial cities, having a large population and constant commercial relations with Cuba, and other places, in some one of which the disease is constantly prevailing, the cause, confes-

sedly limited in its operation and short-lived, is accidental and foreign, than that it is the result of domestic influences, unaided by a foreign agent. We know that when the epidemic begins early in the summer, it ceases long before frost; and that a freeze invariably arrests it when it originates late in the season ; and yet in each instance, after the cessation of the epidemic, cases occasionally occur for weeks and months. If the cause of these cases be not foreign ; if these diseases have not been contracted from exposure to the infection imported in some vessel, it appears to me more reasonable to assign them to the previous epidemic cause, which in localities and under peculiar circumstances favorable to its preservation, may have existed in a form isolated, but in sufficient strength to excite the disease. A case occurred in the winter of 1842 in Opelousas, long after ice was seen, and several weeks after the fever had ceased to exist. It was a violent case of yellow fever. No one will admit that the cause of this case was domestic in its origin, or developed by influences native to the town. If the cause of this disease be indigenous to New Orleans, why does it never prevail in the other towns until after it has existed as an epidemic in the city. The violence of the epidemic this year, considered in connection with the known corruption and profligacy of the times, lends, it must be confessed, some importance to the theory, that as civilization progresses, involving social and moral improvements, the disease will gradually disappear. It appears to me that if even in one instance the epidemic in New Orleans or Mobile can be traced directly to a foreign cause, these two cities are as dependent upon importation for the disease, as that they do unquestionably transmit it to all the other towns.

The doctrine that it depends on miasm, heat combined with dryness or with moisture, putrid exhalations from filth in any or all forms, upon organic or inorganic poisons, meteorological conditions of earth and air, as incident to the United States, combined in any possible manner or singly, meets with contradictions, and affords no satisfactory solution of the presence and absence of the disease. Believing in its foreign origin, and knowing the fact that it often delights in its severest manifestations in places remarkable for cleanliness and their general salubrity ; believing that the most rigid observance of hygienic rules, and the freest use of disinfecting agents, are impotent in the prevention of the disease, I would yet advocate as strongly as any one the use of every means to promote cleanliness in the streets and back-yards, alleys and suburbs.

The animalcular origin of this disease is with marked ability presented and supported by Dr. Nott; in which doctrine I would willingly acquiesce, if its distinguished advocate would admit as invariable the foreign origin of the animalculæ. I have long been inclined to the opinion that the time is fast approaching, when most febrile diseases will be attributed, and justly, to a similar cause—to an animalcular origin.

It has been my lot to witness and to pass through, in active professional employment, four severe epidemics of this terrible scourge, and the remarks which I have and shall make are almost entirely suggested by the facts which I have witnessed. It will be readily inferred from what is already said that I regard the establishment of a quarantine around New Orleans as not only indispensable to the safety of the city, but of the whole State; and that it is the duty of the Legislature to pass laws for its establishment and most rigorous enforcement.

From numerous and unquestioned facts, it would appear to me to follow, as indispensable conclusions, that New Orleans and the interior towns, in their liability to yellow fever, hold the same relations; that the disease is not the native product of either; that the cause being foreign, must be imported; that it is innocuous unless it meets a peculiarly affected atmosphere; that this atmosphere is dependent on summer heat of a certain previous duration and on a crowded population; that through the influence of a numerous and crowded population, the cause of the disease may be developed to the extent of producing occasional cases, while the same year it cannot be developed in country towns.

What do we know of the cause of any idiopathic fever? Are we as well acquainted with the circumstances and laws which govern the causation of any of these complaints as we are of those which attend the production of yellow fever? The facts and circumstances accompanying the development of these affections present themselves to my mind with more clearness and precision than those we assign to the development of any idiopathic fever whatever.

The numerous facts already cited appear to me to divest this disease of all the attributes of contagion. I cannot believe, from what I have seen, that the effluvia from sick persons crowded together, however abundant it may be, can per se communicate the disease. I refer to the example of eleven cases in one small house in the country, all of an aggravated character, and the entire exemption of the nurses from the disease. The exclusive occurrence and limitation of this affection to thickly peopled districts and towns and the impossibility of its communication in the country through the direct medium of patients, are attri-

butes which do not belong to contagion. The great characteristic of a contagious disease is a disregard of zones, climates and seasons ; of country and of town. Contagion demands no infected atmosphere; it requires no known adjuvants for its communication ; its action is confined within a narrow sphere or circle, beyond which it cannot travel. Its poison is not self-productive or progressive ; it cannot engender a prevailing distempered atmosphere. In all these respects the poison of yellow fever differs from contagion. There is a resemblance between the contagious and yellow fever, in their exempting patients from second attacks; and yet in this respect the resemblance is not complete, for individuals have been known to have had several attacks of yellow fever. I believe this second attack is rare in persons who continue to reside at the South ; but that a prolonged residence at the North may renew the susceptibility to the disease. One of the severest cases I saw this year was a young man, who declares he had a severe attack in the city in 1847, and who has ever since continued to reside in the South. He was a clerk in a large store, which was closed early. Before the epidemic ceased this clerk returned, unbarred the doors and windows, overhauled the goods, and was thus engaged for two days, exposed to a concentrated imprisoned infection, when he was taken sick and narrowly escaped. Of nineteen individuals, who it is well ascertained had had the yellow fever in former years, who remained in Washington throughout the epidemic, and who discharged towards the sick and dead all the duties which man could perform, not one—though the disease spared scarcely another human being—experienced the slightest indisposition ; a fact strongly in favor of the opinion, that a genuine attack will prevent a repetition of the disease.

I will here introduce the entire chapter, entitled "General Conclusions," from the pamphlet* of the late Professor Carpenter on the Origin of Yellow Fever.

* This pamphlet, entitled "Sketches from the History of Yellow Fever," showing its origin, etc., was published in the year 1844. It may I think with a regard to truth be said to have fallen still-born from the press. It was my good fortune to have enjoyed the friendship of the author, who to a most noble nature and most refined feelings, added all the high attributes and rare qualities of a mind, which, eminently practical in its turn, was indeed strictly disciplined in the rigid rules of philosophic enquiry ; and who, fully imbued with a deep love of his profession, and profoundly versed in all its important collateral sciences, would, had life and ealth been spared him, have successfully labored for the extension of the medical domain. He died in 1848, in the prime of manhood. This pamphlet, presenting a long array of facts, the result of observation and laborious research, while it rebuked the "hesitation of the selfish," scattered to the winds all the cobweb fancies, the hypotheses and sophistries of the imaginative and scientific. No wonder it fell still-born from the press.

"1st. Yellow fever is a disease not native to the continent of America, but of foreign origin, introduced first from Siam, and afterwards aggravated in its type by the importation of the Bulam Fever. [See Historical Notice.]

"2d. No well authenticated case of the specific disease called yellow fever has yet been known to occur on the American continent under circumstances which precluded the possibility of infection, or even rendered it probable that it originated independent of transmission, either by going into infected localities, opening boxes or parcels from infected places, visiting boats or vessels from infected towns or opening rooms closed during the prevalence of an epidemic."*

"3d. Since the introduction of yellow fever into America, it has always existed on some part of its coast. It prevails almost perpetually near the Equator, where the temperature of winter is rarely low enough to destroy the infection; and it is carried by commerce to the countries lying north during the portion of the year between February and November, and to the regions to the southward from August to May. Thus in Surinam and Demarara it is indifferent as to seasons; in Campeachy, Vera Cruz and Havana it begins from February to May; in the United States from June to October; while at Pernambuco and Rio Janeiro it prevails generally from November to May. In this way it prevails perpetually, changiag its place as the seasons vary, visiting the cities as soon as they are filled with fresh subjects, and where commerce offers facilities for its introduction.

"4th. Yellow fever is a disease sui generis and peculiar, and not a grade or type of bilious fever, as is shown by the fact, that as in the plague, measles, small-pox and other specific and infectious diseases, one attack diminishes the liability to and almost exempts from a second attack; while it in no way diminishes the liability to bilious fevers at all. Neither do attacks of bilious fevers of the severest grades in any manner diminish the liability to take the yellow fever.

"5th. The yellow fever is not produced by a crowded population; neither by heat, moisture, marsh air, miasm, filth, nor by any combination and concurrence of these; otherwise it should always occur, when these concur, and should not occur when the particular combination is absent, neither of which we find to be true.

"6. The transmission of yellow fever depends exclusively on inter-

* In large cities the complicated relations of daily life render it difficult to trace up the infection to its source, and it is only in small towns that we can always arrive with certainty at the desired information.

course and commercial relations; any city being liable to infection in the precise ratio of its proximity to and of its unrestricted communication with ports or places where the disease is epidemic. It is from this circumstance that quarantine derives its pre-eminent efficacy in the exclusion of this disease from cities. Dr. Townsend remarks, that "although the imperfection of medical science places the cure of the disease too often beyond our control, and that our means of counteracting its progress are limited, a wise Providence has indemnified us for these losses by putting into our hands an effectual method of totally preventing its occurrence, by shutting out its introduction from abroad. It is left for us to carry into execution what our own judgment must now teach us is the only resort that is left.' 'I firmly believe that the source of the disease can be only cut off by a rigorous code of quarantine restrictions.' [Townsend, p. 228.]

"7. Yollow fever requires for its transmission a moderate summer temperature, a certain accumulation of people, as the crew of a vessel, or the population of a town, city, etc.

"8. Under certain circumstances of population and temperature, the introduction of persons with yellow fever, or of the air from places where the disease is epidemic, will frequently give rise to new cases, and finally to an epidemic of the disease.

"The infection may be conveyed:

"1stly. In boats or vessels which remain at the wharves, etc., in the infected city, receiving and discharging freight, and then closing their hatches upon the contained air, may become the real vehicles of the transmission.

"2dly. Boxes or bales containing goods, particularly woollens, if packed and closed in an infected atmosphere, may convey the infection.

"3dly. The clothes, bedding, etc., which have been used by persons with yellow fever, have been known to communicate the disease.

"9th. Certain ports are almost annually subject to epidemics of yellow fever. These during the summer we should always regard as infected, and during that period we should enforce quarantine against all vessels sailing from or touching at them. Now such rigor against the towns of the United States would be unnecessary, for should cases of yellow fever occur in any of our towns we should hear of it by mail before it could become epidemic in the place, and often in less time than it would be required for a vessel to arrive here from there. But with the West Indies the case is different; for we learn that at the quarantine in New York, the first intimation they have of the disease

prevailing in these places, is from cases of, or death by it occurring on vessels arriving from there ; so that if we only enforce quarantine against those places where the disease is known to prevail, we may have "cargoes of infection" introduced into our cities before the health officer even suspects any place of being the seat of an epidemic.

"10. The healthy state of a vessel's crew is no proof that she may not be infected; for the crew may all be acclimated, while the infection may be sealed up in her hold, or contained in the cargo, etc., and may only exhibit itself after the arrival at a healthy port, and among the unacclimated persons who may visit, or receive freight from her. [See Bayley's letter, Townsend, p. 92.]

"11. Cleansing and ventilation do not always destroy the infection of a vessel. Therefore quarantine, with its precautionary measures is not a sufficient guarantee for the public health. [See case of the brig Enterprize, also case of ship Diana, which introduced the disease into Brooklyn in 1823.

"12. The only measure by which the public safety can be guarded is, to prevent all vessels coming from sickly ports or places from coming above the quarantine ground, whether their crews be sickly or not. Provision should be made, enabling them to discharge and receive freights, safely and expeditiously, and arrangements should be maintained by which the freight so discharged should be delivered to the consignees as soon as the time expires which may be deemed necessary for its perfect ventilation and disinfection.

"13. The principal difficulties against which it will be necersary to guard in establishing quarantine for New Orleans, is presented by the Tow Boats engaged in towing vessels from the mouth of the river. These boats, by communicating with infected vessels, or towing them up abreast, or even at hawser's length, become liable to infection, and in turn become the medium of infecting the city. The fact is established beyond a doubt, that in two of the years during the time the quarantine was in force here, the introduction of the disease was mainly attributable to these boats ; and experience should lead us to provide against similar disasters in future.

"14. It would certainly be safest to prohibit Tow Boats from towing up vessels from infected places at all ; as in the various turnings of their course and shifting of the wind, it is possible that infection would be communicated at even hawser's length ; at any rate whatever the law provides should be most strictly and scrupulously enforced, and Tow Boats should be placed under the heaviest bonds to answer their observance of all the particulars of the law.

"15. An accurate account should be kept by the quarantine establishment of the state of health on board of each of these boats, which should be subject to the same kind of examination as vessels coming in from sea; they should be required to report the occurrence of any case of fever, or any infectious or contagious disease occurring on board; and during the period from the first of May to the 1st of December, these sick should immediately be sent to the quarantine infirmary.

"16. In case of their crews becoming infected with any of the contagious or infectious disorders contemplated by quarantine arrangements, they should be subjected to the same restriction and rules as the other infected vessels.

"17. They should be particularly prohibited from taking as passengers any person from vessels declared in quarantine, or from receiving any freight, box, parcel, or package, from on board of such vessels.

"18. A landing or wharf should be provided for Tow Boats at a point not in front of the thickly inhabited portions of the city."

With regard to the type or character of the disease which prevailed in Washington, it may well be doubted if in malignancy it was exceeded anywhere else. Dr. Bayles, who, on the part of the "Howard Association" arrived amongst us toward the close of the epidemic and saw a good many cases, told me that neither in the city nor in the towns which he visited during the epidemic, did the disease present any greater violence or malignancy than some of thecases he saw in Washington. If not so fatal, it was as malignant, if not more so, than the epidemic which prevailed in Opelousas in the year 1837.

Perhaps the annals of medicine do not present a more violent and malignant form of disease than that which was prevalent in our late epidemic in many of the free mulatto families living in and near the town. In one house I saw five out of ten die. In all these cases, except those that died before the third day, there were seen after the third day petechiæ with a point of pus in the centre, interspersed amongst a thick crop of small accuminated pustulous eruptions; and amongst these, in two cases, were seen large pustules, filled with white, thick, opaque matter, and some as large as a pea. In some of these cases these large pustules were so numerous as to excite an apprehension of a combination of the varioloid with the disease. What was remarkable, we saw in the same neighborhood the almost complete extirpation of one family, and there was in an adjoining family not a single death.

Amongst the blacks, contrary to what has hitherto been observed, the characteristic symptoms of the disease, particularly at the outset,

were generally most violent. In some of the fatal cases there was hemorrhage from the nose and anus, but never, that I know of, black vomit. Black vomit occurred amongst the mulattoes, but it was not so common as amongst the whites.

Of the different classes, as near as I can form a judgment, the mortality amongst the mulattoes was the greatest; next that of the whites; and it was the least amongst the blacks. Of about 400 who were sick in Washington during the epidemic with a fever, the mildest cases of which presented clear evidences of the epidemic influence, there died in our town 70 persons; that is, at the rate of something over sixteen per cent. This great mortality was aided much by other causes than the actual severity of the disease; as for example panic; the want of good nursing; relapses from imprudence in eating; delay in obtaining medical aid; extreme old age in several cases; in a few intemperance; and more particularly drastic medicines and calomel—medicines presumed by some to be required by the severity of the disease, and freely taken without medical advice.

Whatever may be the cause of this disease, the conditions necessary to its existence, and which it is useless here to recapitulate, are demonstrable. Whenever it has affected a person once, it loses its power of affecting him a second time, or the system loses its susceptibility to its influence. The exceptions to this fact are rare; and we see in many, nay in all epidemics, that some individuals may, with entire impunity, inhale the infectious atmosphere. Even in this town, during the late disease, some few persons who never had the disease escaped. In some instances on the second, most generally on the third day after exposure to the infection, the disease manifests itself. I have noticed that the country cases occurred almost always during the third or beginning of the fourth day after the individuals had left town; very rarely later than the third. I have never known one taken after the fourth day. I know not how to explain those examples in town of speedy and of procrastinated attacks of persons equally exposed to the cause of the disease, except on the supposition that the latter, after absorbing it, excrete it from their systems; that the morbific cause, yielding to the resistance which life invests the tissues with, is discharged without injury, perhaps through the kidneys and alimentary canal. The eighteen persons who in former years had had this fever, and some twenty-five others who never had had it, and all of whom escaped this year, were constantly for two months exposed to the infection in every form and under all circumstances. It would be folly to presume they never took the poison into their systems. Then it was excreted from their systems;

and their continued good health demonstrates, that in these cases the poison could not incorporate with and contaminate the blood, affect the nervous system or paralyze the molecular nutritive actions on the remote tissues of the body. As these persons continued to receive it into their systems, so they continued to discharge it. In conversation with many of them, I have been assured that during the epidemic their passages were occasionally of a dark or black color, and sometimes unusually offensive; that they also observed a peculiar disagreeable smell from their urine; sometimes as if slightly scented with garlic or the root of valerian. They all complained occasionally of a strange muscular debility; some of spasms; these probably were the effects of fatigue.

But unfortunately the unacclimated are rarely able to resist the cause of yellow fever; and here, for the purpose of illustration of what remarks I may make on the mode of action of the morbific cause on the system, as well as to present the history of our epidemic, I will endeavor to describe such symptoms as in a majority of the cases marked the progress of the disease to both a happy and fatal result. Having felt no previous indisposition, the patient in the enjoyment of a good and unimpaired constitution, rises in the morning with a feeling of heaviness or weariness; he is not disposed to take exercise; complains of a dull feeling or one of tightness across the forehead; his appetite, though impaired, does not prevent his eating some breakfast; he walks out, but exercise brings no relief; his skin is moist and cool; by and by he has some pain in the head, qualms of the stomach, and chilly sensations striking through his body; a rigor or a chill, varying in intensity and in duration, compels him to take his bed. Within the space of from a few minutes to an hour, the hot stage comes on. The fever is established. The pain in the head, back and limbs is very intense. The whole surface assumes a reddish color. The eyes more or less red; look dull and heavy, and are the seat of a burning sensation. There is nausea, and the breakfast, which from two to six hours previously was eaten, is readily discharged in an undigested state. Abundant perspiration ensues naturally, or is easily brought on. The tongue is moist, with reddish eyes; the taste is much corrupted; the lips and mucous membranes are very red. His thirst persists, but is easily gratified for the moment. He may throw up at the onset some yellow bile. His pulse strong and hard at first, beats 120 in the minute. His bowels are easily moved; urine at first pale, but soon assuming a red color, is discharged in ordinary quantities, and may impart a disagreeable

smell. Restlessness exists, and the countenance expresses suffering and anxiety.

During the second day the leading symptoms may remain unchanged, or present no important modifications. But yet on a close examination the skin will be found to have lost its heat in a considerable degree; the pulse has also lost its resistance; it gives a wavy, rolling sensation to the touch, and is sometimes fuller; it still retains its frequency, and when the case was progressing favorably, at this early period, could be often detected evidences of improvement; the tongue would be found more coated, without increased redness of edges. The pains much abated; eyes more natural; less nausea; increased secretions, particularly of urine.

On or during the third day, when in the midst of our pestilence, our fears were confirmed or dissipated in regard to the result of such cases as I am describing; when tending to a favorable result, a sensible improvement would present itself in the general appearance of the patient; his prostration would be great, but without restlessness or anxiety; he feels more naturally, and will tell you he is very sick, or that he thinks he is better; the eye looks brighter, the complexion less dusky; the body, shrunken and considerably emaciated, feels warm and hard to the touch; his pains are slight or entirely gone, except that of the eyes or forehead, which never ceases before the fifth or sixth day; his tongue is moist; nausea ceases to annoy; the dejections are more natural; the urine is abundant and of a strong odor; he retains light food, as chicken water, beef tea, strained gruel; his taste is improved. With patience and prudence these symptoms announce a speedy and happy recovery.

But if the case tended to a fatal termination, we saw, before the end of the third day, a manifest aggravation of the symptoms. Great restlessness, an indescribable "malaise," a lethargy of the moral,intellectual and sensational functions ensue and go on increasing. Every organ in the body appears oppressed, as if they had been overtasked; if sensible of his danger, he will sometimes manifest a painful vigilance to catch the opinions of his attendants; or he lays in a listless condition, sleeping unless roused, when, with an apparent effort to gather together his mental faculties, and will answer you sensibly and rationally. Again he falls asleep, or he is heard muttering something indicating an unsettled mind; his eyes have an anxious, wobegone expression; his complexion becomes swarthy; a dirty tinge is visible about the eyes and neck, and a dark red streak encircles the neck, extending up to the ears. The tongue is of a dusky red, often dry and smooth; the throat

is moistened by a tenacious, viscid fluid, exceedingly annoying; thirst is considerable, but a sensation of fulness of the stomach makes him afraid to drink. The gums are swollen, soft and red. Vomiting ensues, consisting of the contents of the stomach and mucus, and is very distressing to the sufferer. The bowels are full and doughy to the touch, and discharge frequently small quantities of thick mucosities, or a matter more consistent, like dirt and water mixed. The entire absence of bile showed that the liver had ceased to act. The urine ceases gradually, and no desire to urinate is felt. These symptoms foretell the approach of black vomit, and with it the patient may live for many hours, sometimes breathing out his life calmly and equably and in the full possession of his senses; or with delirium more or less wild; or in deep coma, or writhing with convulsions, he gives up the ghost.

This awful disease, running its course in a few days, is now considered an idiopathic fever; that is, one consisting in functional, not anatomical lesions. The entire absence of structural lesions on dissection, in many most violent cases, the speed with which the disease hastens to a termination, the rapidity of death, the rapidity of recovery—all repudiate the idea of inflammation, and point emphatically to a poisonous impression made on the tissues and organic functions, involving the whole system in disease. The poisonous shaft has sunk deep into the vitals; the sources of life are vitiated; the organs which furnish the aliment of life, are stricken with paralysis of their functions. Let us, keeping in mind the phenomena of this disease, endeavor to establish some theory of the modus operandi of the cause. We have seen that beyond a doubt this cause, in many individuals who never had the disease, as well as in those who have had it, is innocuous in its effects. It unquestionably is introduced into the system of these persons; it traverses the body through its sanguineous current; and without affecting the latter, or that other system, which, like the watchful sentinel, is never slow to utter the cry of alarm whenever nature, in the remotest ramifications of her being, is affected—the nervous system, it passes out an innocuous excretion. With the masses of unacclimated persons the result is different. They absorb the poison. In many instances the health-restoring power of nature successfully for a time combats against the deleterious effects of the poison; but finally this power succumbs. The infection penetrates the system; it attaches itself to, it incorporates itself with, it spends its power on the remote tissues. The molecular movements, the nutritive actions taking place in the remote tissues between the arterial blood and their elements, are more or less interrupted; and in the fatal case which has

been described, there is no proof, there is not a symptom which indicates that they have throughout the disease even unto death been resumed; on the contrary, there is every evidence that they have very speedily been entirely arrested. With and in proportion to the impairment of the action of the nutritive system, there ensues incessantly a derangement of every organ of the body; because the blood is thereby deprived of many elements, and to an extent proportioned to the intensity of the lesions of the nutritive system that are required for the formation and constitution of its natural venous and arterial character; and with the announcemeut of pain in the head and spinal marrow, the energies of the heart are called into high action, and what is called fever, an effort of nature to expel an offending cause, is seen to exist. We see in numerous cases, like the one detailed, not a symptom marking structural lesion throughout the disease, and the nervous system, impaired no doubt in its functional action, appears to me in many instances to manifest less derangement than any system or organ of the body. As the disease progresses with giant steps and in a manner visible to the eye, to a fatal termination, disclosing the deep and fatal hold it has on the functions of life, parching up the tissues, arresting the secretions, vitiating all the fluids of the body, we often see the nervous system apparently unaffected, for it announces no sensational or intellectual alienation; indeed it would appear from some innate force to retrieve its energies, and the mind will be brighter and calmer as death approaches. As said, all the secretions in the body are diminished in quantity and vitiated in quality. If we draw blood from the arm at the onset, we shall in all cases find that this fluid has already undergone peculiar modifications of color, consistency and of coagulability. It has already lost its uniform, dark, venous hue, and it presents a striated appearance, with a mahogany color; being left at rest, it forms a semi-consistent coagulated mass, with a scanty supply of serum. As the disease progresses, we shall find this fluid still more changed; its color is now red; its consistence is much diminished; its coagulability lessened. As it proceeds to a fatal termination the blood continues to present more marked evidences of a departure from its healthy condition. It loses its reddish hue, which is replaced by that of a dirty pale, yellowish color; the whole mass apparently has undergone a complete dissolution or change of character. It sometimes has lost entirely its property of coagulation, or if it has not entirely, it presents no healthy looking serum. In no other disease—in no other condition of the system, have I ever witnessed this yellowish color and dissolved condition of the blood. For my part I do not doubt its existence in all bad and

fatal cases of purely idiopathic yellow fever, Now it is we see the skin presenting a dark white or waxy hue, or a pale yellow tinge becomes more or less visible about the breast, neck and eyes. The patient often presents all the appearance of one bloated by intemperance, and whose system seems oppressed by prolonged dissipation. A heavy load seems to press upon every organ, arresting every function except those indispensable to life itself. The diluted blood finally exudes through the weakened mucous membranes, no longer affording a barrier to its exit.

In the matter discharged from the stomach, mucous flocculi, resembling perfectly pieces of spider's web, of a sooty black, settle at the bottom. The coloring matter increases until genuine black vomit is discharged in more or less abundance. In black vomit, the color is derived from the blood, which, mixing with the contents of the stomach, more or less acid, gives sometimes a black, sometimes a reddish color. In some cases this matter is darker than the color produced by mingling blood with acids. But again, towards the close of the disease, in same cases, as the heart loses its force, and the blood seems hardly to circulate in the capillary tissues, another change takes place ia the color of the blood. It now assumes a dark hue. A few hours previously of a yellow color, it is now discharged from the mucous membranes of a dark hue; and it has seemed to me sometimes to grow darker even on exposure to the atmosphere.

This remarkable phenomenon appears to me to present in the symptoms of the disease an explanation of its production. In the entire prostration of the digestive organs and of the vital organic actions in the remote tissues, the element of carbon, so necessary to the functional actions of all the secretory organs, ceases to be present in the blood. The lungs in the meantime appear to be endowed with a power superior to that they possess in health. The blood receives rich supplies of oxygen. In the progress of the disease we mark the period of stimulation and its gradual abatement; the supervention of entire prostration; the debility; to use the expression, the fatally intoxicated condition of every organ and tissue of the body. The tissues become flaccid, and soft, and swollen; sometimes we feel distinct crepitation, and we discover gas in the swollen blood-vessels. It appears to me now that the superabundant oxygen is forced to leave the blood, or if even previously it has commenced to diffuse itself through the tissues, that now the natural affinity between it and carbon, existing perhaps abundantly in the tissues, is manifested in a free evolution of carbonic acid gas. Entering the blood, the acid restores the venous dark color. I

have come to the opinion, after the closest observation in my power of all the symptoms of the disease and the best reflection I could give to every possible mode of their production, that the effect of the cause of yellow fever is in the mild cases an impediment or interruption, in the severe, a prostration or suspension of the nutritive actions which take place between the arterial blood and the elements or atoms of the remote tissues, in which operation there is a depuration of the useless and effete matters, and a substitution of new vitalized elements; in the matter depurated, the most important and without doubt the most abundant element, is carbon; one which largely predominates in the constituents of bile, and enters into urine to a certain extent, and one whose presence in blood must be indispensable to life. Observe the character of the blood in all of its conditions in the different phases of the disease, its loss of venous and arterial color, of consistence and of coagulability —the diminished and perverted secretions—the long array of fatal symptoms, and it appears to me impossible to avoid the conclusion that all this results, and from necessity, from super-oxygenation of the blood and from the absence of carbon, the indispensable element of most secretions, and the antidote of oxygen in the blood. Other most serious changes no doubt occur in the blood, but we can hardly conceive that any could be more deleterious than such as would necessarily result from the condition I presume to exist.

I have sat and watched at the bedside of many, and I have thought that I could mark the progress of the disease both to a fatal and a favorable result; in some instances silently and insidiously it proceeds; again, with the boldness of an enemy aware of its mastery, it unmasks itself and exhibits the manacles with which it has bound the energies of nature; and then, as if to diversify the intensity of its character and the awful majesty of its power, it attacks some organ or system, and will induce such agony as will beggar all description; and again we think we can see the antagonistic forces arrayed against each other—that of nature, contending with all her might against the enemy—now thwarted in her efforts—finally successful; and during the contest hope and fear alternately prevail, for in it no quarter is asked or given, and the result must be life or death.

Yellow fever, it is true, runs a rapid course; but nevertheless I have satisfied myself that a loss of flesh, a shrinking of the tissues was always a favorable symptom; and on the contrary, a retention of embonpoint, accompanied as it is almost always with a doughy sensation on pressure, was unfavorable. With emaciation the secretions are abundant and hemorrhage rare; and yet under such circumstances we often

see fever continuing for several days; but it is a frank, open affection, which readily yields to rest and mucilaginous drinks.

A Dr. Stephens wrote a book for the purpose of showing (if I remember correctly, for it is now twenty-one years since I read the book) that yellow fever, the autumnal bilious fever, Asiatic cholera, etc., presented but one great pathological condition, to wit, an excess of acids in the blood, or in other words, the absence of the saline elements. The remedies were of course such as were calculated to supply the absent ingredients in the vital fluids. Now the effect of acids on the blood is to bleachen it. The venous blood is supposed to derive its color from the carbonic acid, the result of the combination of the oxygen taken into the blood in respiration, with the carbon received through the chyle and lymph; or more directly eliminated from the tissues in the process of nutrition. Now in yellow fever the appearance of the blood shows that there are no permanent acids in it whatever until after decomposition of solids and fluids commences. If acids are formed during the first days of the disease, they must combine immediately with some base, and form such salts as impart to the blood the red or yellow hue.

My object in dwelling upon the chemical changes in the blood is to call the attention of chemists and physicians to a subject, a full investigation of which will throw a flood of light not only on the disease in question, its cause, pathology and treatment, but will reflect important light upon all diseases, particularly of the class of fevers. We know the symptoms, "the cries of suffering nature" in yellow fever, and the question of importance is, can the chemist enlighten us as to the nature of the changes which the solids and fluids are experiencing during the whole progress of the affection, and knowing, to learn from them the means of counteraction. Is the doctrine of Stephens, which I remember reading with a good deal of interest, and which derives some semblance of respect from the appearance of the blood in our congestive fever and in Asiatic cholera, correct with regard to yellow fever? It has never, that I know, been refuted. Perhaps its ridiculousness has rescued it from refutation. How are we ever to acquire true knowledge of the nature of those diseases in which functional action is alone involved, and from which result diminished or vitiated secretions and impeded or arrested molecular actions, if the chemist shall not come to our aid and disclose the true constitution of the body?—in disease, what healthy elements are absent, what noxious ones present; the excess or deficiency of any; what new compounds may exist. If the morbific causes of these diseases be beyond the reach of chemical analysis

let us yet hope that persevering, systematic and successful investigations will be made on the perverted constitution of the solids and fluids, establishing results or facts which will present the characteristics of fevers; what it is that tends to eruptions, to putridity; upon what depends fever and congestion; the phenomena of continuousness and remissions and intermissions; and which will form a better starting for reasoning on, if they do not explain the nature of causes. Diseases are but effects, and though the cause may be the same, individuals, in consequence of a difference of constitution and diathesis, will present modifications of symptoms, and some more symptoms than another, justly attributable to the same cause. Directly opposite symptoms ought, in my mind, rather to indicate a difference of causes than mere modifications of tissues and systems; and I believe the time is approaching when many diseases now assembled into one group or family, considered mere modifications of a type, having a presumed identity of cause and similarity if not identity of nature, will take separate and distinct places in nosology.

Returning from this digression, it is certainly a matter of paramount importance to determine whether or not there is in this disease an absence or deficiency of carbon in the blood, resulting from an impairment or suspension of the functions of digestion, nutrition and absorption. I will simply state that the diluted condition of the blood, its loss of the venous and arterial color; the diminution, vitiation and arrest of the secretions; indeed all the symptoms appear to me to sustain the ideas I have expressed, to wit, that in yellow fever the great primary derangement is a modification of the nutritive atomic actions, from which result two other elements of disease, persistent in their nature, and which, though but secondary links in the chain of effects, become in their turn the immediate and prolific causes of that long train of symptoms, which, according to their intensity, stamp upon the disease its varied character, in its different periods and forms, and that they consist in—1st, a deficiency or absence of carbon, and 2d, a superabundance of oxygen in the blood. In yellow fever the great primary and persistent morbid elements, the first links in the chain of morbid effects, the immediate and prolific causes in their turn of that long train of symptoms, which give to the disease oftentimes an aspect the most terrible and trucculent, consist in an absence or deficiency of carbon, and a superabundance of oxygen in the blood.

I will now give the history of a few cases, whose leading peculiarities and symptoms and whose treatment are fresh in my memory. The wife of Lelasling, aged about 25, mother of four children, a large

fat woman, about three-fourths white, of a temperament bordering on the lymphatic, complained of general indisposition on the 30th of August. Still feeling unwell, she on the 31st, in the absence of her cook, prepared breakfast and dinner for the family, of which latter meal she partook with a ravenous and insatiable appetite. She ate enormously; four times as much, she told me, as ever before at one meal. Two hours after she was taken with a violent ague. At 7 o'clock that evening she presented the common symptoms of the fever; hot skin, redness of face and eyes, with a dull heavy look, intensely severe pains in the head, eyes, back and limbs. Pulse was not full, but very quick, great restlessness, slight evidences of delirium; tongue clean, with reddish edges; intolerable nausea, with a sensation of suffocation, but no vomiting. Prescribed immediately a small dose of ipecacuanha, which, with a draught of warm water, excited vomiting, in which she discharged an incredible amount of undigested food. Ordered a foot bath; pills of rhubarb, comp. ext. of colocynth and blue mass, five of 2 grains each, to be followed speedily by injections, and 20 ounces of blood to be taken from the back of the head and neck; teas and common drinks ad lib.

September 1st, 6 o'clock in the morning. During the night there was perspiration and purging, and she slept, as was said, well. The cupping was neglected. I found her this morning worse in every respect. Pains less violent, tongue redder, nausea, quick pulse, dry skin, marked disposition to sleep. She was cupped immediately on the back of the head, on the neck and between the shoulders; blood not flowing freely in consequence of obesity and flaccidity of tissue. The cups were repeated in the course of the day. Bicarb. of soda with sweet spirits of nitre, emollient injections, foot baths, and cold applications to the head, etc., during the day, failed to ameliorate a single symptom. During the ensuing night coma with convulsive twitchings of the muscles, occasionally amounting to spasms, ensued, and suppression of urine.

In forty hours from the time of her attack she was in a moribund condition, and in a little over two days she breathed her last. An irresistible desire to sleep, the patient hardly being able to keep awake to answer questions, though when fully aroused the intellect and senses appear natural, is a most unfavorable, if not one of the fatal symptoms in this disease. A full development of the adipose tissue is one of the most unfavorable circumstances in the condition of the patient. The disease is almost always fatal in very fat subjects, according to my experience. The lymphatic or scrofulous temperament, and the venereal

poison in a secondary form, are also very unfavorable. The compli cation of the latter is always, indeed, as far as my observation extends' fatal.

2d. Mr. ———, aged 30, of medium size, very strong and robust, of sanguineous temperament, a working man, occasionally will take a frolic. At 5 o'clock on the 5th of September, was taken, without premonitory symptoms, with a chill. The hot stage soon came on, and with it great suffering. At 10 o'clock at night I found him slightly delirious, with high fever, pulse full, strong and resisting and 110 in the minute. Tongue clean and red, some nausea, excessive pains, great restlessness. The blood looked as if it would escape from the lips and skin of hic face ; eyes were very red, but dull. I immediately bled him to about 45 ounces, with general but moderate relief; ordered a hot mustard pediluvium, warm teas, and a dose consisting of 20 grains of quinine, one and a half of opium and 10 grains of blue mass, to be followed in four hours by a table spoonful of castor oil. He was restless during the night, but sweated freely and was moderately purged.

Early next morning, 6th September, I found him restless, with dry, hot skin, full, strong pulse, great pain in the head, &c. He was bled to the amount of 25 ounces with relief, and ordered mustard pediluvia, warm teas, mucilaginous drinks, with sweet spirits of nitre, bicarb. of soda and injections. In the course of this day the pain in the head returning with severity, he was cupped in the temples with complete relief of the pain.

During the second and third days his condition was far from being satisfactory. Nausea continued to increase ; his restlessness was such that he would go from bed to bed. His complexion became dark and swarthy, his eyes very dull ; he would draw deep sighs, and complain of very distressing sensations in the stomach. His mouth was dry and his throat moistened with a sticky, viscid mucus. Towards the fourth day he began to throw up serous looking matter with black cobweb deposite. The chloride of soda, in half teaspoonful doses, partly diluted, was administered every two hours, and between them small quantities of the carb. of soda, with the occasional addition of some morphine. Brandy frictions and injections were freely employed. He had now also almost complete suppression of urine ; what he discharged being very small in quantity and very red. He bled also excessively from the nose. Black vomit ensued in abundance, and in a genuine form, but under the use of the remedies above prescribed it entirely ceased before the close of the fourth day.

But now appeared other most alarming symptoms; old venereal sores broke out afresh. The scrotum began to redden and swell, and wherever the skin had been broken in other parts of the body it began to inflame. An erysipelatous inflammation succeeded them, and around those believed to be venereal it spread with great rapidity. The sores themselves began about the seventh day to assume a gangrenous aspect, the scrotum to swell more and more, and also to be threatened with gangrene. In the meantime, that is, during the fifth and sixth days, the patient in all other respects appeared to be doing very well. His evacuations had become healthy, the urine abundant, appetite good; indeed he expressed himself as being well, with the exception of his sores, which were painful.

On the eighth day, notwithstanding the use of bark and charcoal, creosote and chloride of soda, red wine, beer, carrot, and vegetable poultices, and the free use of tonics and stimulants, the parts began to mortify and he died on the ninth day of his disease.

Another case, which I did not see, occurred, that was represented as very much like the one given. In both subjects there was no doubt of syphilitic taint; and to it I attribute death in both instances. To the chloride of soda, which was freely given in my case, aided by the free use of brandy in injections and by frictions, I also attribute the entire cessation of black vomit.

3d. M. Daneo, about 40 years old, of robust frame, with great muscular development, of a bilious temperament and excellent constitution, had been absent from town amongst friends twenty miles distant. On the morning of the 5th of September feeling badly, he started for town. He was soon seized with a severe ague, which compelled him to lie for several hours on the ground. He reached home with difficulty, riding on horseback, by 10 o'clock at night and immediately sent for me. He told me he had suffered a great deal and had got home with great difficulty. In fact, on his arrival he had to be lifted from the gate to his bed, for he could not walk. At eleven o'clock at night, about fifteen hours after the chill, his pulse was neither strong nor quick, though full; skin moderately hot; tongue moist but red; great thirst and restlessness. He had some pain in his head and back; he complained of an insupportable sense of fatigue; he felt sore and bruised, as if he had been beaten with a stick.

Ordered an injection with mustard, 20 grains of sulphate of quinine, 2 of opium and 10 grains of blue mass at a dose; mustard foot bath and peach leaf tea, cold or warm as he liked, with other drinks, water

in particular, ad libitum ; also to be sponged with weak brandy and water.

Next morning, at an early hour, found that he had perspired moderately ; his face and eyes were very red, skin dry and hot, severe pain in the head ; pulse about 90, full and strong ; restless, thirst, slight nausea, with the same feeling of fatigue. He insisted on being bled, to which I consented and drew away 35 ounces of blood with marked abatement of pain. Prescribed 20 grains of quinine, to be followed, after free perspiration, with oil, and the common adjuvants. All the medicines seemed to have most happy effects, and he looked and expressed himself much better.

During the third day (17th September) of his disease nearly all the symptoms were satisfactory; very little pain, no nausea, all the organs seemed acting well, but there was that extreme lassitude and feeling of soreness, of which he never ceased to complain.

On the morning of the 8th, the fourth day of his sickness, I found him quite lively ; he represented himself as being well, and indeed after a minute examination I saw no indication for any other prescription than rest, perfect quiet and chicken water. But he still felt fatigued, and as if he had been bruised, and his complexion was of a waxy hue. A few hours later I dropped in to see him, and notwithstanding my great anxiety for him, his account of himself and the aggregate of his symptoms, deluded me with a conviction that his recovery was certain. But at 2 o'clock of the afternoon this delusion was entirely dissipated. The insidious disease could no longer conceal its fatal inroad on life itself. At the door the nurse told me Mr. D. had wandered occasionally in mind since I saw him ; that he slept too much. I found him asleep, and his face bore that peculiar expression, with a falling of the muscles, which indicates the approach of death ; his complexion was more waxy ; his nose, the tips of his fingers and toes were of cadaveric coldness. I asked if he had vomited, and was shown the vessel in which he had vomited black matter. I awoke him; with a marked effort he collected his thoughts and answered me rationally ; he complained of great malaise, an indescribable weariness ; otherwise he said he felt well. All hope for him was fled. A consultation was asked for by a friend. The consulted physician, inexperienced in the disease, ridiculed my prognosis. It was impossible Daneo could live ; but in six hours afterwards, black vomit having increased, the poor man perished. Great soreness of muscles, with a feeling of excessive fatigue, I have noticed in some cases, and I have always regarded as very unfavorable. But in the pre

ceding case I attributed it to the long ride he took on the day he felt sick, after his fever commenced.

Nelson King, small in stature, of a nervo-sanguineous temperament, feeble muscular development, 30 years old, cooper by trade, of steady habits, was deeply afflicted by the recent loss of his wife ; fell sick on the morning of the 8th, presenting all the symptoms of the prevailing disease. He was bled in the beginning, and immediately took twenty grains of quinine. This medicine caused some perspiration with the assistance of foot baths and warm teas, but in other respects its action was evidently pernicious. The brain became bewildered, he would kick the blankets off, his skin was soon hot and dry, his pulse though continued to be full and to beat at about 100 in the minute ; his tongue became dry. I resorted to cups on the back of the neck, mild purgatives, as manna and rhubarb infusion, gave freely mucilaginous drinks, with nitre and soda alternately ; repeated mustard pediluvia ; emollient injections. He was well nursed, but black vomit declared itself on the fourth day. He threw it up for 24 hours ; at first in small quantities, presented in the form of the dusky black flakes of soot, or pieces of spider's web of the same color, sinking in a sero-mucous matter ; soon the whole matter discharged was intensely black, then it gradually changed to a substance more like dissolved incoagulable blood, which finally ceased entirely. During this period the quantity of urine passed was so small as to indicate, in connexion with an absence of much desire to urinate, almost complete suppression of the secretion. In cases which had occurred in my practice already, the common remedy recommended in black vomit, acid chloride of soda, in which I reposed great confidence, had entirely failed, and I was induced to try the creosote. This medicine was resorted to immediately and in drop doses, in four table spoonsful of water, repeated every hour three times. Its effect was most happy, for it relieved the patient of the fulness he complained of in the stomach, changed immediately the secretion of the throat and mouth, retarded the efforts at vomiting, and under its use, with the aid of brandy toddy, brandy injections and frictions after the first three doses being given at more and more lengthened intervals, the black vomit entirely disappeared, urine was discharged and soon in abundance ; his stomach relieved ; then water gruel, and he rapidly recovered.

On the seventh day from his attack, that is on the 14th of September, he was discharged cured. He regained his strength very rapidly. In a few days he was riding about and superintending his business. About

the 25th there occurred a sudden change from very warm to very cool, almost frosty weather. I met him returning home from a long ride late one night, dressed in summer apparel, in frosty weather. The next day, the 27th of the month, symptoms of tetanus came on. In the course of this day they rapidly augmented. Hot fomentations, opium and quinine, assafœtida, &c., were used, in vain. His spasms affected all the muscles, voluntary and involuntary, except the heart, and in four days he died in great agony.

There were three circumstances which I thought imparted considerable interest to this case, to wit, the manifest injury which resulted from quinine, the success of the creosote in arresting black vamit, and the occurrence of most violent tetanus, developed by cold, and to which the previous attack of fever must, in my opinion, be considered the predisposing cause.

Mrs. Hinckley, aged about 30, and Mr. N. Offutt, aged about 45, each of weakly constitution, of impaired or feeble digestive organs, and possessing what may be called the dry temperament, were attacked—Mad. H. on the 19th and Mr. O. on the 24th of September. The disease in each case presented almost identical symptoms. There was in each case constant and distressing nausea, which resisted every remedy used, as soda, morphine, hyoscyamus, peach leaf and cinnamon tea and charcoal, effervescing draughts, chloride of soda, warm and cold fomentations and mustard over the stomach, and ended in black vomit towards the close of the fourth day. Pulse frequent, beating 125 per minute; eyes dull; great languor and occasionally much restlessness and malaise; skin dry and warm but not hot; dull heavy pain in the head, eyes and back. With the exception of 20 grains of quinine given Mrs. H., and cups to relieve its distressing effects, the same routine of medicines was used in both cases; mild purgatives and injections and pediluvia need only to be added to those above mentioned. So soon as black vomit declared itself the creosote was resorted to, in doses of one drop in two table spoonsful or more of water, and repeated every two hours, for several doses.

In the case of Mr. O. the black vomit was speedily arrested. Mrs. H. threw up an immense quantity before it was stopped. Each of these very intelligent patients was aware of their danger, for they believed the black vomit a fatal symptom. They recovered very slowly but completely. Mrs. H. during her sickness complained of a severe pain in her right knee, which finally ceased. After her recovery the calf of the same leg became swollen and extremely painful; œdema and erysipelas ensued; finally pus collected in a diffuse form under the muscles.

She suffered for three weeks intensely. So soon as fluctuation was discovered a puncture was made ; an immense quantity of very unhealthy pus was discharged, and she has rapidly recuperated since. Mr. O. presented a very languishing condition for many days after a cessation of all the worst symptoms. The clean, extremely dry and cracked tongue, and continued perverted taste, indicated a sad condition of the stomach. The subnitrate of bismuth, in 100 grain doses, administered in a saucer of gruel three times a day for two days, was followed by a great improvement in the appetite and digestion and by full secretion of mucus in the mouth, and by it I believe his recovery was greatly hastened.

Mrs. Read, aged 18, brunette, medium size, constitution excellent, of moderate embonpoint, recently married ; had waited a good deal on the sick ; without noticeable premonition was attacked with chill, which soon ended in a hot fever on the morning of the 13th, with all the symptoms in great severity of the prevailing epidemic. Thirty ounces of blood were drawn from her arm, with great relief and softening of the pulse. A purgative of blue mass, extract of juglans and rhubarb, was given, which operated speedily and effectively. Six hours after her attack the pains had returned, with nausea and restlessness. The foot baths, etc., had been freely used. She was again bled to the amount of twenty ounces, with moderate relief ; she fell asleep, and her arm continued bleeding and before any one was aware of it a considerable quantity of blood was lost, but with the happy effect of almost complete relief from all pain. Her nausea was distressing ; all taste completely lost. I gave her brandy and water to sip, and every two hours 10 drops of chloride of soda, to be continued all night unless it should clearly disagree with her. Next day nausea much diminished, general appearance better, eyes brighter ; tongue furred, moist, and redder than in health ; urine was freely discharged ; skin warm and moist ; less restlessness. Ordered the brandy and water to be taken regularly ; occasional injections ; soda powders were occasionally given to promote alvine evacuations. Nothing else was given. She rapidly grew better and better, and though the case commenced with considerable severity, the recovery was the most speedy and happy one I ever witnessed.

Mr. R., the husband of the preceding patient, immediately after her recovery was also severely attacked with all the symptoms marking the incipient stage of the fever. His attentions to the sick had been unceasing by both day and night, of excellent constitution, and accustomed to

labor; he hoped to avoid the disease by means of occasional purgatives, and had been well purged two days previously. At the onset he informed me that his urine was very freely secreted. Prescription, hot foot bath, mucilaginous drinks with sweet spirits of nitre, towels to his head wrung out of cold water, and a mild purgative injection during the day. Under this treatment, which was continued without intermission, all the symptoms, as fever, pains in the head and back, nausea, redness of face and eyes, considerable restlessness, etc., gradually abated, and the patient was able to sit up on the fifth day, complaining only of great prostration. The above remedies were continued because he perspired freely, and because of excessive urinary secretion; with which was hourly perceptible a gradual improvement in the condition of the patient. His emaciation was excessive for this disease; prostration was also most marked. A case of rapid recovery from a pure attack of this disease, is, in a pathological point of view, as interesting as one in which we mark every step the case takes to death. In the one case, as health returns, the secretions are more abundant and natural, and their constituent elements are derived from no food taken in from without, but from the remote tissues, whose molecular movements, once interrupted or paralyzed by the morbific cause, are now renewed with increased activity by the influence of the fever itself, and the elements necessary to secretion are furnished to the blood; and this fluid, by means of the carbon and other elements it receives, is relieved of its surplus of oxygen; and the energies of life now sink to a state of prostration proportioned to their previous intensity of action. It is but a temporary debility; the machine has received no injury. It is speedily reanimated. But in the other case, progressing to a fatal termination, we see from the beginning that a contest exists within the system. All the powers of life manifest an intensity of action and the presence of a formidable enemy. The patient, until stricken down, has been indisposed for hours, or he has cheated himself with the hope that he was well. A friend tells him, hours before the attack, to lie down and rest, for he must be sick, or been indulging too freely in wine, for his eye is dull, his cheek injected; his walk is languid. Already the blood is corrupted, from a diminution of the necessary supplies from within, and from the constantly augmenting proportion of oxygen. Fever comes on; the heart is tumultuously and violently roused; and now the secretions are diminished and vitiated from the start. The powers of life grow weaker and weaker. Every organ appears inebriated; a viscid mucus sticks in the throat and glues together the coats of the intestines. Medicines are impotent. Incipient decomposition ensues; black blood exudes from the mucous membranes, in dependent parts it

stagnates and forms black spots; and death under these circumstances soon closes the scene. I have been told that the lamented Doctor Diggs, after wiping black grumous blood from his lips, observed to his attendants, "this is the matter of black vomit; I know that I am dying, but it is remarkable that I do not suffer, nor feel disease in a single organ in my body."

Two little girls, each aged ten, of the lymphatic temperament, blue eyes, red hair, passed through the disease with apparent rapidity. The daughter of Capt. Marsh, on the fifth and sixth days discharged by the mouth and anus an incredible quantity of pure black vomit. The other, daughter of Mullen, discharged by the bowels a considerable quantity of this black matter. The creosote was administered to both, and for the whole period to the daughter of Marsh, while she discharged the black matter. The prescription was 6 drops to 8 ounces water and a tea spoonful every two or three hours. Brandy frictions and stimulants were freely employed. To one champagne was given. All the unfavorable symptoms ceased, and for two days they both promised to recover, but then an irritative fever commenced, with restlessness, pain in the head, red tongue, hot fever, small frequent pulse, irritable stomach, beating of carotids, delirium, finally coma, and after six or seven hours they perished, having presented all the symptoms of typhus fever.

Mr. B——, aged about 30, of medium stature, dark bilious complexion, intemperate, and for several weeks more so than usual. The appearance of this man, who led a rather solitary life, and seems to have been intimate with nobody, had attracted attention for some days, in consequence of his haggard looks. He had had a chill, with some febrile reaction, several days previous to his attack, for which medicine had been prescribed. He was reading a newspaper when he fell from his chair in a fit, marked with spasms, which seemed to affect more particularly the muscles of the upper extremity and face, and with insensibility. There was some froth in the mouth, which could be opened, but he could not swallow; his eyes half opened, pupils very much dilated and contracting on the approach of the candle. The spasms gradually diminished; pulse at first was very weak, but in a few hours it argued considerable fever; then again it sunk. In the course of about fourteen hours he looked like one in a comatose condition, and died in about 28 hours from the commencement of the fit. I am very sensible of the great imperfection of the above description. I saw the case only twice, in company with Dr. Bayles, who arrived in Washington towards the close of our epidemic, and to whom is due every consideration of res-

pect and the deepest gratitude for his assiduous and successful attention to many sick persons. In the sufferings of this unfortunate man we did not discover the leading symptoms of either apoplexy or epilepsy. The case resembled neither in its general appearances or symptoms, which indicated congestion of the base of the brain and spinal marrow. It was known that he had been complaining for several days, and that the brain and mucous system had but short respite from spirituous stimulation.

Another case, and in my opinion an illustration of the preceding and fully worthy of record, has often been detailed to me by a venerable gentleman, now deceased. He and another clerk were serving a great many years ago together in a mercantile concern in New Orleans during the prevalence of a very malignant epidemic. Late one night he parted from his comrade, who did not complain of being sick. The latter next morning did not arrive at the usual hour at the office, but finally entered in a hurried manner, with his clothes soiled with mud, with a haggard look and injected eye, and approaching his employer, told him that he had left Louisville that morning, and was happy to present to him the compliments of his friends in that city. His appearance inspired great anxiety ; he confessed that his unprecedented feat had greatly fatigued him, and consented to take repose on a bed in an adjoining room.

Dr. Flood, a name familiar to the old inhabitants of the city, was immediately called in. After seeing the patient, he was about leaving when he was reminded that he had made no prescription. I can make none, says he, other than to advise the speedy engagement of the services of the priest and undertaker. In fact the poor man died in a very few hours.

Both of the above cases have their parallels in the history of this disease. For my part I do not doubt that the death of B—— was due to the poison of yellow fever, which had been preying on his vitals for several days ; that the alcohol had retarded the eruption of the painful malady, stimulating his secretory organs, imparting activity to the nutritive actions, and doubly supplying carbon to the blood, thus enabling him to contend for hours, nay for days, against the enemy, which though never ceasing to make its forays on his system, finally overcame perhaps in a momentary withdrawal of the alcoholic stimulus, all opposition, and involved in irretrievable ruin certain portions of the nervous system, which, on the cessation of artificial excitement, were left an unresisting prey to any general morbific influence to which the system might have been subjected.

In this case, the base of the brain—the organs of sensuality and of the animal passions—those organs which make man the mere animal, and the abuse of which make him a brute; organs no doubt predominating in the case, were less able to resist the morbific cause, and the man died foaming at the mouth, with head thrown back, with convulsive twitchings, with an entire loss of the little intellect he ever had.

With regard to the other case, there seemed to exist a mental intoxication, an aberration of mind. His habitual application to business, involving a constant exercise of thought, connected in all probability with a peculiar mental idiosyncrasy, rendered the organs of thought more obnoxious to the cause of the disease, and he manifested derangement, perversion of the mind.

In 1837 a case of peculiar interest occurred in my practice. The gentleman (subject of the case) in moments of relaxation was the gayest of the gay; he was engaged in very extensive commercial business in New Orleans; nor had his house escaped the universal blight with which the whole commerce of our country was at that time visited. His attack was ushered in with an exhibition of gaiety and an uncontrolable desire to dance. This peculiarity ceased in a few hours; indeed it was replaced by deep gloom and despondency.

I feel almost an irresistible impulse to add other cases, which appear to me to merit attention, from the light they throw upon the disease. But I have already written much more than I had any expectation of doing at the start; for the moment I overleaped the province of the mere narrator of events witnessed. I have become involved in the discussion of abstract subjects, and I have announced opinions requiring support and illustration, and which I believe highly important, even though they may not attract the notice of any one. I present these opinions with all humility, but with the satisfaction that in doing so I have discharged a duty; and with the conviction, that if they should attract attention and merit investigation, however visionary they may be, the facts upon which they rest will be found no "phantom of the imagination," but nature's truths.

I must now proceed to a hasty consideration of the leading symptoms and their modifications, as manifested in our late epidemic, and finish with some remarks upon the treatment, or the chief remedies used.

Premonitory symptoms, which in a very large majority of cases were complained of, have been noticed. It has happened to me frequently to beg individuals to cease from exertion, to seek repose and quiet, and to censure many after the disease was on them for not taking to their beds. Such facts are constantly observed in every epidemic of

yellow fever, and they are peculiar to the disease. One of the primary effects of the cause of this disease—an effect already noticed—is a delusive hope, oftentimes indulged in, and manifested frequently in a strange exhibition of gayety, even until the patient is stricken down and forced to take his bed.

The fever sometimes came on very slowly, and you could mark every step it took in its development until it became violent and confirmed; and in some of these cases there was no chill at the beginning. But in nine-tenths of the cases a cold stage, greatly varying in intensity, from simple stretching or cold sensations to agues and rigors, more or less violent, and presenting periods of duration of from a few minutes to several hours, preceded the hot stage.

Fever.—With few exceptions, there was extreme heat of skin at the onset of the disease, with a large and full pulse, variable in frequency from 90 to 125 strokes per minute, and averaging about 110. In some cases it was strong and resisting; in others, though full and frequent, it was soft and yielding to pressure. In a few individuals of weak and impaired constitutions, it was small and frequent from the beginning. The fever varied in duration, in some cases declining at the expiration of 30 hours; in others lasting for three days. In some fatal cases, at the end of 48 hours the fever had entirely ceased, and there ensued that remarkable prostration, which at some period is certain to arrive. The patient for some 24 hours will present a condition of restlessness, with nausea and great malaise, without being sensible of any local suffering, and then a tumultuous beating of the heart, with occasionally throbbing of the carotids and increased heat of skin, with an aggravation of all the symptoms, will come on; and frequently before black vomit or death he will for hours be tranquil and present the most deceptive appearance of recovery. In some cases presenting favorable symptoms, towards the fourth day considerable fever was renewed after it had also almost entirely ceased for 24 hours; but with this fever there were increased secretions and other favorable symptoms, which announced, without disappointment, a speedy recovery. Towards evening there was in many cases an increase of the febrile symptoms, but upon the whole the fever consisted of but one paroxysm.

In the midst of the epidemic, surrounded by yellow fever patients, a medical confrere had a severe attack of fever in the purely intermittent form. I saw him as the hot stage of the second paroxysm was passing off, and his affection presented the symptoms, the general physiognomy of yellow fever. But the fever ceased, and during the intermittence

which continued, as well as I remember, about 14 hours, he was free from bodily pain. After the third paroxysm the fever did not return, and he recovered as speedily as one generally does from intermittent fever.

Though intermittence, according to my experience, is not an element of yellow fever, yet I would not deny that exceptional cases of it might occur. I saw a case which commenced with fever in an intermittent form. With the third paroxysm the disease assumed the complete character of yellow fever, and in its progress, which resulted in recovery, it presented many very unfavorable symptoms, as retention of urine, great anxiety, sighing, an ashy countenance, and as death approached the pulse lost its force and frequency, and medicine for several hours before dissolution ceased entirely to be felt.

In many cases, after complete convalescence and during its progress, the pulse, retaining its fulness, falls in frequency considerably below the healthy standard. I am particularly reminded of three young men, Jews, whose pulse during this period beat, in one but 38 per minute, in the other two 40; otherwise there was nothing remarkable in their convalescence.

Tongue. The appearances of the tongue in this disease are not always to be relied on. In some few cases it did not for some days exhibit any diseased appearance at all; but in a majority of instances it was from the beginning slightly furred or coated in the middle, with its borders and end slightly redder than in health. Even when dry, parched and with fissures or cracks on its surface, it is not always a bad symptom; sometimes in the progress of the disease it becomes smooth and dry, swollen and of a purple color; sometimes moist, then dry. In some fatal cases of negroes it became perfectly yellow. Most patients in yellow fever have a peculiar mode of showing the tongue. When asked to let you see it, and before you have finished, they sud denly protrude it, with a spasmodic jerk.

A spongy, swollen condition of the gums is a common and always an unfavorable, though not necessarily a fatal symptom. It merits the closest consideration on the part of the physician, as the indicator of serious derangement of the mucous membranes.

Symptoms of the Stomach and Bowels.—More or less nausea was almost an universal symptom. In some cases of children, for the first two days the stomach would retain nothing. In cases of grown persons, notwithstanding this symptom, medicines were generally retained. In some instances when the stomach would not tolerate wa-

ter, it would retain medicines. This nausea, always accompanied with a sensation of fulness of the stomach, as the case improved generally diminished and passed away; though in some cases, which resulted favorably as late as the third and fourth day, it continued to distress the patients. These symptoms increase as black vomit approaches, and yet some patients complaining of a deadly sickness and distension of the stomach, would not towards the close of the disease vomit at all. In many of these cases, after death, abundance of matter like that of black vomit flowed from the mouth.

According to my observation, and all that I can learn, black vomit occurred in a large majority of cases.* Death took place in many instances before the morbid condition necessary to black vomit existed: before that tenuity of the blood existed enabling it to exude from the debilitated mucous tissue; from congestion of important organs or from an exhaustion of the vital forces. In many of the cases in which it was not discharged, it is highly probable it would have been found in the stomach on dissection. Certain it is that many of these cases which I witnessed there was presented the greatest possible intensity of disease, a more appalling aspect of irretrievably vitiated solids and fluids than in cases in which it occurred. Some of these cases, running to the seventh and ninth, and one in particular to the thirteenth day, were disease personified—awfully repulsive—terrible to behold. In some of the cases which I witnessed, this matter was thrown up with some considerable effort or straining; but in general it was discharged suddenly and without effort.

In 1839 I saw a patient, a powerful man, without retching or straining, eject, as if it were spouted, a stream of black vomit twenty feet from the bed on which he was lying; then he rose on his feet, walked a short distance, was about falling when he was lifted back to his bed, and died almost in an instant.

Black vomit, according to my observation, occurs in this disease under two distinct conditions. In some of them it is not a fatal symptom; in the other it is the infallible precursor of death—nay, one of the effects of incipient dissolution. In the one condition there has taken place a great change in the blood, as regards color, consistency and coagulation; but as yet there is no decomposition of organic matter. Through the mucous tissues, rendered patulous by disease, the diluted blood passes out, not passively, but in obedience to the vis a tergo, the

* It occurred in about three-fourths of the whites, in about three-fifths of the mulattoes, and in one instance among the blacks, that I could learn.

impulsive efforts of the heart. We see active, lively hemorrhages from the nose and other parts, followed often by great improvement; and at this time we may have black vomit, the result of an hemorrhage in the stomach. In this condition, though imperfectly performed, there is still capillary circulation. At any rate that state of the capillary system in which the blood merely permeates the minute cells, often stagnating in them, does not yet exist. Up to this time the recuperative energies of nature are not abolished. Nature may yet, and she often does, resume her energies, re-excite the forces of life, re-establish the nutritive actions, re-commence operations in her laboratory, and excrete from the remote tissues, and throw into the channel of the circulation the elements which alone can neutralize the morbific influence, and bring back health. But in the other condition, incipient decomposition has assailed both solids and fluids. The forces of life are paralyzed beyond resuscitation. Chemical changes are commencing in the tissues and blood; oxyde of carbon and perhaps other various gases are being produced and in quantity in an increasing ratio. They make the blood black. The mucous tissues, in their tumid and patulous state, cannot contain this blood. Black vomit, under these circumstances, is but an effect of partial death.

In an article many years ago on the epidemics of Opelousas of 1837 and '39, I stated that black vomit was a fatal symptom, according to my observation. Subsequently I received from my friends, Dr. Hill of Opelousas and Mallard of Grand Coteau, a full statement of all the particulars of a case occurring to each in his private practice, of a recovery after genuine and copious black vomit. I regret very much that the histories of these cases, minutely and circumstantially related by their authors, have been mislaid and cannot be found.

Pain, from pressure on the stomach, existed in some, and not in other cases; and sometimes this gastric sensibility was extreme, and I observed it in not only fatal, but in cases that recovered. In two cases, particularly, which resulted in health, this sensitiveness in the epigastrium was so exquisite, that they would not permit that region to be touched. There was no other evidence of gastric irritation. Nor do I believe there existed any. It appears to me like many other similar fevers elsewhere, to be neuralgic in its nature. Some complained of an emptyness; others that the stomach felt folded up, or its coats glued together.

I saw two patients, slightly affected with hiccough, get well. It was present in many of the fatal cases; I should think about one-fourth.

Occasionally it was so violent, that every paroxysm, easily heard at a great distance, appeared to agonize the patient. It is one of the most distressing, and most generally a fatal symptom.

It is not often that a sinking in of the anterior walls of the abdomen is seen in this disease. I saw it occur in a negro man some days before his death, which took place on the thirteenth day from the attack. It may be said, indeed, that almost universally pressure over the bowels imparts a feeling of tumidity, a flatulent or doughy sensation to the touch. They were occasionally the seat of pain, in different periods of the disease, but this was rarely an object of serious attention. Sometimes the bowels were with difficulty moved; but as a general rule the mildest purgatives sufficed.

Castor oil acted very well in this epidemic. There were some cases in which patients had taken drastic medicines, which purged severely, and in which afterwards no evacuations could be procured. They were invariably fatal. The colon may, in such cases, throughout its course so contract its calibre as to be impermeable. Such a case I once saw on a post-mortem examination.

The discharge from the stomach and bowels varied in its nature. In the beginning there was occasionally thrown up bile, that is, a yellowish fluid, thin and containing much mucus; but there was always, as far as I observed, an absence of that bright yellow color, of that peculiar bitterness of taste, and of the peculiar consistence and general physical appearance of true, healthy bile. It was but two days ago that I called the special attention of an old and esteemed physician, Doctor Acher of Opelousas, to this peculiar matter. Some few hours after her attack—quite a servere one—this matter was puked by Miss T. It was a pale yellowish fluid, which on casual examination would have been pronounced pure bile by every one. It was discharged into a purely white vessel. Shake it as much as you pleased, it left not a tinge on the side of the vessel. The Doctor examined it minutely, and decided that the most important constituents of bile were very imperfect or entirely absent in it. He emphatically denied that it was bile. The departure of this fluid from the healthy appearance of bile varied in extent in different cases. In the above case its vitiation was complete.

In the beginning of the disease the first alvine evacuations often indicated the presence of healthy bile, but purgation soon put an end to it, and then we saw sometimes yellowish evacuations marked by the presence of some imperfectly elaborated bile, or matter discharged without any appearance of bile whatever. I am forced to the conclu-

sion, that in a violent or malignant form of yellow fever the elimination of pure healthy bile from the liver has never been seen, and is an impossibility, which results from no nervous derangement, or structural lesion of this organ, but from the simple fact that the elements in the blood required for the elaboration of healthy bile—and in particular carbon and its compounds—are deficient or entirely absent.

Dark brownish, consistent evacuations, I regarded as almost the certain harbinger of a healthy convalescence. The passages were sometimes very copious and very offensive, which last circumstance was regarded as favorable. When destitute of smell, or having the odor of macerated flesh, just before putrefaction, when thin, half mucus or watery, of a clay or ash colored hue, or appearing like the washings of meat, sero-sanguinolent, or of a black, fuligenous appearance, or looking like black vomit—in all these cases they were very unfavorable, and if connected with other bad symptoms, fatal. I saw several patients, who during convalescence passed thick, tarry matter, apparently with advantage.

Urine.—In some few cases this fluid presented no remarkable change in color or quantity. In two instances particularly it was discharged in very increased quantities, and with the happiest effects. In one of these cases it was redder than it is naturally. As a general rule I believe it was diminished in quantity and much reddened in color. In the case of a child it was also discharged in very considerable quantity and for some time involuntarily. It was of an orange color in this case, and when shaken in the vessel, left on the sides a yellow tinge and stained the linen of the same marked color.

I saw one case of retention of urine from the beginning, in an individual of a bilious lymphatic temperament, of impaired constitution. It gradually diminished in quantity until the catheter proved complete suppression, and he died in the beginning of the fifth day of his disease with black vomit. He took of his own accord, in the first eight hours of his disease, 60 grains of quinine in two doses.

I must regard retention, as well as gradually diminishing secretion of urine, very unfavorable. Suppression of urine is in my opinion a fatal symptom. It may result from a lesion of structure ; but dissections rarely showing such lesions, we are bound to look to the general symptoms of the disease for an explanation. And to what else, after a due examination of them—of the diseased blood, and vitiated and diminished and arrested secretions—than to an absence in the blood of the constituent elements of urine ?

Hemorrhage.—This symptom, in its connexion with black vomit, has already been noticed. The great tendency of the morbid condition in this disease is to hemorrhage. In the mild cases, in those in which, according to my views, the morbific cause has merely modified the nutritive functions, and in which nature, on the accession of fever, resumes, often to an excess, her chemical nutritive actions, we do not see hemorrhages. Whenever these actions are imperfectly performed, tardily resumed or arrested, the diluted blood is discharged through the diseased mucous membranes. This hemorrhage is of itself an evidence, not only of an increased liquidity of the blood, but also of diseased mucous membranes.

Bleeding from the nose occurred in a large number of cases. In very many instances it was decidedly beneficial; when it co-existed with a red face, injected eye and pain in the head, I have seen it as late as the fifth day dissipate these symptoms and greatly comfort the patient. This symptom occurring from the nose, mouth or anus, with evidence of a more or less active capillary circulation, and without black vomit, seems to exude from the membranes, has assumed a darkish aspect; when pressure on the skin leaves a pale color, which gradually ceases; when it is accompanied with a waxy color of the skin or swarthiness, and when preceded by great nausea and great but ineffectual efforts to vomit—we may regard hemorrhage as a fatal symptom, and be prepared for copious discharges of black vomit.

In the case of a lady 55 years old, of considerable severity, within the first 24 hours there ensued a considerable hemorrhage from the womb. She had ceased to menstruate since six years. Within a few hours the discharge ceased and her symptoms were aggravated. I bled her to the amount of 25 ounces. The hemorrhage was resumed and flowed with increased abundance, and she recovered in a very happy manner, with the aid of a mild purgative, mucilaginous drinks, a foot bath, and nothing else.

In one young lady, who had a very violent attack, on the third day a copious hemorrhage from the womb commenced; with it the symptoms improved and she recovered. Her period was not expected for a week. In another young lady, who was attacked five days after her period, there ensued a sanguineous uterine discharge.

Abortion occurred in two cases; one in the seventh, the other in the eighth month of pregnancy; both children were still-born and the unhappy mothers perished. A relapse, resulting from excessive imprudence in the one case, and tampering with "No. 6" in the other,

brought on these accidents and death. In many other instances pregnant women passed safely through the disease.

Skin.—It is generally red and hot. In some lymphatic or scrofulous persons, in the first days of the disease, the complexion is of a pale white color, which improves as the fever diminishes. In some persons of the sanguine temperament the blood appeared as if it would burst out through the pores of the face. Such persons are peculiarly subject to hemorrhage. Perspiration, except in some few cases, and those mostly children, was easily induced; and when easily maintained I regarded it a most favorable symptom. From the secretions, etc., of the patients there was always observed a peculiarly disagreeable smell, hard to describe and an ashy complexion; one of a waxy or greenish hue, with viscid sweat, a death-like coldness of the end of the nose, fingers and toes, were bad symptoms—sometimes accompanying (and then fatal) black vomit.

In a majority of the fatal cases, a yellowish tinge, more or less evident, was visible in the face, neck, breast, and sometimes in other parts. After death I was told the body, in a very large majority of cases, became yellow; in some few it was of an unnatural pale white. The yellow appearance was totally unlike the jaundice, as seen every day under other circumstances. It was not the dark yellow of common jaundice; it was of a pale orange tint. I do not believe it was the result of absorption into the blood of secreted bilious matter. Neither the symptoms during life, nor the scalpel after death, reveal the slightest evidence of a secretion of bile in these cases; or if they do, most rarely.

In those instances in which I saw the yellowish matter discharged during the first one or two days, and which ended in death, there was no discoloration of the skin during life. I allude to two individuals particularly, and I do not know whether they turned yellow after death. If the jaundiced appearance depends on reabsorption of bilious matter, why does it so often never appear until after death, and then as it were instantaneously, and in quantity sufficient to impart the peculiar tinge to all the surfaces within reach of vision?

Though clearly inferrible from previous remarks, I will nevertheless express the opinion that in this affection, at its onset in all severe attacks, there existed more or less imperfectly constituted elements of bile in the blood, from which was occasionally eliminated a very imperfect bile; that in proportion to the energies of the nutritive system and correspondent absorption furnishing elements to the blood, would the bili-

ous secretion be less vitiated; that in the arrest of all functional action of the liver, as often unquestionably exists at an early period of the disease, these imperfectly constituted elements of bile in the blood, either find an exit through the kidneys or internal mucous membranes, or they remain in the blood, and are probably deposited in the skin at that period of incipient decomposition which precedes death, and immediately after death are separated from the blood, staining with a yellowish deep tinge the whole periphery of the body, presenting, except sometimes in isolated portions, no previous discoloration at all. I express opinions founded on general observation. There are cases in which, after the fever has ceased, and nature has resumed healthy action, these elements of bile are deposited in the skin, producing a universal jaundiced complexion. The liver and kidneys refusing them they are, as it were, excreted from the blood chiefly in the cutaneous tissue.

In three fatal cases of negroes, young and remarkably robust, of the bilious temperament, the conjunctiva, mucous membranes, every tissue, as far as the eye could determine, seemed completely injected with a dirty yellowish coloring matter. The tongue, in its entire surface and substance presented this color. Every tissue and fluid of the body was changed in their very composition. The principle of life alone existed. One died on the sixth and one on the seventh. The third died on the thirteenth, and this condition in his case originated during a relapse.

Nervous System.—If a full exercise of the functions of mind, of will, of sensation, of respiration, of voluntary motion, be indicative of a healthy condition of the nervous system, then in a great many cases, from the beginning of the disease unto death, this system would appear to continue in a state of integrity. But whatever may be the peculiar derangement it may sustain, dissections show that it and the membranes are as little liable to structural derangement as any other organs of the body; indeed the great nervous centres less so. Chemical changes, and black vomit, the last links in the lengthened chain of morbid phenomena will very often ensue, without any manifestation of nervous derangement whatever, except of that function which is necessary, or may possibly be necessary for healthy action of the nutritive function, or for its restitution. It is indeed passing strange, if a primary affection of this system be the fons et origo of the physical and functional changes which constitute the essential character of this disease.

But how the nervous system or portions of it should be so deranged in rare cases as to give rise in some instances to delirium, at different periods of the affection; sometimes to coma towards its end; occasionally to nervous twitchings and even convulsions, and how at the onset the most atrocious and speedily fatal convulsions, does not appear strange; rather is it remarkable, considering the peculiarities of temperament and idiosyncrasies, natural and artificial, that these nervous phenomena are not more frequent. Such nervous derangements ought not, it appears to me, to be regarded pathognomonic, but exceptional symptoms of the disease. How often did it happen that our most painful apprehensions were excited by the very absence of all those symptoms which announce suffering of the nervous system; nearly all of its functions appeared intact; yet the patient was perishing. Is it a vitiated innervation which corrupts the blood before the fever is established, which produces the ever varying modifications in its character, which diminishes and vitiates the secretions, which exsiccates the kidneys and liver, which finally decomposes the living tissues, and discharges tho diluted blood through the mucous membranes, while in other respects there seems to be no lesion of the nervous system. That in the nervous tissue there is derangement of the molecular nutritive actions, as there is in all the tissues of the body, admits, it seems to me, of no doubt; but the role which this system plays throughout the affection, seems to me to reject the doctrine of a primary vitiation, and to divest it of all claims to a serious consideration in discussing the pathology of the disease.

Treatment.—The history of the treatment of this disease derives, I fear, very unimportant accessions from observation of, and experience in, our epidemic; already but a bundle of strange and contradictory recipes, it has not, it is to be feared, ceased to contribute to the disgrace of medical aid, or at least to disclose the profound ignorance of the profession, as regards the nature of the disease, and our inability to counteract its effects and oppose its march. It may without a fear of contradiction be asserted, that notwithstanding the admitted necessity of medicine and counsel, there is not in the catalogue of grave affections one which can present a more varied treatment, or one which really demands less medication than yellow fever. Mercury has had its day; but it is now remembered only in connexion with its victims. A few years since and quinine was enthroned high above its compeers; and it was an act of extreme temerity even to question its infallible supremacy in the disease. After a short reign it has been deposed;

but the throne is left vacant. Blood-letting boldly practised, active emetics, purgatives, etc., have had their advocates and their opponents; the former lauding their beneficial effects, the latter condemning them as dangerous.

The fact appears to be that this disease, though produced by a specific cause, and presenting in its genuine form, in my opinion at least, specific lesions of tissue and of function, can, in our imperfect knowledge of its nature and of animal chemistry, be opposed by no specific remedy. In idiopathic fevers, it appears to me, that recovery can only be promoted by a medication which exerts the special influence of aiding and abetting nature, in her efforts to restore all the functional actions, the arrest or modification of which is the first link and fruitful cause of all the morbid effects which make up the disease ; and hence perhaps there is a great error on the part of many in assigning to medicines an inherent immediate action for good. We have no remedy which neutralizes or eradicates the morbific cause. In truth we have not the least conception of the causes of idiopathic fevers. Does quinine neutralize and destroy the miasmatic poison which we imagine to be the cause of intermittent fever? or does it aid nature to restore functional action and to excrete it from the system ? And thus it is that in regard to yellow fever, a disease, often in its severe form cured solely by the efforts of nature, and it is to be feared more often aggravated than benefitted by medicine, we witness a great variety of opinion as regards the effects of remedies, as well as the great reputation which certain medicines have acquired in consequence of favorable circumstances.

Emetics.—Having witnessed frequently in this disease a remarkable proneness to gastro-intestinal irritation, I resorted to the use of emetics only in the beginning of the disease, and only when the stomach was surcharged and oppressed by offending matter, as undigested food or muco-bilious secretions. I used indiscriminately the ipecacuanha, warm water sometimes alone, sometimes with salt and mustard, or with one separately. It was an easy matter to procure free emesis with any of these substances. A tumbler of water with half a tea spoonful of mustard and the same quantity of salt, or with a tea spoonful of either, evacuated the contents of the stomach readily and did not tend to purgation.

In consultation with another physician, tartar emetic was given to one patient, a fat subject, presenting under any treatment a poor prospect of recovery. Hypercatharsis ensued and the patient died. A

grain was given every ten minutes; after the second dose he puked copiously; the patient threw up yellowish matter in abundance, and commenced purging without a third dose or any more being given. From its effects in this case, and from what I have known of it in other cases, I think it ought never to be used under any circumstances in this disease.

Purgatives.—A mild but efficient purge seems called for, and I always gave one so soon as perspiration was established, or combined it in the beginning with quinine. When the stomach would retain it, I still used the old remedy, an infusion of manna and rhubarb with a small addition of magnesia; but when irritable, as it was in a vast majority of cases, a dose consisting Ext. Juglans, of Dandelion, Blue Mass and Rhubarb, each 5 grains, made into pills, and repeated when necessary, acted as a pleasant and effectual purge. When I desired more speedy purgation, I added to the above 5 grains of Comp. Ex. of Colocynth, and never saw drastic effects from it.

According to the demand for an action on the bowels, injections were used; sometimes purely mucilaginous; again, of molasses and soap and water; and again with mustard, a tea spoonful of which in warm water was certain to evacuate the lower bowels.

In the progress of the disease purgatives were sometimes required; and of all medicines, a half table spoonful of castor oil, with a tea spoonful of brandy, operated most speedily and gently. In some cases of tardy convalescence, and in those accompanied with yellowish skin, it was often necessary to administer blue mass and comp. rhubarb pill; and when, as happened sometimes, the dejections were black and tenacious, it was necessary to use castor oil or comp. ext. of colocynth.

Frequent, thin, whitish, or dark colored evacuations from the bowels, occurring at any period of the disease, is a very unfavorable symptom. In the use of emetics and purgatives, beware of gastric and intestinal irritation, for in yellow fever there is undoubtedly a tendency to it; and with it urinary discharges rarely continue long.

Opiates.—In the severe epidemic of Opelousas in 1839, having an abundant supply of pure Thridace, or Lactucarium, obtained for a special purpose in Paris, I used it in every case, and with such happy effects as seemed almost to entitle it to the character of a specific. Certainly I saw in its use no injury inflicted on the emunctories, but all the other happy effects of opium. But here it should be observed that the

fever of '39, though intensely violent in its incipient symptoms and accompanied with pure black vomit in all fatal cases, should rather after all be called a self-curing disease, for with a full bleeding at the beginning, a mild purgative, a foot bath, and a good dose of Lactucarium ninety-seven in every hundred got well, and speedily. In the late epidemic I had no Lactucarium. Occasionally I used Hyoscyamus and Dover's Powder; sometimes Morphine in a solution of Soda. I used these preparations only when Opium was imperiously required, sometimes with good effects, again without any noticeable improvement in the case.

Quinine.—I used this remedy in a great many cases in doses varying from fifteen to twenty-five grains in grown persons, repeated sometimes twice or three times; in some cases uncombined, in others with from one to two grains of Opium (with ten grains of blue mass) and again combined with both opium and blue mass; and I cannot attribute a single case of cure to this unaided remedy. In almost all cases it produued a relaxation of the system and copious perspiration, but it did not effect an arrest or abortion of the disease. In some instances it affected the brain in a most distressing manner, for the relief of which cups, cold applications and foot baths were impotent. It is very true a great many patients who took quinine got well; but a good many died.

It must be borne in mind that in Washington the type of the fever was of a most malignant character, presenting in some cases, in the first days of the disease, typhoid symptoms; and in many cases and in blacks particularly, terminating in the pure typhus of old authors. In '37 the fatality of the epidemic in Opelousas was horrible. Of the first fifty cases I do not believe five got well. As already remarked, that of 1839 was perfectly manageable. The mortality did not exceed four per cent. In 1842 it was again very fatal. In neither of these epidemics was quinine used in heroic doses. In the late Washington epidemic it was employed, and in many cases its strongest advocates could not have made any objection as regards the quantity used. I saw it given to the extent of 30 grains. If in my experience I cannot concur in an indiscriminate recommendation of the remedy, neither will I condemn it. I can well conceive that in the Opelousas fever of '39, as many recoveries would have taken place from the employment of quinine, as actually did take place without it, and that the results would have been confirmative of those subsequently obtained in New Orleans. But in the late epidemic, neither quinine, with all the adjuvants

that medicine could muster and skill apply, nor any other remedy as yet known, could have warded off death in all cases. It was inevitable. To say that quinine would not cure this disease, that is, cut it short, onght not to be construed into a condemnation of the remedy. Upon the highest authority its happy effects in the fever in previous years has entitled its employment to the epithet of the "abortive treatment." The resumé of my experience with it amounts to this—that in our late epidemic it did not cut short the disease; that a majority of those who took quinine recovered; that some who took it died; and that in a few cases it exerted a decidedly injurious influence; and it will be justly inferred that this resumé would be applicable to all remedies that were employed.

Adjuvants.—Under this head I will consider a large number of remedial agents, which, though commonly regarded as auxiliaries, merit rather in my opinion to be considered the heroic remedies in the disease. They are, mustard pediluvia, warm teas, mucilaginous drinks, cold or warm, rendered alkaline and nitrous; injections, purgative and emollient; warm and cold frictions, and fomentations with cloths wrung out of hot brandy or whiskey and water, all over the body; weak brandy toddy and injections; the use of "eau sedative" and chloroform locally, etc., and injunctions of perfect quietude of body and mind; the recumbent position should be added. How many grave attacks of yellow fever in every epidemic, and in the practice of every physician, have resulted in the most happy recovery from the employment of only the above means, used in part or in whole? For my part, I could fill a volume with the history of such recoveries.

In our ignorance of the nature of the cause of this disease; in the utter absence of any theory, whose plausibility has entitled it to more than an ephemeral reputation, of the mode in which this cause acts on the system; without any light upon the modification of the solids and fluids, derived from chemical analysis, it might beforehand be confessed that medicines, according to the circumstances, and particularly the period of the disease in which they were applied, and more especially according to the type of the fever prevailing, would be followed by different effects, that for example emetics, purgatives, quinine and blood-letting, while under certain circumstances they would be followed by happy results, under other circumstances, or in a different type of the disease, they would exert no salutary effects. And such, in truth, is the history of the effects of the leading remedies in yellow fever. Hence

in my opinion it follows, that in the present state of our knowledge of its pathology (by which I mean not only the mode in which the cause acts, but the nature of, and relations between the effects produced) and the uncertainty of the effects of potent agents employed in the disease, it is most natural to place our chief reliance in those remedies termed "adjuvants," and to promote their favorable action by the use of an emetic, a purge, quinine or venesection, as may be indicated in the particular case, or which in the peculiar type of the epidemic experience may sanction.

By some writers it has been questioned, whether perspiration in this disease was beneficial or not. They have witnessed recoveries which were not accompanied with perspiration, and they have seen death occur notwithstanding the most copious sweating. For my part, I have always regarded this symptom, whether occurring spontaneously or promoted by foot baths and common teas, and frictions, etc., as of favorable augury in the disease. With it the sufferings of the patient almost always diminish, the burning heat ceases and the pulse grows softer. In many cases the symptoms manifest such evident improvement with the perspiration, that the physician feels justified in prescribing only such remedies as tend to encourage it, contented with the proof manifested in the improving condition of his patient, that nature is resuming those functions which have been modified or impeded by the action of the morbific cause. Mustard or stimulating foot baths, besides promoting perspiration, oftentimes, by their revulsive effect, greatly relieve the head. Warm teas, as orange leaf, sage, black teas, etc., are generally agreeable and conducive to perspiration; and for this purpose a glass of cold water will frequently answer better than any thing else. As the fever begins to abate, and the period of debility approaches, nothing is so soothing and grateful as brandy or whiskey frictions, with occasional injections of either, properly diluted with water. From both patients derive great benefit. I can entertain no doubt of the happiest effects from the frictions from the beginning of the disease; the liquor in the early stages of the disease should be only more diluted. It is I believe a singular fact, that in yellow fever the use of alcoholic frictions, injections and drinks may be pushed to a greater extent with impunity than in any other disease—nay, than in health. Towards the middle and decline of our epidemic I made a more free use of brandy and good whiskey externally and internally, and always I thought with great advantage to the sick. The case of Mrs. Read will be remembered. Brandy was given to relieve nausea; its happy effect induced its continuance. She took a much larger quantity of it

than she could have taken in health; its effect was soothing, not stimulating.

The late Dr. Harrison records the case of an Irishman,* who evidently cured himself with whiskey. He did not see him until the fifth day after his attack, which was a severe one, and he was assured that the patient, previously to his visit, had taken nothing but free potations of whiskey. He recovered. Cases analogous to the above have been observed by almost every physician of large experience in this disease. I could cite many instances in which I gave brandy freely from an early period of the disease, and particularly of individuals who indulged in it freely in health. Spirituous drinks, taken with moderation, and a generous diet, constitute, I believe, in the opinion of the experienced, not only the surest prophylactic against the disease, but furnishe to the tissues that vigor of recuperative action favorable to recovery, and the loss of which, entailed by the strictest diet and abstemiousness, has consigned to the grave more talent and virtue than any other auxiliary employed in the service of this fatal disease.

An intelligent and honorable gentleman of this town, a few days ago gave me the following statement; in its truth I put full trust, but give it as a second hand fact for what it is worth. He had the facts directly from the captain of the vessel:

Captain Genish left Boston in 1837 with a cargo of flour for Matanzas. On his arrival in port he found the yellow fever prevailing, and he was prevented from discharging his cargo by some temporary restrictions which the Government had recently imposed on such articles as his freight. He had not long been in port before the epidemic seized on his crew, consisting of seven sailors and first and second mates. He employed the best medical aid and yet his seven sailors perished. The survivors, the captain and two mates, agreed to dispense with medical aid, and to treat each other in the event of their falling sick; and the treatment determined on was the free use of ice to the head and brandy and water by the mouth. They all three fell sick and got well under the above treatment. The great confidence I have in the relator is my reason for citing this case. But it is not more apposite in the illustration of the nature of the disease than many other analogous cases fully known and proven. But there exists in this disease very often such irritability of the stomach or tendency to it, that nothing stimulating can be taken. Whenever the stomach tolerates it, I believe, properly diluted, it will prove beneficial. I attribute the

* It is quite likely that what is good for an Irishman would be equally good for an Englishman.

good effects of alcoholic frictions, &c., to absorption; to the carbon entering the blood and combining with a portion of the oxygen of which that fluid contains a superabundance, leaving the remaining elements of the brandy to form water, of which the tissues stand in so much need; and thus it is that I explain, not only the impunity with which whiskey in some cases may be drank, but its probable good effects, and the marked amelioration which always results from alcoholic frictions. How muchsoever in violation of all previous theories and teachings, I revertheless feel a conviction that all carbonaceous substances, not poisonous in their nature, but presenting the carbon in an innocent combination and readily absorbable, will prove eminently serviceable in the treatment of this disease; and that if in the dispensations of an offended God the country shall again be swept over by this besom destruction, that they will merit the attention of the profession.

Alkaline and Nitrous Drinks.—As a general rule, the supercarbonate of soda, in small doses, with the addition of sweet spirits of nitre, at intervals more or less long, during the first two or three days of the disease, were given, with the view of correcting acidity and promoting the urinary secretion. The chloride of soda (Labarraques' solution) merits, in my opinion, more than a passing notice. I saw in 1839 a young man, of the nervo-bilious temperament, get well under the use of this preparation alone. I saw him for the first time on the fourth day. He presented a strongly marked ataxic condition, a jaundiced skin and hemorrhage from the mouth, nose and anus. The chloride of soda was administered by the mouth and anus, and contrary to all expectations he got well.

In many cases in our late epidemic it arrested nausea when nothing else would. I gave it in doses of from 10 to 50 drops in as much water as the patient would drink at a draught, and repeated it every one, two or three hours, according to circumstances. In two cases the black vomit yielded to it. As soon as nausea ceased or was greatly diminished, the intervals between its administration were lengthened, and the medicine finally suspended.

Creosote.—This preparation, as has already been stated, arrested black vomit in five cases, three of which entirely recovered; but in the other two cases, both children, after an apparent improvement in all the symptoms, a typhus affection ensued and destroyed life. The creosote is the only remedy that I know of that will, by exciting, heal these secretions, relieve the patient of that distressing and unfavorable

symptom, which consists in a thick, viscid, glutinous matter in the mouth and throat, and which excites a constant but ineffectual hawking, and is always accompanied with restlessness, more or less dryness of skin, &c. In such cases the creosote often arrests nausea and that sensation of distress and fulness of the stomach—both of which are apt to be present—acting like a charm.

In some cases the creosote and chloride of soda were given in alternate doses, every one or two hours. When the condition of the patient is hourly deteriorating, with such spmptoms present and declaring themselves, as a swollen, clean, red, more or less dry tongue, a glistening eye, but dull look, a swarthy complexion, a lowering, anxious expression, increasing restlessness, pains in the legs, a heaviness in the epigastrium, sometimes a painful and exhausting straining to vomit, diminished and vitiated secretions, dry skin or clammy sweat—I know of nothing in which I can repose so much hope and confidence as in half drop doses of creosote in as much water as the patient can conveniently drink at a time, and repeated every half, every hour or at least longer intervals, according to circumstances, aided with repeated and continued frictions and injections with strong brandy and water. The first two or three doses are apt to be thrown up; but nausea and all the horrid train of symptoms mentioned will often soon disappear. But then the treatment should not be stopped; the remedies should be continued at longer intervals, according to circumstances. The phosphate of lime was a remedy unknown to me, until I had the pleasure of meeting with Dr. Bayles of New Orleans, late in the epidemic. I saw it prescribed by him under unfavorable circumstances and with happy effects. In a case now under treatment I gave it on the third day of the disease, with a view to relieve some nausea and gastric distress, in six grain doses, repeated every four hours, with a good result.

Cupping and Blood-letting.—In many of the cases, after practising venesection, some blood was drawn by cups. In nearly all the cases in which no blood was drawn by bleeding, it was drawn in quantities more or less considerable by cups. The peculiar suffering complained of in the head, eyes and back, strongly indicates the necessity of this therapeutic agent. Upon the whole I am inclined to think it was beneficial, its good effects being more marked, when it was employed early in the disease. In some cases, though practised to considerable extent, it failed entirely in affording any general or local improvement of the patient, Again at the onset of the disease, and particularly in children, I saw it dissipate pain and delirium as by a charm.

promote perspiration and apparently exert a powerful salutary effect upon the system.

With regard to blood-letting, I am still a strenuous advocate of it in particular cases and when practised in the first hours of the disease. To doubt its beneficial effects, nay its indispensable necessity in some cases, would be to doubt one's senses; to disregard the most clamorous demand for its employment. I repeat that I am not an advocate for bleeding in this disease, except at its beginning, and rarely after the first 24 hours, believing that when the loss of blood may after this period be required, for any local complication or symptom, cups would be preferable to the lancet. In cases of a cachectic or scorbutic habit, of the lymphatic or scrofulous temperament, in individuals whose constitutions were impaired by intemperance or by chronic disease or old age; when the pulse was small and quick I never resorted to the lancet. Nor in cases of robust constitutions and sanguine temperament, marked by a free or excessive action of the kidneys and abundant perspiration, would I use or advise the lancet. As examples illustrative of my views, I would refer to the cases already detailed of Mr. and Mrs. Read. The lady is young and healthy and of a constitution of ordinary vigor. I saw her early after her attack, suffering with much pain, highly injected face and eyes, &c. I bled her and repeated the bleeding soon after, and her arm again bled considerably in consequence of a derangement of the bandage. Mr. R., of sanguineous temperament, of most robust constitution, was not bled at all, nor even cupped; though he presented all the symptoms and his pulse was soft and full, his skin perspiring well, and the urinary discharge superabundant. It was an interesting case, and every three hours, at often repeated visits, it was manifest to me that nature was all sufficient to accomplish her work of rapid recovery.

The case of Mr. Graham and wife will illustrate this question of blood-letting. Mr. G. fell sick during the night of the 7th of September; his temperament partakes of the nervous, the bilious and sanguineous, apparently in equal proportions. He is a hard-working man, and strong, unimpaired constitution. I saw him at two o'clock on the morning of the 8th. It was the case of a strong man contending against a violent disease; his pulse was full, strong, hard, and 120 in the minute; face red, eye injected, tongue red, mouth dry, skin hot, restlessness extreme; occasionally he was delirious, &c. I bled him 50 ounces with scarcely any abatement of his suffering. Gave him a foot bath and an active purge, that is pills of Juglans, rhubarb, comp. ex. of colocynth and blue mass. In the morning his situation was

about the same ; bled him 30 ounces ; ordered cathr. enema and foot-bath with teas. The bleeding greatly relieved him ; the medicine with aid of injection purged well ; his skin began to perspire freely ; and he became composed. The headache returned in the course of the day, and he was cupped to the amount of 8 ounces with partial relief. For the next three days the treatment consisted of soda, cold mucilaginous injections, and similar drinks, with sweet spirits of nitre, foot baths, whiskey frictions, occasionally a dose of the chloride of soda, during which period his recovery was very doubtful, but on the fourth day his improvement was evident and his recovery rapid, unmarked by more than ordinary debility.

Mrs. G. was the counterpart of her husband in temperament, physical development, &c. She was of large frame and of great powers of endurance. She was violently attacked immediately after her husband's recovery. The action of the heart, turgescence of blood vessels, redness of face and eye, pains, &c., were excessive ; her fright, anxiety and restlessness were extreme. She refused positively to be bled or cupped. I was almost on the point of leaving her, but consented to make a prescription. I ordered a mustard pediluvium and injection, cold cloths to the head, to be constantly renewed, and immediately after the action of the injection 25 grains of quinine, 2 of ipecac, with 10 grains of blue mass. Abundant perspiration ensued, and for a time she appeared to be doing well ; but the symptoms returned in a worse form, accompanied with evident cerebral engorgement. She obstinately objected to cups and the lancet. The repetition of the quinine was contra-indicated. In the course of twelve hours she became delirious, which soon terminated in spasms and coma, and death took place in 36 hours from her attack.

But for the lancet I am very certain the husband's career would have ended like that of his wife. I believe the wife would now be alive had she been properly and at the start depleted. In the epidemics of 1837 and 1839 in Opelousas, the beneficial effects, nay the imperious necessity of blood-letting was confirmed by the experience of every one. In 1837 nine-tenths of the first patients treated exclusively by medicines perished. Subsequently the mortality was greatly diminished ; and in my practice it could have been attributed only to the free employment of the lancet. In 1839 we bled, with scarcely an exception, and we literally saved all of our patients. The opponents of blood-letting are constantly invoking attention to the lesions of the nervous system, to which alone their attention seems to be directed, and to the great muscular prostration which inevitably en-

sues as the fever declines; and the objection they apply to bleeding is, that it accelerates nervous prostration and aggravates the muscular debility. I saw nothing in 1837 or '39 or in our late epidemic to substantiate such an argument.

In almost every family which I treated, it is easy to institute a comparative estimate of different treatments. I will cite several. The family of G. Carantain. It embraced seven white persons, all of whom had genuine attacks. Four were well depleted at the beginning, three were not bled; of the latter one was cupped twice. The recovery of the four that were bled was speedier, more happy and less marked by prostration, nervous or muscular, than was the recovery of the other three.

Of four black negroes, all of whom were attacked, there were two very severely attacked. One of them was copiously bled, the other was not depleted at all. The first recovered very speedily, and in the case of the second the prostration was so excessive, as for several days to excite the most serious doubts of a recovery.

In the family of Mr. T. M. Anderson four young gentlemen were successfully treated in violent attacks of the fever. Their constitutions and temperaments were very similar. Three of them were copiously bled in the beginning of the attack; two of them were twice bled. The speedier and more happy recovery of these three than of him who was neither bled nor cupped, was a matter of general observation and of surprise to many. The prostration of the latter was more intense and prolonged than what was experienced by the other three.

The results in the large families of Capt. Marsh, Capt. Hinckley, Mr. N. Offutt, &c., coincided completely with those detailed. With regard to 61 fatal cases, I can state from personal knowledge and reliable information, that only in 14 was venesection practised. Hence in 47 cases it was not practised; and in the 14 cases in which the lancet was resorted to, I know that in more than a majority of them it was employed under unfavorable circumstances; and in six of them not before the second day. In one case death resulted from the supervention of another distinct disease. In one case the disease appeared to expend its force upon the brain of the sufferer, and there was confirmed lunacy and aphony from the beginning. In another the recovery was almost certain, when from great imprudence the fever returned and resulted in death.

Amongst the free colored people no blood-letting was practised in the first and fatal case. In the eight succeeding cases blood was drawn more or less copiously, and four were fatal. In all, with the exception

of two fatal cases, blood was drawn within a few hours after the attack; and in one of these two cases not until twelve, and in the other not until sixteen hours had elapsed. With such unfavorable results I desisted from blood-letting amongst the mulattoes; but the subsequent cases I saw were mild in comparison with those of the family in which five died.

Now, according to my observation I do not pretend to say that in every case marked by youth, previous good health and the sanguineous temperament, blood-letting is indispensable; but the important question is, are there not many cases in which it is indispensable to a cure? It appears to me as clear as the noonday sun, that there is a plenty of such cases; and yet the eloquent descriptions given of the deep and profound lesions of the nervous system in this disease—of the fast approaching debility which invariably prostrates the mighty and the strong, as well as the infirm and effeminate, and the direct and positive opposition made by some against venesection at any stage of the disease, cannot but frighten the inexperienced from even occasional employment of the lancet, the rash and improper use of which has no doubt often accelerated death. But if, as has been said, this instrument can vie with the sword in the number of its victims, I am myself fully persuaded that in yellow fever, to its non-employment is to be attributed many a death; perhaps more so in the country than in the city; in which latter place the disease hunts up its most numerous victims amongst those whose constitutions are either enervated by poverty or impaired by dissipation.

To obtain the good effects of blood-letting in severe attacks of yellow fever, it should be practised at an early period. A few hours often suffice for the complete establishment of those modifications of both solids and fluids, in which not only blood-letting, but all active therapeutic agents are not only useless but pernicious. When the action of the nutritive functions is suspended or paralyzed; when the blood has materially changed, in color, in consistence and in coagulability, then bleeding, and, a fortiori, its repetition, would be useless, nay highly injurious.

The great object of blood-letting is, not to bleed the poison out of the system, but to elicit and renew the energies of the nutritive functions in the remote elementary tissues. During the early hours of the disease, when the red blood has injected the face, the eyes, and the skin; when the head, the eyes, the back and limbs are tortured with pain; when the heart, goaded to supernatural exertions, propels with dangerous celerity the super-oxygenated blood through the delicate

tissues of the organs, I would bleed, that I might lessen the labor of the heart; that I might diminish the danger to which organs would be exposed from a virtual excess of blood; I would bleed quickly and copiously, that by lessening the quantity of the intoxicating fluid I might awaken in these tissues the recuperative energies,—the instincts of life, paralyzed by the poisonous cause of the disease, that by exciting into renewed action the nutritive functions and the absorbent system, these elements might be supplied to the blood, necessary to the healthy functional actions of all the nutritive organs, to the probable elimination of the poison itself, and to the restoration of health.

I have said a great deal more on the epidemic of Washington than I intended; for I had proposed to myself simply the discharge of a duty which circumstances devolved on me; to give an account of the origin and progress of the disease—its symptoms and general treatment.

Almost unknowingly to myself, and yet with a diffidence increasing as I became involved in the abstract discussion, I have been led to an enquiry into the pathology of the disease—that is, the peculiar action of the cause and its effects. I am well aware that books have been written by the ablest men to establish peculiar theories; that by Sydenham, perhaps the ablest of them all, the humoral doctrine has been so reconstructed as to challenge even at this day the favorable judgment of men; that a Cullen has applied his intellectual acumen to the investigation of fever; that Clutterbuck has labored to establish that inflammation of the brain is the primary derangement; that the modern Sangrado of France, in his eloquent diatribes against entities, ontologism, and non-essentiality, has yet given to fever a local habitation and a name, and can see in it nothing but a gastro enteritis; that Clanny and a host of others, even including him whose name I venerate in medicine above all living names—George Andral, has again vindicated the claims of a more rational humoralism—and yet in mine ignorance I have felt that a vacuum still existed. I have presumed that the causes of fever, including the congestive, and the cholera, &c., were, though unlike in nature, yet similar in form, and that to produce their effects, they must necessarily be introduced into the system. I have passed in array before my mind the varied morbid effects, as announced in the symptoms of all these febrile complaints, and I have been bewildered in the attempt to explain how effects so varied and opposite could result from the primary vitiation of either the blood or the nervous system, or from an encephalitis, or a gastro-enteritis. In

looking at the human body I have thought that it should be regarded as one great whole, consisting of many parts and systems; that though some of these systems play more important roles than others, they are no more liable to disease in fever than all the other elements which constitute the other tissues of the body, and that of all the functions of this complicated body the most important one is that, which, from the elements derived from without, and in part from within, is engaged constantly in the nutrition and renovation of the tissues, and particularly in the elimination of the effete matters. The blood, I fully believed, might be charged with the morbific causes of fever, and yet be compatible with a healthy exercise of those actions which take place between the ultimate atoms of the solids and fluids, from which the life of every organ and tissue emanates, and consequently from which emanates the totality of movements or actions which constitute life; and that this poison might be excreted without affecting the organization of the blood, or impressing a peculiar derangement on the nervous system or exciting any local inflammation.

In cholera and congestive fever, for example, I was unable to account for the rapid prostration and the sudden and extreme emaciation which take place, and for the thick black blood in the vessels, upon the sup position that the causes primarily produced the above mentioned derangements; when I remembered that analogous causes according to the different theories, producing the same primary derangements, developed an altogether different train of phenomena in yellow fever, typhus, &c. I came to the conclusion from an observation of the effects and phenomena characterizing those states of the system to which the inapplicable term "fever" is given, that the first link, the invariable antecedent in the chain of these effects must be a depravation of the functions of life itself, which exists only and exclusively in the nutritive atomic actions; a full and complete exercise of all and every one in every tissue of the body is necessary to perfect health; that these causes, thus modifying and affecting these functions, inherent in all the tissues, and upon which life and health depend, according to their nature and constitution produce the various effects we witness, as for example, in congestive fever and in cholera, in scarlatina and small-pox, in the continued, remittent and intermittent fevers; in all the atomic or typhoid; and that in yellow fever, the simplest type or form of these complaints, we can mark the progress of the morbific cause in its primary attack upon the nutritive actions, and the subsequent vitiation of the blood and fluids, engendering in their turn, according to individual susceptibilities,

sometimes a scarcely perceptible, sometimes a predominating derangement of the nervous system, as well as all the other symptoms.

Certain elements are necessary to healthy nutritive action, but their existence in improper proportions will derange the functions of life and finally destroy them. Oxygen is indispensable in the animal chemistry, to allow nature to dispose of the carbon, and to manufacture those constituents which are necessary to health; its superabundance as well as that of carbon is prejudicial to healthy atomic actions. Either pushed to a certain extent will arrest these actions and kill. If the atomic nutritive actions are not affected, so that the natural supply and waste continue, it is immaterial what noxious poisons are present in the blood; they are innocuous and health continues. The narcotic poisons, embracing all those which, in conseqeence of their affinity with its atoms or elements act directly upon the nervous system, must necessarily arrest the nutritive actions and thus induce death. And with regard to the causes of fevers, according to their intensity, will be the modifications of the sources of life, and their effects may be a speedy arrest of the nutritive vital functions and death, or such a train of symptoms and effects, which characterize these affections in all their grades; effects dependent for their modifications, in every individual, upon temperament, or rather the physical constitution, which always imparts an individual character to every case of fever whatever.

A thorough revision—a pruning of redundancies—the expression of many ideas in simpler and more comprehensible language—the adoption of more order and system in its arrangement, might tend to make this article acceptable to the Physician. But engagements which have been neglected, and the necessity of an immediate departure for the North, almost deny a re-perusal of what I have written. This much I will say, that I have given the subject the best reflection I was capable of, and that if in the history of our epidemic, as faithful as it was in my power to give, I have supplied a single fact—if I have suggested a single truth, or succeeded in eliciting attention to phenomena which hitherto have been neglected, and whose full investigation is, in my opinion, indispensable to a better knowledge of the nature of the disease, I shall feel that I have fully accomplished my undertaking, and paid a portion of that debt which the divine and noble art of medicine exacts from all of its disciples.

V.—LETTERS ON YELLOW FEVER AT MEMPHIS, TENN., IN 1853.

By Geo. A. Smith, M. D., Physician to the Memphis Hospital, and W. J. Tuck, M. D., of Memphis.

(We commend the following letters to the special attention of our readers. *Ed.*)

Memphis, Tenn., Dec., 1853.

Editor N. O. Med. and Surgical Journal:

Dear Sir—With the belief that it may be a matter of some interest to trace the phenomena of Yellow Fever when carried away from the head quarters of its epidemic fury, the following remarks have been written upon some cases which were brought for treatment to the Memphis Hospital.

The city of Memphis is well known to be by river about 800 miles above New Orleans, and is the lowest point of any considerable population which escaped the fearful epidemic of 1853, having been not only free from Yellow Fever, but favored with a much healthier summer and autumn than has fallen to its lot for several years. The mortality from May to August, inclusive, (generally the unhealthy season here) was in 1853 considerably less than half what it had been the two preceding years.

The nearest point to Memphis at which Yellow Fever prevailed as an epidemic was Napoleon, Arkansas, about 200 miles below; and it ascended to no point above the city; so that Memphis was the lowest point on the Mississippi river, of any considerable population, which enjoyed this happy immunity. On the other hand, it prevailed with virulence at Natchez, Vicksburg, and several of the smaller towns intermediate.

All the cases which were brought to the Memphis Hospital, in number 62, were landed from the river, and contracted the disease in New-Orleans and its immediate vicinity. The mortality, as might be expected, was very great, from the fact that they were not landed till the disease had taken full possession of the system, and then carried the distance of a mile from the wharf-boat to the Hospital, on an open dray. Of the 36 who died, 17 lived less than twelve hours after their arrival.

Three methods of treatment were adopted, of which the most successful, in fact the ouly successful one, was the following: On their arrival, after a hot mustard foot-bath and being warmly covered in bed, a laxative, if necessary, was administered, of an infusion of senna,

rhubarb and manna ; cups and mustard to epigastrium, with ice given solid and iced mucilage ; opiates, particularly the Black Drop, and milk punch ad libitum. Several cases were cured under this treatment, after the black vomit had been well established. The other methods tried were that of calomel aud opium in broken doses, and quinine in scruple doses, with twenty drops of laudanum—both entirely without success.

The question of the contagiousness of Yellow Fever was fully tested, and the result confirms the impression, that without the pre-requisite conditions of atmospheric and climatic derangement, Yellow Fever is no more liable to be communicated from one subject to another than gout or rheumatism. At any rate the 62 patients who were treated in the Memphis Hospital were all of them distributed promiscuously through the wards of the institution ; treated there throughout their progress to recovery or death, and no single instance occurred of the disease spreading beyond the patients imported with the disease upon them.

The above brief statement is respectfully submitted to you. In relation to so terrible a scourge as the epidemic of 1853, it is presumed that as large an accumulation of facts as possible is most desirable—facts derived from observations made under all possible circumstances; and, provided they are plainly and truthfully stated, ought to be acceptable, whatever the sources from which they are derived. The circumstances under which my observations were made were in this respect peculiar, indeed almost unique, as Memphis was probably the only place where a considerable number of *imported cases* could have been treated, in a region itself exempt from epidemic influences.

Respectfuily,

Geo. A. Smith.

Remark.—We are at a loss to conceive how any one could expect benefit from doses of 20 grs. quinine and 20 drops laudanum in the advanced stages of Yellow Fever—say the third or fourth day. We have seen no such treatment recommended by any advocate of quinine, bnt on the contrary, always forbidden. Calomel and opium in this stage is not much better. F., *Ed.*

Editor N. O. Med. and Surgical Journal :

Memphis, Tenn., March 19, 1853.

My Dear Sir—I applied a few days since to Dr. Smith, Physician of the Hospital here, in relation to the snbject of your enquiry, and he

informed me that he had recently transmitted all the statistical information connected with the subject to Dr. Hester, and which probably reached its destination about the time of his death. The Doctor wished me to refer you to that letter. He informed me at the same time that there were 62 cases of Yellow Fever admitted into the Hospital, which were taken from the boats landing at this place and 36 deaths, aud that in no instance was the disease communicated to others. As well as my memory serves me, we had only three cases of Yellow Fever in this city; two of these had been exposed to the epidemic in New Orleans, and probably contracted the seeds of the disease there; the third was a citizen of our place and had not been absent, and it was regarded at the time as a sporadic case, such as occasionally occurs in this city.

You request me to give my views in regard to the contagiousness of Yellow Fever. So far as I have been competent to investigate this subject, and so far as my own observation extends, I am a decided non-contagionist. During my residence in New Orleans, in the summer of 1841, and while one of the attending Physicians of the Charity Hospital during the epidemic of that summer, I think it was the unanimous sentiment of all the prominent physicians of the city that the disease was not contagious, nor do I think any one pretended at that time that the epidemic of that season was imported from abroad. Such, I believe, have been your own statements, as evinced in your writings on this subject. Whether the opinions of the physicians of your city have undergone any change in relation to this matter since the breaking out of the late epidemic, I have not been able to gather any accurate information. Still the intimation has been thrown out, that circumstances have been developed in connexion with the recent epidemic, which have led some to alter their opinions and to favor the doctrine of contagion. However that may be, we are all perfectly well satisfied *here* that the disease could not be communicated by contagion in the atmosphere of our city; and it seems to me, if I understand the meaning of the word contagion, in its proper acceptation, that contagious diseases may be communicated and reproduced from one person to another, without any regard to the temperature or latitude; as for instance, small-pox, scarlet fever, measles, etc.

Now, let us look for a moment to the facts of the case in our city. We adopted no quarantine regulations; we had instituted no especial sanitary regulations with the view of preventing an incursion of the epidemic. Boats freighted with merchandize, saturated with the atmosphere of an infected city, landed at our wharf almost every day; a

number of persons affected with the disease were carried through our streets to the Hospital, and some of them dying in private families, and yet not a case was communicated to any of the nurses, friends or physicians.

It certainly must be a very singular and unprecedented sort of contagious disease which could not be communicated in a single instance, under such favorable circumstances! when the streets of our city were as filthy as usual; when the weather was very warm; when there was no avoidance of exposure, and when every condition existed to promote contagiousness! Does any person suppose for a moment, that there could be brought into our city such a number of cases of any of those diseases ordinarily termed contagious, without the occurrence of a single instance of communication to the unprotected and exposed? During the month of September it fell to my lot to be called upon professionally to attend some cases of Yellow Fever occurring on the steamer H. D. Bacon, from New Orleans. When the boat arrived here some ten cases were taken off and sent to our Hospital; I was retained professionally to attend some of the officers to their point of destination, St. Louis. I must confess that I had some apprehension of danger when I first went on board the boat; not from fear of contagion, but from the infected atmosphere, which I supposed might still have remained to some extent. The boat was a large one, heavily freighted with merchandize, and a large number of the crew and passengers were sick, some of whom died before reaching here. Here, certainly, was as fine and fair an opportunity for an unacclimated and unprotected person to contract a contagious disease as we can well suppose to exist; and yet what are the facts of the case? Not a passenger, out of some dozen or more, who took passage on the boat, above the infected region, suffered from any symptom of the disease. And yet the second clerk, and the captain, who was on duty until he reached St. Louis, had contracted the seeds of the disease in New Orleans, and after I returned home to Memphis I learned that both had died of Yellow Fever several days after their arrival at St. Louis. I am happy to state that the several cases which were under my care were convalescent when I reached St. Louis, and I returned immediately home; and although exposed constantly day and night to whatever source of contagion there might have been, yet my health remained not only unaffected, but was better than usual.

It is said that the epidemic which has swept with so much fatality over the fairest regions of our southern country, differs in some respects from the preceding epidemics. We are disposed to believe the differ-

ence exists rather in degree than in kind. The cases we have seen here, imported from your city, have been just the same in symptoms as those we witnessed in New Orleans in 1841. It is true, the recent epidemic has been much more malignant and fatal than any of the previous ones; and it is stated that many more of those supposed to be acclimated were affected with the disease than at any previous period; but the main peculiarity that has been insisted upon is, that the epidemic extended over a much larger scope of country than it has been previously known to do and occurred in many towns and villages, where it was never known before to exist; thus appearing to favor the opinion entertained by some, that the disease was transmitted to those localities by contagious influence, and not originating from local causes.

Now it is a very important question to determine, whether, in those cases, the disease did *originate* in certain localities, or was trans mitted by persons affected with the disease, steamboats, etc.

We have long since learned enough of human nature to know that the citizens of any particular town or locality are very slow to acknowledge their location a sickly or unhealthy one; and this is very natural; and no doubt many of the citizens of various villages in Louisiana and Mississippi are disposed to attribute the visitation of the epidemic among them to importation from New Orleans. Natchez has been in the habit of doing this for the last thirty years; and yet with all her quarantine regulations, as likewise those of Vicksburg, the disease has prevailed in those places with as much or more fatality than in New Orleans. It is true the quarantine regulations were not as rigid as they might have been; yet if we are correctly informed no steamboats from New Orleans were permitted to land, nor were any of the sick on those boats taken care of until the epidemic made its appearance in those towns. Now compare this condition of things with that which existed in Memphis, not much more than a day's travel on a fast boat. Here we imposed no restrictions—had no quarantine. As before mentioned, large quantities of goods, which had been remaining for weeks in New Orleans, were daily put off at our wharf and distributed through the city; many of these goods of a molient character, well calculated to act as fomites to convey contagious effluvia; and moreover, a number of persons affected with Yellow Fever, daily taken from the boats and dying within the precincts of the city, and yet not a single instance of its communication occurring.

But the enquiry is raised, if the disease was not communicated or transmitted from one point to another, in the region South of us, why did it occur and how did it originate in some of those localities hitherto

regarded as healthy and where the epidemic was never before known to exist? We reply to this by asking, how did it originate in New Orleans, Mobile and other places, at the time preceding which the epidemic was never known to occur in those latitudes? The fact is, a combination of *local causes*, we believe, sprang up at that time; which combination had never existed previously.

We are very much inclined to the opinion, that there existed in the whole region of country embraced within a certain line of latitude South of us, a *certain predisposing epidemic constitution of atmosphere*, which aided in giving rise to so extensive an epidemic. We believe that in the same way there is a predisposing and peculiar constitution of atmosphere prevailing when cholera appears in our country, only the latter is more extensive, and is but little influenced by climate, whereas the former is confined almost exclusively to hot latitudes. The probability is that the increased fatality and malignity of the epidemic this year was the result of a greater degree of this predisposing influence in the atmospheric constitution. This being the case, and other causes, such as animal or vegetable miasm, being the same as in previous epidemics, we may readily explain why the recent one should have been more fatal and malignant as well as extensive in its scope. Now supposing this predisposing atmospheric constitution to exist throughout the whole of a certain region of a warm climate, may we not thus account for the development of the disease in certain localities where it has never occurred before, without resorting to the doctrine of communication or contagion?

This opinion would seem to be corroborated by the fact, that in some of these localities it has been found impossible, from the best information we can obtain, to trace out the least communication of any kind with those towns or places where the epidemic was prevailing. And may we not in this way account for the exemption of our city and that of all other towns on the river north of a certain latitude, say that of Napoleon or Lake Providence? In this city surely we had enough of animal and vegetable filth to produce disease, as well as every circumstance to favor contagion, but we did not have the predisposing constitution of atmosphere. We are therefore disposed to believe that the disease is of local origin and not contagious; and we offer the above theory as a sufficient one, we believe, to account for the extension and prevalence of the disease in Southern localities not hitherto subject to it, without the necessity of resorting to the doctrine of contagion, importation or transmission.

Yours, respectfully,

 W. J. Tuck.

V.—YELLOW FEVER IN PLAQUEMINE, PARISH OF IBERVILLE.

BY J. B. HACKER, M. D.

An Epidemic Yellow Fever prevailed in this town from the first week in September until the latter part of October, mostly among the unacclimated at first, but subsequently attacking those that had resided many years in the State, even natives who had never left the place and negroes.

The first case occurred in a German, employed in a cabinet maker's shop. He went to the city during the epidemic, but did not remain there more than a day or two. Two or three days after his return he was taken with Yellow Fever and recovered.

A few days after (about the 25th of August) another man in the same house was attacked and died of black vomit; before he died his brother and mother got sick and both died of black vomit; the mother on the 3d and son on the 6th of September. These four cases occurred in the same house.

About the same time with the first case, a man came from Baton Rouge in the evening and remained all night in a small cabin on the river. Next morning he took lodging at the Iberville Hotel, took sick and died on the 26th of August of black vomit. I am told this man had been in the city a few days before. No other case occurred in this house.

After these few cases the disease increased rapidly and continued until the latter part of October, when it declined gradually, with an occasional case to this time, 9th December. As the disease declined in town it prevailed more in the surrounding country, on the coast above extensively and below Plaquemine and on Bayou Plaquemine. For a considerable time it remained in the first street from the river, and the few cases that were observed at first in the back part of town were in those that had been in contact with the sick.

In the majority of cases that came under my observation, the fever began with a chill of greater or less severity, followed by violent headache, referred to the forehead over the eyes, pain in the back and abdominal extremities, burning heat of the skin, face flushed, eyes injected, tongue covered with a thick white fur in the middle, red on the edges and at the point, generally large; not much thirst, urine scanty, bowels easily moved; sensibility of stomach on pressure; nausea, and in some cases vomiting; pulse full and frequent, but with the exception of a few cases, not hard, the number of pulsations ranged between 100 and

120 in a minute. These symptoms continued three days, with a very well marked exacerbation at the approach of every twenty-four hours. On the fourth day a complete state of apyrexia supervened, and with well directed care the patient recovered. When black vomit supervened, it was generally on the fourth day; hemorrhage from the gums, bowels, nose, etc., were frequent accompaniments and generally happened on the third or fourth day; in a few cases the epistaxis was profuse and obstinate.

A number of blacks were attacked, but the disease seemed less fatal. As far as I observed in my practice, the cases were easily managed and recovered favorably, although precautionary measures were not as strictly observed as with the whites.

I shall conclude this brief history of the disease in this place by mentioning an instance of its appearance on a plantation about ten miles from this, on Bayou Plaquemine. I will simply relate the facts as they occurred.

On the 17th of September I was requested to call at Captain S.'s house, to see the pilot of the steamboat Pitser Miller, then in front of the house. I found him with high fever, headache, pain in the back and limbs, and all the other symptoms of Yellow Fever. By my advice he was taken from the boat into a room distant from the Captain's house about sixty feet, where he received all the attendance necessary, and recovered in the course of seven or eight days. Soon after and while he was convalescent and still on the plantation, several cases occurred; the first in a black man, who nursed him and was most of his time in the room; the second case was the Captain's child, then his mother, and subsequently several negroes, making in all nine cases. I will remark that none of those attacked had been in town during the epidemic, and that there was no Yellow Fever in the neighborhood at the time.

I might relate other similar instances, did I not fear to occupy too much space in your estimable Journal.

[It is to be regretted that the author did not state whether the pilot of the P. Miller had been at any place where Yellow Fever was prevailing a short time before he was attacked. As Bayou Plaquemine is seldom open to the river in September, it is presumable that the boat was laid up at the time. *Ed.*]

VI.—YELLOW FEVER AT FRANKLIN, LA.

BY J. W. LYMAN, M. D.

FRANKLIN, La. Feb. 14, 1854.

Dr. E. D. Fenner:

DEAR SIR—Yours of the 4th instant reached me four or five days since, and I take pleasure in employing my earliest leisure in giving you, as far as in my power, the required information.

In this town there were only five cases of Yellow Fever last fall, and these all occurred in the house of one of our most valuable citizens, Mr. Simeon Smith. I will first, as briefly as possible, give you a history of these cases, and make a statement of the collateral circumstances existing previous to and at the time of their occurrence.

CASE 1.—Mrs. Smith, aged 45 years, was taken with a chill just before daylight, October 19th, which was speedily followed by fever, accompanied with pains in the head, back, epigastrium, etc. At first I looked on the case as an ordinary but pretty severe attack of remittent fever, but before the end of the second day, my mind was fully made up that I had to deal with a genuine case of Yellow Fever. The fever yielded at the end of the third day, and the patient ultimately recovered, after extreme prostration and a tedious convalescence. The three next cases occurred in persons who were in close attendance on Mrs. S. from a very early period of her attack.

CASE 2.—Mrs. Johnson, aged 30 years, stepdaughter of case 1, had chilly sensations at ten o'clock, A. M., October 23d, soon followed by fever. This case differed in no material respect from No. 1, except that she had moderate bleeding from the gums and retention of urine. She slowly recovered.

CASE 3.—Mrs. McMillan, aged 32 years, sister to the above, was taken with the chill about one o'clock, A. M., October 24th. Hers was a very violent case from the first moment of attack, and progressed steadily on from bad to worse, to a fatal termination, which took place at 6 o'clock, A. M., October 28th. This patient was six months advanced in pregnancy, and aborted 24 hours before death. Several hours before death she threw up small quantities of black vomit.

CASE 4.—Simeon Smith, aged 52 years, husband of case No. 1, and father of cases 2 and 3, by a former marriage. Taken with a chill at daylight October 24th. This case was at first apparently no severer than cases 1 and 2; but in the evening of October 28th, the patient without any very assignable cause, fell into a state of profound coma,

and died October 29th, at 11 o'clock, A. M. After death, black vomit passed in considerable quantities from his bowels, as I was informed by an experienced nurse ; one who has seen much of the disease in New Orleans and Mobile. In the night of the 24th, the same day on which the two last cases were taken, we had a pretty heavy frost, and small quantities of ice were seen by one or two persons in the neighborhood. Another heavy frost occurred on the night of the 29th.

CASE 5.—Ella, aged 11 years, daughter of cases 1 and 4. Taken with a slight chill November 24th. Being unable, from indisposition, to leave my house, this case fell under the care of Dr. C. M. Smith. For the first two or three days her case was regarded as one of our autumnal remittents, but this opinion was changed on the third or fourth day, when she threw up a large quantity of black vomit. This was saved and has been shown to several physicians conversant with its appearance, and no one expresses the least doubt of its character. The patient recovered after a lingering convalescence.

In relating the above cases I have avoided entering into a minute detail of symptoms ; it being my design to give, in as few words as possible, such a history of the cases as would leave no doubt in your mind, as there is none in mine, as to the true nature of the disease. In relation to the treatment, I have nothing new to offer. I adopted what I believe is usually called the expectant plan, viz., mustard foot baths, a cathartic to procure free evacuations within the first 24 hours, diluent drinks, flaxseed injections subsequently, if deemed requisite, minute doses of morphine to procure rest, when not positively contra-indicated. In fine, after the first day the patient was trusted pretty much to nature and the watchful care of attentive nurses. Of the various modes of treating this insidious disease I am by no means properly qualified to speak understandingly ; but I must confess that I am far from being satisfied with my own treatment, nor have I any more confidence in that which I have known pursued by others.

Here it may be proper to refer to the local causes of epidemic disease which existed in Franklin, where the disease commenced. It is an undoubted fact that our little town, never very notorious for its filthiness, was never in a cleaner condition—was never freer from filth and other local causes which are so often appealed to as the originators of pestilential diseases, than it was on the 19th October, when the first case occurred.

Our Town Council, with commendable zeal, had been more than usually active in their endeavors to improve our sanitary condition. In fact, no appreciable local causes existed in or about our town ; and if a

scientific committee had been appointed to select the dryest, cleanest and healthiest spot in the whole town, it would most certainly have selected the residence of Simeon Smith; and yet, strange to say, here, and here only, did the fell destroyer begin and end his ravages. In the absence then of any such causes, where shall we look for one adequate to the production of the disease? Whence came the infection, and in what manner did it reach this family? These are questions much easier asked than answered, and I, for one, am unable to solve them; but I submit the following statement of facts, which although of apparently trifling importance, may assist, in connexion with facts gathered from other sources, in throwing some light on the mode of propagation of this disease.

Some time about the 25th of September, in consequence of positive information being received here that Yellow Fever was prevailing in Centreville—a little village five miles below Franklin on the Teche—our Town Council adopted pretty stringent measures to prevent a free communication between the two places. Persons living in Centreville and having business to transact in Franklin were permitted to remain here but three or four hours during the day, and our citizens were warned that if they visited any infected place, they would have to submit to a nine days' quarantine before they could return to their houses. The latter part of this arrangement was not always enforced, and at *this time* physicians were exempt from the restriction. A barrier was drawn across the road half a mile below town, and a guard stationed there to admit or reject persons from below. This barrier was kept up with tolerable efficiency until about the tenth or twelfth of October, when Centreville being reported healthy, the prohibition was withdrawn for one or two days.

On the 14th October Mr. Smith (case 4th) sent his negro man with horses and wagon to the store in Centreville, in which he was a partner, to bring up some goods to his store in Franklin. The clerk informs me that the following articles only were brought up, viz., one one bale of oakum, box of axes, box of children's shoes, and a paper package containing India Rubber coats. These goods had arrived in a vessel from New York, and had been but three days in Centreville. Mrs. S. (case 1) was in the store when these goods arrived and were opened.

This day (October 14th) the fever again broke out in Centreville, and a system of non-intercourse more rigid than the first was established. On the 19th Mrs. S. was attacked, and the only known com-

munication of the Smith family with an infected place was as stated above. At that time the family consisted of Mr. S. and wife, five children, a clerk and five or six servants. Mrs. M., (case 3) living in Franklin, and Mrs. J. (case 2) living three miles from town) both came to their father's on the day Mrs. S. was taken. Mr. H. Smith, eldest son of Mr. S. Smith, who arrived from the North a few days before Ellen (case 5) was attacked, and who had been pretty constantly in attendance on her, escaped. Of all the nurses and medical attendants, not one was attacked.

This, I believe, comprises all that is worth relating in regard to the Yellow Fever in Franklin ; above this point there was not one case, while the lower end of the parish suffered to a great extent. And now a few words in reference to our famous quarantine, and I have done. This town had suffered severely by the yellow fever in 1839, and to avoid, if possible, another such calamity, our Police Jury met and adopted certain quarantine regulations, in which most good citizens acquiesced, believing that in their existing uncertainty as to the contagiousness or non-contagiousness, transmissibility or non-transmissibility of yellow fever, it would be the part of prudence and humanity to adopt such reasonable measures as would be calculated to stay the pestilence and thereby escape its fearful consequences.

A quarantine station was established, and a house prepared for a hospital near the junction of the Atchafalaya and Teche, and a physician and attendants appointed to examine steamboats and other crafts plying in this trade. All vessels from infected regions were required to undergo nine days' quarantine, dating from the time of leaving such infected region. Another station was established on the Atchafalaya, a short distance below Pattersonville, and a physician appointed to examine the mail boat and vessels coming in from sea.

Shortly after the quarantine went into operation, (about the 20th of August) the steamboats Pitser Miller and Planter arrived at the station. The latter served out the required time and came up the Bayou without opposition. The former, instead of remaining at quarantine, crossed Grand Lake to Fausse Pointe, discharged part of her cargo and passengers, returned to quarantine and landed a young man who was in the last stage of yellow fever, and who had black vomit in half an hour after he was placed in charge of the Physician, and died next morning. Under these circumstances the quarantine physician properly refused to grant a permit to the boat to proceed up the Bayou.

Notwithstanding this refusal, the commander of the boat being, one would suppose, somewhat wearied with such an inactive and unprofitable life, and withal not over-abundantly supplied with the creature comforts, determined to take the responsibility of leaving and risking the consequences.

The boat then left, discharged at Pattersonville what cargo she had for that place, and proceeded up the Teche to within half a mile of Franklin, where she received warning to proceed no farther. After some little time spent in parleying and deliberation, and seeing a pretty formidable posse collected on the wharf, determined to dispute her passage, she returned to quarantine ground, discharged the portion of her cargo intended for this and other places on the Teche, and recrossed the lake.

Some considerable difficulty, which came near being attended with serious results, occurred with the mail boat Fairy, principally in consequence of some of her passengers being unable to take the oath required by the Board of Health, and the refusal of the Fairy to proceed to quarantine. After detaining her below Franklin for two or three hours, the difficulty was adjusted without bloodshed, by a sort of compromise, not very satisfactory to any of the parties concerned.

The quarantine was not generally so rigidly enforced as it should have been, in order to test the question of the transmissibility of yellow fever; but much more so than could have been expected from persons who were so little acquainted with the best modes of organizing and sustaining it.

So far as I have heard, the first case of yellow fever in this parish occurred in a young man from New Orleans, who crossed the lake on the Fairy and was permitted to land three miles above Pattersonville, on condition that he should remain in the country and not in the villages. After remaining on a plantation a few days, he was taken sick, and the proprietor, fearing he might have yellow fever, had him conveyed to Pattersonville. This man ultimately recovered, and I am informed that one of the physicians of Pattersonville pronounced his disease yellow fever. Be this as it may, it was not long before cases occurred in rapid succession, and of so unequivocal a character as to leave no doubts of their nature; and it is a fact of notoriety that this same physician (Dr. Grant) pronounced the prevailing disease to be yellow fever long before the other resident physicians, some three or four in number, would acquiesce in his opinion.

I have heard many other reports in relation to the spread of the epi-

demic at Pattersonville and Berwick's Bay, but as I can give nothing as facts, I should be going beyond my intentions in giving mere rumors.

This concludes all I have to say at present on yellow fever, and should you be of opinion that any thing contained in this communication may be made subservient to the interests of humanity and science, you are at liberty to use it as you may think most conducive to those ends.

Yours, &c.,

J. W. Lyman.

VII.—THE YELLOW FEVER AT GRAND GULF, MISS, IN 1853.

BY E. M'ALLISTER, M. D., PORT GIBSON.

Editor N. O. Med. and Sur. Journal:

Dear Sir—I propose giving you a brief account of my practice and observations in the late yellow fever epidemic of Grand Gulf.

The disease assumed the character of sthenic. The pulse ranged from 120 to 180 beats per minute, full and strong; the skin intensely hot, cheeks flushed, eyes injected, pains in the muscles as if neuralgic, tongue whitish on the dorsum and red at tip. A disease of one paroxysm, with slight remissions, terminating in three or four days, in death or convalescence. The tendency to death seemed to be by congestion of some vital part; of the lungs, brain, stomach, liver, &c. The most plausible theory of black vomit is, that it is caused by congestion; and the effusion of blood into the cavity of the stomach as its result. It has been pretty well demonstrated in pathological researches, that the jaundiced condition of the skin and eyes is produced by obstruction in the liver, gall-stone, structural or functional derangement. The idea that it can or ever has been produced by any other cause, is perfectly gratuitous, according to the lights before us, as we have no evidence of the fact.

Conforming my practice to the above diagnosis, I adopted the system of cooling and depletory remedies, to unload oppression and reduce excessive excitement—to wit:

The sick room was freely ventilated:

When the fever rose and the skin became hot, either with or without perspiration on it, the patient was put into a cold plunge bath, in which he was allowed to remain until his skin became cool and his pulse reduced to near its natural heat. This required from three to ten min-

utes. Cold water was at the same time poured on his head, and his skin rubbed with towels. He was then put to bed and two or three blankets thrown over him. If a natural perspiration came on, the blankets were allowed to remain on him six or eight hours, and a cup or two of warm tea administered; but if the fever rose again, the blankets were removed, plenty of cold water given to drink and another bath given—some one of the following: Plunge bath, shower, wet sheet pack, sitz bath, head bath and sponge. Iced water was applied to the head when it could be had; and if the feet became cool, they were put in hot water. If the patient was of a weakly habit, water of milder temperature was used to the body. Throughout the whole course of the disease some one of these baths were used whenever the fever rose.

After quieting the irritability and excitability of the system to some extent, which could generally be effected in a few hours by the above means, I gave from two to four of the following doses, made into pills, at intervals of one hour: Calomel, grs. v; blue mass, grs. v; ext. hyosciamus gr. 1. If this did dot operate on the bowels in due time, Cook's pills, comp. cath. pills or castor oil was given. These medicines agreed well with the stomach and operated gently on the bowels, bringing off dark bilious matter. A few grains nitrate of potash, largely diluted in water, was also given every three hours; also an occasional opiate.

In some few instances, in which there seemed to be great torpidity of the secretive functions, I gave ipecac emetics with good effect. If after using the above means 20 or 30 hours, the violence of the symptoms continued to recur, with a full, strong pulse, I resorted to the lancet. This I found necessary in four cases, all of whom I bled freely from the arm, and with the happiest results, and all of them recovered. One of these cases deserves special notice. She was a lady of a rather delicate frame, and about seven months advanced in pregnancy. After being sick about twenty-four hours and using means, her symptoms rose to a violence I had never before witnessed. The commotion of her heart, arteries and nerves suggested to my mind the idea of a tornado existing in her system. Believing this state of things could exist but a short time without a destruction of organization; and that the lancet was the only means by which relief could be obtained, I accordingly bled her from the arm. The first bleeding the blood was allowed to flow to the amount of thirty-two ounces, as near as I could guess, with but slight impression on the pulse. I then stopped the bleeding and waited two hours, at the expiration of which time her symptoms had

risen to their former violence. I again corded the same arm and from the same orifice allowed the blood to flow to the amount, I suppose, of at least twenty-four ounces, while she remained in a sitting posture on the bedside. The desired effect was then produced, a relaxation of the pulse, when all the sympathies of her system acted in harmony. After this I did not find much difficulty in conducting the case to a convalescence. She went her full time in utero-gestation, was delivered of a fine boy and is now in good health.

Many cases which occurred were of so mild a type that nothing was required but cold water to the head and one cathartic.

In treating my cases, I avoided the use of all articles at all tonic, stimulant or excitant.

Such is a brief outline of a system of medical practice pursued by me in the late epidemic of Grand Gulf, the good effect of which, in relieving the sufferings of the sick and restoring them to health were so signally manifest, that all who witnessed them were filled with admiration.

I treated fifty cases, all white except eight, and all recovered except one, a child about four years old.

As regards the cases treated here on the quinine, stimulating and heating plan, I can say nothing from experience, for I treated none in this way; but from what I can learn from others, very few of the bad cases to whom this practice was administered recovered; and when used in mild cases, it produced a great deal of unnecessary suffering.

According to my observation, the disease was no more severe, under similar circumstances, with persons who had lately come to the country than with natives, or those who had been here a long time.

In all classes of people, I think the young and middle aged suffered most.

As regards the contagious nature of the disease there is much contradictory testimony afloat. I know of no incontestible instance in which the disease seemed to have been communicated from one person to another. It is also perhaps useless to speculate on the origin of the disease in Grand Gulf and Port Gibson. Some attribute it to importation.

Grand Gulf is situated on the river bank, on a level plain. Port Gibson is seven miles back in the country, but on rather a level locality; both places on rich alluvial soil.

For the last eighteen years in this country I have been a pretty close observer of the seasons, and I am sure I never before witnessed such

repeated floods of rain, attended with such excess of thunder and lightning, and succeeded by such hot, sultry days, as occurred here the latter part of the last summer. Many times the localities on which the villages stand presented the aspect of marshy districts—with all the concomitants of heat, moisture and vegetable matter, exposed to the sun in a state of decay; and I will venture the opinion, that whenever there is a similar combination of circumstances, they will be succeeded by a similar or some other epidemic disease.

Port Gibson, Miss., Dec. 28, 1853.

Part Second.

EXCERPTA.

I.—ON THE ZYMOTIC THEORY OF ESSENTIAL FEVERS AND OTHER DISORDERED CONDITIONS OF THE BLOOD.

Extract from the Prize Essay af S. G. Armor, M. D., of Cleveland, O.

There are few inquiries in pathological science of more interest than those which relate to changed conditions of the blood; for whether we regard it as endowed with a distinct vitality and obedient to the general laws of cellular growth, development and decay; or as ministering to the nutritive and textural wants of the system in the elaboration of fibrin from elements furnished by primary assimilation; or as connected with important chemical changes essential to a healthy action of the system; whether we regard the blood as contributing to one or more of these purposes in the animal economy it becomes at once evident that destruction of its vitality, or change in any of its constituent elements, must be followed by serious constitutional disturbances. Hence the interest with which its diseases should bes tudied and the importance of understanding, in a curative point of view, the primary and secondary impression of disease upon this fluid.

It must be confessed, however, that the question of priority or sequences, although of much importance to him who thinks or reasons about the nature, origin and phenomena of disease, is often one of difficult solution. But to arrive at greater certainty on this point if possible, so far at least as relates to the essential features, is the object of this Essay, and if I shall succeed in any degree in pointing out the distinction between symptoms of diseased action and diseased action itself, I will have, to some extent at least, accomplished my object.

In M. Andral's classifications of Lesions, in which he makes all disease to exist, he embraces some in which no notable change of either organization or composition can be detected. Yet it is worthy of inquiry as to whether this eminent pathologist has not included in his lesions some which are but symptoms, not properly diseases—actions and not states.

It is not my purpose, however, at present, to enter this field of inquiry. I desire to call attention to another question in which no such controversy can arise.

In the essential or idiopathic forms of fever, it is evident that change has been induced in the blood by the admixture of foreign matters. The proof of this consists in the fact—

1st. That diseases analogous to those fevers have been induced by injecting putrid matter into the veins of animals.

2d. These fevers are readily produced by the introduction of animal poisons into the blood, as in the case of small-pox, measles, &c.

3d. These poisons are known to operate through the medium of the air, by thus obtaining access to the blood through the lungs.

4th. The non-contagious fevers, such as intermittents and remittents, are universally admitted to depend upon a poisoned or changed condition of the atmosphere.

5th. Actual observation establishes the fact that the blood is altered in all essential or idiopathic fevers.

The best point of departure, therefore, is the general fact (for it should be regarded as such) that all essential fevers depend primarily on a poisoning of the blood, and the proof as to primary impression will be given in illustration of the facts already cited.

It must not be inferred, however, that I am laboring to establish the identity of fevers. No such inference can be legitimately drawn from any fact or reason which I shall present. True, so far as the general fact is concerned that all foreign matters, when introduced into the blood, change either its physical, chemical or vital properties, all essential fevers may be regarded as a unit. Yet observation abundantly establishes the fact, that different poisons act differently on the human constitution, and upon the peculiar and specific character of each depends not only the destructive effect on the blood, but the local lesions that will ensue. Urea and its compounds, if retained in the blood, affect the brain and nervous system, and are apt to give rise to a low grade of inflammation in serous and sero-fibrous tissues; while mucous structures will suffer but little. But the small-pox virus spends its force upon mucous and cutaneous structures, and leaves unharmed the serous and fibrous structures.

There can be no explanation given of this other than the general fact that the tissue or viscus affected seems to be that which has an affinity for the poison which has to be eliminated from the blood. In this process of elimination, inflammation and its sequela are excited and local disease becomes manifest. Hence all essential fevers should be regarded as distinct in species, according to the circumstance of the primary sedative impression. This is the only true and rational classification of fevers.

We feel authorized in asserting then, as a starting point in our reasonings, what observation abundantly establishes, that each specific miasm has its own peculiar and distinct law of development. But in the absence of all reliable information, as to the essential nature of these miasms, it would be idle to speculate. Our knowledge on this point must, at least for the present, rest on observation.

But it will be at once perceived that our knowledge of the action of remedial agents is not more certain. Indeed the perfect analogue of one is found in the other, and the reasoning applied to one applies with equal force to the other. Thus, that mercury will excite inflammation of the salivary glands; arsenic, the mucous structures; belladonna, the skin; ergot, the uterus, etc., has long been a matter of observation. But why they should be so is just as obscure as why the typhoid poison should select for its destructive action the glands of Peyer, or the small-pox poison should spend its influence upon the dermoid structure. The articles of the materia medica furnish a just illustration of the action of all foreign substances in the production of disease. Mercury, arsenic or croton oil, if uncontrolled by the judicious skill of the physi-

sician, is capable of giving rise to diseased action with as much certainty and as varied in its manifestations as either of the animal poisons to which I have alluded ; and analogy would lead us to suppose that if we could control the one, as we can the other, miasmatic poisons might be used as therapeutic agents. That all agents that affect the vitality or composition of the blood bear certain pathological relations, cannot be doubted ; but that by no means proves the doctrine of identity. As well might we assert the identity of small-pox and typhus fever, from the fact that the fibrin of the blood is found defective in both.

An important point however to be established, before conclusions are drawn, is the fact, that the blood does undergo change in disease, and from medication, diet, etc. ; for if this be denied, our conclusions will be without a predicate, and therefore unsound. But the chemists have happily settled this point by furnishing us accurate analyses of the blood both in health and disease. In the condition of health the venous blood of a man, as represented by the number 1000, is composed of—

Serum, - - -	869,1547
Globules (fibrine included)	130,8458
	1000

This varies, however, according to sex, age, temperament, kinds of food, evacuations, etc. The rapidity with which some of the solid constituents of the blood are diminished by blood-letting, for example, is very remarkable. Thus, according to the researshes of Dumas, the blood of a robust young man, of 23 years of age, gave—

At the first venesection,

Water, - - -	780,210
Globules, - - -	139,129
Albumen, Salts, Fatty and extractive matters,	80,661
	1000

At the third venesection,

Water, - - -	853,46
Globules, - - -	76,19
Albumen, Salts, Fatty and extractive matters,	70,35
	1000

The more solid constituents of the blood, it will be seen, are rapidly supplied by a compensating quantity of non-sanguineous fluid ; and hence the value of blood-letting when it is desirable to promote absorption.

Diet and drinks also very readily affect the constitution of the blood. Ac-

cording to M. Denis, in the blood of a young man 21 years of age were found—

Water, - - -	770
Globules, - - -	154
Albumen, &c., - -	76
	1000

After forty days' use of watery drinks—

Water, - - -	804
Globules, - - -	111.9
Albumen, - - -	84.1
	1000

It will be thus seen, that the blood is very readily changed in its constitution by blood-letting, diet and exercise. It sustains direct relations also to the air we breathe, to the water we drink, to the food we eat, and to the excretions of the body, by which it is purified; and that a fluid which is presented to us in such a compound and complicated form, and sustaining so many relations to the various modifying influences which surround it, should not become a frequent seat of disease, would be indeed an anomaly in nature.

Among the various hypotheses to account for fever, a zymosis, or fermentation of the blood has prevailed, under one form or other, from remote antiquity. But it has been so inseparably connected with the old humoral pathology, that it has received little consideration. Recently this hypothesis (for I shall regard it as such at present) has been rendered, to say the least of it, very plausible by the researches of the distinguished Liebig.

In his Animal Chemistry he calls attention to the fact that no other component part of the organism can be compared to the blood in respect of the feeble resistance it offers to exterior influences, and the reason assigned is, that "it is not an organ which is formed, but an organ in a state of formation." The following quotation embodies in a few words the main leading thought of the author on this subject:

"The chemical force and the vital principle hold each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood."

This then is an important starting point in our reasoning process; for if it be really possessed of a low vitality, we may logically arrive at the conclusion, by an *a priori* argument, if we knew nothing of the facts in confirmation of it, that all fevers produced by endemic, epidemic or infectious causes, have their origin in a primary diseased condition of the blood.

A zymotic change of the blood is due, according to Liebig, to a decomposing organic molecule in the interior of the human body. This molecule, by a law of catalysis, induction, or contact, has the power of imparting its own motion to another molecule, with which it may be in contact. Hence chemists have defined it to be "decomposition by contact," or the "action of presence." We have illustrations of this law in the power which small quantities of substances, in a state of change, possess of causing unlimited quantities to pass into the same state; and it is an interesting fact, worthy of note in this connection, that all substances which readily suffer this transformation are, without exception, bodies which contain nitrogen. A large portion of the blood

being composed of this element, we might readily conclude that it is the vital principle alone that keeps it from spontaneously passing into this condition of transformation. If the catalytic force be greater than the resistance offered by the vital principle, the blood must pass into a condition of decomposition.

It may be asked, however, with reference to this law of zymosis, or induction, is there any evidence to show that the introduction of putrid matter into the animal system does give rise to effects which are at all comparable with those of fever? If not, the law which has been announced is but a speculation, and at best, an hypothesis. But let us see. "It is a fact," says Liebig, "that subjects in anatomical theatres frequently pass into a state of decomposition, which is communicated to the blood of the living body." And the fact observed by Magendie, that putrifying blood, brain, eggs, etc., laid on recent wounds, cause vomiting, lassitude and death, after a longer or shorter interval, has never as yet been contradicted. Numerous experiments have demonstrated that putrid matter injected into the blood of healthy animals will give rise to a set of symptoms which are very analogous to typhus. "If a small portion of putrid matter," says Dr. Armstrong, "be accidentally introduced into the blood during dissection, or if the experiment be made upon the lower animals, it produces fever having exactly the character of typhus under its continued form, and no individual could confidently pronounce that it differed from it." Bernard has also shown that by injecting yeast or sugar into the circulation, many of the ordinary kinds of fermentation are excited, giving rise to a disease very analogous to typhoid fever, accompanied by prostration of strength, bloody fluxes, ecchymosis, and a black and uncoagulated condition of the blood. "Lastly, it is," says Liebig, quoting from Henle, "a universal observation that the origin of epidemic diseases is often to be traced to the putrefaction of large quantities of animal and vegetable matters; that miasmatic diseases are endemic in places where the decomposition of organic matter is constantly taking place, as in marshy and moist localities; that they are developed epidemically under the same circumstances after inundations; also in places where a large number of people are crowded together, with insufficient ventilation, as in ships, prisons, and besieged places."

It is also worthy of note that these factitious fevers, produced by the introduction of deleterious substances directly into the blood are analogous, both in their symptoms and pathological lesions, to those produced by the sting or bite of certain animals; they present, also, the same general class of symptoms that are present in small-pox, malignant scarlatina and other eruptive diseases.

In Mr. Walker's work on Grave Yards, he also presents an array of facts, which prove, beyond all controversy, that putrid animal exhalations have given rise to diseases that have raged like a pestilence or epidemic. He cites an instructive instance, which occurred in 1733, at the parish of St. Saturnine, in Burgundy. A sexton, while letting down a corpse into the vault, accidentally broke a coffin which contained the body of a fat man that had been buried 23 days. A discharge of sanies followed, which greatly annoyed the assistants; and, "of one hundred and twenty young persons of both sexes who assembled to receive their first communion, all but six fell dangerously ill, together with the cure, the grave-digger and sixty other persons." The disease is described as a putrid, verminous fever, accompanied with hemorrhage, eruption and inflammation.

Facts in support of these views might be accumulated at great length. Dr. Francis Home communicated measles by means of a drop of blood from a patient affected with the disease. And the experiments of M. Gendrin, as given in Williams' Principles of Medicine, is a striking one in point. "A man who

had been skinning a diseased animal was seized with a putrid fever, attended with an eruption of sloughing pustules. Some blood taken from this man was injected within the cellular texture of the groin of a cat; the animal was soon after affected with vomiting of bile, dyspnœa, frequent, small and irregular pulse, dry, brown tongue, slight convulsions, and died seven hours after the injection." The same pathologist induced in animals various and severe symptoms, followed by death, by injecting into their veins the blood of persons laboring under small-pox. MM. Dupuy and Lauret also communicated the malignant pustular disease known as "Charbon," by injecting into the veins of the healthy horse a minute quantity of blood of the diseased animal. Andral relates an extraordinary case in which a malignant fever, followed with pustular eruption and death, was occasioned by the mere contact of the lips with the diseased blood of an animal.

May we not then infer from these facts, that the blood is the hot-bed in which many malignant diseases are propagated, whether by ova, parasites, cell germs, or zymotic action.

But our proof does not rest here. Clinical observation has long since established the alteration of the blood in diseases which are termed putrid; the blood appears to be in a partial state of dissolution; its vitality is destroyed, and its fibrine either not elaborated, or dissolved in the process of putrefaction. As a result of this decomposition, an increased quantity of hydro-sulphate of ammonia has been found in the blood of patients suffering from typhus and other malignant diseases; and hence the alkaline reaction of the urine that is so often observed to be present in these fevers.

These observations have been made the basis for the support of a great group of maladies which go by the name of zymotic diseases, and include, according to the statistical nosology of Mr. Farr, small-pox, chicken-pox, all eruptive diseases, influenza, scurvey, purpura, ague, remittent fever, yellow fever, typhus, puerperal, plague, hospital gangrene, etc. And in proof of their zymotic origin the fact has been offered—1st, that the vitality of the blood is low, and that it therefore readily suffers transformation; 2d, that we can produce in animals and man factitious diseases by inoculations or injections of putrid or contagious matter having all the characteristics of the essential fevers; and 3d, that clinical observation establishes the fact that the blood is changed. And that the febrile phenomena present in these fevers indicate a condition of the system independent of inflammatory action, I infer from the fact—1st, that in the absence of complication during the progress of the disease, there is no evidence of inflammation revealed by post mortem inspection; and 2d, that the symptoms co-exist with a diminution of the fibrin of the blood, and diminished tolerance of the loss of blood. In some of the most malignant forms of fever—those in w hich the fibrin of the blood is at its minimum—there is often not much heat of body and but little increase of pulse. The patient often dies in the cold stage of such fevers, without in fact having any fever! Evidently, therefore, the term fever may be used in two very different senses—in one, signifying a collection of symptoms depending on local inflammation; and in the other, a condition of the system entirely independent of such inflammation. In one the term indicates the name of a disease, and in the other the name of a symptom. Hence the distinction between essential and symptomatic fever; and hence the inference, also, that the essential fevers have their origin in certain qualitative changes of the blood, caused by the introductian of foreign matters.

Further proof that the general class of diseases which have been termed zymotic have their origin in the blood is drawn from the symptoms usually present. These will be found accurately detailed by all standard writers on General Pathology, under the head of "Necraemia, or death beginning with

the blood," such as petechiæ and vibices on the external surface, the occurrence of hemorrhage in internal parts, the general fluidity of the blood, its frequently dark and otherwise altered aspect, its proneness to pass into decomposition, the general prostration of all the vital powers, the dark tongue, sordes on the teeth, suspended secretion, and the general arrest of molecular nutrition. Indeed the very universality of the diseased action points to a cause more general than can be found in any individual function.

I have thus far spoken of admixture of foreign elements in the blood from without. There are causes, however, which operate upon it intrinsically as well as extrinsically. Thus, defective excretion is followed by a direct backward action on the blood, resulting in changes of its chemical or vital properties. The excretory organs are the natural emunctories, through which effete matters, generated within the organism, are expelled from the blood. The product of the various excretions may be regarded, therefore, as the correct expression of the numerous changes that are taking place both in the healthy and diseased animal fabric. In febrile diseases these organs are generally suspended in the exercise of their healthy function, an increase of perspiration, or in the flow of urine, or a spontaneous diarrhœa, being generally accompanied with a subsidence of the febrile phenomena.

Relatively considered, the kidneys may be regarded as the most important emunctories through which morbid matter is expelled from the blood. The experiments of Orfila on this subject are highly satisfactory. He found that the pernicious effects of small and repeated doses of arsenic could be readily averted in animals, by giving them at the same time a diuretic medicine; and the converse fact has been frequently observed, namely, that persons who suffer from disease of the kidney, by which its function is impaired, very readily contract infectious disease, and are apt to suffer from their effects. It has also been observed that opium, arsenic, mercury, etc., operate with dangerous energy on such patients.

(*Peninsular Journal of Medicine, Jan.* 1854.)

II.—*The Uses of Galvanism in Obstetric Practice.*

BY ROBERT BARNES, M. D.

(From the London Lancet, Jan. 1854.)

I pass over all those means of rousing the uterus from a state of inertia which occur to the mind of the practitioner conversant with the physiology of parturition. An encouraging word, the restoration of hope and confidence, a timely stimulating draught, the pressure of the hand upon the uterus externally, the use of cold variously applied, all these and a thousand other means may, in numberless cases, supersede the resort to ergot. It is my object to direct attention to an agent which is, I believe, an absolute substitute for ergot, and one which may be resorted to with confidence in every form of labor marked by defective uterine action, and in every case where it is desired to excite contraction of the uterus. The agent which answers to these conditions is galvanism.

From time to time many valuable but isolated observations upon the use of galvanism in different cases of obstetric practice have been published. But no systematic attempt has been made to prove that in galvanism we possess an agent capable of universal application wherever we require a safe and effectual stimulus to the muscular structure of the uterus. I shall consider the

uses of galvanism in the successive epochs of gestation and parturition, beginning with its use in the induction of premature labor.

The Use of Galvanism in the Induction of Premature Labor.

In 1803 Herder* suggested the use of electro-galvanism for the induction of premature labor. In August, 1844, Doctors Horninger and Jacobi† succeeded in bringing on labor by the electro-galvanic apparatus after other means had failed. The application was immediately followed by uterine action, and the child was born in an hour from the commencement of the operation. A successful case under the hands of Mr. Demsey is also referred to by Dr. Golding Bird. My researches into what has been written on the subject have not been sufficiently minute to enable me to say that no other similar cases have been recorded. In January, 1851, I myself had an opportunity of testing the efficacy of this agent,

Case 3.—I have already referred to this case for the purpose of illustrating the effect of contraction of the uterus upon the fœtal circulation. The result, although perfectly satisfactory, was by no means so speedily accomplished as in the case of Horninger and Jacobi. I had previously endeavored to bring on labor by puncturing the membranes, and inserting a sponge-plug in the cervix uteri. This proceeding was followed by no symptom of labor. On the 23d of Jauuary I applied the galvanic battery for half an hour, placing one pole on either side of the uterus. Immediately after commencing the shocks the bladder was irresistibly emptied, to the evident annoyance of the patient. The womb was felt to become hard, and the patient herself was sensible of contractions and increased movements of the fœtus. The contractions did not continue on the cessation of the galvanism, and I therefore repeated the applications on the 24th and 26th, for about an hour each time. On the 26th a "show" took place. On the evening of the 27th slight pains were felt; the cord was presenting, a small loop coming through the os uteri, which was now dilated to the size of a shilling, but feeling rigid. She had had rather copious flooding in the day time, but it had stopped. The head was felt lying on the pubes in front of the os uteri, the cord coming down in the free space behind it. On the morning of the 28th, the galvanism having been applied at intervals all night, the pains had increased. I have already mentioned how the galvanism increased or originated contraction. At nine A. M. the child was born. It was apparently not more than six months old. The patient had certainly reckoned falsely. The child's heart was pulsating; the chest made three or four convulsive heaves, at which the mouth opened, but no air seemed to enter; the lungs refused to expand; the walls of the chest were drawn in towards the spine. I endeavored to excite respiration by the galvanic apparatus, but although I could at will cause a respiratory effort, the child was evidently too immature to live. The womb contracted favorably, and the placenta being withdrawn was found healthy. The patient recovered without a bad symptom.

The excellent effect of galvanism in this case led me to recommend the use of the same agent to my friend, Mr. Mansford, who has favored me with the following account :

Case 4.—The lady whose case led me to attempt the induction of premature labor, was in the forty-first year of her age and the thirtieth week of her fifth pregnancy. On the 8th of November, 1852, having ruptured the membranes, I introduced one wire of the apparatns within the os uteri, and placed

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* Diagnostische Praktische Beitrage zur Erweiterung der Geburtshulfe, Leipsig, 1803.

† Busch's Neue Zeitschrift fur Geburtskunde, vol. xvi.

the other in contact with the spine. From the one introduced in the uterus I had removed the brass handle, and twisted the wire upon itself so as to form a loop sufficiently curved to insure its remaining steadily in its proper place. I also carefully enveloped a considerable portion of this wire with lint, as well to protect the vagina from the twisted portion and extremity, as to prevent the galvanic current from being diverted from the uterus. I then increased the power until it produced "the most severe cutting pains in the loins," "great bearing down," and "a dreadful commotion in the womb." These were my patient's own expressions. This operation was repeated on the 9th and 10th, each morning for half an hour; the effect however, had not been as yet altogether satisfactory, as I had not been able to maintain a continuous action; but on the fourth morning, viz., the 11th, I remedied this defect, and kept up a continuous current for three quarters of an hour, when my patient begged me to desist, which I did, and determined to wait a few days to see if this might accomplish the desired effect. Happily on the 14th, without any further interference, labor commenced and terminated within four hours in the birth of a living child, and not a single untoward symptom occurred spontaneously. It was altogether a most satisfactory case.

The foregoing results are directly at variance with the opinion of Dr. Golding Bird, who says:

"The result I have arrived at is, that this agent, like the ergot of rye, and perhaps other *ecbolic* remedies, generally fails to develop uterine action *de novo*. Hence, though I believe it will generally fail to produce premature labor, it will as generally succeed in stimulating the uterus to vigorous contraction after labor has actually commenced."

In weighing this negative opinion, it should however be observed, that the latitude of qualification implied in the word "generally," deprives it of all precision of meaning.

It would lead me beyond my present purpose to discuss the relative advantages of galvanism and the douche, recommended by Dr. Kiwisch, and other methods. I will simply remark, that whatever method be determined upon for the purpose of bringing on labor, the stimulating property of galvanism on the uterus will be a most useful adjuvant.

I will briefly refer to the great superiority of this method over the use of ergot of rye. An unexpected obstacle to the expulsion of the fœtus may arise after the administration of ergot; there is consequently danger of rupture of uterus. How, for example, can we foretell that the child will not be driven into the pelvis in a transverse position? Secondly, there is the great improbability that the child will be born within any reasonable period after the administration of ergot; many doses are required; there is the risk of ergotism to the mother; and the peril to the child rises in proportion to the amount of ergot given; moreover, it is extremely uncertain whether the ergot will act at all.

The use of Galvanism in Inertia during the first and second stages of Labor.

I will now illustrate the effect of Galvanism in lingering labor from uterine inertia. An interesting case of this nature is recorded by Mr. Cleveland,* which was brought to a close within fifteen minutes after the use of the electro-galvanic apparatus had commenced. Mr. Houghton† also relates four cases of arrested labor from atony of the uterus, brought to a successful termination by the agency of galvanism. In three of these ergot had previously failed.

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* Medical Gazette, June, 1845.

† Dublin Quarterly Journal, February, 1852.

In a similar case I had myself experienced the like good effect, but I prefer citing the following account supplied to me by my friend, Dr. McKenzie:

Case 5.—"I was sent for one morning to a young woman who had been admitted in labor at the Paddington Infirmary, and on examination I found that the head presented. Although she had been several hours in labor the os uteri was but little dilated. I saw her in the course of the same afternoon, but still found very little dilatation. At 10 P. M. but little progress had been made. I now determined to try the effects of galvanism, and applied one pole of a single current machine to the spine, and the other, by means of Radford's director, to the neck of the uterus. The current was from time to time intermitted, and uterine action of a vigorous character was excited. In about an hour a fine living child was born. So vigorous were the expulsive efforts during the passage of the head through the os externum, that I was obliged to take particular pains to prevent rupture of the perinæum. The impression left on my mind by this case was, that galvanism should not be employed except very cautiously in primaparæ, or in any other instance in which the perinæum is rigid or imperfectly developed."

Galvanism may also be usefully employed in many cases of hemorrhages before the birth of the child.

A judicious application of this agent may, in many cases of arrest of the head from inertia, obviate the necessity of resorting to the use of the forceps

The use of Galvanism in the third stage of Labor and in Hemorrhage.

We possess a greater amount of evidence of the value of galvanism in the third stage of labor. Dr. Radford has contributed many valuable observations, exemplifying the power of galvanism in exciting contraction of the uterus in cases of post-partum hemorrhage. These are too well known to require to be cited. Mr. Houghton has added other cases which occurred under his own observation. The only instance I will adduce here is one which occurred recently to Dr. McKenzie.

Case 6.—The patient had been upwards of forty-eight hours in labor, under the care of Dr. Keogh, who had called in Mr. Clark, by whom I was sent for. When I saw the patient uterine action had entirely ceased, and I found, on examination, that the head was impacted in the pelvis, the face presenting with the chin to the left cotyloid cavity. As the patient was exhausted, an opiate had been given, and as she was disposed to sleep, we agreed to meet again in some hours, and if uterine action did not return, to deliver by the forceps. At the appointed time no return of uterine action had taken place. I applied the forceps; the operation was accomplished with extreme difficulty, and the woman was delivered of a fine, large, living child. I left the patient shortly afterwards, but the next day on meeting Dr. Keogh and Mr. Clark, I learned that great apprehension had been felt throughout the night as to the occurrence of hemorrhage, inasmuch as the uterus had remained flaccid and uncontracted, and at the time of my visit it had reached above the umbilicus and was very soft and flabby. I proposed galvanism and applied one pole to the spine and the other to the neck of the uterus, occasionally intermitting the current. This was done for half an hour, and evident uterine action was excited, the uterus becoming harder and smaller, and on removing the poles two large coagula were expelled. The next day the uterus was more contracted and smaller, and no hemorrhage had occurred. Galvanism was again used for half an hour. The uterus certainly contracted under its influence. The following day no hemorrhage had occurred, and the condition of the uterus was such as not to require any further recourse to the agent. The woman from this time recovered in a most favorable manner." Dr. McKenzie adds the following remarks, in which I entirely concur: "It appears to me that the results of galvanism in this case were highly satisfactory, because coagula retained in

the uterus, from atony of the organ, are not only calculated to occasion hemorrhage, but by undergoing a species of putrefactive decay, to give rise to fever and all the consequences of vitiation of the blood. Under such circumstances, I have known the hand forcibly introduced into the uterus many days after labor for the removal of such coagula, with very disastrous results —results which this case shows may be obviated by having recourse to galvanism."*

Other uses of Galvanism in Obstetric Practice.

There is another case of not unfrequent occurrence in obstetric practice, in which galvanism may be of eminent service—temporary paralysis of the bladder following delivery. A case I have already related illustrates the power of galvanism in causing contraction of the bladder. Doctors Goodwin and Radford† describe an interesting case, in which the catheter was employed two or three times a day, and could not be dispensed with. On Dr. Goodwin's suggestion galvanism was tried, and the first application proved successful.

I would especially recommend the use of galvanism in those cases in which the action of the uterus has been unfortunately paralyzed under the influence of chloroform. In such cases I believe no other stimulus that can be applied will answer with equal certainty or efficacy.

I am also sanguine as to the value of galvanism in exciting respiration in asphyxiated children.

There is another class of cases in which galvanism promises to be of the greatest service. A most interesting case has been recorded, in which Doctor Tyler Smith‡ was enabled to produce expansion of the neck of the uterus, and to bring an intra-uterine polypus into view, so as to admit of the application of a ligature, by the application of galvanism after ergot had failed. I have also employed it with success for the purpose of causing the expulsion of hyatids. This case occurred in connexion with my colleague, Mr. Forbes, and I will relate so much of the account as bears upon the question before us.

Case 7.—Ann W., aged forty-two, had had eight children and three abortions. She applied to Mr. Forbes on the 17th of June last, having anasarca of the legs. Two months before she suffered a burning pain in the region of the womb. She had menstruated up to Christmas last. Since that date there had been a little hemorrhagic discharge at intervals. For the last month there had been a continual discharge of colored fluid. Her health is much impaired and her strength lowered. On the 18th, while in bed, she felt a vaginal discharge, and on getting up passed a large quantity of blood. The pulse was weak, thready, 108; face blanched; headache intense. No pain preceded the hemorrhage. There was a tumor in the seat of the pregnant womb, extending more to the right side, and reaching to the umbilicus; it was firm and elastic, tender on pressure, which did not bring on labor pains. The os uteri was the size of a shilling and rigid. No placental murmur or sounds of fœtal heart heard. The breasts were quite flaccid. The os slightly expanded towards the afternoon. A dead fœtus, or some diseased condition of the ovum was suggested. In consultation, Dr. Barnes suggested galvanism to cause contraction; this had the desired effect and Mr. Forbes was enabled to bring down a bunch of hydatids. The vagina was then plugged and the abdomen bandaged. The disposition to contraction thus given, more hydatids were

* Further observations are required in order to determine the action of galvanism upon the fœtus in utero. Present experience however does not indicate that it exerts any injurious effects.

† Provincial Medical Journal, December, 1844.

‡ The Lancet, 1852.

afterwards passed. Tincture of ergot of rye was then given in small doses. Early on the morning of the 19th the patient passed a large mass of hydatids, which were expelled suddenly with a pain like that of labor. She was quite exhausted with loss of blood and previous disease; symptoms of inflammation appeared, and she sank the same night. The post mortem examination revealed a large fibrous tumor in the walls of the uterus, and an advanced stage of granular degeneration of the kidney.

In such a condition of the uterus and the patient, none of the ordinary means of exciting contraction could have been employed with equal safety and advantage. The necessity of inducing contraction to expel the contents of the womb and arrest the hemorrhage was obvious, and the utility of galvanism in accomplishing this was manifest. I am disposed to regret that the galvanism was not more freely used. The expulsion of the hydatid placenta might have been hastened.

It is beyond the strict scope of this paper, but I may be permitted to refer to the advantages attending the use of galvanism in amenorrhœa, hysteria and other diseases of females, advantages which have been clearly established by Dr. Golding Bird, Dr. Gull, and others. The stimulating influence of galvanism is well worthy of trial for the purpose of exciting the lacteal secretion.

Mode of Apylying Galvanism.

I have now gone through a series of illustrations, affording evidence of the use and value of galvanism in most of the forms of labor characterized by defective uterine action, and in other cases where the indication is to excite the contractile property of the uterus. I will conclude this paper with a brief description of the mode in which this powerful agent should be applied, and a summary of the advantages it especially possesses in obstetrical practice over ergot of rye. The ordinary electro-magnetic apparatus in use for medical purposes is, I believe, the best form that can be employed. The principle of this apparatus consists in the induction of magnetic currents by a current of electricity, and the production of a rapid succession of feeble shocks by continual interruptions to the current. I have observed that the uterine contractions are always provoked at the break and renewal of the circuit. Repeated shocks act as a far more effectual and certain stimulus to uterine contractility than a continued current. It is probably through inattention to this fact that some practitioners have failed in effecting contraction of the uterus by means of galvanism. As to the mode of applying the poles, I do not think it necessary to apply one over the spine and the other to the neck of the uterus, as is usually done. I have found the application of the discs, covered with thin flannel moistened in water, one on either side of the abdomen over the uterus, much more convenient and quite as effectual. The practice of applying one pole over the spine and the other to the neck of the uterus, further seems to me to be based upon an erroneous view of the mode in which galvanism acts upon muscular fibre. When the poles are thus applied, one to the spine and the other to the cervix uteri, it is doubtful whether the ensuing contraction of the uterus is due to primary excitation of the spinal marrow. It is proved by the experiments of Matteucci, and it is confirmed by general observation, that galvanism acts directly upon the muscular fibre, stimulating it to contraction. It is clear that this direct action can be as effectually obtained by passing the shocks through the uterus, by placing the poles on either side of the abdomen. I would not be understood to affirm that this immediate action of galvanism upon the muscular fibre is its sole mode of action, but that it is the primary and essential one; this primary peristaltic action commenced, the secondary and

tertiary diastaltic emotional and voluntary reaction upon the uterus follow.* The duration of the application must depend upon the requirements of the case. It is often found that nothing but a primary excitation is wanted, and that this being supplied, the uterus will go on contracting spontaneously. In those cases where it is required to originate uterine contraction, as in the induction of premature labor, several applications of an hour's duration will be necessary. The uterus cannot be roused to perfect action before the appointed time without repeated stimulation.

The special advantage of Galvanism as an Agent for producing Uterine Contraction.

Among the advantages of galvanism, more especially worthy of attention are—

1st. The simplicity of the operation.

2d. The extensive range of cases in which it may be successfully employed, rendering the electro-magnetic apparatus a desirable addition to the armamentarium of the obstetric practitioner.

3d. The perfectly manageable character of the agent. Its action may be broken off and renewed at pleasure. The moment we think the uterus is acting too powerfully under its use, we may instantly withdraw the exciting agency, and leave the uterus to the ordinary physiological stimuli, which seldom impel the organ to undue activity. It moreover admits of easy regulation; both the strength and duration of this agent are completely under our command. We have it in our power to imitate in a remarkable manner the natural pains, both as to intensity and intermission. Ergot has neither measure nor certainty.

* It will be seen from what has been said above, that I cannot concur implicitly in the views or conclusions of Dr. Golding Bird, expressed in the following quotation: "In the magneto-electric coil in which currents are excited by repeatedly breaking contact by a vibrating bar, we have two currents moving in opposite directions, to each of which the patient is submitted. Now these currents are of unequal strength, and if the most energetic—that on breaking contact—be passed in the direction of the vis nervosa, it will produce painful contractions, which, the moment it passes in the opposite direction, will become relaxed; for a direct current tends to produce contraction; an inverse current paralysis. Hence, I should urge the accoucheur not to employ the apparatus in which both these currents are produced, but simply the single current machine. In using this I would suggest the positive conductor to be placed over the lumbo-sacral region, and the other to be carried by gentle friction over the abdominal surface. In this way powerful uterine contractions may be easily produced."

Are we, when we place the positive conductor over the spine, and the negative one over the abdominal surface, warranted in as uming that we are passing a current in the direction of the vis nervosa? Are we so much as warranted in assuming that we are acting upon the contractility of the uterus through the medium of nerves at all? Conclusions such as these should be based upon demonstration. Has the uterus been isolated from all surrounding textures, preserving only its connexions with the spinal and sympathetic nervous systems; and have the effects of passing a "single current" along the nerves in a centripetal and centrifugal direction been observed? No. But has this been observed: that muscular fibre, both voluntary and involuntary, will contract under the galvanic stimulus when its relations with the nervous centres are severed altogether! The only legitimate conclusion from the known facts appears to be, that whether the shocks of the single or double current machines be passed through the uterus in one direction or in the other, whether through the lumbo-sacral region and the abdomen or cervix uteri, or from side to side of the abdominal walls, the uterine muscular fibre is stimulated and will contract. The observations of Matteucci, upon which the reasonings of Dr. Golding Bird appear to be built, apply more especially to the effects of very feeble currents upon the motor and sensitive nerves of the final system and the voluntary muscles.

4th. Its peculiar appropriateness and efficacy in cases of extreme exhaustion of the system, where deglutition is difficult or impossible, or where the stomach rejects every thing; where any other mechanical application to the uterus is dangerous or inconvenient, and especially where the introduction of the hand into the uterus would be likely to be attended by injury or even a fatal result. Indeed it may be truly said, that in cases of extreme exhaustion galvanism is the last resource left to us. The galvanic stimulus can be applied when every thing beside is out of the question. The uterine muscular fibre will respond to the stimulus when the nervous system is utterly prostrate, when the heart has ceased to beat, when the patient is moribund or even dead.

5th. Galvanism is less exhausting to the system than ergot or most other means of exciting contraction. It acts less directly upon the uterine muscular fibre and scarcely taxes at all the general powers of the system.

6th. It does not necessarily preclude or supersede the use of other remedies tending to fulfil the same indication.*

Devonshire Square, Oct. 1853.

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* An apparatus which combines to the greatest extent compactness, portability, and efficiency, is an especial desideratum to the obstetric practitioner. I know of no instrument that possesses these advantages to the same extent as that of Mr. Hearder, of Plymouth. This instrument is well made, it does not occupy one half the space of those usually sold, and while the maximum power is considerably greater, it admits, by the most simple means of accurate and minute graduation. This last quality, independent of its obvious utility in regulating the power according to the effect desired, is one which the scientific practitioner will appreciate, as affording the means of comparing his own observations with those of others. Further improvements in this machine are being effected, which will carry compactness to the furthest extent, and render it in every way admirably suited to the obstetric practitioner.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on the Eclectic Southern Practice of Medicine.* By J. Cam. Massie, M. D. Philad. Thos. Cowperthwaite & Co. 1854.

It was the intention of the late lamented editor of this Journal to have given a review of the work before us, in the January number; had the design been executed, its excellencies would doubtless have been more forcibly presented to the reader, and its defects and shortcomings (if any it have) would have received more thorough exposition than from the pen of the present writer, into whose hands, at the eleventh hour, the volume has fallen; with just time enough, however, and just space enough, we trust, to do it ample justice—resting our claims to unconditional pardon, if in doctrine, arrangement, matter, our views may sometimes run counter to that urbanity which of right belongs to authors who give us books for edification, blended with physic for our bodily infirmities.

It is seldom that we meet with books of Southern birth; upon the practice of medicine two or three physicians of the South have written and published; at the solicitation of personal friends Dr. Massie has published a volume containing 716 pages, the title of which we have already given. It is, in its character and doctrine, essentially *eclectic*—of the school *eclectic*, which the author in his preface avows to be "the true school of medicine," and which, in his opinion, "may now be said to reign" triumphant! In this department Dr. Massie will doubtless be found to be an energetic and pains-taking laborer—an unpretending yet warm advocate; as such we are bound to regard the work under consideration as written more for the advocacy of some new theory in medicine than as relating to "the practice as especially adapted to the peculiar manner in which diseases manifest themselves in Texas," or out of Texas; for we have searched in vain the pages

before us for any *substantive* peculiarities of diseases of the South, aside from the fevers which impressed Dr. Massie with the importance and the necessity of placing before the profession his chosen practice. Had Dr. Massie informed us in setting forth the plan of his practice of medicine, that it was his intention to dive into the department of Surgery and Midwifery in order to secure "the consciousness of having endeavored to be useful" in his undertaking, we should at once have called the title a misnomer, and have been ready to suggest a separate volume upon each subject; but having no intimation of this kind, fancy our surprise upon finding Gonorrhœa and Syphilis following closely upon "Dropsy of the Cellular Membrane," and diseases of the Ear in turn being the antecedents of Amenorrhœa and Dysmenorrhœa! But so it is. Verily "truth is sometimes stranger than fiction."

In the reply of the author to the friends at whose suggestion the present work was undertaken, we find him expressing his opinion upon the manner in which it ought to be written, in these words, "In short, a book which would be a desirable addition to the library of the practitioner, and moreover a valuable aid to every father of a family"—our surprise then is vastly heightened as we proceed with the subject of Gonorrhœa and read the following passages: "The first consideration in regard to this disease is, to institute an inquiry, and if possible, ascertain how it can be prevented. The condom, if perfect, is of course an absolute preventive. To insure this, however, it cannot be used the second time without having been thoroughly washed in a soap of a very costly kind. Persons who use these instruments, which in special cases are recommended by surgeons, both to prevent conception and infection, purchase them by the package and run no risk from the second use."

And is it a book in which such subjects are treated, and upon the pages of which such a sentence as this last is to be found, that the author would think a valuable acquisition and guide to the head of every family—and upon which the sons and daughters might satiate a prurient imagination with baneful delight?

We should imagine that least of all would fathers of families desire to be instructed upon the subject of Gonorrhœa and Syphilis, (aside from the employment of a means which refinement and morals most sternly repudiates) when religion, modesty and honor preclude them from being the participants of such loathsome contagion! We can understand that heads of families should take pleasure in considering medical science chaste, whilst they would be scrupulously careful not to unveil every avenue of its mystic pathways.

Besides, the article referred to is not "an absolute preventive;" it

frequently serves only to drive the enemy to another quarter for attack. Ricord observes, "it fulfils the office of a bad umbrella, which the tempest may rend, and which, protecting badly enough from the storm, does not prevent the feet from getting wet." Or it may be, as a woman with great *esprit* has remarked, "a cuirass against pleasure and a cobweb against danger."

Thus much upon the doctrine, arrangement, matter—in which are embraced the principal novelties of the book. Upon the pathology of disease and the morbid changes which take place in the various fluids and solids of the body, we find a marked deficiency; upon the appropriateness of the treatment of given diseases, we are not competent to form an opinion, the agents employed for the most part being such as Eclectics, Thompsonians, &c., have made essentially their own.

The number of articles which enter into many of the prescriptions which Dr. Massie has given, being as many as eight or ten, if each have efficacy, we should say were vastly too numerous. A little philosophy, little medicine, based upon correct diagnosis, being far more likely to meet the indications of the case. That this is no fanciful allusion, we will give an instance or two, taken from the article on Consumption, which the author informs us he has used with advantage:

"℞ Elecampane.
Asclepias, aa 1 ℔. (Pleurisy Root.)
Macrotrys. (Black Cohosh.)
Comfrey.
Eupatorium Perfol. (Boneset.)
Hoarhound.
Sycamore Bark.
Iceland Moss, aa ½ ℔.
Sanguinaria, ¼ ℔. (Bloodroot.)"

Or—

"℞ Spikenard.
Hoarhound.
Elecampane.
Comfrey, aa ℥ iss
Caulophyllum. (Blue Berry.)
Macrotrys. (Black Cohosh.)
Ictodes, aa ℥ i. (Skunk Cabbage.)
Eupatorium Perfol. (Boneset.)
Lycopus. (L. Virginius.)
Ampelopsis, aa ℥ ij. (Bryonia.)"

Lest we should be chargeable with showing partiality for *round*

numbers in the art of prescribing, we will transcribe one other, which the author has employed with like benefit in the same disease :

" ℞	Hydrocyanic Acid,	gtt. xij.
	Syrup of Tolu,	℥ ss.
Then take	Powdered Gum Arabic	℥ ss.
	Water, fl.	℥ viij.

Dissolve the two last and add the former.

Mix. A table spoonful every three hours."

To this, as a palliative, we cheerfully say very good, with the correction of the acid, Hydroscy. Offic.; since, to have used Scheel's Prussic Acid pure, in the quantity and in the dose prescribed, would, in most cases, speedily have destroyed life.

Much of the treatment of this disease, as suggested by Dr. Massie, we must regard as peculiarly inappropriate ; for instance, it is stated that " Emetics, or remedies given to nauseate, are particularly recommended ;" and so is tincture of assafœtida for " inward pains," and Holland Gin for " dropsy of the head." Yet may we call in question the authority, nor tremble if we doubt the efficacy. The author of the Southern Practice should surely have stated that it was only in the *early* stage of Phthisis that emetics could be (if at any time) admissible, and then only theoretically, it being mere assumption that they can remove tubercle when formed. Those who have watched with the utmost solicitude the *rise and progress* of this insidious, yet too fatal disease—who amid hope and suspense, anxiety and dread, have seen the slow and withering decay of something that on earth was highly prized—may readily be pardoned if they turn in haste from that which must be designated—*puerile.*

In the treatment of the various types of fever, Dr. Massie is generally more judicious, and but for his *erratic* propensities, (which we are most willing to believe have their origin more in a proneness to *dissent* from established principles, than from an antipathy to the principles themselves) would, we think, meet with general approval. As illustrative of the wholesome opinion entertained by Dr. Massie, we cannot omit to quote from the 78th page, on " Fevers in General." "Our own impression is, whether in health or out of health, that in this climate the general system, from the effects of heat, is so much relaxed, that the least change of weather predisposes us to ague ; and it is under this head that, with feelings of some reluctance, I feel it to be an imperative duty to state, that in this climate our systems require a stimulant."

Farther comment upon Dr. Massie's book may be needless ; besides

our allotted space is well nigh consumed. How then shall we conclude It cannet be expected that we should

> "Next the great orators consult, and thence
> Draw all the moving turns of eloquence,"

in lavish praise of our author, who is even less known to us than to fame; but we may of right express an opinion boldly, fearlessly, pointedly (and in these reside, if we mistake not, the essence and embodiment of written thought that were worth the reading) upon that which is for the medical world—medical; or upon that which, purporting to be medical, is non-medical.

The theory and practice of Dr. Massie is evidently Eclectic; as such we are willing to believe that it is the best which has been published, and transfer it to the school which bears that name for its verdict. To the medical profession at large, we think the "Eclectic Southern Practice" will be made available when the pages of numerous standard works upon the practice of medicine shall be effaced by time and their prototypes mouldered away—but not *till* then. G. T. B.

New Orleans, Feb. 2d, 1853.

II.—*On the Etiology, Pathology and Treatment of Fibro-Bronchitis and Rheumatic Pneumunia.* By Thomas H. Buckler, M. D. Philadelphia, Blanchard & Lea. 1853. pp. 150. 8vo.

A new volume treating upon diseases of the chest, and particularly upoe one class which has been so carefully and scientifically investigated by M. Grisolle, when advancing new views should be received with great caution. From the writings of Bouillaud we first learn the coincidence of rheumatism with pleurisy and pneumonia; but in no writings do we find any positive mention of this coincidence or association with bronchitis. In this essay we see it classed with bronchitis, and the object of the work, in the author's words, "is to point out, as clearly as possible, the distinctive characters of fibrous or rheumatic inflammation of the bronchial tubes, and at the same time to show the differential diagnosis between it and ordinary catarrh." Its seat is in the fibrous coat of the bronchial tubes and not in the mucous lining, and for the purpose of marking this distinction the word rheumatic is used. Rheumatic pneumonia with the author "is never idiopathic, but occurs

as a secondary lesion, and is always symptomatic of, and directly dependent on, pre-existing bronchitis."

If rheumatism affects the serous, fibrous and cartilaginous tissues—if we find the fibrous coat of the bladder attacked by this disease, or the intestines the seat of the disease, as it was supposed by Stoll, we may clearly reason that the fibrous coat of the bronchial tubes would not be exempt. Most organs have had their different tissues separated and the diseases of each assigned to them. It is only of late years, however, that the diseases of the eye were known under any other name than the general one of opthalmia; now, each tissue has its diseases separately designated on the nosological table. Not so with the bronchial tubes; this is the first author who has associated inflammation with any other than the mucous membranes.

Consistent with these views, we here find bronchitis divided into three forms—mucous, fibrous or rheumatic, and fibrous or gouty—either of which may be acute, sub-acute and chronic.

Before showing the relation between fibro-bronchitis and rheumatic pneumonia, the "Vascular Mechanism of the Pulmonary Circulation," which is the heading of the second division of the work, is explained. The lungs have two distinct circulations; that which performs the oxidation of the blood, the depurative circulation; and that which supplies the nutrition of the parenchyma and to the bronchial tubes. The first returns the oxygenized blood to the heart by the pulmonary veins, while from the bronchial tubes and the pulmonary parenchyma the blood is returned to the general circulation by the corresponding venous circulation, as in other portions of the system.

In ordinary pneumonia, the capillaries becoming congested, there is an afflux of blood towards that part, which prevents, by the closure of many of the air cells from the increased flow of blood, the proper depuration of this fluid; the retained elements, especially the carbonic acid, adds to the difficulty, the terminal branches of the nutritive arteries pour out their plastic lymph, the serum is pressed out into the adjacent areolar tissue. according as the stasis becomes more complete and seriously established, and thus a portion or the whole of the parenchyma of the lung may become engorged.

Such being the explanations of ordinary pneumonia, the author, by analogous causes, attempts to explain the origin of rheumatic pneumonia, and shows the link of union between it and fibrous or rheumatic bronchitis. "Having explained the mode in which the vascular lesions take place and the order of their occurrence in ordinary pneumonia, it is well to remember that the fibrous tissue of the bronchi is tra-

versed solely by the minute branches of the nutritive arteries, and that where symptomatic pneumonia happens as a consequence of fibrous bronchitis, the order in which the vascular lesions take place is precisely the reverse of their occurrence, as already pointed out, in simple uncomplicated pneumonia. Insoluble uric acid, or its compounds, phosphates, or the extractive matters found in the urine, not being eliminated from the blood, are deposited in the meshes of the fibrous tissue, exciting nervous irritation, followed by vascular lesions, exudations, transfusions,and all the general phenomena of rheumatic inflammation. This process having set in, a symptomatic remora of blood takes place in the depuratory capillaries belonging to the inflamed bronchi, and a congestion, leading to an engorgement, reaching the first and second stage of pneumonia, and rarely going beyond it, often takes place. This form of inflammation is propagated from the fibrous tissue of the bronchi, both by contiguous and continous sympathy."

What is this element called rheumatic, is the issue upon which this essay turns.

It would seem from the next section of this monograph, that certain phrases in the works of writers from the earliest ages to the present time furnish indications that such an element was perceived by them, but none had well observed its influence in the production of pneumonia.

The rheumatic element is that which directly acts in producing the diseases under consideration, and rheumatic inflammation generally in any of the tissues affected by rheumatism. In discussing this subject, four forms of rheumatic inflammation are mentioned, which are induced by as many several causes.

"1st. It is believed that the most common producing cause of rheumatism is the presence in the blood of insoluble lithic acid and lithate of soda, which salts being arrested in the terminal blood-vessels supplying the white tissues, act as irritants, and thus become the primary link in the chain of morbid phenomena constituting, so far as this cause is concerned, one form of rheumatic inflammation.

"2d. It is believed that rheumatic inflammations of another class depend upon the retention in the blood of large quantities of nitrogenized matter, which is eliminated during the healthy performance of the various functions, almost exclusively through the excretory axhalants of the skin.

"3dly. There is a form of rheumatism depending on the abnormal presence of earthy phosphates in the blood; and under these circumstances, an excess of the triple phosphates of lime, soda and magnesia

will often be found in the urine, but not uniformly; the solubility of these salts, and consequent capacity of the kidneys to eliminate them, depending in a great measure on the proportion of phosphoric acid united with the earthy bases.

"There is still a *fourth* variety of rheumatism, depending, it would seem, upon the presence in the blood of those compounds which are found in the urine, and called extractive matters, the chemical composition of which is not yet ascertained."

The report of 11 cases follows this exposé of the author's views—cases which show the element called rheumatic to be predominant, and that the treatment essential to the elimination of this element was that best adapted. In all these cases rheumatism was either one of the antecedents, or else was developed during the course of the disease. In all, the peculiar salts common to the rheumatic diathesis were found in the urine, and in most the auscultatory sounds of the heart furnished additional evidence of the character of the disease. An analysis of 16 other cases is given, drawn from different sources, where the characteristic rheumatic element is shown to exist.

Nothing new is presented in relation to the treatment. The object is to elucidate the etiology and pathology of thoracic disease, the details of which we have attempted to present to our readers, as they are developed by the author.

It is seen by this brief analysis of this monograph, that starting with the proposition that ordinary idiopathic bronchitis never induces pneumonia, a cause is sought for its occasionally consecutive appearance. The rheumatic element, "or the presence of insoluble lithic acid or lythate of soda the most common cause of rheumatic inflammation, or the insoluble extractive matter of the blood" is the excitor. These detained in the capillaries become the irritants which produce the afflux of blood, from which follows that chain of phenomena we have shown as occurring in ordinary pneumonia. As long as these salts remain in the blood and are detained in the capillaries, so long will the disease persist in its course. A solvent for these salts is sought, and the alkaline treatment conjoined to the ordinary treatment, is that found best adapted to the occasion.

Several years ago the same author was an advocate for the exhibition of phosphate of ammonia in rheumatic diathesis, as being the best solvent for those salts, which he esteemed the morbid elements of the disease. This opinion, he says in this essay, has been confirmed by the testimony of Dr. Bird. It is this salt he especially recommends in the treatment of the disease now under consideration.

We would recommend this monograph to our readers. The views,

though novel, are yet interesting, and not without a foundation in pathology and animal chemistry. Many cases of troublesome bronchial disease can doubtless be relieved by the alkaline treatment, and much annoyance be saved to the physician.

"If these observations *should* be verified by others," then will this essay, which the author puts forward as a rejected address, be a valuable contribution to practical medicine, in having more closely established the pathology and treatment of that class of diseases which has already been so well revealed by pure scientific research and experience.

J. H. D.

III.—*History of the Epidemic Yellow Fever at New Orleans in* 1853. By E. D. Fenner, M. D. New York, 1854. Pamphlet. pp. 84.

Inasmuch as it was the intention of the late Dr. Hester to have added this entire work to the January No. of the Journal, which was prevented by the unexpected delay of its appearance, we have thought it advisable to give a brief summary of the chief points set forth by the author. This will furnish a satisfactory idea of its scope to those who may wish to have it sent to them by mail.

The work opens with a notice of the author's efforts for the last ten years to lay before the profession and the public full and faithful accounts of the Yellow Fever that has prevailed in New Orleans and the region around. He deplores the indifference to the subject displayed by the community, and deprecates, as altogether erroneous, the course pursued by the city authorities, newspaper press and influential citizens.

The history of the late Epidemic commences with a general account of the weather from the beginning of the year down to the 1st of October. Then follows an account of the *sanitary condition* of the city during this time, which is shown to have been very bad. Then comes a minute and circumstantial account of the first 26 cases that occurred in the various quarters of the city. The author now reviews the facts observed in *ten different localities*, and says—"if any one can trace any sort of connection or communication between the first cases of the disease as they appeared in the different localities I have pointed out, or any thing like the *gradual spread of an imported contagion or infection from one or more points to the region around*, I can only say it is more than I have been able to do." He then points out particularly the facts bearing on the supposed connection between this Yellow Fever and that

of Rio Janeiro. Then follows extracts from the author's Diary and the newspapers of the day showing the progress of the epidemic, the different types of fever prevailing and the mortality, &c., to its close. He says the Epidemic prevailed from 60 to 70 days, which is the customary period of Yellow Fever epidemics in this city. He notices particularly the fact that "*the Epidemic prevailed in the unpaved and of course, least improved parts of the city all around long before it did in the central and best improved parts.* We then have remarks on "*the general character of the disease*"—"*special observations,*" and the *Treatment.* Then we find *Statistics of Fever*, made up from the living and the dead, showing the various types of fever that prevailed at the time. We then have reports and statistics from the Hospitals, Asylums, Prisons and Benevolent Societies of the city.

The "*Mortality of the Epidemic*" is then given, with estimates of the probable *ratio* of deaths to cases, and cases to population. The author now again reviews all the facts he had presented, and concludes his account of the Epidemic with some remarks on Quarantine and the sanitary measures that promise the most effective protection against future Epidemics.

Addendum.—The author has added to his history some interesting extracts from "*The Second Report of the General Board of Health of England on Quarantine in Yellow Fever,*" which are worthy the profound consideration of legislators and city authorities, whose duty it is to guard and protect the lives and property of the people.

The author of this pamphlet has the gratification to learn just now that the Louisiana Legislature, at the instance of their *Joint Committee on Quarantine*, has ordered a large number of copies for the use of the members. Will not the Legislatures of Alabama and Mississippi do the same, as they are equally interested?

The price of the pamphlet has been reduced to 25 cents and that in muslin to 50 cents, with a view to extending its circulation as far as possible.

The New Orleans Medical and Surgical Journal.

Vol. X.] NEW-ORLEANS, MARCH 1, 1854. [No. 5.

CHANGE OF EDITORS.

With the present number the undersigned closes, for the second time, his editorial connexion with the New Orleans Medical and Surgical Journal. From the fact of his having been one of its original founders, he confesses to have ever felt a sort of *parental* interest in its successful progress. Indeed, in accordance with the wish expressed by many subscribers, he was willing again to become its Proprietor and Editor, if this could have been effected upon reasonable terms; but it has been ordered otherwise, and the undersigned can only express the hope that the Journal may continue to enjoy the favor and patronage that were extended to its late lamented Editor and Proprietor. As evidence of his interest and good-will in its behalf, he will continue to contribute to its pages as circumstances and his leisure will permit, and hopes all the former correspondents will do the same. He is gratified to know that the editorship is entrusted, for the future, to such able hands as Dr. Bennet Dowler, a name too well known to the medical profession of the United States to require either introduction or endorsement on the present occasion. There can be but little doubt that the Journal will be conducted with zeal and ability by Doctor Dowler, and, we would fain hope, with entire satisfaction to its patrons. The appeal to those indebted that was made in the last number, in behalf of Mrs. Hester, we trust will not be forgotten. Let all who are in arrears promptly pay up, and she will thus be enabled to employ men of talent to conduct her Journal.

After this number the Journal will pass into the hands of Dr. Dowler, as will appear from his salutatory address.

The undersigned takes this occasion to renew to the readers of the Journal the assurance of his grateful acknowledgments for the many marks of favor and esteem he has received at their hands.

E. D. FENNER.

TO THE FRIENDS OF THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

The unexpected death of the lamented Dr. Hester—which deprived the medical profession of an honored member—a young wife of an adoring husband—this Journal of a talented Editor, has, in the course of events, placed the undersigned in the perilous, responsible, and unsought position of conductor of a Journal hitherto deemed a reliable representative and able expositor of the

art of Healing, emanating from the great medical, not less than the geographical centre of the South-Western region of the Republic—a region of vast expansions wherein the climate, races, and diseases are scarcely less marked with peculiarities than its topography, geology, fauna and flora.

While the incoming Editor claims for himself no exemption from prejudice, bias, and error, he has the wish, the will, and the expectation to make this Journal, as far as in him lies, an independent, impersonal, disinterested, and catholic one in which the friends of science may meet as on a common platform —instructing and receiving instruction—conferring and receiving honors, by promoting the best interests of humanity, namely, the healing of the sick.

The friends of science who may honor this Journal with their contributions, will, doubtlessly see the propriety of avoiding whatsoever may tend to excite sectarian and partizan influences—personal, sectional, and national prejudices.

The science of Medicine, affiliated as it is with many collateral sciences, affords ample scope to its cultivators without the necessity of descending to attack quacks, cliques, and mercenary practitioners who sacrifice truth, honesty and science, without scruple, for the love of money, or, a love of that species of notoriety which is ephemeral even for its purpose, but lasting for its moral degradation.

New Orleans constitutes the key-stone city medically, not less than geographically, of the vast littoral arch washed by the Gulf of Mexico, which trends from the Rio Grande, deeply indenting the continent, resting on the Atlantic, presenting a varied climate.

Correspondents will favor this Journal by communicating with all possible brevity accurate accounts of the climatic and sanitary condition, the medical topography, the endemic and epidemic diseases and the modes of treatment prevalent in the towns and rural districts within the circle of their observation and practice, with special reference to natives, strangers, and acclimating processes, and medical police and jurisprudence—including the hygiene, the anatomical characteristics, diseases, medical treatment of the Indian and the black races, both pure and mixed—with the medical jurisprudence pertaining to the redhibitory vices and maladies of slaves, both absolute and relative, mental and corporeal, as defined by the Civil Code of Louisiana, by legislative enactments, and by judicial decisions. In this department of Forensic Medicine, it is hoped that some of our learned jurisconsults and physicians will confer a benefit upon millions of their compatriots in the Southern States by practical contributions to the pages of this Journal, tending to illustrate and determine questions of high import to the public and of no little difficulty to the medical witness.

In the mere business affairs of a Medical Journal the public can seldom feel much concern. All avowals of disinterestedness are received with a good de-

gree of incredulity as in the other affairs of life, in which self-interest is pretty sure to predominate. In the present instance, it may be acceptable to the friends of the late Dr. Hester to know that his accomplished widow is the sole proprietress of this Journal. A similar case does not probably exist. She has appointed as her Agent, Mr. W. P. Johnston, an able accountant and a gentleman of integrity, at the Picayune office, 66 Camp street, New Orleans, to whom all letters and remittances must be directed. Instructed to that effect by the succession of the late Dr. Hester, the undersigned on this occasion departs so far from the duties of editor as to call the attention of the delinquent subscribers to this Journal, to the necessity of settling up all arrears without delay. This is at the least one of "woman's rights" about which there can be no dispute, and which every man of honor will respect. The amount of outstanding debts due the Journal is great, increasing, and must be diminished. Let this appeal suffice. Justice is great. Conscience is omnipotent among true Æsculapians. Publishing is costly in New Orleans. Pay! And, having paid, let each obtain a new subscriber, as honest and as punctual as himself,—whereupon, he may soon expect to see the New Orleans Medical and Surgical Journal enlarged, and illustrated with engravings, and able to pay its contributors for their valuable writings. This proposal, so easy of execution, if carried into effect at once among the rich States washed by the Gulf of Mexico, would at the same time enlarge the sphere of the Journal's usefulness and enhance its value in a scientific point of view. A Doctor who half starves his Journal, or his horse, is unwise on the score of economy.

The acceptance of Articles for this Journal must not be construed by the reader as implying the approval or disapproval by the Editor; he will not accept any article which he has sufficient reason for believing to be false in fact, immoral in import, or injurious to the public and the profession. Articles valuable for their facts, though defective in orthography and style, will be considered, revised, and arranged for publication in strict accordance with the writer's views, without the editor's assuming the responsibility of either the facts or opinions which may be advanced on any occasion.

All articles provisionally accepted will be acknowledged, if not published immediately; others, may be considered as not accepted, and may be reclaimed within two months.

Papers of moderate length, having a practical bearing, will be preferred for publication in this Journal.

Editor's office, No. 30 Camp street, or residence, Tchoupitoulas street, between Louisa and Poyfarré streets, New Orleans.

BENNET DOWLER.

March 1, 1854.

YELLOW FEVER.

As this disease has attracted a great deal of attention of late and we had on hand a number of communications on the subject, we thought it best to insert them all in this number, for the convenience of future reference. This is done at the risk of tiring the reader and the sacrifice of many interesting extracts from our exchanges, but we trust the step will be approved. The next number will probably contain a greater variety of matter.

HEALTH OF THE CITY.

We stated in our last issue that our city was remarkably healthy at that time. The winter is now nearly over and we may say we have but seldom witnessed less sickness at this season. True, we have had some scarlatina, measles, chest-complaints and bowel-affections, but hardly so much as usual. There were 21 deaths from cholera during the month of January and only 23 deaths from fevers of all types. About the 4th of February there were seven or eight cases of cholera admitted into the Charity Hospital, mostly blacks, but some of them white. These cases did not originate in the city, but were brought in from different directions—some down the Red and Mississippi rivers, others were sailors from Boston and New York. The disease did not spread at all in the Hospital, though no precautions were taken to prevent it. Diarrhœa was quite common in the city at that time, but of very mild and tractable form. There has been but little sickness up to this date in February, (24th).

The weather since the 1st of January has been remarkably changeable; sometimes very warm, again very cold. There were several white frosts in January, and quite a freeze on the 22d. Icicles were seen hanging from the eves of the houses, but the ground was not frozen. There was rain on five days in January, and on six in February, with a good deal of damp, cloudy weather. On the 16th of February there was a light sleet for an hour or two.

The streets have been very muddy and filthy. There is no Board of Health and no special attention is paid to the sanitary condition of the city. With characteristic indifference this important matter will probably continue to be neglected till the city is visited by another epidemic of some kind, when our Aldermanic sages will again be roused from their lethargy and spend large sums of money in vain efforts to stay its progress. It is to be feared that our ill-fated city is yet to be scourged many times before her Councilmen become convinced of the importance of sanitary measures. They are now disposed to rest their chief hopes of safety upon the efficacy of *Quarantine* to prevent the introduction of disease from abroad, but if they would only examine their home-premises with discerning eyes, they would find sources of danger much

more to be feared. When they have removed these, if the city should still be visited by pestilence, they may then look abroad for its causes.

The subject of Qurantine at this time engages the serious attention of our Legislature. The experiment may be tried, but we confidently predict that it will never protect this city against Yellow Fever, whilst its streets, allies, vacant lots, &c., are suffered to remain in the filthy condition in which they are at present.

MORTALITY.—We here insert the Report from the Cemeteries for the entire month of January, collated by Dr. J. C. Simonds, Secretary to the late Board of Health. This is all that has been done, the weekly reports in the city papers having long since been discontinued.

Report of Interments in the Cemeteries of New Orleans during the month of January, 1854.

II.—Cholera, 21; diarrhœa, 11; do chronic, 3; dysentery, 9; do. chronic, 3; fever, 2; do intermittent, 1; do remittent, 1; do congestive, 1; do pernicious, 2, do typhoid, 9; do typhus, 6; do ship, 3; catarrh, 2; cholera infantum, 1; croup, 5—80.

III.—Whooping cough, 4; measles, 5; scarlatina, 11; small pox, 2; varioloid, 1.--23.

IV.—Marasmus, 2; teething, 5; debility, 5; decline, 5; anasarca, 1; dropsy, 7; hemorrhage, 2; gangrene 1; asphyxia, 1—29.

V.—Apoplexy, 8; congestion of brain, 5; cerebritis, 4; meningitis, 2; phenitis, 1; encephalitis, 2; cerebral fever, 1; hydrocephalus, 5; effusion of brain, 2; disease of brain, 2; softening of do 1; epilepsy, 2; convulsions 22; cramps, 3 tetanus, 3; lockjaw, 20; paralysis, 2—85.

VI.—Bronchitis, 5, pleurisy, 2; pneumonia, 8; consumption, 65; hydrothorax, 1; asthma, 3; angina, 1; pulmonary disease, 8—96.

VII.—Pericarditis, 1; aneurism of heart, 1; disease of heart, 1; dissolution of blood, 1—4.

VIII.—Gastritis, 2; gastro enteritis, 3; enteritis, 11; colic, bilious, 1; indigestion, 1; hematemisis, 1; congestion of bowels, 1; intestinal consumption, 1; tympanitis, 1; cancer of stomach, 1; peritonitis, 2; ascites, 7; worms, 1; jaundice, 1; liver, inflammation of, 1; do abscess of, 2; do hypertrophy of, 2 —39.

IX.—Bright's disease, 1.

XI.—Cancer of breast, 1; metritis, 2; childbed, 4; puerperal fever, 1—8.

XII.—Rheumatism, 1.

XIII.—Scurvy, 1.

XV.—Old age, 9.

XVI.—Still-born, 15; premature 2—17.

XVII.—Fracture of scull, 2; contusion, 1; accident, 2; traumatic tetanus, 2; drowned, 4; burns and scalds, 5; cold, 1—17.

XVIII.—Want of care, 1; wound of head, 1; hydrophobia, 1—2.

XIX.—Delirium tremens, 6; intemperance, 2; suicide, 1—9.

Zymotic diseases, 103; sporadic do 284; external causes, 29—416. Not stated 79. Total, 487; of whom were reported as males, 295; females, 174; not stated, 18—487.

Whites, 384; colored, 88; not stated, 15—487.

Under 5 years of age, 183; from 5 to 15 years, 32; do 15 to 60 years, 202; over 60 years, 23; not stated, 47—487.

Interments on each day in January, 1854.

Jan.	Jan.	Jan.
1.....14	11.....7	21.....20
2.....19	12.....11	22.....14
3.....19	13.....17	23.....17
4.....21	14.....16	24.....18
5.....10	15.....13	25.....24
6.....16	16.....21	26.....18
7..... 8	17.....11	27.....14
8.....11	18.....11	28.....10
9.....15	19.....14	29.....21
10.....11	20.....16	30.....15
		31.....18

Total.....	470
Date not stated.....	17
Total.....	487

RECAPITULATION.

Interments in the cemeteries during the month of January:

Protestant, Girod street, 34; Hebrew, Metairie Ridge, 1; Odd Fellows' Rest, 8; Cypress Grove No. 1, 28; Cypress Grove No. 2, 58; Charity Hospital, 46; St. Patrick's, 84; Catholic No. 1, Second District, 8; Catholic No. 2, Second District, 45; St. Vincent de Paul, Third District, 78; Lafayette, Fourth District, 97. Total, 487.

J. C. Simonds, M. D.,
Secretary of the Board of Health.

LOUISIANA STATE MEDICAL SOCIETY.

We hope the physicians of Louisiana will bear in mind that the State Medical Society, instituted in 1850, will hold its regular annual session in this city on the second Monday of March. A large delegation from the country would be cordially welcomed by the physicians of the city. The reports of the standing committees are expected to possess increased interest this year. The recent visitations of pestilence have in some degree served to impress upon the people the importance of having an able medical faculty to protect them, and there can be no doubt that if the State Society were sustained with proper spirit and energy it would soon be productive of much benefit.

DR. MARSHALL HALL.

In the month of January we had a visit from this distinguished physiologist and London practitioner, which will long be remembered by those who had the pleasure of making his acquaintance. Dr. Hall is a man about 65 years of age, of short statue, but quite robust and active. If we mistake not, he has the

reputation of having an irritable temper, though this character is by no means indicated in his bright, cheerful and apparently amiable countenance. We found him remarkably sociable and unassuming in his manners. Whilst here he delivered two lectures on the nervous system, which were listened to with profound attention by a large audience of physicians and medical students. He also performed some experiments on the alligator, illustrative of his peculiar views of the physiology of the nervous system, which appeared to be conclusive and satisfactory to most of those who witnessed them, but not to *every one*. The animals were rather small, besides being somewhat torpid at this season of the year. We have full reports of Dr. Hall's two Lectures, which we had intended to publish in the present number, but are compelled to reserve them for want of space. They are highly interesting and of great practical value.

On the 18th January the physicians of New Orleans gave Dr. Hall a splendid dinner at the St. Charles Hotel, which was characterized by the utmost harmony, good feeling and vivacity. We had prepared a full report of this festival, which must be excluded.

Dr. Hall left us for Havana on the 23d January, expressing much gratification at his visit and carrying with him the good wishes of a host of new friends in this quarter.

THE MAYOR OF BOSTON, A PHYSICIAN.

We are gratified to learn that Dr. J. V. C. Smith, Editor of the *Boston Medical and Surgical Journal*, has been elected Mayor of this enlightened city. In this the people have displayed their wonted good sense as well as their just appreciation of private worth and professional merit. It would be well for all the cities of the Union if they had such men as Dr. Smith for Mayor. One other important office should always be confided to an intelligent physician, that is the *Coronership*.

We learn through a note from Dr. Smith that he has taken in Dr. Jones as assistant Editor of the Journal. This is the gentleman who conducted it whilst Dr. Smith was absent on his European and Eastern tour two or three years ago, and performed the duty very well indeed. We heartily wish continued success to this useful Journal.

ALABAMA STATE MEDICAL ASSOCIATION.

This Association met in the city of Montgomery on the second Tuesday of January, 1854, and after a pleasant and interesting session of three days, adjourned to meet in the city of Mobile on the second Monday in December next. Officers for the present year:

L. H. Anderson, President.
J. C. Harris, 1st Vice President.
G. E. Redwood, 2d " "
F. A. Bates, 3d " "
G. A. Ketchum, Cor. Sec'y.
R. Miller, 1st Rec. Sec'y.
J. C. Marks, 2d " "
W. P. Reese, Treasurer.
W. Taylor, Orator.

The Association is in a prosperous condition, its course is onward. It is to be hoped that soon all the worthy members of the profession throughout the State will be numbered among its "Fellows."

MEETING OF THE AMERICAN MEDICAL ASSOCIATION.

(We insert with pleasure the following notice, and sincerely hope the Medicat Profession in the South and West will be extensively and ably represented on the occasion referred to. *Ed.*)

The seventh annual meeting of the American Medical Association will be held in the city of St. Louis on Tuesday, May 2d, 1854.

The Secretaries of all Societies and all other bodies entitled to representation in the Association, are requested to forward to the undersigned correct lists of their respective delegations as soon as they may be appointed, and it is earnestly desired by the Committee of Arrangements that the appointments be made at as early a period as possible.

The following are extracts from Art. 2d of the Constitution:

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

ganized medical institution of good standing shall have the privilege of sending one delegate."

"Delegates representing the medical staffs of the United States Army and Navy shall be appointed by the chiefs of the Army and Navy Medical Bureaux, The number of delegates so appointed shall be four from the Army Medical Officers and an equal number from the Navy Medical Officers."

The latter clause, in relation to delegates from the Army and Navy, was adopted as an amendment to article 2d of the constitution, at the last annual meeting of the Association, held in New York in May, 1853.

E. S. LEMOINE,
One of the Secretaries—Saint Louis.

The medical press of the United States is respectfully requested to copy the foregoing.

ERRATA,

In the second Original Article of this number.

The author of this article has pointed out the following errors, which we are glad to be able to correct so soon. In the first place the author's name is mis-spelt. It is Gorrie, instead of Garrie.

At page 585, the 5th and 6th lines should read—"They are considered as retarding the progress of philosophy and keeping it in the rear of all other sciences." At page 590, 20th line from the top, read *combination* for "*combustion.*" At page 592, 13th line, read *have*, instead of "*gave.* Same page, 14th line, add *afforded* after "*has.*" At page 596, 6th line from the bottom, read *vortical* for "*vertical.*" Page 597, 5th line from top, read *may* for "*must.*" Page 600, last line, read *process* for "*proofs.*"

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1854.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich

WEEKLY— 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
January 7	65 .00	39 .00	26 .00	30 .40	30 .00	40. 0	E.	3 .½	0	0.000
" 13	64 .00	32 .00	31 .00	30 .24	29 .10	1. 14	SE.	3.	2	1.180
" 20	79 .00	44 .00	35 .00	30 .28	29 .95	33. 0	E.	2.	3	0.430
" 27	75 .00	30 .00	45 .00	30 .50	29 .90	60. 0	NE.	3.	2	0.895
Febry. 3	62. 00	43 .00	19 .00	30 .26	29 .99	27. 0	SE.	2.	1	0.250
" 10	64 .00	41 .00	23 00	30 .34	29 .90	44. 0	NW.	2 .½	1	0.500
" 17	87 .00	37 .00	56 .00	30 .20	29 .90	30. 0	NE.	4 .¾	4	0.965
24	67 .00	43 .00	24 .00	30 .19	29 .88	29 .0	E.	4 .¾	3	5 098

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL, (NEW-ORLEANS,)

For November, 1854.

	SEX.	JANUARY.	
ADMISSIONS -	Males	884	
Do. - -	Females	208	
		——1092	
DISCHARGES -	Males	620	
Do. - -	Females	152	
		——	772
DEATHS - -	Males	74	
Do. - -	Females	21	
		——	95
BIRTHS - -	Males	4	
Do. - -	Females	7	
STILL-BORN -		4	
		——	
			15

Number remaining, February 1st, 638.

THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

VOL. X. MAY, 1854. No. 6.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia: Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

EDITOR'S OFFICE—NO. 30 CAMP STREET:

RESIDENCE—Tchoupitoulas street, between Louisa and Poyfarre street

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AGENT, NO. 66 CAMP STREET.

NEW ORLEANS:

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1854.

Editor's Office—Notices.

MAY 1, 1854.

NEW MEDICAL JOURNALS.

New Orleans Medical News and Hospital Gazette—a semi-monthly Journal, edited by Drs. Choppin, Beard, Schlater and Boyer. Four numbers of the Journal have already been published, affording in matter, manner, appearance and promptness, satisfactory guarantees of success.

The Georgia Blister and Critic—a monthly Journal, devoted to Southern Medical Literature and the Exposition of the Diseases and Physical Peculiarities of the Negro Race: By H. A. Ramsey, M. D., Atlanta, Ga.

BOOKS RECEIVED.

A Treatise on Acute and Chronic Diseases of the Neck of the Uterus; illustrated with numerous Plates, colored and plain: By Chas. D. Meigs, M. D., Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College; Member of the American Medical Association; American Philosophical Society, and one of the Council; Vice President of the Philadelphia College of Physicians; Author of "Obstetrics, the Science and the Art;" "Woman, her Diseases and Remedies;" A Treatise on certain of the Diseases of Young Children, etc., etc. 8to. pp. 116, 22 plates. Philadelphia: Blanchard & Lea. 1854. From Mr. J. C. Morgan, bookseller, Exchange Place.

Pneumonia—its supposed connection, Pathological and Etiological, with Autumnal Fevers; including an inquiry into the existence and morbid agency of Malaria: By R. La Roche, M. D., Member of the American Philosophical Society; of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine, and Foreign Associate of the Medical Society of Emulation of Paris; of the Academies of Sciences of Turin, Copenhagen, Stockholm and Nancy; of the Medical Society of

Marseilles, Lyons, etc., etc. 8to. pp. 502. Philadelphia: Blanchard & Lea. 1854. From Mr. J. C. Morgan, bookseller, Exchange Place. [A gentleman has promised a review of this work.]

Elementary Chemistry, Theoretical and Practical: By GEORGE FOWNES, F. R. S., late Professor of Practical Chemistry in University College, London; edited, with additions, by Robert Bridges, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy, etc., etc.; a new American from the last and revised London edition, with numerous illustrations on wood. Philadelphia: Blanchard & Lea. 1853. Royal 12mo. pp. 555. From Mr. J. C. Morgan, bookseller, Exchange Place. [An estimable work.]

Homœopathy—its Tenets and Tendencies, Theoretical, Theological and Therapeutical: By JAMES Y. SIMPSON, M. D., F R. S. E.; Professor of Midwifery, University of Edinburgh, and Physician Accoucheur to the Queen for Scotland; President of the Medico-Chirurgical Society; lately President of the Royal College of Physicians; formerly President of the Royal Medical Society and of the Physical Society, Edinburgh; Foreign Member of the Imperial Academy of Medicine of France; Member of the Society of Surgery of Paris, and of various Medical Societies in Stockholm, Copenhagen, Berlin, Ghent, etc. First American from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8to. pp. 304. From Mr. T. L. White, bookseller, 105 Canal street.

The Transactions of the American Medical Association: Vol. VI. Philadelphia: Printed for the Association by T. K. & P. G. Collins. 1853. 8to. pp. 869. Fine paper, plain and colored plates. From Mr. J. C. Morgan, bookseller, Exchange Place.

On Rheumatism, Rheumatic Gout and Sciatica, and their Pathology, Symptoms and Treatment: By HENRY WILLIAM FULLER, M. D., Cantab., Fellow of the Royal College of Physicians, London; Assistant Physician to St. George's Hospital, &c., &c. New York: Samuel S. & William Wood, 261 Pearl street. 1854. From the publishers, through Mr. T. L. White, bookseller, 105 Canal street.

A Treatise on Venereal Diseases: By A. Vidal, (De Cassis) Surgeon of the Venereal Hospital of Paris; Author of the Traité de Pathologie externe et de Médecine Opératiore, etc., etc., with colored plates; translated and edited by Geo. C. Blackman, M. D., Fellow of the Royal Medical and Chirurgical Society of London; formerly one of the Physicians to the Eastern and Northern Dispensaries, New York. New York: Samuel S. & William Wood, 261 Pearl street. 1854. 8to. pp. 499. From the publishers, through Mr. T. L. White, bookseller, 105 Canal street.

Homœopathy fairly represented—a reply to Professor Simpson's "Homœopathy" misrepresented: By William Henderson, M. D., Professor of General Pathology in the University of Edinburgh. First American, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8to., pp. 302. From Mr. T. L. White, bookseller, 105 Canal street.

Letters on Syphilis—By Ph. Ricord, Chirurgien de l'Hopital du Midi, &c. &c.; translated by D. D. Slade, member of the Massachusetts Med. Society, &c.; formerly House Surgeon at the Mass. General Hospital, &c. Boston: David Clapp. 1853. pp. 404; paper cover. From Mr. J. B. Steel, bookseller, 60 Camp st.

Lectures on the Diseases of Infancy and Childhood—By Chas. West, M. D., F. R. C. P., &c. Second American, from the second and enlarged London edition. Philadelphia: Blanchard & Lea. 1854. 8to., pp. 487. From Mr. J. B. Steel, bookseller, 60 Camp st.

Types of Mankind, or Ethnological researches, based upon the ancient monuments, paintings, sculptures, and crania of Races, and upon their natural, geographical, philological, and biblical history; illustrated by selections from the inedited papers of Samuel Geo. Morton, M. D. (late President of the Academy of Natural Sciences of Philada.), and by additional contributions from Prof. L. Agassiz, LLD., W. Usher, M. D., and Prof. H. S. Patterson, M. D.: By J. C. Nott, M. D., Mobile, Ala., and Geo. R. Gliddon, formerly U. S. Consul at Cairo. Philadelphia: Lippincott, Grambo & Co. 1854. pp. 738.

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THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL,
FOR MAY, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—CASE OF OSSIFICATION OF THE PLACENTA.*

BY M. TROY, M. D.

On the 23d of January, 1854, I was requested by my friend, Dr. Ulmer, to assist him in the delivery of Mrs. A., æt. 19, who had been eighteen hours in labor with her first child.

In descending into the cavity of the pelvis, the occiput of the child had been arrested by the pubis, and though the uterus continued to contract powerfully, no progress had been made in the labor, for the eight or ten hours the head had been so situated.

Owing to the strong and almost continuous action of the womb, it was with some difficulty that a lever (the handle of a common table spoon) could be introduced, and the head dislodged, which being done, however, the lady was soon delivered of a fine daughter.

The placenta did not come away, and slight traction upon the cord produced no effect. After fifteen or twenty minutes, it was evident that hour-glass contraction of the womb was about to take place, and Dr. Ulmer introduced his hand for the purpose of delivering the placenta. He found it firmly adherent to the fundus of the uterus, and it was with the greatest difficulty that he could separate it from the womb—and during the separation, which had to be effected by

* The next article, which is supplemental to this, is submitted under the belief that an isolated case will be less likely than several cases to arrest and fix the practitioner's attention upon ossific adhesion as one cause of placental retention. Among the deplorable effects of retained placentæ may be reckoned hæmorrhages, purulent discharges of the most offensive kind, and a low fever, resembling typhoid and hectic.—[Editor.

almost *pinching the placenta off*, a distinct crepitation could be felt by him and heard by listening attentively.

The pain caused by these efforts was declared by the patient to be more severe than any she had experienced during her unusually severe labor. But this slow and painful operation was happily completed, and the separation of the entire organ effected, when a contraction soon forced both it and the hand from the uterus, which now contracted firmly and regularly.

There was, during all this time, no serious hæmorrhage (though we were terribly apprehensive of it,) and the patient retained her strength very well.

A large opiate was given immediately upon the delivery of the placenta, and she was soon quite comfortable.

Upon examining the after-birth, we discovered the source of the trouble. The placenta was not circular, but somewhat avoid in shape—one diameter being much longer than the other—and *wedge shaped*, that is, one margin of the longer diameter was abrupt and very thick, from which the organ gradually thinned off to a sharp edge at the other margin. In fact, the shape of the organ did not differ materially from that of the great lobe of the liver.

There was nothing peculiar in the attachment of the cord.

But the uterine surface of the placenta was the most remarkable and interesting portion. It was sprinkled more or less thickly throughout, with a calcareous deposit, which gave it a gritty feel when touched, and created a distinct noise when any hard substance was drawn across it, similar to the sound we perceived when its connection with the womb was broken up.

The deposit was white, and had an amorphous appearance, more like the incrustations upon the teeth than anything else. It was in masses or patches from the size of a dime to that of a millet seed, or much less—the whole uterine surface felt as if thickly studded with broken points, invisible in the substance of the placenta. The whole amount of earthy matter in the placenta was estimated, by guess, to be about 1 drachm, but of course it may have been more or less.

The deposition of bony matter in this situation is fortunately a very rare freak of Nature, and retention of the placenta from this cause, is hardly mentioned by obstetricians. But for the sake, both

of the practitioner and patient, the possibility of the occasional occurrence should be borne in mind. Dr. Garrison, of New Jersey, has reported in the American Journal of Medical Sciences, for January, 1852, the case of a lady where this condition was present in several successive pregnancies, and in each confinement it gave rise to the most alarming and dangerous hæmorrhage.

In Mrs. A.'s case there was fortunately nothing of the kind, and she has been doing very well ever since her delivery.

The service which the publication of Dr. Garrison's cases was to me in this instance, makes me deem the case more worthy of publication than I otherwise would. Such a case, if left to the powers of nature, would almost surely end fatally, as the womb is altogether unable to break up the bony attachment of the placenta.

I need not mention that neither Dr. Ulmer nor myself had ever before met with a similar case.

CAHABA, Ala., January 31st, 1854.

ART. II.—OSSIFICATION OF THE PLACENTA, WITH PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS.

[*Supplementary to the preceding article.*]

BY B. DOWLER, M. D.

MAY 19, 1838.—Mrs. * * *, aged 23; Magazine street, New Orleans; *primipara;* breach-case; child still-born. During prolonged, patient, yet unavailing attempts to deliver the placenta by traction with the cord, the latter broke; considerable but not alarming flooding; ergot failed; waited several hours; explored the uterus; found an hour-glass contraction; was unable to pass the stricture, and consequently failed to reach the placenta; after about six hours' delay (the retention having continued nearly all day,) and after having been urged most importunately to deliver the placenta without further delay, I succeeded in passing the constricted isthmus; found the uterus greatly elongated; its fundus had arisen to the inferior portion of the epigastrium; the hour-glass contraction around the wrist was strong, almost paralyzing, and unremitting; the maternal surface of

the placenta was everywhere found adherent to the uterus by bony and parenchymatous structure, which was gradually detached, portion by portion, by the nails and tips of the fingers, the motions of which could be distinctly seen through the clothes covering the lady; the separation was attended with moderate pain and flooding. As soon as the detachment was effected, the mass was brought away in the hand.

The recovery was as speedy as in the most favorable cases of parturition.

The bony matter intimately connected with the placental, consisted chiefly of plates with fine cancelli and reticulations, resisting pressure like the egg-shells of small birds, crepitating audibly on being crushed.

In such cases (which are more frequent than is generally supposed) an unskillful and violent force applied to a strong umbilical cord, might readily invert the uterus. The uterine contraction, though urged by ergot, can scarcely be relied on in such cases, while prolonged floodings, and, finally, slow putrefaction, would in most cases endanger life from anæmia, or fever of a typhoid character.

The placental mass in these cases being interspersed with bony plates, reticulations and cancelli, is comparatively incompressible, or virtually so as it respects the natural forces inherent in the uterus.

When the maternal disc of the placenta is thus adherent by osseous structure, the uterine action being strong, hour-glass contraction, from the configuration of the resisting mass, might be to some extent explicable, as such a body, after the expulsion of the child, would probably cause irritation and a more violent contraction in the fundus and central part of the womb, giving its upper portion the form of an inverted cone or hour-glass. Judging from several cases of hour-glass contraction where neither adhesion nor ossification was the cause of retention, it would appear that this curious phenomenon is not always attended with menacing hæmorrhage. For this there would appear to be in some cases a mechanical explanation, inasmuch as the organ closes so completely centrally, that it is difficult to find the strictured passage, particularly if the cord has been broken off at its placental extremity, so that with a complete stricture or closure, the upper portion of the uterus is a plenum, its walls closely investing and moulding themselves to the placental mass. If hour-glass con-

tractions were intermittent, or alternated with relaxations, this explanation would not be admissible—under the most favorable circumstances it is not reliable in these ossific implantations.

In retention of the placenta the judicious practitioner will be slow in distrusting Nature, and will be in no hurry to make a manual exploration of the uterine cavity. But in almost all cases of unusual delay of placental delivery, there is a moral force or pressure from without which can scarcely be resisted. Every feminine voice is lifted up in favor of complete deliverance, and against considerable delays. So far as the experience of the writer has extended, he must confess that he never has had cause to regret any bad result from conceding to the urgent requests of friends, on these occasions, though at the time, he may have often wished to avoid interference when danger was not imminent.

The following extract is so important, as showing the necessity of active interference to prevent impending death from a cause almost wholly overlooked by systematic writers, that its length will be readily excused, and the more so, as it may be the means of saving life when it is menaced by uterine hæmorrhage occurring in connection with ossified adhesions of the placenta to the uterus.

Dr. C. Garrison, of Swedesboro (N. J.,) in his interesting account of the ossification of the placenta occurring several times in the same individual, observes, that among obstetrical writers he had never found any satisfactory account of this phenomenon. Having been called (Oct. 25th, 1835,) to attend Mrs. J. H. in her second confinement, Dr. G. learned that great difficulty had previously taken place in her first labor, in delivering the placenta;—her attendant having been obliged to send to Philadelphia for Dr. Meigs, to assist him;—the latter with difficulty to himself and pain to her, finally succeeded, but not without considerable hæmorrhage. In her second accouchement, Dr. G. found no difficulty in delivering the child. "But," he adds,

"In about ten minutes there was a tremendous gush of blood, which was quickly followed by another quite as large as the first, the result of which was great and almost fatal prostration; the pulsations at the wrist were scarcely felt, the face became pale, the hands cold, and the respiration sighing; she grew sick and vomited, complained of being very cold, pushed down the bed clothes, gasped for breath, and in a very short time after the last discharge, was in a profound syncope. In the meantime, I had poured a pitcher of iced water, which was previously prepared, over the abdomen, and introduced my hand into the uterus, quickly and without any obstacle, as the hæmorrhage had produced complete relaxation of

both the uterus and external parts; but on reaching the placenta, and attempting to introduce my fingers between it and the uterus, for the purpose of effecting its detachment, I found it impossible to do so, as the union between them seemed perfectly firm and unyielding. After a moment's consideration, I determined to make a separation at all hazards, if it was possible, as death was inevitable if the placenta should be long retained. I began the operation slowly and cautiously, as the separation could only be accomplished by actually tearing or scratching the placenta from the surface of the womb, which could not be done except by the exertion of considerable force, more than it seemed possible that she could survive; and it was accompanied by a kind of crackling noise, which was heard distinctly both by myself and the attendants at the bedside. I soon found that I must hasten the delivery, or the patient would die before it was effected, and, letting go the cord, I placed my hand over the outside of the uterus, and as rapidly as I was able, proceeded to loosen the placenta from its attachments. In this manner I succeeded in separating the whole mass, though in detached portions, which, together with my hand, were soon expelled by a smart contraction of the uterus. But although I had succeeded in removing the placenta, I still felt extremely anxious about the patient; she was yet in a state of complete unconsciousness. I gave her a *large* dose of opium, applied ice to the abdomen, and kept up frictions over the outside of the uterus until it was firmly contracted and all hæmorrhage had ceased. She roused up for a moment, but the syncope immediately returned, and it was a considerable length of time before she showed any persistent signs of returning animation.

"On examining the placenta, I found all that surface of it which came in contact with the uterus, in a state of ossification; it presented, through a common pocket glass, a kind of cellular arrangement, similar in appearance to what might be produced by sticking the whole surface full of the husks of wheat which had been broken in two, leaving the broken edge of the husk turned outward. It was undoubtedly the separation of this bony matter which gave rise to the crackling noise which had been heard during the process of delivery. I could reproduce the same sound in the placenta after it was expelled; it crackled in my fingers like frozen grass or ground. I had never upon any previous occasion been compelled to use an equal degree of force in the delivery of the placenta, and I felt very doubtful as to what might be the issue of it; the utmost strength of my fingers was barely able to break up this bony union.

"Since this time I have attended Mrs. H. in *five confinements*, and in all but one there was a similar condition of the after-birth, accompanied by similar phenomena of excessive flooding and great difficulty of separating the bony surfaces of the uterus and placenta.

"Her last labor occurred about two years since, accompanied with the same circumstances—bony adhesions, and hæmorrhage; for many hours I altogether despaired of her recovery."—[*Am. Jour. Med. Sci.—West. Jour. Med.*, 1852.]

These osseous deposits appear to occupy a middle ground between physiological and pathological anatomy proper, with a bias towards the latter. In old age they are so frequent in certain tissues, particularly the cartilaginous, as to appear more like a normal law than a pathological alteration in the economy. Injurious they certainly are in some structures, as in the valves of the heart, by interfering with the functions of this organ, while, for example, in the tracheal rings, and cartilaginous portions of the ribs and in some other structures, no diseased action usually results from osseous degeneration. During an attendance for several years upon the slaves of a banking institution of this city, a very old negro, seldom sick, has been noticed whose arteries are extensively ossified.

In numerous examinations of subjects who had perished from fever and other maladies, often in the bloom of life, I have found in not a few instances ossifications in the cellular, parenchymatous, membraneous and muscular tissues. In one subject the serous membranes of the brain, chest, and abdomen, were universally studded with salient spiculæ or granular ossiform points, both isolated and in patches, presenting a rough, grater-like surface, originating apparently from the sub-serous tissue. Without wishing to multiply examples, I may allude to two cases of recent occurrence illustrative of non-pathological and the pathological conditions incidental to ossific degeneration: C. W.; born in Germany; aged 25; who died of yellow fever, Sept. 16, 1853; was entirely free from preternatural ossifications excepting the stylo-hyoid muscle, the whole of which consisted of bone except a slight cartilaginous portion of its upper end at its connection with the styloid process. In the lungs and bronchial glands, ossific and friable calcarious concretions often occur without producing symptoms of disease. In June, 1852, Mr. J. F., aged about 25—born in Philadelphia—clerk in a cotton press—was seized with an apparently sub-acute pneumonia in the first stage;—without entering into the details of his case, it is sufficient to mention that an abscess having formed in the lung, broke;—a vast amount of the most fœtid, thick, dusky colored pus was discharged, including an irregular mass of bone perforated in the center by an opening corresponding to a bronchial tube and its rings at one of its bifurcations; he also vomited a small mass of a chalky matter without cohesion, shapeless. This patient gradually recovered, and still retains his usual health.

Many of these concretions, particularly those in the lungs and bronchial glands, and about the articulations of gouty patients, are but amorphous, loose combinations of calcarious earth, containing but little animal matter, falling short of completely organized bone in cohesion, gelatine, and other animal matters.

Pathological ossification takes this character from its interference with the due performance of a physiological function, or from its causing inflammation as a foreign body in the soft tissues.

One occasional result of acute hyperæmia or inflammatory engorgement, is osseous transformation, which in its turn may finally become the cause of a new inflammation after the subsidence of the primary.

ART. III.—TWO CASES OF ORGANIC DISEASE OF THE HEART; WITH THE HISTORY, DIAGNOSIS, AND POST MORTEM OF EACH.

BY D. MACGIBBON, M. D.,

Visiting Physician, Charity Hospital, New Orleans.

The following two cases of organic disease of the heart were reported to the Physico-Medical Society at its session 25th February, 1854, and are now offered for insertion in the New Orleans Medical Journal. The history of these, together with the pathological conditions found at the *post mortem*, present some points of especial interest in each, which are deemed worthy of notice. The cases occurred under my care in the Charity Hospital:

The first is that of a Spaniard, a fisherman, forty-eight years of age, of stout muscular make, and bilio-sanguine temperament. He had been, as I understand since, frequently of late years, in the hospital; at one time treated for one disease, at another for something else. He entered towards the close of last year, suffering from an attack of acute bronchitis, and for the first time came under my charge. With appropriate treatment he soon got well of this complaint. On auscultating him a peculiar cardiac murmur was heard, which led me to diagnose organic disease of the heart as present in his case; and he was advised to remain in the house, but feeling himself relieved of the cough, the chief aliment for which he entered, he preferred being discharged, which was done. He returned soon after, on the 14th January, 1854, suffering much from an oppressive sensation, principally experienced about the region of the heart; which was aggravated by some distension of the abdomen, consequent on constipation, which was now, more or less, habitual with him. He would most frequently be found, at the visit hour, sitting up in bed, as he could not lie down well, from a sense of increased distress in the horizontal position, nor could he, as a matter of course, obtain proper sleep either.

The murmur, referred to above, was heard even more distinctly than when he was previously in the hospital. It was loud, and of a *cooing* character. It accompanied the systole of the heart, and could be heard best over mid-sternum; from whence it was transmitted up-

wards and to the right. It could also be heard distinct enough to the left of the hollow at the spine, near the third and fourth vertebræ. The heart's impulse was strong; but its beatings were regular. The pulse at the wrist was not noticed to have anything peculiar about it. No other sound, either normal or abnormal, could be said to be detected on auscultation. From all these circumstances—the character of the murmur—the time *when* and place *where* that was best heard, together with the fact elicited from the patient, that he had, at some former period, an attack of rehumatism, I had little hesitation in concluding that constriction of the aorta existed, with accompanying hypertrophy. The patient was seen at different times by several physicians and a number of the students, who listened to the *cooing*, and knew the diagnosis made. In the way of treatment, Croton oil was found the most useful in relieving the constipation; and tincture of aconite was mainly employed to relieve the præcordial distress and procure rest.

On the evening of the 9th February, the patient suddenly expired, and the next morning the body was examined, when the following pathological changes, as far as the heart is concerned, were found:

The organ, as a whole, was somewhat larger than natural; the walls of the left ventricle were thicker than usual, but there was no constriction of the aorta, as diagnosed. The caliber of that vessel at its commencement was of normal dimensions, but immediately beyond the sigmoid valves, and partially involving these, it bulged out to an abnormal degree on one side, forming, as it were, an incipient aneurism at this point. The cause of this bulging of the vessel was explained by the presence of an old cicatrix, rather larger than a five cent piece—showing that ulceration of the serous and fibrous tissues occurred at some former period at this point, and that the weakened tissues at the time partly gave way to the force of the blood current, as is known to take place in aneurism.

This dilatation in the aorta, in all probability, produced a change or whirling in the current, similar to that which is supposed to take place where the blood passes a constriction in the aorta; and thus, I suppose, gave rise to the peculiar musical murmur which was heard. Walshe, while he states that a murmur, as above heard, habitually signifies simple or rough constriction of the aortic orifice, also says

that in rare cases a cardiac murmur very closely simulating this type may arise from other causes, which he mentions, and the lesion in this case is indicated as one of these, as may be seen by a reference to page 214 of his work on the Heart and Lungs, American edition.

The integrity of two of the aortic valves was in some measure impaired by the ulceration above referred to, which extended to and destroyed a portion of the conjoined column formed by one of the horns of each. These valves, thus diminished, were no longer in a condition to perform their part in shutting up the aortic orifice. The third one, however, was intact; the three, with the exception referred to, were otherwise quite healthy. No doubt some degree of regurgitation took place here during life, though, as already mentioned, no abnormal murmur to indicate this was detected. Why a diastolic murmur was not detected, I cannot say, unless it be, for that best of all reasons, that it did not exist. And this view is strengthened by a reference to the authority already cited, where it will be found stated (page 216) that "mere incapacity of valves, healthy in themselves, but too small to fill the widened mouth of the aorta, has in rare instances produced this murmur;" so that, under the circumstance, its absence would seem rather to conform with the rule than the exception.

Supposing the case to have been as here represented, then the absence of constriction of the aortic orifice, with a systolic murmur heard as above—and, again, the occurrence of regurgitation where no diastolic murmur existed, are all points worthy of being remembered in making a diagnosis in any similar case.

Was the organic state of the heart the cause of the sudden death in this case? I have no doubt of it. Andral, who is a good authority in these matters, says that this is no uncommon occurrence in individuals affected with such complaints, even though but little advanced, and which do not as yet manifest their existence by any well marked symptom. It was, therefore, not unexpected.

The other case is one which had been a much longer time under observation, and is more interesting from the particular form met with being one much more rarely diagnosed, and from the opportunity of verifying the accuracy of the diagnosis by the *post mortem* examination of the case.

An Irish laborer, aged thirty-seven years, entered the hospital on the 29th July, 1853. When he entered he was suffering from remittent fever. There was some œdema of the lower extremities, and some fluid in the abdominal cavity. He was anæmic looking. There was, besides, blowing cardiac murmur heard on auscultation, which led me to conclude he had organic disease of the heart. He mentioned that he had been in good health up till recently, and that he had been working on some one of the neighboring railroad cuttings until attacked with the fever for which he entered.

By proper treatment he soon got clear of this latter, but was advised, in consequence of the other affection, to remain in the hospital. He took the advice. Here rest and a nourishing diet, which were what were mainly depended on, might do something to ward off, for a space, a fatal termination, which otherwise would be assuredly hastened by his being compelled to work in the swamps, as he had been.

The dropsical effusion gradually went on augmenting, notwithstanding that he was getting mild diuretics—as the muriated tincture of iron, with sweet spirits of nitre, and the infusion of juniper berries for drink. The anæmic condition of the system also increased.

To give relief to the tension and sense of suffocation, consequent on the greatly enlarged abdomen, it was deemed necessary, towards the close of the year, to discharge the fluid. Paracentesis* was therefore performed, when two large buckets full of fluid were withdrawn, with, for the time, manifest relief. The liver, which previous to this could not be felt, now showed itself through the flaccid parietes very distinctly. It seemed harder, and its free edge to be more obtuse than normal, and also to be larger.

At this time he lay in a bed close to that occupied by the preceding patient, and, like him, was seen by a number of medical gentlemen and students, who listened to the murmur and noticed the points of contrast. The murmur in both was systolic, but, unlike the former, this one was transmitted downwards; this, too, had nothing of the

* PARACENTESIS ABDOMINIS.—In New Orleans, where surgery is usually practiced with an ability which compares favorably with other cities, I have, nevertheless, the best reason (based on post mortem examinations) for believing that in several cases patients have perished from hæmorrhage following paracentesis, owing to the division of the epigastric artery—an accident which even the unskillful may easily avoid by operating in the course of the linea alba.—[ED.

musical tone of the other, but was an excellent specimen of the *bruit de soufflet* of the French. Before mentioning the diagnosis, let me state more fully the grounds upon which that was made. It is known, as a general rule, that nineteen out of every twenty cases of organic disease of the heart met with are situated in the left side of the heart, and that mitral regurgitation is the most frequent of these. Recollecting that the murmur in this case was systolic and transmitted downwards, the lesion of the mitral valves was the first to suggest itself. But a little more care, with frequent opportunities for auscultating the case, induced me rather to place the lesion in the tricuspid valves, and tricuspid regurgitation was accordingly diagnosed. Two circumstances especially guided me in forming this conclusion: First, the abnormal murmur was heard *best immediately to the left of the ensiform cartilage*—much more so, indeed, than under the left nipple; and in the second place, I satisfied myself that I could hear, a little above the left nipple, both a systolic and a diastolic click, the former of which I would not expect to hear were the mitral valve the seat of lesion. Again, tricuspid regurgitation accounted better for the condition of the liver, and the consequent ascites the patient suffered from—and which was now daily threatening his existence, for the fluid had again accumulated to as distressing a degree as before. In the beginning of the month of February he was again "tapped."—He had been for some time constantly bed-ridden. His general health had become greatly impaired. On both the upper and lower extremities were purpuræ hæmorrhagica patches, exhibiting this anæmic condition most markedly.—Before one bucket full of the fluid was withdrawn he had become so faint that further attempts were discontinued. No pain in the abdomen, following on this, was complained of; but diarrhœa supervened and great prostration; the countenance took on, too, a cadaveric expression, and he died on the night of the 16th of February. The next morning a *post mortem* examination was made, and the following results obtained:

On opening the abdomen and freeing it of the contained fluid, which was somewhat red in color, its whole peritoneal covering was found softened by recent inflammation, which, probably, took its rise in the recent operation. In subjects of this kind a low species of inflammatory action not unfrequently is set up, as is well known, without the

usual symptoms of inflammation having shown themselves, to awaken suspicion as to what was going on. This I have seen in some puerperal cases. The portion covering the intestines was of a darkish red color, and easily scraped off, when the other tissues were shown to be unaffected. A few slight shreds of lymph were also observed about the cavity.

The liver was granular on the surface; and, when cut into, the same diseased condition was found to pervade that organ throughout. The heart was a little larger than natural, and this was found to be occasioned, principally, by increased thickness in the walls of the right ventricle; which, when both ventricles were laid open, were seen to be as thick as those of the left. The right auricle was also found to be greatly distended. The radiating fleshy fasciculi in the *auricula*, like the *columnæ carneæ* in the corresponding ventricle, were hypertrophied: the whole showing most convincingly that tricuspid regurgitation had existed, as diagnosed. The *chordæ tendineæ* of one of the tricuspid valves were shortened, and the apex and border of this one were somewhat puckered, so that it could no longer fall back properly to assist in closing the auriculo-ventricular opening at the heart's systole. The other valves of the heart were all healthy. Nothing beyond of importance was noted.

The distended condition of the right auricle with blood, which must have existed here in life, prevented the blood from the ascending vena cava emptying itself in it as it should, and this continuing would necessarily obstruct the circulation in the liver, and produce in that organ a degree of congestion, ultimately leading to chronic inflammation and thickening of its tissues, with, we may well suppose, in the end, the Cirrhosis met with.

As this change in the liver advanced we can see how the vena portæ would become more and more obstructed; and thus give rise to the effusion of serum in the abdominal cavity, with the other train of symptoms which complicated, and added to the severity of the case: all which had their origin in the organic affection of the heart. This is the usual, and most rational, explanation of these cases.

Although medical art can do but comparatively little for the cure of organic diseases of the heart, yet it is well that they be carefully studied. When assured of their existence, by means of auscultation,

it is surely something to be able to advise the unfortunate patient of the best course to pursue, and otherwise to assist him, to render his life more comfortable; if not also to prolong that. Nor is the knowledge which a close study of these diseases affords devoid of value, should it in some of these, when death is lurking in the cup, only serve to prepare the doomed victim for the worst.

ART. IV.—EXPERIMENTS WITH PHOSPHORUS, AND REMARKS UPON ITS DOSE AND ACTION, WHEN GIVEN IN THE FORM OF ALCOHOLIC TINCTURE OR SOLUTION.

BY WM. M. BOLING, M. D.,

OF MONTGOMERY, ALA.

More especially since I have used in my practice the Veratrum Viride, the discovery of an article equally certain, prompt, powerful and reliable in its action as a cardiac sedative, but without some objections that have seemed to me to pertain to the operation of this remedy, I have thought a great desideratum. By an article presented to the Alabama State Medical Association, at its meeting in Selma, in December, 1852, with impressions that I had previously received in regard to it, my attention was directed to the Gelseminum Sempervirens, or Yellow Jessamine. I entered upon my experiments with this article, somewhat sanguine of success, but disappointment was the result. After a number of experiments with it, in which certain effects said to result from its use, such as dimness of vision, trembling, &c., were produced in a marked degree, I could not discover that it at all reduced the frequency of the pulse. My experiments with it were upon healthy subjects.

In a well written and exceedingly plausible paper, which was published in the New Orleans Medical and Surgical Journal for January, 1854, on Pneumonia, by Doctor S. Ames, of Montgomery, *Phosphorus* is mentioned as a remedy of great value in this disease, and its curative influence, if I mistake not, ascribed to a cardiac sedative operation. I have been induced to make some experiments also with this article; the results of which I will give.

Two preparations of the article are mentioned by Doctor Ames, to wit: a saturated Tincture in Anhydrous Alcohol, which he supposes to be equal in strength to the Ethereal Tincture, or to contain four grains to the ounce; and a diluted tincture, made by adding nine parts of alcohol to one part of the saturated tincture. As a dose of the saturated tincture, from half a drop to two drops are spoken of, but it is stated that the medicine "cannot be continued in the smallest quantity just mentioned for any great length of time, without inducing considerable disturbance of the stomach, as shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." It is also remarked that "its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome, or even dangerous, after several repetitions at intervals of three or four hours;" and that "this quality was developed in one instance by repeating it in a dose of two drops of the strong alcoholic solution three times at intervals of twenty-four hours." In regard to its sedative action, Dr. Ames remarks, that "its sedative or contra-stimulant, is its medicinal or therapeutic action," and in a note, makes the following statement: "Two young gentlemen, my personal as well as professional friends, have been recently engaged in some experiments to test the effects of phosphorus on persons in health; they themselves being the subjects of the experiments. These gentlemen (Doctor Pollard and Doctor Oliver) found that a single dose of two drops of the saturated alcoholic solution invariably reduced the force and frequency of the pulse. The changes in frequency ranged in the number of pulsations to the minute, between eight and twelve beats. A change was perceptible in about twenty minutes, which reached its maximum in from an hour to an hour and a half."

Doctor Ames however, for reasons given in his paper, prefers the weaker solution, and his method of administration is, to add sixteen drops of it to four ounces of water, of which he gives a teaspoonful every "third or fourth hour—usually every fourth." As there are thirty-two teaspoonfuls in four fluid ounces, the quantity of the diluted tincture thus given at a dose, would be half a drop. This dose Doctor Ames prefers "after many trials with larger ones."

I have been thus particular in referring to the peculiar effects said by Doctor Ames to result from the use of these two preparations of phos-

phorus, but more especially to the dose and method of administration, that comparison may be conveniently instituted with the dose and manner of administration followed by myself, and any fault or deficiency in the latter, detected and pointed out. I feel inclined to pursue the subject further, if I have failed in any way to conduct my experiments satisfactorily.

The pulse—as most persons know, and as any one may satisfy himself of—will be found to vary in many persons, if not in every one, somewhat in frequency at different times in the twenty-four hours; and often, even when felt at short intervals, will be found so to vary from trivial or inappreciable causes. To prevent any fallacy from this circumstance, it will be noticed, that in my experiments on some days, the frequency of the pulse at different hours is given, though the phosphorus was omitted. A comparison may thus be made between the pulse on the days that the phosphorus was, and on the days that it was not, given.

My first subject was Sam, a healthy mulatto boy, seven years old. On the 25th of February, having kept him in the recumbent posture sometime, his pulse being 102, at 1 o'clock, P. M. I gave him seven drops of the saturated tincture. At 2 o'clock, P. M., pulse variable, from 90 to 108. 3 P. M. (subject asleep,) pulse 94. The dose of seven drops repeated. 4 P. M. (subject asleep,) pulse 104.

Feb'y. 26th—1 o'clock, P. M., pulse 100; 1½ o'clock, pulse 114; 2 o'clock, pulse 100. No phosphorus was given to-day. The subsequent experiments with Sam were with the diluted tincture. The experiments already given were conducted with the subject in the recumbent posture. In the following, he was generally, when a dose was to be given or the pulse counted, called in from play:

March 10th—11 A. M., pulse 100; half a drop of the diluted tincture given. 12 M., pulse 103; half a drop given. 1 P. M., pulse 100 to 106; one drop given. 2 P. M., pulse 104; two drops given. 3 P. M., pulse 108; two drops given. 4½ P. M., pulse 92; two drops given. 6 P. M., pulse 88, and five minutes later, 96.

March 11th—Half after twelve, pulse 96. 1 P. M., pulse 104. 2 P. M., pulse 100. 3 P. M., pulse 101. 5 P. M., pulse 86. 6 P. M., pulse 92. No phosphorus was given to-day.

March 12th—10 A. M., pulse 98; 50 drops given. 11 A. M., pulse 102. 1 P. M., pulse 96; 100 drops given. 2 P. M., pulse 104. 5 P. M., pulse 100; 200 drops given. 5½ P. M., pulse 100; 200 drops

given—making in all 550 drops in seven and a half hours. 6¼ P. M., pulse 107. 7½ P. M., pulse 94.

March 13th—8 A. M., pulse 92; 500 drops given. 9½ A. M., pulse 92.

March 17th—4 P. M., pulse 104; nine hundred and ten (910) drops, being exactly one ounce, given at a dose. Here a child seven years old took, at a single dose, one thousand eight hundred and twenty (1820) of Docter Ames' doses for the adult. Half after four P. M., pulse 116. 5 P. M., pulse 120. 7 P. M., pulse 104, and the subject "feels very well."

The two hours immediately succeeding each of the last two doses, Sam spent riding in my buggy with me, and attending to my horse at the different stopping places; and though I noticed him carefully, no appreciable effect was manifested—the variation in the pulse alone excepted—otherwise than that he seemed a little merry, which was fairly, I think it will be admitted, attributable to the alcoholic part of the dose—about half an ounce in one instance, and an ounce in the other. It would scarcely seem necessary, while the large quantity of alcohol would render it improper, to repeat, or to carry the —— joke any further with Sam.

My second subject was a negro man, about 25 years old, under treatment, by means of the bandage and the recumbent posture, for a small ulcer on the foot. Otherwise, he was healthy. He lay in my office during the experiments, which *were all conducted with the saturated tincture.*

Feb. 27th—1 P. M., pulse 78; ten drops given. 4 P. M., pulse 72.

Feb. 28th—12 M., pulse 78; ten drops given. 1 P. M., pulse 80. 3 P. M., pulse 72. 4 P. M., pulse 78.

March 1st—11 A. M., pulse 74. 2 P. M., pulse 74. 3 P. M., pulse 68. 4 P. M., pulse 80. No phosphorus was given to-day.

March 2d—12 M., pulse 72; 20 drops given. 1½ P. M., pulse 71. 3 P. M., pulse 67.

March 3d—12 M., pulse 74; 25 drops given. 2 P. M., pulse 74; repeat the dose. 3 P. M., pulse 67. 3½ P. M., pulse 72.

March 4th—11 A. M., pulse 80. 12½, pulse 72. 3 P. M., pulse 68. No phosphorus was given to-day.

March 5th—12 M., pulse 74; 40 drops given. 1 P. M., pulse 74. 3 P. M., pulse 66.

March 6th—9 A. M., pulse 70; 60 drops given. 10 A. M., pulse

71. 1 P. M., pulse 74; 70 drops given. 2 P. M., pulse 72. 3 P. M., pulse 80. 4 P. M., pulse 80.

March 7th—12 M., pulse 78. 12½, pulse 64; 80 drops given. 2½ P. M., pulse 76.

March 8th—Half after twelve o'clock, pulse 74; 100 drops given. 1½ P. M., pulse 76. 2½ P. M., pulse 74. 3 P. M., pulse 74; 10 drops given. 4½ P. M., pulse 84; 21 drops given. 6¼ P. M., pulse 84.

March 9th—8 A. M., pulse 74; 2 drops given. 9 A. M., pulse 74; 30 drops given. 10 A. M., pulse 76; 40 drops given. 10½ A. M., pulse 76. 2 P. M., pulse 70. 3 P. M., pulse 78; 50 drops given. 3½ P. M., pulse 75; 50 drops given. 4 P. M., pulse 84. 5½ P. M., pulse 84; 100 drops given—making in all 272 drops of the saturated tincture administered during the day. 6½ P. M., pulse 82.

March 10th—7½ A. M., pulse 76. 8½ A. M., pulse 70. 10 A. M., pulse 80. 11 A. M., pulse 80. 12 M., pulse 82. 1½ P. M., pulse 76. 2 P. M., pulse 68. 3 P. M., pulse 74. 4½ P. M., pulse 80. 6 P. M., pulse 82. No phosphorus was administered to-day.

March 11th—No phosphorus was given, but the pulse being counted at intervals, was found to vary about as it did yesterday.

March 12th—10 A. M., pulse 76; 100 drops given. 11 A. M., pulse 82. 1 P. M., pulse 74. 2 P. M., pulse 82.

March 13th—8 A. M., pulse 72; 2 drops given. 9½ A. M., pulse 80. 10½ A. M., pulse 84; 2 drops given. 1½ P. M., pulse 78; 2 drops given. 2½ P. M., pulse 80; 2 drops given. 3½ P. M., pulse 78; 2 drops given. 4½ P. M., pulse 80.

March 14th—9 A. M., pulse 74; 150 drops given. 9½ A. M., pulse 74. 12½, pulse 84. 1½ P. M., pulse 80.

March 15th—9 A. M., pulse 72; 200 drops of the saturated tincture given. 10 A. M., pulse 72. 11 A. M., pulse 74. The subject was questioned and examined particularly. There was no nausea or vomiting; no burning sensation, or feeling of oppression in the stomach or epigastric region; in fact, he says he feels "very well." 1 P. M., pulse 76. A short time before he took the two hundred drops he ate a hearty breakfast, and at 2 P. M., with decided relish, a substantial dinner of bacon, cabbage, potatoes and corn bread. 4 P. M., pulse 80.

How much further the dose might be augmented with safety and without *appreciable effect*, I am at present unprepared to say; but reasons I think will appear as we proceed, that will render it not very

improbable that the quantity of alcohol rather than any suppositious quantity of phosphorus, the preparations, as prescribed and given may contain, should form the only necessary limitation of the dose. To be ready for the press, I must here close my experiments so far as they are to be used in the present paper, though I shall probably continue them hereafter.

In my experiments, two methods of administration were principally pursued. In one, the tincture was dropped into a glass with water and given to the subject, sometimes before the entire disengagement of the whitish vapor that rises as the solution is mixed with water; and often a part of the vapor would escape with the first expiration after the dose was swallowed. In the other method, the solution was dropped into a vial with water; the vial was then corked and shaken, and the dose poured into a glass when given. These are the two plans pursued it would seem by those who have used the article here as a curative agent in pneumonia. The latter seems to be the one adopted in the prescription given by Dr. Ames in his paper. The preparation that I at first used in my experiments I obtained from the Apothecary from whom I am in the habit principally of procuring medicines for my own use, and who prepared it at my request; but though I had no reason to suppose that the article thus procured was not of good quality, I subsequently supplied myself with both the diluted and saturated tinctures from the Apothecary who prepared the tinctures used by Doctor Ames.

As to the effect of the article upon the pulse, others may judge from the experiments recorded as well as myself. To me as a sedative it appeared to be entirely null; and this I apprehend will be the ultimate verdict of the profession. From my previous reading I had been led to expect a stimulant action from the larger doses, but no appreciable effect of the kind was observed that was not fairly attributable to another and more obvious cause than the phosphorus, to wit, the alcohol. This I think, as the subject is further developed, will not seem at all strange. In not one single respect was any effect that I could discover produced by the phosphorus. In no instance was there either disturbance of the stomach, nausea, vomiting, a burning sensation or a feeling of oppression at the epigastrium, though from the quantity of alcohol alone in the last two doses taken by Sam, something of the kind might not unreasonably have been expected. In no instance was there any alteration in the appearance of the tongue, diminution of

appetite, or appreciable alteration or disturbance of the system in any way. My subjects ate with a good appetite, drank, slept, and in every respect deported themselves—while all their functions apparently were performed—as if nothing had been given; a temporary alcoholic excitement in the case of Sam after the last two doses alone excepted. When my subjects were asked how they felt after taking a dose, large or small, the answer generally given was, "very well," or "first rate."

It is stated in effect by Doctor Ames, that when the dose of phosphorus is enlarged beyond a certain point, the sedative influence is counteracted or prevented by a stimulant action, the result of the "local inflammation it excites" in the stomach and bowels. Now as nothing of this preventive or counteracting character occurred in my cases—the sedative action of the article therefore not being in the slightest degree interfered with, seeing that in the experiments mentioned by Doctor Ames the pulse was *invariably reduced in frequency* from eight to twelve beats in the minute in the course of from an hour to an hour and a half by a *single dose of two drops* of the saturated tincture—to what an extreme state of sedation should my subject have been depressed who took two hundred (200) drops of the same at a single dose, and again one hundred drops at one dose, and two doses of fifty drops each, besides several smaller doses, making in the aggregate two hundred and seventy-two (272) drops, all in nine hours and a half; while the degree of inflammation of the stomach and bowels that should have occurred, but did not, is awful, even to think of.

Further, in regard to the stimulant action of these tinctures. It will be observed, that after the administration of several of the doses, a slight *increase* in the frequency of the pulse took place. Still, the variation does not seem greater, omitting the doses of the 13th and 17th of March taken by Sam (in which so obvious a cause for the excitement was present, as in one instance an ounce, and in the other about half an ounce of alcohol,) than on other occasions where the smaller doses were given, or when it was omitted entirely. If, however, it should be supposed by any that a stimulant action from phosphorus was manifested on the occasion referred to, the absence of all symptoms of the local inflammation of the stomach and bowels, to which Doctor Ames regards the stimulant action as secondary and consequential, would show that it could not in my experiments be fairly attributed to any such cause. It would seem not improbable then, admitting that any stimulant effect really was produced by the phos-

phorus, that the received explanation of authors of a diffusive operation may be the correct one. It will not be doubted, however, that phosphorus may be so administered as to produce a local irritant action upon the stomach, consequent upon which there would be a febrile reaction.

Besides the experiments given, I have also taken a good deal of the article; indeed, had taken it sometime before Icommenced its administration to Sam. I took it at first for a while with the occasional omission of a dose on account of absence from my office on professional business; but in one instance I took for eight successive days as much as five drops of the saturated tincture three times a day without the omission of a single dose, and in no respect did I ever experience any appreciable effect from its use. Though in every instance I was careful to drop out the full number intended, of five drops, I did not regard it as a matter of any importance when a few drops, say a half a dozen or so, more than that number happened to escape, but took the dose as if but the intended number was contained in it.

In connection with this experimental practice upon myself, I will again call attention to the views of Dr. Ames in regard to the effects of the tinctures of phosphorus and their dose. Thus he says, speaking of the saturated tincture, that it "cannot be continued in the smallest quantity just mentioned,"—half a drop—"for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." Though he admits that in the quantity of two drops, "a *single* dose, or *perhaps a few* doses may be given with impunity," he would evidently regard any lengthened use of it in such a dose, as a very grave and serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses of two drops each, at intervals of twenty-four hours. It is most desirable that we should yet be able to discover and explain the cause of these discrepancies. While Doctor Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—being just ten times the quantity—a long time, and for eight days without omission of a single dose, without effect. While under his observation, from a cumulative action, dangerous effects resulted from three doses of two drops each, administered at intervals of twenty-four hours,

being in all six drops taken in the course of three days, yet I have taken for eight successive days three doses of five drops each, or fifteen drops per day, without effect. Indeed, unless I should discover something in its action, which has never as yet been manifested in any of my experiments, from my own experience with the article, and with all the lights at present before me, I should not hesitate, were it not for the mere trouble of the thing, to continue it in the same manner for years.

In the healthy subject at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet not only did the subjects of my experiments take it in doses as mentioned, so immeasurably greater than the doses with which such effects are said by Doctor Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus, while they sometimes took it in the middle of the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions I myself having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my vial, taken the dose, and proceeded with my meal without disrelish or any subsequent manifest effect.

A word or two of explanation in reference to the dose recommended by Doctor Ames, that I may not be charged with rashness or temerity on account of the doses given to my subjects, may not be out of place. It will be seen that these took at different times doses of various sizes, from the favorite one of Doctor Ames—half a drop of the diluted tincture—up to two hundred drops of the saturated tincture.

It is supposed by Doctor Ames that the saturated alcoholic tincture contains four grains to the ounce. The diluted tincture would in that case contain four grains to ten ounces, or there would be one grain in two and a half fluid ounces. Doctor Ames' dose then of half a drop, supposing the drop to be equal to the minim, would be the one-twenty-four-hundredth ($\frac{1}{2400}$) part of a grain, there being twelve hundred minims in two and a half fluid ounces. But the drop is *not* equal to the minim. While there are but sixty minims in the fluidrachm—Professor Wood tells us that it takes one hundred and thirty-eight

drops of alcohol to make a fluidrachm—it would seem scarcely probable that the small quantity of phosphorus held in the diluted tincture would cause any material variation in the size of the drop. In sundry trials made by myself in which several vials were used, I found that the number of drops of the diluted tincture necessary to make a fluidrachm varied with the shape of the vial and the thickness of the lip, from 100 to 125. Let us take, however, the smaller number, though the drops in that case will be found above the average size. Of these there would be just two thousand in the two and a half ounces of the diluted tincture, and each drop therefore would contain the one-two-thousandth ($\frac{1}{2000}$) part of a grain, consequently the half drop dose would contain the one-four-thousandth ($\frac{1}{4000}$) part of a grain; or there are four thousand of Dr. Ames' doses in a single grain. The strength of the saturated tincture is ten times that of the diluted tincture, consequently my subject who swallowed two hundred drops of the former at a single dose, took what was equal to four thousand (4000) of Doctor Ames' doses of the diluted tincture, and this without any effect.

The above estimates are all based upon the supposition of the correctness of the assumption of Doctor Ames, that the saturated alcoholic tincture contains in solution four grains of phosphorus to the ounce, as the etherial tincture it is said does. We are not told by Doctor Ames whether he has any authority for this opinion, nor does he speak of any experiments performed by himself to test the solubility of phosphorus in alcohol. It would appear to be then a mere supposition, that an ounce of the saturated tincture contains four grains. Without some experimental tests we cannot feel certain that it may not contain more, or that it may not contain less, than the quantity mentioned. No author that I have consulted says anything as to the exact quantity of phosphorus that any given quantity of alcohol will dissolve. Many of our very highest authorities on the Materia Medica do not even speak of an alcoholic solution; and those who refer to it at all, at least all that I have consulted, speak of phosphorus as very insoluble in alcohol as compared with its solubility in ether.

In the absence then of any definite statements on the subject, I was led to institute myself a series of experiments with the view of ascertaining with greater certainty the solubility of phosphorus in alcohol. In a vial containing one ounce of anhydrous alcohol, I placed

four grains of phosphorus, in another two grains, and in another one grain. At the end of fourteen days—the time usually considered necessary for the preparation of tinctures by maceration, the time directed by a majority of the pharmacopœias—of the four grains, about one-fourth or less was dissolved; of the two grains, about one-half or less, and of the single grain, there still remained a portion undissolved. On testing these several tinctures by dropping them in water, they all gave off the white vapors as freely as any of the tinctures supplied to me by the apothecaries. It is fair I think to say then, that the saturated alcoholic tincture, instead of containing in solution four grains to the ounce, contains in reality but about one grain to the ounce. It may possibly be a little more; it would seem as likely to be less. The favorite preparation then of Dr. Ames—the diluted tincture—would contain one grain to ten ounces. Allowing, as heretofore, one hundred drops to be equal to sixty minims, or a fluidrachm—and this is a low estimate—we have in the ten ounces eight thousand (8000) drops. Eight thousand drops then of the diluted tincture will contain one grain of phosphorus, and one drop will contain the one-eight-thousandth ($\frac{1}{8000}$) part of a grain. The favorite dose of this tincture with Dr. Ames is half a drop, and the quantity of phosphorus consequently contained in it is the one-sixteen-thousandth ($\frac{1}{16000}$) part of a grain.

But it is very probable, even, that not all, indeed that but a small proportion of the almost inconceivably minute quantity of phosphorus contained in the dose is received by the patient. In using the saturated tincture or solution at the moment of contact between the drop and the water, a quantity of vapor is given off in the form either of phosphorus as such set free, or as one of its acids formed in consequence of chemical decomposition and combination. Possibly this may be the cause of the apparent nullity of effect in my experiments; and if so we are necessarily forced to the supposition of an equal nullity, from the same cause, in the hands of others, who use the alcoholic solution suspended in water; and this is the method pursued by Doctor Ames. Now it does not seem at all improbable, on the contrary extremely probable, that the action or chemical change alluded to, attended with a proportionate loss, also takes place when the diluted tincture is combined with the water, though the disengaged vapor, on account of the minute quantity of phosphorus contained, may not be appreciable

to the sight. The alcoholic solution or tincture, it is more than probable then, is not the best preparation of phosphorus, by which to secure with any certainty, its full and peculiar operation, whatever this may be, upon the system. Lobstein, who seems to have made the subject of phosphorus something of a hobby, prefers its administration in the form of etherial tincture, and tells us that such a preparation "is not decomposed by cold water," as seems to be the case with the alcoholic tincture.

I have heard it remarked by individuals that certain physicians* (naming them) must be in the habit of using *very strong* medicine, for that when they dropped it in water "it fairly smoked." Had the dropping been conducted in the dark, the conflagration from which the smoke was given off, might also have been seen. It would not, therefore, however it is probable, have been believed, that the strength of the medicine was all smoke—vapor—but rather that "'twas spirit Pandemonium," at the very least.

Lobstein—who from the case to be referred to, there is grounds to suspect, must have been decidedly something of a Charlatan—once, from his own account, took advantage of one of the singularities of phosphorus, to operate to an extent beyond the influence of mere smoke, upon the imaginations of the persons surrounding a patient that he had been called to see. A woman fainted. He gave her a few drops of phosphoric ether. "In two minutes after," he tells us, "a flame was observed to proceed from her mouth, which caused all present to cry out with astonishment. I observed, humorously," he goes on, "that she had a devil in her. Nevertheless, I gave her a second dose, and after a short time she opened her eyes and exclaimed 'ah, I feel very well!' The bystanders were so much surprised that they really believed that the devil had flown out of her mouth,"—exorcised, as a matter of course, by the medicine given.

I have the permission of my friend Doctor Baldwin to say, that he has performed a series of experiments somewhat similar to mine,—has even given larger doses—and with like success.

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*With *several* of the physicians of Montgomery, who are no doubt indebted principally to Doctor Ames for directing their attention to it, phosphorus, in the form spoken of in the text, has been for some three or four years past, quite a favorite remedy in pneumonia, and one to which a remarkable degree of success claimed, in the treatment of this disease, is, in a considerable degree, ascribed by them.

In conclusion, I would remark, that though prior to the experiments here recorded, I had taken and given, experimentally, a good deal of the alcoholic solutions of phosphorus, I have never, in any instance, given them in a case of acute or dangerous disease. It would seem to have been truly said, that "there are more false *facts* than false theories in medicine."

ART. V.—AN INQUIRY, ANALOGICAL AND EXPERIMENTAL, INTO THE DIFFERENT ELECTRICAL CONDITIONS OF ARTERIAL AND VENOUS BLOOD.

BY JOHN GORRIE, M. D.

[Continued from page 602.]

In the preceding number of this Journal I deduced, from the analogies afforded by the mechanical structure of the blood globules, the general physical properties of arterial and venous blood, the structure of the arterial coats, the comparative chemical composition of the two kinds of blood, and the probable dependence of some functions of the organic system upon electricity, the hypothetical conclusion that the normal electrical condition of arterial is one of higher tension than that of venous blood.

Before entering upon the second part of my inquiry, I deem it proper to remark, that in the physical sciences generally, and in physiology particularly, there are many propositions which have not been, and perhaps cannot be, proved by experiment, that, nevertheless, the most matter of fact mind does and must look upon as indisputably true. It is admitted as a sound principle of inductive philosophy, that where analogy accords rationally with the cause, adaptation and end of an object, it is not absolutely indispensable to its credibility that it should be tested by experiment. If this test of truth were always necessary to the progress of knowledge, its acquisition would often be seriously impeded, and sometimes wholly arrested. In the assumption that the blood is endowed with electrical action, I assume the existence of a force which, besides its support from analogy, the

wants of the animal system seem so imperatively to require, and which harmonizes so well with the phenomena of life that, it might appear, experiment was scarcely required to strengthen the position.

But however valuable logical deductions and the ingenious adaptation of means to ends may be in the pursuit of knowledge, they are not deemed sufficient to establish the existence of a physiological fact, if its nature admits of being proved by experiment or direct observation. Though furnishing principles towards elucidating every science upon which the learned and unlearned are compelled to act, they cannot be admitted as positive and certain sources of knowledge. Indispensable to the progress of physiological research, from the light they throw upon parts of the organic structure remote from observation, or beyond the reach of experiment, and imparting interest which urges to the investigation of doubtful questions, they are yet of more value as illustrations than as direct proofs.

In numerous instances of physiological research, where analogical reasoning establishes an affirmative position, it may not be within the bounds of experiment or observation to either confirm or demonstrate its falseness; the utmost it can do is to show a negative result. A simple experiment, referred to by Dunglison,* in relation to our theme, illustrates the position. "Muller," he says, "was unable to discover with the galvanometer any electric current in the blood, and he perceived no variation in the needle of the multiplicator when he inserted one wire into an artery of a living animal and the other into a vein." But in physiological, as in ethical science, the proof of a negative can never be more than presumptive. To deny a position founded upon positive deductions, whether furnished by analogical or moral evidence, cannot be deemed sufficient to set it aside. The experiment of Muller, though affording no confirmation, is yet incapable of confuting my hypothesis.

Throughout nature the operations of the imponderable forces are generally pursued in a manner so slow and silent that they are with difficulty perceived by the senses, even when aided by the most ingenious artificial apparatus. Their effects are commonly manifested in a way which the philosopher's art can neither imitate nor explain.

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*Physiology, Vol. 2, p. 122—7th Edition.

Even when producing the most stupendous results on the inorganic world, we often find it impossible to obtain direct evidences of their nature; and they have always such an energetic tendency to equilibrium that where the cause is satisfactorily known to our reflections, an excess of it is seldom maintained long enough to admit of a deliberate examination of its properties by our senses.

In the homogeneous and good conducting properties of the components of organic bodies, we perceive the evidences that any disturbance in the equilibrium of their imponderable forces must be feeble in character and of short duration; and, indeed, if it were not subject to rapid renewal, it would be wholly beyond the reach of observation. For these reasons the very existence of electricity in a form proper to animal organization was long looked upon as equivocal and even denied. Consisting of slight deviations from, or rapid revolutions around a neutral point, the phenomena were so obscure that under the influence of an hoypothesis supposed to be universal in its application, they were only considered as partaking of those properties of electricity common to all matter. Though fairly deducible from the earliest as well as all subsequent galvanic experiments with organic elements, the distinct existence of animal electricity was not admitted till Matteucci, more than half a century after its discovery, showed that the facts refused to submit to explanation on any principle.

It is important to a correct understanding of the subject before us, that clear and distinct conceptions should be entertained of the inherent difficulties attending its experimental elucidation. If the indications of electricity in organic solids have hitherto been found obscure, we can readily conceive that it must be difficult to show the presence of so subtle and diffused an agent in organic fluids. The experiment of Muller is apparently the most simple in plan and the most appropriate in principle that could be devised for ascertaining the comparative electrical states of arterial and venous blood. It presents the blood, to a very delicate measurer of electricity, when in its most natural condition; when it is in constant motion, and consequently in that state in which it develops the electricity of friction; when it is excluded from the air and the causes that tend to alter its natural properties, and when it is maintained at an equable temperature. But it is subject to the objection, among others, that it exposes

an exceedingly small surface of blood to the delicate collecting and conducting wires of a galvanometer, and in this circumstance alone may be found the cause of its failure.

Under all circumstances there are principles which stand in the way of an experimental development of the electrical tension of the blood, the chief among which are the following:

1st. The action of electricity itself upon the blood. It is found that when an electrical current traverses, or is set free from blood, albumen coagulates at and forms an insulating coating around its positive pole, while its most prominent source of electricity, oxygen, is set free from the surface of contact. Indeed, the mere presence of a metal in blood, (which can hardly be dispensed with in forming an electrical circle) is sufficient to cause a coagulum upon its surface,* the effect of which forms another objection to Muller's experiment; for acting upon objects so small as the wires of a galvanometer, it must almost instantly intercept the current or even evolution of electricity, and probably before a circuit with such an instrument could be completed.

2d. The physical properties of blood. Experiments with the present object in view, made on blood out of the body, are divested of some of the advantages of those made on it within its vessels, and are subjected to some peculiar disadvantages. In conducting them the blood must be at rest, and is thus prevented from evolving the electricity of friction; it is exposed to the air, and to a change of properties from the mechanical action of that fluid; and it is liable to be lowered in temperature, and from this cause, to lose a portion of its electrical tension. The extent of the obstruction, under these circumstances, to a successful experiment, may be judged of from the assertion of Liebig, that "no other component part of the organism can be compared with blood in respect to the feeble resistance which it offers to change from exterior influences." Though the energy of its vital principle enables it to offer, for a short period, a powerful resistance to the ordinary influence of material agents, yet its physical properties are held together with so weak an affinity that "any disturbance, however trifling, or from whatever cause it may

*Carpenter's Physiology, page 533.

proceed, effects an immediate change in them." Of course the state of the imponderable forces must differ with every change in the physical properties of blood, and any disturbance from that state which characterizes its normal condition must rapidly tend to equilibrium.

3d. The chemical properties of blood. The points of resemblance between the chemical properties of arterial and venous blood have been stated to be numerous and intimate. It is also found that each kind possesses so little power of maintaining its few distinguishing peculiarities that the moment it is withdrawn from the body it tends—the arterial by its affinity for carbonic acid, the venous by absorbing oxygen—to assume the characteristics of the other. It is obvious, from a consideration of general electro-chemical principles, that any difference of electrical excitement between the two varieties, must be one of exceedingly low intensity, and of very difficult measurement.

With these explanations of the action and reaction of electricity and blood, and the extreme liability of the latter to a change of physical and chemical properties, we can readily understand that it must be difficult to obtain a practical manifestation of its electrical tension; and that, other things being equal, the difficulty must be greater when withdrawn from than when contained within its vessels. But however contrary in appearance to Nature, it will be presently shown that all the disadvantages of the latter mode of experimenting are compensated by the large quantity of blood and unlimited extent of surface it admits of being operated upon.

On account of the difficulties attending the experimental investigation of this whole subject in the direct way, the electro-physiologist, in place of it, has frequently resorted to, and seems generally to have been satisfied with, the less difficult but inverted or indirect mode. Though inferior to that of direct experiment, and conducted for the most part with objects in view different from the present, it has yet tended to elucidate the electrical states of the blood. It is obvious, that if the blood by a change of properties develops electricity, the influence between them must be mutual, and the latter in its turn must affect the blood so as also to change the chemical character of its constituents. In this inverted action as applied to the solids of the animal system, is comprised the most familiar experiments in electro-physiology: no muscle is insensible to the stimulus

of electricity; the digestive function has been carried on by it; and experiments indicate that even the sensorial powers may be set in motion by galvanic and electro-magnetic forces. If, then, it can be shown that the properties of both arterial and venous blood may be changed so as to make each the other by electrical action, it must be evident that not only is there a difference between their electrical conditions, but, also, that their peculiar nature is intimately connected with this difference.

The most observable alterations in the physical properties of blood are the loss of the black and the acquisition of the red color in venous, and the change of the red to black in arterial. That these modifications of color do not depend, as commonly supposed, upon a mere alteration of affinity for oxygen or any chemical change of composition, is evident from the experiments of Fourcroy,* since repeated by Sherer.† The former found that arterial blood left in oxygen gradually darkens, and that after this change, oxygen will not redden it; and the latter has shown "that the bright red color is dependent on other causes than oxidation, and that the dark tint of the venous does not arise from carbonic acid or carbon." Early in the history of galvanism, experimenters‡ announced a discovery, which, though for a long time doubted, has been more recently confirmed, viz: that the fibrine of the blood immediately after it leaves its vessels, may be made to contract by the galvanic apparatus. Dr. Philip not only puts this discovery past doubt by new evidence, but at the same time makes the additional observation, that if the galvanic instrument be applied to fresh drawn arterial blood, an evolution of heat amounting to 3° or 4° F. takes place while the blood assumes the venous hue. He adduces these facts as favorable to his hypothesis of the identity of the nervous and galvanic energies;‖ but whatever may be their value in this respect, they certainly lend some support to the doctrine of an electrical agency in the conversion of one kind of blood into another. It may be further mentioned in connection with this part of the subject, that Dr. Moran, in an inaugural thesis,§ asserts, as the result of the experiments instituted by himself, that recent venous

* Quoted by Thompson in Annals of Philosophy.
† Ranking's Abstract: vol. i, p. 286.
‡ Circaud, and subsequently Delamatherie.
‖ Bostock's Physiology: vol. ii, page 225.
§ De Effectibus Electricitatis Quibusdam. 1820.

blood, acted on by galvanism, becomes at the positive pole, blacker and thicker, but at the negative pole, redder, thinner and spumous. On the other hand, Dr. Miller, in an Appendix* to Philip's "Treatise on Chronic Diseases," affirms that the positive pole of a galvanic battery, immersed in venous blood, removes the modena and restores the florid color; and, on the contrary, the negative pole deprives arterial blood of its florid hue, and substitutes the dark or modena. The discrepancy in the reported results of so simple an experiment may be owing to a careless or confused use of the terms negative and positive, or to a want of the requisite precautions in one or the other of the experimenters, to guard against deception in the observations. In repeating the experiment, with some modifications, the results appeared to me to conform to the representations of Dr. Miller. Upon a consideration of the whole of the experiments, they may be considered as establishing, so far as the inverse action of electricity can do so, that arterial and venous blood bear a positive and negative relation to each other; and, taken in connection with the facts stated by Fourcroy and Sherer, they indicate the probability, at least, that electricity is the efficient cause operating in the change from one kind to the other.

The results of indirect or inverse experiments, like deductions from analogy, are not to be received as conclusive evidences of a scientific fact, when those of direct experiments are of possible attainment. It must, therefore, be regarded as fortunate, for the important object we are striving after, that it admits of being verified by what may be considered experiments of a direct character.

It must be obvious from the considerations heretofore presented, that an experiment which shall determine correctly, or even with an approximation to accuracy, the electrical relations of arterial and venous blood, demands very great delicacy of means and extreme precaution in their management. Though these fluids comprise a greater diversity of chemical composition than muscle and its tendinous aponeurosis, from which Matteucci demonstrated so readily the existence of an electrical current, yet, being more diluted and diffused in water, the contrivance which shows it in the latter is unaffected in the

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*Page 294.

former. To obtain a clear manifestation of an electrical relation between two animal fluids, it would, indeed, be desirable that some instrument, corresponding in delicacy with the balance of torsion for static, and the galvanometer for magnetic, and adapted to this peculiar electricity, should be invented. The prepared frog of Galvani furnishes this delicacy for indicating the apparently static form of the electricity of organic solids; but something even more delicate seems to be required to measure that developed by the contact of animal fluids. Besides sensitiveness of apparatus, it is essential to success that a large quantity of blood should be presented, over a very large surface, for the appropriate action of electricity. And, as it has been shown, that both arterial and venous blood approach very rapidly a common state, not only as regards their imponderable agencies, but also in the same striking degree their physical elements, the experiment should be made quickly, and, if possible, without exposure to the atmosphere.

To fulfill these indications as far as possible, and to insure accuracy of observation in the experiments about to be described, every precaution in my power was taken. I am aware that the precision which belongs to the importance, and should characterize the delicacy of electro-physiological experiments, has not been attained; and I fear the experiments will be contemned by those accustomed to the use of the nice appliances of a complete laboratory. But the idea involved, strongly impressed me with its truth and value, and I felt it a duty and pleasure to pursue it with such means as I could command to its practical development. Of the readiness of indication of some important portions of the apparatus I employed, I was unable to determine, from the want of a standard of comparison. Living in a frontier town, remote from cities, where the nice mechanical arts requisite for the construction of philosophical instruments are pursued as a business, I was compelled to supply, by my own ingenuity, any deficiency thence arising; and the apparatus I used was of domestic construction. In the course of the experiments a galvanometer was found indispensable; and as one of artistic construction was not to be obtained, the following was used as a substitute. Ten yards of fine copper wire covered with cotton thread were wound into the form of a parallelogram, having its sides equal in width to the length of the magnetic needle used,

and admitting of the motion of the latter within its area. This coil was placed upon a piece of varnished pine board as an insulating base. The needle was suspended by a silk thread from a support formed of copper wire, the ends of which were fastened to the piece of board and bent at right angles about six inches above the surface of the coil. The whole apparatus was covered by a bell glass to screen it from the action of the wind. It was not made a static, and, therefore, had not the delicacy of that form of the instrument. The delicacy of other portions of the apparatus was not a consideration of so great importance, and, on that account, I make no special reference to it.

Accuracy in physiological observation, like success in the experimental sciences generally, is seldom attained but after successive attempts and failures; and this truth was fully exemplified in the present investigation. It may subserve an interesting purpose to relate one among several experiments from which decided results were expected, but which failed totally, or produced either trivial or deceptive effects. Electricians tell us that a galvanic circle may be formed, and a current of electricity established, by the use of one metal and two liquids, as when a zinc plate is converted into a box, and acted upon on one side by diluted acid, and on the other by solution of common salt. Moreover, Aldini and many other electro-physiologists have obtained distinct signs of an electric current by touching the muscles and nerves of a frog with a single metal, and Matteucci by connecting two different parts of a muscle by galvanometer wires. For these reasons, I thought it possible similar evidences of electricity might be obtained by presenting large surfaces of venous and arterial blood to opposite sides of a single metalic conductor, or, what I considered the same thing, to two conductors of the same metal.

To make this experiment, I took two similar glass jars, each of about a pint capacity, and carefully increased their insulating properties by coating their outer surfaces with a varnish made of shellac dissolved in alcohol. Into each jar I introduced a sheet of zinc twenty-four inches long and four inches wide, cut from a larger sheet, and rolled into a form of a scroll. To the upper edge of each scroll a copper wire six inches long and the twentieth of an inch in diameter was soldered. The mouths of the jars were closed with corks covered with sealing wax, but each having an orifice of about one-fifth of an inch in diameter, to admit the passage of a flexible gum-elastic tube of the

same diameter; and another small hole through which the wire soldered to the scroll passed. These jars were placed upon a plate of window glass, and the whole put upon a pine table.

The apparatus being thus far ready for experiment, a galvanoscopic frog was next prepared after the manner described by Matteucci;* that is, by cutting through the middle of its spine, separating carefully all the integuments, muscles and bones of its thigh and pelvis, and dividing one of the lumbar plexuses of nerves as it passes out of the vertebral column. This, put into a glass tube covered with an insulating varnish, forms, with its long nervous filament (composed of the lumbar plexus and the crural nerve, extending beyond the end of the tube,) a most sensible electrometer for determining the presence of electricity in any two solid parts of an animal; and it was thought might be made also to measure that of animal fluids.

The blood experimented with was taken from sheep. Considerations connected with the properties of the globules have led me to infer that there is a relation between them and the electricity of the blood; and that in proportion as their number is greater or less the latter is greater or less. The blood of the sheep was selected because it contains in the normal state less globules than that of man, and hence it was concluded that if it manifested electrical tension, that of man must afford it in a higher state. This animal was also chosen for the experiment, because its timidity and passiveness rendered it easily manageable. Having laid bare and put a temporary ligature around the external jugular vein and carotid artery of a young and lively but full grown one, I introduced one of the flexible tubes before alluded to into an incision made in the former, and dividing the artery, I inserted about three-quarters of an inch of the section next to the heart into the other tube.† The other ends of the tubes were now passed through the perforations in the corks into the jars, and at the same time the ligatures were loosed from the vein and artery and slipped so as to include the tubes, and again tied.

Everything being considered ready for the experiment, the blood was allowed to flow into the jars; and its discharge was so regulated by the ligatures and pressure with the fingers, as to admit as nearly as possible an equal quantity into each jar. During the flow I applied

* Lectures on Physical Phenomena: page 177.

† It was necessary to insert the artery into the tube, because in the sheep this vessel is too small to admit the introduction of an inorganic tube through which blood would freely flow.

the delicate galvanoscope above described, holding it by its glass envelope so as to touch each of the wires connected with the zinc scrolls with different parts of the nerve; and though one of the two spectators present doubted whether he perceived any motion, the other and myself saw distinct but slight contractions of the leg, which were renewed two or three times, or as often as the contact was broken and the nerve reapplied. When about twenty cubic inches of blood had flown into each jar, the animal became convulsed, and the communication between the jars and the blood vessels was broken, and with the separation all galvanic influence ceased.

In a similar experiment, and as a further test of the supposed efficiency of this mode of experimenting, the conducting wires from the scrolls were connected with the wires of the galvanometer, and immediately a sensible agitation and a slight deflection of the needle, with an elevation of the north, and, of course, a depression of the south pole, were perceived.

This experiment may be considered the same in principle as that of Muller, before mentioned, and differing in execution only in the extent of surface of blood brought into action and in being withdrawn from the body. The scrolls of zinc acted merely as collectors and conductors of the electricity of the two kinds of blood, taking no share in the manifestation of force. But though the effects produced were consistent with what we may suppose the action of electricity from animal liquids would be, yet as they may have arisen from impurities in the zinc, or possibly from the shaking of the apparatus, and were altogether so minute and uncertain, they cannot be considered conclusive.

Unable to obtain sufficiently satisfactory evidence of an electrical current between arterial and venous blood by an instrument in which the circle was formed of a single metal and the two liquids, I resorted to the use of a battery constructed, on the general principles of galvanic machines, of two metals. This plan appeared to me to promise greater if not more accurate results. The modification of the instrument I employed is the simple circle made by rolling two metallic sheets, one of copper and the other of zinc, into the form of a double scroll. Each sheet was fifteen inches long and four inches wide, and was separated from the other by strips of gutta percha; it was furnished with a wire terminating in a cup to contain mercury for the convenience of easily making and breaking the circuit. The double

scroll was placed in one of the glass jars prepared as before mentioned.

This instrument and a galvanoscopic frog being ready, I laid bare the external jugular vein of another sheep destined for slaughter, and introduced into it one end of a flexible tube and inserted the other into the jar containing the battery. As soon as the blood was allowed to flow into the apparatus and the nerve of the frog was applied to the wire conductors, the limb was seen strongly convulsed, and the convulsion was repeated as often as the nerve was reapplied during the flow. When the quantity of blood reached a mark on the jar, which by previous measurement indicated twenty cubic inches, the tube was withdrawn from the vein, and the violence of the contractions of the muscles of the frog were immediately lessened, and soon ceased.

After freeing the battery of the venous blood, the carotid artery of the sheep was laid bare and divided, and, as in the former experiment, inserted into the flexible tube, the further end of which entered the glass jar. As with the venous blood so with the arterial; the flow, on the application of the nerve to the conductors, was instantly accompanied by a contraction of the muscles of the frog; and the contraction was apparently more violent than that from the venous blood, and increased as the quantity of blood increased.

These experiments, though neither so delicate, direct nor promising of accuracy as the former, prove to the satisfaction of the electrician the existence of an electric current; but as the physiologist knows that muscular contractions may arise from mechanical action and currents of heat, as well from volition or electricity, it is necessary to have recourse to the galvanometer, or some other unequivocal test of electricity, to place beyond doubt its existence in the blood. Moreover, such an instrument is indispensable to determine its relative quantity and its direction or character in each kind of blood.

The experiments just described were repeated on a vigorous young sheep, with the single change that instead of the nerve of a frog the wires of the galvanometer, inserted into the mercurial cups of the battery, were used to test the presence of electricity. As soon as the venous blood began to flow into the battery, the needle of the galvanometer was seen to be deflected, and with the increased flow the deflection was increased till the maximum divergence amounted to about

sixty-five degrees. When the battery was charged with arterial blood the same phenomena were presented with a maximum deflection of the needle of nearly seventy degrees.

The direction of the deflection, when the needle was suspended above the coil of wire, was in all the experiments towards the left hand, or west; but in one or two instances in which the needle was placed below the coil, it was towards the right hand, or east! The greater amount of it from the action of arterial than venous blood, shows that the electrical excitement was greater in the former, while it indicates a current flowing towards the latter; the former liquid bearing the relation of an anode or electro-negative body, and the latter that of the cathode or electro-positive body. Hence, the experiments may be considered as authorizing conclusions as to the kind, the comparative quantity, and even the absolute quantity of electricity in both species of blood.

It may be proper to remark that these experiments were not made consecutively within a short period of time; but have been carried on irregularly, and as opportunity or convenience would allow, over a series of years, and it is only after a long and earnest investigation of them in every practicable relation, that I have obtained my own consent to offer their results to the world.

The remarkable phenomena thus made manifest, though undoubtedly produced by the difference between the physical and chemical properties of arterial and venous blood, are not dependant on the saline matters contained in them, because the most careful analyses are incapable of detecting the slightest difference, in this respect, either in quantity or quality between one and the other. Nor are they to be considered as the mere effects of common electricity, produced by the mutual contact of three dissimilar bodies, but as the evidence of the different quantities of electricity in each kind of blood, because in that fluid only was there any difference of combination between the two electrical circles. In accordance with Galvani's hypothesis in relation to the solids, they show that a proper animal electricity is inherent in the blood, which, if we could devise a sufficiently delicate apparatus to render apparent, would not require the assistance of any external excitant for its development.

The cause of the phenomena is no doubt magnetism, induced by the electric current set free by the energy of the chemical changes of the

blood in the batteries. The difference in the intensity of the manifestation between the two kinds is owing to the arterial losing or lessening its distinctive chemical characteristics by combining with the zinc in a greater proportion than the venous. Every fresh accession of arterial blood by the battery caused it to lose more or less of its peculiar character and to set free a larger proportion of electricity, and in the same proportion the limb of the frog was convulsed, or the galvanometer needle was deflected. The process brings arterial blood so far as it is in contact with the zinc into the same state, in regard to electrical properties, as the venous. And these effects, insensible from a small collector of electricity, as from the wires of a galvanometer inserted into the blood vessels, and scarcely visible from a large surface in a simple circuit, formed of a single metal and the two liquids, are readily manifested by an equally large surface of a common form of galvanic battery.

Here then we may consider that through the attraction and repulsion manifested by the aid of the galvanoscopic frog and the galvanometer, in connection with the evidence afforded by analogy, we have ample proof of the existence in the blood of that primordeal force in an active state, which, in whatever form it may present itself to the understanding, we recognize as electricity. This proof, however, as a result of the investigations of one person, must be considered as adduced from a single experiment, and a single experiment on so novel, complex, intricate and obscure a subject, is of very little value. To throw a clear light upon electrical manifestations from organic fluids, the experiments ought to be many times repeated with better appliances than I possess, and by those more skilled in the use of electrical instruments and in making physiological vivisections than I can pretend to be; and thus, from the comparison of many and precise observations, deduce undoubted results.

I have inferred from both experiment and reflection on the nature of the electricity developed, that the other, and perhaps more marked and unequivocal evidences of an extraordinary presence of this universal energy, viz: the shock, the spark, chemical decomposition and change of temperature, cannot be rendered apparent; but the failure may be owing to the coarseness of the means employed or inefficiency in their application. Nor is this absence of effects or dissimilarity of

properties greater than between several other of the imponderables which are generally recognized as dependent on an identity of cause. Thus magnetism has never been seen with only one pole or one kind of magnetism; it is incapable of transferring its properties; it gives a permanent direction to an unobstructed needle; it cannot be insulated; in all these respects it finds no analogy in the effects arising from other forms of electricity, and yet no one doubts that it is other than a modification of this energy. It is not therefore indispensable to a proof of the existence of electricity in the blood, that it should manifest all and precisely the same phenomena that it does in inorganic, or even in other kinds of organic matter; yet I make no doubt their presence will be shown by future investigators.

Considering that we have proof of a disturbance of equilibrium and of the existence of electrical force in the blood, it will be proper to take a cursory but somewhat less general notice than has been heretofore made of the duties it may perform in the animal economy. I am fully aware of the prevailing tendency and error of the day to exaggerate the agency of electricity, and apply it to the explanation of every mystery in Nature. Like fire in the hands of the alchymists, we see philosophical speculators prone to regard it as a power without limit; one with which all things may be accomplished and without which nothing can be understood; and that, like fire, it has led to innumerable hypothesis and even experiments, which have been attended with no other result than the wisdom taught by painful failures. But the discovery of the electrical relation of arterial to venous blood, though new as a demonstrable fact, has been long expected; and as Nature makes nothing in vain, it is impossible to conceive of so energetic a force in the animal system without supposing it destined to fulfill important duties. Coming in aid of other physical discoveries of modern times, it meets the wants of philosophy, and will no doubt contribute to divest physiology of what remains of its speculative and metaphysical character.

The difference between the electricity of the two varieties of blood is seen to be slight, but accords alike with the physical and chemical difference in their properties; and, considered as a cause, is in perfect harmony with the physiological wants of the animal system. It is not without analogies showing that very great action may be pro-

duced in the vital economy by very slight physical causes. Setting aside the vaccine lymph as inapplicable, from its vital action, to an illustration of the subject, we know that strychnine introduced into the system in too small a quantity to be detected in the blood by chemical analysis, will produce very considerable convulsive action over the whole muscular system. To enter upon an inquiry concerning the manner in which the electrical energy acts upon the animal functions, would at present be premature, because, from the nature of the subject, it would require a long and elaborate train of previous investigation. At some future day, when the accuracy of the discovery shall be confirmed by renewed and extended experiments, it will be shown that in conjunction with other physical forces, it will readily and fully explain the function of nutrition, and, as already intimated, the systemic capillary circulation. On the present occasion I will simply remark, in exposition of the method I propose to pursue, that Liebig* and other physiologists assure us the composition of muscular fibre, so far as chemical analysis can demonstrate it, is identical with that of dried blood; and hence the only difference between them, in the normal state, is that the former does not contain the free gases found in the latter. Now, this very slight difference of chemical composition is sufficient to indicate, upon the principles we have endeavored throughout this essay to elucidate, that there must be a difference of electrical condition, and consequently in accordance with known electrical laws, an attraction between them. Moreover, Matteucci has shown that there is a current of electricity constantly flowing from the muscular fibre to its aponeurotic accompaniments, which must tend to diminish the electricity in it and increase the difference and attraction between it and blood. There cannot therefore be in the properties of electricity, or in the lowness of its tension, as detected by experiment in blood, any insuperable objection to its being considered the force through which the organic functions are made to operate.

Extended to pathology a solution of this interesting question may aid previous investigations, and tend to supercede the conjectural manner in which the nature of many diseases are still viewed. It

*Animal Chemistry, page 69.

may produce an epoch in this department of medical science analogous in importance to that formed in the last age by the discoveries of Bichat and his followers in general anatomy. Like them, it may increase the knowledge of the animal system, enlarge the domains of pathology, and give an impulse to the progress of every department of medicine. Here the parallel may end, for it is not derogating from the value of Bichat's labors to consider that they have accomplished all of importance that they are capable of effecting; and it may be said, that if we wish to give a higher perfection to medical science and advance it to that state which has been in all ages a great object of human toil, an exact science, we cannot look for aid to any other source of knowledge with a larger prospect of success than to the electricity of the blood.

In pursuit of this grand desideratum the modern physician has ceased to repose with the calm content that satisfied his predecessor upon the knowledge of disease in the living system which he may derive from an examination of the dead body. Equally unworthy of a blind reliance does he deem those ingenious but imaginary or improved laws of disease derived from the "existence and circulation of a nervous fluid, the presiding influence of the ganglionic system or the vital attractions and repulsions of the circulating fluids." Even the solid knowledge derived from the study of anatomy and physiology, though absolutely necessary, is alone deemed of little value towards establishing a complete pathology. There is a growing conviction that in addition to these studies, it is indispensable that all the methods to which the exact sciences owe their success should be adopted and energetically pursued if we wish to insure the continual progress of medicine.

As an example of recent improvement in medical science, from which high hopes of utility have been predicated, organic chemistry may be mentioned. Under the influence of this science the changes wrought upon the food by the solvent juices of the mouth and stomach, and the admixture of bile, pancreatic liquor and intestinal secretions; the new combinations which the blood forms by its assimilation and admixture with the chyle; the changes which it enters into in the glands, the membranes, the skin and the various tissues; and the mutual chemical action of this fluid and the atmosphere in respiration,

have been carefully examined. By these labors new lights have been thrown upon physiology; and new views of pathology, having an important bearing upon practical medicine, have been established. Applied to the blood, they have unfolded its composition in health and disease, and have enabled us to form accurate conceptions of the diagnosis, as well as aided us in the treatment of a numerous class of diseases. But the chemistry of the living system, unlike that of our laboratories, furnishes no clue to its modes of operation; we know of its existence only by its results. Hence, in its relation to diseases in general, it has by no means attained the practical value that might otherwise have been expected from it; and able men have spent much time and patient assiduity in pathologico-chemical researches without at all promoting the progress of medical science.

It may be regarded as an axiom of medical science, that there can be no physiological action without its influence over pathological states of the system; and, therefore, in the discovery of the electrical relation of arterial to venous blood, a new field has been opened which may supply all that experience has shown to be wanting in pathology. Modern researches in this science have deduced the clear conclusion that diseases may arise from the action of morbific matter which has been directly introduced into the current of the circulating fluid, and which has affected both its physical and vital properties;* and in the course of this essay, I have adduced sufficient reasons to warrant the probable conclusion that this morbific agent may be, at least sometimes, a redundancy or deficiency of electricity in the blood. But at present the subject is merely inchoate and suggestive; the first consideration to its investigation should be a determination of what is the usual proportion of electricity in the blood; and next, how far, in its free or combined state, it may vary from such a standard; to neither of which has more than an approach been made. All that we know with an approximation to certainty, is that it bears an equal relation to life with organic chemistry, and must exert a similar influence over the changes produced in the various portions of the organism. The relation points to a simple and rational view of the nature of disease, and through an intimate and accurate examination of it the physician

* Carpenter's Physiology: page 535.

may be enabled to find a more simple and certain method of treating the latter than he at present employs, and thus create a prouder destiny for his science.

In the administration of medicines the value of such a discovery may be readily conceived to be important. For slow as the progress of the other departments of medical science has been, and imperfect as their condition still is, there is much on which the physician may congratulate himself, in comparison with the knowledge he possesses of the action of medicines on the human system and their agency in curing disease. The proof of a comparative electricity in the blood asserts a liability to a pathological condition, requiring electro-therapeutical principles to remedy. That medicines act remedially through their electrical attractions and repulsions, has been vaguely guessed at by every recent projector in medical science, and can scarcely be doubted by any one who considers the present state of electro-physiology; and though this is not their only mode of operation, yet it may be presumed that an investigation of their electrical properties will often aid physicians in determining on the propriety of their administration. Modern chemistry has done much by separating the active principles of medicines from the inert matter which encumbers them in their natural state, to facilitate a knowledge of their electrical relations to other bodies, and it is within the range of a probable science that these relations to the blood, and through them to disease, will be accurately determined. When this is accomplished we shall be able to bring to light many obscure operations of medicines, obtain a base on which to construct new and grand principles in therapeutics, and place the power of alleviating human suffering on as secure a foundation as the complicated and peculiar nature of a living being will admit.

To the considerations already presented, recommending this subject to the attention of physicians, there are others which urge them to its examination. Physiology is the basis of all medical improvement, and in precise proportion as our survey of it becomes more accurate and extended, it is rendered more solid. The wonderful structure of the animal system will probably never permit us to look upon it as a merely physical apparatus, yet the demands of science require that the evidently magnified principles of vitality should be reduced to their

natural spheres, or if truth requires, wholly subverted in favor of those more cognizable by the human understanding. The spirit of the age will not tolerate in the devotee to science a quiet indifference, whether a conjectural cause of any of nature's phenomena be true or not, and still less will it sanction a satisfied unconcern as to the source whence the human functions spring. From the physician, as emphatically the student of nature, is expected not only an inquiry into cause, but an investigation of the whole empire of nature and a determination of the applicability of every species of knowledge to the improvement of his art. While the mode of action of a single disease to which his fellow man is liable continues beyond his comprehension, or he is unable to direct its course to a state of health, both his science and art are imperfect, and he is imperatively called upon to look diligently for their improvement. Enlarging his views and extending the boundaries of nature, offer the most obvious and simple means of effecting this improvement. Every accession to a knowledge of the electricity of the animal system opens a new field for his exploration, undoubtedly extends the boundary of nature, makes an approach to the knowledge of vitality, and holds out a promise of an increased ability to manage it for the benefit of his race.

In conclusion I may remark, that if the imperfect inquiry I have been enabled to make shall prove to be founded on ascertained facts, and my deductions shall be found legitimately drawn, a new electro-vital affinity will have been established. That such an affinity exists in the animal system, and that a knowledge of it may be considered to have a practical tendency to advance both the science and art of medicine, I have no doubt, but to what extent I have succeeded in demonstrating the former, or how far my suggestions are correct in regard to the latter, I am not the proper person, nor will it be left to me to say. I am pursuaded that difficulties will have to be overcome, and discoveries will have to be made before its connection with disease will assume a position according with the rigid demands of science; and though I believe I have taken an important step towards the attainment of this object, yet, from the intricacy and delicacy of the subject, and my limited knowledge of experimental electro-physiology, I am not without distrust in regard to the accuracy of some of my conclusions.

ART. VI.—IODIDE OF POTASSIUM IN LEAD POISONING.

BY SAMUEL L. GRIER, M. D.,

OF ADAMS COUNTY, MISS.

I beg leave to report, through the New Orleans Medical and Surgical Journal, a few cases of lead poisoning which recently occurred in my practice, the treatment of which, and the success attending it, may serve to confirm, in some measure, the theory of M. Melsens, in regard to the action of iodide of potassium as an antidote to lead and mercury. The views of M. Melsens, if sustained by practical testimony, will certainly establish a most important principle in toxicology, and must be considered among the most valuable of the many contributions which Chemistry has made to Medical Science.

On the 15th of November last, I was called to the "Anchorage" plantation, where I found a woman suffering violent abdominal pains, accompanied with bilious vomiting. There was no perceptible acceleration of the pulse, but apparently an increased volume of the same. No fever; the tongue natural, and an entire absence of any indications of inflammation of the abdominal viscera. Supposing it to be a case of bilious colic, I made use of venesection; prescribed a mercurial cathartic, combined with an anodyne, and directed a blister to be applied over the seat of pain. At my next visit, after an absence of twenty-four hours, I found the girl somewhat relieved, but was informed there were several other cases in the quarters, who were affected in a similar manner, but with less violent symptoms. I found in all, some eight cases that were suffering the same abdominal pain, with vomiting in almost every instance, and attended in all by a constipated condition of the bowels. This prevalence of the disorder at once suggested the idea of some poisonous substance taken into the stomach, as the common cause; but to find what this could be, was attended with some difficulty, and some time elapsed before the producing cause was discovered. I had the cisterns examined, expecting to find leaden pumps; but no such cause existed, and it was not until my third visit to the plantation that an explanation of the mystery was supplied. Some of the *meal* which the hands were eating was brought to me, in which I discovered particles of lead which had

evidently been triturated with the grain. On further investigation it was found that in repairing the mill some time previously, lead had been used to fix the spindle; this lump of metal, commonly called the "bushing" of the mill, had dropped from its position, and so became subject to the action of the stones in grinding. In examining the patients for further evidences of the effects of lead, I noticed, what I had before overlooked, the blue margin of the gums, more or less distinct in all of them, the number now amounting to twelve or more.

Finding the course of treatment which up to this time had been employed, had in most instances afforded but temporary relief, I determined to try the effect of the iodide of potassium, as recommended by M. Melsens, and which has been presented to the notice of the profession, through the medium of a translation, by most of our medical journals. I made a solution of the iodide, and placed *three* patients under its use, aiming to give about 30 or 40 grains daily in divided doses. Under its administration I observed a gradual but most decided improvement in these three cases, while the other cases under the ordinary palliative treatment, showed a greater tendency to a recurrence of the same symptoms, after a short period of relief. I then began its use with all the negroes under treatment, amounting by this time to more than twenty in number, and about two-thirds of the whole plantation force. In many of them a most marked and immediate improvement followed the change of treatment, and in the course of a few days I had the satisfaction of seeing that all of them were decidedly convalescent.

It is proper to state, however, that in many of these cases, the result was rendered less demonstrative from the fact that other remedial means were used simultaneously with the iodide. These were chiefly blisters to the abdomen, anodynes given internally when the paroxysms of pain were unusually severe, and cathartics with turpentine enemata to overcome the constipation, which was a prominent symptom. Some will perhaps pronounce these to have been the efficient curative agents, but such was not my conviction at the time, and I certainly had no other desire than to see the iodide fairly and impartially tested.

In further support of my opinion of the efficacy of the iodide of

potassium in these cases of saturnine poisoning, I will relate the history of an individual instance among them, in which a somewhat different plan was pursued. The *overseer* was attacked among the last on the place, and about the time the lead was discovered in the meal. His gums had the blue line around the margin very distinctly marked; he had very little vomiting, but entire anorexia, and suffered severe paroxysmal pain. The most obstinate constipation attended his attack, and he applied to me for something to relieve this particular symptom, which continued for several days with only temporary relief, obtained by the most persevering and active measures. His attack being prolonged some time after the negroes were convalescing, and finding at this time that the iodide had inadvertently been omitted in his case, I immediately placed him under the action of the same dose which had been administered to the others. Two days after, as I approached the place, I met him out riding on horseback, and was informed by him that the tendency to constipation was very much lessened, and that all his unpleasant symptoms were gradually disappearing.

I offer this hasty sketch of my trial of iodide of potassium in lead poisoning for what it may be worth. Others may deem it worth an experiment when occasion offers, and thus its value as an antidote will be finally and satisfactorily determined.

These people had been using the poisoned meal for three weeks or more, although some of them noticed it turned black when mixed with water. In no instance did it produce fatal effects, and in none was paralysis of the extremities induced, the ordinary remote effect of saturnine poisoning. In two cases dropsical effusions followed, and one suffered for a time from neuralgia of the chest and limbs. No other sendelæ were observed.

Before closing allow me to call the attention of country practitioners to the manner in which this poisoning was produced. Such an accident may occur at any time on plantations, and would very likely be overlooked until after it had worked extensive mischief. My friend, Dr. C. J. Stone, of Natchez, informs me that a similar instance of lead poisoning came under his observation in an adjoining county, in which very serious and fatal effects were produced before the cause was discovered and removed.

ART. VII—A CASE OF SHOULDER PRESENTATION—EVISCERATION.

BY W. TAYLOR, M. D.

OF TALLADEGA, ALA.

Although not an advocate for meddlesome interference in cases of labor, I beg leave to give the following example of a case in which it was necessary to eviscerate the fœtus in order to bring it away:

On the 18th of January last, I was called to see Sarah, property of E. McGee, Esq., living about five miles from town. She was a stout woman, of black complexion, æt. about 30, and a multipara, in the eighth month of pregnancy. Has had an attack of measles, which has induced premature labor.

Met the case in consultation with Dr. H. P. Graham, who had been with her some hours previously. Found the right arm presenting and protruding from the vulva. Head in the left iliac fossa, with the back looking anteriorly, and lying against the pelvis. We made every effort to bring down the feet, but could not do so, in consequence of the rigid and powerful contractions of the uterus. The doctor told me that the waters had been expelled before he reached the case. Finding it impossible to turn the fœtus (and it being dead,) I removed both arms at the shoulder joint with a common pocket-case bistoury (having no other instrument at hand,) and divided the spine between the second and third dorsal vertebra. It was necessary to use great caution in the operation, in order to avoid cutting the soft parts of the mother. After dividing the spine, I enlarged the incision with my fingers until I was enabled to introduce my hand into the cavity of the thorax, when I removed its contents; and introducing my hand still further, I lacerated the diaphragm and brought away the contents of the abdomen. This being effected, the fœtus was easily flexed upon itself, and was removed without further difficulty. The mother made a good recovery.

No one can deprecate the use of instruments in delivery more than the writer, and he only had recourse to them in this instance as a last resource. No one is more fully aware that the operation of eviscerating the fœtus, should in only very rare instances supersede the practice of turning under transverse presentations. But when he reached

the patient, he found that many hours had elapsed since the rupture of the membranes—the fœtal body was so firmly wedged within the pelvis that the feat of turning was rendered impossible, or would evidently be attended with the most imminent danger.

Under these exigencies, with the aid and advice of the attending physician, with no other instruments than those contained in a common pocket-case, and without any precise rules respecting the operation, we contented ourselves with endeavoring to get the fœtus away, piece-meal, in the best manner we could.

ADDENDUM BY THE EDITOR.

What has been termed spontaneous evolution or version in shoulder presentations, is but a forlorn hope; at least, it is a resource of Nature that cannot be safely relied on in all cases. Dr. William M. Boling, of Montgomery, Ala., in his late able essay on this subject, gives a more favorable view, together with examples from his own practice, from which he concludes that in the South a majority of cases of this kind terminate happily if not interfered with by art. A case where Nature had her own way is subjoined. An excellent lady, the mother of several children, removed with her family to a new remote neighborhood, between the Little Kanahway and Elk rivers, in Virginia. The nearest accoucheur lived fifty miles off. The route passed through a wilderness where no house had yet appeared for twenty-five miles. She was in labor; the arm presented; Nature relied on—from necessity, not choice—finally, when she was dying after two or three days' labor, she requested a friend to cut off the presenting arm of the child. Soon after Mrs. S. expired.

On the other hand Nature is great. In a case of this kind of malposition, having been called by a midwife of New Orleans, at a late period when turning was found impracticable, the lady—a *primipara*—young and vigorous, was bled freely; took an opiate; the natural forces caused the arm slowly to recede; the feet were brought down and the child delivered after a prolonged labor.

ART. VIII.—TWO CASES OF TRANSVERSE PRESENTATION.

BY JAMES S. DAVIS, M. D.,

OF SALEM, MISSISSIPPI.

Case 1st.—On the 4th of October, 1852, I was requested by Dr. R. S. Wily, of this place, to see with him a negro woman, the property of Capt. L———. Upon my arrival I was informed by the Doctor that the case was one of transverse presentation, with the right arm protruding; he further informed me that he and Dr. Ayres, who had also been called in, had made several ineffectual attempts to turn; at their solicitation I also made an attempt at turning, but from the extreme tenderness of the soft parts, as evinced by her piteous cries, I was compelled to abandon the operation, having met with no better success than my predecessors. Our patient had been in labor many hours, during which time the uterine contractions had been steady and severe, and being much fatigued ourselves, and thinking she needed repose, an anodyne was agreed upon and administered; after which it was agreed in consultation that embryotomy afforded her the only chance of delivery. The anodyne was repeated and fomentations ordered to be assiduously applied for the purpose of soothing the swollen and irritated parts. After waiting sufficient time, and being satisfied of the death of the fœtus, I made with Smellie's scissors a transverse incision below the axilla; after which, with a strong pocket-knife, having the blade wrapped up to within an inch of the point, several of the ribs were broken and divided; I then gave way to Dr. A., who with a hook proceeded to remove the thoracic viscera; just as he had drawn the lungs through the incision he remarked that an "evolution" was taking place, and immediately withdrew the hook. In a few minutes, to our surprise and gratification, the head ascended and the nates presented, and the fœtus was delivered without further manual aid. Immediate shampooing was used on the abdomen over the uterus to promote its contractions, and in due time the placenta was delivered. We remained with her until we were satisfied that permanent uterine contractions had taken place, when another anodyne was given, and we left her for the night. On the next day we all visited her again, and found her doing well.

From that time Dr. W. attended her. She made a rapid recovery, and in three weeks left with her master for Western Texas.

Case 2d.—On Sunday, 20th November, 1853, I was summoned to ride about eight miles in the country to see Mrs. M———, a large and robust woman, aged about twenty-five years, in labor with her first child. The messenger informed me that she had been in labor since the preceding Thursday night, during which time she had been attended by Mrs. Sledge, a very kind and intelligent midwife residing in the neighborhood. After having made some interrogatories of the lady and friends, I made a per vaginam examination, and found the os uteri dilated to the size of a five cent piece, and so high up as just to allow me to reach it with the point of the index finger; the lady informed me that she felt very much fatigued from the protracted labor and want of sleep; her pulse was over one hundred, full and strong; skin dry and hot; much thirst and restlessness. I at once abstracted eighteen or twenty ounces of blood and gave her a full dose of morphine, and left her to repose the balance of the night. About 5 o'clock, A. M., I was aroused and told that my services were needed. I found her having strong pains, which had commenced only a short time previously. On examination I found a bag of waters protruding, not of the usual wedge shape, but larger at the presenting part than at the os uteri. I was still unable to make out the presentation, owing to the extreme height of the os tincæl. The weather being very cool, I did not remain by the bedside of the patient all the time. While sitting at the fire I was admonished that the waters had been discharged. Again examining, I found with extreme regret the right arm presenting with the umbilical cord. I immediately made an attempt to turn, but owing to the powerful uterine contractions I was unable to reach the feet. I gave an opiate, waited some time and tried again, but with like success. I then frankly told the friends that an operation would have to be performed, and desired a consultation.

My partner not being at home, Dr. Whittow was sent for, and requested to bring the necessary instruments. When Dr. W. arrived (which was about noon,) he examined the case, and agreed with me that evisceration was necessary. Having satisfied ourselves by want of pulsation in the cord that life was extinct, we commenced the operation by making an intercostal opening under the arm, and having

fractured several ribs and made a crucial incision, we removed the thoracic and abdominal viscera, and then made an effort at delivery; but on account of the large size of the child, we did not succeed. Thinking that amputation of the presenting arm might facilitate the operation, it was taken off at the shoulder joint; but still, with the aid of hooks and other appliances, we could not succeed. Being worn out ourselves, and the patient complaining a great deal, we gave her an anodyne and laid down to rest awhile. It was not long before we were aroused by her cries and importunities. Again examining, we found the child had turned so far over as to present the left arm. We made some efforts by using the arm as a lever to move the head upwards; failing at this, traction was employed, with the hope that the child would double and come away; still our efforts proved abortive. Dr. W. then amputated the left arm. Having made some further incisions, and removing the remaining portions of viscera, the blunt hook was fastened around the spine, and strong efforts made to elevate the head and make the breach present. By using as much tractile force as we thought prudent, we at last, after sixteen hours hard work, doubled the child upon itself and delivered it; but with all our efforts we could not prevent the head from emerging first. The placenta was soon removed. We gave the mother a dose of morphia and placed her comfortably in bed, enjoining the strictest quietude. I visited the patient several times afterwards, and had the proud satisfaction of seeing her recover without the first untoward symptom.

In case 1st, I feel satisfied that spontaneous evolution would have taken place without an operation; in truth, it did occur before the viscera were extracted. The force that was used in removing the lungs, doubtless facilitated its occurrence. But in case 2d, I am assured from the length of time that intervened from the commencement of the operation to the termination of the labor (sixteen hours,) so fortunate a result could not have taken place. The large size of the child forbade our entertaining the least hope of so desirable a result. I should say that whenever evolution takes place, that the fœtus must either be not fully developed, or else it must be putrescent, or there must be on the part of the mother unusual pelvic capacity. And where a contrary state of things exists, delay, that would hazard the life of the mother in the least, would be exceedingly reprehensible.

ART. IX.—TWO CASES OF TETANUS.

The first by Dr. J. U. Ball, *of Louisiana; the second by* Dr. A. Poitevin, *of Mobile—Translated by the Editor from the Revue de Thérapeutique Médico-Chirurgicale.*

Dr. Ball says: On the 10th of October last I was called to see a sick boy at the plantation of Madame Woolfork, Parish of Iberville, La. Upon my arrival I found the boy rolling and twisting himself in every possible shape. Sometimes during the paroxysm he would rest only upon his head and heels, whilst his body would be raised into the shape of an arch. In a few instances the body would be bent forward so that the head and knees were in contact, and the patient rolled together like a ball; after which the patient would be bent to the side, &c. In this condition I found the patient; unable to articulate; eyes set, and writhing under pain of the most excruciating type. I inquired closely into the history of the case, when I was informed that the boy had been perfectly healthy up to the time of his present illness. Early in the morning I was informed that the patient complained of some slight headache; ate no breakfast, but did not feel sick enough to keep from work. He accordingly went to carting wood, and about 1 o'clock, P. M., he was found lying by the feet of his mules, rolling and twisting as above described. The manner in which the boy was found entangled with his mules, led me at once to suppose that he had received some local injury which gave rise to traumatic tetanus. I therefore examined the body minutely, without being able to discover any marks or bruises. I decided the case to be idiopathic tetanus, and treated him in the following manner: I administered chloroform in the outset to relax his system and relieve the intense pain under which he was at the time laboring. After the effects of the chloroform had passed off the exacerbation immediately returned. The patient being of a plethoric habit, I bled him freely, which was attended with a happy result. My next visit found the patient in great distress, bowels loose. I ordered the following: ℞. Ol. Ricini; Ol. Terebinthina, aa ℥ i. M.; to be given at a draught. Upon my next visit I found that the patient had passed a

great deal of indurated fæces; spasms somewhat lighter; interval longer. I continued the use of the chloroform. My next visit found him in great pain with spasm. I applied a blister along the course of his spine, and permitted him to drink freely of brandy, with sixty drops of laudanum three times a day, which was continued until the tenth day of his illness, when the patient was able to pursue his usual avocation.

Dr. POITEVIN says: During the two years in which I practiced medicine in Pensacola (Western Florida,) I have been called on to treat four cases of Traumatic Tetanus by the remedies usually employed: such as opium, mercurial frictions, cold and hot affusions, anti-spasmodics, etc. My four patients died.

A short time ago an English sailor, George Smith, a strong man, of a very sanguine temperament, had me called in. In working on board of his ship, he had through carelessness walked on the blade of an open knife which a companion had let fall; it cut the lower part of the big toe at its articulation with the metatarsus; the incision, which had little depth, might have been about half an inch long. At first he paid no attention to this slight cut, but continued at work, always taking care however to wash the wound twice a day to cleanse it, covering it afterwards with a piece of linen. Six days after this accident Smith sent for me; I found him with a strong trismus, which obliged me to use a knife as a lever to open his teeth sufficiently to enable him to drink. His body was as stiff as an iron bar, a spasmodic shudder threatened to suffocate him; the patient, who was perfectly conscious, showed me by a look that he suffered everywhere. His skin was hot, his face injected; a low plaintive cry succeeded each muscular shock. In this strongly characterized case of tetanus, anticipating as morally certain that Smith could not live in this state if I had recourse to the same remedies which failed in four similar cases, I prescribed accordingly 6 grains (30 centigram.) of tartar emetic (tartre stibié) in 180 grammes of liquid, with 40 drops of laudanum. The patient was to take a large spoonful every second hour. I came back seven hours after; no vomiting; two stools; skin

moist; same general state; I ordered a continuance of the potion. Twelve hours elapsed after my second visit, when I found my patient bathed in perspiration; I never saw so much in my life; no vomiting; two more stools. Smith spoke with a good deal of facility; the muscles of the jaw were hardly contracted at all; the patient sat up; his thighs were bent; the spasmodic shock had ceased four hours ago.

I stopped the potion, ordered soup and a little wine. The fourth day of his illness George Smith went on board of his ship entirely cured.

I shall certainly have recourse to the emetic on the first occasion. Tetanus is very common in this country; the heat is very excessive during the summer; the negroes, and a great number of white laborers are imprudent enough to walk barefoot, and sometimes get a nail in the sole of the foot, sometimes splinters of wood. The number of deaths each year from tetanus is truly frightful. We should, it appears to me, think more of the use of emetics; a more even temperature, as we have seen, is established immediately, and they produce relaxation of the muscular rigidity. I will add, that when the foot is wounded, it is imprudent to wet the wound. Those who abstain from it are rarely attacked by lockjaw.

Mobile, Aug. 20th, 1853.

ART. X.—A CASE OF TETANUS, WITH PATHOLOGICAL REMARKS.

BY B. DOWLER, M. D.

[*Supplementary to the preceding article.*]

Unsuccessful cases are seldom reported. I select one, because the treatment adopted was similar to that of Dr. Poitevin in the preceding article, and though the result was unfortunate, it is for this reason now submitted to the reader.

1848, August 2d, 8 A. M. Hellemenn, living on the river bank, in McDonogh, opposite New Orleans, resident seventeen years, aged nineteen; stout, muscular and of good constitution, was wounded with a splinter in the sole of the foot, at the anterior articulation of the second metatarsal bone. A portion of the splinter was extracted by himself without delay. He continued his avocations as usual for a

week, when finding that his foot was becoming very painful, he re-opened the wound, and, as he said, extracted another splinter an inch long. The following day, having been attacked with rigidity in his jaws, neck and back, he sent for me. The foot was free from swelling. The wound, which was small and open, was enlarged; no splinters were found; a slight trace of pus was noticed; the wound, now excessively painful, bled freely, and the bleeding was encouraged by the immersion of the foot in warm water.

His jaws were rigidly closed upon two wooden wedges half as thick as the finger, which his friends had with difficulty inserted between his teeth. The muscles of his neck, back and loins rigidly contracted, gave the body that backward bending called opisthotonos; shoulders retracted; stricture and pain in the chest, which increased upon attempting to make a deep inspiration; respiration very imperfect—twenty-four in a minute; occasional, not constant; rigidity in the limb injured—the left; believes he cannot swallow; dense mucosity in the mouth, which he discharges with difficulty; pulse regular—80; skin moist; axilla 100¾°; eyes natural; lids incline to close; rather costive; urinates; intelligence unimpaired; temper irritable.

During forty minutes he reluctantly and imperfectly inhaled from a sponge two ounces of ether; imperfectly, because the stricture, rigidity and immobility of his chest and diaphragm restricted his breathing to such a degree that probably but little ether entered the lungs; the rigidity relaxed for a time; the wedges full loose; the mouth could be opened an inch or more. Advantage was taken of this to give a dose of castor oil, which, after many painful efforts, he swallowed; cups to the nape; frictions of sweet oil and mustard; flaxseed poultices to the foot; sulphate of morphia to be given after the operation of the oil.

Evening: 5 to 6½ o'clock. Symptoms worse; opisthotonos extremely severe, curving the body violently backwards, being in paroxysms every two to four minutes; the muscles of the back and loins strongly contracted, having a wood-like hardness; jaws rigidly closed; senses regular; two defecations; the temperature of the skin natural; pulse small, variable—100; gave six or seven grains of tartar emetic in an ounce of water; but little water was used, and all was given at once, because of his difficulty and dread of swallowing; virtually he

had hydrophobic symptoms. Afterwards toast water was offered, but after many attempts, only one or two ounces were swallowed. Hearing from me that the medicine might vomit him, he became greatly alarmed, asserting that it was impossible for him to vomit without suffocating to death. His fears were nearly realized, for vomiting coming on, he appeared to be temporarily asphyxiated; face livid; pulse thready, irregular and quick; the lips and teeth closed so much that it was with difficulty that the mucosities and fluids ejected from the throat and stomach passed out at all. The paroxysms of strangulation having passed, in about one hour, great but temporary improvement took place; the paroxysms of muscular contraction abated in frequency and force; the pulse became regular; the external veins became fuller; copious sweats and some sleep followed. During the night he became much worse; refused to take morphia or other medicine; great dread of the pain in drinking.

August 3d, 6 A. M. Died, after a struggle of forty-eight hours. Two hours and a half after death I found the body laid out as usual, and free from the rigor mortis, being supple and preternaturally hot, though having forgot to carry my thermometer with me, I could not test this fact instrumentally. The absence of the death-stiffness, a significant fact, will not be now dwelt on; nor will any pathological investigation of this formidable disease be attempted in this place.

The spinal physiologists and pathologists, who, though unable to find morbid alterations in the spinal cord, which they regard as the exclusive seat of tetanus, seek to establish their theory by the abundance and steadfastness of their faith, ignoring the muscular system altogether. Whether tetanus be regarded as a purely dynamic or a purely structural disease, the obvious phenomena, particularly before death, display themselves prominently in the muscles. To ascribe the local knottings or muscular contractions called cramps to the cord, exclusively, where no changes can be seen, felt, or in any way be detected, is to reject facts for the sake of theory, and is no better a system of philosophy than that, now too prevalent, which teaches that there are four kinds of conducting nerves, each having a separate office; two for the brain, the sensational and volitional; two for the cord, the excitor and the motor—all being mere conductors to the centre; all of which are assumed; none of which can be shown as anatomical

facts, either to the naked eye or by the miscroscope! Yet they are spoken of as anatomical facts, arcs, tracts and conductors—drawn on the blackboard; engraved in maps and charts! When even normal anatomy is not based on physical materiality, pathological anatomy is not amenable to materializing tests.

Rhetoricians maintain that obscurity, "thick clouds and dark," "the moon's doubtful and malignant light," favor sublimity. This Æsthetic canon is not adapted to physiology, pathology, normal or morbid anatomy. It is not, however, wonderful that gentlemen who get all these from the imagination, should take from the same fruitful realm hydrophobia, epilepsy, spasms, cramps, tetanus and the like, as the types of the physiology, the diseases and pathological anatomy of the spinal marrow and what they call the true spinal system.

The pathological tenebrosity which envelops hydrophobia, epilepsy, spasms, wry-neck, cramps, tetanus and the like, makes these diseases the worst possible types for founding a new system; and yet they are taken as the standards of the spinal system, wherein no characteristic changes in the nerves have been ascertained; while the muscular system, in which many changes are known to take place, at least during the progress of these convulsive maladies, is ignored! This method of forming a system, whether it be based on function or structure, is the reverse of that adopted by naturalists, who seek not the obscure but the well known typical forms which characterize a group, family, class or genus, and which most truly represent the whole, and yet, at the same time, each of the species. Thus the hard-shelled, four-winged June-bug, beetle, or scarabæus, is the fundamental type of more than twenty-thousand known species in the class of insects called the Coleoptera; the common house-fly, or the mosquito, is the type of the Diptera, or two-winged class. Pathology has its types; fever has its intermittent, remittent and continued types; so has inflammation, the skin-diseases, &c.; pathological anatomy has its types, as cohesion, color, size, &c.

Spinal pathologists have, except in rare cases, signally failed in showing anatomical changes of the cord in tetanus. Hence, it is the worst possible type for founding a durable system. For whether tetanus be regarded as a purely dynamic or a purely structural disease, the muscular system seems to perform the principal, if not the perma-

nent rôle. That the spinal cord is the sole seat of convulsive diseases may be true, but the evidence to prove this weighty postulate has not been adduced.

Mr. Miller, in his excellent work on the Principles of Surgery, says of traumatic tetanus, "it is in the nerves of the part that inflammatory change is to be looked for, not in the spinal cord."

Baron Dupuytren, who, after treating a patient for tetanus unsuccessfully, begins his account of the post mortem examination thus: "Great rigidity of all those parts of the body which had not been affected by tetanus; but the muscles of the neck, shoulders, and in general all those regions in which the tetanic rigidity had existed, were completely relaxed."—(Clin. Lect.) The muscles have been sometimes found ruptured.

Sir A. Cooper says: "Nerves are very rarely inflamed. Wounds of the nerves, though dreadfully painful at the moment, are followed by little irritation. * * * The spasmodic and tetanic symptoms which follow punctured wounds, are the effects of injury to tendinous (and muscular) rather than nervous parts." He frequently cut out portions of large and important nerves without any marked constitutional irritation, or other unpleasant symptoms. "These instances," he remarks, "to which many more might be added, as well as the usual seat of the wound which produces tetanus, leads me to believe that it is rather the result of injury to tendinous than to nervous structures."—(Lect. 1, 245, iii. 125.)

Physical or anatomical alteration of the substance of the spinal cord would be more likely to cause palsy than tetanus; a loss rather than an increase of muscular tension. The rapidity with which tetanus sometimes runs its course, can be better explained as a muscular disease than as an alteration in the tissue of the cord. A preternatural development of the muscular force would, as analogy shows, cause death from exhaustion. A race horse from over-exertion; a wild bird from attempts to get through a glass window, sometimes die instantly. Besides this exhaustion of the muscular force, incidental to the most violent form of tetanus, the rigidity of the muscles would arrest or obstruct the circulation of the blood by compressing the vessels, and might arrest the action of the heart and diaphragm. The London Cyclopædia, of 1850, says that the late Professor Robison, of Edin-

burgh, had a negro patient who had scratched his thumb with a china plate, and died in a quarter of an hour afterward of tetanus.

Those physiologists who assign an host of dissimilar diseases (as epilepsy and tetanus) to the spinal cord as their primary and exclusive seat, regard strychnine as a fundamental therapeutic test of their postulates. It must be confessed that the action of this alkaloid of the nux vomica on the muscular system, approximates tetanus. But this in no degree favors the truth of the aforesaid theory, but goes indeed against it; for there is much more reason to think that as the muscles are richer in blood vessels than the cord, the strychnine would reach the former first, and most extensively, by means of the blood, and communicate to them the first shock tetanic. The blood, the muscle and the cord all take parts in the pathological drama.

ART. XI.—IMMOBILITY OF THE LOWER JAW.

BY J. J. MCELRATH, M. D.,

OF CAMDEN COUNTY, ARKANSAS.

Functional derangements of the "inferior maxillary bone" are occasionally observed as the result of structural changes of the soft parts within the mouth, in consequence of violent mercurial action. It may indeed be affirmed without exaggeration, that "immobility* of the lower jaw," whether partial or complete, is more frequently attributable to this than to any other cause, or, perhaps, than to all other causes combined. The present conservative mode, however, of using that powerful but valuable remedy, renders the occurrence of such accidents much less frequent now than was the case a dozen or twenty years ago. The only three cases that have come under my observation, in which surgical interference appeared to be imperiously demanded, were of several years standing. In two of these the affection had existed for more than twenty years; the immobility, however,

* This word is not used in its most restricted sense generally in surgical nomenclature, but is, by common consent, made applicable, without qualification, to partial as well as complete loss of motion in different parts of the body.

was not entirely complete; yet the remaining action was so limited, that but little assistance was derived from this organ (lower jaw) in the masticating process. No treatment, so far as I know, has ever been instituted for the relief of either of them.

The subject of the present communication, Miss Margaret Watkins, aged twenty years, had mercury (calomel) administered to her for the cure of an intermittent fever, in the fall of 1846—then in her thirteenth year. The history of her case at that time, as detailed to me by her father, Col. Jacob Watkins, was simply this. To use his own language: She was badly salivated, but not worse than he had seen other cases. Under the use of stimulating, astringent, and other detergent washes, gargles, &c., the disease (salivation) gradually subsided, with the exception of the sores (ulcers) on the gums and contiguous lining of the cheek on the left side. These parts presented several large superficial ulcers (deep no doubt for this locality,) which did not seem to be benefitted by any of the applications that were used, until at the expiration of about six weeks, when a broad, thin, partially detached lamella of bone made its appearance, and was removed—(from the inferior maxillary); after which, the healing process advanced rapidly, so rapidly, indeed, that on the morning of the day following that on which this "osseous lamella" came away, the patient could but barely separate the jaws sufficiently to take nourishment; the effort causing considerable pain, too, she was permitted to use her pleasure. No further attention being given to the treatment at the time, supposing, as did the attending physician, that this latter symptom (pain) was the cause of the apparent inability to use the jaw. At the expiration of a very few days, however, the stiffness was found to have increased to such a degree as rendered the bone immovable. Some exertions were now made to overcome the difficulty, but no amount of force the medical attendant felt himself justified in using, was sufficient to produce the slightest effect. Similar attempts were repeated at intervals, and continued for several weeks with no better success, when they were finally abandoned as useless. In this unfortunate condition has she remained ever since—up to the month of October last—subsisting upon fluids, with only such solid food as she was enabled to introduce through a small space or opening between the superior and inferior bicuspids of the sound side (right side.)

I have been thus particular in reciting the previous history of this case, as indicating clearly by what simple means such accidents may

be avoided. Their occurrence, in fact, can result from nothing short of culpable neglect, or what is worse, culpable ignorance. As will be seen in the subsequent treatment, pledgets of lint carefully introduced between the contiguous diseased surfaces, and constantly retained there, or frequently replaced for a few days, together with moderate exercise of the parts during the day, and a forced open position of the jaws (if necessary) whilst sleeping, will be altogether sufficient to prevent adhesions, or excessive contractions, and, consequently, any material impairment of function in this organ (lower jaw) from such cause. This preventive treatment must be adopted immediately upon the slightest difficulty being experienced in extending the jaw, or in giving to it its proper healthy action; more especially should such a difficulty occur during the existence of a considerable degree of ulceration, or soon after some irritating substance has been removed or come away, as "spicula or lamella of bone, decayed teeth, sloughs," &c. The process of adhesion in these highly vascular structures is, when not interfered with by some disturbing cause, a very rapid one, requiring at most but a few days, and sometimes even but a few hours for the contiguous diseased surfaces to become so firmly agglutinated together as to resist the action of the depressor muscles of jaw. Should such accident, however, take place, and the bone become, either partially or completely, immovable before the attention has been directed to it—(I have known this to occur in a single night more than once in my own practice)—then, provided not more than twenty to thirty hours have transpired, a moderate degree of force properly applied upon the chin, the head being fixed, will be sufficient to break up and overcome the incipient adhesions and contractions, and restore to the jaw its normal action; when the above treatment persevered in will prevent their recurrence.

Although functional injuries of the lower jaw from mercurial action is a rare occurrence at the present day, yet it is, nevertheless, sufficiently frequent to justify our best directed efforts to prevent it, more especially when we reflect upon the fact, that a large majority at least of those who have been thus unfortunate, are suffered to remain so during life; as well from an unwillingness on the part of themselves to submit to the action of cutting instruments, or other painful operations for relief, as from a disinclination with most physicians (out of cities) to undertake any treatment in such cases beyond temporizing means.

As respects the appearance and condition of Miss Watkins at the time of the operation, it is somewhat remarkable, that notwithstanding she had been deprived the power of masticating, and forced to subsist, in a great measure, upon fluids, there was no apparent lack of physical development, nor was there any very marked deformity. The mouth was slightly retracted towards the affected side (left side,) which cheek was somewhat more prominent than the opposite one. When the lips were separated so as to display the teeth, the superior incisors presented an unnatural appearance—they having grown to an unusual length, projecting over, and reaching down in front of their inferior antagonists. The chin was also slightly retracted; indeed, the inferior maxillary in its whole contour, was somewhat imperfectly developed; yet when the features were composed, these irregularities were scarcely observed, unless the attention was specially directed to them. Excepting a barely perceptible lateral motion, the jaw was perfectly immovable—such force as I was able to apply upon the chin, producing no visible separation of the teeth; nor was such attempt, although frequently repeated, attended with the least pain. Immediately within the angle of the mouth, on the left side, was a firm, unyielding band of inodular tissue (cicatrix) embracing the gums above and below, and fastening the jaws firmly together. In attempting to explore the parts beyond this adventitious formation by means of a common probe, it was found impossible to pass this instrument more than a few lines along the surface of the teeth, so closely were they invested by the contracting tissues. By a careful examination and comparison of the opposite cheek, this hard, unyielding substance could be distinctly felt through the superimposed parts, extending back to near the anterior border of coronoid process. The right cheek was entirely free from morbid adhesions or contractions. The molar teeth on this side, however, were in a manner dove-tailed together, the depressions on the grinding surface of the superior set being occupied or filled up by corresponding elevations on those of the inferior set, and vice versa So complete was this adaptation of their antagonizing surfaces that nothing could be passed between them, not even the point of the thinest spatula, or smallest probe. The lateral motion of the jaw being only sufficient to allow the sliding of these teeth upon each other through a mere fraction of their width.

The operation, which was performed on the 17th day of October

1853, and its results may be summed up in a few words. The patient being seated, the head resting against and supported upon the back of a chair prepared for the occasion, and the lips, near their left commissure, being separated and drawn outwards by an assistant; a straight, narrow pointed bistoury was forced, flatwise and with the edge downwards, along the external surface of the teeth, opposite their junction, to the extent of something more than an inch beyond the anterior stricture, and then with a thrusting or sawing motion carried downwards, parallel with and sufficiently near the bone to avoid wounding the facial artery, at the same time not so near as to divest this organ of its proper fibrous covering, dividing the tissues completely down to the base of the alveolar process. The edge of the knife being now reversed, the parts above were divided in the manner and to about the same extent. Blood was given out freely from a number of wounded arterial branches, and some little time was required to arrest the flow, which being accomplished, the finger was passed into the space thus gained, when the same unyielding substance was found still beyond, closely investing the teeth, occupying and filling up even the vertical spaces that would otherwise have existed between them in consequence of their imperfect development. With a little force the liberated parts were held sufficiently open to allow the dissection to be completed; which was here effected, both below and above, (regardless of Steno's Duct) by repeated strokes of a scalpel, and was continued back to the posterior surface of the last molar teeth. This latter part of the operation occupied much time, and was exceedingly painful; so great was the pain that it was with much difficulty I could induce the patient to suffer me to proceed. At almost every stroke of the knife it was necessary to constrict the bleeding vessels and sponge out the blood, in order to remove the temporary obscurity of the parts which its presence produced. These abnormal tissues had in many places acquired an almost cartilaginous hardness, rendering their division both difficult and tedious, retarding too, in some degree, no doubt, by their inelastic character, the closure of the divided vessels.

The separation having been thus completed, the fastenings and connecting bridles all divided, and the cheek freed from its attachments to the gums, an effort was made to depress the bone, by means of

force directed upon the chin, but to my great disappointment, as well as that of the patient and her friends, the jaw was as completely immovable as before the operation. These attempts being persevered in sufficiently to prove their total inadequacy, an opening was next made by forcing out the first inferior bicuspid tooth, and through this a strong piece of tape was passed and brought out between the antagonist bicuspids on the opposite side; upon the ends of this, carried below the chin, as much force was exerted as I could impart with the whole strength of my arms; which proved as ineffectual, however, as did the previous attempts upon the chin. Under these discouraging circumstances, having no mechanical apparatus, by which a greater degree of force might be applied, except a small, badly constructed screw dilator, and this unavailable for the want of space or surface to act upon, I could think of no means to effect a liberation of the jaw short of bisecting the masseter muscle. And even this expedient, after a careful examination of the muscle, with the finger introduced behind it, seemed to be unecessary, as the remaining rigidity was not sufficient to resist the force that had been used. Such was the anxiety of the patient, however, to be relieved, that she readily consented to any further operation that was thought advisable, but insisted upon the selection of a subsequent day for the proceedings; to this I saw no objection.

To prevent a renewal of the adhesions and contractions, pledgets of lint were introduced, so as to separate completely the raw surfaces. This dressing was renewed once every day, or oftener, as the convenience or comfort of the patient required. Smart reaction, with considerable swelling of the cheek, took place, but gradually subsided without treatment, in a few days. When the parts were examined on the 22d, (five days after the operation) the adventitious formation which had been separated from its connections with the gums above and below, and left as an irregularly circumscribed solid cicatrix, embracing the whole internal face of the cheek, appeared to have lost its vitality, and was becoming detached from the healthy structure beneath and sloughing away. No part of the molars were visible at this time, except that portion of their external surface which had been denuded by the knife; fleshy connections were observed extending from the superior to the inferior gums, and embracing the

last named teeth on their posterior and anterior aspects. Pressure upon the chin was again applied without benefit, the mouth being too painful to admit of further dissections. Tepid water ablutions were directed to be frequently used, and the lint dressings (spread with simple ointment) constantly renewed. During the succeeding two weeks the mouth had the appearance and fœtor of a severe mercurial ptyalism, affecting as well the sound as the diseased side, and extending even to the submaxillary and sublingual glands. In the meantime the whole of the abnormal tissues had come away, leaving the substance of the cheek on this side (left side) as soft and elastic as that of the sound side. By a very careful examination on the 29th, I was unable to detect any cause sufficient to prevent the jaws from being separated, unless possibly I was mistaken in my diagnosis, as to the condition of the articulation, and was preparing to make another effort by means of force directed upon the chin, when to my agreeable surprise, with but little pressure, the teeth separated, not freely nor widely, but sufficiently to admit of the easy protrusion of the tongue. Attempts to increase the separation, however, could not be borne, so great was the pain thereby induced in the neighborhood of the joints, and was therefore desisted from. A weak solution of zinci sulphas was now directed to be used once or twice every day until the pseudo-ptyalism should subside; the patient made to exercise the jaw in masticating as much as possible; the lint dressings to be retained, and whilst sleeping the jaws to be kept asunder with a little force, by a simple contrivance of wood, interposed between the ranges of teeth on either side, and secured by means of a bandage carried over the vertex. This treatment was persisted in without intermission until the 22d of November; at which time the bone had acquired about two-thirds of its natural action in the vertical direction, with a somewhat greater improvement in the action of the pterygoid muscles. That portion of the new surfaces which had been formed by the action of the knife upon the inferior gums and contiguous lining of the cheek, was still uncicatrized, having a granular appearance, and showing a disposition in many places to ulcerate. Fearing that the dressings might perhaps be the cause of this tardy recuperation, these were now discontinued, and a strong solution of zinci sulphas frequently applied; so great however was the tendency to a reproduction of the adhesions

in the absence of an interposing substance, that on the succeeding morning, the patient having neglected to apply the dilating apparatus as usual when retiring to bed the previous evening, the jaws were completely locked, as the lady herself expressed it, and could not be separated without assistance. Similar accidents occurred on two subsequent occasions, and from the same kind of neglect; yet, by timely interference and a reapplication and continuance of the dressings, no material obstruction to the progress of cure was caused by them. Upon a further and more careful examination some days afterwards, portions of the alveoler process were detected in a carious condition, and in order to facilitate the necrosis and separation of this source of irritation, it was found necessary to remove the remaining inferior bicuspid and molar teeth on this side; this however was no great misfortune, as their imperfect condition rendered their preservation, even for a few years, impossible. After the extraction of the teeth, the necrosed fragments of bone were soon spontaneously thrown off, and a few applications of the solid nitras argenti speedily effected cicatrization.

The tendency to a renewal of the contractions was strongly manifested for some weeks after the entire disappearance of all visible disease, rendering the dilating apparatus constantly necessary, and occasionally even the knife was required to destroy some newly formed sub-mucous bridle or bridles. It was not until the last of December, that all treatment could be safely dispensed with.

No further liberation of the jaw was gained after the 22d November, and the whole and only effect of the treatment after that time, was in securing permanently the advantages then obtained.

Would the division of the masseter muscle have been, or would it even now be sufficient to effect the complete liberation of the bone? Or, is it not more probable that the long-continued inaction of these muscles, at a time of life, too, when the physical system is being rapidly developed, has rendered them, as well as the temporals, permanently deficient (atrophied,) so that to effect anything by this means, it would be necessary to bisect the right as well as the left muscle?

In reflecting upon the various steps pursued in the treatment of this case, although entirely satisfactory, in the result at least, to the patient and her friends, it is not difficult to point out many errors, the avoid-

ance of which would, doubtless, have very much abbreviated the treatment, if not have secured a more perfect liberation of the jaw. In view of all the difficulties encountered in this, were a similar case to present itself for treatment, I should deem it best—whether the separation of the soft parts were accomplished in the same manner or by substituting for the latter (after) part of the dissections, horizontal sub-cutaneous division of the masseter muscle—to effect at once, and at any reasonable hazard, the separation of the jaws to as near the full extent as possible, which might be done after the removal of the teeth, if necessary, to procure surface to act upon, by means of some properly constructed dilating instrumen (perhaps a modification of the screw and lever dilator used by Professor Mott for a like purpose could be made efficient,) and the inhalation of chloroform when not contra-indicated, to prevent the sufferings.

ART. XII—CASE OF VESICO-VAGINAL FISTULA—CURE.

BY N. BOZEMAN, M. D.,

OF MONTGOMERY, ALA.

Emily, aged about 30, property of H. W. B. Price, of Daleville, was admitted into my Infirmary on the 26th of December last. She is large and stout; the mother of eight children; and had always enjoyed good health until the birth of her last one. This was in August, 1852; at which time she became the subject of the above disease. Labor lasted about sixty hours; craniotomy was then resorted to by the attending physician, and delivery speedily effected. A few days afterwards it was discovered that urine passed through the vagina—a circumstance first indicating the existence of vesico-vaginal fistula. In addition to this injury she had an attack of rheumatism, and from both of which she did not recover entirely for several months. Since then, however, her general health has been very good, menstruation regular, &c.

Upon examination of the parts, I found them as follows: uterus retroverted and slightly prolapsed; vagina quite capacious, but very much altered in appearance by the abnormal position of the womb; its anterior wall only two and a half inches, while its posterior seemed to be of the usual length. Running vertically through the anterior and a portion of the posterior lip of the os uteri, was to be seen a cleft an inch or more in length. At the anterior extremity of this cleft was the fistulous opening, partially filled by a herniated condition of the mucous coat of the bladder. It was triangular in shape; its base resting against the neck of the womb, and its apex presenting forward and sufficiently large to admit the index finger. The consequences of such a disease are too well known to require a notice in this short report.

Having satisfied myself as to the nature of the case, my attention was next directed to the mode I should pursue in an operation. Fully appreciating the difficulties which I had so often seen my friend, Dr. Sims,* encounter in similar cases, I of course had many misgivings as to the result of any proceeding. I determined, however, to make an effort.

Accordingly on the 11th of March, assisted by several of my medical friends, I proceeded to operate in the following manner: The patient was placed upon a suitable table on her knees and elbows. The parts were now brought into view by the lever speculum and a reflected sun-light. The edges of the fistula, formed partly by the anterior lip of the os uteri, were then freshened by means of a delicate tenaculum and small scalpel. This being done, the next, and what I considered the most difficult step was to be taken, namely: applying the sutures and clamps in such a way as to effect perfect apposition, and thereby obtain union by the first intention. Here I had the neck of the womb, an unyielding tissue, to deal with; and

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*J. Marion Sims, M. D., late of Montgomery, Ala., now of New York, of whom Dr. Bozeman makes such honorable mention in the above interesting case, is at home and abroad now justly regarded as the highest authority on this difficult branch of surgical practice. Dr. Sims himself says that "all we know upon this subject worth knowing, is due to America and France"—the two names which stand out in the boldest relief, being Professor Mettauer, of Virginia, and the indefatigable Jobert, of Paris. Dr. Sims attributes the first successful operation for Vesico-vaginal Fistula in this country to Professor Haywood, of Boston, who has operated with entire success nine times. Professor Pancoast, of Philadelphia, has operated several times with like results.—Editor.

to remedy this, it was necessary that the compensating parts should be so adjusted that the mechanical contrivance employed would not defeat the object sought to be obtained, by creating an undue amount of inflammation. From the nature of the parts, therefore, I determined to place the clamps longitudinally binding their upper extremities so as to be applied one on each side of the anterior lip of the os uteri, thus enabling me to secure the upper suture in the most advantageous way. The clamps were one inch in length, and contained four openings for the sutures. These were of silver wire, and were lodged one after another in their respective places, by being attached to the end of a silk thread carried through at a proper distance from the edges of the fistula, by a short, straight and spear-pointed needle. A clamp was now secured to the distal extremities of the wire and made to occupy its place. On the proximal ends the other clamp was passed down to its place. Traction then being made upon these ends of the wire, perfect apposition of the denuded edges was effected. This was now maintained by compressing with a pair of strong forceps a small, perforated shot previously slid down upon each wire to the clamp. After this, the wire was cut off close to the shot, and the patient put to bed. The self-retaining catheter was next introduced into the bladder and there allowed to remain, excepting when its removal became necessary to clear the mucous and earthy deposit from it. This was required twice a day. During the whole treatment the patient was kept upon her back, and the bowels prevented from acting by the free use of opiates. On the fifteenth day I removed the suture apparatus, and was most happy to find union of the parts perfect. The patient now has entire control over her urine, and says she feels as well in this respect as she ever did.

Whether the fistula in this case was caused by the use of instruments in effecting delivery, or by the sloughing process, I cannot positively say, but am inclined to the former belief. However produced, I think it affords several points of interest; showing in the first place to be a part of the same wound in the cervix uteri as indicated by the cleft or line of cicatrization; and in the second place, beautifully illustrates the extent of Nature's efforts to repair such injuries, as well as some of the valuable resources employed by art to overcome such obstacles as she herself fails to do.

I take great pleasure in saying, that the success I have had in this case, I attribute entirely to the advantages derived from those principles of treatment laid down by Dr. Sims. Every surgeon who has or may make an application of them in the treatment of such cases as the above, must feel a sense of pride in awarding to him the praise he so justly deserves.

April 7th, 1854.

Part Second.

EXCERPTA.

The *Excerpta department* of a Medical Journal is one of peculiar difficulty in reference to the expectations and wishes of different classes of subscribers; some of whom, taking no other Journal but this, would prefer to have this department so expanded as to contain a summary of the progress of Medicine, copious extracts, and the most interesting memoirs emanating from every point of the horizon; while others (a large class) who receive several Medical Journals, look with little favor, if not aversion, at articles copied from Journal to Journal which they are compelled to buy three or four times. A compromise between these extremes seems to be the only alternative, although the greater the amount of *Excerpta* the smaller is the editorial labor in conducting a periodical, seeing that a perusal of the Journals and a few marginal strokes of the pen are all that is necessary to guide the printer and make up a certain number of pages, and thus afford an Editor the pleasures of idleness. But even the *Excerpta* may afford the lover of work, work, if not distinction. "There is but one method," says Sidney Smith, "and that is labor; and a man that is not willing to pay that price for distinction had better at once dedicate himself to the pursuits of the fox. There are many modes of being frivolous; there is but one mode of being intellectually great."

Art. I.—*Dr.* John Davey's *views of the Nervous System.—Uses of the Ganglionic System.*

Dr. Davey read a paper on this subject before the Medical Society of London; the principal object of which was to prove the independency of the organic nervous system, and, what is more, the dependency of the integrity of the cerebro-spinal system, in common with all the organism, on it. To prove his position, Dr. Davey brought forward a variety of facts, more or less startling, and these selected with much apparent care, seemed to tell much in favor of the physiological views insisted on. After some preliminary remarks, intended to show the unsatisfactory and contradictory opinions expressed by our most popular writers on medicine (physiology,) viz: Wagner, Todd, Bowman, Carpenter and others, concerning the ganglionic system, he affirmed on the authority of many good names, that the ganglionic system of the sympathetic nerve are those parts first formed in the fœtus, and that this same fact obtains equally, it was premised, through the whole animal kingdom. The early organism of birds was referred to in confirmation of that opinion, which assigns to the solar ganglion and its dependencies an existence anterior to any other part of the animal fabric. Especial reference was made to the two monstrosities recorded by Mr. Lawrence and Dr. Marshall Hall. The first of these it is known was born without a brain, but with the spinal cord complete; but the second, still more wonderful, was born without either a brain or spinal cord.

Dr. Davey argued, that if in the latter instance the functions of secretion, absorption, &c., were duly and efficiently performed without any aid from the cerebro-spinal system, then was this latter in no instance either requisite or necessary in any way to the integrity of such functions in the animal economy. The ganglionic nervous system, said he, is perfect at birth, and its functions are also perfect. This is completely organized, whilst the brain is nothing more than a mere pulpy mass, without any kind of function or use to the individual in possession of the same; the one is in active and unceasing operation, the other is but a blank, doing nothing, useless; the ganglionic nervous system executes its functions instinctively, whereas the brain, if not the spinal cord, requires time and experience, and direction, ere it performs its functions, either for good or evil. Dr. Davey compared the monstrosity of Dr. Hall, organically considered, to the lower classes of animal life, the medusariæ; these, he said, performed the functions of an instinctive and preservative character; executed those functions only which are strictly vital, such as secretion, absorption, nutrition, and so on, and therefore was it to be inferred, and most fairly and logically, that the same animals were possessed of a sympathetic nervous system and none other; the nervous organism of both the amyencephalous monster and the zoophyte was precisely similar, and their animal functions were on a par. As a

further illustration of the opinions advocated, he cited some rare examples of vivisections performed by himself with the view of testing anatomically the relative importance of the ganglionic and cerebro-spinal nervous centres. It was shown that in frogs and fish, both the brain and spinal cord may be destroyed, and that, nevertheless, all the strictly vital functions will continue to be carried on as before.

These animals, it was observed, offered peculiar facilities for the performance of such experiments, from the peculiarity of their respiratory apparatus; but, added the author, you have only to establish an artificial respiratory process or function in the higher order of animals, *i. e.* those who breathe normally with the aid of a complex set of nerves and muscles, for that which is purely natural, and then may the spinal cord and brain be destroyed with an almost equal impunity, so far as the said vital acts are concerned in them. The phenomena of sleep and disease were referred to as offering strong and corroborative evidence of the physiological opinions insisted on; and, in so far as the latter is concerned, Dr. Davey mentioned that the wards of the lunatic asylum would afford many instances of individuals who were reduced to a mere vegetative or organic existence by disorder affecting the brain and spinal cord; such patients, he said, lived oftentimes many years with their cerebro-spinal organism so disorganized as to be perfectly useless to them; unconscious; without feeling, emotion or desire; void of thought; without hope, joy or passion; lost to all normal sensation, or perhaps without feeling of any kind, and incapable of only the most imperfect motive power; enfeebled; paralytic; they nevertheless digest, secrete, absorb, in a word, carry on, year by year, the strictly vital functions exactly as the malorganized fœtus does; exactly as the frog or fish deprived of the brain and spinal cord did; exactly as the polypus is in the habit of doing. He then referred the Society to the experiments of Sir B. Brodie, performed many years since, for the purpose of proving that those nerves having a cerebro-spinal origin had no kind of influence on the reparation of injuries, and so on. Frogs, guinea-pigs and dogs were the subjects of Sir B. Brodie's experiments; and it was found that the destruction even of the lumbar spinal cord, much less that of the crural and sciatic nerves, neither retarded nor impaired in any way the reparative process in the lower extremities; thus wounds and fractures made in the limbs so deprived of cerebro-spinal nervous influence, healed and united as readily and completely as under all ordinary circumstances. Dr. Davey explains the facts recorded by Sir B. Brodie, by saying that the ganglionic nervous power was necessarily left intact; and this it was which caused the wounds to heal and the fractures to unite; and insisted on it that although great and serious injuries to the brain and cord were borne with impunity, and for the reasons above stated, yet were such altogether impossible, in so far as the solar ganglion and its dependencies were concerned. It was very truly said, that a comparatively feeble blow on the epigastrium over the solar ganglion would destroy life; and

that it was a very common thing among boys to suffer greatly from slight accidental blows taking effect on the pit of the stomach, and the great danger of physical violence, even in a slight degree, to the epigastrium, was well known to the prize fighting gentry, who forbade the combatants to strike below the sternum; and if, as the author asserts, the solar ganglion be the seat of life, the *locale* of the *impetum faciens* of old writers, the irritability, the motions without force, of Haller, then can we easily account for the facts just cited. Instinct and animal heat were treated of by Dr. Davey as specific functions of the sympathetic nervous system; but our space will not allow us to do more than thus allude to this part of his paper.—*London Lancet.*

Art. II.—*Additional Experiments on the Excitability of Paralyzed and Healthy Limbs by the Galvanic Current:* By R. B. Todd, M.D. F.R.S.

[The Lancet, August 6, 1853.]

In the summer of 1847, Dr. Todd submitted to the Medical and Chirurgical Society of London the results of experiments tried with the view of testing the accuracy of Dr. M. Hall's dogma, that limbs paralyzed by lesion of the brain became more excitable than the healthy ones by the galvanic current, in consequence of an increased irritability of the paralyzed muscles. The present communication comprises the results of experiments to determine the difference on the influence of the current according to its direction, and also to ascertain whether any real difference of physiological effect exists when the galvanic trough, or the magnetico-electric or electro-dynamic machine is used. Thirteen healthy individuals were subjected to experiment, and with the following results:

1st. That the obvious physiological effect was produced only on completing or on interrupting the galvanic circuit.

2d. That more vigorous contractions were excited on the completion than on the interruption of the circuit.

3d. That the completion or the interruption of the direct current produced more vigorous contractions than the completion or interruption of the inverse current.

These experiments were made with a Cruikshank's battery, charged with very dilute sulphuric acid. The magneto-electric rotation instrument and the coil machine (electro-dynamic) were afterwards used, and it was found that the same effects precisely were produced, and the same variation in the intensity of the contractions according as the current was direct or inverse. Fifteen cases of hemiplegic paralysis, caused by lesion of the brain, are afterwards detailed. The results of the galvanic experiments on these cases were as follows:

1st. That of the fifteen cases, in only three was there any approach to a greater excitability of the paralyzed than of the sound limb, and that in two of these it was manifested only under the influence of the inverse current.

2d. That in three of the cases both the coil machine and the battery were used, and with precisely the same results; and that, in one of the cases, the coil machine alone was used, and with a result which corresponded with those obtained in similar cases by the galvanic battery.

3d. That in each of the three cases in which a greater excitability existed in the paralytic limbs, the paralyzing lesion in the brain was more or less of an irritative kind. In one case the irritation was probably connected with an incipient process of cicatrization.

4th. That in many of the experiments all degrees of galvanic power were used, and with no other difference than that of degree; the amount of physiological effect being exactly proportionate to the power of the galvanic stimulus.—*Ranking, Abst.*

Art. III.—*Of certain Pathological States of the Blood, and of their Treatment:* By James Copland, M.D. F.R.S., *President of the Royal Medical and Chirurgical Society.*

The author, after describing various symptoms and signs of irritation of the blood, and noting more particularly the changes observed in the excretions, &c., deduced a series of inferences as the bases upon which he founded his practice and treatment. He arranged the vitiations of the blood under certain heads or categories, according to the causes, extrinsic or pathological, producing them, with reference to the indications of treatment, and these comprehended the following seven orders:

1. Vitiations produced by imperfect assimilation or development of the blood-globules.

2. Vitiations occasioned by the increased action of the organs, which waste or decompose the hæmato-globulin—which increase the fibrine and augment the urea.

3. Contaminations arising from the absorption of purulent, sanious or other morbid matters into the circulation, or from the imbibition of any of these by the veins or cellular tissue.

4. Alterations sometimes supervening on the foregoing, or complicating the latter, such as fibrinous coagula or concretions, or inflammation of arteries, veins or lymphatics, puriform infiltrations, or fomentations.

5. Vitiations occasioned by the imperfect performance, or by the interruption or suppression of a depurating function.

6. Contamination produced by morbid miasms, or by specific seminia, as in malignant, pestilential and septic maladies.

7. The inoculation of poisonous secretions or fluids, as the fluids from erysipelatous inflammation, from asthenic or diffusive inflammation, from bodies recently dead from malignant diseases, or from putrid animal matters.

The treatment appropriate to each of these orders or categories of blood vitiation might be differently estimated by different observers; the author professing, however, to give only the results of his own observation and experience. His practice had been based upon a close observation, and upon rational inferences from such observation. The treatment adopted by the author in these various conditions was then detailed, illustrated here and there by some very instructive cases. The author dwelt at some length on the treatment of that morbid state of the blood which occurred in acute rheumatism, and which is characterized by the redundancy of the fibrinous and ureal constituents of the blood.

What medicines would counteract the disposition to fibrinous attraction in the blood, or such as might exist? Calomel, and calomel and opium, diaphoretics, emetics, purgatives, were doubtless excellent initiatory means to diminish excrementitious plethora; but to promote the depuratory functions he had found the greatest advantage from magnesia and its citrate, the carbonates and citrates of the fixed alkalies, the biborates of soda and potass, the nitrate and chlorate of potass, sublimed and precipitated sulphur, &c., &c., as well as the various preparations of cinchona and turpentine. For the treatment of the sixth category, the advantages derived from large doses of turpentine were detailed; and the author concluded by expressing his hopes that he should be excused for having made so frequent reference to his own writings, where many of the matters comprised in this extensive subject were more fully discussed; but he had his own originality in some topics to vindicate, as several authors who had recently written, had considered that opinions and ideas were fair objects of plunder, if they could be conveyed away without reference to their originators, and in a different array of words.—*Lancet.*

Art. IV.—*Cholera.*

In the *Revue de Thérap. Méd. Chir.* for March, 1854, Dr. Suley propounds a new theory as to the cause of cholera. The earth is, according to him, not an inorganic mass, but a living organized being, having as its parasites vegetables and animals which live at its expense. The water of this planet serves the purposes which the blood

does among animals; the air and electricity constitute its nervous fluid. The being called the earth is very liable to sickness, (un état pathologique—les conditions morbides.) The sickness of the earth affects its vegetable parasites, which become thereby poisonous, and cause the cholera. He refers to the diseases which appeared among the potatoes, (the potato rot) vines and other vegetables, simultaneously with the invasion of cholera, as proofs of his theory. Mother Earth suffers from local rather than from constitutional or general diseases. Hence epidemics appear only where these local diseases of the earth appear. The vegetables which grow upon these unsound spots of the earth constitute the principal diet of the poor, who, therefore, are the chief sufferers from cholera.

The Doctor's treatment, (a most delectable one) for which he claims great success, consists of carbonated waters, (eaux gazeuses) seltzer and various wines—particularly champagne, which is his favorite prescription—one bottle per hour. Champagne has a two-fold advantage; namely, it dissipates the patient's panic and cures his disease! Such is the substance, though not a literal translation of Dr. Suley's views of the cause and cure of cholera.

Art. V.—*Report upon the cases of Tetanus in the Jamjetjee Jejeebhoy Hospital, Bombay, from January*, 1845, *to December*, 1851: By J. Peet, Assistant Surgeon to the Hospital.

[Trans. of the Med. and Phys. Society of Bombay, No. 1, New Series, Bombay, 1853.]

Mr. Peet informs us, that during his seven years, 195 cases of tetanus were admitted into the hospital, exclusive of some 79 or 80 persons who were attacked subsequently to their admission, making the whole number treated during this period about 260. This is the largest number of cases of tetanus treated in any single hospital in the world. Mr. Peet has carefully analyzed this collection of cases, and the following are his conclusions:

1st. That the idiopathic form of tetanus is much more frequent in Bombay than in other parts of the world; and that, contrary to the experience of the disease in other places, it is more severe and fatal than the traumatic species.

2d. That it is often traceable to direct exposures at those seasons during which there are the greatest alterations of temperature.

3d. That of the traumatic form, many cases are ushered in by distinct febrile symptoms; but there is not sufficient reason to conclude that this constitutional disturbance is evidence of any more decided state of inflammatory action in the nervous centres than is present in cases where febrile symptoms are altogether absent. Nor would it appear that this febrile state is any indication of the severity of acute nature of the attack.

4th. That there is little doubt that there exists in tetanus, as in other diseases, a period of incubation, but there are no facts to determine the length of time over which that state may extend.

5th. That in many cases the more marked symptoms are preceded by a peculiar expression of face, to which sufficient prominence is not, perhaps, usually given, as a means of detecting the approach of the disease, and that this changed expression may exist for several hours before any other symptoms of tetanus is present.

6th. That no reliance can be placed upon the frequency of the pulse as evidence of the severity or otherwise of the attack; but the condition of the pulse in regard to strength is of much importance.

7th. That hurried respiration and dysphagia almost invariably indicate a fatal termination.

Mr. Peet remarks, in regard to the various modes of treatment pursued, "that the result has been nearly the same in each; namely, that of leading to the conviction that there is no one mode of treatment better than another in the management of this disease." He would, however, except from this general statement the assiduous use of nutritious food; this should be considered an essential element in whatever plan is adopted, for on this, rather than the medicines employed, will the favorable termination of the case depend. "The inhalation of ether and chloroform," he says, "has been well tested. That it affords great, and for a time, complete relief, there cannot be a question, but it has not appeared to have any influence in preventing, or even delaying, the fatal termination." The majority of cases were treated with preparations of the Indian hemp, but Mr. Peet does not consider it superior to other narcotics. The report closes with a table of all the cases treated during this period.—*Ranking, Abst., Dec.* 1853.

Art. VI.—*Lithotrity Performed on the same Patient forty-eight times.*

At a late meeting of the Medical Society of London, Mr. Coulson exhibited the bladder taken from a man aged eighty-three, on whom lithotrity had been performed forty-eight times during twenty years. Whether one or more fragments of the original calculus may have been left in the bladder, and became nuclei of secondary formations, or whether the bladder was at first completely freed, and the relapses depended on the same constitutional disposition which gave rise originally to the deposit of calculous matter from the urine, Mr. Coulson was unable to say. He did not see the case until the middle of last year. It cannot be denied that relapse occurs more frequently after lithotrity than lithotomy. The Norwich tables of Mr. Crosse show 12 cases of relapse after 704 operations of lithotomy, or 1 in 58. From

records of operations performed at the Hospital of La Charité, in Paris, between the years 1806 and 1831, it appears that 6 cases of relapse presented themselves after 70 cases of lithotomy, or about 1 in 11. M. Civiale states that the proportion indicated by returns which he received from Bavaria, is 5 in 162 operations (1 in 32); from Bohemia, 1 in 46 operations; from Dalmatia, 1 in 43 operations. At the Luneville Hospital, founded by Stanislaus, King of Poland, for the treatment of calculous disorders, the register shows 13 cases of relapse after 1492 operations of lithotomy, or 1 in 116 cases. The most correct registers are probably those kept at the Norwich and Luneville Hospitals, and from them it appears that relapse occurred after lithotomy once in 58 cases at Norwich, and once in 116 cases at Luneville. For lithotrity there are no other data than those furnished by M. Civiale from his own practice. After 548 operations, relapse followed in fifty-five cases, giving a proportion of nearly 1 in every ten cases. After lithotrity, every tenth patient may suffer relapse; after lithotomy the proportion seems reduced to 1 in 60. Mr. Coulson did not think sufficient attention was paid to the condition of the urine after lithotrity, as an indication of the existence of fragments in the bladder. It is not enough that the painful symptoms produced by the stone shall have subsided, and no portion can be detected by the sound or lithotrite. If the urine when first passed is at all turbid, and especially if it contains any exudation-corpuscles, first pointed out by Dr. Golding Bird, there can be little doubt that some fragment remains behind. This distinguished physician laid great stress on these bodies, as indicating the presence of stone, and Mr. Coulson knew two cases lately, in which, in consequence of the existence of these corpuscles, persevering efforts were successfully made in search of stone, although on previous examinations none could be detected.—*Lancet.*

Art. VII.—*Elimination of Lead by Iodide of Potassium.* Reported to the Biological Society by J. Outram, Jun., Lecturer on Chemistry in the New York Preparatory School of Medicine.

The value of Iodide of Potassium as a therapeutical agent in cases of Lead and Mercurial Poisoning, is now well known, many cases having been cured by this treatment alone in our hospitals. M. Melsens (the originator of this treatment) has given several cases of both lead and mercurial poisoning which he had treated successfully by this means. He was of opinion that the metal was acted upon by the iodide of potassium converted into a soluble salt, and eliminated through the kidneys. To prove this to be the case, the metal must

of course be found in the urine. This M. Melsens did not show, as he did not examine the urine chemically.

I have lately had an opportunity of examining the urine of several patients at the City Hospital, who have been under the above treatment for lead disease, and the experiments have entirely corroborated M. Melsens' theory, viz:

That the lead *is not* eliminated before treatment; and

That it is eliminated in the urine after treatment.

The following is the process to which I subjected the urine. I evaporated it to dryness, and burned the residue until all the organic matter was driven off. This residue I boiled in dilute nitric acid, filtered, and then precipitated the lead by a stream of sulphuretted hydrogen gas. Where the metal was present, it was thrown down as a black sulphuret.

I examined a number of samples of urine before the patients had been put on M. Melsens' treatment, and could not detect any trace of metal; while in those examined after treatment, the evidences of the metal were well marked. Sometimes, however, the lead could not be detected until the patient had been under treatment for some time. In most of the cases in which I detected the metal, the patients had been under treatment for at least four days; but in one case, which I examined every second or third day from the time of his admission into the hospital, it was about two weeks from the time of his being put on treatment until the time I first detected the metal in his urine. The dose of iodide of potassium that was given in those cases was ℥ i daily, in divided doses.

There was one patient to whom iodide of potassium was given for another case. After a few days, the characteristic blue line appeared on the gums, and in a day or two afterwards I detected lead in his urine.

The quantity of urine examined each time was not less than six ounces. I have examined twelve cases from the hospital, and three or four from private practice, and all of them with highly satisfactory results.—*Medical Times.*

Saturnine colic has been occasioned by the application of lead plasters (Emplast. Plumbi)—lead in the state of oxide—diachylum. Hence these should be avoided as applications to ulcers, unless no substitute is possible, which is seldom if ever the case.

It would be very desirable to ascertain whether any symptoms of lead poisoning, as colic, palsy, epilepsy, neuralgia, have been observed in the vast mining districts of the West, where many individuals have long been at work.—Editor.

Art. VIII.—*Valves in the Splenic Vein.*

Dr. Crisp, in addition to the preparations exhibited at the first meeting of the Physiological Society, showed the splenic vein of a giraffe, a dyker boc, and a Rocky Mountain deer, (which he had lately dissected) all containing valves. The splenic vein of the giraffe first exhibited was cut short, and only four valves were seen; but in the present specimen, from a male, which recently died at the Regent's-park Zoological Gardens, there were six valves within the space of three inches. Dr. Crisp thought that the discovery of these valves in a certain class of animals only was the first step towards the elucidation of one of the functions of the spleen.

Malpighian Corpuscles of the Spleen shown by the Application of Heat and Nitric Acid.

Dr. Crisp exhibited a portion of the fried spleen of a sheep, in which the Malpighian bodies were distinctly seen in the form of white spots, from the coagulation of the albumen. The same effect was produced in some other animals on the application of heat and nitric acid. He had made this discovery about twenty months since, and he thought it had likewise an important bearing upon another of the functions of the spleen.—*Lancet.*

Art. IX.—*On the Pathology and Treatment of Uterine Catarrh and Internal Metritis:* By E. J. Tilt, M. D., Senior Physician to the Farringdon General Dispensary and Lying-in Charity, and to the Paddington Free Dispensary for Women and Children.

This gentleman in a paper dated in January, 1854, and published in the Lancet, concludes by comparing these two maladies thus:

"Uterine catarrh is very frequent; internal metritis very rare. Uterine catarrh almost exclusively affects the neck of the womb; internal metritis its body. Uterine catarrh is as frequently observed in the single as in the married; internal metritis seems to affect almost exclusively those who have borne children. In uterine catarrh the discharge is viscous; in internal metritis, serous or sanguinous, and and very abundant. Uterine catarrh gives rise to no abnormal growths; internal metritis frequently does. In uterine catarrh life is never compromised; it is not unfrequently so in internal metritis. Injections have been found useless and often dangerous in uterine catarrh, but are sometimes serviceable in internal metritis."

Dr. T. defines internal metritis as a sub-acute or chronic inflammation of the mucous membrane lining the body of the womb. He

applies the tincture of iodine, or the iodide of iron with a *sable* paint brush, introducing it as far as possible into the neck of the womb, having first cleared away the uterine mucosity; on withdrawing the brush, he paints the vaginal portion of the neck of the womb. A drachm of acetate of lead in a pint of decoction of poppy-heads, he considers as forming the best injection in such cases. The incidental menorrhagia, he treats not only by the usual remedies, justly attributing much virtue to the influence of opium, but he recommends what few females will submit to, an issue or a seton above the pubis; and, what still fewer will allow, namely, "the introduction of a brush as far as possible into the neck of the uterus," and, what the fewest of all, if any will permit, that is, the introduction of a *curette* or sound wherewith to scrape the cavity of the womb, under the mere hypothesis that possibly vegetations or morbid grows may lurk within; a threefold combination of tools—speculum, brush, *curette*—a method which Dr. T. follows, even in menorrhagia; one which, however, he says "is almost new."

That the *élite* of the profession should, on so many occasions, find it necessary in the treatment of female maladies, to use numerous instruments, for example, pessaries, whether of wood, glass, or metal, for virgins or matrons, is marvelous; and, at the same time, too, well adapted to dilate, inflame and denaturalize the structure, functions and physiological finalities of the vaginal canal and its affiliated organs. That the speculum, curette, brush, probe, bougie, sound and pessary are sometimes necessary, is indisputable, and, doubtlessly, Dr. Tilt is skillful in their application.—[Editor.

Art. X.—*Pharmacy—New Process for Coating Pills.*

M. Calloud, (de Chambéry) in the *Journal de Pharmacie*, xxiii, 301, treats of the subject of enveloping medicinal substances in a covering to prevent unpleasant taste. After having tried gum, starch and sugar without satisfaction, owing to the hygroscopic tendency of the sugar and gum in moist air or with a moist mass, and their tendency to crack when very dry, he had recourse to the dried mucilage of flax-seed prepared with sugar, with success.

The following is his method:

Take of	Flaxseed,	one part.
	White sugar,	three parts.
	Spring water,	a sufficient quantity.

A thick mucilage is obtained by carefully boiling the seeds, the sugar is added, and the whole of the moisture evaporated by careful desiccation.

This mixture is but very slightly hygroscopic, may be reduced to fine powder, and employed for covering pills. This operation is effected extemporaneously with great facility. The pills slightly moistened, are rolled in the mucilaginous powder, by which they are coated with a layer of the compound.

M. Calloud has used this chiefly for carbonate of iron pills, but it may be applied to other kinds.

Garot's process of coating pills with gelatine is most applicable to disagreeably odorous substances, as assafœtida, castor, valerian, etc., which are completely masked by it. When the gelantine is previously colored with carmine the pills bore the appearance of hawthorn berries.

M. Calloud suggests another process applicable in certain cases, which is the use of the butter of cacao as a covering for pills, where, owing to the gastric irritation, the unmasked pills will cause disagreeable symptoms. The process is very simple. The prepared pills are thrown into melted butter of cacao, then removed with a perforated skimmer, and finally rolled in finely powdered sugar, or what is better, sugar of milk.—*American Journal of Pharmacy, Jan.*, 1854.

Art. XI.—*Negligence in Writing and Putting up Prescriptions.*

The following case which occurred near London, quoted in the American Journal of Pharmacy, by Prof. Wm. Porter, Jr., of Philadelphia, for September 1853, taken from the Pharmaceutical Journal, of London, for July, 1853, "shows the importance of caution in the mode of ordering strong medicines in prescriptions; also the necessity of attention to the dose in dispensing. It is not necessary to mention the names of the parties concerned. The presciption was as follows:

℞. Strychnos,
Nucis Vomici, ℈ij.
Bismuth Trisnit., ʒ iss.
M. Ft. Pulv., xxiv.

The young man put in strychnia and nux vomica, *of each*, ℈ij. The patient took one dose and almost immediately expired. The accident arose from the fact that the word *strychnos*, being written on a separate line, was considered by the dispenser to be another ingredient, instead of being, as it was, the generic title of nux vomica. This would not have misled a young man qualified for his business, who considered for a moment the strength of the dose."

Art. XII.—*Malignant Diseases.*

Professor Gross, of Louisville, Ky., was appointed by the American Medical Association in 1851, to report on the results of Surgical Operations in Malignant Diseases—which report he submitted as instructed at the meeting in 1853. This report, which occupies 158 pages of the transactions, is exuberant with historical lore, analytic research, statistical tables, and the personal experiences of the author and his medical compatriots. The conclusions of a surgeon and author of world-wide reputation, are thus summed up by himself:

GENERAL CONCLUSIONS.

From the facts and statements which have now been presented, embracing the opinions of many of the most intelligent, experienced, and distinguished practitioners in different ages and in different parts of the world, the following conclusions may be legitimately deduced:

First. That cancerous affections, particularly those of the mammary gland, have always, with a few rare exceptions, been regarded by practitioners as incurable by the knife and escharotics. This opinion, commencing with Hippocrates, the father of medicine, has prevailed from the earliest records of the profession to the present moment. Nature never cures a disease of this kind; nor can this be effected by any medicine or internal remedies known to the profession.

Second. That excision, however early and thoroughly executed, is nearly always, in genuine cancer, followed by a relapse at a period varying from a few weeks to several months from the time of the operation.

Third. That nearly all practitioners, from the time of Hippocrates to the present day, have been and are still averse to any operation for the removal of cancerous tumors after the establishment of ulceration, rapid growth, firm adhesions, organic change in the skin, lymphatic invasion, the cancerous dyscrasy, or serious constitutional derangement; on the ground that, if had recourse to under these circumstances, the malady almost inevitably recurs in a very short time, and frequently destroys the patient more rapidly than when it is permitted to pursue its own course.

Fourth. That in all cases of *acute* carcinoma, or, in other words, in all cases of this disease attended with very rapid development and great bulk of the tumor, extirpation is improper and unjustifiable, inasmuch as it will only tend to expedite the fatal result, which, under such circumstances, always takes place in a very short time.

Fifth. That all operations performed for the removal of encephaloid cancer and its different varieties, are more certainly followed by rapid relapse than operations performed upon scirrhous or hard cancer.

Sixth. That in nearly all operations for cancerous diseases hitherto reported, the history has been imperfectly presented, being deficient in the details which are necessary to a complete and thorough understanding of the subject in each case. This remark is particularly true in reference to the diagnosis of the malady, the minute examination of the morbid structure, and the history of the case after the operation, as to the period of relapse, the time and nature of the patient's death, and the result of the *post-mortem* examination.

Seventh. That cancerous affections of the lip and skin, now usually described under the name of cancroid diseases, are less liable to relapse after extirpation than genuine cancerous maladies, or those which are characterized by the existence of the true cancer-cell and cancer-juice.

Eighth. That although practitioners have always been aware, from the earliest professional records, of the great liability of cancer to relapse after extirpation, a great majority of them have always been, and still are, in favor of operation in the early stage of the disease, especially in schirrus, before the tumor has made much progress, or before there is any disease of the lymphatic ganglions, or evidence of the cancerous cachexy.

Ninth. That many cases of tumors, especially tumors of the breast and testicle, supposed to be cancerous, are in reality not cancerous, but of a benign character, and, consequently, readily curable by ablation, whether effected by the knife or by escharotics. It is to this circumstance that we must ascribe the astonishing success which is said to have attended the practice of Hill, of Scotland, Nooth, of England, and Flajani, of Italy.

Tenth. That all operators insist upon the most thorough excision possible; removing not merely the diseased mass, but also a portion of the surrounding and apparently healthy tissues, as well as all enlarged and indurated ganglions.

Eleventh. That the practice has always prevailed, and still obtains, to save if possible a sufficient amount of healthy integument to cover the wound, and to unite if possible the wound by the first intention, on the ground that these precautions will tend much to retard, if not to prevent, a recurrence of the disease.

Twelfth. That much stress is laid by writers upon a properly regulated diet, and attention to the bowels and secretions after operation, as means of retarding and preventing relapse.

Thirteenth. That there is no remedy, medicine, or method of treatment which has the power, so far as we are enabled to judge of its virtues, of preventing the reproduction of the morbid action after operation, no matter how early or how thoroughly it may be performed.

Fourteenth. That life has occasionally been prolonged, and even saved, by operation after relapse, as in some of the remarkable cases mentioned in a previous part of this report; but that, as a general rule, such a procedure is as incompetent to effect a permanent cure as a first extirpation.

The following points may be considered as of an unsettled character; at all events, opinion respecting them is much divided, and farther observation is necessary before they can be positively determined, either affirmatively or negatively.

First. Excision is of doubtful propriety in all cases in which the disease is of hereditary origin, or where it occurs in several members of the same family.

Second. It is doubtful whether an operation should be performed when the patient is very young and the disease is of rapid growth. There is reason to believe that surgical interference, in such a case, will only expedite the fatal issue, which is generally inevitable.

Third. It is problematical whether an operation should be performed when the disease is attended by suppression of the menses, or by great irregularity of this discharge.

Fourth. Not a few surgeons regard a resort to the knife as of questionable efficacy when there is a quickened state of the pulse, occasioned by the local irritation.

Fifth. There appears to be no general agreement among surgeons as to whether extirpation is proper when there are two or more coexistent and accessible cancerous tumors.

Sixth. It is supposed, but the fact is not established, that excision of carcinomatous tumors only tends to hasten the patient's death.

Seventh. It is doubtful whether, as has been asserted by different surgeons, the prospect of a permanent cure is greater, all other things being equal, after an operation on an old cancer, than after an operation on a cancer of recent standing.

Eighth. It has been stated by writers of great respectability, among others by Dr. Macfarlane, of Glasgow, that in robust women of sanguine temperament, the reappearance of cancerous disease, and its subsequent progress, are more rapid after operation than in nervous or lymphatic persons; an assumption demanding verification.

Ninth. It requires to be proved whether excision ought to be performed in the ulcerated stage of malignant disease, as a means of prolonging life and of procuring comparative relief from suffering.

In bringing my labors to a close, I feel conscious that I have added nothing whatever to our previous information of malignant diseases. Nor, in truth, could this have been reasonably expected. My sole aim has been to sum up our knowledge upon the subject; to construct, as it were, a mirror which should reflect the practice and opinions of our predecessors and contemporaries, and thus serve as a guide to future travelers in the same path. In a word, my object has been to show, not only what has been done, but what remains to be done. The facts which I have collected, if useful at all, are so rather in a negative than in a positive point of view. Our knowledge of the results of surgical operations in malignant affections is in a state of transition, which has much to hope from the future, but can gain little, if anything, from the past. What the microscope and animal chemistry, guided by the hand of modern science, may do for the subject, it would perhaps be

premature to predict; already they have rendered the cause essential service, and it would certainly be unphilosophical to suppose that they are incapable of affording farther light.

Finally, in drawing up this report, I have rarely appealed to my own experience, believing that, even if it were fully exposed, it could add nothing of real value to the general stock of our knowledge upon a subject respecting which so much has been written by others. In point of number, indeed, my own cases could not be put in competition with the extensive scale of facts brought forward by some of the surgeons, the results of whose labors it has been my duty to bring before the Association. I have little, indeed I might say, no confidence in any operation for malignant diseases, except the cancroid varieties; and I have for years past, as Professor of Surgery in the University of Louisville, deemed it my duty to discourage a resort to the knife in all cases of the kind, especially in scirrhus and encephaloid of the mammary glands, ample experience having satisfied me of the utter futility of such an expedient, however early and efficiently employed. In cutaneous cancer, on the other hand, my rule has always been to operate, provided the disorder has not advanced so far as to preclude the possibility of removing the whole of the morbid growth; and provided, also, that there is no evidence of constitutional infection. To this rule I shall rigidly adhere, unless my own experience, or the experience of others, shall show me the impropriety of it, when I shall most cheerfully abandon it.

LOUISVILLE, April 20, 1853. S. D. GROSS, *Ch'n.*

Art. XIII.—*The Relations of the Pulse to certain states of Respiration* :* By S. WEIR MITCHELL, M.D., of Philadelphia.

The mere numerical relations of respiration to the pulse, and conversely, of the pulse to respiration, are so well known as to demand no further notice here. Increased activity in the one commonly entails a like state in the other. To this general law there is a very remarkable exception, whose insertion at this point is justified by its singular novelty and interest.

The substance of the following observations is taken from an unpublished essay upon the state usually known as the "mesmeric sleep," an abstract of which was read by its author, Prof. J. K. Mitchell, before the Philadelphia College of Physicians, in the year 1839.

Thirty-seven cases of "animal induction," or induced somnambulism,

*The numerical tableaux illustrative of the fundamental postulates of this essay are omitted for want of space. This paper, printed in April, 1854, cannot be credited with certainty to any particular journal, because the sheet received does not designate any one.—[EDITOR.

were studied with reference to the relations of the pulse and respiration. The result is briefly stated in the following summary of a very elaborate table:

Pulse before sleep.	Pulse of sleep.	Respiration before sleep.	Respiration during sleep.
Average: 81.7	Average: 105	Average: 19.04	Average: 19.68
Difference: 23.3		Difference: 0.64	

The pulse of the mesmeric state was in every instance greater than that of the waking condition; the least excess being 8, the greatest 48. If one extraordinary case be omitted from the table, the average proportion of the numbers of respirations would be as 18.9 in the waking condition to 18 in the sleep.

While, therefore, the pulse always quickens, the respiration either falls absolutely below the normal waking standard, or undergoes no change. The proportion of the pulse to the respiratory movements was four to one in the waking condition, and nearly five to one in the sleep; an excess of twenty-five per cent. for the mesmeric slumber.

The facts to which I would now desire to call attention are easily brought within range of the reader's personal experience.

Let a finger be placed on the pulse, and let the chest be held fixed in full inspiration, the pulse will be observed to become less frequent.

Numerous individual exceptions occur, and, in a few cases, negative or exceptional results are obtained. On the other hand, the observer is sometimes alarmed by the long pause between successive pulsations. In one or two cases I have been forced, on this account, to suspend altogether any further research. Again: let the chest be fixed in a state of extreme expiration, and the pulse will almost invariably quicken.

Persons who are susceptible of these changes will thus observe that there is a point, midway between these extreme conditions of respiration, at which fixation of the chest fails to affect the pulse. If, starting from this point as the zero of influence, the lungs are slowly inflated to fullness, the pulse will usually beat less and less rapidly. In like manner, during the movement of expiration, the heart quickens its pulsations, and sometimes they suddenly increase to a still greater extent when a state of complete expiration is attained. In a few rare cases, these changes may be studied in the thoracic movements of those whose natural respiration is excessively long and laborious.

For brevity and convenience I shall term the two pulses induced by extreme respiratory conditions, respectively, the inspiratory and the expiratory pulses.

In forty persons the average normal pulse was 80.5. In fixed inspiration it fell to 70.15; in fixed expiration it rose to 93; giving, as the average of extreme difference, 23 per minute.

After very careful study of all the phenomena, in many experiments and vivisections, I feel disposed to refer the induced pulses of extreme respiratory states, to causes chiefly physical.

I confess to some pleasure in thus placing these interesting facts within the domain of the laws of dynamics. This pleasure is at least not lessened by the knowledge that I am indicating a new region for research, and that many of the points in question have hitherto escaped the eager reach of physiological induction.

The present state of knowledge in this direction, is best illustrated by the following quotation from a standard text book of the highest character:

"The pulmonary circulation is unaffected by atmospheric pressure, and is not exposed to the influence of the pressure of muscles. The force by which it is accomplished, and the course of the blood, are alike simple."—*Kirke's and Paget's Handbook of Physiology, p.* 132, *2d Am. ed.*

I have repeatedly observed in rabbits the phenomena recorded by Reid, the pulse becoming slow during inspiration, and fast during expiration, when those movements were long and laborious. Animals may be forced to breathe thus, and in fact, I have sometimes noticed similar phenomena in man. They have been in part explained by Reid. He believed that, during dilatation of the lung, the flow of blood from its spongy tissue was necessarily lessened. The left heart would thus receive a smaller share of blood, and would therefore contract less often.

During expiration, a portion of blood, as well as the air of the lung, is expelled; this blood, moving in the tide of circulation, enters the left heart and compels it thus to contract often enough to get rid of the overplus of fluid. Hence a more rapid pulse.

Fixed Inspiration.—When the lung is fixed in the state of complete expansion, new dynamical relations arise, and the pulse becomes slow.

After several fruitless efforts to apply the hæmadynamometer within the tide of the pulmonary circulation, I attained the desired ends in a very simple and novel manner.

I observed that the exposed lung of an animal, kept alive by artificial respiration, flushed as it shrunk in expiration. This is possibly due to the closer aggregation of the capillaries. It might also be caused, in part at least, by actual increase in the amount of blood, which the most minute vessels contain during complete expiration. Indeed, the extreme pallor of the surface of the lung, in deep inspiration, gave some weight to this idea.

To test these inferences, I opened a rabbit, and while artificial respiration was made, I carefully scratched through the pleura with a cataract needle, thus wounding the capillary vessels of the living lung. The lungs were fully dilated while I effected this, and, to my sur-

prise, scarcely a drop of blood followed the instrument. As the lungs fell in expiration, the wound began to bleed—always bleeding most freely in complete expiration. This experiment was repeated many times, in the presence of Drs. Brinton, Morehouse and Da Costa, and always with the same result, so long as we took care to injure no large vessel. The inference seems to me a fair one, that, in full inspiration the capillary circulation of the lungs is somewhat impeded. If this be so, then in fixed and complete inspiration the left heart will, for a time, receive less blood than usual, and the excess will exist in the right heart and pulmonary artery. Is this the cause of the slow pulse of full inspiratory fixation? Some light is cast upon the subject by a study of the phenomena of asphyxia. In that condition the heart beats slowly, as soon as the circulation begins to be arrested in the minuter vessels of the lung. The conditions are then to some degree alike—too much blood on the right side, too little on the left. Without doubt, the slow pulse of asphyxia is also due, in part, to other causes, but the circumstances I have named have always been admitted to exert a large share in the phenomena in question. While offering this as a partial, and, I admit, not a wholly satisfactory account of the pulse change in inspiration, I do not mean to exclude the element of reflex nervous impression.

Finally, in sudden and violent muscular exertion, the right heart receives for a moment an excess of blood; yet, in this case, it beats more rapidly. In inspiratory fixation there is also a surplus of blood in the right heart; but, in this instance, it is a *reflux* of fluid which congests the pulmonary arteries. We have also, in inspiratory fixation, the other additional element of deficient supply to the left heart. We have no further means of estimating the relative effect of these several sets of conditions to which the two hearts may be subjected.

1. It seems to me clear enough that the pulmonary circulation is modified by the various conditions of respiration in which the lungs may be placed. Do not relatively similar effects attend every respiratory movement, however simple? We cannot demonstrate this upon the healthy living lung, yet the inference would seem to be a fair one

2. During complete inspiration the tide of blood is momentarily retarded in the capillaries of the lung. Aeration of the vital fluid then takes place with the greatest facility; and during expiration, and more especially in complete expiration, the blood thus fully aerated is expelled from the lungs by the rapidly acting heart. In other words, the circulation in the lungs is slower when these organs contain most air, and becomes most easy and rapid during the movement of expiration.

The effect of respiration, in mechanically diverting the blood from the course of the fœtal circulation, is also of interest in this connection.

At this point I shall leave this interesting subject. If I have failed to observe correctly, or to reason justly, I shall at least have called to the task more careful observers and more able thinkers.

In the course of this paper I have avoided detailed accounts of individual vivisections, prefering rather to state results than to overload my

pages with useless matter. Many of the vivisections alluded to were made by the skillful hands of my friends, Drs. Morehouse and Brinton, aided by our private pupils, whom I desire to thank for their kind and ready aid.

Art. XIV—*Fevers—their Pathology, Treatment, &c.* Digested from Braithwaite's Retrospect, Jan. 1854; taken from the Med. Times and Gaz., The London Lancet, &c.

On the Relation between Therapeutics and Pathology.

[Dr. Johnson truly observes that on few subjects is there such diversity of opinion as upon the effects of remedies in disease, their modes of action, and the best methods of administering them. This, in a great measure, he believes to be owing to the carelessness manifested as to the natural history of the disease. If we had a specific for most diseases, as quinine for ague, the importance of diagnosis and scientific pathology would not be so great. They are the more important, however, since, as we do not possess many of this class of remedies, we are obliged to treat diseases on what are termed *general principles;* and it is improbable that for many diseases specific remedies will ever be found.]

There is, probably, no better test of the truth or soundness of any doctrine, when applied to practice, than the fact of its being consistent with the dictates of well-informed common sense; and, perhaps, I could give no better illustration of the beneficial influence which has been exerted by this kind of appeal, than is to be afforded by the modified views now prevalent with regard to the nature and treatment of inflammation. It certainly does appear contrary to common sense to suppose that a disease, whatever may be its name or its nature, which has been induced by exhausting and depressing influences—by excessive labor, by loss of rest, by abstinence, by hæmorrhage, or by some other drain upon the bodily strength—that a disease so originating can be removed by a recourse to measures which have the same exhausting and depressing tendency—by the free abstraction of blood, by a rigidly low diet, and by active purgatives. Science has listened to the voice of common sense in this matter, and in spite of the dogma *similia similibus curantur*, a method of practice which was originally suggested by imperfect and erroneous views respecting the nature of inflammation, has been greatly modified, and with the happiest results. It is now becoming more and more apparent, that the *quantity* of blood in the vessels of an inflamed part is not the primary and most important fact in the phenomena of inflammation; that the so-called increased afflux, or determination of blood, is in reality produced by impeded flow of blood through the capillary vessels, resulting from an altered relation between the blood and the tissues with which it is

brought into contact. This altered relation between the blood and particular tissues, may depend upon an excess of nutrient materials in the circulating fluid, or it may depend upon a directly opposite condition, or upon other changes in the *quality* of the blood. So that, however great may be the resemblance between the local phenomena of inflammation in different cases, the treatment must be conducted with a constant reference to the causes which have given rise to the disease.

The importance of ascertaining the remote originating cause of a disease is great in proportion to the degree of doubt which there may be as to the precise nature of the local changes upon which the symptoms in any given case are supposed to depend.—*George Johnson.*

Eighteen cases of Typhus Fever treated by the free exhibition of Brandy—one death.

[In all these eighteen cases the type of the disease was of the well-marked typhus character—presenting the following symptoms previous to the commencement of the treatment:]

A copious eruption of scattered measle-like spots (mulberry or typhus rash); bowels either confined or but slightly relaxed; great prostration of strength; delirium (in six cases coma was present); a small and very rapid pulse. It may be well to premise, that they were treated as is done in almost all general hospitals in the open wards, their beds being purposely arranged so as to occur at some distance from each other, in order to prevent the accumulation of contagious emanations. The treatment pursued consisted in administering, either every hour or every half hour, day and night, from half an ounce to an ounce of brandy, with a draught every second hour, containing sp. æth. chlorici min. x., ammoniæ carbonatis gr. v., aq. pur. ℥ j. The patients were induced to drink as much strong beef-tea as possible; the head was always shaved; and, in most, a blister was applied to the scalp. We are indebted to the careful observations, noted daily by Mr. Macnamara, the clinical assistant in charge of the cases, for the whole of the particulars respecting them. The first to which we shall allude was a very severe attack, and happened to an elderly and unfavorable subject; the beneficial effects of the alcoholic stimulant is strikingly shown, and there even appears some cause to infer the superiority of brandy over wine.

On the day that the administration of brandy, etc., was commenced, the pulse had, in five cases, a frequency of 136 per minute; in three, of 126; in seven, of from 120 to 126; and in one, of 116. After the measures above specified had been pursued for four days, the pulse had, in eight cases, fallen to 92; in five others it had fallen below 92 on the fifth day, and in the remaining four to below 90 on the sixth. Again, taking the day on which the treatment was commenced as our starting point, the skin, previously hot and dry, relaxed and became moist and perspirable on the fifth day, in nine case; on

the sixth day, in five cases; on the twelfth day, in one case, and in the remaining two the date of this crisis was not recorded.

The degree of success exhibited by the above facts is, we suspect, very considerably beyond that usually obtained in cases of so severe a type as those under consideration, and is very encouraging to a pursuance of a similar plan of treatment in future. That the success did really depend on the treatment, appeared to be conclusively evidenced in several cases, in which the pulse progressively increased in frequency up to the time that the brandy was ordered, steadily fell from that day forwards. The relapses of one or two, in consequence of the accidentally inefficient administration of the remedy, also afford important support to the same conclusion. In respect to the numerical age of the fever at which the brandy treatment was commenced, it varied so much in the different cases that there does not appear to be any practical advantage in attempting to state it. In all, however, the first stage had passed, and low "typhus" had become fully developed.—*Under the care of Dr. Todd.*

Quinine in Fever.

[Cinchona bark has been used in continued fever since the period of its introduction into England. Sydenham did not use it in continued fever, but Morton, his contemporary, gave it boldly, ℥ j. every three or four hours, with great success.]

Dr. Sims expressed himself thus: "I solemnly declare that I never saw a case of nervous, low, putrid, or malignant fever, when the patient could be brought to take this medicine in sufficient quantity, turn out unfortunately." He did not rely on less than six or seven ounces given in little more than two days. Drs. Fordyce, Armstrong and others, were of an opposite opinion.

[Our readers are aware, that under the form of sulphate of quinine, Dr. Dundas has recently introduced it into this country as a remedy for the ordinary forms of continued fever. The character of the disease is the ordinary one, presenting no peculiarities.]

The most frequent complication was sub-acute bronchitis; a few cases of pneumonia and a few of pleurisy occurred. Bowel complication of a serious nature was extremely rare.

Before administering the quinine it is well that the patient should have an emetic, for of itself it is beneficial, and after its operation the quinine rarely disagrees with the stomach. It may be given at all stages of the disease, unless there be great prostration. The dose of the quinine is from three to ten grains every two or three hours, given in the form of powder. From the commencement the patient is allowed three pints of milk and one of beef-tea daily, and, if at a late stage of the disease, or the patient has been a drunkard, wine or whiskey is given. Purgatives do not appear to be necessary, for the emetic usually relieves the intestines as well as the stomach;

and if constipation should occur, a simple enema is generally sufficient to remove it.

[A severe case of typhus—a rare disease in New Orleans, except among recent immigrants—occurred in the editor's practice, which terminated successfully in a young lady of Mr. Levi's family in Annunciation street, while these sheets were passing through the press. The treatment consisted of small doses of opium, morphia, ferro-cyanate of quinine, pil. hydr. sinapisms, a large abdominal blister, wine, a persistent horizontal position for two weeks, and an occasional enema. The skin, abdomen, chest, throat and head were all implicated; the debility extreme. How British typhus can agree with the digestion "of three pints of milk and one of strong beef tea" per day, is marvelous. The patient in this climate is apt to loathe and vomit food.]

In those cases where the quinine treatment was commenced during the first seven days of the fever, the average duration of the disease was 4.4 days after the first administration of the remedy; when during the second week 5.8 days. From cases treated during or after the third week no inference can be drawn, the disease having then nearly run its course.

Of sixty-one uncomplicated cases, eleven were under the treatment during the first seven days; of these, two were free from fever on the second day after treatment; one on the third day; two on the fourth day; three on the fifth day; one on the sixth day; one on the seventh day; one on the eleventh day. In twenty-four, the treatment was commenced during the second week; of these, one was free from fever on the second day after treatment; two on the third day; five on the fourth day; seven on the fifth day; three on the sixth day; two on the seventh day; one on the eighth day; two on the ninth day; one on the twelfth day. In twenty-six, the length of time that the fever had previously existed was not ascertained; of these, five were free from fever on the second day after treatment; six on the third day; three on the fourth day; four on the fifth day; three on the seventh; one on the eighth day; three on the ninth day; one on the twelfth day.

The quinine was also given in twenty-seven complicated cases, the chest being involved in twenty-two, the abdomen in two, and in three both the abdomen and the chest were affected. Of the twenty-two cases where the chest was involved (the affection being, as before remarked, generally sub-acute bronchitis), fifteen were treated during the first week; of these, one was free from fever on the third day after treatment; one on the fourth day; two on the fifth day; four on the sixth day; two on the eighth day; one on the ninth day; two on the tenth day; one on the eleventh day, and one on the twelfth day. In seven the treatment was commenced during the second week; of

these, two were free from fever on the third day; two on the fourth day; one on the sixth day; one on the seventh day; one on the tenth day.

The remedy employed in the above complication, in conjunction with the quinine, was a large mustard jacket placed round the whole of the chest, both back and front, applied twice or thrice daily, and left on as long as the patient could bear it. It possesses the advantage of not weakening the patient, at the same time being an excellent counter-irritant. Great care should be taken, more particularly in winter, to prevent the patient taking fresh cold while at the night chair; in hospitals we think the use of the bed-pan the best preventive.

Two cases occurred with abdominal complication; of these one was admitted on the ninth day of the fever, and recovered on the sixth of treatment; the other was admitted on the fifth day of fever, and recovered on the seventh day of treatment.

Three cases occurred with abdominal and chest affections; of these two were admitted on the fifth day of the disease, and were free from fever on the fifth day of treatment; one admitted on the eighth day of the fever was free from it on the tenth.

Thirteen cases in which the quinine was given proved fatal; of these five died within forty-eight hours after admission. Of the remaining eight, one was admitted on the eighth day of fever, and died on the eighth of treatment. One was admitted on the fifth day of fever, and died on the sixth of treatment; this man had persistent hiccough, congestion of lungs, and had had a severe attack of fever seven months before. One was admitted on the sevenh day of fever, and died on the tenth of treatment; this patient had been allowed nothing but barley water previous to admission; tubercles in the right lung. One was admitted on the fourteenth day of fever and died on the fifth of treatment; the quinine seemed to produce no effects. One was admitted on the ninth day of fever and died on the sixth of treatment; this patient had two kinds of spots, the one disappearing, the other unaffected by pressure; had been a hard drinker. One was admitted on the fifth day of fever and died on the eighth of treatment; a woman seventy-six years old; this was the only fatal case where the pulse was reduced below 100. One was admitted on the fourteenth day of fever and died on the fourth of treatment; the quinine did no good. One was admitted on the eighth day of fever and died on the third of treatment; had involuntary motions when admitted.

The effects of the quinine on the pulse is, in favorable cases, sometimes very remarkable; in one case it fell from 104 to 72 in twenty-four hours; in another from 140 to 84 in seventy-six hours; but generally speaking the reduction is gradual and steady. In cases which terminated fatally, the quinine, with one exception, never reduced the pulse below 100; it sometimes fell from 148 to 124, from 112 to 108, from 140 to 114, but not below 100. None of the fatal cases

were under treatment before the fifth day. In complicated cases the effect on the pulse is less marked than in those which are free from complication.—*Dr. Robert Gee and William Eddowes.*

Clinical Remarks on Yellow Fever.

[Dr. Cummins observes that the small doses of quinine, recommended in our Pharmacopœias, have no effect in yellow fever, and that many cases are lost from the dread of pushing the remedy to the necessary extent.]

Most persons believe that when deafness is produced there is no further indication for its use; but this is a fatal error, as in most cases it is necessary to push it for a much longer period. It is difficult to lay down any fixed rules as to the quantity that should be given in any individual case; but if a well-founded and long established rule of medicine is borne in mind, that in certain diseases ten times or more of the ordinary dose of medicine is required to produce its effect, and that yellow fever is one of those diseases, it will be found that the best guide for the administration of quinine is the *amount of nervous depression present.*

Another general rule of medical science must also regulate the administration of this medicine—that depression follows the excitement of all stimuli. Now it is certain that quinine acts as a stimulus to the nervous system, and that its action as such is temporary, and invariably followed by depression. Now as long as the yellow fever poison is in the system, anything which promotes depression of nervous action must increase the disease; it is therefore obvious that the primary or stimulant action of this medicine must be kept up by continuing its administration until the poison has passed off the nervous system, which it is certain to do in the third stage; for, as I have mentioned, it has its limit of operation like other poisons, and wears itself out in three or four days, leaving only its effects behind. It is true that the more quinine given, the greater will be the depression when its use is discontinued; but if this occurs after the poison passes away, it will be a depression *sui generis*, and not an aggravation of the specific depression induced by the poison. I do not mean for a moment to make light of the depression which follows the use of large quantities of quinine; on the contrary, I consider it as the probable source of the tedious convalescence which *occasionally* is observed after yellow fever, and which renders removal from the tropics necessary. But I would say that by comparison with the specific depression of yellow fever it becomes trivial. We have a choice of two evils before us, either by pushing quinine to its full extent, to increase the depression of convalescence, which can be cured by change of climate, &c., or to increase the specific depression of the poison, by allowing the quinine depression to exist along with it; and who that knows the awfully destructive nature of the yellow fever depression would hesitate in choosing the first? It may

be supposed from my remarks upon the duration of the poison that I consider it a disease which, when once established, must go on for a certain number of days, but this is far from being my opinion; for I have frequently seen a most violent attack cut short by thirty or forty grains of quinine administered *early*, and perhaps repeated in an hour. Now, the possibility of such an occurrence as this seems to contradict my remarks upon the danger of leaving off quinine too early; but there is a vast difference between cutting short a disease and only suspending its action, and the danger of the after depression of quinine is not to be apprehended in the former. If a patient is seen *very early*, and we determine upon attempting to cut short the disease, it is better to give thirty grains of quinine and twenty grains of calomel immediately, and another similar dose conjoined with five grains of James' powder an hour afterwards. This should be followed in two hours by an ounce and a half of castor oil. It is extremely probable diaphoresis will then be established, the pain in the head and back will have subsided, the congestion have been removed, the bowels well opened, and all the functions perfectly restored to health. This is cutting short the disease, and we need not fear the depression which follows the quinine in such a case; but it is no easy matter to determine exactly whether the disease is stopped or suspended; and we must watch the case closely for at least twenty-four hours, and if the symptoms are not completely removed, we have lost time and injured our patient by permitting the secondary effect of the quinine to become established, so that an attempt to cut short the disease is not without a certain degree of risk; at the same time, if tried *early*, it very frequently succeeds. It must therefore remain a question for the physician to decide whether the symptoms are sufficiently recent to admit of their cause being rooted out of the system, and my own experience limits the attempt to five or six hours. We must not allow an idiosyncracy to interfere with the full use of quinine.—*Dr. W. J. Cummins.*

Practical Observations on Yellow Fever and its Treatment with Spirits of Turpentine.

[To Dr. Gilbert King is due the credit of having first used this remedy in bilious remittent fever. Mr. Laird is of opinion that this disease is not of an inflammatory diathesis, but a blood poison from external influences—the organic lesions being secondary. Hemorrhages from the nose, mouth and bowels of a passive character, proved that the blood was in a dissolved and deteriorated condition.]

It appears natural to conclude that the blood, after its primary impregnation, or rather at the commencement of the attack, becomes afterwards further contaminated by the non-elimination of the different excretions, as is known to take place in certain forms of jaundice, in milk fever and disease of the kidneys.

With this view, therefore, in regard to the pathology of yellow fever, and there is good reason to believe also of cholera, supported as it is by the analogy of the action of certain poisons on the system, I will now endeavor to prove the consistency and reasonableness of the turpentine mode of cure. During the epidemic it was first prescribed by Dr. King, in five very bad cases of this disease, with the view solely, I believe, of restraining passive hemorrhage, when its peculiar salutary influence was so strikingly observed. This happened on the 27th of August, about a month after the fever had broken out; a revulsive plan of treatment, including general and local bleeding, to a large extent, having up to this period been practiced with very unsatisfactory results. After these experiments on the five cases alluded to, all of whom remarkably recovered (and two were reported as having had black vomit), the turpentine was afterwards given in every case and in every stage of the disease, with what success documents then sent into office will show. When I joined the hospital on the 19th of September, the epidemic may be said to have been at its height, and there were then 103 cases of fever under treatment. The general mode pursued was a moderate bleeding at the commencement (from 12 to 20 ounces), followed by cupping or leeching, if it seemed necessary.

If the patient had received no medicine prior to admission, the primæ viæ were cleared out by a purgative dose of calomel or blue pill, along with the solution of Epsom salts, and the turpentine then given in doses of twenty minims in a little camphorated water three times a day. In consequence of strangury, it was occasionally administered in combination of sweet spirits of nitre; but this troublesome symptom was happily of but rare occurrence, and then generally consequent on the application of blisters. Small doses of tincture of opium, and also castor oil, were occasionally combined with it, in cases attended with frequent bloody and otherwise vitiated dejections, or in an opposite state of the bowels. The auxiliary treatment comprised sinapisms and blisters to the epigastrium, emollient enemata, and during the stage of debility, wine negus, beef tea, &c., cinchona injections. The principal remedy being, therefore, the turpentine, since in the generality of the cases nothing else was given, I consider it, without any prejudice whatever, as a remedy in bilious remittent fever, not only perfectly consistent with, but particularly indicated by, the symptoms during life, and also the appearances after death, for the following reasons: First, the hemorrhagic character of the disease, depending, doubtless, on some change either in the chemical or vital properties of the circulating fluid; secondly, from the speedy embarrassment of the different excretory functions, particularly that of the kidneys, a diminished or suppressed excretion of urine having been particularly observed here as the most unfavorable symptom, and proved by the frequently contracted and empty condition of the bladder in fatal cases. As regards the first proposition, since this medicine is universally acknowledged as producing certain

styptic and other salutary effects, melæna and ordinary passive hemorrhage, is it not reasonable to infer that it will be equally effectual in fevers, not recognizing inflammation as their proximate cause and attended with a somewhat similar pathological condition? In regard to the second proposition, its well known physiological influence on the renal and cutaneous vessels proves it to be, as a diuretic and sudorific, the very remedy which Nature points out in the means she herself adopts in the spontaneous cure of malignant fever. Besides these long and well known properties, and also its acknowledged action on the nervous system, I believe that turpentine possesses antiseptic or antiscorbutic qualities, as I have in several instances observed in scurvy, and particularly in scorbutic ulcers of the leg, indicated by their florid, healthy appearance, after a few doses of this medicine. That it is not in the doses above mentioned a stimulant, but, on the contrary, an indirect sedative, and therefore not counter-indicated in the first stage of yellow fever, our experience here most satisfactorily proved in hundreds of cases, as it has also since done in the treatment of dysentery. Unprejudiced as I hope I am, and being anxious to bring this really valuable remedy more into notice by an honest appeal to plain ascertained facts, I beg respectfully to state, in the most earnest and unqualified manner, that instead of exciting the circulation it quieted it, and allayed the urgency of the primary pyrexia by quickly (and in some cases almost immediately) restoring the pent-up secretions. The moderate bleeding, which was generally practiced at the commencement, and frequently found essentially necessary to divert local determinations, no doubt materially assisted it in its action on the skin and kidneys, and on this account must be considered as a very important step in this mode of treatment.—*James Laird.*

Art. XV.—*Professor* J. L. Riddell's *Opinion on the Causes of Yellow Fever, &c., &c.*

To the Sanitary Commission of New Orleans:

Gentlemen—In compliance with your resolution of Dec. 21, I have the honor to present you the accompanying records of testimony, respecting the origin and spread of Yellow Fever this year, in some of the Southern towns, as high up the Mississippi as Lake Providence.

Deep interest is universally manifested in the labors of our commission; and all possible facilities were tendered me in the prosecution of my inquiries. My constant regret has been, that want of time would not permit me to prosecute them further.

Of course I found conflicting opinions, and now and then statements more or less contradictory; yet from all, as well as from data previously in possession, it appears to me the following inferences are deducible:

1st. That our yellow fever of 1853 has not been personally contagious; that the poison, virus or material cause producing it, does not emanate in an active condition from the person of the patient laboring under the disease.

2d. That the disease has been marked by characters of infection and infectious communicability, the poisonous matter (doubtless some species of living organism) maturing its germ or spores on the surface of solids devoid of life, surrounded by confined or impure air; which germs become diffused in the impure atmosphere.

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

4th. That the towns and plantations of the Southwest have this year derived their yellow fever from New Orleans.

5th. That although black vomit fevers or types of yellow fever may perhaps originate in this region, yet, that the germs of our epidemic of 1853, have probably been derived from countries further South.

6th. That the mixture of equal parts by weight, of black oxide of manganese, sulphuric acid and water, which in the cold will continue for many days to develop ozone, promises to be the most convenient, most economical and most efficient disinfectant ever used; and therefore deserves hereafter a fair trial.

7th. It is proper and feasible for New Orleans to have some kind of quarantine in certain months of the year, which will exclude filthy persons, filthy clothing and filthy ships, until they are fumigated; and goods from West Indian, South American and Mexican ports, until they are fumigated.

8th. The city should be kept cleaner than heretofore, by efficient drainage, and sanitary regulations carried into effect.

9th. Legal ordinances should be framed and carried into effect, to prevent the undue huddling together of human beings within the limits of the city.

Respectfully, J. L. RIDDELL,
Member of the Sanitary Commission of New Orleans.

New Orleans, January 1, 1854.

Part Third.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Prelusory Remarks.

"Curious conjectures he may always make,
And either side of dubious questions take."

CRABBE.

ÆSTHIC science affords no contrast more striking than that between the Ideal and the Actual in the important department of literature devoted to critical and analytic Reviews. Practically, what is and what ought to be, must be often regarded as belonging to differential categories. The causes which operate—the impelling motives, all powerful in the rendition of a critical verdict, damnatory or eulogistic—do not always appear—are not always avowed—cannot always be divined; and will not always bear the light of day. Silence, "faint praise," and open condemnation, may have the same warp and woof, the same purpose, the same result.

Praise and censure, awarded by chance, in stereotyped phrases, without analysis and without just appreciation, are powerless for good, mighty for evil. But who will venture upon innovation and reformation? What are the causes that lead to the evils intimated? A few of these may be enumerated. A Medical Review, like a College, ought to be distributed among different persons with a reference to their qualifications to pronounce upon what they actually know in the particular branches which they have studied thoroughly. They should read the book as well as review it. A reviewer should not write anonymously, but sign his name to his communications. The author reviewed and his book, not the publisher, should be named and considered. The gift of a book from the publisher, how desirable soever

it may be to the donee, is not without effect; the praise almost constantly awarded, aids the sale more, far more, than an ordinary advertisement. The publisher's name, and the bookseller's, too, is usually mentioned with great commendation, and the public are urged to buy the work of that particular bookseller who bestows the book, while, perhaps, a dozen of others equally worthy of patronage are passed by.

"Silence is great." The New Orleans Medical and Surgical Journal does not affect to be over-righteous, and proposes at present no radical reform, except that of not preferring, in print, one bookseller to all others, and that of recommending the signing of the name to the critical verdicts given for or against authors and their works. This latter doctrine, advocated some years ago by the present Editor, and since adopted by that most able work, "The British and Foreign Medico-Chirurgical Review," was expressed as follows in a Review of the work of the late Dr. Drake; and it is hoped that the reader will for this once, now that the writer is entering upon the career of Editor, pardon the liberty of quoting one's self:

An author expects commendation; if he be vain, it gratifies his ostentation; if disinterested, it diffuses throughout his entire being an inexpressible satisfaction, which arises from the hope that his labors will promote the well being of society. But in no case can a well balanced mind derive real pleasure from the approval of others, when he at the same time feels conscious that he does not deserve it. Whatever may be the aims of an author, they must fail if his work be not read and appreciated. An unread book is, in effect, no book—a nonentity. "The misfortune of a book," says Boileau, "is not the being ill spoken of, but the not being spoken of at all."

One of the ends aimed at by the modern system of periodical reviews—a self-imposed one—is that of preluding the true value of the books which appear from time to time. The self-appointed forerunner and critical informer-general, whose vocation consists in giving opinions about other people and their works, is, in the present organization of society, a necessary evil, but still a happy invention for those who cannot or will not examine for themselves. It has been supposed that reviewers have a predilection for bad books, because "bad books make good reviews, as bad wine makes good vinegar."

The writer of a good book would probably be, in most cases, the

best reviewer of it, were he willing to express his own estimate of its value. After all, a review is but one man's opinion, perhaps an impertinent one, concerning another. Seeing that this is the case, why should the reviewer, as is the custom with critics who write in the English language, assume the ponderous editorial WE, and launch forth his anonymous, perhaps sectarian dicta, masked by impersonality. The French, who invented reviews as early as 1665, affix their proper names to contributions of this kind. It is natural for the dove to wish to know whether the hawk is the reviewer. The wolf and the lamb, the fox and the hound, must have a similar curiosity. But so unlimited is the assumed jurisdiction of a reviewer, in many cases, that it is not necessary to speak of the work reviewed at all; for its title will serve as the point of departure for an independent essay. Reviews, however, proceed in general upon the principle of the division of labor. "People do not think for themselves; their reviewer thinks for them." A grievous evil this is. Gœthe says:

"Books are now written, not to be read for the sake of information and instruction, but to be reviewed, to the end that people may talk and descant upon them *ad infinitum*. Since it has been the custom to review books, no soul reads them but the reviewer, and he too but so-so. To be sure it rarely happens now that any one has something new to say, and communicate something proper to himself, the growth of his own brain, instructive and worked out with love and industry; so it is all one in the end."

An analytic Medical Review is one of the needs of the South, whither many valuable books never come, or come tardily; a Review which would give an exhaustive analysis of all that is new and useful in foreign and native works; works which are not the mere handbooks republished on the commercial principle. A few hundred dollars per annum spent in the purchase and speedy transmission to the South of the most able works appearing in insular and continental Europe would afford, upon careful analysis in a Medical Review, much practical matter of great value which is inaccessible to the practitioner in the South, thereby saving money and time as well.

Rev. I.—*Homœopathy: its Tenets and Tendencies, Theoretical, Theological, and Therapeutical:* By JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery, University of Edinburgh; and Physician-accoucheur to the Queen for Scotland; President of the Medico-Chirurgical Society, and of the Obstetric Society: lately President of the Royal College of Physicians; formerly President of the Royal Medical Society, and of the Royal Physical Society, Edinburgh; Foreign Member of the Imperial Academy of Medicine of France; Member of the Society of Surgery of Paris; and of various Medical Societies in Stockholm, Copenhagen, Berlin, Ghent, etc. First American, from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. Pp. 302, 8vo.

Homœopathy fairly represented; a reply to Professor Simpson's 'Homœopathy' misrepresented: By WILLIAM HENDERSON, M. D., professor of general pathology in the University of Edinburgh. First American edition, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8vo., pp. 302.

ALTHOUGH the material interests of property, trade, commerce, as well as social rights, may, in an enlightened nation, be safely entrusted to majorities, yet many scientific truths in astronomy, metaphysics, anatomy, physiology, surgery, and therapeutics, which require years of profound study for their comprehension by even the greatest intellects, could not be submitted to vote with any reasonable expectation of a just verdict from the mass of the community. In medicine, majorities are not always on the right side, nor minorities always on the wrong; and the profitable side may be with the few as well as with the many—with the false as well as with the true—with the dishonest as well as with the honest; while neutrality for the sake of gain is little better than open warfare against the truth. A slightly differing friend is not far removed from an avowed enemy.

To the discerners of the spirit of our era, it must be evident that extremes form its characteristic features; witness its forward and retrograde marches—its brilliant discoveries on the one hand, and its ineffable delusions on the other. If the dark ages made less progress in the physical and medical sciences than the present, they were less disfigured by palpable fanaticisms and pretended revelations. The mythic in antiquity embodied the ideal in its most beautiful forms: our age, reversing the classical models, offers the most hideous instead, among which is quackery.

All of the quackeries which in every age have deformed the face of society, claim a common origin—claim to be the true interpreters of the eternal, immutable, paramount laws of Nature—claim to be truth itself, which is one and indivisible—claim to be founded on experience, which, when properly appreciated and translated, is but another word

for the truth. Appealing to their experience, the apostles of deception often deceive without being themselves deceived; they proclaim belief without believing—publicly solving the problems of health and disease, of life and death, of which inborn conscience tells them they know little if anything.

Homœopathists may boast of the intelligence of their followers, including all sorts of celebrities. Perhaps they can find in equal numbers of their clients as much general intelligence, if not more, than is found adhering to the regular profession. History shows that in physic this logic is invalid. All charlatanries have a good proportion of philosophers on their side—philosophers in everything but in medical science. The learned have always patronised innumerable delusions, as they now do spiritual knockings. These, however, meteor-like, pass away, to be replaced by others equally erratic and illusory; while the fundamental principles of medical science, like the fixed stars, have twinkled with more or less brightness, though often obscured by clouds of error, from the days of Hippocrates to the present era. The march of legitimate medicine is onward;—its pathway, illuminated ever and anon by new gushes of light, preludes for the future a more glorious day. No science can boast of discoveries so interesting to humanity. It is sufficient to name the discovery of the circulation—of vaccination—new remedies—new surgical processes—anæsthesia, or pain-destroying agents, and so on—all comparatively recent. It cannot boast, it is true, of a panacea adapted to the ever-varying condition of a single disease;—Charlatans of every era and of every land boast of infallible panaceas for all diseases. The fears and the credulity of the multitude supply the place of authentic evidence.

> "Tincture or syrup, lotion, drop or pill,
> All tempt the sick to trust the lying bill;
> With cruel avarice still they recommend
> More draughts, more syrups, to the journey's end:
> 'I feel it not'—'Then take it every hour:'
> 'It makes me worse;—'Why, then, it shows its power,'
> 'I fear to die;'—'Let not your spirits sink,'
> 'You're always safe while you believe and drink.'"
>
> CRABBE.

It may be doubted whether well informed Homœopathists believe the fundamental, peculiar, and differential principles which distingush Homœopathy from what is reproachfully styled Allopathy. If the homœopathists cured, and the allopathists killed all their patients, the

fundamental characteristics of the former would not be thereby established; for example, the efficacy or potency of infinitesimal divisions of a dose would not be proven, but simply this result—that non-medication is better than any medication. The expectant method of many French pathologists, albeit not homœopathists, would be confirmed—nothing more; least of all, the potency of smallness, and the omnipotency of negation.

On the score of general intelligence, the homœopathists are not, as before admitted, so vastly inferior to the regular profession as many imagine.* On all sides charlatanry is epidemic, and the more it is characterized by talents misapplied, by intelligence misdirected, and by sagacity which never deviates from the lures of self-interest, the more does its treason against humanity acquire a damning potency not found in any of its dynamizations, how much soever they be shaken.

Beyond the confines of the mathematics, the logician could scarcely find topics on which he could found propositions more evidently impossible and contradictory than those which constitute some of the fundamental principles of homœopathy. It may be doubted, as before intimated, whether the professed believers in this system believe in it at all; for sincere belief is not tested so satisfactorily by words as by actions. Many erroneous systems of belief are wholly ignored in practice. Thus it is with the science of phrenology. The lawyers, judges, and juries, never consult the bumps of benevolence and destructiveness, nor amativeness, in the prisoner's cranium, in a case of a suspected murder or infanticide. Victoria, Napoleon, Nicholas, Abdul Medjid, have at this moment armed navies ploughing the Baltic, the Mediterranean, the Ægean and the Black seas—mailed legions glittering in sunbeams and moonlight, converging rapidly upon the Danube, Bulgaria, Wallachia, Moldavia, and the confines of Western Asia, without having examined whether the bump of war or peace is developed under half a million of plumed helmets—whether these countless hosts are hastening to embrace and kiss, or mow each other down like grass. Were phrenology a practical science, it would be of infi-

* Professor Henderson of the University of Edinburgh, the most distinguished homœopathist of the day, in his recent work in answer to his colleague, Professor Simpsom, has the following as a part of the caption of one of his chapters: "*Homœopathy countenanced by great folks and by the clergy.*"

nite use in all the walks of life, and particularly in the science of killing.

Thus the philosophers who, with the learned Bishop Berkeley, believe in the non-existence of matter, and who contend that ideas alone have a positive existence, show that they do believe in fire, water, great guns, grape, cannister, swords, bayonets, smoke, lamp-posts, women, wine and potatoes—they call things by wrong names—belief, unbelief—certainty, doubt—demonstration, deceptive negation—a therapeutical nothingness—an intensified entity—a flat no—a consenting yes; all of which being admitted, it follows most logically that a grain of quinine, morphia, strychnia, or arsenic, becomes more powerful and dangerous the more it is divided, until

"Nothing is
But what is not."

Homœopathy, if legitimately carried out in practice, amounts to this, namely: it has no influence upon the sick except through the imagination. Nature, as all know, will often effect a cure where no drug, or even a hurtful one, is given. That homœopathists often cure their patients, is doubtlessly true. Instead of doses of infinitesimal nothingness, their patients swallow allopathic ones under a wrong name and in a most concentrated form. Pious frauds!

It has been shown, for example, in the cholera of 1849, at Cincinnati—a city in which homœopathy is somewhat endemic—that homœopathists gave "the strongest tincture of camphor in doses of one or two drops every five minutes; and from three to five drops every three minutes—equal to fifteen or twenty grains of camphor every hour."* Now although this may be called infinitesimal dosing, it exceeds the doses directed by the pharmacopœia of the regular profession. Cases nearer home might be mentioned. A letter is at hand, stating that in a Southern State, not far from Louisiana, one of the regulars, lately of high standing, now asks the sick, "will you be treated homœopathically or allopathically?" A gentleman well acquainted with a celebrated homœopathist in an adjoining State, says that the latter puts the same question; but has the goodness to say that homœopathy is his favorite method. Diplomacy can go no farther.

* "The Cholera in Cincinnati," by the Rev. S. A. Latta, M. D., 1850.

Mr. Lee, cited by Dr. Simpson, says that

"A professor of homœopathy in Leipsic, was accustomed to ask his patients by which method they would be treated, as both were equally good."

"The fate of the Duke di Cannizzarro, well known some years ago in London as the Count St. Antonio, having to take homœopathic globules at intervals for some slight ailment, in order to save trouble he took three doses at once, and died two hours afterwards; the supposed globules having been a concentrated preparation of nux vomica." "Dr. Taylor, the distinguished toxicologist and chemist, of Guy's Hospital, found one-third of a grain of morphia in a powder *professedly* homœopathic and infinitesimal."

The following extract from Dr. Simpson's work can scarcely be called amusing: impending death and the unutterable griefs of the sick room, are little adapted to excite merriment:

"The author of the 'Confessions of an Homœopathist,' in referring to the due and adequate drugging of the Hahnemannic doses, amusingly remarks: 'Patients who are skeptical of the truths of homœopathy, from a love of variety, or a hundred other reasons, will consult you. As these persons are inclined to ridicule infinitesimal doses, it is sometimes highly useful to give them powerful doses of various highly concentrated medicines, in globules similar in appearance to all the rest, but consisting of morphia, strychnine, arsenic, corrosive sublimate, and such like: a few of these mingled with your sugar and starch globules, will cause effects to be felt by the skeptic which will quickly overcome his disbelief: he generally makes an excellent patient, and often a good decoy-duck. Never scruple in paralytic cases to give strychnine largely, but never allow it to be supposed that you are giving more at a dose than the one-hundred-thousandth of a grain. This rule may be followed in other complaints with other very active drugs, such as croton oil; but this is one of our profoundest secrets, and must be kept so. Were it known, our wonder-working powers would be reduced in the estimation of the public and the regulars.' "

Quackery is one of the social evils. Indeed, in a free government, it is an inalienable right. Attack quacks, they will make their fortunes—they will cry out, persecution! The public straightway sympathises with them. The law may fix fines and direct imprisonment as punishments in order to deter them from practicing, but in vain, as experience in Louisiana and elsewhere proves. Legislation against quackery is as powerless as logic, learning, and the decalogue.

The optimist, if not Professor Simpson, might find something to admire in homœopathy, inasmuch as it is the most decent, if not the most rational system of quackery extant. Were it honestly carried

out in practice, it would fullfil one of the golden rules of therapeutics often violated, namely: *do no harm;* or, as Moses hath it—"Thou shalt not kill;" though in a violent disease requiring active medication, infinitesimals would *do no good.* Homœopathy ought to be encouraged, provided there is reason to think that it will displace or prevent worse systems. Homœopathy will have its day. It is fashionable; and being fashionable, profitable. It was Byron who said' "cash is virtue." It was Burke who said of the beautiful, it must be small, without angles and so forth. What novelty, charms, philters, and fascinations corruscate from globulistic forms! During the last epidemic in New Orleans, a certain Æsculapian never gave less, but often more than four ounces of castor oil—a dose for an ogre. Some years ago another declared that the way he cured yellow fever, was this: he bled enough to float a 74 gun-ship, and gave calomel enough to load her. Homœopathy is doing good service by opposing such Allopathy as this.

Dr. Simpson comes forth mailed in logic; his good sword gleaming with syllogisms, to chop up homœopathy, little and beautiful as it is. He gives a full exposition of "its tenets—quotes chapter and verse of Hahnemann—homœopathic oaths—doctrines medical and theological—doses, potencies, triturations, shakes, dilutions, attenuations, and globules—showing the enormous potency of an infinitesimal dose taken by simply smelling—detailing how non-medicated masses take from the medicated globules by contact or infection, like small pox or itch, medical potencies of the highest kind—how "a single dry globule imbibed with a high medicinal dynamization, communicates, according to Hahnemann, to 13,500 unmedicated globules, with which it is shaken for five minutes, *medicinal power fully equal to what it possesses itself,* without suffering any diminution of power itself;" and, finally, the Professor offers the calculations made by four or five of the most eminent mathematicians of Edinburgh, based on the statements of the homœopathists, showing that the thirtieth attenuation or decillionth is as one followed by seventy-two ciphers to the primary drop—that the proper dose contended for by the homœopathists for any drug weighing one grain requires, in the fifteenth trituration or potency, a mass equal to sixty-two times the size of the earth. Hahnemann directs a higher potency or division, namely: the thirtieth; a grain

thus attenuated would require a mass as many times larger than the earth as are expressed by the following figures:

61,000,000,000,000,000,000,000,000,000,000.

One grain of a drug dissolved in one and a half tea-spoonfuls of alcohol would, at its thirtieth attenuation, require a mass of alcohol filling up the entire space within the orbit of Neptune—of this, a drop or globule forms a dose.

"Medical jurists and lawyers know of no poison, however strong and powerful, the billionth or decillionth of which would in the least degree affect a man or harm a fly. They tell us that the mere division of their drugs invests them with "tremendous" power, but they could not injure or affect the frailest insect with one of their "tremendous" billionths or decillionths. For what, in fact, is a billionth of a grain? Why, if a simple grain of sulphur were divided, as the homœopathists use it and other drugs, into billionths, and if our common parent, Adam, when called into existence some 6,000 years ago, had then begun swallowing a billionth every second, and if he had been permitted to live up to the present time, doing nothing but swallowing night and day sixty billionths every minute, he would as yet have completed only a small part of his task. It would require him to work and swallow at the same rate for 24,000 years yet to come, in order to finish one single grain of a drug, which has little effect on his present descendants in doses of 25 or 50 grains. Yet the homœopathists believe that a few of the sulphur billionths which Adam would have swallowed during these 30,000 years would cure, forsooth, an attack of jaundice. If we appeal to our standard—*the standard of common sense*—should not men holding principles of such unmitigated ridiculousness be requested to withdraw from this and other medical societies? But a billionth was, after all, only the sixth dilution of a grain. Homœopathists have dilutions far more refined, and doses far more infinitesimal than that. Some amongst them allege that drugs should always be used in their thirtieth dilution, or in decillionths, and Hahnemann averred that even the smell of such a dose is enough. When they speak of the decillionth of a grain they seem little to reckon what a decillionth amounts to. For it is a sum the mere figures of which can scarcely give us any conception of its infinitesimal amount, (viz:

1,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000)

The world is computed to contain at the present time some nine hundred millions of human beings. If all these nine hundred millions of human beings had lived during the past six thousand years, and each of them had swallowed, every moment of their existence, a decillionth of a grain, such as the homœopaths use, they would not, during these six thousand years, have finished one single grain."

These absurdities have been exposed a hundred times in the medi-

cal journals and in the newspapers, not only without retarding, but probably with the effect of accelerating the march of homœopathy. Professor Simpson calls the bad, *bad*. But will his colleague, the professor of the Practice of Physic in the University of Edinburgh, Dr. Henderson, the homœopathist, be converted to the Simpsonian platform under the torture of arithmetical pincers, saws, wedges, racks, wheels, pullies and weights.

Science has its optimists—believers, that all things are for the best—that the millennial glory of perfect knowledge is at hand. But can they account for the prevalence of Mormonism, clairvoyant mesmerism, Silesian hydropathy, infinitesimal homœopathy, spiritual knockings, and ghostly saturnalia, as being indubitable proofs of the superior intelligence and profound researches of this progressive era? Is pure reason as yet popular in any one of the moral, physical and medical sciences? Has it ever been—will it ever be paramount? The Kants and the Newtons, the Bichâts and the Franklins, still belong to the minority.

Quackery always was, is now, and will be prevalent in company with ignorance, credulity, and illusory expectations, inviting arrogance, deception and cupidity to profit by the same, as the great fortunes thus made testify.

Wm. Henderson, M. D., probably the ablest defender of homœopathy that has appeared, in a work just from the press, in reply to his professional colleague, quotes and adopts Hahnemann's grand test of homœopathic treatment, namely, that the medicinal agent which causes a particular disease in the healthy, is the remedy which cures the same disease in the sick. Hahnemann* says:

"Those violent tertian and quotidian fevers which I cured four and six weeks ago by means of a few drops of cinchona tincture, I observed in myself yesterday and to-day almost exactly, after gradually taking, while in perfect health, four drachms of good cinchona bark, by way of experiment."

In the tropical margin of this Republic upon which the wave of homœopathy is now surging, the Jesuit's bark (cinchona) has been used in doses indefinitely large and small, for morning bitters, where

* Hahnemann—born in Upper Saxony, in 1755—died at Paris, in 1843; aged 88.

no disease existed, and for real and imaginary diseases—from debility, sore legs, and anæmia, up to typhoid and yellow fever—the human stomach and intestines have been filled with its active principle, quinine, at the rate of from one to two ounces in twenty-four hours, and yet not a single case of tertian or quotidian ague has resulted which could be attributed to this agent. Now as this is the *experimentum crucis* of homœopathists, it is useless to allude to any weaker test. The friends of Hahnemann declare that he, by

"Trying the peruvian bark upon his own person, was led to the discovery of the homœopathic law—Lond. Cyc., 1849, vii, 270—believing that the mode of operation of all remedial agents was in perfect harmony with this principle. It should be stated that, although these experiments have been often repeated, no uniform results have taken place. And thus the first law of *similia similibus* gleamed upon him."—*Ib.*, vi, 972.

In this, their strongest proof, they fail. The homœopathic world united cannot create in the sick or the healthful a well-defined ague by cinchona, though the latter may be as good as that which was swallowed by the Spanish Viceroy's lady, the Countess de Cinchon, who was cured of a fever by it, at Lima, more than two centuries since. Dr. Henderson seeks to qualify this great test: he says, with *naiveté*,

"That cinchona *does* produce symptoms of fever in man (though not in all) when taken largely in health, is attested by the experience of some dozen of persons on whom Hahnemann proved the drug." (235.)

The Edinburgh professor could easily find many systems which he deems erroneous, and which are attested by more than twelve experiments or witnesses. He might count them by millions. Verily, too much faith is as bad as none in physic. Dr. H. hunts up some northern authorities to prove his faith, namely, that chills and fevers are produced by quinine; of which hallucination he might cure himself by dropping down into the low latitude of New Orleans, where the use as well as the abuse of that medicine is remarkable, and where, during the long inundation, the quantity of water is great, whereby the lower dilutions, as is supposed, might be effectuated conformably to homœopathicity. He could then try the potency of his similars—whether the smell or the swallowing of an infinitesimal dose would cure congestive chills, algid fevers, and pernicious remittents and inter-

mittents. "The smaller doses are more powerful than the larger," as he affirms. One might as well be killed, therefore, by "the larger."

Dr. Henderson calls in requisition his ten years' experiences, from which he furnishes the four meagre examples which follow:

1. "A married woman took the one-hundredth of a drop of the spirit of turpentine for a dose, every four hours; it took five days to remove the morbid state—hematuria. 2. A middle aged lady, long subject to constipation, got the third attenuation of nux vomica—half a drop twice a day—in four days she was suffering from over doses of strychnine; every drop contained only the millionth of a drop of the mother tincture of nux vomica. 3. An old lady was salivated by the sixth attenuation of mercury. 4. One dose of sepia of the thirtieth attenuation produced dangerous effects."

These four cases, making a little over a page, are as unsatisfactory, futile and imperfect as can be imagined. In neither of the last cases does he mention how many drops were given or smelt. In the second case the millionth of a drop of the tincture of nux vomica (the active agent in which is strychnine) proved an overdose and poisonous; dangerous, because so small; regulars give from five to twenty drops for a dose; safe, because so large; equal to one-twelfth or one-sixth of a grain of strychnine. Dr. Henderson's four test-cases are the worst reported ones that have yet appeared in print.

Of this favorite remedy of homœopathists, strychnia, not one of the affiliated fraternity would for the universe take four grains, while no regular physician would hesitate to take himself, or give to his infant child, the awfully potentized dose of the millionth of a drop of the lowest or highest attenuation of the tincture. A few grains of the active principle will kill man, dog or alligator. Medicines are dangerous in large doses, and yet acquire increased potency by infinitesimal subdivision! If this be science, what is absurdity? Latin does not solve this mystery—Similia similibus curantur!

Hahnemann's provings prove, according to Dr. Simpson and others, that in a healthy person lime produces 1090 symptoms of disease—a statement which Dr. Henderson thinks is *probably* too high (270)—taking the lowest number of symptoms, according to his statement, they amount to 181,666; this list is formidable enough. Every individual ought to have at the least all these symptoms who drinks the ordinary hard (lime) waters of the springs, pumps and wells which

are used by more than half of this nation. The carbonate of lime is held in solution in every degree of dilution in the drinking water of the multitude, very generally, in almost all lands, as well as at the most fashionable watering places. The Sans Souci Spring, at Ballston, contains in 231 cubic inches of water, 247 grains of solid matter, nearly one-fifth being carbonate of lime, which, according to Dr. Simpson's reckonings of Hahnemann's provings, produce 1090 distinct symptoms!

But Dr. Henderson boldly assumes that the lime in these waters and in other substances is not absorbed. But if it is not absorbable how can the homœopathists make out their provings; that is, 1090 symptoms? Now, man being "of the earth, earthy," is little more than an animated column of lime, dressed up for a time in "the flesh," which itself contains lime, while his skeleton contains little else. "Lime," says Prof. Carpenter, "is one of the most universally diffused of all mineral bodies, for there are very few animal or vegetable substances in which it does not exist. The principal forms in which it is an element of animal nutrition, are the carbonate and phosphate.

A hen, be it known Dr. *Hen*-derson, that gets lime in no form, lays eggs without hard shells. Prof. Carpenter says:

"That the degree of development of the soft tissues depends in a great degree upon the supply of the carbonate of lime which the animals receive. Thus the mollusca, which inhabit the sea, find in its waters the proportion of that substance which they require; but those dwelling in streams and fresh water lakes, which contain but a small quantity of lime, form very thin shells; whilst the very same species inhabiting lakes, which, from peculiar local causes, contain a large impregnation of calcareous matter, form shells of remarkable thickness. The crustacea, which periodically throw off their calcareous envelope, are enabled to renew it with rapidity by a very curious provision. There is laid up in the walls of their stomachs a considerable supply of calcareous matter, in the form of little concretions, which are commonly known as crabs' eyes. When the shell is thrown off, this matter is taken up by the circulating current, and is thrown out to the surface, mingled with the animal matter of which the shell is composed. This hardens in a day or two, and the new covering is complete. The concretions in the stomach are then found to have disappeared; but they are gradually replaced before the supply of lime they contain is again drawn upon."—*Phys.* § 440, 441.

Prof. Henderson's labored "provings" that lime is not absorbable,

lacks proof. Common table salt, the non-homœopathic world will be astonished to learn, is little better than lime, causing just 450 symptoms in the healthy individual, and consequently cures the same number in a sick one. A list of these, as given by homœopathists, is the craziest that ever emanated from a mad house.

Crabbe (called by Byron "Nature's sternest poet, yet the best") speaks on this wise to charlatans of every grade, and his animadversions are none the less valuable for being "married to immortal verse." Hippocratic prose cannot be truer:

> "Can all the real knowledge ye possess,
> Or those—if such there are—who more than guess,
> Atone for each impostor's wild mistakes,
> And mend the blunders pride or folly makes?
> What thought so wild, what airy dream so light
> That will not prompt a theorist to write?
> What case so prevalent, what proof so strong,
> That will convince him his attempt is wrong?"

[EDITOR.

Rev. II.—*A Treatise on the Diseases of the Eye:* By W. LAWRENCE, F. R. S., Surgeon Extraordinary to the Queen, &c., &c., &c. A New Edition, edited with numerous additions, 243 Illustrations, by Isaac Hayes, M. D., Surgeon to Wills' Hospital, Fellow of the Philadelphia College of Physicians, Member of the American Medical Association, of the Philosophical Society, of the Academy of Natural Sciences of Philadelphia, etc., etc., etc. Philadelphia: Blanchard & Lea. 1854. 8to. pp. 948.

Although several excellent treatises in the English language appeared cotemporaneously with that of Mr. Lawrence, none have surpassed it in popularity, ability and usefulness. Founded on the authors's course of lectures in the London Opthalmic Infirmary and published first in 1833, and republished in Washington the next year—enlarged and revised in a second edition in 1840, it has grown so as to be nearly twice its original size. Its qualitative equals its quantitative contents. The typography, paper and illustrations are excellent, while the editor's name is a guarantee that the contents of the work fullfil the highest behests of opthalmic pathology and practice as they now exist. Although this book is a magnificent monograph; a Cheops among the pyramids of eye literature, it is not good for a review; being too massive; too long known; too much esteemed, and withal, too impregnable. As a material mass, its *vis inertia*, or static force, is too great for the dynamic force of the biceps and other muscles of the arm when

the book is placed at the wrong end of the lever, that is, in the hand. The work would make two volumes without being deficient in ponderosity. The putting of the volumnious works of an author, as Scott, Byron, Addison and others, into one volume, is a violation of the laws of finality or adaptation, and deserves the attention of the Bridgewater authors who adopted this fundamental principle in their speculations on Nature's works.

As the hanging of a man is often carried into effect upon circumstantial testimony, so may the apotheosis of an unread edition of a book like this, seeing the number, character, knowledge and competency of the witnesses who have pronounced favorably upon it. Although time and space are unlimited, a bi-monthly journal is neither; but is the antithesis, or, as Kunt would say, the antinomy of both. Even the title pages of the books which reach this flat margin of the Republic are almost enough to fill the Review department of the New Orleans Medical and Surgical Journal. In Nature (as the experimental philosophers will have finally to admit), the validity of synthetical judgments will be at last recognized, though in reviewing, without reading the work reviewed, such judgments are liable to be erroneous.

In the next edition of this eye-encyclopœdia it is hoped that a life of Mr. Lawrence, that is, his literary life, will be given, so that certain Æsculapians may know how it has happened that this gentleman, whose works on the Natural and Surgical Sciences, of which the eye forms but a part, have been written amid the hurry of a large practice. Indolence and ignorance are often masked under the hurry of business and the immensity of practice, so that there is no time to think, much less to write. Indeed the latter are sought to be placed as incompatible with the actualities of practice. Warren, Gross, Mott, Dunglison, Meigs, Dickson, Stokes, Graves, Watson, Andral, Louis, Velpean, and many more, have been as great in practice as in writing. If the number of physicians in New Orleans be nearly two to every thousand inhabitants; if there be but little sickness except during epidemics, which strike down for a time poor strangers—all of which may be maintained—surely there must be time enough to rival Philadelphia itself in making books, not to mention Medical Journals. At all events, the pretended antagonism between literary labor, logic and learning and practice, is a stroke of policy in which sciolists take

great delight, to the great danger of the unfortunates who swallow their blundering prescriptions.

Passing from this digression back to Lawrence and Hays on the Eye, the preface, by the latter, will close this notice:

"A new edition of Mr. Lawrence's Treatise being demanded," says the editor, Dr. Hays, "and the author having declined to revise it, we have, with his approbation, complied with the request of the publishers to prepare the work for the press. In the performance of this task, we have not felt that we should be justified in omitting any portion of the original, but have endeavored to make such additions as will render it a faithful exponent of the present state of opthalmic pathology and practice. In relation to one subject, however, the theory of inflammation, it has not been deemed necessary to carry out this plan. * * * Among the additions which have been made may be noticed a full account of the recent microscopical investigation into the structure and pathology of the eye; the descriptions of several affections not treated of in the original; an account of the catoptric examination of the eye, and of its employment as a means of diagnosis; a description of recently invented instruments for illuminating the retina, and of some new methods of examing the interior structure of the eye; two hundred and forty-three illustrations, some of them from original drawings, and a very full index.

There have also been introduced in the several chapters on the more important diseases, the results of our experience in regard to their treatment, derived from more than a third of a century's devotion to the subject, during all of which period we have been attached to some public institution for the treatment of the diseases of the eye."—[EDITOR.

Rev. III.—*A Practical Treatise on the Diseases of Children:* By D. FRANCIS CONDIE, M. D.; Secretary of the College of Physicians; Member of the American Medical Association; of the American Philosophical Society, etc. Fourth edition revised and augmented. Philadelphia: Blanchard & Lee. 1853.

"Books, like children," says Crabbe, "when established, have doubtless our parental affection and good wishes; we rejoice to hear that they are doing well, and are received and respected in good company; but it is to manuscripts in the study, as to children in the nursery, that our care, our anxiety, and our tenderness, are principally directed. "Dr. Condie's "*children*" "are doing well"—"are received"—"are respected, and are in good company," and can take care of themselves. "It is with poems" (and books on children as well) "as it is with actions," says Goethe, "it is bad when they have

to be justified," or reviewed after reaching the fourth edition. Let Dr. Condie, therefore, look after his "manuscripts in the study," content with the verdict of the profession which renders a formal review a willful supererogation, and the more so as this last edition is, in its exteriorations and internalities (forgive "the un-English"), much improved—exceeding the first by nearly 100 pages. Wherefore, if the book be a bad one, Dr. Condie must be a great sinner, and cannot justify himself as did Miss * * *, who, having been charged with maternity, excused herself by saying *it was only a very little one.* Dr. Condie is responsible for 732 pages, octavo.—[EDITOR.

Since writing the above the editor's attention was accidentally directed to the following unfinished review of the works of Drs. Meigs and Condie on the diseases of children. It is, without doubt, the last lines penned by a hand that has "lost its cunning," palsied by the touch of death; the last lines traced by lustrous eyes, now shorn of their brightness; the last thoughts transferred to paper from a brain probably nearly as large as Cuvier's, now shrouded in the impenetrable darkness of the tomb; unagitated by the molten waves of passion and those strugglings after the great truths of science which seem always near, but still recede like the rainbow which the youth vainly tries to overtake.

This memorial is copied word for word, omitting only the titles of the books. It ends like his life, in the middle of a period, and apart from its literary merit, will be read with a mournful interest by his friends, with whom he has communed so often through the pages of this journal, now that he has passed to the realms of death:

"These two volumes embrace over fourteen hundred pages on the nature and treatment of the diseases of children; they were written by men of high standing in the profession, and emanated from the same book-making community. We think it a great pity that the two authors, after the French fashion of book-making, did not unite their talents and experience and give us the fruits of their combined labors in one medium sized volume. Howbeit, we must take them as we find them, and compare the one with the other on particular points of practice.

With the "*Hygienic Management of Children,*" begins Dr. Con-

die, and he proceeds to point out the external agencies, both climatic and dietetic, which may tend to modify the sanitary condition of the infantile organism. Of air, temperature, cleanliness, bathing, clothing, food, sleep, exercise, moral treatment, &c., Dr. Condie treats at considerable length, and gives us some excellent rules for the government and preservation of the health of infants.

In childhood, the brain, lungs and bowels are the organs most liable to functional and organic disease; the first, because of its tender structure, its relative bulk and extreme impressibility; the second, because of their extreme susceptibility to external impressions; to climatic changes and the paramount importance of the functions which they are constantly required to fullfil in the work of hæmatosis; the third

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Rev. IV.—*The Transactions of the American Medical Association.* Vol. vi. Philadelphia: Printed for the Association by T. K. and P. G. Collins. 1853. 8vo., pp. 869. Illustrated with plain and colored plates.

The Annual Meeting of the American Medical Association took place at New York on the 3d of May, 1853. Thirty States, Territories, the District of Columbia, the Army, Navy, and the American Medical Society of Paris, were duly represented by delegates.

Dr. Beverly R. Wellford, of Virginia, President of the Association, having delivered his address, received the thanks of the Association for the same, with a request that a copy of it be furnished for publication, which was conceded. Chaste in style, elevated in sentiment, this address aims at a grand finality necessary to the well-being of society, namely, the thorough education of medical men in all that pertains to the art and science of healing; Dr. Wellford wishes to invoke the power of the Legislature as a protection against quackery.

Although it is a long time since the editor of this journal has visited his native State, Virginia, he believes his knowledge of public opinion in that State warrants him in saying that the laws which Dr. Wellford desires, if passed by the Legislature, would soon or late prove a dead letter and a nuisance, as much so as the medical laws of Louisiana have proved; the laws of the latter were all that could be desired; yet in every legal attempt to enforce them in New Orleans, both the law and the regular faculty were defeated, and charlatanry chanted its *Io pæan* in the face of both. Such is public opinion—

and whether right or wrong the effect is all the same. So long as public opinion remains unchanged, all legislation for the purpose of protecting the regular profession and society against charlatans is useless, and to the profession injurious, giving to the latter the odious appearances without any of the benefits of a monopoly. The positive evils of this sort of legislation are not the less real because masked.

The following Reports, exceeding 800 pages, are without exception creditable to their authors, and are published in the order and with the titles following: Medical Education, by Dr. L. Pitcher—14 pages; Medical Literature, by Dr. N. S. Davis—38 pages; The Agency of Refrigeration, produced by upward radiation heat as an exciting cause of disease, by Drs. G. Emerson, I. Hays, and W. S. W. Ruschenberger—14 pages; Results of Surgical Operations in Malignant Diseases, by Dr. S. D. Gross—158 pages; Epidemics of Tennessee and Kentucky, by Drs. W. L. Sutton, T. Lipscomb, E. B. Haskins, F. A. Ramsey, and A. Evans—45 pages; Acute and Chronic Diseases of the neck of the Uterus, by Dr. C. D. Meigs—147 pages;* Typhoidal Fevers, by Drs. H. F. Campbell, and C. F. Quintard; Coxalgia or Hip Disease, by Dr. A. March—64 pages—a monograph of extraordinary merit, founded on an analysis of two hundred morbid specimens from all points of the compass,—whence he concludes, contrary to the common opinion, that spontaneous dislocation of the hip seldom or never takes place as the result of absorption, ulceration, or destruction of the ligaments of the joint and of the acetabulum, and of the contraction of the muscles surrounding the joint. Dr. G. Buck, Surgeon to the New York Hospital, submitted a report "on the Surgical Treatment of Morbid Growths within the Larynx," 26 pages, rich in facts of gloomy import, illustrative of a neglected branch of medical and surgical pathology—including a deplorable case, ultimately fatal, in which the author performed tracheotomy three times, for the removal of intra-tracheal vegetations and polypoid tumors. Dr. H. F. Campbell's paper on the Sympathetic Nerve in Reflex Phenomena, consists of 5 pages. Dr. W. L. Atlee, of Philadelphia, received one of the two prizes offered by the Association. His essay entitled "The Surgical Treatment of certain Fibrous Tumours of the Uterus," (95 pages) is of high prac-

* Prof. Meig's contribution, republished in a book, has been reviewed in this journal by a gentleman of New Orleans.

tical significance with but little speculative tendency; surgical eventualities here replace etiological generalizations—personal experience, historico-pathological induction; it is therefore an unanalyzable essay, but not the less valuable for that reason.

The Prize Essay of Dr. J. Burnett, of Boston, "The Cell—its Physiology, Pathology, and Philosophy, as deduced from original investigations—to which is added its History and Criticism"—a broad title—but an elaborate, though not an analyzable treatise, in which the past and the present, the transcendental and the experimental physiological anatomy, histology, and teleological anatomy—the finalities of development and microscopic provings are converged into a focus of 187 pages. Altogether it appears to be a very able monograph upon the microscopic phenomena illustrative of the higher generalizations of cell-physiology and histological anatomy; but inasmuch as it rises so far above the usual range of professional thought and education, its postulates if erroneous will seldom be detected—if just seldom tested and appreciated, and the more so as at the bed-side of the sick, little good comparatively has yet been achieved; and even in the study many conflicting and unsatisfactory findings must bewilder an understanding addicted to rigid logic unbiased by exaggeration and unseduced by microscopic illusions, although what is already known, preludes a glorious future for scientific histology and pathology.—EDITOR.

Rev. V.—*A Treatise on the Acute and Chronic Diseases of the Neck* of the Uterus—Illustrated with numerous plates, colored and plain: By CHARLES D. MEIGS, M. D., Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College; Member of the American Medical Association, &c., &c. Philadelphia: 1854.

It is a very ungracious task to review a medical book and to do justice to all parties. There are three parties interested, each claiming impartial justice. 1st. The author of the work. 2d. The profession to whom it is to be a guide, and 3d. The public, who are to profit or to suffer, as its doctrines may be good or bad.

When the author is unknown, or without influence, everything he writes is severely scanned by the profession before it is made the bases of action, and the public are in no danger of suffering from any

erroneous doctrines he may advance, or from any mal-practice he may recommend. Not so, however, when the author, like Dr. Meigs, occupies the high position of a Professor in a popular medical school in a great city, renowned for its medical learning; when he has a large practice and a fame so far spread that patients from different States flock to him for medical advice; when he has age and experience on his side, and an unexceptionable character to recommend him; the opinions, doctrines and practice of such an author, whether right or wrong, so far from being scrutinized or examined into, are too apt to be taken upon trust, and to be adopted by that large portion of the profession who bow to authority and do not take the trouble of thinking and examining for themselves. But when a work, written by such an author, has also been *ostensively* approved by the American Medical Association and published in the transactions of that learned body, as Dr. Meigs' treatise on the diseases of the os uteri has been, it becomes doubly potent for good or for evil, because the unthinking class regard the *ostensive* as a *real* approval of the entire Association, or a majority thereof, in all the leading doctrines contained in the book. Whereas, in truth, it should be regarded only in the light of a communication sent to the Association, which was not read or discussed by the Association, but only by a committee appointed to draw it up. The Association could scarcely do otherwise than receive the report of the committee and publish it, as a matter of course, in the transactions of that body. By so doing none of the members had any obligation imposed on them to approve of the doctrines or leading views contained in the report, except those on the committee, consisting in all of but three members, Dr. Yardley, of Philadelphia, and Prof. Channing, of Boston, being the other two. But as our author, in the dedication of the book, takes occasion to deprive them of any credit in writing it, they cannot bear any blame for the thing written. So far, therefore, from the work being, as it might at first sight appear, an embodiment of American medical knowledge on the subject of the diseases of the os uteri, it is truly nothing but the essay of a single individual, our author himself, who alone is responsible for its doctrines and practical teaching. There is no evidence that the American Medical Association, or a single member of that body, has adopted it as a rule of faith and practice.

Having thus unlocked the defences thrown around the work by its connection with the Association, it lies open, like any other individual production, to the shafts of criticism. Yet, after all, so high is the position of our author, that it was difficult to find a person willing to risk his reputation *pro bono publico* by playing the critic or reviewer. The editor of this journal knows how urgently he asked for a review and how positively he was refused, time and again, by the present reviewer, on the ground that he had very serious objections to Prof. Meigs' work; that its author was among his friends, and he would not willingly wound his feelings by stating those objections; that few men have climbed so high in that better philosophy which can bear the friendly probe to be thrust into those tender places that human weakness and vanity may, by chance, have made. At length a circumstance, soon to be mentioned, occurred at the eleventh hour, when the Journal was going to the press, which produced the conviction that the interest of the profession and the public good imperiously demanded a review of our author's work by some one or other, and the same circumstance just alluded to, indicated that the unpleasant duty was allotted to him, and as a duty he performs it. He has played with a good many other subjects, but he has studied the diseases of the uterus.

Before entering on the merits of the work, it is proper to observe, that there is one feature of it very remarkable, considering that it was addressed to a learned body of physicians. A good portion of the book, including most of the pictures, would hardly have been appropriate if addressed to students. Indeed, most students, who have paid any attention to anatomy, or ever dissected a subject, would have their pride wounded in being suspected of ignorance on these anatomical points, which it seems to be the intention of a good portion of the work to teach and to illustrate by plates. Such, for instance, as the natural size, situation and appearance of the uterus and its anatomical relations. If the work was intended for the general reader, or to give popular instruction to those unacquainted with anatomy and physiology, the meaning of those anatomical plates and drawings of the old fashioned speculum would be apparent. But what possible use can they be to a physician or even to a student in the profession of an anatomy? At page 30, our author apologizes for "the want of artistic excellence" in these drawings, but they are

too pretty for the shop, and an apology is necessay for introducing them at all, if the work was not designed for the boudoir.

But to come to the merits of the book. At page 89, our author says: "*No one can deny that prolapsus uteri is a disorder or weakness of the vagina and its ligaments, and that to cure the prolapsus we must cure the vagina first.*" Almost every enlightened physician of Europe and America denies it. It is believed that our author stands almost alone in attributing prolapsus uteri, *as a general rule*, to such a cause. But what are the means he recommends to cure the vagina, accused so unjustly of being the cause of that evil heretofore attributed to the uterus and its ligaments, the laxity of the abdominal muscles and the impaction of the bowels? A hoop or ring pessary. We look in vain through our author's work for any other remedy, and find nothing but an allusion to astringent injections, which are recommended in certain cases. The ring pessary has been long before the profession, and has been found to be useful only in a very limited number of cases, principally such forms of prolapsus as occur in elderly women. At page 93, our author informs us that his attention was first called to the advantages of a ring pessary in the case of "*an aged woman*" by "*a nurse,*" who constructed it of whale-bone. Whale-bone, perhaps, has some advantages over wood, ivory, &c., as being easier introduced, but any other merits are not perceived. But our author claims an improvement in pessaries—"*Made,*" he says, "*of absolutely pure silver cylinders, bent into a circle and gilt with fire gilding.*" He further says, that "*this is perhaps the most perfect of these instruments, yet difficult to use and to obtain*"—(page 97.) Not having told his medical brethren exactly how he applies this instrument, or where it can be obtained, they will be compelled to send their patients to him to profit by its advantages, if it has any over other instruments of the kind. Indeed, one of the reviewer's patients, in whose hands he lately met with the work now under review (and which led him to consent to review it), was seriously contemplating a journey from New Orleans to Philadelphia to obtain one of those rings and have it applied by the Professor himself. She had been told that she had ulceration and hypertrophy of the uterus, causing the pains she complained of and the inability to walk. She quoted the book under review to prove that ulceration of the uterus was one of the rarest complaints imaginable, and not of frequent occurrence, as her physi-

cian said it was. She was satisfied in some measure on being informed that the thing called *moluscum* in Prof. Meigs' work, and mentioned therein as being very common, was the very thing which Bennet and other authors have almost universally agreed to call "*ulceration.*"

The reviewer deems it needless to tell the profession that a great authority of the last century, John Hunter, had had his attention called to this *moluscum* long before its discovery by the Philadelphia Professor. What the latter took to be something like a "*soft moluscum*" and left unexplained for the world to wonder at, John Hunter—great enough without a title—accurately described and explained as a phenomenon attending *ulceration*, viz: "That ulceration consists in a removal of parts back into the system by the action of the absorbents," and "it is evident," says Hunter, "that nature, in order to effect this object, must throw the part to be absorbed into a state which yields to this operation." It was this *softening* under the ulceration process that led the Professor into the error of supposing that he had made a discovery in medicine, and strightway called into requisition his knowledge of the fine arts to illustrate it. He rides the crayon hobby well; but it is plain he let it run away with him, otherwise he would not have given us a book of pictures not needed in the shop and dissecting room, but rather too attractive to the eye of public curiosity.

Our author seems to have but one remedy for all the acute and chronic diseases of the neck of the uterus, and that consists in what he calls "*antiphlogistic contacts with nitrate of silver.*" Strange to say, he gives no very special directions how those contacts are made. He tells how they are *not made.* "*They are not made by destroying the part,*" or "*by producing no effect upon the part.*" The distance between the limits he sets up is so great, that the nearest way to find what the true antiphlogistic contact is, would be to go to Philadelphia. The case represented by a drawing in plate the 15th, we are told at page 74, was treated for months "*by means of nitrate cauterizations destructive as to the hæmatome, but antiphlogistic as to the cervical ring.*" But he immediately afterwards, on the same page, confuses all the ideas his readers may have formed of what he means by "destructive and antiphlogistic contacts," by candidly confessing that the hæmatome or fungus, so far from being destroyed, was not reduced at all, except "perhaps some positive reduction." How the contact with

the crayon, as he calls it, could be destructive to the hæmatome and for the hæmatome to be but little reduced, if reduced at all, is a question he has left unexplained.

Next we have our author's "*nacent polypi,*" as he calls them, introduced to our notice in the text, at pages 75, 76 and 77, and further illustrated among the pictures which make up nearly half the book. He describes them as "*small vivaces or bunches of red cellular tissue, peeping out from the os tincæ, or jutting beyond it, and attached by a delicate pedicle or foot stalk.*" He winds up the history of those cases with some very good remarks on the importance of a correct diagnosis in such affections, yet he fails to come to any diagnosis at all of the cause of these nacent polypi. The assertion that they resemble nacent polypi is no diagnosis. Duparcque, a French writer of high authority, says: "It is not uncommon to find the neck of the uterus studded with vegetations of various forms, volume and consistence. In the greatest number of cases these vegetations are soft and more or less bloody." But Duparcque shrinks from a positive diagnosis, and is rather inclined to the opinion that they arise from some remote venereal taint, and gives several cases, the 45th among the rest, in support of that idea. Hunter regards such excrescences on other parts of the mucous surfaces in the same light. The speculum was not in use in his day. The reviewer has had a longer experience with the speculum than the author under review—it having been introduced in this section of country anterior to its being adopted in Philadelphia. While he and those of the Northern cities, educated in the Puritanical school, were raising their hands in holy horror at the bare mention of such a means of exploring disease, the reviewer had already seized upon the speculum to investigate a variety of diseases; among the rest, the yaws, which appeared on several plantations in the circle of his practice. He long ago satisfied himself that the uterine vegetations the author speaks of, required the deuto-chloride of mercury, guaiacum and diaphoretics to effect a cure. Twisting them off and cauterizing the part would not prevent their return. Mr. Abernethy suspected they originated from distinct poisons. The constitutional treatment would effect a cure, in process of time, without the local, but united the cure was more speedy. That such affections are produced and propagated, aggravated, arrested, and also cured, in obedience to

the laws governing frambœsial affections more than syphilis, there is good reason to believe. Our author's 18th plate exhibits the same disease, but he does not seem to recognize it. Exactly a similar frambœsoid case was sent by the reviewer to Dr. Foster of this city, an expert surgeon, who excised it and applied caustic. It soon began to reappear, but was entirely dissipated, without a resort to caustic or the knife again, by the constitutional treatment for yaws.

In conclusion, it may be mentioned, that in one thing, very remarkable, our author is not singular. It seems to be the besetting sin, not only of him, but of the medical writers generally in our large cities, to have their eyes so steadily fixed *Eastward* on every petty compiler, book-maker and medical journal of London and Paris, as to be perfectly blind to what is going on in this great Continent where their lots have been cast. The idea never seems to have occurred to them that the circumstances which mould diseases into the forms met with in cramped-up London and Paris and their hospitals, do not obtain in wide, open, free America, and consequently the treatment, imported into this country, does not fit our diseases, like imported gloves, shoes and hats fit our hands, feet and heads. Yet no dandy is prouder in sporting his attire of the latest European fashion, or looks with more contempt on his countrymen in homespun, than the class of physicians just mentioned, in straining every new and fashionable plan of treatment, just imported from Europe, on their patients, and live or die, fit or no fit, making them wear it; scorning everything more fitting and appropriate discovered by their own countrymen. Hence our author has made no mention of American physicians West of the Schuylkill, or their method of treating uterine diseases, yet often quoting an old Madame Boivin of Paris. He does not seem to know there lived a man—he lived so far from Philadelphia, in the back woods—away down South in Virginia, and almost as far West as the Blue Ridge Mountain (Dr. John E. Cooke), who not only cured, in a few days, uterine hypertrophy, menorrhagia, prolapsus and leuchorrhœa, arising from visceral obstructions and engorgements of the liver, by a bold use of purgatives that touched the bile and brought away the atra-bilious matter, but taught others the same practice, ex-cathedra, in two medical schools in the Mississippi valley.

He does not seem to be aware of the superior virtues of iodine

over nitrate of silver, directly applied to the hypertrophied cervix in many cases. Nor has he made any mention of that other mode of reducing hypertrophy of the womb, as enlarged spleens are reduced by the *sang-dragon orientale.* He knows how to use Lallemand's port-caustic in making applications to the cervical cavity, but he does not know how to use a better thing—long, strong, fine staple cotton, wound round a flexible probang or gum-elastic male catheter of small size, with a strong wire in it, for the same purpose. His Lallemand's port-caustic tells him nothing of the morbid condition of the part to which it is applied; whereas the cotton gives accurate information of the condition of the parts beyond the sight—being stained with pus, mucous or blood, according to the morbid affection of the canal into which it is introduced. On prosoposcopia, or the art of being able to tell, by the countenance, the diseases of the womb, our author has not given a word of information. It is no very great stretch of medical knowledge for a doctor to be able, in many cases, to tell a woman what ails her, and to give her a minute detail of her symptoms, without her speaking a word—a great saving to her modesty and a key to her confidence. Good overseers can read negroes by their countenance. Hippocrates introduced the art of reading diseases that way—a way which our author has neglected to show. The great Luzenberg of New Orleans had some fine portraits of the expression of countenance of yellow fever in its various stages. Good drawings of the expression of countenance in the various forms of uterine diseases, would be a work which our author is well qualified to perform, and such would be its great utility, that it could not fail to immortalize his name. Nature, as if to spare female delicacy, has kindly stamped their peculiar diseases on their countenance in characters so faint, that none but the practiced eye of the physician can read or even perceive.

Our author uses the old fashioned cylinder, in place of the improved speculum with two or more blades. He objects to the bi-valve, because the folds of the vagina fall in between the blades, obstructing the view. But he surely knows that this objection only applies to elderly persons, or to those whose vital powers have been greatly exhausted by disease, or by the unphilosophical practice of too long confining patients, afflicted with uterine diseases, to a recumbent pos-

ture. In young women, unless the health has been greatly impaired, the walls of the vagina never obtrude themselves between the blades of the instrument. Our author tells us nothing of the use of the speculum in pregnant women, although he doubtless knows that abortion, in many cases, can alone be prevented by applications, made directly to the cervix, through that instrument. He raises no warning voice against ruining the health and spoiling the complexion by tanning the vagina with astringent injections—but sanctions the practice at page 36, in the very cases in which it does the most harm to check the discharge—viz : when there is no apparent disease, and "*the discharge proceeds from the muciparous apparatus*"—omitting to remind his readers that in such cases the discharge is vicarious—a kind effort of Nature to eliminate from the system some effete organized matter which the inactivity of some other organ has failed to throw off. To arrest a vicarious discharge by tanning the vagina into a kind of leather with astringents, must, from necessity, be ruinous to the organ, and to the general health. Besides the leucorrhœas, menorrhagias, prolapsuses, ulcerations and hypertrophies, caused by the disordered states of the hepatic and digestive systems, of which our author said nothing, there is a much greater number of such affections radicated in a defective hæmatosis and a deficient pulmonary exhalation, from an ascetic life, the depressing passions and sedentary habits. Our author does not seem to have dreamed, that when arising from such a cause, they vanish before the improved Willardian theory, or American doctrine of the motive power of the blood, called hæmatokinety, reduced to practice. These are, no doubt, the cases which he has found to be so rebellious to his crayon.

There was method in Dr. Cooke's madness in proposing to cure all female complaints by keeping up an artificial diarrhœa of atrabilious matter; there was method in the madness of Samuel Thompson, that prince of empirics, who tried to accomplish the same thing by a course of steam, lobelia emetics and red pepper. Both astonished the regulars in effecting many remarkable cures, in cases where their treatment had failed—but they were cases wherein the ground work of the uterine ailments rested upon those hepatic derangements and visceral congestions, so rife in hot climates and paludal districts, which European writers on the diseases of the uterus had never taken into account,

because they never saw them. But where is the method in that madness, imported from Europe, which proposes to cure nearly all such complaints by burning the womb with lunar caustic? If our author would visit the Choctaw nation of Indians, he would find that they are ahead of the French and English in a liberal use of the cautery—preferring the *actual* to the *potential*. They apply it outside on the pubis and back and not to the womb itself. European authors are beginning to recommend this practice, without giving the Choctaws any credit, who have used it from time immemorial. They make all kind of *touches* with the remedy, from those "*which destroy the part to those which produce little or no effect upon the part*," including, of course, between the two extremes, our author's "*antiphlogistic touches*." Alas! how long will the imported madness of treating all, or nearly all, the diseases of the os uteri with caustic, be epidemic among the shining lights of the American Medical profession? Can flashy pictures of the Rubens school, drawn from Nature as large as life, and ornamented with a surplusage of red drapery that nature disowns, spin out the day of such a delusion beyond the brief span of delusions in general?

SAMUEL A. CARTWRIGHT, M. D.

New Orleans, April 24, 1854.

Rev. VI.—*On the Subject of Priority in the Medication of the Larynx and Trachea.*—By Horace Green, M. D. New York, 1854.—p. 17.

Professor Horace Green, M. D., of New York, has published a pamphlet asserting his claim of priority in the direct local Medication of the Larynx and Trachea by means of a sponge-probang saturated with the solution of the nitrate of silver, for the treatment of chronic and acute affections of these structures—a postulate which with good reason he desires to warrant and defend against all persons and claims whatsoever, as well on his own account as on the account of his friends, at home and abroad, who have privately acquiesced in, or publicly contended for his priority in this behalf. A great scientific association in Insular Europe takes for its motto—"To discover, not to apply." The opposite extreme, that of rejecting or neglecting discoveries, the application of which cannot be at once ascertained, is no small error. All discoveries have their preludes, which tend to prepare men's minds for their reception. But he who proves, discovers. The critics in both hemispheres, after some delay, having taken the parallax of Dr. Horace Green, agreed with few exceptions, very well in assigning his place among discoverers. But dissenters have lately discovered in the heavens, as they report, Dr. David Green as the fixed star, in place of Dr. Horace Green, who is nothing more than erratic nebulosity.

Dr. H. Green clearly shows how much others had done before him—how little they had left him to do—and how important his further advances have been; while at the same time he shows with what intensity he can hate his opponents—an intensity which in a calmer hour a magnanimous spirit will think of with regret.

It is not intended in this place to reproduce the authority of the competent critics who have given verdicts in his favor, but to give Dr. H. Green's summary of his claims in his own candid words. He says—

"This, then, constitutes a brief history of what has been done in Europe, by those who have employed the local application of caustics, in the treatment of diseases of the air-passages. By this, it will be seen, that no one had succeeded, or claimed to have succeeded, in

passing the sponge-probang, wet with the caustic solution, into the larynx, until after the announcement in my work, published in 1846, that 'it is an operation which, in the treatment of laryngeal disease, I have been in the practice of performing every day for several years.

"Previous to that time, the medication of the larynx and trachea by cauterizations, in the numerous forms of disease of these organs, had only been ventured upon by a few individuals in Europe; and in the practice of these, it was limited to the 'sponging of the back of the throat,' or, at the most, to the application of the solution to the apperture of the glottis, or, by pressure of the sponge, to the discharge of the fluid into the larynx. In this country, so far as I am aware, previous to that time, the employment of caustic solutions to the interior of the larynx and trachea, was 'entirely neglected.' Now, this treatment receives the sanction of, and is employed by, the most eminent men of our profession, not only in my own but in almost every country in Europe. It has not only proved successful in the treatment of follicular disease of the air tubes, and in the ordinary forms of angina, but eminently so in the management of many cases of whooping-cough, and of membranous croup. If there is any honor in the revival and introduction of this practice, *that honor I claim.*"

EDITOR.

Part Fourth.

MEDICAL INTELLIGENCE.

Art. I.—*New kind of Objective for the Microscope: By* Professor J. L. Riddell, M. D., of the University of Louisiana.

At a meeting of the Physico-Medical Society, April 15th, Prof. J. L. Riddell announced that, in accordance with a plan which he had formed several years ago, he was now engaged in the construction of a new kind of objective for the microscope, which seemed to promise superior results. It is simply an elipsoidal or paraboloidal reflector, made of homogenous glass. The following figure was shown as representing a vertical section:

The dot (.) at O shows the position of the focus of a parabola, or one of the foci of a very long elipse. At or near this point the object to be viewed is to be placed. Concentric with this point the lower end of the glass paraboloid is removed by a hemispherical concavity to an extent sufficient to give working focal distance. A and B are sides of the paraboloid. M N is the upper limit of the glass, a

transverse section of the paraboloid, which is either plane or slightly concave.

Rays of light emanating from points near O enter the paraboloid without sensible refraction; they impinge upon A and B and suffer total internal reflection, and then emerge without disturbance at M and N, to be ultimately received by the field glass of the ocular.

Prof. Riddell acknowledges that the mechanical execution of such objectives, would be considered as beset with difficulties. He thought, however, that the methods he had devised, and the means he had carefully provided, would enable him completely to overcome them.

For use in his binocular miscroscope, Prof. R. proposes to make this objective in two vertical halves. With ordinary (not erecting) eye-pieces, the binocular image produced by it will be both erect and orthoscopic.

Art. II.—*Medico-Legal Jurisprudence—Ethnological, Physiological* and Sanitary Observations. (Suggested by a recent trial indicated in a foot note.*)

Sociology,† whether it be considered as a natural or an artificial science, presents no question of deeper import in the South than the paramount one of *status*. The rights of property and the abstractions of philosophy "pale their ineffectual fires" before the stern and inexorable actualities of Race. The right to exist alone takes precedence over that of blood, even in this most democratic Republic. Physiologists may not be able to explain how African blood differs from the Caucasian and the Indian, contaminating both from generation to generation. The great world careth not for the physiologist. The bravest of the sentimental philosophers who ignore this distinction in

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*Fourth District Court of New Orleans—George Pandelly vs. Victor Wiltz, embracing all the testimony adduced and taken during the trial of this interesting case, together with an Appendix, containing testimony taken in a former suit and not presented to the court and jury in this case. Published by John M. Burk, Book-binder and Stationer, No. 113 Chartres street, New Orleans: J. L. Sollée, 137 Chartres street. 1854.

†M. Comte, author of *Philosophie Positive*, the ablest critic, analyst and historian of the Sciences of the present age, uses this term, *Sociology*, fearless of the Academy.

theory, are the first to adopt it in practice, guarding with increasing care against contacts and fusions with an inferior race, setting at naught all its attempts to obtain social and political equality. Whether inferiority of race necessarily carries with it inferiority of social rights, is a question which transcends the legitimate limits of physiology. Inasmuch, however, as Race is a fundamental principle pervading forensic medicine, it becomes one of high import in this relation, while apart from this, it deserves the utmost attention of the physiologist, the natural historian, the comparative anatomist and the ethnologist.

The following remarks have been suggested by the late unparalleled excitement caused by a judicial inquiry instituted in the Fourth District Court of New Orleans, in relation to the status of a prominent and an esteemed citizen. The verdict, generally acquiesced in, not to say desiderated, was very anomalous in character, though favorable to this citizen.*

The testimony, historical, documentary, ecclesiastical, legal, social, traditional and oral, is fraught with physiological, sanitary, medico-legal and ethnological interest, covering a period of one hundred and fifty years. The knowledge which these witnesses had of the Indians is of the greatest value, because intimate and authentic. The number, character, agreement and motives of these affiants will not be considered, only so far as their testimony on a few of its interesting points, incidentally mentioned, relate to the physiological, ethnological and sanitary aspects of humanity for a century and a half upon the shores of the Mississippi. Among these witnesses are generals who "have set squadrons on the field"—statesmen and jurisconsults who have electrified Senates and Courts with their eloquence, and fair ones, who have done still more.

Sanitary Developments.—The ancient population, as the Creoles are usually termed, must strike the eye of the observer as thickly sprinkled with gray heads, indicating advanced age among both whites and blacks. Pass from the streets to the grave yards, and it will appear that the former generation lived, as the inscriptions will prove, to ages scarcely paralleled in modern times. The Pandely trial exhibits inci-

* "We, the Jury, find a verdict in favor of plaintiff, without damages. This verdict indirectly affected an extensive family connection among the oldest, most talented and worthy in the city, and was looked to perhaps as a portentous one to others, and the boldest "held his breath for a time."

dentally the longevity of the ancient population in a still more favorable light, for although the numerous witnesses may be regarded as selected ages in a certain sense, yet they prove, orally and by documents, that their ancestors were, like themselves, long lived, for a century and a half back. Some of these witnesses had never been beyond the limits of Louisiana, others seldom beyond those of the city. Feminine ages, rarely revealed to even the census-taker, were unveiled by the potency of judicial oaths; one confessed to 110 years; twelve children; pronounced "a curse on all laws but such as love hath made," or, to use her own language, she would "not marry her only husband, nor put herself in the yoke for anybody." So spoke the faithful * * *, now well advanced in her second century.

Of twenty witnesses whose ages are deposed to, there was one of 110; one of 93; one of 85; one of 80; one of 77; two of 74; one of 72; one of 70; one of 69; one of 68; one of 66; one of 64; one of 63; two of 62; one of 59; one of 56; two of 54; which afford an average life of 70:6 years—more than "three score and ten." Can the present effeminate, dissipated and fashionable generation promise itself the same patriarchal longevity?

A Creole lady, aged 59, deposed that her grandfather "could read the finest print without spectacles at the age of 98, at which age he died."

Another lady, aged 56, says: "My father at the time of his demise, eight or nine years ago, was more than 110 years of age." One of the most youthful of the witnesses testified that his maternal ancestor conquered an entire century before she departed this life. The documentary evidence, as the church records of burial, indicate great longevity.

The sanitary import of these facts is most interesting and deserving of elucidation. Sanitarians do not seem to be aware, much less do they attempt to explain the great longevity which probably obtains very generally in, and bordering upon the tropics, and which is susceptible of an easy explanation upon a well known physiological principle, namely, that the heat-producing process is impaired in advanced life, and the more so where the winter is bleak, cold and prolonged. Hence the snow of winter is the winding sheet of the hoary head, as the mortuary records of perhaps all countries testify; and it is believed,

even in New Orleans where the winters are mild, that this season proves the most dangerous to the old. In a warm climate where summer is eternal, the demands upon the vital powers for the continued creation of animal heat are less imperious and are less interfered with by external agents. Indeed the latter, in a mild climate, are auxiliaries, not the enemies to the calorifacient function of the old and feeble. New Orleans, often called the Crescent City, from its geographical configuration, deserves a more flattering *sobriquet*, namely, the Centenarian City—the City of the Ancients.

Ethnological Developments—The Hair—Color—Fecundity.—What has become of the populous tribes of Indians (an ethnologist might ask) which not long since swarmed in Louisiana? Mr. Marigny says that the Choctaws, from 1699 to 1785, mustered 8000 warriors. The French were under the necessity of temporizing with them. This interesting ethnological question will be waived in order to take up the testimony which more directly falls within the range of this inquiry.

The short woolly hair of the negro, the long, straight, coarse hair of the Indian, for a century back, as well as at present, was a subject of judicial inquiry, although its microscopic anatomy was not referred to. The terms used were numerous — ondée, crépus, gratiné, &c.

M. Bouligny deposed: Witness has seen a great many mulattoes and mulatresses, issue of negroes and white men, that have many of them flat, straight hair. He notices one instance: if the Court and Jury could see a girl at Mr. Daquin's of this city (her mother is a negress), they would see a perfect image of an Indian girl, she has the hair of a sauvagesse.

The same witness says of another woman: *She has the hair of a negress.* Has not examined the hair of ——— as an amateur, but saw it; she had *negro hair. She had the hair of a mulatresse* (les cheveux d'une mulatresse sont crêpus.) There is but one species of hair of a mulatresse, and this crêpu (*kinky* or *woolly*), with but few exceptions. *Her hair was that of a mulatresse, very* KINKY *or* WOOLLY.

Joaquim Courcelle, aged 77, deposed: Has seen the hair of these people, he could hardly look at them without seeing their hair. The hair of * * * was the hair "d'une femme de sa classe," (*of a woman of her cast,*) that of a mulatresse. Deponent says that her hair was undulating, (*ondée*) that sometimes when she was well combed and dressed, the hair seemed to be more flat, without being more straight on that account; but, when she had her hair in *négligé*, her hair was more undulating, and was raised [higher.] Saw her head *en négligé*, and has seen her sometimes having her hair well combed, but cannot say often.

The following deposition is a puzzling one for the ethnologist who takes his postulates and proofs from book worms:

Pierre Deverges sworn: Says he was born in 1806 in this city. They had in their family two negroes imported from Africa. Their hair was as black as their skin, and straight, a little curled (*ondée.*) They were brother and sister. The brother had his mind deranged, and became notorious from this fact. Witness'

family is an old family of this country. The name of the black African referred to by witness was Célestin. That boy came from the Foular nation, on the coast of Africa. The brother and sister both came from the coast, and had the same features, they both had *noses* such as *negroes* have, to-wit: flat. They had piercing eyes, they were first very intelligent, until one of them, the boy, became deranged. They looked like all negroes, with the solitary exception of the shape of the hair.

A female descendant of the Pawnee Indians, born in New Orleans, aged 110, deposed that Mme. ——— "had very *straight* hair; the last time witness saw her, her hair was *white* and as *straight* as witnesses; (witness exhibits her hair.) When she was young she heard M. called by the name of "*la belle sauvagesse*" (the beautiful Indian); but in those days, when Indians were low in public estimation, those of Indian blood did not relish the name of *Indian*, but preferred to be called and considered as mulattresses or quarteroons. * * * * F. when young had the most beautiful hair witness ever saw; they formed a roll as thick as witness' arm; when undressed, they fell below her waist; they were jet black and straight as a line. * * * Witness was a slave, as Indians were then slaves; Indians being caught in their own country or tribes, were brought here and compelled to labor. Witness served as a slave, because she knew no better, and submitted to power. Witness never left this State; never went further than the Red Church."

A gentleman deposed concerning this female witness, to whom he paid an official visit, "that she opened her dress to let witness see her breast; let down her hair for witness to examine; her hair was very straight and gray; her color was that of a half-breed by a white man; witness *saw her daughter, who was of a brighter complexion*, and was *more like an Indian than her mother*."

These significant facts, confirmed by the Right Rev. Bishop Portier, Gen. Waul and others, here and elsewhere, cannot fail to strike the attention of ethnological physiologists of the different schools.

"Mme. had the color of a *sauvagesse;* she was redder than a mulattress, or a yellow person."

"Great distinction was made about fifty years ago between the Bohemians, Mulattoes, Quarteroons and Negroes."

"She was of a tawny (*basanée*) complexion, like all old mulattresses and Indian women."

A free woman of color, born in New Orleans, aged 72, swore that her father was white, her mother was black and "had a straight

nose." She deposed to a lady's hair, which was "very black, straight, coarse, not curly, not *ondées*." Another she characterized as having high cheek bones, a straight nose—*la sauvagesse*—both regarded as mixed with the Indian blood.

This witness lived with a gentleman fifty years—had seven children, all now living—facts not favorable to hybridity, sterility, and short life among mulattoes.

In the South, whatever infertility may be noticed in mulattresses is owing, not to their whiteness or hybridity, but to their dissoluteness. The same truth applies to the white race of like immorality.

The Right Reverend Bishop Portier deposed that Mme. * * * "had real Indian features, high cheek-bones and a peculiar shape of the forehead; besides, there was something serious, and grave, and an air of melancholy about the face, which is characteristic of the Indian race. The impression was corroborated in witness' mind by the differing complexions of the children in the colored or African race; as the family descend, the first descent is half African and half white; the quarteroon contains one-fourth of the African and three-fourths of the white, and so on. The African blood *disappears*, as it were, in succession of time. It is not so in the mixed white and Indian blood; for the writer observes, and he has known facts to the same effect, that the Indian blood and features will reappear in the third and fourth generation. Witness thinks that the same observation applies to * * *, some being more brown than others, and some having the distinctive Indian features.

It was among the children of * * * that witness observed this distinction. The color of * * * and her children was a clear white color, and Mrs. * * * and her other sisters had the Indian mark.

Witness had known the family of * * * here. The father was a Mexican and had Indian blood, and was keeper of the king of Spain's store in Mobile. The father married a Miss * * * in Mobile, a white person: the son married a Miss * * * of New Orleans, also a white lady: and there is a considerable difference in the color and features of their children; some look like squaws and are very dark, and others are of white color. This family occupied a respectable position in Mobile, and no question ever arose as to their status and position in life. There is nothing more mysterious than the products or offspring of mixed races."

General Planché: In his younger days had many occasions to see Indians, they were much more numerous then, and the complexion of the Indian races differed very much in the different tribes, some were darker than others. Has not noticed that when Indian females get old they become more tawny.

General T. N. Waul, sworn, says that he has had occasion to see the complexion, features, and appearance of various tribes of Indians, particularly of the Southern Indians.

There is a great variety of complexion among them. So far as he has seen the Indians of the South, the Choctaws and Chickasaws are very nearly of the same complexion. The Cherokees are lighter, the Creeks are darker. The Seminoles become much darker, and the color of the skin increases as they approach the sea board. The Karanquois of Texas are the darkest of all. Some of the Mexican Indians, of whom he has seen a few specimens, were as black as African negroes although they had no African blood in them, and their hair was perfectly straight, and their features more prominent than those of the North American Indians—sharp features, high brows. They resemble in appearance as nearly as witness can compare them, a Spaniard or Castilian painted black.

Witness observed the old people in the tribes to be generally darker than the young people.

Has seen officers and soldiers of the Mexican army and the Mexican population generally.

So far as the army is concerned there is every kind of feature and color among the soldiers. So far as the families are concerned, he remembers the family of Castillo, a very respectable one. Some of its members were as black as negroes, others were brighter. They enjoyed all the rights of citizens in the State of Texas, civil and political. The first time witness saw one of the black brothers come to vote, he inquired and found that the question of legitimacy had never been contested.

Deponent heard Bishop Portier's cross-examination with respect to the question submitted to him about the effect produced from the mixture of the Greek race with any other race in this country, and his answer that he never thought of it, and that there is nothing more mysterious than the product or offspring of mixed races.

Answers: Witness also says that he knows of nothing more mysterious than the product or offspring of mixed races; but from his observation of the Indian race the most mysterious thing of all is, that in families said to be descendants from a mixed ancestry, a portion of which is Indian, no matter how remote, children are frequently born, showing in complexion, hair and appearance, the Indian ancestry to a great or greater extent, than the appearance in the ordinary half-breed.

Witness says the families he has known most remote of the Indian race to which he has alluded, claim to be descended from Pocahontas.

Being requested to say how many instances occurred under his own observation, of the Indian blood being returned or shown for a number of generations,

Answers: I have known three or four instances where families that were said to be descended from Indians, and in all of whom there was occasionally some member of the family plainly and distinctly marked with Indian lineaments and complexion. There was invariably a great distinction between the members of the family, and there were always some members that did not retain their resemblance of Indian origin.

Colonel Hamilton Smith, a great admirer of the *morale* of the negro, quotes several authorities to show that some tribes in Dongola and Sennaar have one lumbar vertebra more than the white race! (Nat. Hist. Man. 101.) This statement is not given as one entitled to belief, but to show how deplorably uncertain the anatomy of the black race is. It is but little creditable to the scientific character of the southern portion of this Republic, where Providence has cast the lot of nearly four millions of blacks, that almost nothing indisputably reliable has been published concerning the anatomical, physiological, and pathological peculiarities of a population so extensive. One class of *savans* contend that the negro blood when mixed with the white, soon produces a set of sickly hybrids; another that this intermixture improves the race—making it more intellectual, vigorous and fertile, and long lived. One says that the Indian blood never can be washed out—the Indian conformation reappearing in all its essential features, in the descendants of the mixed red and white races forever, while the negro blood fuses with the white, all its primary elements becoming merged, is completely lost! These enigmas, hypotheses and con-

tradictions might be solved, verified, and reconciled by hosts of witnesses and observations. Men having the wish, the will, and the energy to undertake these investigations, might put these questions, now doubtful, at rest forever; and while thus earning immortality for themselves, confer benefits upon others. The field is American; its cultivation *ought* to be by Americans. Let contagion and quarantine and miasma, and the causes of yellow fever, cholera and the like, rest, being unfruitful; let them be replaced with inquiries more hopeful of satisfactory results.

A private letter which recently reached the latitude of New Orleans from an American functionary to a distant foreign court, expresses a wish to get further proof of the anatomical facts which have been advanced purporting to show that mulattresses have few or no ovarian cells. For if they have been thus spayed by nature, they must be as unproductive as so many animals, spayed by art, and consequently do not belong to the human species at all! Hence, in the same vein, others say they are hybrids! It is surely time that anatomists should look into these matters, and not allow the imagination to run riot, while the facts lie at their door. Although the New Orleans Medical and Surgical Journal is not tinctured with these new-fangled beliefs, it is open to conviction upon evidence of a reliable character.

John Bachman, D. D., of Charleston, one of the ablest of living naturalists, in his controversy with the late eminent Dr. Morton, of Philadelphia, thus addresses the latter—*

> And now I will endeavor to answer the question you have put me—"how long would the mixed breed of mulatto offspring last, were they compelled to marry among themselves?" You answer "not beyond the third or fourth generation." My reply is, they would last till the day of judgment. I have resided in situations where I have possessed the amplest opportunities of observing the fertility of these mulattoes. The males and females are equally prolific. Among individuals with loose morals, they are in this respect characterized by the same tendency that exists in the whites similarly degraded; but, even here, the fertility of the mulatto female is decidedly greater than that of the white woman under the same circumstances. At the moment I am writing, my eye is from time to time directed to a free mulatto carpenter, superintending the building of an adjoining house. I knew his respectable parents before him, and am acquainted with this man's children—they are in color what are usually called light mulattoes—they have for generations past married with those of their own color and grade. This man weighs about one hundred pounds more than either of us—all the brothers, sisters and relatives, have reared large families of children; I doubt, indeed, whether among any of our white inhabitants instances of greater fertility can be produced. Could you favor me with a visit here, and examine some fifty families that I would

* Charleston Medical Journal, September, 1850.

be prepared to point out to you. I am confident you would greatly modify your statement of their dying out after three or four generations, if you did not entirely abandon the ground you have assumed. Although I have seen mulattoes that have arrived at a great age, I am not prepared to say that as a general rule they attain to the age of either of their predecessors. Still they cannot be said to be short lived—they raise large families of children, and I have often supposed that they were even more prolific than the whites. I have seen on an average a greater number of cases of sterility among white females than I have discovered among the mulattoes. Our records are so imperfectly kept, and your theory of repugnance is so little carried into practice, that it is not easy to trace the mulattoes who have regularly intermarried beyond five or six generations, but as there is no greater tendency to sterility in the sixth generation than there was in the first, and as sterility is even rarer among them than in the whites, we are warranted in believing that in this particular they partake of the characteristics of the admixture of the Caucasian with the ancient Huns, who for ages and centuries have continued to increase and multiply as rapidly, and are as healthy and long lived as either of the unmixed races from whom they have originated. Indeed, I have seen the descendants of an admixture of all the five varieties of Blumenbach, and probably one-fourth of the world is now composed of individuals of mixed blood, yet I have never seen any races that evidenced a tendency to sterility. The American Indians, as far as I have been able to ascertain, are less prolific than any others, but the causes of this must be traced not to any intermixture, for such products I have always found more fertile than the native Indians, but to the slavish drudgery of the females, and to the irregular wandering lives of deprivation and suffering to which the tribes are exposed. In the lower animals it is the same—all varieties are prolific with any other variety of their own species."

Without venturing to take ground as to which of these gigantic intellects should have precedence as authorities in the natural history of man, it may be said that he who has descended to the tomb, mourned wherever science is loved, had fewer opportunities of observing the black race than Dr. Bachman, whose home has always been in the hive of the American Africa.*

Dr. Bachman's views of the fertility of the mulattoes are confirmed in New Orleans, where, if anywhere in the Republic, they can be fully tested; because the free colored population of this class is here the most numerous, wealthy, intelligent, and *white*. In fact the *white*, after a few fusions, has, in not a few cases, almost completely, perhaps quite effaced the *black element*, so that the past rests on a few vague expiring rumors, and not on ethnological characteristics. The colored element, thus merged and lost, can boast of a fecundity, and probably health and longevity equal to the pure races of like habits and morals.

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*The sun has its spots. Intellects cast in a gigantic mould sometimes assume an imperial attitude as well as altitude. The following Bachmanian corruscation upon the darker masses of humanity below, is what it is:

"The naturalists of America are composed of a very small band. You (Morton) and another eminently distinguished naturalist and friend, who welcomed me with cordiality, and honored me with his confidence when I was an invalid and a stranger in a foreign land, and whom I love as a brother, *are the only two individuals in our country with whom I would be willing to discuss this subject even in a single line.*"

The disputed doctrine of the unity of the human race is not now under consideration. But, if the doctrine of mulatto-hybridity be an unwarrantable assumption, and, if, on the contrary, the anatomical and physiological characteristics of the black race soon merge themselves in the white, then it follows that the white and black races are less remote from each other; less separated by indestructible barriers, than the Indian and white, provided that the statements made by the witnesses in this trial, and by others, shall be found to be true upon further and more complete investigation.

The *morale* of this judicial drama, the *dénouement* of which was of unutterable import to a large family connection of elevated position, including indirectly one of the most eminent orators of modern times, awakens associations which it were useless to amplify. Let the following picture suffice: "Mme. was now led into court by her son and brought before the jury. She was requested to unloose her hair for the inspection of the court; all agreed in describing the appearance of Mme. as an elderly lady, small in stature, with prominent Indian features, of a copper color, with deep set eyes, a high forehead, high cheek bones and with long straight hair." But it is said justice is blind, and saw not this scene—the unveiled face, the disheveled hair—for forensic examination.

Take another, the gifted gentleman alluded to, the father of nine children, *with an increasing family*, was, as the testimony disclosed, recently at the head of a college having fifty pupils, and in prosperous circumstances; but the *rumor* reduced his pupils to five; his family to distress, and his library to seizure for debt; whereupon the son of genius returned to New Orleans, his native city, and simultaneously with the prolonged trial alluded to in this paper, delivered a course of lectures upon Ancient Literature, in a strain of surpassing eloquence to brilliant auditories! Here is an example of the moral sublime in action that beggars fiction, and is honorable to humanity.

The importance of this trial, as the possible forerunner of others, has been scarcely appreciated. It will be seen by the reader how carefully the present writer has sought to avoid names and intrusions upon the sanctity of the fireside.—[Editor.

Art. III.—*Amputation at the Shoulder Joint.*

THIBODAUX, La., March, 1854.

DR. DOWLER, *Editor New Orleans Medical Journal:*

Sir—It is usual for publication to be made of all important operations in surgery, more especially if the operation be performed in a city, by an aspiring surgeon. And while we are, from the force of obvious circumstances, compelled to admit that a city combines more scientific men than the country, we do not admit that the city has all the science, to the exclusion of the country.

And in proof of this, sir, permit me to say to the lovers of surgical science, through the medium of your valuable journal, that quite recently, in the town of Thibodaux, an operation in surgery was performed which demanded for its accomplishment the highest degree of skill that falls to the lot of any one man to possess. The operation to which we allude is the shoulder joint operation. It was performed with great skill, and equal expedition, by Dr. F. C. Ewing, in the presence of several other physicians. It is an old, and no less true saying, that the highest degree of human accomplishment is only found in the possession of the truly modest. This, sir, holds good in reference to Dr. Ewing. His extreme modesty forbade him to give publicity to his own skill; but neither his scientific attainments nor his modesty have been lost sight of by your friend,

S. E. MCKINLEY.

The above letter is published with the hope that a circumstantial history of the case alluded to may be elicited, and the more so, because rumors have been more or less current of a contradictory character. Dr. McKinley's letter, how accurate soever it may be, gives no particulars, and does not expressly mention whether he was present at the operation.

There can be no danger of damaging any one's "modesty" by doing good in the cause of humanity and truth. Both the patient and the surgeon are interested in knowing what are the chances of success from an operation so formidable, and which has seldom been performed, even on the field of battle, except by Baron Larrey. Dr. Peaslee, of the New York Medical College, who recently performed this amputation, says that only twenty-six cases in all have been

reported in this country. Dr. S. Smith, junior, editor of the New York Journal of Medicine, whose statistical researches on this operation have been extensive and elaborate, says that Dr. John Warren, of Boston, first performed this operation in America in 1781, and that the hospital reports of France show 18 cases and 13 deaths; the British, 28 cases and 11 deaths; the American, 15 cases and 8 deaths.—(N. Y. Jour. Med., Jan. 1853.)

Professor Ferguson remarks—"The celebrated instance recorded by Cheselden, of Wood, the miller, who had his right arm and scapula torn off by machinery, and several of a similar kind which have been seen and recorded by Carmichael, Dorsey and others, have exhibited the wonderful powers of Nature in sustaining life, even after such frightful and extensive mutilations." "It is only within the last half century," says Professor Pancoast, "that amputation at the shoulder joint has been admitted as a regular process of art."—[Ed.

Art. IV.—*Medical Legislation.*

It appears from the last number of the American Journal of Pharmacy, that during the recent session of the Legislature of Virginia a bill was introduced, requiring that the box, bottle, or envelope, containing any nostrum or quack medicine exposed for sale, shall have connected therewith a label, in which the ingredients of such nostrum or quack medicine shall be printed in English. "Five of the Richmond papers were entirely in opposition to the bill, but whether their sympathy with quackery had any connection with their income from quack advertisements," or whether the druggists, who at a regular meeting opposed the bill, as an interference with the profits of their trade, are questions of little importance, because the Ancient Dominion has always allowed quackery unlimited ingress, egress and progress. Such a law, unexceptionable though it be, could not be enforced.—[Editor.

Art. V.—*Legislative Reports on Public Health :* 1854.

A Majority Report and a Minority Report were submitted to the Legislature on this subject; both being characterised by ability, commendable research and zeal, though somewhat different in their conclusions. The limits of this Journal will not allow of a fair analysis of the one or the other. Only portions will be given at present:

That the yellow fever *never* takes its origin or propagates in a pure atmosphere.

That epidemic yellow fever, cholera, influenza, &c., depending on atmospheric constituents for their origin and propagation, *are not contagious*, as the term is generally understood, viz: a disease depending upon a specific poison, generated by, and emanating from a person, and producing its like in another.

That the yellow fever *can be* and *has been* of domestic origin in hundreds of instances, in different localities and periods, during the last hundred years.

That we are convinced that *no quarantine* regulations will have any protecting effect in preserving the health of the city of New Orleans or the State from the spread of epidemic diseases.

That the only possible good to be derived from any sanitary regulations, would be by the appointment of a health officer, to be stationed at the Balize, to go out with the pilots, and while the vessels are coming into the river, to examine as to the existence of ship fever and small pox, and these diseases only. In case of ship fever and small pox, clean the ship, wash the bodies and clothes of the well, and send them on their way rejoicing; the sick to be detained under some cheap, airy shelter until well enough to travel.

We believe no possible benefit can be derived from the simplest or most complete system of quarantine at any other locality, and the process above suggested at the Balize is not put forth to operate as a *system* of quarantine, but for the sole purpose of preventing the herding of the sick and filthy immigrants into the already crowded and ill-ventilated tenements of the poorer classes in New Orleans.

We would suggest—That every street in the city and its suburbs should be paved.

That every lot in the city and suburbs should be graded to a height *above* that of the surrounding side-walks, &c.

Thoroughly drain, by ditching or grading, all the swamp land in the rear of the city, exposing no more swamp land to the action of the sun's rays, by felling the timber (its natural protection), thereby creating a source of malaria equal to the famous Pontine marshes near Rome.

Erect a sufficient number of water works on the river to throw a constant stream of fresh water down every gutter in the city and su-

burbs, at all times when the river is too low to flow the water without the works.

A better construction of the dwellings of the poorer classes, viz: airy and well floored, high and dry above the surface of the earth.

E. E. KITTREDGE,
F. A. WILLIAMSON,
J. A. BRAUD,
CHAS. SEUZENEAU.

From the Majority Report on Public Health, by Messrs. A. Trudeau, R. W. James, B. Olney, R. M. Kearney, and J. S. Williams, of the Legislature of Louisiana (1854), the following statement is taken:

Yellow fever proper cannot be imported and spread *without the existence of an atmospheric predisposition to that disease.* If we cannot prevent yellow fever here—if it be a disease of a strictly local origin—if the cleaning of our streets, the draining of our swamps, and all sanitary measures must fail, we may at all events contract the sphere of its action by depriving it of that infectious character which it took in 1853.

Art. VI.—*Statistics of the Charity Hospital for* 1853.

From an official Legislative document in the State Journal (the Louisiana Courier) of the 29th of April, 1854, the following Statistics of the Charity Hospital for the year 1853, have been translated from the French and condensed:

Total admissions, 13,759—of which number 10,126 were males, and 3,633 females; discharged, 10,733—of which 7,971 were males, and 2,762 females; deaths, 3,164—males 2,426—females 738. In 1852 the admissions were much more numerous, amounting to 18,031, while the deaths (2,098) were about one-third less. The nationalities of 1853 were distributed among these admissions thus: United States 1,306; Foreign countries 12,338; unknown 120. Of foreigners 7,217 were Irish; 2,260 Germans; 843 French; 519 English; 415 Prussians; 195 Swiss; 144 Scotch; 74 Swedes. 68 Danes, &c.

The principal causes of death were, of yellow fever 1,890; (cured of this disease 1,427); diarrhœa 117; dysentery 110; cholera infantum 115; typhus 59; typhoid 57; pernicious intermittent 20; remittent 12; consumption 199; coup de soleil 12—leaving only 573 deaths from all other causes.

The ratio of admissions declined from January to April, and augmented from May to July, then slowly declined to the end of the year.

It is very remarkable, that the ratio of admissions of natives of the United States was not affected by the epidemic, having been, from January to May, 551 for these five months. The next five months gave precisely the same number, 551! During the first-named period of five months, the Irish admissions amounted to 2,750; during the next five months to 3,541.

Births in the Hospital for 1853: boys 61; girls 74; still-born 24: total—159.

Art. VII.—*Sanitary Remarks.*

The salubrity of the cities of the South, including New Orleans, is satisfactory, and demands no special notice. While the New Orleans Medical Journal will conceal nothing, suppress nothing as to the true sanitary condition of the city, how unfavorable soever that may be, it will indulge in no dismal predictions calculated to excite terrorism, for the simple reason that medical science has discovered no data, no precursors upon which to found a reliable calculation whether an epidemic will or will not appear during the current or any other year.

Art. VIII.—*Medical Memoranda.*

Necrology.—M. Roux, the celebrated Surgeon of the Hôtel-Dieu, at Paris, died on the 25th of March last, from the effusion of blood in the brain; aged 75.—(*Revue de Thér.*, April, 1854.)

Fistula in Ano, has been successfully treated by injections of the per chloride of iron, by Dr. A. Miergues.—(*Ib.*)

Headache.—Dr. Murphy thus classifies Headache:

The Anæemic.	The Congestive.
The Rheumatic.	The Periosteal.—(*Lancet.*)

Treatment of Nævus.—Dr. Cumming has treated 8 cases of nævi; of which number 7 have been completely cured, by tartar-emetic plaster; the cicatrices which followed, though large, were not unsightly.—(*Ib.*)

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