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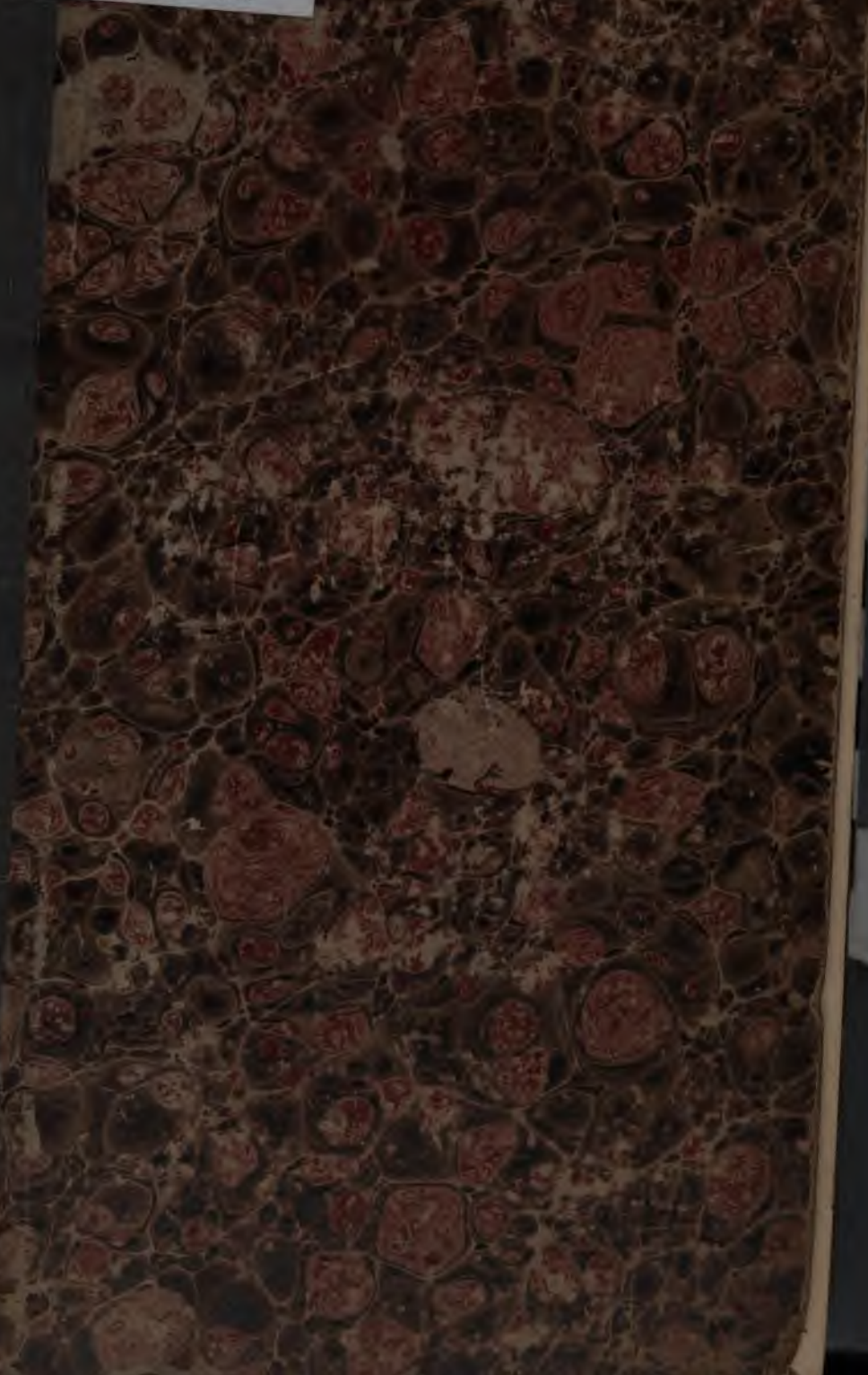
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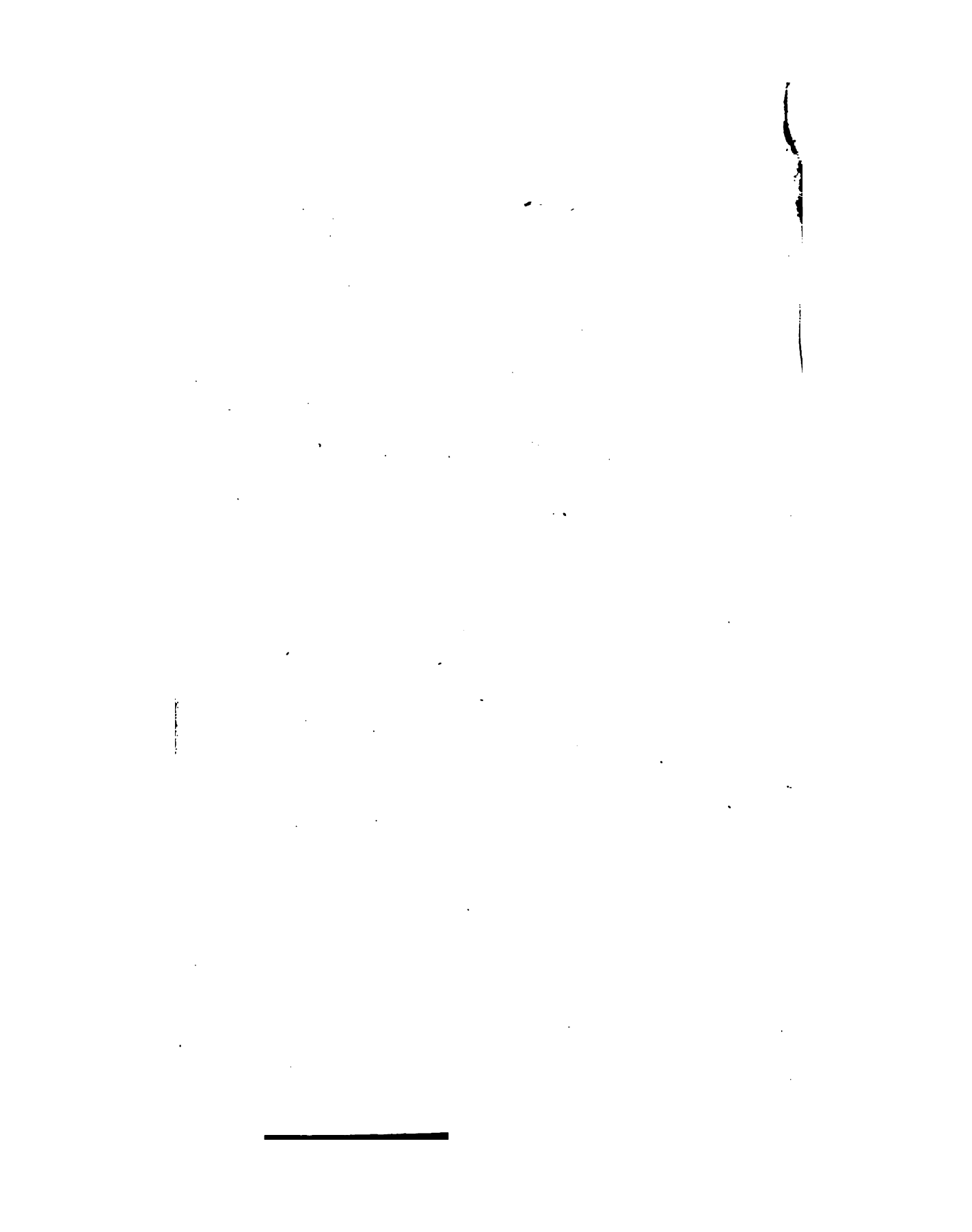
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JUNE, 1850.

NO. 1.

ORIGINAL COMMUNICATIONS.

ART. I.—*Case of Poisoning by Arsenic, and Chemical Examination of the Stomach.* BY GEO. HADLEY, M. D., Professor of Chemistry and Pharmacy in the University of Buffalo.

THE following account of the symptoms and treatment of this case is from the notes of the attending physician, Dr. J. Darling, of Smithport, Pa. It does not contain the circumstances as fully detailed as in the testimony given before the court, but is the same as far as it goes.

Of the testimony, rendered on the trial, we have very little precise information. It appeared that the defendant, Mr. Robbins, was the husband of the deceased; that he lived on bad terms with his wife; that he had purchased arsenic not long before; that he himself prepared the food, on taking which, his wife and another individual became sick, &c., &c. The jury found him guilty on the indictment for poisoning.

Extract from Dr. Darling's Journal

"*Sunday, August 5, 1849.* Saw Mrs. R. 48 hours from the commencement of her illness. Appearances at that time: Pulse variable; tongue moderately coated; countenance flushed and slightly swollen; inextinguishable thirst; intense burning pain at the pit of the stomach; nausea, recurring at intervals of 10 or 15 minutes, with retchings and slight vomiting—the matter vomited has the appearance of water tinged with bile; severe pain in the head; partial fainting and difficult respiration; general restlessness; face covered with cold sweat; icy coldness, at

times particularly, of the extremities, but, for the most part, hot; occasional dimness of vision; bowels costive.

Prescribed a cathartic of calomel and jalap to be followed by salts and senna, and sinapisms to the extremities and scrobiculus cordis.

Monday morning, Aug. 6. Cathartic operated four or five times during the night: vomiting mostly subsided; appearances otherwise somewhat aggravated—pain in the head, thirst and arterial excitement increased; performed V. S. to the extent of about 12 oz., and did not see her again until Wednesday, August 8. Countenance at that time flushed and puffy, and covered with cold, clammy sweat; tongue dry and foul; breath intolerably offensive; pulse nearly imperceptible at the wrist, and irregular; extremities permanently cold; mind delirious; great difficulty of swallowing, even of fluids; says her "throat is growing up"; hands and feet numb; vision much impaired; nails blue; breathing laborious and difficult; occasionally hickup; convulsions at frequent intervals; violent palpitation of the heart; stools dark colored and peculiarly offensive; urine scanty; picks and works at her teeth a good deal; has at times a livid, mottled appearance, and, in one or two places, a separation of the epidermis of an inch or more in extent. Died Thursday morning before daylight."

Examination of the Stomach.—Dr. Darling left with me, on the 13th of August, 1849, to be examined for arsenic, the stomach of the patient, whose case is described above. A little alcohol had been poured into the bottle containing it, to prevent putrefaction. The paper wrappers were taken off, and, without removing the contents, or making any examination, the bottle was carefully sealed, and, in this condition, it remained till Nov. 21, 1849.

At this date the bottle was opened and the stomach removed, and subjected to a careful examination, both as to its condition and contents. The smell was slightly putrid, like that of corpses preserved in alcohol; but putrefaction had not taken place to an extent sufficient to produce any disorganization. The alcohol, in which it was immersed, was very little changed in appearance, or discolored by any matters it had taken. Both orifices of the stomach were tied with a black silk thread, and an inch or two of intestine remained attached to the pylorus.

On opening the stomach no particles of food could be discovered in the interior; no trace of arsenic in substance, or indeed any foreign matter; the only contents being about two ounces of a brown, somewhat slimy fluid, quite homogeneous in consistence. The walls of the stomach retained their ordinary appearance and texture, and presented in no

the slightest mark of lesion, with the single exception of some small, blackish spots scattered over an area of about one by one-half inches.

Preliminary trial—Reinsch's test.—The first trials for arsenic were made on the liquids contained in the bottle—on the alcohol, and on the fluid within the cavity of the stomach. With a view of ascertaining what might be expected in the investigation, the method of testing devised by Reinsch was tried on the latter liquid. A small portion of it was digested with dilute muriatic acid and metallic copper, and the metal afterwards heated in a small glass tube; a parallel experiment being made with the same dilute acid and the metal. The poison, however, was present in too small quantity, or the experiment was tried on too small a scale to afford any satisfactory result.

Method Adopted, and Apparatus.—From this trial, and from the circumstances of the case, it was inferred that the separation of the arsenic by hydrogen, or Marsh's test, in some one or other of its modifications, would be the best, if not the only method applicable.

The gas-bottle, employed for generating the hydrogen, was constructed by taking a half pint salt-mouth and inserting into it, bottom upward, a tincture bottle, whose bottom had been cut off, of the same size, with a tube running down from it to the bottom of the first bottle; the joinings all being made air tight. In the top of the salt-mouth bottle a hole was drilled and a stopcock cemented therein. This was connected with a glass tube a few inches long, filled with dry cotton, to the end of which a capillary tube could be attached by a perforated cork. The method of operating with this apparatus will be obvious without further description. It possesses this advantage over the gas-bottles commonly employed, that it is not necessary to free the liquid tested from organic matter, because the frothy bubbles, which fill the jar, break down entirely, immediately on the concussion occasioned by the first bubble, which escapes through the tube, after the lower bottle becomes filled with gas. The hydrogen may then be drawn off through the stop-cock, being sufficiently dried by passing through the tube of cotton, and may be examined by any of the customary methods.

Reagents, and purity of.—The only reagents employed, in this process, were metallic zinc and dilute sulphuric acid. A few fragments of an old and very pure pig of zinc were thrown into the bottle, and some sulphuric acid, and some rain water, were set aside for the experiments. The gas cooled from these materials when burned in a jet, gave not the slightest

appearance of arsenic, on bringing a cold piece of porcelain down upon the flame.

Contents of the Stomach Examined.—The dilute acid was then poured off, and, the same zinc remaining, a portion of the slimy liquid from the stomach substituted in its place, and sufficient acid added to produce a very moderate action. The gas now evolved burned with a flame like that of pure hydrogen. This was uniformly repeated on lighting. We are not aware that this phenomenon has been remarked before: but at this stage of the investigation no reliance was placed upon it; more especially as not the slightest stain could be obtained from the flame on white porcelain.

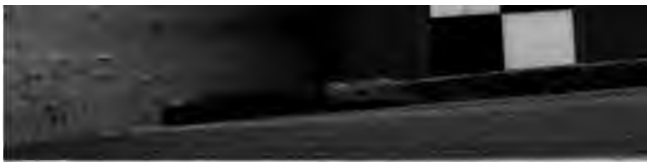
By another Method.—It was evident therefore, either that arsenic was not present, or that its quantity was so small as to require a more delicate process for its detection. The method recommended by Berzelius was adopted. To the tube filled with cotton, connected with the stopcock of the gas-bottle, a small capillary tube of hard glass containing no lead, was attached, in such a way that a portion of it could be kept red hot in the flame of a lamp. When hydrogen containing arsenic is passed through such a tube, the arsenic is separated in the metallic state, and, in consequence of its volatility, is deposited a short distance beyond the point where it is exposed to the heat.

Now on passing the gas through this tube, taking the precaution to keep it cool beyond the heated portion, no stain was observed in the first instance. After several bottles of gas, however, had been passed through it, slight spots made their appearance.

The Alcohol tested.—On substituting in the apparatus the pickle, in which the stomach was preserved, somewhat concentrated by evaporation, and repeating the passage of the gas through the same red hot tube, which was used in the last experiment, this deposit increased so as to form a brown ring around the tube, which, by reflected light, exhibited a slightly metallic appearance. No crystalline structure was detected in it when examined with a glass magnifying more than forty diameters.

The tube containing this crust was removed, and, in order to preserve the sublimate, was carefully sealed with the flame of the blowpipe, a short distance each side of the brown ring.

Brown matter Oxidized.—When heated gently over a lamp, the brown stain immediately disappeared, and, on a colder part of the tube, condensed as a white sublimate; which, by a gentle heat, could be again volatilized, and so driven along the tube condensing unchanged. So small, however,



was its amount, that it was not very evident to the eye, unless it was held against a dark background; when it became perfectly distinct, and appeared like a minute white cloud lining the tube, well defined on the side where the heat had been applied, but more scattered and feeble on the other.

Examined with a Magnifier.—To make the nature of this sublimate more certain, it was examined with a microscope. Under a power of between 50 and 100 diameters, (the microscope is not at hand, so it is impossible to state its power exactly) it was resolved into beautiful and distinct transparent crystals, having a high degree of lustre and brilliancy. Their form was manifestly the regular octahedron.

Not Antimony.—All these reactions are sufficiently distinctive of arsenic. The behaviour of antimony, under the same circumstances, resembles them more nearly than that of any other substance. But, had this been the metal, the white sublimate, consisting of octahedral crystals of oxide of antimony, would not have been so readily or completely volatalizable; and the metal itself, obtained in the first instance, would have been deposited in the heated portion of the tube, and not beyond it.

Source of the Arsenic.—As the reagents employed in these experiments had not been subjected to a test so rigid, it remained still to prove that the source of the arsenic was not in them. To determine this point, the zinc used in the last experiments was washed several times with the acid and water, and the gas then passed, as before, through a red hot tube. All that could be obtained by continuing this process, was, not even a dark stain, but a very slight white sublimate in the place of the brown ring, or rather nearer the flame. On sealing the tube and heating as before, this was found to be readily and completely sublimable; but so small as to be seen with difficulty, and, under the same magnifying power as was used before, presented no appearance, which could be decided positively to be crystalline.

Possibly this feeble sublimate may have been arsenic: but, if it were, it might be supposed to be due to the fluids from the stomach, which, owing to the imperfect washing of the zinc, still may have adhered to the rough surfaces of its fragments.

The gas-bottle was therefore emptied and washed, some new zinc of the same description introduced, and the same acid and water employed as in the former experiments. The passage of the gas through the hot tube was pushed to a great extent, the gas-bottle being emptied above twenty

brownish spots now appeared, scattered along the tube for the space of one inch. Towards the close of the process a little moisture had got into the tube, which was driven out by letting more hydrogen pass through it. On treating this tube as before, there appeared, when seen under a magnifier, a sublimate as of drops of moisture, and which seemed to consist of some deliquescent substance not readily dried by opening the tube and passing dry air through it. The dark spots, which were more feeble than any seen before, were not sublimed by heat, or only with great difficulty, a temperature but little below redness not causing them entirely to disappear. This trial was not regarded as perfectly satisfactory, for the moisture obscured and rendered somewhat uncertain the result.

Method by burning the Hydrogen.—Another method of operating was then adopted. Instead of passing the hydrogen through a red hot tube, it was burned in a small jet, over which a tube was inverted one foot long and $\frac{3}{8}$ of an inch in diameter, so bent as to condense and retain all the moisture, the product of the combustion, with whatever else it might hold in solution. The same zinc that was used in the last experiment, remaining in the bottle and the acid and water as before, the combustion of the gas was continued for two hours, until two or three grammes of water were condensed. This fluid was poured into a watch-glass and allowed to evaporate spontaneously.

The liquids, which had been tested for arsenic in the gas-bottle before, were now introduced again, and the process conducted as in the last, till the gas-bottle had been emptied six times. The condensed water was poured into another watch-glass and set aside to evaporate.

Under a magnifier the latter watch-glass showed considerable crystalline matter—mostly beautiful prismatic needles, sometimes arranged in radiated groups. The incrustation in the first watch-glass presented no crystalline appearance of this distinctness or character.

Tested by Ammonio-nitrate of Silver.—A drop of distilled water was then placed in each glass, and, after the sediment was dissolved, a small portion of a drop of carefully prepared ammonia-nitrate of silver added to each. The contents of the first—that obtained by the combustion of gas from the zinc and acid—turned a little brown with no distinct precipitation or any further change. In the other glass there appeared instantly a lemon-yellow precipitate, exceedingly minute, but distinct and characteristic: like arsenite of silver; and resembling nothing else, except tribasic phosphate of silver, which could not be present under the conditions of the experiment. The precipitate soon became greenish-yellow, and then dark

on standing. The attempt to reduce the arsenic from it to the metallic state failed; probably in consequence of the almost infinitesimal quantity on which the experiment had to be made.

Examination of the Stomach itself.—There remained still the stomach itself to be experimented on; and it seemed desirable to make an examination of that with greater care and precaution than had been used in the preceding experiments. The old lot of sulphuric acid had become exhausted and some new was therefore procured. The gas-bottle was reconstructed, so as to be used more conveniently, and the capillary tube attached to it re-arranged, so that a portion of it, one to one and a half inches long, could be readily kept at a red heat over an alcohol lamp, for an indefinite time; while the part beyond, sheltered from the heat by a perforated screen of brass foil, was carefully kept cool by a bit of moist paper.

Purity of the Reagents.—The first experiment was directed to ascertaining the purity of the reagents; and in this some fragments of common sheet zinc were employed. The passage of the gas was continued for about half an hour, the tube being red hot the whole time. Very soon the tube became brown at the distance of one fourth inch beyond the screen, and, at the close of the experiment, presented a fine iridescent steel-colored ring, brown on the edge farthest from the flame, and the whole about one fourth of an inch in length. The interior diameters of the tubes used in these investigations were between one-sixteenth and one-eighth of an inch. This ring readily disappeared on gently heating it over a lamp, condensing again, at a little distance, as a white sublimate, which, under a magnifier, showed the bright triangular faces of the crystals of arsenious acid. Some of the substances therefore used in this experiment contained arsenic.

The zinc was first rejected, and some new taken, which was supposed to be from the same pig, as that used in previous experiments. On passing the gas from this through the red hot tube, for one hour, a darkish spot made its appearance close to the screen of brass foil. On sealing the tube and heating, this appeared to melt and run together, without subliming. A very slight white cloud was seen, quite distant from the part heated, which on resubliming and examining under the microscope, was found to consist solely of little drops of moisture. In another part of the tube a very few scattered crystals were discovered, which seemed to be prismatic; but it was difficult to determine their form.

A few more pieces of zinc were then added to that already in the bottle,

and acid being supplied from time to time, care was taken to evolve the gas more slowly, and not to pass it so rapidly through the tube. This was continued till the bottle had been emptied eleven or twelve times. The tube, sealed and examined under a magnifier, showed no dark stain. On heating, a very slight cloud, as of moisture, was seen, which on re-heating entirely disappeared, and nothing could be discovered in the whole tube, from one end to the other, when examined carefully with the highest powers of the microscope. These trials were regarded as sufficiently conclusive of the purity of all the materials used in the analysis.

Solution of the Stomach.—About two thirds of the stomach was cut into small pieces, and digested at a gentle heat, in pure distilled water. On straining it, while warm, a solution was obtained which slightly gelatinized on cooling. The gas-bottle being brought to a temperature a little elevated above that of the room, by standing in a basin of warm water, this decoction was introduced, and the process conducted as before, the same zinc remaining in the bottle. Even with the precaution of warming the bottle, the frothy bubbles were so tenacious that it was necessary to evolve the gas with exceeding slowness. The process was continued several hours, the gas-bottle being one-half emptied about ten times, making in all about two and one-half pints, passed through the tube. Beyond the heated portion, the tube became slightly stained very soon, and at the close of the experiment was lined with a brown ring, extending along its bore for the space of one-half inch; but continuous only near the flame, and in the part more distant consisting of scattered spots.

This tube, after being cut off and sealed at both ends, was gently heated on one side for an instant, in the flame of an alcohol lamp. The portion of the brown crust to which the heat was applied, instantly disappeared and immediately condensed again, presenting the appearance of a white cloud lining the tube, which, under a magnifier, was seen to consist of beautiful, lustrous, octahedral crystals.

Repeated.—This experiment was again repeated on the same materials, with the same result. It was also repeated on the same materials, with the addition of other matters before experimented on. The only thing worthy of note in the repetitions was, the apparent diminution in the quantity of arsenic with each successive trial. This result is entirely in accordance with comparative experiments made on liquids, to which minute portions of arsenic had been added.

In all these experiments there has been separated from the stomach and from the fluids in contact with it, a substance which could not be obtained

from the reagents employed in the same process, and was, therefore, not contained in them, or only in proportions too minute to be detected. This substance cannot be other than arsenic.

Not antimony.—Antimony exhibits reactions which bear a closer resemblance to those described above, than any other substance. It is separated from its solutions in the same way, by nitrogen, and is again reduced by a red heat, to the metallic state. On heating this metal, with exposure to the air, it is converted into a white sublimate, oxide of antimony, isodimorphous with arsenous acid, and which consists sometimes of similar, bright, octohedral crystals. This change, however, is not effected at so low a temperature as with arsenic; the sublimation of the oxide of antimony also requires a much greater heat, and is not so entire and complete, because a part of it is apt to undergo a higher degree of oxidation, and in this state ceases to be volatile. But more than this, the metal itself is volatile, and is therefore not deposited beyond the heated portion of the tube, but in it: and the tube is blackened or browned at a different place from what it is if arsenic is present.

These points, conclusive enough in themselves, are still farther confirmed by the yellow precipitate, thrown down by ammonio-nitrate of silver, in the water, obtained from the combustion of the hydrogen. This, in itself, is diagnostic; for no such precipitate could have been obtained if the metal had been antimony.

Conclusion.—These experiments, therefore, were regarded as giving to the conclusion that arsenic was present a high degree of certainty. Had the poison been found in greater quantity, other tests might have been applied, and its existence might have been rendered more evident. Yet the reactions obtained are distinctive, and there is no reason to doubt or hesitate at the conclusion.

Quantity.—The amount of arsenic which was obtained is, indeed, frightfully minute. Trial experiments show that if arsenous acid constitutes but $\frac{2}{100,000}$ of the liquids in the gas bottle, the burning jet gives instantly a brown spot on porcelain, and, if held longer in the flame, the spot becomes bright and metallic, like polished steel. If the arsenic is in the proportion of $\frac{1}{100,000}$ or less, no stain makes its appearance. The proportion, therefore, of the contents of the bottle containing the stomach, which could have consisted of this poison, must have been less than $\frac{2}{100,000}$ or $\frac{1}{500}$ of 1 per cent. Other comparative experiments lead to the conclusion that it must have been in proportion greater than $\frac{1}{1,000,000}$. Suppose it $\frac{1}{500,000}$ and that the stomach and liquids with it weighed $2\frac{1}{2}$ pounds, avordupois,

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about 10,000 grains. This gives for the total amount of arsenic present $\frac{1}{50}$ of a grain! However strange such a conclusion may seem, it agrees well enough with the minuteness of the sublimate obtained in the various experiments.

Source of the Arsenic.—The quantity is so small that perhaps a greater uncertainty may arise as to its source. May it not have been arsenic acid, which, it has been supposed, may replace, in virtue of its isomorphism, the phosphoric acid in the animal structures; especially the phosphate of lime, by arseniate of lime in bone? That this ever takes place, may be regarded as still needing demonstration. It would seem to be an uncommon occurrence at best. The tissues, certainly, in which we might expect to detect it, would not be the soft ones; but those of bony consistence, which are composed, in large part, of phosphate of lime. The origin of the arsenic found here can hardly be ascribed to such a source.

Does the quantity agree with the facts of the case.—It may be more to the point to enquire if the results of the chemical examination are such as might have been expected from the character of the case, as it appeared in the testimony given before the court. The woman took the poison in a hearty meal; vomited constantly for forty-eight hours; and at this time, when first seen by a physician, was retching and vomiting every few minutes; this state of things being accompanied by an "inextinguishable thirst." After this she was cathartacised, and died six days or more from the commencement of her illness. Had arsenic in substance remained in the stomach, she could not, probably, have survived thus long.

Administered mixed with the food, most of it must have been thrown off with the first vomiting. It seems rather surprising that any trace of it should have remained, even in the tissues of the stomach, than that so little should have been found. This, at least, may be safely said, that in a case of such character, the small amount of arsenic detected is what might have been expected, and should be no matter of surprise or suspicion.

ART. II—*Unusual Cases, reported by FRANK H. HAMILTON, Professor of Surgery in the University of Buffalo, and one of the Surgeons to the Buffalo Hospital of the Sisters of Charity.*

Maggots in the Ear.—July 14, 1849. Jonah Harvey, aged 24 years, a brick-maker, called upon me and gave the following account of himself. On the 12 inst., at 2 P. M., a "common fly" got into his ear, and in his attempts to dislodge it, the fly was driven farther in, where it remained about 30 minutes before it was entirely removed. Within 12 hours from this

time, Harvey felt something alive and moving in his ear. On the 13th, 28 hours after the fly was in his ear, several maggots dropped out, each about two lines in length. On the 14th, Dr. Lockwood, of this city, removed two, and at 12 A. M., of the same day, forty-six hours after the occurrence, I removed twenty-one, each one about five lines in length, and very active, so that probably not less than forty maggots were hatched. No more were seen after this. During the time they occupied the meatus, they occasioned constant uneasiness, and sometimes severe pain. Harvey has had a discharge from his ear for many years, which was still continuing at the time of the occurrence, and its odor probably attracted the fly, and induced it to deposit its larvae. When the maggots were removed and carefully deposited in a dry box, they ceased to grow, and died in about 8 or 10 hours.

Curious Affection of the Raphe.—A child, aged 3 years, has phymosis and adhesion of the prepuce to the glans penis. There exists, also along the raphe of the scrotum, extending from an inch in front of the anus to the glans penis, an elevated sinus with transparent walls of about the size of a crow quill, closed at both ends, and nearly filled with a whitish, cream-like fluid, which can be seen to pass from one part to another when pressure is made. I opened the sinus at one point, and expressed some of the fluid. The parents had discovered it some time since, but cannot say whether it was congenital.

Deaf but not Dumb.—Henry Streeter, aged 3 years, Oak street, Buffalo, an intelligent looking child. Until three weeks since he could talk "well and plain." He then had a fever, accompanied with convulsions. He has had a discharge from both his ears since he was one year old, which still continues, and was unabated during the fever. He complained, much of the time during the fever, of his head.

Two weeks have now elapsed since his complete recovery, and he has not spoken a word. He answers promptly and correctly, all questions put to him, even when addressed in a somewhat low tone, but his answers are made only by intelligent signs. He laughs and shouts, but does not articulate sounds.

In the second vol., p. 574 of this Journal, I have reported the case of a child, aged eight years, who was intelligent and healthy, and whose hearing was perfect; she could utter simple sounds, but was unable to articulate more than eight or ten words.

Hair out of Place.—John Armstrong, corner of Eagle and Cedar

streets, Buffalo, aged 38, a stucco worker. First noticed, about two weeks before he called on me, an itching on his foot; on examination, he found a black hair of about the size and color of a cat's "feeler," projecting from the top of the foot, opposite the tarso-phalangeal articulation of the little toe. The thickest part of the hair was outward, and it was about an inch and a quarter in length. He seized it, and attempted to pull it out. He pulled quite hard, until it lifted up the skin and gave him considerable pain, when it broke off about a quarter of an inch above the skin. After this, it began to pain him, and now (spring of 1848,) the whole foot is swollen, red and cedematous, and especially tender about the joint of the little toe. It hurts him much to move the joint."

April 1850, I saw him again, and he says the remainder of the hair worked its way through on the bottom of the foot about six weeks after he called on me; the escape of the hair being preceded by a small pustule. It was about an inch and a quarter in length, and sharpened at the end which first appeared below. The joint, and parts in its neighborhood are still tender.

Armstrong says a cat with black "feelers," he has occasionally found at his feet in bed, and this hair resembled in color, size, &c., the "feelers" of this cat. He believes it was one of them. He is a man in good circumstances, and was wearing at the time, woolen socks and high boots. He is an Englishman by birth, and left England 16 years ago and took up his residence in Georgia, and remained either there or in South Carolina about four years; has seen, and often cut out from others the "filialia," and knows this was not of that character. It had no appearance of a head and it was too firm. Besides it is now 12 years since he left the South, and he has never before had anything of the kind.

Horn on the Face.—Arnold Slocum, aged 50 years, keeper of "Huff's Hotel," in Buffalo; complexion florid; a good liver. His mother was fleshy and florid, and had for twenty years, or more, a brownish, scaly spot on her face, which her friends suspected was cancerous, yet it never became a sore. She died a'et 74. His Father; whom Mr. A. Slocum resembles more than his mother, died at 65, and had for many years a sore on the inside of his lower lip. About 16 years ago, the son, the subject of this notice, discovered a yellowish, branny spot on the side of his nose. It disappeared about 10 years since, but lately it has reappeared. There is also a similar spot at the outer angle of the eye, which came recently. Nine years ago, a spot precisely like this appeared about half an inch below the centre of the right, lower lid, and it remained without material change

until a year since, when he pulled the crust off, and the surface bled. From this time a horn commenced growing, which has attained now 7 lines in length, narrow at its summit, and about 5 lines in breadth at its base. It has the color and firmness of horn. Around the base, the skin was elevated to about one line in breadth and height, and was vascular and sensitive.

To-day, I excised the horn freely by two elliptical incisions. It did not extend beneath the skin. The hæmorrhage was trivial.

ART. III. — *Hydrocephalus Internus. Chronic Diarrhœa.* By CLINTON COLEGROVE M. D.

DR. FLINT: — The following transcript is perhaps hardly interesting enough to merit publication, but trustful that its insertion may help to stimulate a more liberal contribution to your excellent Journal, I put it at your disposal. Its want of commanding points will be pardoned, if it should prove a suggestive impost on some laborious contemporary's dusty diary.

"March 11, 1850 — I have been latterly rather skeptical concerning the advantage of a professional diary, or note book, notwithstanding the almost universal testimony to its utility. I have doubted whether in reality, the record of cases would avail to impress their peculiar features more vividly and lastingly on the mind. And modernly, so numerous are the records and annotations of medical men, it cannot reasonably be expected, except in rare instances, that the medical world will derive particular benefit from the minute labors of their own ranks, since their name is "legion." Yet as an agreeable relaxation in the interims of toil, and as possibly a source of retrospective satisfaction, I will record perhaps the more striking and important cases with whose management, I am entrusted, either directly or incidentally.

I was called to see a child of Perry Hardy, and *en route*, Mr. Hardy, sen., requested me to call and see a sick girl at his house. Immediately I saw her. I concluded her symptoms grave, if not alarming. She had severe pain in her head, extending also to her back. She was continually complaining, uneasy, impatient of interrogation or manipulation, and inclined to somnolency or coma. Pulse 100, small and compressible. Countenance *pale*, though there was perceptible throbbing of the carotid arteries, and synchronous vibration of the head. She had been seized on the Friday or Saturday previous, four days having elapsed since the onset of the attack. The disease was ushered in by vomiting, head-ache, and exceed-

ing irritability of the system. Constitution of the patient, scrofulous; temperament, nervous.

"Shall I bleed?" This question, almost as soon as it occurred, I answered negatively, but, as I afterwards thought, incorrectly. I was dissuaded from the adoption of this measure, from the *paleness* of the countenance, smallness and feebleness of the pulse, temperament of the patient, as well as from a lurking suspicion of the possibility of mistaking an active nervous disorder for inflammation of the brain. How then *did* I diagnose the disease? I answer, I did not *positively* diagnose it at first. But I ventured to pronounce it an attack of accute hydrocephalus. The age of the patient (12) might at first seem to contradict my opinion, but not absolutely. The rapidity of the progress of the case, also would seemingly invalidate this conclusion. Prescribed a dose of calomel and aloes, and small doses of calomel and James' Powder every three hours. The succeeding day revealed a decided change for the worse, and I regarded the case so hopeless as to hesitate about making any prescription.— But, deeming inaction at least culpable, I could not remain idle. Patient was now insensible, although screaming violently. Eyes staring and fixed, pupils dilated, countenance hippocratic, with strabismus. Hydrocephalus internus, is sometimes treated as synonymous with *arachnitis*. The uneasiness and irritability of the patient, conjoined with the violence of the pain, indicated, to me, membranous inflammation. Doubtless effusion of water had now occurred. I observed what is characteristic of hydrocephalus, an obstinate inclination of the head backwards against the pillow, as well as a disposition to *lie flat in bed*. Prescribed blisters to the calves of the legs—also Dover's powder with antimony and calomel in minute doses. If I erred at all in the treatment of this case, it was, I think, in withholding venesection, at my first visit. Inasmuch as this is pronounced the *sine qua non* in cerebral inflammation, I regretted that I did not pursue it, although I question its utility in the present instance. Treatment should have been earlier sought. She sunk at 3 P. M. of this day.

March 21. About two weeks since, I was requested to see Doa. S. Hudson, aged 75, of nervous temperament, and very spare habit. He had long suffered from a harassing cough, although his general health had been usually pretty good. Since August last, he has suffered almost incessantly from diarrhœa. He says he has had repeatedly, 15 or 20 dejections *per diem*. These were of a mucous character. This constant drain had emaciated and reduced him till he scarcely ventured from his house. When I saw him, he complained of anorexia, debility, œdema of the feet

and general *malaise*. In accordance with the recommendation of Dr. Lee, I prescribed Nit. Arg. 3 grs. to the oz., and directed 5 minims to be taken thrice *per diem*, increasing the dose gradually, according to the effect produced. I advised this extreme caution both from the susceptibility to medicinal impressions on the part of the patient, and from an innate suspicion of the insecurity of largely prescribing an article so powerful, aside from the ill-advisedness of too suddenly checking a chronic discharge. It will be seen that patient got $\frac{1}{4}$ th gr. a day. Prescribed also 3 grs. pulvis Dover's, at bed time every day. I heard little of my patient until yesterday,— when calling, I was delighted to learn that the discharge was completely arrested, and a *perfect cure* accomplished, at least so far as such a result could be secured in so short a space. Patient said—"I'm as regular as ever I was in the world." I must confess I did not anticipate a result so auspicious. Appetite considerably improved. I should have said, that for six months last previous, a slight interruption of the accustomed flux, exacerbates his cough, so that they uniformly alternated. But with his diarrhœa, his cough had also abated considerably. In short, I considered this a signal triumph of medicine over disease — and as every body had anticipated the deacon's gradual decline, the sudden disappearance of his disease will be considered, perhaps, marvellous. I am inclined to pronounce a *minuter* dose than the books and journals recommend, the safest and best. Dr. Chapman has recommended its prescription to an extent I should not dare to adopt abruptly.

The first of these cases appears to me interesting as exemplifying the occasional difficulty of discriminating between hydrocephalus and encephalitis. There was an evident intermingling of symptoms proper to both affections, and I cannot help suspecting that this is much oftener the case than from systematic treatises we might infer. Indeed, what would be *a priori*, more natural than to expect the coincidence of membranous and cerebral inflammation in the same case? It also illustrates the embarrassment which may sometimes befall the practitioner, in deciding one point, viz., venesection. The advanced period of the disease, with the circumstances already specified, dissuaded me from adopting a measure Herculean for weal or woe, and about the propriety of which I am still doubtful.

Yours, truly,

C. COLEGROVE.

Sardinia, April, 16, 1850.

ART. IV.—*Anomalous case of Hernia.* BY WM. RING, M. D.

William Bedford, a colored man, came under my observation about the 1st of December, at the Erie county work-house. Age about 60, constitution much impaired by exposure. On examination, I found that he had a strangulated inguinal hernia, about twenty years ago, which was relieved by the ordinary operation, since which time he has worn the common truss. My attention was directed to a tumor on the back and middle part of the thigh, near the size of a large cocoa-nut, with fluctuation on pressure, and moveable, resembling very much an encysted tumor, so far as external appearances were concerned. He complained of pain up and down the thigh, of difficulty in passing water, and general weakness, and these were much aggravated by walking. The tumor made its appearance at the same time as the hernia, was on the same side of the body, and was about as large as a hen's egg. It remained of that size until last fall, when the hernia accidentally became strangulated again, but was relieved by the taxis, and after this the tumor enlarged to the size mentioned above when first examined. He wished me to extirpate it. In consultation with Prof. Hamilton, we concluded to let it remain as it was. From frequent attacks of diarrhœa, he died on the 18th of March, at the poor-house, whither he had been removed a few days before. By the kindness of Dr. John P. Pride, physician to that establishment, I was permitted to make a *post mortem* examination, fifteen hours after death, Dr. Pride being present with me.

On dissection we found that a knuckle of small intestine, a portion of the ileum, had passed out of the cavity of the abdomen, beneath poupart's ligament, over the iliacus internus muscle, thence following the conjoined tendon of the psoas magnus and iliacus internus to the trochanter minor of the os femoris, thence passing under the gluteus maximus directly under the fascia lata of the thigh, where the tumor made its appearance. The bowel, where it passed under poupart's ligament, was on the outside of the femoral vessels, and crural nerve, and two inches from the inguinal hernia outwardly. It passed under the nerve and the femoral artery and vein. The muscular structures were discolored and hardened along the course of the hernia. The intestine had become thickened, and adherent to the adjoining parts, and contracted in its calibre to half an inch, and was filled with the ordinary contents of the bowels. The rectum was also contracted and thickened, and there was a fistula in ano opening on the outside of the anus.

NITRATE OF SILVER IN CROUP.

The points of interest in the case which suggest themselves to my mind are:

First, Its appearing at the same time as the inguinal hernia.

Second, The uncommon situation in which it passed out of the abdomen, — on the outside of the large vessels and nerves of the thigh.

Third, The great distance to which the bowel had protruded, — about thirteen inches.

Fourth, The great length of time the hernia had existed, and life had been sustained with it in that condition.

WILLIAM RING.

BUFFALO, 1850.

Case of Pseudo-membranous laryngitis, (croup,) treated by topical application of the nitrate of silver. BY P. H. STRONG, M. D.

DR. FLINT,—DEAR SIR:

Since you are pleased to regard the following case as of sufficient interest to the profession to merit publication, I do not feel at liberty to decline your request to that end. Herewith you have my notes of the case, mainly as they were jotted down at the time, with little thought of publication.

Wednesday evening, 9½ o'clock, April 10, 1850, I was summoned to see a child of Mrs. M——, North Division street, but resident of Chase county, and just from there. Found her a fair, robust child, of 3½ years, laboring under well marked symptoms of fully developed croup. Pulse rapid, skin burning, slightly livid, respiration frequent, laborious, markedly stridulous, with head thrown back, nostrils compressed, cough characteristically dry and ringing, though perhaps more suffocated and bronchial than in usual form, voice reduced to a whisper. They dated the first of croupal symptoms some five hours anterior to my visit, from exposure to chilling wind,—though she had not appeared quite well, I believe, for some days previous. Having neither of my favorite emetics for such cases at hand, (*Blood root* and *Turpeth mineral*), and the necessity for nausea and emetics being urgent, I gave her Ipecac. and tartar emet. for that purpose. After a thorough vomiting she seemed sensibly relieved in respiration, cough, heat of skin, etc. I left her for the night, upon two grs. of calomel every two hours, with a contingent *Turpeth mineral* emetic upon the least aggravation of symptoms. They thought the emetic indicated in the middle of the night, and gave it with evident relief.

Thursday morning, April 11.—At my visit she seemed decidedly better, with croupal symptoms but trifling; continued calomel every four hours, and directed nauseant and expectorant doses of Inf. Sanguinaria stately, and sufficient of same to vomit upon re-appearance of symptoms, and if she grew worse, to summon me at once.

Friday, April 12.—At my stated visit this morning, there was a marked aggravation of symptoms, in rapidity of pulse, heat and hue of skin, frequent and stridulous respirations, cough, etc., etc. They now reported her as getting worse through the night, but for some unaccountable reason they had failed to apprise me of it.

As the calomel was loosening the bowels undesirably, and the emetic failed of full relief, I felt anxious to try the topical treatment by strong *Nit. Arg.*

Having had neither experience in, nor observation of the treatment, I hastily consulted several medical gentlemen, who were alike inexperienced. Dr. White, having had the nearest to an experience, of any one my time allowed me to see, his advice and assistance were requested, and obligingly rendered.

The bent probang, armed with a fine sponge of some $\frac{3}{8}$ inch diameter when dry, and saturated in solution of crystals of *Nit. Arg.*, Gii to an oz. of water, was applied to the *fauces* and *epiglottis*, and thence passed into the *aperture of the glottis*, without difficulty, and with little distress to the patient. The sponge, when returned, was somewhat coated with tenacious mucus, and the patient vomited slightly of the same immediately. The effect upon the symptoms was not very marked; the stridulous respiration was, perhaps, slightly lessened. Not being fully satisfied, though encouraged by this trial, we thought it advisable to meet on the case again in $1\frac{1}{2}$ hours, and repeat it if indicated. Accordingly we met again at $11\frac{1}{2}$ A. M., and deeming the topical treatment as the only hope for the little sufferer, and there being, at least, no important results from the former trial, we repeated the application, with more than double the former strength, (Div to $i\overline{3}$). The sponge now was more freely coated with *muco-fibrinous* secretion, and more was again ejected by vomiting, with shreds of *albuminous exudation*. The disturbance to the patient by strangling was but slight and momentary, and the relief to the breathing was more palpable than before—though not all we could wish. We concluded to meet again at $3\frac{1}{2}$ o'clock, P. M., and watch the result. At that hour the difficulty in respiration seemed not so referable to the *Rima-glottidis*, as to lower points in the air passages. The characteristic croupal

symptoms might be said to be relieved somewhat, but the energies of the system seemed flagging—it being now some forty-eight hours from the onset of the disease. We viewed the case as less hopeful; but the caustic application suggested to our mind, the *only* hope, and a re-application of the same was tried, with but partial relief. Patient continued through the day and succeeding night, with no change, except a gradual sinking of vital energies, till Saturday, April 13, 9 o'clock, a. m., at which time her symptoms presented in this wise: Pulse very weak and so rapid as not to be counted; respirations thrice their normal frequency, suffocative and stridulous, though this apparently was more traceable to the *larynx* or *trachea* than to the *glottis*. Again the caustic application appeared the only hope, and a forlorn one at that. Promising not to lessen its chance, it was again resorted to, with little relief resulting for the time. Indeed for some five or six hours thereafter, the prognosis seemed so unpromising as almost to forbid persistence in the attempt to administer any thing. For that length of time a fatal termination was hourly looked for. However did persist in recommending a preparation of *sanguinaria* and *lobelia* while she could swallow. She continued apparently failing till night-fall, when she fell asleep, and continued to rest well through the night, her oppressive and stridulous respiration abating hour by hour. Such was their morning report of her.

You, perhaps, Mr. Editor, may imagine, I cannot express, the nature and extent of my surprise, when, at my 9 o'clock morning visit the next day, Sunday 14th, I found my patient neither dead nor dying, as was my expectation, but alive and with a better prospect for continuing so, than she had had since the date of her attack, some three and a half days previous. All her urgent symptoms were effectually mitigated—croupal cough and respiration, cyanosed hue of skin, ebrile excitement, etc. She continued to improve steadily from that time forward to complete restoration to health.

In taking a retrospect of the case, while I would not deny qualified power to the other means resorted to, my conviction was, and is, that the case was hastening to a fatal result, and that not long deferred, till recourse was had to the topical treatment by caustic.

And whilst I would not underrate the inherent recuperative powers of the system, and the *possibility* of an agreeable termination in this case without it, and aware moreover of the evil of deducing too rash conclusions from single cases of results of treatment, still the marked effects that followed the application, as the case presented, suggests it very clearly to my

mind as saving the life of my patient. I am happy in this to have the concurring opinion of the counsel in the case.

One or two points seem worthy of particular notice:

1st. The surprising impunity with which so strong (iv. to v. drach. to the ℥.) a solution of nitrate silver (stronger so far as I know, than Dr. Green or any other advocate of the practice has recommended), was passed through the rima-glottidis and freely applied to the inner surface of the larynx, and that, too, without appreciable inconvenience to the little patient, except at the moment of obstruction of the passage by the sponge.

2nd. The varying effects of the application at the different stages. When the characteristic respiration and cough were plainly referable to obstruction quite at the opening of the larynx, by the adventitious membrane, the relief was immediate and palpable. Later in the disease, when the obstruction seemed lower in the air passages, the relief was not so evident *at once*, but most gratefully so after the lapse of a few hours. This may be accounted for from the intimate nervous sympathy existing between different points of the same membrane—or, as Doctor Green supposes, from the actual contact of the caustic liquid, as it courses its way down the mucous surface. The sponge being saturated, and thus freely imparting its contents upon touching any point, this last opinion seems not irrational.

This case is the more readily furnished you, Mr. Editor, from the conviction with which the present reporter is possessed, that my professional brethren (here at least,) have hitherto failed to consider (as the undersigned confesses to have done) the claim that this practice has upon our regard. This may have obtained measurably from the fact that this locality has hitherto been far less obnoxious to that terrific disease *Croup*, than more easterly localities. However this may be, it now evidently has become of sufficient frequency, and stalks through its course sufficiently defiant of our ordinary means, especially when summoned, (as is usually the case perhaps) only after the disease is fully formed, as to make it excusable, if not imperative, to consider well a more excellent way—if such is commended to us by facts well attested—the results of actual, cautious experience.

Is it said, or oftener thought, "The practice is quite too heroic?" It may be replied, with equal truth, that we have to do with an heroic enemy, and the skillful general is made such, precisely by considering well the position and strength of his foe, and mustering and marshaling his re-

sources accordingly. An advanced stage of a case of croup is eminently a crisis in which, what is well done, must needs be done boldly and quickly. No one can have felt more distrustful of the recommendation, when first broached, of freely introducing a concentrated solution of lunar-caustic directly into the larynx of a child, nor more shrinking and fearful at my first trial of it, than the present writer. Still, I could not shut my eyes, neither methinks can any one, to the array of evidence in its favor; my own being only an isolated case added to a multitude that are recorded.

I may be mistaken in the assumption that it is not practised by my professional brethren in this city. If so, my object in reporting this will be gained by securing, if possible, the report of such cases as may exist. And if not mistaken, my reward will be full, to have commended to the practical consideration, and to have attracted the attention of the profession to what appears an important resource in a real exigency.

I shall not soon forget my anxiety, when my patient was almost at death's door by suffocation, to find some medical friend who could say *he had tried it*, to relieve measurably that trepidation which we are apt to feel, at first trial of (to us) untried and bold expedient.

In the hope that the case will fulfil your expectations in subserving the interest of our profession, I subscribe myself,

Yours respectfully,

P. H. STRONG.

BUFFALO, April 1850.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Remarks on the Comparative Value of the defferent Anæsthetic Agents.

By GEORGE HAYWRAD, M. D., one of the surgeons to the Massachusetts General Hospital.

THIS is the title of a small pamphlet republished from the Boston Medical and Surgical Journal. We transfer to our columns all that relates to the subject practically.

*1st. Of Sulphuric Ether.**

When sulphuric ether was first administered by inhalation, it was by means of a pretty formidable-looking and expensive apparatus. Various instruments for this purpose were constructed, both in this country and Europe. The same objections applied to all of them. They were so formed as to create a well-founded apprehension that the supply of atmospheric air would not in every case be sufficient. It was difficult to guard against this; and from this cause, some patients, soon after the discovery was made, nearly lost their lives by asphyxia.

Besides, to use them with entire success required, in a greater or less degree, the coöperation of the individual to whom the ether was administered. This of course could not always be had, and the consequence was that very frequently a sufficient degree of insensibility was not produced, and even when it was, it could not be kept up as long as in many cases was desirable.

The cost of the apparatus, too, was a serious objection, though a vastly less important one than either of the others that I have named. At the same time it was so great, that if some simpler and less expensive mode of administering ether had not been found, it may well be doubted whether the benefits of the discovery would have been as rapidly and extensively diffused as they have been.

But all these objections are entirely obviated by the use of a bell-shaped sponge of fine texture. This should be large enough to cover the nose and mouth. The patient is required to do nothing. The apparatus is simple and not costly.

This mode was adopted at the Massachusetts Hospital in a few months after the first use of ether there by inhalation; I am not aware that it was previously used anywhere else, and I presume that it is now the only method by which ether is inhaled.

The quantity necessary to produce the desired effect must vary in different cases. In surgical operations, requiring from five to ten minutes for their performance, from three to six ounces is usually sufficient. The ether, however, should be of the purest kind, that is the rectified, which has undergone a second distillation, by means of which it parts with a considerable portion of its alcohol. Yet a much greater quantity than what has been named can be used with perfect safety, and the patient may be kept for a much longer time under its influence without danger, by occasionally removing the sponge, and re-applying it when he gives signs of returning sensibility.

By administering it gradually, many unpleasant effects are avoided. The great irritation of the larynx and air passages, accompanied by urgent and convulsive cough, is in most cases entirely prevented. The vapor of the ether should be so mixed with atmospheric air, that respiration should be neither laborious nor painful. The irritability of the parts with which the ether comes in contact is by degrees overcome, and then the sponge may be applied directly to the face, and if necessary compressed in some measure so as to exclude to a greater degree the atmospheric air. When the desired effect is produced, which is usually in from three to five minutes, the patient has no control over the voluntary muscles; he cannot speak; he cannot open his eyes, when directed to do so; his muscles become completely relaxed, and the pulse, which at the beginning of the inhalation is frequent and often rises during the process to 140 beats in a minute or more, become slower, and I have very often known it to fall to 60. The patient is then insensible and unconscious, and the surgeon may begin his operation with great confidence that he will inflict no suffering. The sponge should then be removed, and reapplied from time to time as circumstances may require. If the ether is not pure, longer time is necessary to produce the desired effect; the brain and nervous system are more excited, and the patient is occasionally violent for a time and with difficulty controlled.

Before using the ether the sponge should be dipped in warm water, and then strongly compressed, leaving it slightly damp. The evaporation seems to go on better in this way than when a sponge is used that has not been previously moistened. In the first instance, the ether should be poured on the inside of the sponge; about two ounces is enough; when more is required, it should be applied to the outside, as it is best not to remove the sponge from the face.

Sulphuric ether of a proper quality used in this way, I am confident, is perfectly safe, and will in almost every instance produce the desired effect. I have administered it to persons of all ages, of every variety of constitution, and in almost every state of the system, and I have never known in a single instance a fatal or alarming result. I have given it to infants of seven weeks old, and to individuals of 75 years, with entire success. I have administered it to persons suffering under chronic pulmonary disease, not only without injury, but in some cases with decided benefit. It is well known that it often gives relief in catarrhal affections of the lungs and in paroxysms of asthma. In fact, I hardly know a state of the system in which I should be deterred from using it, if I were called upon to perform a surgical operation.

The advantages, then, of sulphuric ether as an anæsthetic agent, are its

entire safety, the ease with which it is administered, and the slight inconvenience which follows its administration. I have already stated that I have never known its inhalation followed by a fatal or alarming effect, and there is reason to doubt whether death has in a single instance been produced by it, when it has been properly administered. One patient is said to have lost his life by its inhalation at the Hospital in Auxerre, in France. This took place in August, 1847. The details of the case are not given with such minuteness as to enable any one to form a satisfactory opinion. It occurred, however, not long after the discovery; before the best mode of exhibiting it was adopted, and the *post-mortem* appearances indicated, as far as any opinion could be formed from them, that death was caused by asphyxia. In a careful examination of some of the leading medical journals of Europe and this country, published during the last three years, I have not been able to find another case in which life was destroyed by the inhalation of sulphuric ether, and there is reason to believe, as I have already intimated, that death would not have taken place in this instance, if the lungs had been abundantly supplied with atmospheric air. It is only wonderful that an agent of such power, used as it often has been in the most reckless manner, by unskilful and ignorant persons, should not have caused far more disastrous results, than any that have hitherto been made known. It teaches us that though it should be used with caution and confided only to skilful hands, the dangers from its use are far less than our preconceived opinions had led us to believe.

The great ease with which it can be administered is not to be overlooked in estimating its advantages. No complicated apparatus is required, and no coöperation of the patient is necessary. A simple sponge, moistened with sulphuric ether and held before the face for two or three minutes, will in almost every instance produce the desired effect.

There are no ill consequences from its use. If it be breathed only for a short time, its effects usually pass off in a few minutes. I have never known them to continue for more than an hour; and in this case the patient had been kept under its influence for forty-five minutes. Nausea and vomiting are not frequent, unless it is inhaled soon after food has been taken. I have not seen convulsions follow its exhibition, nor any delirium, except a slight and transitory kind, such as arises from intoxicating liquors. I confess that I was much surprised to learn, by carefully watching its effects, to what a small extent and for how short a time it disturbed the functions of the nervous system, and how rare it was to find headache among the consequences of its inhalation.

If, however, the state of narcotism should continue longer than is necessary for the purposes for which it was produced, the means that seem to me the most likely to remove it, are the dashing of cold water in the face; the application of strong stimulants, as the carbonate of ammonia, to the nose; and, as soon as the patient can swallow, the administration of a small quantity of hot spirit and water. The object is to increase the action of the heart, so that the blood may circulate more rapidly through the lungs, and thus be enabled to part with the vapor of the ether that is mixed with it. When narcotism arises from any noxious substance taken into the stomach, we adopt means to empty that organ as soon as possible by the stomach pump or an emetic. The principle of the treatment in the two cases is the same; the object being in both to remove the cause

of the peculiar state of the system under which the patient is laboring.

The only objections of which I am aware to sulphuric ether as an anæsthetic agent, are its pungent odor, which is offensive to some persons, and the no inconsiderable degree of irritation which its inhalation occasionally produces in the air passages. This irritation, I am confident, may be in great measure prevented by proper attention to the mode of its exhibition and the quality of the article used. Admitting these objections to be as great as they have been said to be by those who have urged them with the most earnestness, they do not in my opinion counterbalance the advantages; and I have no hesitation in saying that I should give it the preference over any other article with which I am acquainted, that is used for the purpose of producing insensibility.

2d. *Of Chloroform.*

Chloroform is the perchlorid of formyle, the radicle of formic acid. It has been ascertained by Dumas to consist of three parts of chlorine to one of the bi-carburet of hydrogen [formyl^a]. It was discovered almost simultaneously nearly twenty years since in France, Germany, and this country.

It was first employed as an anæsthetic agent by Professor Simpson, of Edinburgh, and he thought that it possessed "various important advantages" over sulphuric ether. He says that "it is far more portable; more manageable and powerful; more agreeable to inhale; is less exciting than ether; and gives us far greater control and command over the superinduction of the anæsthetic state." If all this were true, it would no doubt be preferable to any other agent with which we are acquainted. But subsequent experience proves that it is not so.

Its only advantages are that it is more agreeable to inhale than ether, and that a less quantity of it answers the purpose. On the other hand, it cannot be denied that fatal effects have followed its inhalation in several instances even when administered by the most judicious hands; that in some cases convulsions have been produced, and in others a great disturbance of the brain causing delirium. In some persons this affection of the mind has continued for several weeks.

There are other objections of a minor character. Chloroform is of an acrid, caustic nature, and if it come in contact with the skin, unless it be protected by some oily substance, severe excoriation is the consequence. Its administration is generally followed by vomiting and headache, which continues for several hours, attended by a great degree of restlessness and want of sleep. Several cases have come under my care, in which the brain and nervous system have been affected to an alarming extent; though in every instance, it was said that a small quantity only of chloroform was administered for the purpose of performing some operation on the teeth.

An individual in this vicinity was thrown into violent convulsions, which continued for three or four days, during all which time she was in a state of complete insensibility, from the inhalation of the vapor of a few drops of chloroform administered by a careful and judicious physician. It would be easy to multiply examples of this kind; but it is not necessary, for there is a stronger ground on which we can rest our opposition to the use of chloroform, that is, its danger to life. This, it is well known, has

already been in several instances destroyed by it. If it can be shown that it has caused the death of a single individual, when properly administered, we cannot fail to have our misgivings of the safety of its exhibition, though it may have been inhaled in almost numberless cases without any ill effect.

I am satisfied that there are already on record at least twenty well-authenticated cases of death from the inhalation of chloroform; and I know not how a conscientious man, knowing this fact, can willingly take the responsibility and expose his patient to this fearful result. One of the conclusions to which M. Malgaigne arrives, in his report on chloroform, to the Academy of Medicine of Paris, cannot be too strongly impressed on the minds of those who feel inclined to use it. "Chloroform possesses a toxic action peculiar to itself, which has been taken advantage of in medicine by arresting it at the period of insensibility, which action, however, may, by being too much prolonged, cause immediate death." The danger is that we cannot always know the precise time to arrest it, and that the fatal blow may be struck before we make the attempt. In other words, chloroform is a poison, and the insensibility which it produces is only the first stage of its poisonous action.

3d. *Of Chloric Ether.*

There are two kinds of chloric ether. The one, the strong or concentrated; and the other, the chloric ether of commerce. They are both tinctures of chloroform, differing from each other only in the relative proportions of the alcohol and chloroform of which they are composed. The concentrated consists of one part of chloroform to nine parts of alcohol; and in the chloric ether of commerce, there is one part of chloroform to fifteen of alcohol. The former is the one that is sometimes used for inhalation.

It is said to have been first recommended for this purpose by one of the most eminent surgeons of Great Britain, William Lawrence, Esq., of London; but I cannot learn that it is now employed in Europe to any extent in this way. In fact, it is hardly spoken of at all in the foreign medical journals that I have seen, and I have examined a large number with this view. It has been tried, however, pretty extensively by Dr. J. C. Warren and Dr. J. Mason Warren, both at the Hospital and in private practice, and I am not aware that any ill effects have followed its use. On the contrary, I believe that they are well satisfied with it, and prefer it to the other anæsthetic agents.

At the same time it cannot be denied that it derives its power of producing insensibility from the chloroform it contains; and it is difficult to understand how the addition of alcohol can deprive it of its dangerous properties, when it well known that the mixture of this substance with sulphuric ether renders it in a great measure unfit for inhalation.

The advantages which it is said to possess are, that its odor is less pungent and disagreeable than that of sulphuric ether, and that it can be inhaled with little or no inconvenience. At the same time it must be admitted that it is necessary to use as much chloric as sulphuric ether, and to continue the inhalation for as long a time to produce the desired effect.

The disadvantages are, that when it comes in contact with the unprotected skin it acts upon it in the same manner as chloroform. From this

cause a patient suffered several months at the Hospital, and I believe much more severely than if he had undergone the operation without the ether. I am confident, too, that it is more apt to produce vomiting, and a greater disturbance of the brain and nervous system, causing headache, restlessness and vigilance, which not unfrequently continue for many hours after its exhibition. Perhaps these last symptoms may be owing to the great amount of alcohol it contained.

I cannot, I confess, divest myself of the belief that chloric ether is an unsafe anæsthetic agent, when I consider that it is simply chloroform diluted with alcohol. It is true, that as far as we know, no fatal effects have hitherto followed its inhalation; but it is also true, that it has as yet been used to a very limited extent, and in all the cases in which it has been exhibited that have come to my knowledge, it has been managed with great caution and judgment. But I fear that if it be used with the same freedom that sulphuric ether is, we shall soon have to record some very different results. We cannot feel confident that it will always be confided to skilful hands only, nor by any means certain that death, when not looked for, may not follow its inhalation.

Boston, April 10, 1850.

On the use of Chloroform and Ether at the Bellevue Hospital, N. Y. By D. MERDITH REESE, M. D., Resident Physician.

THE following is a portion of an article entitled "*Notes of Hospital Practice at Bellevue, N. Y.*," communicated for the *American Journal of Medical Sciences*, by Dr. Reese. It suggests a combination of Anæsthetic agents which merits the attention of the profession.

Chloroform and Ether.—The employment of these anæsthetic agents by inhalation has been signally successful in every department of hospital service. At first, and immediately after its introduction into the Massachusetts General Hospital, the ether was used in all surgical operations of any magnitude, and in various painful and spasmodic diseases. No permanent evil effects followed, except in a single instance in which cerebral disease, probably of organic character, had pre-existed, and in this case the operation for the removal of extensive exostosis was necessarily tedious, and the inhalation had to be continued, and more than once repeated. The wound healed kindly, but the patient had to be subsequently sent to the insane department, and is deemed incurable. As, however, he had exhibited indications of insanity long prior to the inhalation, the mischief cannot fairly be ascribed to the etherization, although it would be indiscreet to employ this agent in any similar case. In no other instance did any evil happen which could be legitimately traced to the use of ether, although it was used very frequently for months, and until the introduction of *chloroform*.

This latter article was adopted as a substitute for the ether, as soon as authentic accounts were received of its employment by Dr. Simpson with success, and chiefly because we had found that the effects of ether were too slow in some cases, unequal in degree in others, while it occasionally failed.

The chloroform used at first was prepared by Dr. John Miller, then chemist and apothecary to the hospital, and was of entire purity and great strength. Our early experience with it developed two objections to its use; namely, it was sometimes too rapid in its action, occasionally cumulative, and deep anæsthesia came on too suddenly; hence we used it with great caution, and closely watched its effects. Anxious to avail ourselves of its greater certainty than ether, and yet avoid too deep anæsthetic effects, we determined to dilute the chloroform with ether, in the proportion of two measures of the latter, by weight, to one of the former. With this mixture, we have had every reason to be satisfied, and have hence very rarely employed the chloroform alone. The suggestion of this mode of dilution was first made by Dr. W. H. Van Buren, one of the visiting surgeons, and at his instance it was prepared and used for a surgical operation, with such entire success that we have employed this mixture ever since. In some instances, it is true, we have found patients so unsusceptible, that we have resorted to the chloroform alone, until the desired insensibility was produced, and then rendered the effect persistent, as long as necessary, by using the mixture of chloroform and ether, regarding it as a safer practice, and equally efficacious to this end.

Our method of employing these agents has been either by applying to the mouth and nostrils a hollowed sponge, moistened with $\mathfrak{3i}$ or $\mathfrak{3ij}$ of chloroform, or $\mathfrak{ʒss}$ of the mixture; or, by means of a towel or handkerchief in lieu of the sponge, and which, on some accounts, is to be preferred. In no case, have we persisted continuously to apply either of these agents, so as to exclude the atmospheric air, but have always allowed the alternate inhalation of the air and that of the chloroform or ether. And our rule has been to remove the agent from the mouth as soon as anæsthetic effects have become manifest, reapplying the sponge or towel occasionally, if necessary. With these precautions, our experience has been so entirely free from any untoward or unpleasant consequences, that we can scarcely feel the force of the objections recently urged against anæsthetic agents, and are constrained to apprehend that the mischiefs described have resulted either from the want of due caution in their employment, or the lack of discrimination in the subjects, by overlooking existing pathological states which contraindicate their use. Certainly, we can have no sympathy with the surgeon whose unsuccessful operations are ascribed by himself to etherization; much less is any man authorized to attribute the want of success in another, to the fact that either chloroform or ether had been used in the case.

The following are a few of the specialities in which these agents have been employed here; namely:—

1st. In *reducing dislocations*; and here our experience has proved the invaluable importance of the discovery which has furnished to the profession these means of completely relaxing the muscles and ligaments of the larger joints. In several instances, luxations at the shoulder and hip of long duration, which, after repeated trials, could not be reduced by any amount of physical force applied in the ordinary way, were, after the inhalation of chloroform by the patient, found capable of ready reduction with the thumb and finger. If no other good were conferred by chloroform, this alone would render the discoverer a benefactor to the world.

2d. In hysterical, epileptic, and puerperal convulsions, we have had

frequent opportunities of witnessing the speedy and effectual relief afforded by the inhalation of chloroform and ether; and, in the latter of these forms of disease, after extensive venesection and the other most potent antispasmodics had been tried in vain.

3d. In delirium tremens, we have frequently been able permanently to calm a violent patient, and induce profound and protracted sleep, after both lupuline and opium had failed.

4th. In paroxysmal or spasmodic asthma, a very few inhalations of ether or chloroform, without even approaching full anæsthesia, will be found more effectual for relief than any other remedy.

In tedious, protracted, or severe labour pains, and especially when malpresentation, or other causes, render painful operations necessary, whether manual or instrumental, the inhalation of chloroform, with the precautions already named, has been found to be uniformly safe, and pre-eminently useful, both to the mother and child; while it divests the most formidable operations of obstetric surgery of their terrors, alike to the physician and his patient. We have not encouraged the use of this agency in natural labours, unattended by any considerable severity, although in these cases no evil has happened to mother or child in our hands. But, in all cases in which it has become important to lessen the sufferings of the mother, we have uniformly administered the chloroform, and have had every reason for undiminished confidence in its innocence and utility. Indeed, we had good ground to believe that, in several cases, our patient would have died undelivered, if we had been without chloroform; and yet, in more than one of these, the child as well as the mother was saved. Nor have we been able to detect, in a single instance, any subsidence or diminution of the labour pains, either in frequency or force, during the moderate degree of anæsthesia, which is all that is necessary for any useful purpose in obstetrical practice. And yet we have been obliged, in some severe labours, to continue the repetition of the inhalation during several hours—applying the chloroform to the nostrils and mouth for a few moments only, however, at the commencement of every pain—our patient importunately pleading for it, after having felt its efficacy in relieving her sufferings. Such has been our hospital experience in certainly one of the largest lying-in departments in our country.

Surgical Operations.—In all cases, when the severe or formidable operations of surgery have been called for, whether by the knife, saw, chisel, or red hot iron, we have not hesitated to give our patients the benefit of chloroform; and, in all this variety of infliction, we have witnessed entire immunity from pain, and in most cases there has been no consciousness of the operation. The limb has remained unheld and unmoved while the actual cautery has been made repeatedly to traverse a joint; and, in one instance of the amputation of a thigh, the patient remained ignorant of his mutilation until the fifth day, when it became necessary to dress the stump, he all the while supposing that his leg was bound up in splints, and complaining of that very common, though imaginary, sensation in the toes of the amputated limb. The success of operations, and the early healing of the wounds thus made, have in no instance been hindered or delayed, so far as we could perceive; and we have even surmised the contrary. In primary amputations, after compound or comminuted fractures, when these are judged necessary, the additional shock to the nervous

system, so much dreaded, is in a great measure obviated by the judicious use of chloroform. And, whatever may be the prejudice existing or engendered by panic-makers, who are for the most part mere theorists, the extensive experience and success of practical men, here and elsewhere, should disabuse the profession and the public of the erroneous views which have been promulgated of late, on the basis of a few unsuccessful and probably indiscreet experiments, and isolated cases of unfortunate termination. The profession should not only allow their patients the advantages derived from this new agency to protect them from suffering, but they should feel that they are not at liberty to hazard the safety of their patients by withholding from sufferers of every class the benefit of anæsthesia, now that science has conferred this boon upon afflicted humanity.

Report of a case of premature labour, artificially induced, with successful results, at the seventh month of gestation, on account of contracted pelvis. BY THOMAS W. BLATCHFORD, M.D. Read before the Rensselaer county Medical Society, Jan. 7, 1850.

MRS. M., thirty-one years of age, short and thick-set, of sanguine temperament and good constitution, was married Dec. 25th, 1845. On the 11th of September, 1846, I was sent for to attend her in her first confinement. The membranes had broken early in the morning, and without pain; pains however soon succeeded, and when I first saw her they were regular and quite severe. Upon examination *per vaginam*, the os tincæ was found but little if at all acted upon, and it was not until the second day that the dilatation was sufficient to ascertain that the head presented. After bleeding, and a full dose of opium, dilatation progressed more rapidly, and was completed about the end of the second day; but notwithstanding the pains for the most part had been severe and forcing, with very short intervals of ease, at the close of the third day the head had progressed only through the upper strait. It now became very doubtful whether labor could be terminated without instrumental assistance. The advice of able counsel was now sought. In consultation it was agreed that inasmuch as the head still receded a little upon pressure, the pains still very forcing, the pulse firm and good, and no marked signs of prostration present, we should still leave the case to nature for a few hours longer. At nine o'clock A. M. we met again. The pains had continued unabated. The head was found in about the same position, but it now seemed completely impacted. The scalp was soft and patulous, no fœtal motion discernible upon examination *per vaginam*. Yet the patient said she distinctly felt life, and the stethoscope confirmed the assertion. Her pulse was somewhat quickened, and she had become very restless, and anxious that the child should be taken away immediately with instruments. We attempted the introduction of the forceps, but the pressure between the bones of the head and those of the pelvis was so great, that their introduction became impossible without endangering the soft parts of the mother. The vectis was tried, but with no better success; and the only alternative was the perforator and the blunt hook, which affected delivery after about half an hour's effort. The child had been dead long enough to smell offensive, and the cuticle was detached in

several places. It weighed a little over six pounds. In tracing the umbilical cord for the placenta, it was discovered that a bag of water presented. It was that of a second child, and a footling case. The membranes were ruptured, the feet grasped, and the labor terminated within about five minutes. The child was in a state of asphyxia, and remained so several minutes, but perseverance in the use of the hot spirit bath and artificial respiration, at length restored it to life. It weighed four pounds, soon became vigorous, and grew finely, and is now, 1850, a very healthy girl, between three and four years of age.

In about fourteen months after that time, Mrs. M. whose health had been excellent, found herself at the full term of a second pregnancy. On the 26th of November, 1847, labor commenced. It progressed much after the same manner as before. It was introduced by the rupture of the membranes. The capacity of the pelvis seemed less if possible than at the first confinement. It did not appear deformed at any one particular point, but of contracted dimensions in every diameter. After the first twenty-four hours her pains were almost constant, seldom affording her a five minutes' interval of ease. The tardy dilation of the os tincae did not seem to interpose any very serious obstacle to the descent of the head, (the head presented,) for it was about as slow after perfect dilatation had taken place as it was before. Bleeding as before was resorted to, but with very little apparent benefit, except in preventing inflammation. Opium she refused to take, in consequence of some unpleasant symptoms which before resulted from its administration.

Nearly seventy hours had now elapsed, and the prospect of delivery without instrumental assistance, seemed as remote as ever. The strength of the patient became exhausted. Her pulse was over a hundred. She insisted that she had not "felt life" for two days, and no signs of life were discoverable either by taxis or stethoscope. The entreaties of both patient and friends became urgent to have me terminate labor as before. At this stage further medical advice was requested. After a careful examination into the case, it was determined to wait no longer, but to use the perforator at once, as offering the only safe course to the mother. The perforator was introduced; the brain and most of the bones of the cranium were extracted, and yet nearly two hours were consumed before labor was terminated. The child weighed nearly eight pounds, and did not exhibit any signs of having been long dead. The patient recovered after an unusually short confinement, and felt no other inconvenience than a slight laceration of perineum, which healed entirely in about two weeks.

From these two trials it became very evident that Mrs. M. could never have a living child of ordinary size at maturity. We were therefore necessarily driven to the conclusion, that this was one of those cases which would justify the induction of premature labor, at a period when the viability of the child might be reasonably calculated upon. Accordingly she was now promised, that should she ever again become pregnant, labor should be brought on at the seventh month, when the child would not probably weigh over four or five pounds, and when its life would not necessarily be endangered. She was further told that her labor would in all probability be very short, terminating after a very few hours' continuance.

During the last summer, it became evident to friends, that Mrs. M. was

again pregnant. She was last unwell 5th May. What doubts she may have entertained herself, were all dissipated when she felt life, the last of September. Very soon she sent for me, and reminded me of my promise. As the seventh month approached, she became increasingly anxious about her situation, and desirous to have labor induced, just as soon as we thought it would answer. Her health had been excellent. She had experienced no other inconvenience from her situation than an obstinate costiveness, and rather an unwielded weight of abdomen. She was unusually large for one at seven months. Her size was thought to be greater than that of most women at nine months.

On Wednesday, 5th of December, 10 o'clock, A. M., being just seven months since she was last unwell, and two and a half since she quickened, every thing being in readiness, with the assistance of Dr. Robbins, half a pint of "tar water" was injected into the womb through a large-sized male catheter, moderately curved, and by means of the syringe of a common self-injecting apparatus. The patient was placed upon her left side, with her knees separated. The fore-finger of my left hand placed upon the posterior lip of the os tincæ, guiding the catheter in its introduction. It passed without the least resistance from two to two and a half inches within the uterus, occasioning not the slightest pain. No fluid escaped from the catheter. The patient then turned upon her back, and was requested to take hold of the catheter herself, and not suffer it to move either backward or forward, which she did. The syringe was then attached to the catheter, and the injection slowly and cautiously passed, to avoid, if possible, the rupture of the membranes. Upon detaching the syringe, a few spoonfuls of fluid escaped through the catheter, tinged with blood, which at first we feared was the liquor amnii. The operation lasted but a few moments.

After remaining about ten minutes in a recumbent posture, she was permitted to get up, which she did, and moved about the house as usual, experiencing no other inconvenience than a constant draining from the vagina, of a small quantity of fluid slightly tinged with blood and tainted with tar, and a sense of weight as if, to use her own expression, "the child had settled down."

Nothing unusual occurred until Friday evening, the seventh, when she was suddenly taken with a chill and rigor, which lasted nearly two hours, accompanied with severe headache. It was succeeded by slight fever. She, however, rested tolerably well during the night, having bathed her feet and taken an active cathartic.

Saturday morning I found her very comfortable, as much so as she had been for weeks, with the exception of a slight draining before mentioned, which, however, she said was not sufficient to require her to wear a napkin. At eleven o'clock, however, and after the operation of the cathartic, she was taken in labor. The pains at first were few and far between, until about one o'clock, P. M., when they became quite violent and frequent. At two o'clock the membranes gave way during a hard pain, and a very large quantity of water was discharged. The nurse and patient both say, more than two quarts. It diminished her size very much; so much that when I entered the room, having been sent for in haste, I was saluted with, "Doctor, see here, I have had my baby, and it is all water." The effect of this large evacuation was to give an almost entire relief from pain.

Upon examination *per vaginam*, no impression had apparently been made upon the os tinæ. During the afternoon and evening she continued free from any severe pain, and rested that night quite as well as she did the night previous. She felt occasionally a heavy bearing down sensation, and once in a while an acute pain in her back; occasioned, she said, by the uncommon motion of the child. If I could give her any thing to make that lie still, she was sure she should not have any pain.

By a little after eight o'clock, Sabbath morning, the pains again returned, at first slight and not very frequent, but they soon became very regular, the interval being about five minutes. An examination at this period detected no change upon the os tinæ. The point of the finger could hardly enter it, still the soft parts were not at all heated, and they were now well lubricated. It became evident that she was even now to have a tedious labor, notwithstanding the caution used. It was not till noon that dilatation could be said to have fairly commenced. The pains, though regular, did not assume any very great degree of severity till about five o'clock, when they began to be very forcing. She complained mostly of her back. Dilatation now went on more rapidly, and by eight o'clock the head could be felt forcing its way through the upper straight. From this time until one o'clock, the pains were very severe, and yet very little progress had apparently been made towards the completion of labor. Dilatation, however, was now perfect. So far so good. But the patient, hitherto firm and resolute, began to manifest signs of restlessness and impatience, and her spirits evidently began to flag. If she could have pain any where besides in her back, she said she could bear it without a complaint. Her cry, and that of some friends, was for me to terminate labor immediately by instruments, or give her something to put her out of misery; she wanted "to die and not to live," &c., &c.

The head was slightly moveable, receding a little after each pain, advancing, however, but little, if any further, during a succeeding effort. I was tempted at this stage to administer the ergot, or to employ the vectis, but the evident viability of the fœtus, the perfect dilatation of the os uteri, the thorough lubrication of the soft parts, and their entire freedom from any undue heat, together with the undiminished energy of the pains, and with all the comforting knowledge that a small child had once passed through the same aperture, encouraged my non-interference. Besides, I must confess, I felt a mighty unwillingness to resort to any aid either medicinal or instrumental, in a seven-month case, or do anything whereby I might endanger the life of the child, so long as the mother's safety did not clearly require it. Under these circumstances, therefore, I determined to leave the case still longer to nature. More than once I regretted the promise I had given her of having an easy time. I was often twitted about it, and asked if "I called that a quick and easy time," &c.

From about half past one o'clock, the pains were nearly continuous, and at times exceedingly severe, resembling much those induced by the administration of ergot, and thus they continued increasing in force and severity if possible, until half past two, A. M., (113 hours from the time the tar water was injected,) when she was delivered of a plump and vigorous child, loudly vociferating its own advent. It weighed nearly four pounds. The placenta soon followed. The secretion of milk was established in the usual time, and the child required no lessons of instructions

to draw it, taking the breast as promptly and as eagerly as if it had been a nine, instead of a seven months' production. The mother recovered without any unpleasant symptoms whatsoever. The almost necessary "soreness and stiffness" after so much exertion, soon passed off, and in ten days she was up and about the room, and in one fortnight dismissed her nurse, assumed the discharge of her domestic affairs, and has the satisfaction, to use her own expression, "of nursing her own infant, with as fair a prospect of raising it as any other mother enjoys." Her sufferings, it is true, were very great, but in her own estimation even, and in that of those who witnessed both, they were not near as severe as those she had before undergone.

To my mind, the five methods of inducing premature labor given by Churchill, in his "system of midwifery,"—and he devotes a whole chapter to the subject, with which every one intending to operate, would do well to make himself familiar,—did not seem to offer the advantages which the simple injection of "tar water" presented. The eryot might endanger the life of the child, about one half being still-born after its employment. To puncturing the membranes, and letting off the waters either suddenly, or little by little, necessarily subjecting the child to great pressure at a period when the tenacity of life is very feeble, there was the same objection. The contracted capacity of the vagina would not in this case permit the "introduction of the hand sufficiently far to detach the membranes with the finger" without causing excessive pain, or even introduce into the os uteri the sponge of Kluge. An attempt to detach the membranes for an inch or two within the os, by means of a catheter, seemed almost of a necessity to endanger the integrity of the membranes; and "abdominal frictions and manipulations, and the warm bath," seemed to be remedies entirely too domestic, unscientific and uncertain.

M. Cohen, of Hamburg, the first I believe to propose injections, states that he had been led to try them for the purpose of inducing premature labor, from noticing their power in developing contractions when introduced into the unimpregnated uterus, "and as," says he, "the pregnant uterus is in a condition apt to contract, he thought injections might be efficaciously used, and that without danger, to bring a delivery in those cases where it is necessary the fetus should be expelled before the full period of pregnancy." He states further that the "he had been in the habit of employing "tar water" for diminishing the excess secretions from the uterine surface," and thus he was led to make use of it for this object as in the case he has detailed.

The reader of M. Cohen's paper, as noticed in several of the leading Medical Periodicals, since 1847, will perceive that I did not repeat the injection after six hours, as M. Cohen recommends. I did not repeat it at all, for the reason that the constant, though slow draining of fluid more or less tinged with blood, led me to believe that the membranes had been ruptured, and notwithstanding all my care to prevent such an occurrence; and I continued of that opinion until informed of the copious and sudden discharge of water after two or three hours severe labor; otherwise, I should have followed M. Cohen's directions to the letter.

NOTE.—March 15, 1850. The child to-day weighs 12½ lbs. has suffered from "sprue, red gum and jaundice." The mother's health is excellent, and the secretion of milk abundant.—*Transactions New York State Medical Society.*

Account of an operation for the removal of an Ovarian Tumor. By ALDEN MARCH, M. D., President, and Professor of Surgery in the Albany Medical College.

On Friday the 7th of December, 1849, the wife of Mr. W. J. P., of Granby, Mass., aged 49 years, was brought to me in company with her husband and her family physician, Dr. Alexander Le Baron Munroe, of the same place, for consultation and advice in what was supposed to be an *Ovarian Tumor*.

Mrs. P. was the mother of five children, the youngest of whom was about seven years of age, in person rather spare, of a good constitution, intelligent, firm, and morally courageous.

The tumor was of three years' growth or more, having been accidentally discovered when when it was about the size of a turkey's egg, in crowding between a bed post and the wall of the room. For a year or two after, it gave her no alarm, or scarcely any anxiety; since it was neither painful nor tender, nor was its moderate increase in size calculated to excite serious apprehensions for the future; so that its existence was not made known to her physician until within eight or nine months of the day of the operation. During the last three or four months the tumor had increased so rapidly that she appeared like a woman far advanced in pregnancy. At last she was so much incommoded as to be entirely prevented from sleeping in the horizontal position.

After inquiring into the history of the case, and making a careful examination of the tumor, the general health and condition of the patient, as to the existence or non existence of any other disease, Dr. Munroe and myself agreed that it was a favorable case for the operation of extirpation. However, before proceeding in a matter of such vast importance to the patient and her friends, to say nothing of my own professional reputation, and that of the practice of the surgical art, I deemed it prudent to solicit a general consultation on Saturday, at which were present Dr. Munroe, Prof. E. Emmons, Prof. J. McNoughton, Prof. J. H. Armsby and Prof. Thos. Hun, my colleagues in the Albany Medical College, who came to the unanimous conclusion, after each had made a critical examination, that if an operation for an attempt at removal was ever warrantable in cases of this kind, this was a peculiarly favorable one.

The tumor was in the main smooth, though apparently lobulated, or having in one or two places a strictured or depressed line; and most of the bulk of it seemed to be made up of fluid, although there were one or two points apparently as large as the fist, which felt like a soft solid. In the abdominal wall there did not appear to be any tenderness or soreness to the sense of touch, nor had the patient ever experienced any pain in the swelling or abdomen, excepting an uneasiness for a few weeks, which was the result of pressure and over-distension of the abdominal walls.

The bowels were thoroughly evacuated on Sunday, by a calomel purgative, and on Monday, December 10th, 1840, at one o'clock, the operation was performed in the presence, and by the aid and assistance of the gentlemen who have already been named as council in the case, as well as several medical students.

An incision was commenced about four inches above the umbilicus and carried in the line of the linea alba to near the pubis. The abdominal wall at the upper part was extremely thin, and by two or three strokes of

the knife the tumor was readily brought into view. The wound was so extensive, being about 12 inches, and the tumor so much exposed, that it was readily discovered by the eye and hands to lie almost loose in the abdomen. To facilitate its extraction, a puncture was made in the front and lower part of it, by which nearly, or all of the water from the cyst was evacuated. The collapsed sac was followed down to its attachment to the right angle of the uterus, by which it was discovered that the ovarium had been dilated into a *monocular*, or single sac, attached to the uterus only by the broad ligament and Fallopian tube, which when twisted upon itself, or gathered together was not larger than the little finger; around which, in mass, a three threaded ligature was very tightly applied, not merely by the strength of a strong man, but by one of my own hands superadded. This was applied very near to the substance of the uterus; and next, all the substance included in the ligature, was severed about half an inch from the point of its application. In using the sponge to cleanse out a trifle of blood that had fallen into the abdomen, the ligature was detached, when a brisk, or rather an alarming hemorrhage ensued. The uterus was seized upon, drawn up, and the part containing the severed vessel, which was of the size of a crow's quill, or larger, was secured between the thumb and finger of my assistant, Dr. Armsby, until I could pass an armed needle with double ligature, the four ends of which I tied each way. The first did not prove effective in completely arresting the flow of blood. Another needle was passed in the same manner and secured as before, which answered the purpose. The patient must have lost nearly a pint of arterial blood, most of which fell into the cavity of the abdomen, and after having been properly removed, the wound in the abdominal wall was closed by eight interrupted sutures, and the ligatures brought out at the lower part of the wound. The abdomen was supported with long adhesive straps, a large sheet folded for a compress, and over the whole a towel or swathe applied after the manner of its use in obstetrical practice.

The shock of the operation, and the loss of blood being so severe for the patient, it was difficult to keep life in her for several hours. However, at the end of six hours, re-action began to take place. Although she was in the horizontal position during the operation, yet she became faint; which rendered it necessary for an assistant to make pressure with his hands on the bowels, to aid in keeping up the circulation. She also experienced a degree of nausea, which I was then rather disposed to attribute to the use of chloroform. Some four or five days after, there was a disposition to retch, but not sufficient to amount to much vomiting. This I attributed to the sympathetic irritation existing between the uterus and stomach. I believe this was all quieted by a little infusion of columbo.

In the management of the case, I constantly and regularly used morphine, for some ten or twelve days, and did not attempt to disturb the bowels with physic for eight or nine days. In the mean time, the bladder became distended with urine, and the bowels with air. The former was relieved by the use of the catheter, and the latter, by passing up the rectum, a large gum-elastic tube. By the adoption of these measures, the patient obtained great relief, and continued to improve from day to day in a very satisfactory manner.

I think the sutures were removed on the eighth day, when union by the

first intention was complete, at every point, except at the lower angle of the wound occupied by the ligatures, with which the divided arteries were severed. There was no redness, nor scarcely any tenderness in any part of the abdomen, except at the lower part or in the region of the uterus. The ligatures had all come away, and the patient had so far recovered her health and strength, as to be able to make a journey of over a hundred miles in one day, on the thirty-fourth day after the operation.

The sac, when distended with its fluid, weighed 18 pounds.

In conclusion, I would say that I am apprehensive that a successful case or two, may encourage us to operate too often; when the favorable prospect, to say the least, might be but little encouraging. A monolocular hydatid, or fluid tumor, is regarded as the most favorable for removal.

There should be no adhesions between the cyst and the wall of the abdomen, omentum, or intestines. The foot stalk or pedicle of the tumor should be small. I should prefer not to have the patient tapped at all, except as an exploring operation just as extirpation was about to take place. I would recommend to the surgeon to place his finger beneath the broad ligament or pedicle of the tumor, and divide with the knife upon it such part as would include the blood vessels, and as they bleed secure them by a small ligature. After these are properly secured, I would then divide the fallopian tube and the remainder of the folds of the peritonium, which make up the broad ligament, as far from the angle of the uterus as practicable, and instead of fetching the ligatures out through the abdominal wall, I would suggest to have them threaded into a long *sail-maker's needle*, and conveyed by the aid of one finger in the vagina, carried high up, either in front or back of the uterus; and with the fingers of the other hand dipping down in the brim and cavity of the pelvis, through the pelvic partition transfixing it into the vagina, and out at the os externum.

By this procedure a less direct communication would be left with the peritoneal cavity; and if any serum or blood should remain in the lower part of the peritoneal cavity, it would in the course of a few days readily find an exit by the side of the ligatures, "per vaginam." In my case I do not think there was the least peritoneal inflammation, except that which was caused by and situated immediately around the ligatures, where they had their exit from the abdominal wall. Why I would apply the ligature to the insulated blood-vessels, and as far as possible from the uterus, would be to avoid irritation and inflammation of that organ, and the fold of the peritoneal membrane, which forms the broad ligament. By this means the fallopian tube needs not be in the least irritated by coming in contact with a foreign substance.—*Transactions of the N. Y. State Medical Society.*

National Medical Convention—For revising the Pharmacopœia of the United States.

The fourth decennial convention for revising the Pharmacopœia of the United States met at Washington on Monday, the 6th instant. The following delegates were present in the convention :

From Rhode Island Medical Society, Dr. Joseph Mauran.

From Geneva Medical College, Dr. James Bryan.

From the College of Pharmacy of the City of New York, Messrs. John Milhau and George D. Coggeshall.

From the Medical Society of New Jersey, Drs. Lewis Condict and Wm. A. Newell.

From the College of Physicians of Philadelphia, Drs. Joseph Carson, Henry Bond, and Francis West.

From the University of Pennsylvania, Dr. George B. Wood and James B. Rogers.

From the Jefferson Medical College of Philadelphia, Dr. Franklin Bache.

From the Medical Faculty of the Pennsylvania College, Dr. H. S. Patterson.

From the Medico-Chirurgical College of Philadelphia, Dr. Clinton G. Stees.

From the Philadelphia College of Pharmacy, Messrs. D. B. Smith, Charles Ellis, and Wm. Proctor, Jr.

From the Medical Society of Delaware, Drs. Isaac Jump and J. W. Thomson.

From the Medical and Chirurgical Faculty of Maryland, Drs. David Stewart and Joshua I. Cohen.

From the Medical Society of the District of Columbia, Drs. J. C. Hall and Harvey Lindsly.

From the National Medical College of the District of Columbia, Drs. Joshua Riley, Thomas Miller, and Edward Foreman.

From the Medical Department of the National Institute, D. C. Drs. Jas. Wynne and S. D. Gale.

From the Georgetown Medical College, Dr. F. Howard.

And from the Rush Medical College, Illinois, Dr. G. N. Fitch.

The credentials of delegates from the New Hampshire Medical Institution, the University of Buffalo, the Medical Department of Hampden Sidney College, the Medical Society of South Carolina, the Medical College of Ohio, the Cincinnati College of Pharmacy, the Missouri Medical Society, and the Medical Faculty of the University of Iowa, were presented by the Vice President of the Convention of 1840; but these delegates did not make their appearance during the session of the convention.

A temporary organization was effected by calling Dr. Lewis Condict, President of the convention of 1840, to the chair, and appointing Dr. Harvey Lindsly, Secretary. A committee of five was then appointed, consisting of Dr. Bache, Dr. Mauraan, Dr. Thomson, Dr. Miller, and Mr. Coggs-hall, to nominate permanent officers of the convention, with instructions to name two Vice Presidents, instead of one, as had been the custom on former occasions. This committee retired, and, after a short consultation, reported the names of the following delegates, viz :

For President, Dr. George B. Wood, of Pennsylvania.

For Vice Presidents, Dr. Joseph Mauraan, of Rhode Island, and Dr. D. Y. Simons, South Carolina.

For Secretary, Dr. Harvey Lindsly, of the District of Columbia; and for Assistant Secretary, Dr. Edward Foreman, of the same place.

The nominations were confirmed by the convention, and the President took the chair.

On motion, it was

Resolved, That the Surgeon-General of the Army, the Chief of the Naval Bureau of medicine and Surgery, and such of the members of the two

Houses of Congress as might be medical graduates, should be invited to take seats in the convention, and participate in its proceedings.

In conformity with the directions of the preceding convention, the committee of revision and publication, appointed by that body, presented a report of their proceedings, which was accepted.

The delegates of the several medical bodies represented in the convention were then called on for contributions towards the revision of the Pharmacopœia; when reports were handed in from the delegate of the Rhode Island Medical Society, from the College of Pharmacy of the city of New York, from the College of Physicians of Philadelphia, from the Philadelphia College of Pharmacy, and from the medical and Chirurgical Faculty of Maryland. These were referred to a committee, consisting of Dr. Bond, Dr. Mauran, Dr. Cohen, Dr. Miller, and Mr. Milhau, with directions to report a plan for the revision and publication of the Pharmacopœia; after which the convention adjourned to the following day.

At the next morning, on Tuesday morning, a committee was appointed to examine the accounts and vouchers presented by the committee of revision and publication of the preceding convention, and reported that they had found them correct.

Dr. Bond, from the committee to which had been referred the reports from various medical bodies represented in the convention, reported the following resolution:

1. That a committee of revision and publication, consisting of nine members, be appointed, to which shall be referred all communications offered to the convention in relation to the revision of the Pharmacopœia, and that three of this committee shall form a quorum.
2. That the committee shall meet in the city of Philadelphia, and be convened as soon as practicable by the chairman.
3. That said committee shall be authorized to publish the work of revision, and to take all other measures which may be necessary to carry out the views and intentions of the convention.
4. That the committee shall have power to fill its own vacancies.
5. That, after the completion of its labors, the committee shall submit a report of its proceedings to the Secretary of this convention, to be held before the next convention.

The resolutions were adopted, and the following delegates appointed on the committee, viz: Dr. Franklin Bache, Dr. Joseph Carson, and Wm. Proctor, Jr. of Philadelphia; Dr. Joseph Mauran, of Providence Rhode Island; Mr. John Milhau, of the city of New York; Dr. J. W. Thomson, of Wilmington, Delaware; Dr. David Stewart, of Baltimore; Dr. Joshua Riley, of the District of Columbia; and Dr. G. N. Fitch, of Logansport, Indiana.

It was resolved that the President of the convention be added to the above committee, and serve as its chairman.

In reference to the manner of calling and the mode of constituting the next decennial convention, to meet in the year 1860, it was

Resolved, That the regulations in reference to the present convention, adopted by that of the year 1840, and published in the 1st edition of Pharmacopœia, should be adopted, with the necessary modifications in relation to the dates; the day of meeting being changed from the first Monday to the first Wednesday in May

A letter was read inviting the members of the convention to a dinner, to be given at the National Hotel, by the medical gentleman of Washington. The invitation was accepted, and the thanks of the convention voted to the gentlemen referred to for their hospitality.

The thanks of the convention were also unanimously voted to Dr. Lewis Condict, President of the last convention, for valuable services; and to the Board of Aldermen, of the city of Washington, for their courtesy in offering their hall for the sittings of this convention.

The convention then adjourned.

After the adjournment, Dr. Wm. B. Chapman, one of the delegates from Cincinnati College of Pharmacy, arriving in Washington, stated to the Secretary his concurrence in the proceedings of the convention.

HARVEY LINDSLY, M. D.

Secretary of the Convention.

Washington, may 9th 1850

Case of Ovariotomy. By WASHINGTON L. ATLEE, M. D., Professor of Medical Chemistry in the Med. Dept. of Penn. College, Philadelphia, Pennsylvania.

In an article, contained in the American Journal of Med. Sciences, for April, 1850, Prof. Atlee reports two cases of "large peritoneal section." In one case the section was exploratory; the tumour found to be uterine and fibrous, and not removed. The operation was made 22d May, 1849, the patient recovered from the operation, and died by erysipelas, Nov. 3d, 1849.

In the second case, there existed an unilocular cyst of the the right ovary; weight, forty pounds. Patient recovered.

The length of the reports of these cases, precludes their insertion.

In the same article, Prof. A. presents an analysis of a large number of cases of ovariotomy, which is interesting and valuable. The latter we select.

The author embraces the occasion to establish his claim to the fruits of his labors, which, it appears, have been appropriated by Mr. Lee, of London. In justice to Prof. Atlee, we copy that portion of his article.

EDITOR BUFFALO MEDICAL JOURNAL.

"In the *Amer. Journ. of Med. Sciences*, April, 1844, I published a table of 101 cases of Ovariotomy, in which I made a synopsis of the important points of each case. Since the publication of that table, I have been watchfully keeping pace with the operation, and have now tabulated 179 cases. I also made an analysis of that table in order that the profession might see at a glance the most important aspects of this operation prominently arranged. I have done the same with my manuscript table, and will submit it to the profession, in order that they may properly estimate the present condition of gastrotomy :—

1. Of these 179 cases, 28 were of the minor section, 133 of the major and 18 unknown. Of the minor operation, 20 recovered, and 8 died, or one in every $3\frac{1}{2}$; of the major, 87 recovered, and 46 died, or one in $2\frac{4}{11}$; of the unknown, 13 recovered, and 5 died, or one in $3\frac{3}{5}$. Total, 120 recovered, 59 died, or one in $3\frac{2}{39}$, or 59 in 179 cases, or 32.96 cases in 100.

2. Of the 179 cases, 34 were not completed, or one in $5\frac{3}{24}$; and, in 6, there was no tumour, or one in $29\frac{2}{5}$ cases.

3. Of the 34 unfinished operations, 19 were the large section, 8 the small, and 7 unknown; 14 of the first recovered, 5 died, or one in $3\frac{4}{5}$; 4 of the minor recovered, 4 died, or one in 2; 6 of the unknown recovered, 1 died, or one in 7. Total, 24 recoveries, 10 deaths, or one in $3\frac{2}{5}$ of the unfinished cases.

4. Of the 6 operations in which no tumor was found, 5 were major, and 1 minor; 3 of the former recovered, 2 died; and the minor recovered — making 4 recoveries, 2 deaths, or one in 3 cases.

5. In 17 cases, other important diseases co-existed; in 4 of these, the operation was left unfinished, and all the patients recovered; death occurred in all the rest but one. 14 of these cases were the major, 2 the minor, and 1 unknown.

6. In 62 cases there were adhesions; in 41, none; in 76, not stated. Of the first, 36 recovered, 26 died, or one in $2\frac{5}{13}$; of the second, 29 recovered, 12 died, or one in $2\frac{5}{15}$ cases.

7. The cause of death in the 59 fatal cases is recorded as follows: From hemorrhage, 12; peritonitis, 12; exhaustion, 3; shock of operation, 2; inflammation of mucous coat of large intestines, 1; gangrene of peritoneum, 1; peritonitis and gangrene, 1; diarrhoea and peritonitis, 1; peritonitis and constitutional debility, 1; inflammation of lungs, 1; ileus and phlebitis of lower limbs, 1; a fall during convalescence, 1; causes not stated, 21. Total, 59.

8. The period of death after the operation in 59 fatal cases is recorded as follows: Died the 70th day, 1; in 6 weeks, 2; in 3 weeks, 1; the 17th day, 1; the 15th day, 1; the 14th day, 1; the 10th day, 1; the 9th day, 1; the 7th day, 3; the 6th day, 5; the 5th day, 2; in 3 days, 3; in 74 hours, 1; in two days, 1; in 44 hours, 1; in 36 hours, 5; in 32 hours, 1; in 30 hours, 1; in 17 hours, 1; in 12 hours, 2; in 11 hours, 1; in 8 hours, 1; in 6 hours, 1; in 4 hours, 1; immediately, 2; time not stated, 18. Total, 59. The average time of death in 41 cases stated, 8 days.

9. Of the 17 cases complicated with other important diseases, 7 were manifestly not proper for the operation, and 8 others, instead of 4, ought to have remained unfinished after the abdominal section was made. Throwing the first 7 cases out of the estimate, would leave 172 legitimate cases, and rating the 4 others, that ought to have remained unfinished, according to the mortality of unfinished operations, it would make 123 recoveries, and 49 deaths, or one in $3\frac{2}{39}$, or $28\frac{2}{33}$ deaths in 100 cases, which I consider the correct rate of mortality of the operation as it is represented by my manuscript table.

10. Under the head of the 8th paragraph, I have stated that death occurred, in one instance, on the 70th day; in two instances, after the expiration of 6 weeks, and, in another case, from a fall during convalescence. Now, I would ask, is it proper to consider the fatal termination in these cases the result of the operation? Or rather, ought they not to be consid-

ered as having recovered from the *operation*, and be so reported? If so, then the fairest estimate would be, after throwing out the 7 cases referred to, 127 recoveries, and 45 deaths; or one in $3\frac{37}{45}$, or $26\frac{1}{15}$ deaths in 100 cases.

11. The rate of mortality has been very much diminished since the publication of my table in 1845. Then, there was one death in every $2\frac{25}{38}$ cases of gastrotomy, or 37.62 deaths in every 100 cases. Since the publication of that table, 78 cases have occurred, in which there was one death in every $3\frac{1}{2}$ cases, or 26.92 deaths in every 100 cases. A diminution of nearly 40 per cent. in the rate of mortality.

12. There has also been a diminution in the proportion of unfinished operations, and, in no case since, has the abdomen been opened for the purpose of removing a tumour when no tumour could be found. It should also be observed that several of the more recent unfinished operations have been of the exploratory character. Hence, diagnosis has also improved.

13. If we compare the above statistics of the mortality of the operation for ovariectomy, with those of the mortality following other capital operations, we will find them in favor of gastrotomy.

Take, for instance, the valuable tables of cases of operation on the larger arteries, published by GEORGE W. NORRIS, M. D., in the *Amer. Journ. of Med. Sciences*. The rate of mortality in these cases is, one in $2\frac{25}{38}$ or 33.45 deaths in 100 cases, while, in all the cases of ovariectomy reported, it is one in $3\frac{2}{3}$, or, 32.96 cases in 100. Taking, however, the more recent cases, as noticed in paragraph 11, the rate of mortality is 25 per cent. greater in the operation on the larger arteries than in gastrotomy.

Dr. Lee, in his work on "Tumours on the Uterus," refers to some statistics by Malgaigne on Amputations, as published in the *Medical Gazette*, 1846. These amputations were performed in the Parisian hospitals from 1836 to 1841, and were 852 in number. They included *all* the extremities, even to fingers and toes. Of these, 332 died, or one in $2\frac{27}{33}$, or 38.97 deaths in every 100 cases. A mortality nearly 45 per cent. greater than that of gastrotomy.

201 of these amputations were of the thigh. Of these, 126 died, or one in $1\frac{25}{33}$, or 62.68 deaths in every 100 cases. Exceeding the mortality of gastrotomy 133 per cent.

192 amputations were of the leg; 106 died, or one in $1\frac{23}{33}$, or 55.21 deaths in every 100 cases; 82 per cent. greater than gastrotomy.

Before concluding this paper, I trust it will not be thought improper, in connection with the above statistics, to allude to a matter of a personal character. In the *Amer. Journ. of Med. Sciences* for April, 1845, page 390, I published a table of cases of ovariectomy that had occurred up to that date, the construction of which, cost much trouble, time, and labor. This table was prepared, at first, solely for my own use, for the convenience of reference; but, believing it would be of service to the profession in the discussion of the question of ovariectomy, I offered it for publication. In January, 1847, "*A Dissertation upon Tumours of the Uterus and its Appendages*" was published by Mr. Thomas Safford Lee, London, for which the Jacksonian Prize was awarded. It is a valuable publication, the most complete epitome of knowledge on ovarian dropsy we possess, and enters into the question of gastrotomy more fully than any treatise of

the kind. At page 183, Mr. Lee says, "*I have carefully collected into a tabular form all the known operations for the extraction of the ovary,*" and then, with the addition of a few cases which occurred since April, 1845, he gives, as the result of his own labor, the very table which I had published in 1845, omitting any acknowledgement whatever. After Mr. Lee enters upon the discussion of the question, on page 183, "*What are the results of the operation already performed?*" he makes use, very frequently, of the facts which I had collected, adopting my arrangement of them, and even my language, without any reference to the source whence he mainly drew the information, upon which were founded the valuable deductions in his book. Feeling aggrieved that my labors had been appropriated without the credit, to which I was justly entitled, having been awarded, I addressed a letter to Mr. Lee, appealing to his sense of justice, and calling his attention to the *omission*. Mr. Lee promptly replied, assuring me of his regret that such an omission should have occurred, and sincerely hoped that I would not consider it a wilful one, acknowledging his indebtedness to me, and promising, should occasion offer, in a second edition of this work, to do me ample justice for this temporary omission. From the gentlemanlike character of Mr. Lee's letter, I was satisfied that the omission was not intentional, and felt willing that the correction be made by himself at the proper time, and so had determined. I think it necessary, however, in justice to myself, to refer to this subject at this time, inasmuch as a distinguished American author has given to Mr. Lee the credit of presenting those very facts to the profession. In a work entitled "*Females and their Diseases, a series of Letters to his Class,*" by Charles D. Meigs, M. D., &c. &c., Philadelphia, 1848, page 315, are these words: "Facts are the things that teach—and I shall close this letter by laying before you the tabular view presented by Dr. T. S. Lee, who, I am sure, will not object to my using so great liberty with his work, the more especially as it may assist in spreading further and wider the knowledge he has been at so great pains to collect, and make it both more public and useful at once." Then follows *my* table with this head, "*Table, by Dr. Lee,*" and the several succeeding pages, through which the table is continued, have, each one, this title: "*Dr. Lee's Table.*" As both Dr. Meigs' book and the *Amer. Med. Journ.* have a wide circulation among American practitioners, it will be perfectly competent for them to compare Dr. Lee's table of 1847 with my table of 1845, and decide *who* "*presented the Tabular View,*" and *who* "*has been at so great pains to collect the knowledge*" therein contained."

Report of the Committee appointed to examine into the condition of the mucous membrane of the intestinal canal in persons dying of Cholera.

Science is positive only when its facts are positive. A subject, the phenomena of which are numerous and complex, can be understood only when each of its phenomena or facts have been analyzed, and positively ascertained.

In Epidemic Cholera, the most prominent and constant phenomena, are purging and vomiting: and in ninety or more, of one hundred cases, these phenomena appear to induce the condition that usually terminates fatally.

It is, therefore, an important object in determining the phenomena of Cholera, to ascertain whether any, and if any, what constant anatomical alterations can be detected in the intestinal canal of cholera patients who have succumbed under the disease.

The College, with the view to obtain, as far as possible, accurate information on this single question, appointed the undersigned, at the meeting held on the 19th day of June last, a committee to investigate this subject.

The Committee having attended to this duty, submit the following report :—

The ordinary autopsical examinations, heretofore practised, have failed to yield any satisfactory information, and are nearly useless for the purposes of science.

Extensive structural lesions may exist, that cannot be seen, or very imperfectly discerned by the unaided sight, and without proper preparation.

It was determined by the Committee that the intestines, before submitted to examination, should be finely injected, and subsequently inspected with a microscope.

This task was undertaken for the committee, by Dr. John Neil, Demonstrator of Anatomy in the University of Pennsylvania. The admirable manner in which he has performed this duty, can be judged of by the beautiful preparations now on the table, which he has presented to the College for its Museum.

The injections are made with turpentine colored with vermilion. It was found by Dr. Neil, that when he employed size, it did not penetrate well, and numbers of capillaries were not filled; the same result occurred when Canada Balsam was used. It led, at first, to the supposition that the capillaries were destroyed by the disease.

The Committee, confining themselves strictly to the single object for which they were appointed, report the following facts as the result of their investigation.

1st. In the recent subject, the peritoneal coat, like all the serous membranes, was in all, remarkably dry. The lubricating serosity is deficient in the serous membranes.

2d. The epithelial layer of the intestinal mucous membrane, was, in all the specimens, either entirely removed, or was detached, adhering loosely as a pulpy layer, mixed with mucous, or an albuminoid substance.

3d. *Peyerian Glands.* Peyer's Glands, were developed to a greater or less extent in all the cases examined.

4th. *Solitary Glands.* These were also developed, and contained, in the recent subject, a minute quantity of white substance. These enlarged solitary glands have the appearance designated by Serres and Nonat, as *Psorenterie*.

The villi covering the gland of Peyer, and the solitary glands, present the same appearances as in other parts of the same intestine.

5th. *Villi.* They are denuded of the epithelial covering, but are unchanged in other respects.

6th. *Capillary Vessels.* These are entire, and manifest no departure from their normal state. The appearances of the capillaries of a cholera intestine, are identical with those of the healthy mucous membrane when the epithelium has been removed. In the natural state, the epithelium, from its thickness, conceals the injected capillaries.

In no instance was a vesicular eruption observed. In some of the dry specimens, there is an appearance that might be mistaken for it, but it is an emphysematous state, resulting from commencing putrefaction.

The foregoing facts are derived from the examination of twenty-five subjects.

SAMUEL JACKSON, M. D.
JOHN NEIL, M. D.
HENRY H. SMITH, M. D.
WILLIAM PEPPER, M. D.

Trans. College of Physicians, Philadelphia.

THE TREATMENT OF EPILEPSY.—DR. CHENEAU has presented to the Academy of Sciences, Paris, a memoir on the treatment of epilepsy, which is well worthy of perusal. He desires to prove that the disease is curable by medicine, resting his principal mode of arresting its progress upon the judicious employment of digitalis. The cases that have proved the most inveterate have yielded to a perseverance in the use of this remedy for a period of six or eight months. He has submitted six instances to the Academy, in which he has been successful; the first occurred at the Bicetre, under the care of Dr. Voisin. A young man, aged twenty, had been epileptic, it is supposed from fright, from his fourteenth year; two months treatment restored him to health. The second was one in which the disease, produced at the age of thirteen, by fright, had lasted till the individual had attained his forty-second year. The treatment commenced the 20th of April, 1847, and after the 16th of June in the same year he had no attacks, which before had appeared, it is true, at only very long intervals; as many as six years having at one time elapsed without any access of paroxysm. The third case is certainly one as singular as any that has been registered upon the rolls of medical science. A young lady, thirteen and a half years old, had been subject for several years, to the disease, which had at length brought on idiocy and paralysis of half the body; the paroxysms were not very frequent, but were of great violence, frightening the persons who nursed her, the countenance wearing a purple and almost a black hue, which sometimes lasted twelve hours after the fit had ceased; the hemiplegia, which was of the right side, prevented the movement of the limbs, and partially affected sensation. The treatment was commenced on the 4th of July, 1846, and by the month of January in the following year, the epileptic fits altogether ceased. A year has elapsed since the young lady has been able to go on with her education, and she is also able to run in the garden and to amuse herself with gymnastic exercises. The fourth noticed is that of a young girl of ten years of age, upon whom epilepsy supervened after fright; it had lasted two years, but soon yielded to the usual remedy of Dr. Cheneau. The fifth case was that of a patient in the Bicetre; he was, when placed under the treatment, sixteen years of age, and had been afflicted by the malady since he was five years old. At first, the paroxysms were but slight; he suddenly turned himself mechanically round any object immediately in his neighborhood; this lasted about a minute. As the disease advanced, he had convulsions, and the fits became very frequent. The remedies were commenced on the 2d of October, 1847, and in January, in the following year, was his last

attack. Since that time his health has become perfectly established. The last case carries with it the same interest as the preceding ones. A boy, of ten years of age, after having been for three years subject to frequent fits, once as many as fourteen in twenty-four hours, was subjected to the use of the digitalis, and at the end of two months was an instance of the excellence of the system pursued by Dr. Cheneau. The essay is well written, and certainly demonstrates that digitalis, properly combined, has cured epilepsy, even when complicated with paralysis and idiotism; that the cure has not been confined to youth, but has been decided even at an advanced age, and that the time occupied has not been of great duration. An incidental subject of discussion has been the subject of the sudden paleness of the face, which has been noted down by the nosologists as one of the symptoms of epilepsy. One of the characteristics, as given by Georget, in the "Dictionnaire de Médecine," is, "extreme pallor of the face, suddenly coming on towards the end of the fit, succeeding to a redness more or less intense which previously existed." In the hospital of the Bicetre, one hundred and twenty fits have been watched with the most scrutinizing care, and this state has never once presented itself; on the contrary, the redness came on during the paroxysm, continued throughout the convulsions, and often lasted for some time afterwards. On no one occasion was the sudden paleness observed.

M. DELASIAUVE, one of the physicians to the Bicetre, is investigating with great attention the resources which we possess for the cure of epilepsy; his observations on the employment of sedatives are of considerable value. To valerian he gives the first place, though he does not speak of it quite in as sanguine language as did Tissot; a decoction of valerian given in doses of two wine glasses full, morning and evening, have produced a radical cure, but the medicine requires to be persevered in for a considerable length of time, otherwise it is of little avail; assafetida decidedly moderates the violence of the access, but it does not seem to produce the same permanent good effect. The hydrocyanate of iron is found in some instances very beneficial; belladonna and digitalis are each serviceable, but Pluvrey of Lille prefers a combination of the two. The root of artemisia is occasionally useful; of liquid ammonia, according to the formula prepared by Martinet, he has some good reason to speak, and will shortly give the results of his experience; camphor is found in those cases where the reproductive system is in a high state of excitement, as not unusually occurs in epileptics, to be of remarkable service; zinc, musk, castor, ambergris, have but little curative power; preparations of copper are to be but little confided in; nitrate of silver has lost the high character it once obtained; sulphate of quinine has also fallen into disrepute. He enters very minutely into the subject of the diet and exercise of the patient, preferring vegetable to animal food. It is to be regretted that the series of papers which this observant practitioner had prepared for the press are for the present suspended, for want of that encouragement which would have been at another period given to inquiries of such deep moment.—*Journal of Insanity.*

HEREDITARY INSANITY.—M. BAILLARGER is engaged on a series of "anatomical, physiological, and pathological researches upon the nervous system," but he has not yet completed the work, nor, in the present state of

psychological science at Paris, is there much prospect of it ; a translation into English, doubtless, would be eagerly read by the profession at large. His observations on the development of the brain form a remarkable addition to the knowledge we have obtained through Reil, Teidmann, and Desmoulins ; he arrives at the conclusion that this organ is developed from within, and is increased by introsusception. His observations upon the hereditary nature of madness are invaluable ; they are the result of a long series of indefatigable labors, and show how much he is in earnest in his inquiries after facts. He chose the three following questions for solution :

Is the madness of the mother more frequently transmitted to the child than that of the father ?

In the case of hereditary madness, is the disease of the mother transmitted to a greater number of children than a similar malady in the father ?

Is madness more often transmitted from the mother to the daughter—and from the father to the sons ?

No less than 600 cases have been investigated by him for the purpose of arriving at some distinct conclusions. It would be difficult for any one but an individual so advantageously placed as is Dr. Baillarger, at the Bicetre, to have been able to collect such a quantity of statistic details as to have furnished him with proper data, for there are no documents existing which could be of much service ; the only report of the kind being one which was made by Aubonel and Thore, at the Bicetre, and this was of so limited a nature as to be of comparatively little value. From Dr. Baillarger's cases the result was, that of 453 insane individuals, 271 had become so through the mother, if such an expression may be allowed, and 182 through the father, answering the first question by a large proportion passing through the female side.

The second question was resolved from 271 families, in which the mother had transmitted the disease—in one infant in 203 cases ; in two infants 62 ; in three, five cases ; in four infants, one ; that is to say, that in one-fourth of the instances the mother's disease was shown in more than one case ; out of 182 families in which the disease descended from the father, a single child was diseased in 152 cases ; two children, 36 times ; three infants, four times ; that is to say, that the madness appeared only in one-sixth of the instances, proving that the mother's malady affected the greater number of the descendants.

The third question, whether the mother more frequently transmitted the disease to the girls, and the father to the sons, out of 346 children, he found that from the mother's side 197 girls, and 119 boys were affected ; and from 215, where the father had been the original source, 128 boys and 87 girls were affected, showing that the madness of the mother more frequently exhibited itself in the females than in the males ; whilst, on the contrary, the madness of the father showed itself in the proportion of a third in the boys over the girls. Many are the physiological deductions which Dr. Baillarger has drawn from these inquiries, which, when the whole of his invaluable work is finished, will throw a great light upon the subject of hereditary madness.—*Journal of Insanity.*

EDITORIAL DEPARTMENT.

Meeting of the American Medical Association.—The third annual meeting of the *American Medical Association* was held at Cincinnati on the 7th. ultimo. The session continued four days, about three hundred and fifty delegates and members (by estimation) being in attendance, representing twenty-four states of the Union. The interesting character of the proceedings, will not, we are sure, suffer, in the minds of those present, by comparison with any of the previous meetings. We are not prepared to present our readers with a complete report of the proceedings. The accounts published in the daily papers are too meagre and inaccurate to be available, and we must therefore await the publication of the volume of Transactions, which, it is to be hoped, will appear more promptly than the volume for the last year. It will also obviously be more appropriate to defer notices of the reports of the standing committees, (some of which were very able,) until they are in print. We shall confine ourselves, now, to a brief sketch of the doings of the Association.

After an address by the late President, JOHN C. WARREN, M. D., of Boston, Mass., on taking the chair, and appointing a Committee on the claims of those proposed as "members by invitation," the Association proceeded to the business of choosing officers for the ensuing year. The mode of nominating, is by a committee consisting of one member from each state represented, the member for each state being appointed by the delegation from that state. The following officers were nominated, and elected :—

President, R. D. Mussey, M. D. of Ohio.
 1st. Vice President, J. B. Johnson, M. D., of Missouri.
 2d. do. A. Lopez, M. D., of Alabama.
 3d. do. Daniel Brainard, M. D., of Illinois.
 4th. do. G. W. Norris, M. D., of Pennsylvania.
 Secretaries, } A. Stille, M. D., of Pennsylvania.
 } N. Desaussure, M. D., of South Carolina.
 Treasurer, Isaac Hays, M. D., of Pennsylvania.

A resolution embodying a tribute of respect to the memory of the late Prof. HARRISON, of Cincinnati, one of the Vice Presidents chosen at the last meeting of the Association, was adopted in an impressive manner, *the members signifying their assent by rising.*

The newly elected officers were then inducted into office, the retiring President making a short address, which was followed by a few remarks by Dr. Mussey, on taking the Chair. A vote of thanks to the late officers was then passed.

Considerable discussion arose on the propriety of adopting more restrictive measures than the Constitution at present provides for, respecting the reception of members by invitation. None of the plans proposed, however, appearing to meet the favor of a majority of the body, the subject, at length, was indefinitely postponed.

The report on Medical Education by the chairman of the committee on this subject, Joseph Roby, M. D., of Maryland, was the first of the reports of the standing committees read. After the reading of the report, Dr. Blatchford, of N. Y., a member of the committee, submitted a series of resolutions, embracing recommendations with respect to extension of the terms of lectures in Medical schools, and clinical instruction; not contained in the report presented by the chairman. The question on the adoption of these resolutions occasioned much and animated debate, which was not terminated until noon of the third day of the session. Among those who engaged in discussions connected with this subject, were Drs. Miller and D. W. Yandell of Louisville, Ky., Annan of Lexington, Ky., J. K. Mitchell, Caspar Morris and Isaac Parrish of Philadelphia, Brainard and Davis of Chicago, Ill., Kerfoot, of Lancaster, Pa., Ware and Storer, of Boston, Mass., Wright and Rieves, of Cincinnati, O., McPheters, of St. Louis, Mo.

The subject of Medical education not only consumed more time, but it involved more exciting topics than any other of the subjects considered during the session. It was finally disposed of by referring the Report of Dr. Roby to the committee on Publication, and the Resolutions of Dr. Blatchford to the committee on Education appointed to report at the next meeting of the Association.

This result suffices to show (what must have been apparent to the majority of the members present) the inutility of devoting so large a portion of the session to deliberations on proposed improvements in Medical instruction. That the subject is one of great importance, and deserves a proportionate share of the attention of the Association, no one will deny; but it is not to be forgotten that there are other subjects which should neither be passed by, nor slighted. It is but one of the objects of the Association to legislate in behalf of those who are entering, or hereafter to enter the medical profession. To develop and diffuse information among the members of the Association, and their professional brethren, is another

object scarcely less important. And for this end, time should be allowed for the reading of all reports, and voluntary communications, on the various branches of medical science, and for discussions therewith connected. Numerous topics also, in addition to those just referred to, claim consideration, in which the honor and usefulness of the Medical profession are more or less involved. At each meeting of the Association, hitherto, the subject of Medical Education has absorbed so much time, that other matters have received less attention than justly belongs to them. We trust there may be an improvement in this particular hereafter, more especially since the discrepancy of opinion as regards medical instruction, appears to increase in proportion to the amount of discussion it receives. Under existing circumstances, it seems to us that the various questions relating to this important subject had better be argued at length in the different Medical journals, and in local associations, awarding them only a fair relative degree of consideration at the meetings of the Association.

The Report of the Committee on Practical Medicine was next read by the Chairman, Prof. J. K. Mitchell, of Philadelphia. The reading of this Report occupied about two hours. It was chiefly devoted to Epidemic Cholera.

The Report of Prof. Huston, (Chairman) on the Adulteration of Medicines, was next read.

The Report on Surgery followed, Prof. R. D. Mussey, Chairman. This occupied about two hours. Much of this report was made extemporaneously.

Dr. Stille, Chairman of Committee on Medical Literature, submitted his Report, an able and elegant production.

The Report on the Medical Sciences, Dr. Parsons, of Providence, R. I., Chairman, owing to the ill-health of Dr. P., was not read, but referred to the Committee on Publication.

The Committee on Obstetrics had not prepared a Report. Prof. Evans, of Chicago; Ill., (a member of the Committee) exhibited and explained a new instrument, invented by himself, designed as a substitute for the obstetrical forceps, of which we shall give some account in another article.

Several voluntary communications were presented, and referred to the committee on Publication, without reading. A paper by Prof. Davis, of Chicago, on the relation of the cerebellum to the sexual propensity, was read.

As already stated, we deem it more appropriate to defer analyses or critical notices of the reports and communications presented, until the appearance of the volume of Transactions.

Various resolutions were adopted during the session, on which, not having copies, we must defer comment, until an authentic publication of the proceedings is received. Among the objects embraced in these resolutions are the following ;

Recommendation to Congress of the application of Navy Surgeons for an assimilated rank in the service ; the publication by the Association of the manuscript work of the late Dr. Forrey ; memorial to Congress for laws regulating international copy-rights ; advising the introduction of the practical study of chemistry into medical schools ; appointing a committee to report on the diseases of animals ; showing the importance of encouraging native medical literature, by patronizing American text-books ; recommendation to the Profession to restrict their patronage to apothecaries not engaged in making or vending secret nostrums, and to repudiate members of the profession who give certificates of the value of patent remedies ; approving of clinical midwifery, and referring the subject to the committee on Education for the coming year. Presuming that some of our readers might feel interested in the last-mentioned topic, from the communications and articles thereupon that have appeared in our pages, we obtained an attested copy of the following preamble and resolution, which were offered by Prof. Miller, of Louisville, Ky., and adopted by an *unanimous* vote of the Association :

"Whereas clinical instruction in Medicine and Surgery is now generally acknowledged to be essential to the proper qualification of students for the practice of these branches of our profession, and whereas it must be admitted that clinical instruction in midwifery would be equally valuable—

"*Resolved*, That the committee on Medical Education be instructed to inquire whether any practicable scheme can be devised to render instruction in Midwifery more practical than it has hitherto been in the Medical Schools of the United States, and report at the next meeting of this Association."

The following are the standing committees for the present year :

Medical Science.—Dr. Bennett Dowler, of New Orleans, La., Chairman ; Drs. Fenner, of New Orleans, T. G. Smith, of Philadelphia, Uhsher, Va, Carr, N. Y., Johnson, Ala., Meeks, Indiana.

Practical Medicine.—Dr. Austin Flint, of Buffalo, N. Y., Chairman ; Drs. N. L. Corbin, Va., R. H. Davis, Baltimore, Md., H. M. Congar, Buffalo, N. Y., J. McNaughton, Albany, N. Y., Norwood, New Orleans, R. Haywood, Indiana.

Surgery.—Dr. Paul F. Eve, Augusta, Ga., Chairman ; Drs. J. N. Simmons, Ga., Gross, Ky., J. Watson, N. Y., A. Pope, St. Louis, Mo., H. H. McGuire, Winchester, Va., Palmer, Michigan.

Obstetrics.—Dr. Storer of Boston, Mass., Chairman ; Drs. Reynolds Boston, Mass., T. M. Smith, Delaware, Miller, Louisville, Ky., Parker, Wisconsin, Morrison, Illinois, Morton, Indiana.

Medical Education.—Dr. Worthington Hooker, Norwich, Conn., Chairman ; Drs. Blatchford, Troy, N. Y., J. B. S. Jackson, Boston, Mass., N. S. Davis, Chicago, Ill., Theobald, Baltimore, Blackburn, Ky.

Medical Literature.—Dr. Thomas Reyburn, of St. Louis, Mo., Chairman ; Drs. McPheters, St. Louis, Mo., Cowper, Newcastle, Del., Lawson, Cincinnati, Ohio, G. Tyler, D. C., Annan, Lexington, Ky., Thomas, Tenn.

On Publication.—Dr. Isaac Hays, of Philadelphia, Chairman : Drs. Alfred Stille, Philadelphia, Dunbar, Baltimore, D. F. Condie, Philadelphia, Isaac Parrish, Philadelphia, S. DeSausure, Charleston, S. C., Sanborn, N. H.

The city of *Charleston, S; C.*, was designated as the place of meeting, on the first Tuesday in May, 1851, and the following committee of Arrangements appointed :

Dr. Frost, Charleston, S. C., Chairman ; Drs. P. C. Gaillard, Charleston, S. C., J. P. Jervey, Do., DeSaussure, Do., Jebby, Do., Wragg, Do., Cain, Do.

The above enumeration of members of the committees is copied from a report in one of the daily newspapers of Cincinnati, and may contain errors, which, when ascertained, we shall correct.

As before stated, the late Association at Cincinnati will compare favorably as respects the interesting character of its proceedings, with any of the previous meetings. The harmony and good feeling that have characterized the deliberations hitherto, were not less conspicuously displayed on this occasion. Differences of opinion were freely expressed, but with due regard to propriety and gentlemanly courtesy ; and although the tone of discussion was often far from being tame and spiritless, it was generally free from personalities and asperity. Members were not disposed to extend the licence of debate to the latitude of licentiousness ; and hence, the violations of strict order and decorum were few in number. The measures adopted by the Association as a legislative body, show a temperate, conservative disposition. The scientific reports are of such a character that the volume of Transactions for this year will continue to be a fair exponent of the progress of American medicine. And, altogether, the claims of the Association upon the deep interest and fostering care of the Medical Profession of this country, will be found to be in no wise diminished.

But the advantages of these annual occasions for assembling medical practitioners from all parts of the United States, are by no means limited to the formal proceedings, and the published reports. The social inter-

course of the members with each other, and with their medical brethren in the place of meeting and its neighborhood; the formation and renewal of friendships, and the opportunity of seeing, hearing, and knowing many with whose names and writings we were already familiar, render these occasions as delightful as they are improving. We were at some pains to ascertain the sentiments of our brethren who had come from distant States to attend the meeting at Cincinnati, and we found none who did not express themselves more than abundantly repaid for the loss of time, and the expense, and the fatigues, which their attendance required, by the gratification derived from the sources just mentioned.

The local popular influence exerted by these meetings in the places at which they are held, is to be included among their beneficial results. To witness such a body convened for objects of science and philanthropy, cannot but impress the minds of citizens in a favorable manner, and must tend to elevate the medical profession in the respect and estimation of the community. On this account, it is desirable that the annual meetings shall be held on each year at different sections of the country, as the Constitution provides; and it is not to be doubted that the moral effect produced in all places in which they are held, render it a matter of policy, as well as pride, for different places to compete for the honor of being selected. The position of Buffalo is such, that after the next annual meeting at Charleston, S. C., it may present strong claims for preference, and we hope that our brethren in Western New York will not fail duly to appreciate the importance of so desirable an event.

In closing this imperfect sketch, it remains to allude to the judicious and liberal arrangements, the social attentions, and the generous hospitalities of our brethren of Cincinnati. To be fully appreciated, these must have been experienced. To those who were present, it were needless to attempt to express sentiments of appreciation, for description could not add to what is already felt by all. Were we able to do justice to this theme, it would tend to enhance in no small degree, the sense of disappointment of those who were unable to be present. To the latter, therefore, we will simply tender our condolence. They who were privileged to participate in the Session of 1850, will ever preserve happy and grateful recollections of the chairman of the committee of arrangements, (Dr. Drake,) and his compeers, in the Queen city of the West.

Transactions of the Medical Society of the State of New York, during its Annual Session, held at Albany, February 5, 1850. 8vo. pp. 276.

This volume embraces, first, several reports and voluntary communications made to the Society, and referred to the publishing committee; and, second, the proceedings at the annual session, in February 1850. Of the former we will proceed to give an enumeration, with a few comments. We have neither leisure, nor space for analyses or extended criticisms. First in order is the address by the late president of the Society, Alexander H. Stevens, M.D. The subject selected for the occasion was the *Public Health*—a subject peculiarly appropriate, inasmuch as the address was delivered at the capitol, in the Hall of the Assembly, a large portion of the members of the Legislature being present. Appended to the address are some remarks on “the sanitary construction of country dwellings, ventilation, drainage, etc.” The address and the appendix contain some valuable suggestions with which it were desirable that the public should be familiar. In treating of public health the author confines his view to the *financial* aspect of the subject, presenting considerations to show how, and in how far, the wealth of the State is affected by ignorance and neglect of the laws regulating the physical welfare of its population.

Art. II. *On the Communicability of Asiatic Cholera*, by ALEXANDER H. STEVENS, M.D.

With due deference to the learned author, we must say that, in our opinion, this is a weak defence of the doctrine of the contagiousness of cholera; but the subject appears to us hardly to admit of very powerful argumentation. We designed, when we first read this article, to review it at length, and discuss fully the subject of which it treats; but we must defer carrying out this intention for some other occasion. We will not, however, forego briefly noticing the manner in which Prof. Stevens states the logical position of the doctrine of non-contagion. We quote the two first paragraphs of the paper as follows:

“Two theories, and two only have been conceived to account for the existence of the Asiatic cholera. It is the design of the following paper to examine each of these in detail, and to endeavor to decide to which of them truth belongs.

“According to the first theory, the disease is caused by a peculiar state of the atmosphere extending over vast regions, imperceptible to the senses, and giving no evidence of its existence by statical, chemical or medical tests, or indeed in any other way than in the causation of the cholera. The starting point of the doctrine is therefore a double hypothesis. It *assumes* without proof: 1st. That this peculiar state of the atmosphere

exists. 2d. That it is the cause of cholera. These propositions have no other support than that which they derive from each other. Cholera exists, therefore there is a peculiar state of the atmosphere. There is a peculiar state of the atmosphere, therefore cholera exists. This is palpably only reasoning in a circle."

Now, in the first place, it is far from being the case that "two theories only have been conceived to account for the existence of the Asiatic cholera." To attribute it to a peculiar state of the atmosphere, is not the only alternative, provided the doctrine of contagion be repudiated. Various hypotheses have been suggested which do not involve states of the atmosphere. But without enlarging on this point, what non-contagionist ever reasoned in the way he is represented as reasoning in the foregoing extract? Who ever assumed that a peculiar state of the atmosphere exist, and, ergo, cholera exists; and who ever argued from the mere existence of cholera that there is a peculiar state of the atmosphere? We might say that this statement is unfair, but it is too nonsensical to complain on that score. The question is simply this, do observations and reasoning sustain, or do they disprove the supposition that cholera is diffused by contagion? This question, in the present state of knowledge, has nothing to do with inquiries as to the most probable theory of causation, provided the communicability of the disease be disproved. To render the attitude of those who disbelieve in the contagiousness of cholera ridiculous, a more ingenious artifice is requisite than is exhibited in the passages we have quoted.

III. *An Historical Sketch of the State of Medicine in the American Colonies, from their first settlement to the period of the Revolution.*
By JOHN B. BECK, M.D., etc.

This is an interesting and valuable contribution. We could wish it were more full. It is gratifying to see that attention is being directed to the subject of American medical history. Prof. N. S. Davis, of Chicago, Ill., is engaged in preparing a work on this subject, which is to appear in successive numbers of the *Northwestern Medical and Surgical Journal*. He is fully competent to the task, and will render an useful service if he perseveres to the completion of the undertaking.

IV. *Contributions to the vital Statistics of the State of New York.* By
LEMUEL SHATTUCK, Esq., of Boston, Mass.

Mr. Shattuck (whose name is identified with the subject of vital Statistics) presents in this paper a series of tables, exhibiting (a) The number of inhabitants in the several counties, enumerated in the National censuses of 1820, 1830, and 1840; and in the State censuses of 1825,

1835 and 1845. (b) The area of each county in square miles and square acres; and the number of inhabitants to the square mile in 1825, 1835 and 1845. (c) The number of births in the several counties returned in the State censuses of 1825, 1835 and 1845; and their relation to the population. (d) The number of marriages in the several counties, returned in the censuses of 1835 and 1845; and their relation to the population. (e) The number of deaths in the several counties, returned in the censuses of 1825, 1835 and 1845; and their relation to the population. (f) The population of the several cities in the State in 1820, 1830 and 1840; and in 1825, 1835 and 1845. (g) The number of births, marriages and deaths, returned in the State censuses of 1825, 1835 and 1845; and their relation to the population in the several cities of the State.

Mr. Shattuck, from the investigation of the censuses upon which these tables are based, arrives at the following conclusions respecting the statistical data attained in that way:

"The result of this examination has led to the conclusion that these census returns are not reliable for accuracy. The design for obtaining them was excellent, but it was not carried out in truthfulness; and the last attempt was the greatest failure. The same results have followed similar attempts elsewhere.

"My own knowledge in these matters, strengthened by this examination, has led me to conclude that full and accurate returns of this kind cannot be obtained by personal inquiry, as these were obtained, after the expiration of a year. Owing to the locomotive habits of our people, causing frequent removals from one place to another, and from other causes, many births, marriages and deaths, will escape notice, and render the returns defective. The only effectual mode of preventing errors in matters of this kind is such a plan of public registration as will secure the record of every birth, every marriage, and every death, with all the important facts in relation to each, *at or near the time in which they occur*. Such a system of registration should be the foundation of all sanitary laws. Without it no accurate information concerning the population, or its sanitary condition can be ascertained.

"Some persons have desired that an attempt should be made, in the census of the United States for this year, to obtain the number of deaths. The results here presented will show the probable value of the information which might be obtained in this way. I have doubted the expediency of such a measure for two reasons; one because the information when obtained could not be relied upon for accuracy; and another because, if it could be accurately obtained for the year 1849, (that being a very sickly year) it would give an unfair exhibition of the average sanitary condition of the country. A strong desire was, however, expressed by the Census board of Washington, that measures should be provided for obtaining the information; and a separate schedule was prepared, at their request, for the purpose."

V. *Annual Address, delivered before the Albany County Medical Society, November, 1749.* By JAMES McNAUGHTON, M.D., President.

The subject of this address is *Epidemic Cholera*. It is mainly a report of the disease as it prevailed at Albany. The topographical situation of cases as they successively occurred, are given, in order to determine what bearing these *facts* have upon the question of the diffusion of cholera by contagion. Prof. M. regards the evidence as conclusive against the opinion that the extension of the disease is dependent upon contagion; but he is not fully satisfied that it is not sometimes so communicated. He gives some instances of *apparent* contagion, or in which the disease seemed to be carried by some *portable*, special, morbid agent. Such instances, it will not be denied, generally occur, whenever, or wherever the epidemic prevails.

The whole number of cases occurring at Albany in 1849, which were reported to the Board of Health, was 8318—number of deaths, 334. Prof. M. states that in many cases of cholera which fell under his own observation, the diarrhoea was preceded by a *moist white tongue*, which he had observed as a striking characteristic of the disease in 1832. He deems this symptom important "as a means of knowing when the epidemic influence threatened the system with an attack, even earlier than the premonitory diarrhoea gives warning."

VI. *Report of the Standing Committee of the Society on Hygiene, or medical statistics.* By CHARLES A. LEE, M. D., Chairman.

Prof. LEE states that no assistance having been rendered him by those associated on the committee, and but few replies having been received to queries addressed to members of the profession in different parts of the State, he has come to the conclusion that the only method of collecting a body of facts which will serve any useful end, is that adopted by the Commonwealth of Massachusetts, viz. the sanitary survey of the state, performed by Commissioners appointed and remunerated by the state. He therefore confines himself to a statement of some of the considerations showing the great need of such a survey. These considerations are very forcible, and should be appreciated more fully than they are, by the medical profession, as well as by the public. To both, the attention of the brief report of Dr. LEE may be commended. He presents a striking contrast between the provisions for the sick poor in Great Britain, and in some portions of this country. The instance related for illustration, from the latter is, (we are sorry to say) the Erie County Poor-house, as it was not long ago. We trust there is some improvement since. In conclusion

Prof. LEE submitted the manuscript work by the late Dr. FORRY, of New-York, (committed to his hands by the talented author shortly before his decease,) in the hope that the Society might secure its publication by incorporating it in their transactions. The Committee on Publication, as we are informed, were desirous of so doing, but it was found that the size of the volume of Transactions would be thereby increased, so as to jeopardize its being issued at the expense of the State. It is to be hoped that the work will appear in the next volume of the Transactions of the American Medical Association, a vote to that effect having passed, with the proviso that the pecuniary resources of the Association are adequate to that end.

VII. *On the Vital Statistics of the city of Brooklyn.* By CHAS. S. J. GOODRICH, M. D.

The whole number of deaths from cholera in 1849, was 650 ; 500 adults, 150 children ; 325 males, and 325 females. Of the adults 75 were natives of the United States, 391 from Ireland and Germany, and 34 from other countries.

VIII. *Medical Topography of the County of Montgomery.* By JOSEPH WHITE, M. D.

Dr. WHITE states that the inhabitants of Montgomery County, as a whole, are less afflicted with *consumption* than the adjoining counties, which are more elevated ; but one village in the County presents a striking exception to this remark. We quote his description of the place referred to, as follows :—

“ There is however one exception to the exemption or less frequency of consumption in Montgomery county. In the south-west part of the county, about three miles north of Sharon Springs, is the village of Ames, containing about 250 inhabitants. It is situated upon the edge of a flat, which at this place is about one mile in breadth and formed by the junction of Sulphur Creek, which has its origin at Sharon Springs, with Canajoharie creek. These flats are somewhat swampy, (there would be no serious difficulty in draining them,) and are quite subject to fogs. There is a mill-dam across Canajoharie creek a mile and a half below the village, which sets the water back about one mile, yet it flows but little more than the natural channel of the stream. There are also upon Sulphur creek, within one mile of the village, three small dams, which flow from one to three acres of ground. The size of these ponds would seem to be too small to influence very materially the atmosphere of their vicinity. All the wells and springs in the village are slightly sulphurous, and highly charged with lime in solution, being supplied, probably by the Sulphur creek above mentioned, which runs through the village.

It is asserted by the inhabitants, and I have no doubt truly, that at least one half of all who die in that village, die of consumption, and yet, one mile, or a mile and a half from the village, the inhabitants are as free from consumption as in any section of the county. The residents of the place are mostly farmers of English descent, whose habits are like others of the same occupation, frugal and industrious, residing in dwellings as comfortable as farmers in good circumstances usually have.

IX. *Analysis of the Byron Acid Spring, near Batavia.* By Dr. GEORGE HAND SMITH, of Rochester.

This spring contains free sulphuric acid in a large proportion, 59,397 of 100,000, and 82,022 parts of proto sulphate of iron and alumina. Its medical powers are stated to be highly tonic, refrigerent and astringent.

Several springs analagous in the composition of their waters, are found in this neighborhood.

X. *Report of a case of premature labor, artificially induced, etc.* By J. W. BLATCHFORD, M. D.

(See Eclectic department of this No. of this Journal.)

XI. *Semi-annual address on Erysipelas, delivered before the Albany Co. Medical Society, in June, 1849.* By JOHN SWINBURNE, M. D., Vice President.

The subject of this paper is to establish the identity of Epidemic Erysipelas, diffuse cellular inflammation, puerperal fever, and phlebitis. The author thinks that the specific characteristics of each depend upon the texture affected, the essential morbid condition being the same. He advocates the contagiousness of puerperal fever. He relies, mainly, for the support of these views, on facts cited from various authors.

XII.—*Case of Poisoning by Corrosive Sublimate.* By BENJ. W. MCCREEDY, M. D., of New York.

This is an interesting case, which we reserve for the Eclectic department of a future No. of this Journal.

XIII.—*Notices of the Cholera at Newark, in 1832.* By JOHN S. DOREY, M. D., in a letter to ALEXANDER H. STEVENS, M. D.

The statements contained in this paper are presented as tending to show the contagiousness of Cholera. The author gives his recollection of events that transpired eight years previous. With a strong bias in favor of the doctrine of contagion, and with a convenient looseness of expression, it may still be questioned if the narration, on the whole, does not go to disprove the communicability of the disease.

XIV.—*Notices of the Cholera at Rockaway in 1849.* By JULIUS AUERBACK, M. D., in a letter to ALEXANDER H. STEVENS, M. D.

This short paper, as well as the foregoing, was communicated by Dr. Stevens, as containing *facts* in support of the contagiousness of Cholera. The only events which appear to bear on the question of contagion, are, that the first case in Rockaway occurred in the person of a Captain Hawser, captain of a small vessel plying between New York and Rockaway, that he was attacked the day after his arrival from the city, and that he had in his vessel household furniture and beds belonging to a man "*whose wife had died shortly before in New York, and was brought to this place for interment.*" (This quotation is italicised in the work.) Several cases subsequently occurred, but the report gives neither respective dates, distances of situation, nor any adequate evidence of the intercourse of the patients affected.

We cannot refrain from expressing surprise that such a paper should be seriously submitted as containing any authenticated facts relative to the question of the contagiousness of Cholera. It seems to us better calculated to bring ridicule on the affirmative of that question.

In so far as the passage italicised is concerned, the facts contained in it are opposed to the hypothesis of contagion. A captain of a vessel direct from a choleraic atmosphere, is attacked with cholera. Some household goods of a person whose wife had lately died with the disease, were on board. The body of the deceased lady had shortly before been brought from the city to Rockaway for interment, but no cholera followed. The captain of the vessel alone was attacked, while the crew, who, it is reasonable to suppose, had been brought into closer proximity with the suspected *fomites*, all escaped. The son of the captain is attacked simultaneously with the father, but, as it is not stated that he was on board the vessel, it may be presumed he was not, although if he were, it would not affect the question. Finally, both patients recover, so that there is room for incredulity as respects the diagnosis. We are sure that only a contagionist whose preconceptions prevented him from seeing aught else than what appeared to favor his peculiar bias of mind, could discern in this summary of events, anything deserving to be dignified with the name of evidence.

XV.—*On the Morbid condition of the Generative Organs, being a paper read before the Chenango Co. Medical Society.* By WM. D. PURPLE, M. D., of Greene, Chenango Co., N. Y.

This communication contains the history of cases which, we agree with

the writer in thinking, are too much neglected and overlooked by the medical profession. We have long been satisfied that the abuse of the sexual function, and especially its unnatural excitation, is the true source of not a few of the maladies which physicians are called upon to treat in young persons of both sexes, and which are not thus referred. We are fully sensible of the delicacy and difficulty of tracing disorders to this source in individual cases, and hence it is that the attempt is not made, or, if made, frequently proves unsuccessful. The physician, to discharge his duty in this particular, must not be restrained by false modesty, the fear of giving offence, or of wounding the feelings of patients or friends by an unfounded imputation; nor must he remain satisfied with a tame, superficial enquiry. He must question directly and plainly, cross-question, and persist until he believes he has arrived at the truth. Were this course faithfully pursued, our art would accomplish, we are persuaded, much more for the moral and physical welfare of our patients than it does at present. Many affections that now tire the patience of the practitioner, and bring opprobrium on the art, would be found to be speedily remediable. This is our belief, based, we are free to confess, chiefly on the observation of cases in which our suspicions have exceeded our knowledge. It is proverbially easier to preach than to practice, and, in this instance, we are sorry to say that we cannot enforce our precepts by reference to our own professional observations.

XVI.—*Account of an Operation for the removal of an Ovarian Tumor.*
By ALDEN MARCH, M. D., etc.

See Eclectic department of this number of this Journal.

XVII.—*Observations on various subjects in Forensic Medicine.* By JENKS S. SPRAGUE, M. D.

The greater portion of this paper is devoted to an account of two cases of persons found dead under circumstances giving occasion for the suspicion of their having been murdered, which, in the opinion of the writer, received inadequate investigation from the professional gentlemen in attendance. In connection with this opinion, the writer takes occasion to indulge a fling at the medical school, at which one of the incompetent medical gentlemen particularly referred to, had lately graduated. This may be in excellent taste, but, if so, our judgment is greatly at fault. It is any thing but a good sign, in our humble estimation, to recount the faults, or deficiencies, of our neighbors. It may argue a want of charity, but we cannot but think that not a few of those who are constantly prating of the igno-

rance and incompetency of their professional brethren, would be better occupied in self-improvement.

Dr. Sprague advocates the appointment of a suitable number of medical examiners in every county, selected with reference to their fitness and acknowledged qualifications for medico-legal examinations. This proposition, which was first made, if we recollect aright, by Prof. Stevens, of N. Y., strikes us as a good one, and we should be glad to see it adopted by the State.

XVIII. *Brief Notices of the Medical Topography and Diseases of Washington county.* By HIRAM CORLISS, M.D.

Dr. Corliss details, briefly, several cases of epidemic erysipelas. Every case treated by him recovered. His treatment consisted of quinine, morphia, and wine.

XIX. *Notices of Deceased Members.*

This embraces a short biography of the late Peter Wendell, M.D.; a sketch of the life of the late Dr. A. Brigham, by Prof. Coventry, which has appeared in the pages of this Journal; and a notice of the decease of Dr. Joseph A. Gallup, of Woodstock, Vt., an honorary member of the State Medical College.

Of the proceedings of the Society we shall give some account, in addition to the synopsis formerly given (No. for March, 1850), in our next number.

A Systematic treatise, Historical, Etiological, and Practical, on the principal diseases of the interior valley of North America, as they appear in the Caucasian, African, Indian and Esquimaux varieties of its population. By DANIEL DRAKE, M. D. Cincinnati, Winthrop B. Smith & Co., publishers, Philadelphia; Grigg, Elliott & Co; New York, Mason & Sand, 1850. 8 vo. pp. 878.

This large volume is the first of a series, in the collection of materials for which the distinguished author has been many years engaged. The general character of the enterprise is expressed by the title-page, but to appreciate fully the comprehensiveness of its scope, and its specific objects, it is necessary to examine the work. By the expression "interior valley of North America," is designated an area estimated to embrace about six millions of square miles, constituting three fourths of the entire continental surface, beginning within the tropics, and terminating within the polar circle; bounded on the south by the Gulf of Mexico, on the north by the Polar Sea and Hudson Bay, on the east by the Appalachian, and on the

west by the Rocky Mountains. After noting the prominent aspects of this vast territory, sketching its hydrographical features, &c., the seas, lakes, rivers and valleys which it embraces, and describing its geological outlines, the author divides the great interior valley for convenience of topographical description, into four hydrographical portions :—1st. the southern or Mexican ; 2d. the Eastern Lake, or St. Lawrence ; 3d. the Hudson ; 4th. the Arctic or Polar. The greater portion of the volume is devoted to investigations of the various conditions, geological, hydrographical, topographical, climatic, social and physiological—of these four divisions, and of the diseases standing in relation to these conditions. The purpose of the author is to study man in connection with the physical circumstances, and the moral influences appertaining to geographical situation, within the limits of the interior valley of North America, and to ascertain to what extent, and in what mode, the diseases to which he is subject are produced, or modified by these circumstances and influences. In so far, the inquiry is historical and etiological. It also enters into the plan of the author to consider, at length, individual diseases, discussing their causes, diagnosis, pathology, and treatment. About one hundred and sixty pages of the present volume are occupied with the consideration of periodical fevers, which the author prefers to designate *Autumnal fevers*.

It will be perceived, if we have succeeded in briefly explaining the plan of the work, that the design is at once grand and philosophical. There are few men of sufficient hardihood to commence such an undertaking, and still fewer of sufficient perseverance to carry on successfully the labors which its prosecution has involved. A large share of the facts collected by Dr. DRAKE, have been obtained by personal explorations—visiting the prominent points in the vast territory over which the survey extends, examining for himself, and gathering details by communication with responsible individuals. Years have been consumed, and thousands of miles annually traversed in pursuit of the vast collection of data which he has now presented to the medical public. To obtain these data, and to arrange them for the induction of truth, requires something more than hardihood and perseverance, viz.—discrimination, judgment, knowledge. If we mistake not, the work will show, what before would not be denied, that the author possesses those higher qualifications, without which energy and industry were of little avail. Not professing to have given so voluminous a work a thorough, careful perusal, much of it being designed for reference, and of special interest only to persons residing in other portions of the Continent—still less, assuming competency to judge of its merits, or de-

merits, in many particulars, we do not hesitate to avow our belief that its publication is an event of no small importance in the progress of our national medical literature, and one which will make the name of **DRAKE** ever memorable in the history of American medicine. As a native production, we hail its appearance with gratification and pride. We trust our brethren over the Union will evince their appreciation of the undertaking by promptly exhausting the edition of this volume, and, in that way, encourage the continuance of labors, for which, and for the satisfaction of seeing the work completed, we sincerely hope, the life and health of the distinguished author may be spared.

Demonstrative Midwifery.—The following note has been handed to us with a request that it be inserted in this Journal. We comply with the request with pleasure, reserving, for a subsequent No., a review of the correspondence contained in the March No. of the last volume of this Journal, and a discussion of the merits of the subject in general. In connection with a future article on the subject, we shall present our readers with the opinions expressed by contemporaneous medical journals, respecting the "innovation" of connecting demonstrative with didactic instruction, in the department of Obstetrics; and respecting, also, the letter contained in our March No., rebuking the practice pursued at the Medical Department of the University of Buffalo. In the letter accompanying the following note, it is stated that the letter expresses the *unanimous* opinion of the profession, so far as Racine is concerned.

RACINE, Wisconsin, May 10th, 1850.

PROF. J. P. WHITE—

Dear Sir: The undersigned, practising Physicians in this city, having perused the correspondence relative to the introduction of demonstrative Midwifery in the University of Buffalo, take pleasure in expressing to you their unqualified approbation of the course pursued in the department of Obstetrics.

Allow us, Sir, to hope and expect, that the work so nobly begun will be steadily prosecuted, until the medical student shall have ample opportunity to qualify himself in *all* the duties of his profession.

WM. WATKINS,
AUG. H. HANCHETT,
B. B. CAREY,
JOSEPH B. TALCOTT,
W. WADSWORTH,

P. LAURENCE PAGE,
SAM'L W. WILSON.
E. JAMIESON,
EDWARD EVERITT,
S. H. GRAVES.

AS The notices of the meeting of the American Medical Association, and of the New York State Society, have excluded a notice of an address by Prof. Coventry, and several other articles prepared for the editorial department, which will appear in our next No.



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NO. 2.

ORIGINAL COMMUNICATIONS.

ART. I.—*Clinical Lecture on the Semeiology of the Eye.* By CHARLES A. LEE, M. D., Prof. of Materia Medica, and Pathology, in the Medical Department of the University of Buffalo.*

GENTLEMEN:—I shall occupy the present hour with some remarks on the Semeiology of the Eye, to which I invite your usual attention.

The disturbances which take place in the action of the external senses may depend upon an altered state of the organs themselves, or they may be symptomatic of an affection of the brain, or of some distant organ. For the integrity of their function, they depend on the healthy state of the brain; that is to say, a diseased condition of this organ, is incompatible with their perfect soundness; they may, it is true, be perverted, or destroyed, by disease set up in the organs themselves. This is probably more often the case with the organ of hearing than with that of sight. Their function also may be interrupted by a disease of the nerves, which convey the impressions to the brain. Now, which of these four causes, are concerned in individual cases of deranged function, must be judged of from the accompanying signs, and from the absence of those usually connected with the other causes. As means of prognosis, those signs indicate,

*It is possible that the phraseology of the following lecture, prepared some time since, may, in a few instances, have been borrowed from other sources, and not marked as quotations at the time; and the authorities have now escaped from recollection. This statement is made to obviate any charge of plagiarism.

of course, most danger, which proceed from the brain itself; and those are least dangerous which proceed from the organs of the senses, or from other remote organs, as dilated pupil, squinting, &c., from the irritation of worms in the intestinal canal.

The functions of the senses may be altered in *four* different ways. They may be *exalted*, *diminished*, *suspended*, or *depraved*, and all these changes may occur from either of the above causes.

You will often find an exaltation of the functions of all the senses, in hysterical and hypochondriacal states, and you will find hearing and sight exalted in the commencement of cerebral diseases, and as generally diminished towards their termination; in all acute diseases, it is a good sign if the senses continue in their natural state; or if their functions have been disturbed, that they have again become normal at the time of the crisis, and after it.

There are various changes which take place in the eye, and the faculty of vision, which are important in a semeiological point of view. The eye may undergo a change with regard to its volume, its color, its position, its form, its brightness, its motions, its power of seeing, its internal structure, and its expression. *Morbid enlargement of the eyes*, may arise from *congestion* in the organ, from *hydrophthalmus*, from *medullary sarcoma*, *fungous excrescence*, *scirrhus* of the eye-ball, *melanosis* of the eye-ball, &c. In *staphyloma pellucidum*, or *conical cornea*, it is only the cornea which is altered in shape, is projecting in the form of a cone more or less acute, producing at first short-sightedness, and afterwards total loss of distinct vision. *Hydrophthalmia* or dropsy of the eye, assumes three forms: 1st. Dropsy of the Aqueous Humour, which usually follows blows, or other injuries of the eye, though it sometimes follows suppression of cutaneous eruptions; this is limited in extent, and usually combined with a paralytic and tremulous state of the iris, and partial amaurosis. 2d. *Sub-Sclerotic Dropsy*, which is an accumulation of fluid between the sclerotica and the external surface of the choroid coat, which is relieved by puncturing the eye and allowing the fluid to escape. 3d. *Sub-Choroid Dropsy*, which is an effusion of water between the choroid and the retina, from inflammation of the choroid coat. This disease has been known in several cases to proceed so far as to cause the absorption of the Vitreous humour, and the compression of the retina into a cord, extending from the optic nerve to the back of the lens. The accumulation of water is usually slow, as is also the loss of vision; but in some cases the effusion is rapid, causing great pain in the eye and head; the choroid pressing against the

sclerotica produces the extenuation of the latter, while the eye-ball undergoes either a general or partial enlargement; and the pupil becomes dilated and sometimes displaced. The treatment here, is to puncture the eye at the usual place of introducing the cataract-needle. 4th. *Dropsy of the Vitreous Humour*. Here the increase in the size of the eye may be chiefly in the posterior part of the eye-ball; the eye assuming the form of a cone, the cornea being pushed forward without undergoing any other change, or the aqueous humor may be diminished in quantity, and the iris pushed forward into contact with the cornea,—the iris not being changed in color, nor the pupil extremely dilated. In this affection the sclerotica generally assumes a deep blue color, short sightedness first appears, then complete amaurosis; the eye becoming extremely hard, and its movements sooner impeded than in aqueous dropsy. The pain in the eye and head is intolerably severe from the commencement. The pressure of the enlarged eye-ball, on the walls of the orbit, sometimes induces caries. 5th. *General Dropsy of the Eye*. Sometimes we observe both the aqueous and vitreous humor increased in quantity at the same time, so that the whole eye is greatly enlarged; attended with excessive pain, loss of motion of the eye, loss of sleep, appetite, and at last, of reason. The patient generally dies, worn out with fever, before the eye gives way. Evacuation of the contents, and extirpation are the remedies. The eye is sometimes enlarged from sanguineous effusion into the aqueous chambers, from a blow on the eye, there is also an internal hemorrhage, arising from causes, not well understood, called *apoplexy of the eye*. Enlargement of the eye sometimes proceeds from *fungous excrescence of the iris*; and also from *scirrhus of the eye-ball, medullary fungus, and melanosis*. These are all highly malignant affections and usually prove fatal. I lately witnessed a case of the medullary fungus, where the tumor burst through the fore-part of the eye, advanced with great rapidity, and reached the size of a hen's egg; presenting a spongoid or fungous texture, and attended finally by copious hemorrhage. The patient was a fine boy of eight years old. The disease proved fatal in about six months; and on examining the tumor after death, I found it destitute of fibres, of a brownish, white substance, and having very much the appearance of brain. This disease was called *spongoid inflammation*, by Prof. Burns, *fungus hæmatodes*, by Mr. Hey, *medullary sarcoma*, by most writers, and *soft cancer* by Mr. Abernethy. The disease is invariably fatal, I believe, whether extirpation is performed or not, and Mackenzie states that it has resisted both external and internal medicines. *Melanosis of the eye-ball*,

is a rare affection, but the disease is readily recognized from the black color of the tumor.

Sometimes the eye appears preternaturally large, in consequence of an effusion of blood, or serum, in the adipose substance of the socket. I lately attended such a case, in a boy who run his head against a post, striking just above the orbit. In six hours the eye was protruded to the extent of nearly half an inch, no other unnatural appearance was visible, and the boy recovered entirely in a few days. You see the eye also protrude very often after the operation for *strabismus*. This is owing not only to a division of the tendon of the internal oblique muscle, but especially to cutting the conjunctival coat over too large a surface, which permits the eye to protrude, it being this muscle which confines the eye chiefly to the orbital cavity. You may easily be deceived in these cases without careful examination; for a protrusion of the eye is often mistaken for an increase in its size. Where the protrusion is gradual, and permanent, it may arise either from fatty and other tumors developed behind the ball of the eye, aneurism, exostosis, osteo-sarcoma, or inflammation of the adipose tissue in the socket, or general inflammation of the ophthalmic cavity. When the protrusion happens from these causes, the eye is generally pushed to one side. In cases of great determination of blood to the head, the eyes are considerably more prominent than at other times, and this is strikingly the case in apoplexy, paralysis, and even in hysterical paroxysms.

The eyes are sometimes morbidly diminished in size. This may happen from atrophy, suppuration, or loss of separate parts of the eye, as the aqueous, or vitreous humour. A sinking of the eye, (commonly called *hollow eye*) occurring gradually, is a sign of *atrophy* of the parts behind the globe, or absorption of the adipose bed, on which it rolls,—you see this in consumption and most chronic diseases, attended with great emaciation; it is usually, a very unfavorable sign. In cases of long fasting, or starvation, it is also a striking symptom. Where it occurs rapidly, as in cholera asphyxia, in which it is strongly marked, after excessive evacuations, either by blood-letting or from the operation of hydragogue cathartics, it denotes a rapid diminution in the serous portion of the blood, and is a symptom of great value in cholera, as well as cholera infantum. In these diseases the eye actually seems to have retreated into the socket, a space, equal, at least, to that of its own diameter. In fevers, according to some of the German pathologists, this sinking is a fatal sign; but I am satisfied this is erroneous. Any cause which produces general emaciation, will pro-

duce a sinking of the eye in the same proportion; and the value or significance of the sign, is to be estimated entirely from other circumstances.

The *color* of the eye affords a valuable diagnostic symptom. A red color of the conjunctiva, is an ordinary symptom of inflammation of this coat; and the local character of the complaint is readily determined by the other symptoms, or circumstances attending the case. It is also a common symptom in hyperæmia, and congestion of the brain, and here its diagnostic import is to be determined by the intensity of the cerebral symptoms. In apoplexy, epilepsy, and other affections, where there is a preternatural flow of blood to the head, you will also find the conjunctiva injected, and a blood-shot eye is the common result of a blow upon this organ. A yellowness of the eye is usually associated with that of the skin, and may be occasioned by an absorption of bile into the blood, or the non-separation (owing to a check to the biliary secretion) of those elements which go to the formation of bile. The latter is probably a cause of jaundice in infants.

A *dirty brown color* of the conjunctiva, is apt to occur in malignant fevers of a typhoid type. In bad cases of small pox and measles, and in gangrene, it yields, almost invariably, an unfavorable prognosis. You sometimes observe a fine rose-red vascular net-work under the conjunctiva—this is a sign of *scleritis*, and is often of rheumatic origin. An injected state of the vessels of the cornea, are signs of *ceratitis*. White opaque spots on the *cornea*, are the result of inflammation; this may terminate in small *ulcers*, by the healing of which, *cicatrices* are formed. In *iritis*, you observe through the pupil, yellow specks, or exudations of lymph on the iris; and if they be of a dirty grey color, and covered with small red, or yellowish grey elevations, you may be pretty sure that it is of a syphilitic character.

Where there is suppuration of the internal eye, you will see whitish purulent matter, through the pupil, floating in the aqueous humour. A red or dirty grey turbidness in the bottom of the eye, is one of the early symptoms of *medullary sarcoma*; while in *incipient cataract* you observe the crystalline lens of a yellowish white, or ash grey color; whilst greenish turbidness indicates *glaucoma*.

The form and size of the *pupil*, are of considerable importance in the diagnosis of cerebral diseases. A *contracted pupil* generally indicates hyperæmia, or inflammation of the brain, or its membranes, or both; and it may arise from idiopathic or symptomatic affections of this organ. A dilated pupil, although often the result of sympathetic cerebral irritation, as

from worms, gastric derangement, hysterical and epileptic fits, is generally produced by pressure upon the brain, either by the effusion of blood or serum, as in hydrocephalus, &c. Such a dilated condition of the pupil is not, however, always the result of pressure, for we find it often in apoplexy. It is a common symptom, also, of narcotic poisoning. The influence of Belladonna in dilating the pupil is well known, and when we wish to produce the full effect of this remedy, we direct it to be given or rubbed around the eye, until there is considerable dilatation of the pupil. You will distinguish contraction of the pupil proceeding from cerebral irritation, from that caused by *iritis* by the following circumstances:—1st. In *iritis*, the pupil is not only contracted, but it is irregular in shape, and immovable. 2nd. There is a beautiful radiating zone of vascular redness, situated in the sclerotica, and immediately surrounding the cornea. 3d. There is a decided change in the color of the iris, from grey or blue to a yellow or greenish tint; and from brown or hazel to a dusky reddish hue. 4th. Closure of the pupil by lymph, unless prevented by active antiphlogistic measures: with the liberal use of calomel. 5th. Dimness of sight, and pain in the eye.

The *lustre of the eye* is closely connected with the state of the brain. You will observe in children, just previous to their being attacked with acute disease, particularly of the brain, (and I have frequently observed the same in croup,) the eye assumes an unnatural brilliance, it fairly sparkles with light, and at the same time, there is great exhilaration of spirits, an exuberance of animal life, which tends to overleap its customary bounds. Parents have often remarked this in their children, though they are, for the most part, ignorant of its true indication. I know many families, where, after it has been pointed out, I have no doubt an acquaintance with the fact has often prevented serious disease, by leading to the early employment of preventive medication. You find the same unnatural lustre of the eye in most cases of hyperæmia and inflammation of the brain, in apoplexy, and in delirium and insanity. Indeed, it is one of those diagnostic marks, by which the experienced eye detects mental disease, where the unpractised observer would discover nothing unnatural. The lustre of the eye fails in states of great prostration of the nervous system, as well as failure of the general strength; hence it is observed in cases of cholera, and cholera infantum, of consumption, low fevers, and poisoning, especially by the acrid mineral poisons. In these low states, you often see the eye covered with a dusky film, which the common people *suppose* is the infallible precursor of speedy death; but though it is a

very unfavorable sign, it is by no means a fatal one, as I have repeatedly observed.

The *motions of the eyes* are regarded by the pathologist, as affording valuable diagnostic and prognostic aid. The eye is moved, as you know, by six voluntary muscles; four straight, a superior, inferior, external and internal; and two oblique, a superior, with its tendon running round a pulley, and an inferior. The straight muscles are for the direction of the eyes, and we become acquainted with the distances, magnitudes and positions of objects, by the sensations which accompany the motions of the muscles of the eye, or in other words, from the consciousness of muscular effort. (*Elliotson.*) Immobility of the eyes is a sign of tonic spasm, or paralysis of the muscles of the eyes, and often, of course, indicates cerebral disease, as effusion of serum or blood, or violent congestion; it is also observed in epilepsy and catalepsy. Too violent movement of the eyes is a sign of clonic spasm, and is a common symptom in acute affections of the brain, as apoplexy, in cases of delirium, and general spasm. Diminished mobility is observed in diseases of great prostration, as typhus fever, cholera, poisoning by arsenic, &c.

Squinting, *strabismus*, may be permanent, when it is owing to a contraction of one of the muscles of the ball, which is now so easily remedied by the operation of myotomy; or temporary, when it is involuntary, and caused by cerebral disease, as hydrocephalus, apoplexy, encephalitis, &c. In such cases, of course, it affords an unfavorable prognosis, but it is also a symptom of worms, and is not unfrequent in hysterical and epileptic paroxysms.

The motions of the iris have the same importance as those of the eyeball, and may result from different causes, both local and general. Its motions are dependent on the third pair of nerves, and when these are divided, the pupil dilates and cannot be made to contract by the most intense light. A fixed iris is the result of tonic spasm, or of paralysis; these may be local, or owing to congestion, or inflammation of the brain. Its contractibility is lessened in all low typhoid states of the system, while it is increased by the opposite condition. The *look* or *expression* generally harmonizes with the features; thus there is a wild look, an anxious look, and a despairing look. A wild expression indicates irritation, hyperæmia or inflammation of the brain; it is generally associated with delirium, and precedes apoplexy. You see an anxious look in all diseases of the heart, and generally wherever there is much obstruction to the respiration or

circulation. The despairing look belongs to violent diseases of the digestive organs, as gastritis, enteritis, acute hepatitis, cholera and dysentery.

Defective Vision. There are various states of defective vision, such as 1, *myopia*, or *near-sightedness*. This may be caused by, 1, too great convexity of the cornea; or, 2, too great thickness of the cornea; or 3d, too great convexity of the crystalline lens; or 4, preternatural density of any, or all of the transparent media of the eye; or 5, preternatural elongation of the eye-ball.

2. *Presbyopia* or far-sightedness. The cause of this is deficient refraction, owing to too great flatness of the cornea, from diminution in the quantity of the aqueous and vitreous humours; or diminished density in any of the refractive media of the eye. The density of the crystalline lens increases with age, but it probably shrinks so as to become less convex. When presbyopia occurs suddenly in subjects much under the age of 40 years, we should be led to suspect, either some derangement of the internal parts of the eye, some pressure behind the eye-ball, or some disease of that portion of the optic apparatus which is contained within the cranium. In some individuals, there is an insensibility to certain colors; they cannot, for example, distinguish *red* from *green*, or *pink* from *blue*; *red* appearing to them merely a *dark* color, while *green* is a shade of *drab*. They can easily distinguish *yellow* and *blue*, but they judge of *orange*, *purple* and *brown* with great difficulty; and even the shades of *black*, *gray* and *white*, they are often unable to decide upon without hesitation. At least five different explanations have been given of this phenomenon; but it seems to me that that furnished by phrenology is the most satisfactory, and this is that the faculty of distinguishing colors depends on a definite portion of brain, called the *organ of color*; and that defects in this faculty must be attributed to this organ, and not to the eye, which is apparently perfect.

Colored vision is not an unfrequent symptom of cerebral derangement. In perfect health, we, of course suffer no imitations of visual sensations, no flashes of light from internal changes in the eye, no false perceptions of *muscæ volitantes*; objects are seen clothed in their natural colors, and no objects are supposed to be seen, except when their image is actually impressed upon the retina. And, yet, we are subject to false visual sensations of various kinds and degrees. When the circulation of the blood is accelerated through the retina and neighboring parts, we can actually see the circulation of the blood in the eye; thus if we look on a white wall well illuminated, we see a kind of net-work, darker than the other parts of

the wall, which appears and vanishes alternately, with every pulsation. This change of color is owing to the compression of the retina by the diastole of the artery. Dr. R. Darwin states that we can see the circulation in the eye at any time, by rubbing the eyes well, and holding the breath so as to accumulate more blood in the head, then turning to the light, the small veins and arteries are distinctly seen. In general, however, the circulation in the eye may easily be seen by examining other spectra with your back to the light till the eye becomes weary, then having covered your closed eye-lids for half a minute, till the spectrum is faded away, which you were examining, turn your face to the light, and removing your hands from the eye-lids, by and by shade them again a little, and the circulation becomes perfectly distinct. It is probably the veins which we see, as the blood in them is darker, and they seem to unite after the usual manner of veins. You will sometimes hear patients complain of the sensations of seeing luminous objects, like a lighted candle, surrounded by the colors of the rainbow, or luminous flashes or sparks. This is a very frequent symptom in amaurosis; it is also present in inflammation of the eye, causing a derangement of the retina, but it may be owing to some derangement of the lenses of the eye, by which the achromatic power of this organ becomes impaired. The same effect may be produced in a degree, by contracting the eye-lids, by which the rays of light are decomposed by inflection merely.

Sometimes persons complain of seeing objects tinged of a yellow, green or bluish color. Thus in jaundice, patients often see objects colored yellow; if blood is effused into the anterior chamber, every thing appears red. This phenomenon has been called *Chrupsia*, from *Kroma*, color, and *Opsis*, vision.

There is a frequent morbid phenomenon, connected with the eye, which has been called *Photopsia*, from *Phos*, light, and *Opsis*, vision. It is sensations of light, independent of vision, as that produced by sneezing, or a sudden blow on the eye, or by galvanism or electricity, as in the well known experiment of applying a piece of zinc and a piece of silver to the tongue, and then bringing them into contact. If any experiment were needed to show that the retina may be so impressed as to produce the sensation of light altogether independently of the actual presence of light, we have only to rub our eyes with the finger, or produce slight pressure on the globe, when flashes of light will instantly appear. In *retinitis*, and also some kinds of *amaurosis*, at their commencement, there will be flashes of light, the appearance of shining stars, a glittering as if from the points of

innumerable needles, and a variety of other lucid spectra; and in some peculiar cases, the patient complains of seeing globes of light, swimming before him, or as if he were looking at a sea of melted gold. These lucid spectra should never be disregarded, or thought lightly of, for they are very often the precursors of convulsive attacks, such as epilepsy; persons inclined to apoplexy, often see showers of shining spectra on raising their heads after stooping; the same appearances attend phrenitis, and cephalitis, and they are particularly troublesome to those who have suffered from internal ophthalmia. The treatment, of course, is to be regulated entirely by the cause.

Ocular Spectra, or *accidental colors* are not unimportant to the pathologist. If you look at a bright object for a short time, and then close your eyes, you see the form of the object, but generally of a different color; this is called the *ocular spectrum* of that object. Buffon gave to colors arising in this way, the name of *accidental colors*, to distinguish them from those which are produced by the decomposition of white light. The causes of this phenomenon are not well understood, but when you find persons complaining of ocular spectra for days, and weeks together, you may expect that it will be followed by some serious affection of the retina. Dr. Brewster, of Edinburgh, nearly lost his sight by the following experiment: On a bright, clear day, when the sun was near the meridian, he formed a very brilliant and distinct image of its disc, by means of the concave mirror of a reflecting telescope. His right eye being tied up, he viewed this luminous disc with the left, through a tube, which prevented any extraneous light from falling upon the retina. When the retina was highly excited by the solar image, he turned his left eye to a white ground, and examined the series of ocular spectra which followed. After uncovering the right eye, a remarkable phenomenon appeared; for on turning it on a white ground, he found that it gave also a colored spectrum. He repeated the experiment twice, in order to be secure against deception, and always with the same result. The spectrum in the left eye was uniformly invigorated by closing the eye-lids, because the images of external objects efface the impression upon the retina, and when he refreshed the spectrum in the left eye, that in the right was also strengthened. On repeating this experiment a third time, the spectrum appeared in both eyes, which seems to prove that the impression of the solar image was conveyed by the optic nerve from the left to the right eye; for the right eye being shut, could not be affected by the luminous change. The effect of this experiment was to reduce the eyes to such a state of ex-

treme debility that they were unfit for any further trials. A spectrum of a darkish hue floated before his left eye for many hours, succeeded by the most excruciating pains, shooting through every part of the head. These pains, accompanied with a slight inflammation in both eyes, lasted for several days. Two years after, the debility of the eyes still continued, and several parts of the retina in both eyes had completely lost their sensibility. (*Ed. Cyclop.*)

You may perform the experiment with perfect safety, by placing a *red wafer* on a sheet of white paper, and fixing the eye for some time steadily to a dot at its centre, then turn the eye to another portion of the paper, and you see a circular spot of the same size as the wafer, but its color will be *green*. You will bear in mind that there are but three primary and distinct colors, *red, yellow* and *blue*, all the other colors being compound or composed of a combination of these. Thus, *orange* is a compound of *red* and *yellow*; *green* of *yellow* and *blue*; *purple* of *blue* and *red*; and *white* is a neutral combination of the three primary colors. An *accidental* color is always that, which added to the original color, produces *white*. Thus if you look at the *red* wafer, the spectrum is *green*; this is the result of the mixture of *yellow* and *blue*, but *yellow* and *blue*, with *red*, go to form white; these are therefore called *complimentary* colors, and so of the others.

Muscæ Volitantes. These are false visual sensations, which have derived their name from their resemblance which they bear to *flies* moving through the air. These differ much, however, in appearance, some being like twisted tubes, partially filled with globules, which sometimes appear in motion, while others look like black spots which follow the motions of the eye.

Mackenzie thinks that the *black spots*, which he compares to the body of a spider, with three or four diverging legs, are often the precursor of *amaurosis*, while the semi-transparent spectra portend nothing serious. I have, however, known the black spectra continue for years without being attended by any serious inconvenience, and finally disappear altogether. Dr. Mackenzie thinks that *muscæ volitantes* are never seen in the sense in which objects out of the eye are seen, "as," he remarks, "opaque spots in any part of the eye, anterior to the retina, could never produce an image on that membrane, sufficiently defined to give rise to such impressions as the generality of *muscæ volitantes*; he, therefore, is inclined to refer them to the retina itself, the semi-transparent being caused by a dilation of the branches of the *arteria centralis retina*, while the dark spots

are the effects of certain portions of the retina having become insensible to light." Mrs. Griffith, of New York, who has written an excellent work on the eye, believes them to be *air bubbles*, floating in the liquor which surrounds the crystalline lens, and is collected within its capsule. She maintains that this phenomenon is neither the cause, nor the consequence of *amaurosis*, but is occasioned solely by a debility of the absorbent vessels of the part. "These bubbles," she remarks, "when single, or even when united like strings of beads, do not obstruct the rays of light, because their axes are independent of each other. But when they cling together in bunches, their axes interfere, and do not allow the rays of light to pass through them, consequently they present as great an obstruction as any other opaque mass. It is simply, therefore," she continues, "through the want of parallelism in the axis of these air-bubbles, that the spectra called *muscæ volitantes*, disturb our vision." (*N. York Lancet*, vol. 2, p. 212.)

There are few symptoms which prove so alarming to nervous people as these appearances in the eye. They often suppose they are about to lose their sight by cataract, or amaurosis. But you may very safely assure them that there is no danger of either of these terminations, unless other symptoms be present. These false perceptions, for so I regard them, do not render objects obscure, as incipient cataract does; nor is there any fixedness or even unnatural slowness of the iris, in simple cases of *muscæ volitantes*.

Spectral Illusions. Hippocrates has noticed the fact in his work, "*De Natura Muliebri*," that spectral illusions are often attended by fatal effects. Dr. Hibbut, in his excellent work entitled "*Sketches of the Philosophy of Apparitions*," has traced them to a great variety of causes, such as *highly excited states of particular temperaments, hysteria, hypochondriasis, the neglect of accustomed periodical blood-letting, febrile and inflammatory affections, inflammation of the brain, delirium tremens, &c.*

These spectral illusions are almost infinite in variety and form, and we may assure those who are troubled with them, that they are merely disordered sensations, symptoms of a peculiar affection of the internal optic apparatus, the effect of which is, to produce a repetition or imitation of former impressions.

Mr. G. Combe thus explains the existence of spectral illusions: "When any of the knowing or reflecting organs is internally active, the mind conceives, or is presented with ideas of the objects which it is fitted to perceive. Thus, locality, coloring, and size being active, we are able, with our



eyes closed, to conceive a landscape, in all its details of hill and dale, sunshine and shade. If this internal activity become morbid, through disease of the organs, their ideas become fixed, and remain involuntarily in the mind: and if this is long continued, it constitutes insanity. Many persons have experienced, when in the dark, vivid impressions of figures of every variety of color and form passing before the mind, sometimes invested in alarming brilliancy and vivacity. I conclude that this arises from an internal excitement of the organs situated at the superciliary ridge, viz: form, locality, coloring, &c., occasioned generally by an unusual accumulation of blood. This affection is, in most cases, only momentary; but suppose that it were to become fixed and continuous, then the mind would be haunted with permanent and vivid conceptions of innumerable and fantastic beings, invested with more than the forms and hues of realities. This would be insanity; not a diseased feeling, such as melancholy, a fury, or religious joy, but an intellectual delusion, so that every sentiment might be sound, and yet this aberration of intellect remain fixed and immovable by the will." This, gentlemen, appears to me a much more satisfactory and philosophical explanation, than that usually given.

By this means, we shall be able to calm the minds of some who otherwise might be led to ascribe their visions to supernatural powers, or who through fear or terror might possibly be driven to insanity.

Night Blindness is a rare affection, but one that we sometimes meet with, and it is one which always gives great alarm. Without, perhaps, any premonitory symptoms, a person finds himself all at once, as night comes on, nearly blind. The physician is immediately sent for, but he finds nothing to be seen, except a dilatation of the pupils. He bleeds the patient, probably, and gives him a cathartic, and in the morning, to his surprise, he finds himself perfectly well. At evening, however, his blindness again returns; if candles are present, they look as if glimmering through a thick mist or fog. Thus the complaint goes on, returning at night, disappearing by day, till after a while, the vision becomes indistinct by day, as well as by night, though not in so great a degree. The patient grows near sighted, and finally, if neglected or mistreated, the complaint ends in incurable amaurosis. Dr. Mackenzie thinks the cause of this disease is in some peculiar state of the choroid coat, rendering the eye insensible to light of a certain intensity. But I am inclined to locate the disease in some peculiar modification of that portion of cerebral substance, in which the optic nerve originates, for it is a well known fact that the disease is frequently sympathetic of derangements of the digestive organs, as

shown by the foul tongue, fetid breath, and deficient appetite. It is also a precursor of scurvy, and in the East Indies, is supposed to be caused by eating hot rice. It is, however, owing in some instances to exposure to an unusual glare of light, especially in warm climates, or sleeping with the eyes exposed to the sun's rays. The treatment is to be regulated by circumstances, but as a general rule, it must be constitutional.

In some instances, as after being long shut up in a dark room or prison, the pupil becomes so much dilated by exposure to the light of day, that the patient is unable to see—this is called *mydriasis*; at night, as the pupil contracts by the withdrawal of the light, vision becomes restored. Again, the pupil may contract so much by day, as to obstruct the sight; but as it relaxes in the dark, the person is again enabled to see. For the same reason, a person laboring under incipient cataract can see better during the evening, than during the brightness of the day, for then the pupil dilates so as to permit more light to reach the retina. This is called *Day-Blindness*. Baron Larrey relates the case of an old man, one of the galley slaves at Brest, who had been shut up in a subterranean dungeon for the space of 33 years; in consequence of which, he was perfectly blind during the day, and could see only at night. Romazzini records the prevalence of this disease, as an epidemic, among boys about ten years of age, in the month of March, which had disappeared by the middle of April. There is another defect of vision, which goes under the name of *amaurosis*. If the retina be incapable of receiving with correctness, impressions of external objects through the medium of light, or, if the optic nerve be unable to convey to the sensorium the impressions made upon the retina; or, if the sensorium be incapable of receiving the impressions conveyed by the optic nerve, the individual must necessarily be affected with a greater or less obscurity in vision, or suffer a total deprivation of vision, according to the degree of inability in these several parts to execute their functions. Now it is a very important, and sometimes a difficult matter, to distinguish these cases, and pronounce with certainty, whether the retina, the optic nerve, or the brain, be the part first and principally affected. There are numerous anatomical and physiological facts which necessarily enter into a consideration of the seat of the different varieties of this disease; but it is absolutely essential that the physician should be most accurately acquainted with the structure and functions of the nervous system, in order to arrive at any thing like certainty in his diagnosis. It would require two or three lectures to do any thing like justice to this affection; I shall, therefore, merely remark as to its *efficient causes*, that they may be 1st, of a *local*,

direct, and *mechanical* nature, such as the pressure of a tumour on the optic nerve; or 2d, of a *local* and *vital* kind, such as a plethoric or congested state of the blood vessels of the brain, or of the eye; and 3d, the cause has been *general* or *constitutional*, such as exhaustion, from blood-letting, or excessive loss of any of the fluids of the body. Its *proximate* cause in a large number of cases, at least, is *pressure*. The *remote* causes are numerous; as hereditary predisposition; over-exertion of the sight, and exposure to too great light; all causes which tend to produce cerebral congestion—poisons, especially those of narcotic vegetables, and especially tobacco; (according to Mackenzie,) gastric and intestinal irritation; exhaustion of the body from any cause whatever, particularly grief, excessive venery, and protracted suckling; blows on the head, &c. These hints, brief as they are, will serve to show that the subject is surrounded with difficulties, and that there is the greatest necessity for exercising the most minute and careful observation, if we hope to make any advancement in the knowledge of this class of diseases. Our prognosis in this affection will depend entirely on its cause and seat, and these must be accurately determined before we can determine any thing, or form any opinion as to its progress and termination. From what I have observed, I am inclined to believe that the cause of amaurosis is oftener seated in the brain than in the eye, hence its obstinacy and general incurability.

Acute Vision. The sight often becomes morbidly acute, 1st, in some cases of inflammation of the brain or its membranes; in nervous fevers; in mania, and in hyperæmia of the brain from acute inflammatory affections of other organs. 2d. In diseases where the sensibility in general is exalted, as hysteria, and hypochondriasis, and in recovery from acute diseases. 3d. In inflammation of different parts of the eye, more especially in catarrhal inflammations, in variola, rubeola, scarlatina, scrofula, and arthritis, and in inflammation of the iris, of the capsule of the lens, and of the retina. Preternatural acuteness of vision often precedes attacks of acute diseases of the brain, as in the case related by Dr. Brachet, of a man in one of the surgical wards of the Bicetre Hospital, at Paris, who suddenly acquired such an astonishing acuteness of sight, that he could see the most minute objects at an enormous distance. Five hours afterwards he felt a slight headache, and in a few hours more was seized with a fatal apoplexy. On dissection, a fresh coagulum of blood was found in the right optic thalamus. The inflammation which had preceded the effusion, had irritated by its proximity that portion of the brain from whence the optic nerve takes its origin. I have known the same happen both to the organ of sight

and hearing, from an attack of hemiplegia; the increased acuteness of both these senses was in the highest degree astonishing.

Double Vision is not unfrequently met with, and it usually proceeds from irritation, inflammation, or effusion upon the brain; it is a common symptom in hysteria, hypochondriasis, worms, intoxication, delirium tremens, &c. Where it proceeds from an affection of the retina, it is a sign of impending amaurosis. In acute diseases, and in gout, it is a sign of metastasis to the brain; if it occurs in affections of the spinal cord, it renders our prognosis more unfavorable by showing a complication with cerebral disease. In *deformed vision*, that is, where one sees objects disfigured, inverted, or crooked, there is much reason to suspect organic disease of the brain.

These are all the remarks which my time will allow me to make on the interesting subject of the eye, especially with reference to its *Semeiology*. I have dwelt the longer upon it because, though one of the smallest organs, it is nevertheless one of the most important in the body; and especially because its diseases furnish more satisfactory and plainer illustrations of the general doctrines and facts of pathology, than any other single organ of the body. Here you see, in fact, almost all diseases in miniature; and from the peculiar structure of the eye, you see them as through a glass, and you learn many of the little wonderful details, in the nature of morbid processes, which but for the observation of them in the eye, would not have been known at all.—(Latham.)

ART. II.—*Application of Chemistry to Practical Medicine*, By M. M. ROGERS, M. D., Author of *Agricultural Chemistry*, "Scientific Agriculture," &c.

The science of Chemistry has for its object the investigation of the properties of all elementary and compound substances, their relations and combinations, the agencies by which their changes are effected, and the laws which govern them. The basis on which this science rests, is facts and experiment; it is purely a demonstrative science, and no hypothetical or speculative views can be *practically* of any service in its advancement and application. Every change which takes place in the elementary constitution of matter in the universe, whether effected by natural causes, or by the operations of art, involves a fixed chemical law, and is due to chemical action.

Chemistry consists of two distinct branches, viz., analysis and synthesis.

Analysis consists in decomposing a compound body and separating its elements. Synthesis consists in uniting simple bodies so as to form a compound. The forces which preside over and cause chemical action are light, caloric, electricity, and magnetism; the relative importance of these forces cannot be exactly estimated in the present state of the science; the question of their individual distinctness or identity with electricity remains unsettled. The science of chemistry, which has achieved greater triumphs over matter, and conferred more practical knowledge of nature upon civilized man, than all other sciences combined, has gradually grown out of the superstitious art of alchemy. Modern chemistry, instead of alluring its votaries into a fruitless search after the "philosopher's stone," the "universal solvent," or the "universal catholicon," crowns their investigations with results which tend to the advancement of civilization, and the increase of human comforts and happiness. Its objects are not limited to the study of abstract laws alone, but extend also to the improvement of the useful arts, the cure of disease, the preparation of food, the development of the laws of organic life, and, finally, to every thing affecting our physical relations to the material universe. No art or profession is so intimately connected with, and dependent upon chemistry, as that of the practice of medicine.

The elementary branches, Anatomy, Physiology, Botany, and *Materia Medica*, are so dependent upon chemistry for the evolution of their principles, that they were never understood, or investigated to much extent, until this science was made a basis of all philosophical research, in relation to the properties of matter. And the successful operations of the deductive branches, *viz.*, Pharmacy and Therapeutics, and in some degree, Surgery and Obstetrics, bear an exact proportion to the amount of correct chemical knowledge employed in their prosecution. All success in the practical branches of medicine, is owing to the discovery and carrying out of chemical laws, although the practitioner may be ignorant of this fact, and may not recognise them as such.

If we admit that all changes whatever, which take place in the elementary constitution of matter, both organic and inorganic, are merely chemical transformations, we see that the practice of medicine proper, is only the aggregate of a series of chemical experiments, and the physician a practical chemist. When we reflect that, so far as yet known, we have but about sixty simple or uncombined elementary substances, and that from these every organic and inorganic compound is made up, we see that the number of chemical changes which take place between these few elements, is abso-

lutely infinite. But a difference in elementary constituents and proportions, is by no means necessary in order to form different substances.

By a principle called *Isomerism*, several substances of very different properties are formed from the same elements, in exactly the same proportions. Cane sugar, milk sugar, and grape sugar, are each composed of carbon, hydrogen, and oxygen, each 12,—or equal parts of carbon and water. Woody fibre, starch, gum, and cane sugar, are each composed of carbon 12, hydrogen 10, and oxygen 10. Oil of turpentine and oil of citron are each composed of carbon 5, and hydrogen 4. Difference in internal molecular arrangement causes the difference in chemical and physical properties in isomeric compounds.

Metamorphosis of organic elements is another interesting point. There are certain organic compounds which, from the complexity of their constitution, and consequent weakness of affinity, are peculiarly disposed to decomposition and change of elementary form. They are disposed to change whenever the balance of opposing forces is destroyed: that is, whenever by the agency of some external disturbing force, as heat, air or water, the affinity which holds these elements together is overcome, the elements are separated entirely, or one element is replaced by another, and another compound formed. Lignine or wood fibre may be changed to starch by boiling in water, drying in an oven, and then fermenting with yeast. In this way the writer has made bread of green beech wood, which was but little inferior to that made from unbolted wheat flour. Woody fibre may be transformed to starch also, by the action of sulphuric acid or caustic potash. Starch when gradually heated to 300 deg. F. assumes a brownish tint, and is changed to gum. By the action of sulphuric acid, also, starch may be changed to gum, and this again to grape sugar. Gum arabic may be changed to sugar by the action of chalk and sulphuric acid. Cane sugar, when heated to 360 deg. F., gives off two atoms of water, and is changed to caramel. Cane sugar may be changed to grape sugar by digesting it in dilute sulphuric acid at a gentle heat. By the agency of casein, milk sugar is transformed to lactic acid, and this again to butyric acid.

Animal chemistry furnishes an explanation of almost every physiological and pathological change which takes place in animal bodies. A few brief remarks on the constitution of some of the animal fluids will show the agency of chemistry in the development of physiology and pathology.

There are four classes of diseases in which the *blood* undergoes peculiar

changes. The *first* class includes acute inflammation, tubercle, and cancer. In these the quantity of fibrine in the blood is increased. The *second* class includes cerebral hemorrhages, and continued fevers without local inflammation. In this class the fibrine is diminished, and the globules increased in proportion to the fibrine. The *third* class includes chlorosis, marasmus, anemia, &c. In this class there is a great diminution of globules. In the *fourth* class, Bright's disease is an example. In this class the fibrine and globules are unchanged, but the quantity of albumen in the serum is diminished. Of all the constituents of the blood, the globules suffer the greatest changes. In acute inflammation there are three times as many globules as in chlorosis. Next to the globules, the fibrine suffers the greatest change in quantity. The salts are least liable to change in quantity, and vary the least in quality and relative proportions. The blood in cholera contains less fluid and alkaline salts, and more solid matter. In diabetes mellitus, the blood often contains traces of sugar. In jaundice the blood contains a small quantity of the green coloring matter of the *bile*. This matter is said to be identical with chlorophyde, the green coloring principle of plants. By the addition of nitric acid, bile assumes first a green, then blue, violet, red, and lastly yellow color. By different degrees of oxydation in this way, the various colored bile of different animals is formed. *Serum* contains two liquids, viz. albumen and pyen. They both coagulate by heat, acetic acid, and alum, so that this is no test for the distinction of pus. Pus globules always sink, while mucus swims in water; and this is the true test. The *liquor amnii* is said to be identical with dropsical fluid. *Chyle* and *lymph* are the same as blood, with an excess of water and the absence of hematosine. *Mucus* is dissolved by alkalies and precipitated by acids and tannin. *Saliva* contains cyanogen in the form of a sulphocyanide of potassium. The urine, chemically considered, is of more importance to be well understood than any other fluid in the body. In its composition it is more complex than any other, and its affinities consequently weaker. It is also composed of those highly nitrogenized compounds which are peculiarly disposed to change of form. It being a fluid which is secreted and discharged from the body in large quantities there is no obstacle to chemical tests and inspection of its properties. From its complexity of constitution, its tendency to rapid decomposition, and its being the vehicle of several effete matters, we may reasonably look for more and greater alterations, both in health and disease, than in any other fluid. The substances which will be found under different circumstances, are urea, uric acid, albumen, coloring matter of the bile, phosphate

of alkalis, lime, sulphates, hydro-chlorate, free sulphuric acid, and phosphoric acids, alkalies, fibrin, casein, mucus, blood, and sugar.

I may briefly mention some of the re-agents used in examining urine in disease. *Nitric acid* is used to detect urea, uric acid, albumen, and the coloring matter of the bile. *Lime water* shows the presence of alkaline phosphates, by precipitating phosphate of lime. *Ammonia* also precipitates phosphate of lime held in solution by free uric acid. *Oxalate of ammonia* precipitates carbonate of lime. *Ammonia* added to this, precipitates ammoniacal phosphate of magnesia. *Acetate of Baryta* shows the presence of sulphuric acid. *Neutral acetate of lead* precipitates the chloride and phosphate of lead,—these are distinguished by the flame given under the blow-pipe. *Solution of alum* produces turbulence in urine that holds albumen or fibrine in solution. *Bichloride of mercury* gives a precipitate in acidified urine, if albumen or casein be present. *Tannin*, or infusion of *nut galls*, precipitates mucus and extractive matter; the latter is precipitated also by *acetate of lead*. *Red and litmus paper* are used as a general test for alkalies and acids. *Yeast*, by exciting vinous fermentation in urine, shows the presence of sugar. A substance called *keistine* is said to be an evidence of pregnancy, if found in the urine. It is a cream-like matter which collects on the surface of urine after standing for a few days in a glass vessel. This test has some exceptions.

The *tests for several metals*, whether found in the system or elsewhere, are important to be known. The test for silver, lead, copper, antimony, tin, and arsenic, is sulphuretted hydrogen. The test for gold is proto-sulphate of iron; for zinc, hydro-sulphate of ammonia; for iron, ferro-cyanide of potassium.

The *antidotes for various poisons* may be briefly enumerated. The antidote for poisoning by antimony, is tannin or nut galls, or yellow cinchona; copper, any form of albumen; arsenic, hydrated per-oxide of iron; corrosive sublimate, albumen; nitrate of silver, common salt; nitrate of potash, no chemical antidote; acetate of lead, epsom salts; sulphate of zinc, chromate of potassa, milk or albumen; acetate of copper, sugar or albumen; prussic acid, aqua ammonia; opium, strong decoction of tea, coffee, or nut galls; acetic acid, magnesia or carbonate of potash; ammonia, vinegar or other acids; carbonate of lead, sulphate of magnesia; iodine, starch; iodide of mercury, albumen; chloride of barium, sulphate of soda, or magnesia; nitric acid, magnesia, or carbonate of potash; oxalic acid, chalk or magnesia; sulphuric acid, magnesia or chalk; tartaric acid, chalk, potash, or *magnesia*.

Another point which should be kept constantly in mind in practice is, a catalogue of the most common medicines which are usually prescribed together, but which are chemically incompatible, and should never be given in combination. When two incompatible medicines are given, a decomposition and re-union take place,—the two are changed in composition, and two new ones, different from either of the first, are formed. The new compounds may be inert or poisonous, and, at least, they are certain to produce a different effect from what was expected. In this way the physician deceives himself; he is puzzled at seeing an old familiar medicine produce new and strange effects,—and is led to condemn new ones, because he has not allowed them to have their legitimate action. It has often been the case, that a bungling unchemical composition has been prescribed by the scientific physician, or mixed together by the nostrum vender, which nevertheless produced wonderful medicinal effects. This was owing to the formation of some new substance, of the presence of which they were ignorant, and which might have been prescribed with less trouble and propriety.

A few of the incompatible medicines are the following: acids with alkalis generally; alkaline carbonates with strong acids; tincture of muriate of iron with gum arabic; cyanate of potash with salts of iron; alum with chalk and mercury; tartarized antimony with vegetable astringents; nitrate of silver with vegetable astringents and spring water; uva ursi with carbonate of soda; nut galls with acetate of lead, opium, or ointment of nitrate of mercury; extract of logwood with acetate of lead; calomel with lead, carbonate of soda, soap, magnesia, seidlitz powders, and sulphate of potash; yellow cinchona with camomile, cloves, catechu, cascarilla, and nearly all vegetable bitters; mucilage with acetate of lead; rhubarb with sulphate of iron and magnesia; valerian with sulphate of iron and nitrate of silver; kino with acetate of lead; Fowler's solution of arsenite of potash with sulphuric acid, most salts and hydro-sulphates; lime water with alkaline carbonates, and most vegetable bitters and astringents; opium with sulphate of zinc or copper; antinomial wine with tincture of blood root, laudanum or other astringents; almost all salts with milk, spring water, mucilage, gelatine, and astringents; quinine with nitric acid.

Many other medicines which are incompatible, are often prescribed in practice, even among those who make no small pretensions to science, and profess to be entirely read up in all branches of the profession. But whatever opinions they may entertain of their own skill, the combination of a poor chemist and a good practitioner is quite as incompatible as any in the

materia medica. This might have been doubted a few years ago; but since the blaze of light from modern chemistry has shone upon our former system of therapeutics, it has swept it away like vapor before the sunbeam. And in fact, with our present ignorance of practical chemistry, we are left almost without a substitute. While some practitioners are laboring to deduce the entire practice from chemical principles, and thereby constantly making dangerous experiments or failures, others discard all new improvements, and plod their way by the dim and doubtful light of past experience. So that the aggregate of empiricism in medical practice, is perhaps as great as at almost any past time. This is no fault of chemistry; it is only for want of systematic knowledge, that men refuse to make this science the basis of therapeutics, or fail in its practical application when they do so. It is true that chemical facts have come upon us so fast that we have had no time to generalize, classify and apply them in practice. No book exclusively on chemical therapeutics, chemical pathology, or chemical practice, has yet been written. Chemical facts which are applicable in practice, are scattered through large volumes, and concealed by abstruse language, so that much labor is required to collect and arrange them.

Among medical men, chemistry is the last branch read or discussed; among medical students, it is the last branch taken up, and the one least studied. And a student could pass an examination before any corps of professors, provided his only deficiency should be that of giving all the incompatible prescriptions in the materia medica, sooner than one who should fail to tell whether there are six or thirteen layers to be raised in the operation for hernia; although by his incompatible prescriptions he might daily endanger the lives of his patients, while he might not have occasion to operate for hernia during his lifetime.

Simon's Chemistry of Man, Liebig's Animal and Vegetable Chemistry, Liebig on the Motion of the Juices in Animal Bodies, and on Endosmosis and Exosmosis, Bird on the Urine, Elliottson on Poisons, Garrodson's Lectures on Chemical Pathology, and Pereira on the Chemistry of Food and Diet, are books which should be read by every intelligent physician.

Rochester, June 3, 1850.

ART. III.—*Contributions to the Study of the Physical Diagnosis of Diseases of the Chest.* BY AUSTIN FLINT, M. D.

The following observations, developed by the study of thoracic diagnosis at the bedside, the writer has ventured to think are not without some inte-

rest and practical value, as well as originality, and they are therefore respectfully submitted to those engaged in cultivating the art of discriminating pulmonary diseases by means of physical signs.

Determination of dullness of resonance by the pitch, or note elicited by percussion.—I have derived, of late, much assistance in determining the existence of dullness on percussion, on one side of the chest, compared with the other side, by directing attention to the character of the *note* as respects *elevation*, or *pitch*. The usual method with those who practice physical exploration, in so far as the acquaintance of the writer extends, is to compare the two sides with reference to a disparity of resonance, and to estimate the degree of disparity, without observing if there be a deviation in harmony of the sounds considered as musical notes. If there be absolute flatness, or an amount of dullness approximating thereto, it is by no means difficult to determine the fact, even if the explorer be inexperienced. Under these circumstances, a comparison of the two sides is hardly necessary. But when the deviation from normal resonance is less marked, its appreciation is not so easy, and to those who have had but little practice, and are deficient in experience, it is often a difficult problem. In the latter case as is well known, we do not estimate dullness by any abstract, or average standard—this cannot be done, inasmuch as the chests of different persons differ considerably in healthy resonance, owing to various causes—but we judge by comparison of one side with the other, assuming, as we may do, that in the vast proportion of cases, the two sides emit, on percussion, (excepting the præcordial region,) an equal resonance. The question in cases of doubt is, are the two sides, at the various points equidistant from the mesial line, equally resonant? Attention to the *pitch* or *elevation of the note*, will, in every case, obviate all doubt as to the answer to this question, provided the explorer have a correct ear, and especially if he has ever cultivated the ability to discriminate nicely musical sounds. *If a disparity exist, the duller resonance invariably has a higher elevation, or pitch;* in other words, the dull sound has a greater number of vibrations. A striking difference may be at once noticed, in this respect, when it might be difficult to appreciate a disparity of mere resonance without directing attention to a comparison of the notes. This method of determining equality, or difference, is not only accurate, but is much more easy, and is at once acquired by the beginner. I have, on several occasions, tested the facility with which a discrimination may be made by a person wholly unaccustomed to physical diagnosis. Thus—a patient presenting obvious, but not strongly marked relative dullness on one side, from tuberculous

deposit, is made the subject of physical examination. The unpracticed ear of an intelligent student, or practitioner, often fails to detect any difference in sound, so long as his attention is directed to the mere resonance. After confessing his inability to perceive any disparity, let him be instructed to listen to the sound produced on each side in succession, as if they were musical notes, and to mark the pitch or elevation. Then, if he have a musical ear, all doubt and difficulty will vanish, and he will feel immediately competent to decide on even a slight variation in the two sides. His delicacy of discrimination will be greater the more he is qualified or accustomed to discover slight shades of musical discords.

To the experienced auscultator there may be nothing new in the foregoing. The subject first suggested itself to the writer at the examination of a patient several months ago. On referring, at that time, to several treatises on physical exploration, he was unable to find any direction attaching to it that importance which it then appeared to him to possess, and which his experience has since confirmed. A difference in pitch, is stated by Bowditch, and Hughes, to be present in some cases of incipient tuberculosis, as a shade of disparity falling somewhat short of positive dullness. It is not, however, mentioned by these, or other writers that have been consulted, as offering a ready and precise means of determining moderate or slight dullness in all cases.

At first I had flattered myself that a discovery had been made by which the proportionate degrees of deviation from normal resonance might be measured—that the scale of gradations in sound would afford a criterion to estimate the amount of disease more accurately than by a rough comparison of resonance—in short, that the *gamut* would be found more minutely applicable to thoracic diagnosis, than in the mere fact of disparity of pitch. Expectations of this kind were, however, soon abandoned, and the value of the idea limited to the objects which have been explained in the foregoing remarks. As assisting to determine quickly and certainly the existence of relative dullness, and as affording a key to the immediate practice of percussion with success by the novice, the writer believes it will be found to merit consideration.

The applicability of the foregoing principle may be demonstrated in the healthy chest by comparing the pitch of the sound on percussion over the præcordial region, with that on the corresponding locality on the right chest. The difference in resonance occasioned by the presence of the heart, overlapped by the lung, is not readily appreciable by the unpracticed, except the attention be directed to the note.

Tympanic resonance, in solidification (from inflammation) of the upper lobe of the lung; and in pleuritis with large effusion.—Tympanic resonance associated with bronchial respiration, is a combination of signs which the auscultator would not expect to find in a case of pneumonitis. That such a combination may exist is important to be known, and I do not recollect having seen the fact noticed by writers on the subject.

The following case affords an illustration:—

John Donaldson, Englishman, married, draper, aged 40, entered Hospital January 26, 1850. He had lately arrived in this country, and had drunk very freely since his arrival, at the same time taking but little food. He had arrived at this city four or five days prior to his entrance, and had been attacked in the railway cars with acute pain in the left chest. On his arrival at Buffalo he was visited by Drs. Wallis and Wilcox, and treated by bleeding and vesication. The day after his arrival he became delirious, insisting upon changing his lodgings and medical attendants, refusing to take remedies, and, at times, he was violent. The treatment, following the bleeding and vesication, consisted of antimony, mercury, and opium. He slept the night before entering the hospital, and was more quiet on the day of his entrance than previously, taking remedies readily, but still manifesting delirium by a desire to change lodgings, and incoherent conversation. During the night after his entrance, his delirium was active—he constantly attempted to get out of bed, talking to himself, imagining he was on board vessel, etc.

On the 27th, his symptoms were as follows:

Expression unsettled. Quiet and docile, but continues to talk deliriously, laboring under the delusion that he was treated badly before coming to the hospital. He appears satisfied with his present situation. Respirations 24, and somewhat labored. No dilatation of *alæ nasi*, nose swelled and reddened. Pulse, 136, developed, and thrilling. Skin mellow, and temperature moderately elevated. One dejection. Tongue moist, and clean, and somewhat reddened.

Physical examination:—Flatness on percussion over middle and inferior thirds of left chest, anteriorly and posteriorly. *Resonance at upper third relatively clearer than on the right side—tympanic in character.* Respiration absent in middle and lower thirds of left chest. At upper third, anteriorly, feeble, bronchial, prolonged expiration; posteriorly, less feeble, bronchial.

The expectoration was small in quantity, and partially opaque.

The patient died during the following night, and the chest was examined

the subsequent day. The left pleural cavity contained considerable turbid fluid, with fibrinous flakes. Lower lobe compressed into a small space. Upper lobe but little, if at all contracted, completely solidified, presenting the physical characters of pneumonitis in the second stage. No tubercular deposit. A small layer of fibrin effused on pleural surface of lung.

Right lung presented old pleuritic adhesions. Substance somewhat œdematous.

How is the existence of tympanitic resonance over solidified lung, in this instance, to be explained? The supposition of emphysema of the upper lobe, which suggested itself as the most plausible explanation at the time of the examination, was disproved by the appearances found after death. No evidence of the evolution of gas in the pleural cavity appeared at the examination, and the effused fluid presented no fœtor, or other indications of decomposition.

The only obvious alternative remaining, is to attribute the resonance to the accumulation of gas within the stomach, the sound being conducted by the heart and solidified lung, but not transmitted through the fluid accumulated in the pleural sac, so as to be apparent by percussing the middle and lower regions of the chest.

The same explanation is probably applicable to the following instance, in which a combination of signs, in some respects similar, was presented. In giving this case, details not relevant to the present purpose will be omitted.

James Kelly, Irish, aged 18, had been in hospital several months, as a surgical patient, for chronic, indolent ulcers, which were healed at the time of the present attack. He was transferred to the medical department, 23d January, 1850. A week previously he had been seized with severe, sharp, lancinating pain in the left chest, shooting into the shoulder. The pain continued the following day, but was less severe. The next night he began to cough. The cough was dry, hacking, and attended by pain and soreness in the upper part of the left chest. It was unattended by expectoration. He had high febrile movement, hot skin, furred tongue, anorexia, and thirst. Expectoration continued slight. The details of the history and treatment for the first week, before he came under my notice, are not recorded; nor for the first two days after he was transferred. On the 25th Jan. the physical signs noted, were as follows:—*On percussion, the upper and middle thirds of the left chest yield a much clearer resonance than the corresponding regions of the right side. The resonance has a tympanitic character; over the lower third of the left chest, the resonance tympanitic. Laterally, in the left side, resonance tympanitic; on left shoulder also*

tympanitic. Over the right side, resonance normal. On auscultation, in left side anteriorly, respiration feeble, tubular, expiration of same length as inspiration. Posteriorly, respiration feeble, tubular, at upper third; *lower third and part of middle third, no respiration, and flatness on percussion*. Over right side respiration vesicular, supplementary, and expiration somewhat prolonged.

The respirations are 28, slightly labored, without dilatation of *alae nasi*. He does not complain of dyspnoea. Pulse 100, and rather small. Tongue moist, and slightly furred. Bowels moved night before last. Anorexia, sweat profusely last night. He is not greatly prostrated. No pain in chest.

Jan. 26th.—Manifest disparity in the two sides, as regards the intercostal depressions. They are marked on the right side, and absent on the left. Mobility of right side slightly greater than of left. Other physical signs the same as on the 25th.

27th.—Aspect improved. The resonance at the upper third of the left chest is less tympanitic, but still clearer than on the right side. Respiration more developed, and approximating to vesicular in character. Pulse 104. Respirations 28, not labored. Expectoration small in quantity. Two sputa observed, solid, of yellowish color, and streaked with blood.

30th.—Improving. Febrile movement continues, but is less in degree. Perspires at night.

Less disparity of resonance at the summit of the chest; but there is still exaggerated clearness on the left side. Tympanitic resonance below line of nipple. Complained of some pain in chest last night. The left chest at inferior lateral portion appears slightly contracted. The two sides were carefully measured several days ago, and found to be exactly equal.

Feb. 13.—Kelly has been constantly improving since the date of the last record. He now sits up most of the day. Tongue clean. Appetite good. Bowels regular. Pulse 100. The chest on the left side, inferiorly, presents distinct contraction, and on measurement is three-fourths of an inch less in circumference than the right. The mobility of the right chest is obviously greater than that of the left. Greater relative clearness of resonance at the upper third of the left chest continues. *Flatness below the level of the nipple*. Respiration at upper third of left chest feeble, and relative duration of expiration increased. Respiration on right side normal. The impulse of the heart is not appreciable, but a slight pulsation can be seen on either side, near the sternum; on the right side, situated

above the level of the nipple, on the left, on a level with the nipple. He has no cough, nor expectoration.

Feb. 24th.—Inferior, anterior portion of chest, on left side, manifestly flattened. Left interscapular space diminished. Less expansibility of the left side than of the right. Impulse of heart felt distinctly, on a level with the nipple, between the nipple and the left margin of the sternum. Left chest resonant throughout, but relatively dull. Respiratory murmur feeble over left side, anteriorly and posteriorly.

March 3d.—Improving. Sits up the greater part of the time. Has occasionally sharp pain in chest. Appetite good. Bowels regular. No cough. Pulse somewhat accelerated. He continued to improve, and was quite well at the expiration of my period of service at the hospital, April 1st.

This case interested me much from the constancy of a tympanitic resonance so general and considerable, over the affected side, as, at one time to lead to the suspicion of pneumo-hydro-thorax. In referring to the notes of the case, of which the foregoing history is an epitome, omitting entirely the treatment, and other particulars, I regret to find the account of the physical explorations not so full as I could have desired. I have omitted, in the sketch that has been given, to mention, what is embraced in the records, that the epigastrium emitted a tympanitic resonance on percussion.

It is sufficiently clear that this was a case of pleuritis with large effusion. Assuming that the tympanitic resonance over the chest was due to the presence of gas in the stomach, it was transmitted through the fluid contents of the pleural cavity in every direction, in this respect differing from the case previously given. It will, however, be observed that as the quantity of fluid diminished, as evidenced by the contraction of the chest, the tympanitic sound was present at the upper, but absent at the lower portion of chest, showing, as in the former case, that the resonance was transmitted with greater facility through condensed lung, (condensed in this instance from compression,) than through a collection of liquid. It was interesting, in this case, to watch the development of the normal vesicular resonance, gradually supplanting the tympanitic.

If the rationale we have offered of the peculiar feature of the above cases be correct, the practical inference is, that we may expect, in some instances of pneumonitis, and pleuritis, to find an abnormally clear resonance on percussion, co-existing with the auscultatory phenomena denoting these

affections; and, without being aware of this unusual combination of physical signs, the diagnosis may be attended with difficulty.

Permanent disparity of the resonance, and respiratory sound of the two sides of the chest, the effect of pleuritis and pneumonitis.—In employing physical exploration, it is important to bear in mind that a marked disparity in resonance, and respiratory sound, may be due to an attack of pleuritis, or of pneumonitis, experienced many years prior to the examination, and, hence, is not to be considered, necessarily, evidence of present disease. Equality in resonance and the respiratory murmur obtains in the vast majority of persons in whom the thoracic organs are healthy, and this rule is of fundamental importance in deriving information from physical signs. A slight disparity, taken in connection with symptoms, and history, often suffices to decide the question as to the existence of tuberculous deposit in the lungs. It is in suspected cases of pulmo-tuberculosis that the rule just mentioned is of especial consequence. If a patient have severe pneumonitis, and recover complete health, the affected lung, nevertheless, may never regain the same amount of vesicular expansion that it had before the attack. The vesicular structure is not compromised in a degree to occasion any evidences that the pulmonary function is impaired, for, in the conservative arrangements of Providence, a capacity of lungs is provided beyond that which the necessities of the organism require. The patient may not be in the least conscious of any defect, while an injury has been produced sufficient to disturb the natural harmony existing between the two sides of the chest. The same effect may follow an accumulation of liquid in the pleural sac, which is completely absorbed, leaving the chest more or less contracted on one side, and the pleural membrane thickened by the organization of the effused fibrin. In either case the proportion of air to solids is lessened, and, hence, there is, ever after, more or less relative dullness on percussion, and a relatively feeble respiratory murmur. On this account it is not so easy to determine, in all cases, the existence of tuberculous deposit, if the patient have, at any past period of his life, suffered from pleuritic, or pneumonic inflammation, as it usually is, if these diseases have not preceded. The importance of obtaining information on this point in cases which present themselves for examination, is, at once, obvious. I have several times met with instances in which a difficulty in diagnosis proceeded from this cause. The following case, which came under observation but a few days since, will serve as an illustration:—A patient over forty years of age, applied to be examined for life assurance. No evidence

appeared from his history, parentage, or present symptoms, of tuberculous disease, unless the existence of chronic pharyngitis, of the variety called granular, be so considered, which, in so far as the observations of the writer go, indicates the reverse of a tubulous cachexy. On exploration of the chest, the right side presented a slight dullness of resonance compared with the left side, and there existed, on the same side, a relatively feeble respiratory murmur. Under these circumstances, tuberculosis was suspected, notwithstanding the patient supposed himself to be well, save the pharyngitis, his aspect denoting health, and nothing, as just remarked, sustaining the suspicion of phthisis derived from the disparity between the two sides of the chest. The patient had never been seriously ill but once, and in that instance he had an attack of fever, followed by inflammation of the right lung. That the right lung was the one affected was shown by the marks of cupping on that side, which were still apparent. This was eighteen years previous. The probability is, that in this instance there is no present disease of the lungs; but the permanent effects of a severe pneumonitis, experienced so many years before, will always render it somewhat more difficult to appreciate the early physical signs of tuberculosis, than if he had never been the subject of pneumonic inflammation.

New method of demonstrating the presence of liquid effusion into the cavity of the pleura on one side.—The physical signs of an accumulation of liquid in the pleural sac are, already, sufficiently ample and explicit; the following additional method, however, may possess some practical value: Suppose a case of pleuritic effusion, limited to one side. If pressure be made on the healthy side, upon the cartilages of the false ribs, nigh to the sternum, an impulse is communicated to the opposite side, which is seen to be communicated equally to the *whole* of the affected side. If, then, an equal pressure be made over a corresponding point on the affected side, i. e. upon the cartilages of the false ribs, nigh to the sternum, the healthy side will be elevated nigh to the sternum, but the whole chest will exhibit little or no motion. The difference in the two instances will be obvious.

The philosophy of the difference is supposed to be this:—when pressure is made on the healthy side, over the yielding cartilages nigh to the sternum, the impulse is communicated to the mediastinum, thence to the liquid contents of the pleural sac, and, by a well known law of hydrostatics, the momentum is distributed by the latter, equally, in all directions. This accounts for the motion being communicated to the whole side, in that instance. When, on the other hand, the pressure is made on the affected

side, in the same relative situation, the impulse, in like manner communicated to the mediastinum, is transmitted to the lung, which readily yields, causing an elevation of the chest directly above it, i. e. nigh to the sternum; but the momentum being lost in the elasticity of the lung, motion is not transmitted to the whole side, or in a less degree than when pressure is made on the healthy chest, in the former instance.

By this method the presence of liquid effusion in one side is shown. The writer has not made the experiment of a similar comparison of pressure on the two sides, in a case of solidification of the lower lobe of the lung on one side.

BUFFALO, May, 1850.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

A Case of Poisoning by Corrosive Sublimate. By BENJ. W. MCCREADY, M. D., of New York. Communicated by Dr. T. R. BECK.

I was called on Friday, December 28, about 11 P. M., by my friend, Dr. G. O. Gunn, to see with him a young woman who had poisoned herself with corrosive sublimate. According to the account of the family, she had taken tea with her father and brother as usual, between six and seven o'clock in the evening, and immediately afterward had retired to her own room. About ten P. M., her brother going up stairs, had his attention attracted by the noise she made in her room, and on entering it, he found her vomiting and retching violently. On being questioned, she stated, that immediately after tea she had poured out a cup full of an alcoholic solution of corrosive sublimate, which was kept in the room as a bug poison, and had swallowed the whole of it. The solution was of the strength of a drachm of the salt to half a pint of alcohol, and as she had swallowed about three ounces of it, she had consequently taken about 22½ grains of the poison. Dr. George O. Gunn was immediately sent for. The vomiting was encouraged by draughts of warm water, and a bowl full of white of eggs was prepared and administered, though from the length of time that had elapsed since the poison had been taken, and the copious vomiting that had since occurred, Dr. G. did not hope for much good effect from their employment.

I found the patient exceedingly restless, with a cool skin, an anxious countenance, and a frequent, feeble pulse. She made no complaint, but when questioned, said that she had a burning pain in her stomach, and that she found a good deal of difficulty in swallowing. There was great thirst, with retching and vomiting. The tongue was pale, but there were some bright red spots, with a well defined margin in the posterior fauces. A full opiate was ordered, with ice and diluent drinks, together with hot fomentations to the abdomen.

On Saturday, to our surprise, Dr. G. and myself found her with a pale but tranquil countenance, the pulse about ninety, and of good volume, the skin cool and the thirst abated. She had two stools during the night, feculent, but containing a little blood. Simple diluents were continued, and it was agreed to administer opiates according to circumstances.

From this time (Saturday,) I did not see the patient again until the following Friday, January 4th. According to Dr. Gunn, she had slight dysentery during Sunday and Monday, having each day five or six mucous passages with a little blood. From this time she had but one stool a day. On Monday suppression of urine occurred, and from Monday till Friday the patient passed no urine at all. The catheter then having been introduced, Dr. G. drew off less than four ounces of somewhat turbid urine. Her skin remained, during the whole time, cool, and the pulse was never above ninety. During Monday, Tuesday and Wednesday the patient did not vomit, but on Wednesday evening she ate, through the injudicious indulgence of an attendant, two tea biscuits, and from this time vomiting recurred, consisting chiefly of the fluids drunk, mingled occasionally with a little dark grumous matter. The patient made no complaint, but appeared to become weaker. She slept a good deal, but was easily roused, and occasionally seemed to wander slightly in her mind. Her gums had become somewhat swollen, and there was slight salivation. I found her with a dull and listless expression of countenance and a feeble pulse, about 90 in a minute. She made no complaint on firm pressure being made over the abdomen. There was frequent vomiting and great prostration of strength. On the following day she died.

A post mortem examination was made twenty-four hours after death by Dr. Holmes, the assistant of the coroner. The abdomen alone was opened, and even of this the examination was very imperfect. Externally the stomach, the small and large intestines appeared very minutely injected with blood. The bladder was contracted and contained no urine. I obtained from Dr. H. the stomach, a portion of the ascending colon, and the kidneys. The stomach was much injected, the mucous membrane softened, and in spots slightly eroded. The portion of the colon was vividly injected, the mucous membrane thickened, and between the longitudinal bands there were a number of well defined rounded ulcerations with elevated edges, about the third of an inch in diameter, and extending completely through the mucous membrane. One kidney was much enlarged, and presented evidences of slight congestion. On the surface of the other kidney, several serous cysts had been developed, causing atrophy of its proper substance. One of these cysts had burst, and had separated extensively its capsule from the kidney; turbid serum with a white curdy matter escaping when the capsule was cut into. The other cysts on being opened were found to be filled with a similar matter.

The case is note-worthy on several accounts.

1st, The general symptoms bore no relation to the amount of local disease. After the first few hours, the pulse was moderately full, and not above ninety, and the temperature not unusual. This might be connected with the state of the blood after the occurrence of the suppression of urine, but it was present previous to the coming on of the suppression.

2d, The amount of inflammation and ulceration of the large intestines was such as occurs commonly with severe dysentery, and yet the dysen-

teric symptoms were slight, much slighter, indeed, than commonly occur in cases of poisoning by corrosive sublimate.

3d, Suppression of urine continued from Monday until Saturday, the day of her death, and yet the patient retained her senses, and was easily roused, exhibiting no signs of cerebral disturbance except slight drowsiness, and some tendency to wandering.—*Transactions N. Y. State Med. Society.*

Observations on the use of Quinine and Opium, in Acute Rheumatism.

By JOSEPH PARRISH, M. D., Editor of the New Jersey Medical Reporter.

Being some miles from home, waiting the movements of an obstetric patient, I conclude to fill some of the blank sheets of my memorandum book, for the printer, with observations on the use of quinine and opium in acute rheumatism. Almost every physician with ample opportunities for experience, after a few years practice, selects from the *materia medica* his favorite remedies for the treatment of certain diseases; for though we cannot always trace in the mind, a clear adaptation of the known properties of a medicine, to the morbid condition of the system for which we may be called upon to prescribe, we are sometimes tempted to depart from the beaten track, and enter the field of hypothesis, when we feel assured by past experience, that there is room for improvement. Acute rheumatism is a very common disease, for which a common mode of treatment has long been acknowledged. The presence of severe pain, active external inflammation, and a pulse above the normal standard, would seem to indicate the propriety of blood-letting as the primary remedy; such is the received opinion of the profession, and it is believed the majority of practitioners act in accordance with it. My own experience has not warranted the adoption of this opinion. The symptoms of the disease under consideration need not be recapitulated here, they are familiar to the whole profession; but there is one peculiarity in rheumatic inflammation which deserves special notice as having an important bearing upon the treatment; it is its fugitive character, its tendency to remove suddenly from place to place, and scarcely ever to terminate in suppuration. At any rate it may be called a *peculiar* inflammation.

If a patient complains of acute throbbing pain in any part of the body, and the skin becomes inflamed and hot, pressure intolerable, or even the slightest touch, and we witness the progress of this action from day to day, till suppuration is established, and an abscess forms, we see a very different state of things from what happens in rheumatic pain and fever. In the latter the suffering may be as acute, the local inflammation as decided, and yet it will suddenly leave that part, and as suddenly fix itself upon a distant region of the body, or perhaps, if there exists any irritation or functional derangement of any of the internal viscera, it may be located there, as in the heart, stomach, or other organ. We have considered this to be a distinctive peculiarity of rheumatic inflammation, which demands of the practitioner a mode of treatment different from what is applicable to common inflammation. Acting upon this view of the subject, the disease has recently been treated as follows, with the happiest results: To evacuate the elementary canal as a preliminary step, is obviously impor-

tant; for this purpose I generally prescribe a combination of ten grains each, of blue mass and prepared chalk, to be followed in a few hours with a full dose of magnesia and wine of colchicum; the bowels being freely moved, from five to ten grains of quinine, with from one to two grains of opium, according to the age and constitution of the patient, and the severity of the pain, are given at night; this will generally procure sleep and freedom from suffering during the following day; the effervescing mixture or small doses of Dover's powder are continued at intervals of two or three hours, and bland mucilaginous drink allowed, which serve to nourish the patient, and keep the bowels soluble. I have not unfrequently known the combination of quinine and opium to operate as a laxative, and thus remove the necessity of any other cathartic. Some twenty cases of the disease I have treated on this plan; not one of them lost a drop of blood, and they all recovered speedily; most of them were females.

The applicability of quinine in scrofulous inflammation is generally admitted; in rheumatic ophthalmia it is often employed, and in erysipelas its efficiency is not questioned. As a journalist and a practitioner of medicine, we are not at liberty to withhold from the common stock, any thing that promises advantage to the sick, or to the profession; hence, these views are submitted with due deference to the opinions of those of more advanced age and experience, and who have enjoyed greater opportunities of treating the disease; but they are the result of careful observation, within the limited sphere of my professional labors, and as such, are submitted to the medical public.—*New Jersey Medical Reporter.*

Statistics of Medical Schools—1849-50.

	<i>Students.</i>	<i>Graduates.</i>
University of Pennsylvania, - - - -	438	
University of the City of New-York, - - -		111
Harvard University, - - - -		23
College of Physicians and Surgeons, N. Y., - - -		49
Medical College of Ohio, - - - -		42
Buffalo University, - - - -	115	27
Yale College, - - - -		16
Rush Medical College, - - - -	120	44
Albany Medical College, - - - -	100	26
University of Louisville, - - - -	376	113
Transylvania University, - - - -	92	32
University of St. Louis, - - - -	110	
University of Missouri, (St. Louis), - - -	110	
Medical College of Evansville, - - - -	40	6
Indiana Central Medical College, - - - -	50	10
Starling Medical College, - - - -	154	
Jefferson Medical College, - - - -	516	211
Pennsylvania Medical College, - - - -	106	34
Medical College of Georgia, - - - -	179	44
Berkshire Medical College, - - - -	93	23

—*North Western Med. and Surg. Journal.*

Injections of Nitrate of Silver in Dysentery. By J. W. RICHARDSON, M. D., of Rutherford Co., Tenn.

Dr. Richardson said that he wished to direct the attention of the Society to the use of injections of nitrate of silver in malignant cases of dysentery. He had first used it in Sept. 1848. The patient was a delicate female, mother of several children, who had been laboring under a severe form of dysentery for five or six days, and who had been well treated in the usual manner by an intelligent physician. All the usual remedies having failed to afford any relief, Dr. R. said that on his second visit to the patient, whilst thinking of the case, and having witnessed the unsuccessful use of many remedies, the inflammation and ulceration of the mucous surface of the large bowels characteristic of dysentery, suggested the nitrate of silver as the very remedy to relieve them. Upon his arrival, he found the patient worse than she was the day before. The discharges were very frequent, consisting of large quantities of dark, gangrenous looking mucus, and more offensive than any he had ever smelled. The woman was nearly pulseless—every discharge produced the most deadly sickness, and she could not be turned on the bed without approaching syncope. Dr. R. advised the nitrate of silver, an injection of which was prepared, about 15 grs. to 4 ounces of water, by the attending physician, and thrown up the rectum. This remained some six hours before it was thrown off, and the bowels were quieted until the next morning, when, as there seemed to be a tendency to a return of the dysenteric symptoms, the remedy was repeated. The disease was arrested immediately, and the patient recovered. Dr. R. said the result of this case, and the speedy and effectual relief afforded by the nitrate of silver, filled him with astonishment and pleasure:—*astonishment*, because he had never before thought of the remedy in intractable cases of dysentery, when he had so often witnessed its remedial virtues in inflammations and ulcerations of various mucous surfaces; and *pleasure*, because he thought that he had made a grand and important discovery! But he said, when reading Braithwaite's Retrospect a few evenings afterwards, he found that the remedy had been used in Europe in similar cases, before he had ever thought of it. See Braithwaite's Retrospect, part 16, p. 156, c. 7, where you will find some valuable facts recorded in favor of the use of nitrate of silver, not only in dysentery, but also in the troublesome, frequent and painful diarrhoea in typhoid fevers, and in the protracted diarrhoea of young children. He thought the remedy might be used in much larger doses.

Dr. R. had used the remedy since, in several cases, after the usual course of treatment had failed, where the patients were completely prostrated, and invariably with success. He used it in about the proportion of 10 grs. to 4 and 6 ounces of rain water, if the latter could be obtained, if not, he used common spring or well water; and he had also combined laudanum, and sulph. morph. with the nitrate, but could not say that the smarting or burning was prevented by the combination. The pain produced by the injection was not *always* sufficient to make the patient complain, though it did sometimes. He thought that he had used as much as 15 or 20 grs., or that this quantity had been used by his advice in one case. He did not desire to fix the precise quantity of the nitrate, nor water to be used

in the enema, so much as to direct the Society to the remedy, and to solicit their experience.

Dr. Avent said that having heard of the use of nitrate of silver as an injection in dysentery, he had tried it in the worst case of dysentery he had ever seen, and which had resisted all treatment for five days. He said he gave only one enema, composed of 20 grs. of the medicine to 4 ounces water, which afforded immediate relief, the griping and purging ceasing instantly. He had also used it in another case, not so bad a case, however, but with marked relief.

Dr. Gordon said that he was not surprised at all at what he had heard from Drs. R. and A. as to the efficiency of nitrate of silver, in malignant cases of dysentery. He was prepared to believe any thing almost as to the remedial effects of this article in diseases of mucous surfaces—indeed it was the greatest of all remedies. But he had been in the habit of treating dysentery in a different way altogether to the one commonly pursued. He converted the dysentery into a diarrhea by giving Epsom salts in small and separate doses, and the patient scarcely ever needed any thing else. He scarcely ever gave any other medicine. The diarrhea ceased of itself by withholding the salts, and the patient got well.—*Proceedings of the Tennessee Med. Society.*

Account of two Cases of Poisoning by Lobelia Inflata. By T. Wood, M. D., Cincinnati, Ohio.

I am inclined to look on quacks as a very useful class of practitioners. They are experimenters, and, strange as it may seem, are seldom in want of voluntary subjects, to swallow any medicine they may fancy to have miraculous virtues; and they seldom fail to persevere in its use, till its effects are known on every constitution, age, and sex, as well as all the diseases that are found in the catalogue of human ills. It is a subject of regret that their experiments cannot be witnessed by competent observers, and the results accurately recorded for the benefit of science; but this is generally out of the question. The enlightened observer is mostly a man having human sympathy, and cannot silently witness suffering that he can prevent or allay. The quack gives his medicine with his mind in a state of mystified confidence that it will do good, and has not enough of knowledge to distinguish between the effects of his nostrum, and the disease that he designs it to cure, and therefore, can look with indifference on a scene that would shock a man of science. If the scientific practitioner were to witness, without disapprobation, one of the many deaths that occur from empirical experimenting, he would be indicted for manslaughter; while the very man who gave the fatal dose, would be acquitted by a jury, on the plea of ignorance. But to return. If a regular record could be kept of the effects of all the nostrums used and vended by quacks in every case, much useful information would be gained by the regular profession; and the many lives that are sacrificed, would not all be lost in vain, but their martyrdom would be a legacy of infinite value to the wise and prudent patrons of the legitimate healing art. But while I regard quacks as conferring great favors on a large portion of the human family, *who are fortunate enough to escape from being their victims*, I cannot help

feeling astonishment at the strange infatuation they exercise over so many persons, and over those who are distinguished for good sense and intelligence, on all subjects, except that of their own health.

Many of our most valuable remedies have been introduced to the notice of the regular profession by quacks, and often at a fearful sacrifice of life, before their true virtues were known; and perhaps none have been introduced at a greater cost of life, than that of lobelia inflata. I have seen many cases of injury by its use as an emetic, but the two following will be sufficient to illustrate its poisonous properties.

CASE 1st. In the summer of 1843, I was requested by the friends of J. Johnston, of Mt. Pleasant, Jefferson county, Ohio, to make a post mortem examination of his remains, and if possible, discover to them the cause of his sudden and unexpected death. During the course of the examination, the following facts were elicited:

Mr. Johnston, a carpenter, had been a healthy hard working man; but from some cause had been troubled for the last three years of his life, with an eruption about the nose, which he was anxious to get rid of, more from its unsightly appearance than from any pain that he had experienced, or danger that he apprehended from its presence. He had taken the advice of several physicians, but none had given him permanent relief; and he had finally concluded that it was his fate to bear his affliction, without a hope of cure. But while he was trying to reconcile his mind to his gloomy destiny, his hopes were suddenly rekindled by a *steam doctor*, who had lately stepped into the professional ranks by a "short cut," and had made his appearance in the village. This man promised to eradicate every vestige of the disease from his nose, by one "course of medicine, to be applied by the most approved principles of Sam'l Thompson's system; and as a further inducement to Mr. J. to give it a trial, told him that if he would commence on Saturday night, after quitting his work, he might go through it on Sunday, and on Monday morning resume his labors; a renovated if not transfigured man, free from all his bodily ills.

Mr. J. was allured by the quack's promises, and determined to test his skill; and on Saturday night about six o'clock, he took the first dose of his medicine, and in thirty-three hours he breathed no more.

The steamer was present at the examination, and gave in substance the following statement of his treatment: "Four table spoonfuls of the Tinc. of lobelia were given, at intervals of fifteen minutes between each dose, with a view to induce vomiting. By the time the last dose was taken, the patient complained of extreme sickness of stomach, entire prostration of strength, and a frothy phlegm was rising into his mouth, nearly strangling him, but no vomiting could be induced. A table spoonful of saleratus was then dissolved in a teacupful of water, and the patient made to swallow it to 'kill the effects of the lobelia,' as Thompson directs. Shortly after this, he was seized with intense burning pain in the stomach, and was given a cup of vinegar to destroy the saleratus." This caused such violent eructations, and biccough, as to alarm Mr. J.'s family, and Drs. Hamilton and Flanner were sent for, but it was too late—their efforts could not save him.

The dissection showed a softened mucous membrane, but the most striking peculiarity was the great distention of the stomach and bowels, by the gas liberated by the action of the vinegar on the potash. As the walls of the abdomen were cut through, the stomach and bowels burst from their

confinement, with a force that seemed to threaten a rupture of their coats. This enormous distention existed in the stomach, extended through the small intestines, and terminated at the ileo-cæcal valve. The colon throughout its whole extent, was contracted so much as nearly to obliterate its cavity—presenting the appearance of a solid cord, about the thickness of a man's finger, and it had nothing in it but a small quantity of mucus.

After the examination was completed, I was requested to make a statement of my opinion of the cause of death, in the council room, to the assembled citizens of the village. The steamer was present, and with the effrontery characteristic of an ignorant, unprincipled charlatan, answered, and explained all that I said, much to the satisfaction of himself and his friends. When I stated my belief that the man lost his life by the steamer's treatment, he declared that the patient was killed by the calomel doctors who had taken him out of his hands, and that if his course of medicine had been gone through with, the man would be still alive.

I endeavored to explain impartially to the audience, the effects of lobelia on the system, when not thrown from the stomach by emesis, and the corrosive effects of saleratus on its coats in such large doses, and lastly, the action of the vinegar on the alkali with the disengagement of gas. When I read from the United States Dispensatory, the evidence of the poisonous nature of lobelia, the steamer read from the work of Dr. Curtis, of Cincinnati, the proof that it was an innocent and harmless plant. As to saleratus, he said it could not hurt any person, for it was eaten every day in our bread and cakes—and to say that vinegar was poison, "it was too ridiculous to deserve any comment from him."

I left Mt. Pleasant convinced of the fallacy of an attempt to reason with a popular audience, on a medical subject; for the steamer made them believe that he was a persecuted man, and their voice was in his favor. His practice, however, was very limited, and his next patient was his own wife, whose death was about as sudden as that of Mr. J.'s, but no examination was had of her case.

The steamer finally abandoned his profession, probably disgusted with the uncertainties of medicine; and the last I knew of him, he was selling candy and pea nuts in the city of Covington.

It appeared from the testimony of his friends, that Mr. Johnston had never vomited in his life, his stomach having been peculiarly insensible to the action of emetics.

CASE 2D. On the 13th of June, 1849, I was called to see John Goffern, who had taken three teaspoonfuls of powdered *lobelia*, under the direction of a steamer who has a store on Main street, above Eighth, Cincinnati.

When I arrived, (about two hours after his taking his first dose,) the patient was covered with a cold clammy sweat—pulse 40, and very feeble—skin blue, particularly about the head and face—abdomen very much distended with fluids—and the whole muscular system relaxed, so that he had no control over the sphincter of the anus to retain injected fluids; and his abdomen felt like a loose sack filled with water—destitute of muscular fibres. The pupils of the eyes were contracted, and the action of the brain much stupefied. He had an insatiate thirst, and complained of intense agony in the stomach. I requested the advice of Dr Latta, who concurred

with me in the opinion that the use of the stomach pump was necessary to save his life. It was used by Dr. L. with success, giving relief in a short time.

The steamer had prescribed the lobelia to cure the ague, but, although the man had experienced the full effects of the medicine, he had a chill on the day following its administration.

The ague soon yielded to the proper use of quinine, and Mr. Goffern has no desire to try lobelia again.—*Western Lancet*.

Treatment of Burns.—Perhaps there is no accident in the catalogue of surgery, wherein greater discrepancy of opinion exists regarding the best treatment, in the primary condition, than that of burns. One succeeds best by the antiphlogistic, another by the stimulating plan—while some contend for a medium between the two. It appears to us, that there can be but one principle upon which should be based the primary treatment of burns, and that is the vital one of inflammation. It is most emphatically inflammation, and of course attended with its results, either in a low or high degree, according as the parts are injured. A simple burn, or that considered of the first degree, wherein there is slight redness of the skin, will readily resolve itself, like unto all simple inflammations, without any, or the mildest and most simple treatment. But when an extensive surface is invaded, and the parts beneath are deeply involved, then the matter is entirely different, and requires a prompt and active treatment. The location of the lesion has something to do in our management and treatment of such injuries; for burns upon the head, face, neck, thorax and abdomen, produce greater constitutional symptoms, and are more dangerous to life, than the same extent of injured surface upon the extremities. The indication, then, required in our management, is, to lessen the pain, and make the inflammatory process as little destructive as possible; and if it cannot be prevented, then, of course, to promote the separation of the dead parts from the living, in a manner best calculated for the comfort and safety of the patient. We have lately had an extensive burn under our care, in the case of a lady, who, while attempting to give what is called a *rum sweat* to another lady, had her own dress take fire from the burning alcohol, nearly all of which was consumed upon her body before any one could render her the least assistance. We were upon the spot within ten minutes after the accident, and found the patient in a very bad condition. Both arms, from the elbows to the top of the shoulder, were much burned; also the left side, up to the axilla, and on to the top of the thorax. There were many kind friends present; some recommending most of the popular quack remedies of the day, while others were for the usual domestic remedies, some of them quite rational. Linen cloths were dipped in alcohol and water, and the burn was dressed with them. They were not suffered to be removed for twenty hours, and during this time they were kept thoroughly saturated with the cold water and alcohol; at the commencement, equal parts of each, but subsequently two thirds of alcohol to one of water. The patient was relieved from pain upon the first application, and would have passed the time in a very comfortable manner, had she not suffered from the nervous excitement produced by the accident and the usual constitutional symptoms attendant upon such an extensive injury. The subsequent treat-

ment has been to leave the parts undressed, or simply smear them over with gum water. On the sixth day (at the time of this notice) there is no appearance of sloughing, and the patient has rallied, and is very comfortable. This plan is recommended by Mr. Kentish; and the simplicity of the dressings, together with the comfort that is afforded the patient, should be sufficient inducements for every practitioner to make use of it without any prejudice.—*Boston Med. and Surg. Journal, (Editorial.)*

Case of Hemorrhagic Diathesis following Suppression of the Catamenia, and attended by Vicarious discharge from the Gums—terminating fatally from hemorrhage following scarification. By I. B. DUNLAP, M. D., of Norristown, Pa.

About the 1st of February last, I was called upon, during the absence of her physician, to visit a young lady about twenty-two years of age, who was discharging blood profusely from the mouth, and who, judging from her appearance, was laboring under considerable hepatic derangement. The attack of hemorrhage had commenced four or five days previous, and had continued without intermission up to the moment that I first saw her. On making inquiry into her previous condition, I was informed that she had not enjoyed good health for the past year, but had labored under more or less acute continuous pain in the right hypochondrium, together with considerable irregularity in her menstrual discharge. For the relief of these difficulties, she had been under medical treatment, but derived no permanent benefit from the advice and prescriptions of her attendants. She remained in this condition for about six months, when she became completely jaundiced, and her catamenia were entirely suppressed. Six weeks from the period at which she last observed any appearance of the monthly discharge, she was attacked with severe pain across the loins, nausea, and a profuse discharge of blood from the mouth; her physician was sent for, who made use of the ordinary remedies for arresting hemorrhage, but without effecting any diminution in the discharge, which continued without intermission for five days, and then suddenly ceased. After this, she enjoyed tolerably good health, suffering no uneasiness with the exception of the pain in her side, until the expiration of a month, when she was again attacked with hemorrhage, and which, under medical treatment, ran its course, as before, uncontrolled. For the last four months she has had these periodical attacks of bleeding, enjoying comparatively good health during the intervals.

Having taken charge of the case by the request of the family, I was determined to watch it closely, and ascertain, if possible, from whence the hemorrhage proceeded. I therefore requested the family to apprise me the moment that the premonitory symptoms of a return of bleeding should present themselves. Accordingly, in the course of a few days I was sent for, and upon opening the patient's mouth, saw distinctly the blood oozing from the gums in front of and behind the teeth, and as far back as the first molars, beyond which I am certain that no blood proceeded. By wiping off the gums the blood in an instant would again swelter out, covering them completely, and by making pressure upon them, blood could be forced out in increased quantities. I may here remark, that the blood was neither

thrown out by hawking, coughing, or vomiting, but discharged by incessant spitting. The bleeding continued to increase, as it had done on other occasions, in spite of all remedial applications, for five or six days, when it spontaneously subsided. During each of these paroxysms of hemorrhage, there was discharged, as near as we were able to ascertain, about six quarts of blood. I now endeavored to restore her catamenia, and correct, if possible, the derangement of the liver. To meet these ends the patient was put upon the use of the sulph. ferri exsiccata with aloes, alternated with blue mass combined with the extracts of taraxacum and gentian; at the same time, counter-irritants were freely applied over the region of the liver, and the nitro-muriatic acid foot-bath made use of every night at bedtime. This course was pursued for four weeks, when a return of bleeding from the gums again took place. This continued some two or three days longer than usual, and the patient was evidently more prostrated from this loss of blood than from any of the previous ones. Port wine with tinct. rhatany, was freely given, and the bleeding appeared to diminish, when, unfortunately for herself, during my absence, on account of the severe pain in her side, and by the advice of a meddling friend, she sent for a cupper, who applied some half a dozen cups on the right hypochondrium, the consequence of which was that the bleeding from the gums entirely ceased, but it was found impossible, by the use of any means, to arrest the hemorrhage from the scarifications. She soon became pulseless, and in less than six hours died. To satisfy myself in regard to the source of the bleeding, I called in my worthy friend Dr. H. D. W. Pauling, who, after examining the case, fully concurred with me in opinion.

Autopsy.—Shortly after death, an examination of the body was made, and the thoracic and abdominal viscera carefully examined. The contents of the thorax were in a perfectly normal condition, and the viscera of the abdomen, with the exception of the liver and ovaries, were in a similar state. The latter organs were somewhat enlarged. In the gall bladder were found seven calculi, each about the size of a hazel nut, of an octangular form, with their faces ground perfectly smooth by rubbing against each other.

I regard this as an interesting case in several particulars. Here was evidently a vicarious hemorrhage, a substitute for the catamenia, and this proceeded altogether from apparently healthy gums, and from but a very minute surface when compared with the large amount of blood thrown out. Again, there was certainly a hemorrhagic condition of the system, which was not at all amenable to remedial agents, as was shown by all the means made use of to arrest the bleeding from the gums, or scarification, all of which proved abortive. But does it not appear remarkable, that the moment the cups began to fill, the bleeding from the gums should cease entirely? We are aware of the fact that blood-letting frequently acts as a derivative in hemorrhage, but never before saw a case where it produced such an instantaneous stoppage to the original bleeding, nor of its occasioning fatal results in consequence of the inability to arrest it.—*N. Y. Jour. of Med.*

A Case of Spontaneous Evolution of the Fœtus. By J. S. MITCHELL, M. D., Charleston, S. C.

Messrs. Editors.—As the question of “Spontaneous Evolution” is still an unsettled one, I have thought it would not be uninteresting to your readers to have the following case detailed.

Doctor Denman was, I think, among the first who boldly asserted that it was possible, under an arm and shoulder presentation, to have an unassisted termination of a case, the breech being first expelled. His idea was, that the action of the uterus, long and forcibly continued, so compacted the body of the fœtus, as to expend upon it the full force of each returning action. The body, in its doubled state, being too large to pass through the pelvis, and the uterus continuing to act with force upon the inferior extremities—they alone being movable—are driven down, and thus being forced lower, the body turns as it were upon its own axis, and the breech is expelled as in an original presentation of the same. This idea of Dr. Denman’s was generally received throughout the profession, as the proper explanation, until the publication, in London, of a pamphlet, by Dr. Douglass, entitled “An Explanation of the real process of the Spontaneous Evolution of the Fœtus.” In this essay, Dr. Douglass denies the position taken by Dr. Denman, and remarks that it is impossible for the uterus, while contracting, to act upon a part only of the compacted body, thus forcing it lower into the pelvis, while the other is allowed to recede into a higher position; he goes on to state that the arm, shoulder and thorax are the first expelled, and the nates and head afterwards. This latter opinion, based upon good reasons, appears now to be the one most generally adopted. By an examination of the following case, it will be evident, however, that it may happen, as stated by Dr. Denman, viz: that a case, under arm and shoulder presentation, may terminate unassisted, by spontaneous evolution, and the breech be expelled first.

On the morning of the tenth of June, 1849, I was sent for to visit Mrs. W., whom I found engaged in her third accouchment. An examination, *per vaginam*, discovered to me a head presentation; the os uteri was slightly dilated, and the pains, though trifling, were frequent. Having other engagements of importance, I ventured to leave with the usual promise of a speedy return. It was not long after my departure, when I was again sent for, and I hurried to the bed-side of my patient. I found, on my arrival, that the head of the fœtus had already passed through the os externum, and before I could make any arrangements for assisting the delivery, the remainder of the body had passed out. I quickly placed my hand upon the abdomen, with a view of securing a proper contraction of the uterus, when I discovered there a tumor of sufficient size to excite some suspicions of the presence of a second in utero. I separated the fœtus from its mother, and prepared for further examination, when I discovered that the last effort of the uterus, which had expelled the first infant, had forced down the arm and shoulder of the second. This condition of things did not last long, however, for the uterus again contracting, I had the satisfaction of *seeing* the arm recede, and the nates, kindly taking its place, protruded through the vulva. I immediately seized it, determined that the capricious conduct of the infant should not again leave me in doubt and anxiety. In due time the nates was delivered, the shoulder and head fol-

lowing in turn. Thus ended favorably to mother and child, a case from which I had a right, under ordinary circumstances, to expect much difficulty; another evidence of the fact that a case, under arm and shoulder presentation, may, by a spontaneous evolution of the fetus, terminate unassisted; the breech being first expelled.—*Charleston Med. Journal.*

Treatment of Nævi Materni. By T. B. CURLING, Esq.—The treatment applicable to nævus must be adapted to its form, according as the growth is cutaneous or the bright scarlet kind; sub-cutaneous, or a livid, puffy swelling; or mixed, partaking of the characters of the cutaneous and sub-cutaneous. Although their external appearance differs considerably, the true structure of the several kinds of nævus is essentially the same. In the cutaneous, the progress of the disease is comparatively slow, owing to the resisting texture of the skin; but this part is so much involved that it is seldom possible to obliterate the growth without the destruction of a superficial layer of the cutis and the formation of a glistening scar. In some instances, after inflammation in the part, or more rarely spontaneously, the bright scarlet appearance fades, and the skin gradually assumes its natural hue. But when a cutaneous nævus is steadily increasing, we must not calculate on this favorable result. In these cases, if the growth be small, and not very prominent, the best mode of getting rid of it is by the application of a powerful escharotic, as the strong nitric acid. If the nævus be of some size, and project, and particularly if it be situated in a part where the areolar tissue is abundant and the skin yielding, as the neck or labium, (which latter is not an uncommon place,) the most effectual plan is to strangulate the growth, either singly, or in two, four or many parts, according to the extent and form of the growth. The removal of a nævus in this way causes a small scar, but the mark is much less evident than would be expected from the size of the tumor, and after the lapse of a few months, at the growing period, is often barely perceptible. In cases in which the nævus is not adapted for tying, owing to its situation on parts which cannot well be destroyed, or where the skin is adherent, the disease may be extirpated by an operation, I believe suggested by Sir B. Brodie, viz: sub-cutaneous cauterization. A very small knife—a small tenotomy one will answer the purpose—is to be introduced through the sound skin at the side of the tumor, and, being passed into its middle, is to be moved about so as to lacerate the morbid tissue in all directions. A fine-pointed probe, coated with the nitrate of silver, is then to be inserted at the small wound, and freely applied to the lacerated part. The application effectually stops the bleeding, which, if not arrested by the pressure of the finger, is generally profuse. It produces inflammation, which leads to obliteration of the nævus. It sometimes causes ulceration, or a small slough of the part, which more certainly secures the removal of the morbid tissue.

The treatment of sub-cutaneous nævus must be conducted somewhat differently from the preceding: it spreads more rapidly than the cutaneous, and generally requires to be attacked without delay; yet even this form of nævus is sometimes stationary, and, after remaining in an indolent state for some months, may dwindle into a small puffy swelling of no impor-

tance. I have occasionally excised from adults a swelling, the progress of which had thus ceased spontaneously. Sub-cutaneous nævi can often be got rid of without destruction of skin, and consequently without any deformity. The principle upon which this is effected is the excitement of inflammation in the reticular tissue, and its consolidation or obliteration by the effusion of lymph. The chief obstacle to success is the indisposition of this tissue to inflammatory action. It may be freely cut up and otherwise actively treated, without more inflammation being excited than is sufficient to repair the mischief and to stop the spreading of the disease for a brief period. Various methods of exciting inflammation are practised: the injection of stimulating fluids, the introduction of setons; the sub-cutaneous application of the nitrate of silver, &c. The plan of passing numerous setons through the tumor has several advantages: it can be used in all cases and in all situations; its effects can be regulated by the period during which the threads are retained, as well as by moistening them with stimulating liquids; it produces no scar, causes but little suffering, and is a pretty effectual remedy. It occasionally fails, and then other means must be resorted to.

The mixed is a common form of nævus, and the most difficult to treat.

The sub-cutaneous portion may be obliterated by the passage of setons, after which the cutaneous will be required to be destroyed by escharotics; but as this double process involves a destruction of skin and the formation of a scar, I generally prefer, if the tumor be not of considerable size, having recourse at once to the ligature, as the most certain and effectual treatment. A mixed nævus, situated on the face, may sometimes be advantageously removed by the sub-cutaneous ligature. In this mode a strong ligature is carried around the base of the tumor, immediately beneath the sound skin at its border, and strangulation of the growth is effected without slough or destruction of the integuments. The sub-cutaneous part of the nævus is effectually destroyed by inflammation and suppuration, and the passage of blood into the cutaneous portion being in a great degree intercepted by constriction of the tissue beneath, this fades and disappears, whilst the nourishment which it receives from the circulation in the adjoining skin prevents it from perishing. This mode of applying a ligature is applicable to many cases of simple sub-cutaneous nævus. It is less painful than, and in other respects preferable to, the plan sometimes adopted, of dividing the skin around the growth before strangulating it with a ligature.—*London Med. Gaz.*

Method of depriving Quinine of its Bitterness. By RICHARD H. THOMAS, M. D., of Baltimore.

BALTIMORE, 2d Month 15th, 1850.

TO DR. ISAAC HAYS:—

Dear Doctor: Believing that I have discovered a method, by which quinine may be quite deprived of its great bitterness, without injuring its virtues in the least, I take this method of making it known to the profession. Perhaps I may have been anticipated; but if it be so, I am not aware of it.

In 8th month (August last,) having occasion to prescribe for a little

patient, who was affected both with diarrhoea and intermittent fever, I ordered a combination of quinine and tannic acid. The child took it so readily, that I tasted it, and was surprised to discover no taste of quinine, which I at once attributed to the combination.

I have since prescribed it in a number of instances, and found that whilst it was equally effectual, it was far more palatable than any other combination of quinine I was acquainted with. On referring to the *American Journal of the Medical Sciences*, Vol. XIX. p. 219 (1836,) it will be found that Dr. Ronander, Secretary of the Swedish Medical Association, recommended, in 1834, the tannate of quinine and cinchonin as the most active ingredients of the Peruvian bark; he asserts that he has cured by their means, several cases of obstinate ague, which had resisted the use of sulphate of quinine, and other powerful remedies, &c., &c. Nothing is said in the extract, from the original paper, in *Hecker's Annals*, December, 1834, of the taste of the tannate of quinine. Compared with the sulphate, it is almost tasteless.

The following is the extemporaneous prescription I am in the habit of ordering for a child two years old: ℞. Quiniæ sulph. gr. x; acid tannici gr. ij; aqua ℥vj; syruga urant. ℥ij. M. A teaspoonful every hour or two.

I enclose a note on the subject, from one of our most intelligent and careful apothecaries:

BALTIMORE, February 6, 1850.

Dear Sir: I find, after trying a number of times, combinations of quiniæ sulphas and acidi tannici in different proportions, that ten grains may be deprived of its bitterness in a great degree, by the addition of one grain and a half of tannic acid. I think this is a proper proportion.

Respectfully, JAMES V. D. STEWART.

DR. R. H. THOMAS.

American Journal of Medical Sciences.

EDITORIAL DEPARTMENT.

Demonstrative Midwifery—Trial for Libel.—A libel suit before the Court of Oyer and Terminer, now in session in this city, has just been concluded, which, from the parties interested being members of the medical profession, and the libelous publication relating to the subject of demonstrative midwifery, it is proper, as a medical journalist, that we should notice. Our readers are already acquainted with the fact that the successful effort to associate demonstrative, with didactic instruction in midwifery, in the Medical Institution of Buffalo, aroused no small degree of opposition among a portion of the medical profession of this city. The letter addressed to the editor of this Journal, and inserted in the number for March, 1850, is sufficient evidence of this fact. In addition to this, considerable excitement among a portion of the community on the subject was produced. How the latter became developed, and the connection subsisting between the opposition in the ranks of the profession, and the excitement existing, to some extent, in the public mind, we will not now discuss, but confine ourselves to the statement of a few facts. The first publication relative to the subject of demonstrative midwifery, was the article contained in the number of this Journal for February, 1850. This article embraced some resolutions adopted by the candidates for graduation attending lectures at the Medical College, expressing their gratitude for the advantages of clinical and demonstrative instruction afforded at the College; together with a letter from Prof. White in acknowledgment of the resolutions, and a few editorial remarks, by way of preface, in which the opinion was expressed, that the plan of teaching midwifery which had been adopted in the Medical College of Buffalo would meet the cordial approbation of the medical profession. We refer to this article to correct some misapprehensions, or, rather, misstatements, and because its propriety has been called in question. It has been said that the adoption of the resolutions was procured through the agency of the Professor of Obstetrics. This was positively denied, under oath, by those who were appointed a committee to draft

the resolutions. It has also been said that they were inserted in this Journal at the solicitation of the individual just referred to. This we aver to be false. They were handed to us by the authorized agent of the graduating class, and inserted without consultation with any person on the subject. To say this, is due to the Prof. of Obstetrics, and to ourselves. The resolutions and the letter were published, because the graduating class requested their publication, and we conceive, not only that they were intrinsically entitled to insertion, but that it would have been censurable to have denied them a place in the Journal. Nor are we persuaded of any impropriety in expressing an editorial opinion that teaching midwifery by demonstration would meet with the cordial approbation of the medical profession, albeit it called forth the extraordinary letter signed by seventeen physicians of Buffalo! We leave it for our readers to decide whether a degree of presumption was displayed in the utterance of such an opinion, greater than in seventeen physicians of Buffalo assuming to denounce it in behalf of the whole profession of the United States! That our opinion was correct, is much more demonstrable now, than then, and we reiterate the opinion, and are prepared to offer pretty decisive evidence of its correctness—but this we leave for the present.

The next publication on the subject of Demonstrative Midwifery, was an editorial article in the Buffalo Daily Commercial Advertiser. Much has been said, in this city, respecting the impolicy and impropriety of the publication of this article, on the ground that its effect was to open the subject before the community, and invite popular discussion of it. We think, with others, that the subject is not a proper one for public discussion, although we differ with many as to the probable result of a fair exposition of its merits, and an appeal to public sentiment. But the truth is, the subject had been already brought before the public, not, it is true, by any formal, fair appeal through the press, but by means of rumor and gossip, with all the distortions, exaggerations, and animadversions which might be expected under such circumstances. We shall not stop to inquire by whom, and for what purposes, the subject thus became the theme of public conversation at the corners of the streets, in taverns, and the sick chamber. This we leave for the imagination of our readers, being resolved to say nothing in our journal, or elsewhere, which shall be inconsistent with the utmost allowance of charity. The article in the Commercial Advertiser was written, avowedly, to quiet public excitement, by assuring the numerous readers of that most respectable print, that the Editor had examined into the facts, and that there was no occasion for any popular dissatisfaction with

the demonstration at the College. Should the correctness of this statement be doubted by any one, we will copy the article referred to, which speaks for itself on this point. The publication for which an indictment for libel was obtained, purported to be a rejoinder to the editorial article just referred to. The libelous publication appeared, as a communication, in the Buffalo Daily Courier of Feb. 22d, 1850. The author of this communication declares that he claims the right to reply to the writer in the Commercial Advertiser, who "has attempted to defend a gross outrage upon public decency;" and in the next paragraph he asserts what has just been stated with regard to the state of public opinion prior to the publication in the Commercial. We quote the language, italicising the passage relating to this point. He says, "I speak of the article in the Commercial of Tuesday, which refers to the recent 'clinical' exhibition at the 'University of Buffalo, Medical Department,' *an article which was evidently intended to foil public opinion, already setting strongly against the perpetrators of the indecency, and through the respectability of the print in which it appeared to give that sentiment another direction.*"

The matter charged as libelous in the remainder of the communication is contained in the following quotations, the italics being ours:—"An *open demonstration of obstetrical practice has been made before a class of medical students. The demonstration consumed nearly or quite eight hours, during a part, at least, of which, the professor of that branch of medical instruction was present. Delicacy forbids me to touch upon the manner in which those hours were passed—suffice it to say that the tedium was relieved by such methods as a congregation of boys would know well how to employ. Thus stand the facts.*" * * * "No school on the face of the earth ever tolerated a like exhibition, save the 'Medical Department of the University of Buffalo.'" * * * "The patient was a woman in humble circumstances, whose poverty, perhaps, overruled her natural modesty. What mattered it, then, if a score of scarcely adolescent youths satisfied their meretricious curiosity at her expense? The Professor had enjoyed his 'clitique,' and his class their *salacious stare*, and under the specious plea of scientific advancement a precedent had been set for *outrage indiscriminate.*"

For the publication of the foregoing, *Horatio N. Loomis M. D.* was indicted by the Grand Jury of this County in April last, and the trial, which was adjourned in May, came on during the June term of the Circuit Court.

It appeared on the trial, that the communication complained of as libelous, was not written by Dr. Loomis, but that his *agency* in the publication

consisted in his procuring it to be set up at the Courier office after it had appeared in the daily edition of the paper, and the type had been distributed, in consequence of which it was inserted in the weekly issue; in his purchasing extra copies, distributing them, reading the article to his patients, and stating that he had been assured of the accuracy of the statements it contained.

The objects of the complainant, Prof. White, in procuring the indictment we understand were, first, to establish to the full satisfaction of the community and the profession, the facts which actually transpired in connection with the demonstration of labor at the College, and, in this way, to vindicate the transaction, the Institution, and himself, from aspersions of which the communication complained of was only a signal instance; and, second, that medical testimony for, and against the utility and propriety of demonstrative teaching might be brought before the ordeal of a legal tribunal. With reference to these objects, as we are informed, stenographical minutes of the trial were taken, and a full report will be published.

A digest of the testimony of those who were present at the demonstration, and the medical opinions elicited at the trial, will probably appear in the next No. of this Journal. Since we have given the statements alleged to be libelous, it is proper to state here that they were not sustained. The reader will perceive, when he reads the evidence, that the demonstration, instead of comprising nearly eight hours, lasted from *two to five minutes*, and that so far from there having been any gratuitous exposure, the genitalia were protected by the hands of the Professor enveloped in napkins, so as to render it doubtful if any part of them could be seen; that the conduct of the class in the lying-in apartment was characterized by the most perfect order and decorum; that so far from this mode of illustration being a novelty, it is practised at Paris, at Berlin, at Giessen, at Prague, at Heidelberg, at Amsterdam, and at Dublin; that instead of being "boys" or "adolescent youths," the graduating class were all, legally, *men*, i. e. at least twenty-one years of age, and that no grounds whatever existed for charging "*meretricious curiosity*," or a "*salacious stare*" upon those for whose improvement the demonstration was instituted.

With respect to the medical testimony, a number of the Physicians of this city were examined, on the part of the prosecution, and defense, some testifying that such demonstrations are proper and useful, and others that they are unnecessary and improper. Some distinguished members of the Profession from abroad were present, viz. Professors Gilman, of New York;

Ackley, of Cleveland; Coventry, of Utica; Lee, of New York; Webster, of Rochester; Dr. Ganson, of Batavia; all of whom, with one exception, were summoned on the part of the prosecution, and all testified to the propriety and utility of demonstrative midwifery.

The trial commenced on Monday, and was not concluded until Friday evening. It was conducted with great ability on both sides. The counsel for the defendant were Hon. N. K. Hall, Henry W. Rogers, Esq., and James O. Putnam, Esq. In the prosecution, Hon. Henry K. Smith was associated with the district attorney, Benj. H. Austin, Esq. The verdict of the Jury was *not guilty*.

Of the justice or injustice of the verdict we have nothing to say. That we have felt an interest in the suit, we do not deny; but this interest has related entirely to the merits of the subject in connection with which the suit originated, and to the motives and ends of those who, with ourselves, are responsible for the credit, or the odium, as the case may be, of the *innovation* which some of our professional brethren profess to consider "offensive to morality and common decency." The consideration of the subject, forced upon us by the extraordinary position taken by a portion of the medical profession of this city, has tended, more and more, to confirm, in our estimation, the importance of uniting with didactic, clinical and demonstrative teaching in obstetrical instruction. In reviewing the medical testimony in the libel case, in a future No., we shall set forth the grounds for this opinion, and, at the same time, consider the reasons offered by those who contend for the *indecent* and *immorality* of demonstrative midwifery.

Objections founded on delicacy and the moral sense, are, so far as our knowledge extends, confined to those of our Buffalo brethren who, at the outset, deemed it incumbent to enter a protest on the subject. We have had some opportunity of comparing views with members of the medical profession in other and different portions of the country, and we have not yet met with any who have taken similar ground. Some have doubted the practicability, or the expediency of the *innovation* at present, but the preponderance of medical opinion we believe to be in favor of its adoption by medical Institutions generally. This we have no doubt will be the case ere many years elapse. As a pretty fair criterion of the prevailing sentiment with the Profession, we may refer to the various medical Journals of the United States, nearly all of which have spoken in favor of demonstrative midwifery. The remarks by our con-

temporaries, on this subject, we shall, in a future No., present to our readers. Our brethren who were impelled to repudiate their fractional participation in the imputation involved in the opinion we ventured to express, that the plan of demonstrative teaching in midwifery would meet with the cordial approbation of the medical profession, will probably be surprised at another assertion which we do not hesitate to make, to wit, that the plan will be sustained by the community at large. We do not regard, as we have already said, the subject one which legitimately falls under the cognizance of public opinion, more than other analogous subjects relating to medical education. But were it necessary, or proper, to resort to public sentiment, and this were done with a fair exposition of the objects to be attained, together with a candid consideration of motives and advantages, without any appeal to prejudices, we should have no doubt as to the result. The subject, unfortunately, has been agitated in this community, not in the manner just mentioned, but by communications in the public prints, deemed by a grand jury libelous, by inflammatory handbills distributed from door to door, and in other modes less conspicuous, but more insidious; and, yet, we are by no means certain that, even under these circumstances, the preponderance of public sentiment among those who have investigated facts, is against it. This, of course, is a matter of opinion, which it is our right to entertain and utter freely, as much as others are at liberty to differ therefrom, as some, if not many, will doubtless do.

The verdict of the *People vs. Loomis*, it is obvious, cannot, with any propriety, be claimed as a disapproval of demonstrative midwifery by a jury of citizens. The jury were not called upon to decide on this matter. In fact, the propriety and utility of demonstrative teaching in obstetrics had but a remote connection with the points at issue, and were admitted rather by mutual consent, and the indulgence of the Court, than from any direct relevancy to the case. Disclaiming, as we do, any feelings having reference to individuals, the ends of the trial, in so far as they have any interest for us, are wholly irrespective of the finding of the jury.

The report of the trial will doubtless be read with interest by the medical public. As connected with the introduction of a mode of teaching instituted alone for purposes relating to the progress of practical instruction in this country; as involving *medical ethics*, and affording indications of medical sentiment of the present time, the trial will rank among the events which make up the history of medicine, and be cited, perhaps, a century hence, as one of the many curiosities which that history embraces.

At the time the grand jury indicted Dr. Loomis, a bill was also found against John E. Roby, Editor of a weekly paper styled the *Christian Advocate*, published in this city, for a libelous publication on the same subject. This cause has not yet come to trial.

A Treatise on the Etiology, Pathology, and Treatment of Congenital Dislocations of the Head of the Femur; illustrated with plates. By JOHN MURRAY CARNOCHAN, M. D., Lecturer on Operative Surgery with Surgical and Pathological Anatomy, &c., &c. New York, S. S. & W. Wood, Publishers, 1850.

We are exceedingly obliged to the Publishers for this valuable monograph. The paper, the typography, the engraving and the *matter*, are all excellent: and it is withal very opportune, for until now no complete treatise on congenital dislocations of the hip joint was to be found in American or English literature. Neither Sir Astley Cooper, nor Sir Benjamin Brodie, both of whom have devoted especial attention to accidents and diseases of the joints, make any mention of this form of disease. In the English translation of Chelius we find a page or two given to the subject, but the laborious annotator, Mr. South, has not thought it worthy of a single paragraph. In France, however, it has recently attracted the attention of some of the most able surgeons and pathologists. Dupuytren wrote a treatise upon it as early as 1826, and since then, Breschet, Sedillot, Pravaz, D'Outrepoint, and Cruvelhier, the two latter of whom our author has omitted to mention, have continued the investigations with considerable zeal and success. The modern German writers, Von Ammon, Schreger, and Stromeyer, ought also not to have been forgotten as having contributed materially to the elucidation of its obscure pathology. It is to M. Jules Guerin, however, of Paris, that we are at present chiefly indebted, and from whose extensive researches, first published in the *Gazette Medicale*, in 1841, Mr. Carnochan has drawn the principal materials of his work.

Chapter *First* is devoted to general remarks upon the subject of which the author is about to treat. The varieties of congenital luxation of the head of the femur in regard to position are, says Mr. Carnochan, four, viz., upward, upward and forward, a sublucation upward and backward, and a complete luxation upward and backward, of which the last only admits of surgical relief, and is the subject of consideration in the present treatise, each of the other forms of displacement occurring only in monstrosities.

Chapter *Second* relates to the anatomical peculiarities of the hip joint,

from the fifth to the ninth month, and is accompanied with an explanatory plate. The anatomy of the bones, muscles and cartilages, is described and delineated, and also, as necessary to a correct understanding of the etiology of the malady, the evolution of the excito-motor nerves is traced from their origin to their complete developement. In reference to the bones, it is observed that the three osseous portions of the acetabulum are gradually formed from as many separate points, so that at the middle period of intrauterine life, the period chosen for this description, three several spaces exist in the bony margin of the acetabulum. The space between the ilium and ischium is the most considerable, and opens towards the ischiatic notch, or the dorsum of the ilium, and it is through this gate that the head of the femur is supposed to escape; a circumstance which must be facilitated moreover, by the position which the legs of the fetus usually occupy in utero, viz. flexed upon the abdomen, by the shallowness of the acetabulum, it being only about two lines in depth even including the ligamentous border with which the cavity is surmounted, and also by the action, either spasmodic or normal, our author believes it to be spasmodic, of the glutei and other muscles.

The Etiology constitutes the theme of the *Third* chapter. Mr. Carnochan suggests in the outset that the term "spontaneous" as applied to this form of luxation, is indefinite, inasmuch as the same term is used to designate a species of luxation which occurs at a later period in consequence of scrofulous disease of the hip: he prefers, therefore, the term "congenital."

The luxation may be complete or incomplete. It may be single, existing only on one side, or double. We are reminded also of a "pseudo-luxation," in which no actual displacement exists, but in which, nevertheless, some of the signs are present. Our author now deliberately approaches the discussion of the various hypotheses by which surgeons have sought to explain the occurrence of these peculiar congenital luxations. This he regards as the great object of investigation, which being fully resolved, all the questions of pathology, prognosis, tréatment, &c., will follow as easy and natural sequences. "*Felix qui potuit rerum cognoscere causas,*" he exclaims, and thence proceeds to discuss, seriatim, the five principal theories; all of which being at length, as he believes, satisfactorily disposed of, he presents a new, and as he trusts, a more rational hypothesis.

We must confess, however, that while we concede to our author in the disposal of this part of his subject much ingenuity, we have by no means been convinced that he has yet furnished us with a certain and invariable causation, nor that there is not much that is reasonable and probable in

some of the older doctrines. Let us take care that we do not reject an opinion simply because it has upon it the mould of age, nor adopt a new opinion for no better reason than that it is the latest.

The notion entertained by Hyppocrates, that these dislocations are the results of falls, blows and other injuries or accidents to the mother, while the child is in utero, is certainly "not probable;" but it is by no means certain that the violent contractions of the uterus while in labour, while the thighs of the infant are forcibly flexed upon its abdomen, may not produce a backward luxation. It is possible, therefore, that in some instances such uterine contractions, or possibly the hands of the accoucheur, making traction upon the groin of the infant, may constitute the sole cause.

The hypothesis of Dupuytren meets with still less favor from Dr. Carnochan. "Original defect in the organization of the germ," or "aberrations of the formative power," is an explanation "vague and inexplicit," says Dr. C., and it "has no support from physiological facts or the acknowledged doctrines regarding embryogeny." If we are not mistaken in what Dupuytren intended to teach, his hypothesis does not differ except in words from that put forward by M. Breschet, but to which Dr. C. has assigned a separate section. "Arrest of developement of the cotyloid cavity" is the more intelligible and significant explanation of M. Breschet, which doctrine is sustained by many parrallel facts in the history of developement. We see daily numerous defective congenital formations, such as supernumerary fingers or toes, hare-lip, fissure of the palate, &c., which will admit of no better explanation than "aberration of the formative power," or "arrest of developement."

Neither can we find in the pathological announcement of M. Cruveilhier, such evidence against M. Breschet as should have led our author to exclaim exultingly, "Now, it so happens, that the cotyloid cavity and the head of the femur have both been found progressing to their normal developement, or to have completed it, in cases of this dislocation which have come under observation." It is not claimed that such a condition of matters has been found in *all* cases, and it is therefore fair to infer that in these exceptional instances, (we are not certain that they are only *exceptional*.) the cause might have been found in "arrest of developement." But Breschet is entitled to a more complete argument from this pathological fact discovered by M. Cruveilhier, and now adduced by Carnochan. It is positively asserted that the bones of the hip joint have in these cases been found "*progressing* to their normal developement." A careful reading will show that it is intended to say that the bones have not yet attained

that growth and development which would correspond with the age of the child, in short, he speaks here of an "arrest of development."

"Certain articular maladies occurring in the fetus during intrauterine life," have produced, according to M. Parise, the malady in question. We shall dismiss this supposition with the simple statement of our belief that according to the evidence presented in the symptoms and autopsies, it must be one of the least frequent causes, if indeed it can ever properly be assigned.

Finally, M. Guerin teaches that it is attributable to a "primitive alteration in the nervous centers," while the author concludes that it depends upon a "perverted condition of the medulla spinalis," and upon this the main issue is taken.

The question between M. Guerin and Mr. Carnochan, as we understand it, is this. Guerin attributes the deformity in question to a *pathological* process in the *nervous centers*—the medulla spinalis or encephalon—in consequence of which portions of these structures are removed, and hence certain muscles dependent upon this *center* for their energy, fail to develop: while Carnochan regards the medulla spinalis either as suffering irritation simply, or "perverted action," or as the center of the excito-motor system *through* which *external* agencies reflect their injuries upon these muscles, and cause them to contract and expel the bone from its socket. The one also regards the muscular shortening as a mere arrest of growth; the other, as a contraction consequent upon irritation, or perverted action.

Now as to the first, it is much more reasonable to suppose with Carnochan, that the absence of portions of the encephalon, or medulla spinalis, whether in connection with deformities or not, is due to arrest of development, rather than to a pathological process. Nor does Guerin present one particle of evidence that in these cases any such process ever existed. It certainly cannot be found in the following passage.

"But if you consider that the specimens and the plates, now under view, show all the degrees of destruction of the nervous centres following in a decreasing series, which commences at the complete disappearance of the encephalon and the spinal marrow, and closes with a simple lesion of the membranes, you will easily understand by what chain of facts and inductions I have been enabled to establish rigorously, the reality of this destruction in those cases, where we no longer find the union of all the characters which appertain to it." p. 67; yet this passage contains the only proof of the position which is offered, as to the rest it is mere

assertion: and we could easily present cases from our own experience in disproof of the assertions.

Whether the second position of Guerin is correct, viz., that the muscles in question have not contracted, but were actually never developed, is altogether another issue, in which it seems to us that he has the right, or in which, to say the least, Guerin is as likely to be correct as Carnochan. M. Guerin, however, had made it a corollary from his first position, and Mr. Carnochan having rejected this, seems to think it necessary to reject also the inference. But listen to the conclusions of our author, p. 86. "What this particular source of irritation or pathologic action—the necessary antecedent of morbid muscular retraction—may be, it is not easy to determine strictly. Whether it amounts to inflammation; or to *ramollissement*; or to vascular congestion of the cineritious matter, or of the white structure in contiguity with it; or simply to what is called nervous irritation, caused by pressure or otherwise; or whether it depends upon an abnormal accumulation of nervous fluid, which by some has been supposed to be as nocuous in regard to the nervous substance, as the accumulation of blood in an inflamed tissue is to the several structures of the body; or whether from some poisonous condition of the blood—the nervous tissue bathed by this fluid as it circulates becoming morbidly impressed;—or whether an imperfect development of a portion of the medulla spinalis, or an atrophy of it, be essential in producing this disturbance of the excito-motor apparatus, are points which can only with great difficulty be decided in a rigorous manner." If, then, this "perverted action" may be described under all of these various morbid conditions, including "imperfect developement of a portion of the medulla spinalis, what reasons can be given why the muscles dependant upon this center may not also be imperfectly developed, and thus no occasion may exist to invoke active contraction or spasm as the cause?

In conclusion we cannot say that in the elucidation of the etiology of this disease, Mr. Carnochan has added much to what was hitherto known, or that he has done much towards clearing up and reconciling the obscurities of other theorists.

Here we shall leave the book for the present; the succeeding chapters, treating respectively of the Symptomatology, Diagnosis, Prognosis, Pathology and Treatment, are more free from speculation, and will well repay the student for the reading.

F. H. H.

Absence of mammary development, and the non-secretion of milk, in a child-bearing female.

EDITOR BUFFALO MEDICAL JOURNAL:—

Dear Sir: I send you an unusual case occurring in my practice nearly three years since. If you think it worth while you can publish it.

Mrs. A. N., confined in the summer of 1847, with her fifth child, has a chest as flat as a male's, with no evidence of the existence of any mammary gland. She has never given milk, and her children have been "brought up by hand;" three of them are now living. In her earlier confinements, efforts were made to procure the secretion, but failed, though I have no reason to doubt their being well directed. There has never been any swelling, pain, or other evidence of milk in the breasts. Her menses appear one month after confinement. She is robust, but not masculine in appearance.

Yours truly,

S. B. HUNT, M. D.

Prof. Coventry's Introductory Lecture.—We were about writing a notice of this excellent discourse, when the following editorial article in the Boston Medical & Surgical Journal met our eye. As it briefly expresses, in substance, what we designed to express, we take the liberty of appropriating it.

"*Medicine a Science and an Art.*"—A Lecture, introductory to the course in the Medical Institution of Geneva College. By C. B. Coventry, M. D., Professor of Obstetrics, &c. March, 1850. Published by the Class." Prof. Coventry's address is one which would be likely to chain the attention of an audience for the allotted hour, without their knowledge of its having fled. It is one of those spontaneous effusions of a good heart and a cultivated mind, which are well calculated to impress upon the student the most profound devotion to the science in which he is engaged. The profession of medicine is defined most clearly; and they that would have it, '*ars conjecturalis*,' should have heard Dr. C's argument to the contrary. The Dr. asks, 'Can a science which has its foundation in the immutable laws of nature be termed a conjectural art? Does the husbandman, when he sows his seed, merely guess it will grow? Our knowledge is obtained from the well-known, established and immutable laws of nature, as it regards the propagation, growth or development and nourishment of the animal economy; the recorded experience of others for ages, as to the causes and symptoms of disease, and from personal observation and experience of ourselves. Disease arises from the violation of the laws or conditions of health.' 'Scarce is the infant enabled to lisp the name of mother, when what is termed education commences. The brain, still imperfectly organized, is stimulated to unnatural exertion

by praise and bribery, and often by exciting some of the worst passions of our nature, rivalry and jealousy.' The innovation in medicine by empirics is illustrated in a most correct and happy manner; and were it not for the press of other matter, we would gladly lay the chapter before our readers."

Journal of Proceedings of the Michigan Medical Association for the years 1849 and 1850, vol. 1. Jackson, Michigan, 1850.

The Michigan Medical Association was first organized under a legislative act, in 1819, while Michigan was under a territorial government. Its legal existence was continued after the organization of a *State* government, until, in 1844, all laws relating to the medical profession were repealed. In 1846, when the statutes were revised, the Society was revived with its former powers and immunities. A satisfactory reorganization, however, was not effected until January, 1849, at which time a convention met at Jackson, the result of which was the establishment of the association on a firm basis. The committee on publication state, that "the only object of those engaged in this undertaking has been to contribute their share to the effort now being put forth throughout this country, to elevate our chosen profession, and establish an association of physicians whose united effort shall be to promote that *advancement* which is the spirit of the age."

The following are the officers of the Association:

President.—Dr. Samuel Denton, of Ann Arbor.

1st Vice President.—Dr. Geo. W. Gorham, of Jackson.

2d Vice President.—Dr. S. R. Arnold, of Monroe.

Secretaries.—Drs. DeLoskie and Miller, of Flint; and Geo. W. Fish, of Jackson.

Treasurer.—Dr. Abram Sager, of Ann Arbor.

Several Standing Committees were appointed to report at the next annual meeting, on practical medicine, surgery, obstetrics, and medical education. A special committee to report on the medical botany of the State was also appointed.

Appended to the proceedings are two addresses, one by the retiring President, Dr. Codman, of Lenawee, and the other by Dr. Field, of Adrian, formerly of N. Y., and now Demonstrator of Anatomy at the Geneva (New York) Medical College.

Both addresses are highly creditable productions. The work concludes with an extract of three pages from the sterling article on *Self-reformation of the Medical Profession*, by Prof. C. B. Coventry, in the March (1850) number of the Buffalo Medical Journal.

Prize Dissertations. By USHER PARSONS, M. D., late Prof. of Anatomy and Surgery, Brown University, and Surgeon U. S. Navy, etc., etc. Second Edition. Providence: printed by T. Albro, 1849. 8 vo. pp. 248.

This volume, published some months since, contains several dissertations which gained the Boylston prizes annually awarded to the successful competitors for the best essays on a stated subject. The author's uniform and successive triumphs in this way, had become so much a matter of course, that, some years ago, when the subjects were announced for the coming year, the Boston Medical and Surgical Journal stated, as an inducement for medical writers to enter the list, that Dr. Parsons had engaged not to engage in the contest.

The Boylston prize fund was instituted in 1803, by Ward Nicholas Boylston, a descendant of Zabdiel Boylston, F. R. S., who introduced into America the practice of inoculation for small pox in 1721. The fund provides a perpetual income of \$100, which is divided between the authors of the best dissertations on two different subjects given out two years before, the successful candidates receiving either the money, or a gold medal of the same value, at their option. The management of the fund, and the adjudication of prizes, are entrusted to a committee. This committee consists of not less than seven, nor more than eleven members, who receive their appointment from the President and fellows of Harvard University.

Competition for the prizes is invited from members of the profession throughout the United States, not being confined, as is the course pursued in some States, to the medical men within the State. These facts we gather from the preface of Dr. Parsons' work,

The subjects of the dissertations which the volume contains, are:

1. On inflammation of the Periosteum, both acute and chronic. Premium awarded in 1827.
2. On the disease called an irritable state of the urinary bladder; its causes and treatment. Premium awarded in 1828.
3. On the connection between cutaneous diseases which are not contagious, and the internal organs. Premium awarded in 1830.
4. On the diagnostic marks of cancer of the breast; and whether it be curable. Premium awarded in 1835.
5. On the comparative influence of Malaria as a cause of Fever. Written 1830.

The subjects of these dissertations are interesting, and practically important. The author, as it is gratuitous to remark, is a gentleman of large

experience, particularly in the department of surgery, deeply versed in the literature of the science, a close thinker, and a judicious practitioner. At the meeting of the American Medical Association in May, 1849, he was appointed chairman of the Committee on Medical Science, and his report in the forth coming volume of transactions, will, doubtless, furnish abundant evidence of the propriety of the selection. In reproducing these dissertations in a form which will ensure their perpetuity, Dr. Parsons has discharged a duty to himself, while he has conferred a benefit on medical literature.

Annual Report of the Physicians of the Marine Hospital, Quarantine, Staten Island, N. Y. Made to the Legislature March 26, 1850. By F. CAMPBELL STEWART, M. D., Physician to the Hospital.

From this report we select as follows:

The greatest number of patients under treatment, at any one time since the first of July, and subsequent to the closing of the public stores, was less than three hundred and fifty; and the smallest number two hundred and twenty-eight.

The number of patients remaining in the several hospitals and public stores on the 23d of April, as ascertained by actual count, was 695.
Number admitted from 23d April to 31st of December, 2,520

Whole number under treatment during the period indicated, - - - - -	3,215
Of these, there were discharged cured or relieved, - - - - -	2,369
Died, including 175 from cholera, - - - - -	503
Remaining in hospital Dec. 31st., - - - - -	343
	3,215

The per centage of deaths, including those from cholera, was about fifteen and a half per cent, and per centage, excluding the cholera cases and deaths, but including ship fever, small pox and all other diseases, was eleven and one-third per cent.

Of the whole number admitted into the institution, between the dates specified,

There were received from on board of vessels directly, - - - - -	1,010
The city and Ward's Island, (under authority from the board of health and Commissioners of Emigration, and for the most part, newly arrived emigrants,) - - - - -	1,478
Cases of emergency, cholera, small pox, &c, from Staten Island, chiefly emigrants, and sailors from the Seaman's Retreat, - - - - -	32
	2,520

Excluding the numerous cases of extreme debility and exhaustion, and also such diseases as presented typhoid symptoms as an accompaniment only, there were received, from the 23d April to 31st December, 813 cases of genuine typhus fever, of which 112 died, giving a mortality of less

than 14 per cent.—a result which should be entirely satisfactory to those who are familiar with the nature of this formidable disease.

Previous to the appearance of the cholera in New York city, many cases of that disease had occurred at the quarantine. Indeed, the Marine Hospital had never been entirely free from it since its first introduction by the packet ship "New York," in December, 1848.

The whole number of cases received into the institution, subsequent to the 23d of April, was 303, of which 175, or about 57½ per cent., proved fatal. This is apparently a large mortality, but, in reality, not greater than the average in other public institutions, or in private practice; for it must be borne in mind that nearly all the cases were sent on shore from vessels arriving from sea, and the patients were often landed in a dying condition, and in so far advanced a stage of the disease, as to preclude the possibility of their deriving benefit from medical treatment. Among the deaths, too, are included some who were inmates of the institution prior to the 23d of April, while the admissions, with this disease, only included such as were received subsequent to that date.

After the cholera had ceased to prevail as an epidemic in the city of New York, numerous cases continued to be received into the Marine Hospital from vessels arriving from sea. The board of health discontinued their daily reports on the 5th of September, between which time and the 20th of November, 178 cases of cholera and cholerae were admitted from on board the following vessels, which had lost, in the aggregate, at sea, 348 of their passengers, most of whom are presumed to have died of the same disease.

The number of cases of small pox and varioloid received into the Marine hospital, from April 23d to December 31st, 1849, was 256; of which 22 only died, giving a mortality of less than nine per cent. for one of the most formidable and fatal diseases that medical men have to encounter. Several of the sick were affected with the most violent form of the confluent variety of small pox, and it is worthy of being noted, that a very large proportion of cases occurred among German emigrants, who are among the most cleanly and healthy of those who come to our shores, and who are subjected at home to strict sanitary regulations in regard to vaccination. A number of sailors are also included in the sum total of patients admitted with small pox and varioloid, and this class of citizens appears to be peculiarly liable to contract the disease.

Dietetical and Medical Hydrology. A Treatise on Baths; including cold, sea, warm, hot, vapor, gas, and mud Baths; also, on the watery regimen, Hydropathy, and Pulmonary Inhalation; with a description of Bathing in ancient and modern times. By JOHN BELL, M. D., etc., etc., etc. Philadelphia: Barrington & Haswell, 1850. 12 mo., pp. 658.

This volume may be appropriately styled a cyclopædia of therapeutical and hygienic Hydrology. The learned author treats of water under all the various aspects in which it becomes a prophylactic and remedial agent, and has collected what relates to the subject in medical literature from Hippocrates to the present day. The work appears opportunely, now that

the employment of water in medical practice is receiving more consideration than it has done for some years past. We do not doubt that, as an important element in the prevention and cure of disease, it has fallen into undeserved neglect. That it has been pressed into the service of quackery, under the denomination of *Hydropathy*, tends to create a prejudice in the minds of many. But it is a remedy legitimately belonging to medicine, and to appreciate its importance, and become acquainted with the various modes and conditions in which it is applicable, the medical inquirer is under no necessity to go without the pale of the literature of the Profession. Dr. Bell's treatise affords ample evidence of the correctness of this remark.

Although we may not concur fully with the learned author in all his views, the work is, we think, calculated to render valuable service, by directing the attention of the Profession to the subject, and thereby aiding to protect from the extravagances of a popular delusion, an agent sufficiently potent for much evil in the hands of the ignorant, and one which, directed with discrimination and judgment, may be made more available for useful ends than it now is in medical practice. The work is for sale at the bookstore of G. Derby & Co.

Southern Medical Reports; consisting of General and Special Reports, on the Medical Topography, Meteorology, and Prevalent Diseases, in the following States: Louisiana, Alabama, Mississippi, North Carolina, South Carolina, Georgia, Florida, Arkansas, Tennessee, Texas. To be published annually. Edited by E. D. FENNER, M. D., of New Orleans, Member of the Am. Med. Association, etc., etc., etc. Volume I, 1849. New Orleans: B. M. Norman, 16 Camp Street. New York: Samuel S. & Wm. Wood, 261 Pearl St. 1850. 8 vo. pp. 472.

This volume is submitted in accordance with the plan announced in a *Prospectus* issued about a year ago, which was copied into this Journal. It is the first of a series of publications, to appear annually, which are to be devoted to reports by different individuals, on the medical topography, meteorology, prevalent diseases, etc., of the *Southern States*. In order that our readers may better appreciate the scope of the work, we quote the following from the Editor's introductory address, in which the objects of the enterprise are lucidly set forth:—

That the immense region of country which
our observations, differs sufficiently in
from the region lying north of it
is distinguished from the North-

ern; and it is also true that the various sections of this great Southern region differ very materially from each other in respect to climate, locality and geological formation. In like manner, a general distinction may be drawn between the prevalent diseases of the North and the South, as well as in the different sections of the South. Thus we may not only mark a difference in the general features of diseases in the various regions and sections, but they really call for a corresponding modification of treatment. Whoever expects to see the same remedies, administered in like doses, and in apparently similar conditions of the system, produce equally beneficial effects in these various regions and sections, will find himself egregiously mistaken. He must learn how to adapt his remedies and their doses to the peculiarities stamped upon disease by climate, locality, habit and mode of living, before he can ever become a successful practitioner.

In view of these considerations and many others now omitted, we shall endeavor to establish a *Magazine*, in which shall be recorded the practical observations and experience of all Southern Physicians whose energies are expanded beyond the bounds of self interest, and whose philanthropy would prompt them to contribute what they can to the relief of suffering humanity.

In our earnest endeavors to arouse the ambition and energies of Southern Physicians, far be from us the desire to see developed any *sectional feeling* beyond the strict proprieties of a laudable competition. We have long been indebted to our brethren of the Northern States and of Europe for their profound researches and indefatigable labors; whilst they have long needed correct and faithful accounts of the ravages of Southern diseases upon the human constitution; and the best method of preventing them. They have been our preceptors in the elements and the principles of medicine; but we have obtained our knowledge of diseases from *personal observation*. Let us henceforth endeavor to reciprocate, with good feeling and proper courtesy, the obligations annually conferred by our brethren of the North and of Europe. We have a richer and more varied field for observation than they have, and, with equal industry, would be able to contribute more to the archives of medicine.

In commenting on the *Prospectus*, in a former No. of this Journal, we expressed our belief that the undertaking was worthy of all praise, and that the well known ability of Dr. Fenner as an observer, thinker, and writer, together with his experience as a medical editor, afforded an amply sufficient guaranty that the duty he proposed to himself would be faithfully and satisfactorily performed. Our pre-convictions are verified by an examination of the volume which has appeared. It contains little or nothing that is valueless, and much that must possess great practical interest for the medical profession of the South. For the latter it is specially designed, but Northern readers, who should desire to know something of the peculiarities of Southern diseases, and their relations to distinctive features of the South involved in Etiology, will do well to patronise Dr. Fenner's work. As another consideration, which we trust many of our readers will duly

appreciate, we may suggest that an effort of this kind to develop and diffuse medical knowledge, and to arouse increased exertion by professional brethren in behalf of science and humanity, should not be regarded with indifference, and treated with neglect, even by those who do not directly participate in the benefits resulting therefrom. To issue a volume of this kind must require a considerable pecuniary outlay, and while we know that Dr. Fenner would not thank us for intimating a wish that his plan might prove a profitable speculation, we may be permitted to express a hope that, for the honor of the profession, the circulation of the work shall be sufficient to reimburse the cost of publication. In view of the consideration just suggested, the Southern Medical Reports has claims upon those of our Northern brethren who are desirous that useful laborers in the fields (not vineyards) of medical science, if they do not receive a reward, shall, at least, not be exposed to injustice; and who are ready to welcome and encourage every truly valuable addition to our native medical literature.

The present volume contains about forty articles, most of which were contributed, originally, for the work, and a portion selected from medical Journals. Some of the most valuable of the articles are from the pen of the Editor.

The price of the work is \$5 per annum. Derby & Co., booksellers, of this city, will forward orders.

British American Medical and Physical Journal, Montreal, C. W. This esteemed contemporary has recently assumed a new and greatly improved typographical appearance. It continues to be edited by Archibald Hall, M. D. Messrs. S. S. & W. Wood, New York, are agents for the United States.

Schiffelin & Co's Extracts.—We have examined some of the extracts prepared by Schiffelin & Co., of New York, and we do not hesitate to pronounce them, in our judgment, superior to any that have, as yet, fallen under our notice. So large a proportion of the extracts heretofore sold by Druggists and Apothecaries has been found to be worthless, that they have of late fallen into comparative disuse. The introduction of better articles is certainly a great desideratum.

BUFFALO MEDICAL JOURNAL

AND

MONTHLY REVIEW.

VOL. 6.

AUGUST, 1850.

NO. 3.

ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* By AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

Having preserved histories of a considerable number of cases of Continued Fever, I propose to analyze them, and report the results in a series of articles for this Journal.

The first step in the analytical investigation, is to separate and classify all the important particulars embraced in the histories of the cases that have been collected. This has been already done. All the important circumstances occurring in the progress of every case (so far as recorded) have been selected and distributed in appropriate divisions. The labor which even this has required, can be best appreciated by those who are practically conversant with such tasks. In order to collocate the facts in the classes to which they belong, it was necessary that the examination of the history of each case should be repeated from twenty to thirty times. Multiplying the whole number of cases by the number of perusals of each case, will afford some idea of the tediousness of the process.

This is but a preliminary stage of the analysis. To develop useful results, the details thus brought into juxtaposition must be farther investigated. How shall this be done? in other words, how are these classified facts to be interrogated? Some may possess interest and value in them—

selves—isolated from the others—but, for the most part, it is by *enumeration* and *comparison* that the fruits of the analysis are to be acquired. It will be an object to determine, whenever practicable, the numerical proportions in which the different traits, symptoms, etc., belonging to the general history of the disease, respectively occur. That is to say, with regard to each of these particulars, in how many of the whole number of cases analyzed is it found to be present. This result is obtained by the simple process of counting. In this way is ascertained the numerical ratio which each bears to the whole number of cases, and, by comparison with each other, the mutual relations of all of them in this respect. By such enumerations, sufficiently often repeated, are developed the laws of a disease in so far as concerns the relative frequency of the production of the various particulars which compose its natural history. And the knowledge of these laws has other advantages than merely the gratification of a taste for statistics. If certain events, or symptoms, be proved to be to be constantly associated with a disease, and the same events, or symptoms, be proved not to be present, with any degree of constancy, in other diseases, it is a logical inference, that some very intimate connection subsists between the events, or symptoms, and the disease. This is an important result of numerical analysis, notwithstanding we are not able to penetrate farther by this method of investigation, and explain the nature of the bonds which bind these events, or symptoms, to the disease. The knowledge of the existence of such connections is certainly important as a point of departure for other investigations instituted with a hope of penetrating deeper into the mysteries of pathology. In like manner, events which are proved by enumeration to be not constant, but more or less frequent in their occurrence, may be considered as proving contingent relations to the disease in proportion to their frequency, or they may be proved to be purely accidental; and thus may be determined, not only what particulars intrinsically belong to the disease, but the relative degrees of relationship which other particulars sustain to it, and that certain events are to be excluded as having no real connection with the disease whatever. This kind of knowledge, moreover, has an important bearing on diagnosis, for, in proportion as an event or symptom is constant, or more or less frequent, is its diagnostic value to be estimated, provided, always, that it does not occur, or is far less frequent in its occurrence, in other diseases. Numerical analysis, therefore, tends to establish the relative importance of the several symptoms of disease as diagnostic criteria of the presence of that disease.

To follow out the trains of inquiry to which the foregoing considerations lead, would be to engage in discussions respecting the resources and limitations of the "*numerical system*," foreign to my present purpose.

Another mode of interrogating the facts contained in a collection of recorded cases of disease, is to compare the different cases with each other as respects associated symptoms. We find, for example, in a greater or less number of cases, a certain symptom; now, in the cases which present this symptom, do we always find certain other symptoms associated, and in the cases not presenting the symptom, are the associated symptoms absent? If analytical examination establishes any such relations, they involve, as a logical inference, some dependent connection between the symptoms thus associated in different cases, although the problem still remains to be solved, which of the symptoms are causes, and which effects, or whether all may not be alike effects of an unobserved, or inappreciable cause.

Again, the cases may be divided into several groups, and comparisons instituted between the groups, in order to ascertain what phenomena, or facts, belong to one or more groups, which do not belong to the others. We may thus be led to establish relations between the distinctive features upon which the division into groups was based, and other circumstances less obvious to observation, which the analysis and comparison reveal. Thus, we may separate fatal cases from those not fatal, and if we should chance to find in all the fatal cases certain characters which are not present in those cases which are not fatal, it would be a fair presumption, to say the least, that some connection existed between these characters and the fatal issue; at all events, knowledge of these characters would have an important practical bearing on the prognosis.

These few remarks are offered, by way of introduction, simply to suggest some of the leading objects to be kept in view in an analytical study of the data embraced in the records of clinical observations, foregoing discussions respecting the nature and scope of the logical principles involved in the application of what is known as the *numerical system* to investigations of disease. The latter is a highly interesting and important subject, but it is altogether too profound and extensive to be treated in an incidental connection. The value of this method of investigating medical truth is doubtless as much exaggerated by some, as it is underrated by many. In general terms, its advantages may be said to consist, not only in the development of a much greater number of results, but in rendering medical observation and experience vastly more precise and accurate. But it is

hardly less desirable to understand its limitations, than to appreciate its advantages; and, in general terms, it furnishes data for delineating with completeness and correctness the *natural history* of diseases, it discloses relations between the events, or symptoms, of disease, it leads to a knowledge of laws regulating the succession of morbid phenomena; but it fails to penetrate the hidden springs upon which the observable characters of disease are dependent, it cannot unfold the links connecting those phenomena which are found to sustain relations with each other, and it leaves unexplained the laws the existence of which it declares.

To be satisfied of the very great utility and importance of recording cases, and subjecting them to analytical examination, it is only necessary to compare the general impressions which are formed while cases are under observation, with the results obtained, subsequently, by enumeration and comparison. These results are frequently as unexpected to the analyst, as to others: they often conflict directly with the anticipations derived from observing and recording the cases while in progress. This fact points to the great source of error in the medical experience of those whose inductions are based on facts retained in the memory alone. It is exceedingly difficult, as the phenomena of disease pass under observation, for the mind to resist becoming committed to generalizations which are often immature and erroneous; and when the mind is thus committed, it is equally difficult to continue observations fairly and impartially. Every person who studies the operations of mind in others, or who candidly reviews his own intellectual experience, must be convinced of the truth of this remark. How common is it to hear assertions made in the most positive manner, that in all cases of a particular disease, certain things are so and so—assertions made to sustain some peculiar views of pathology, diagnosis, or therapeutics; when a few observations by one who is unbiassed, or skeptical as to the correctness of the assertions, suffice for their disapproval. It does by no means follow that such rash declarations denote either dishonesty or stupidity; but they show that the mind, without vigilant watching, imposes upon itself. The prime error is in arriving at conclusions too hastily, without sufficient data, or proper investigation. So soon as this false step is taken, the mind is constantly seeking for evidence to sustain its position. Facts incompatible with such a position are not only overlooked, or excluded from the memory, but, sometimes, by an obliquity, and almost perverseness of judgment, even appear to sustain the false views which are pertinaciously cherished. This, it will be admitted, is a highly colored representation, but it is not a fancy sketch; exemplars are not so rare as not to be

occasionally met with; of a lesser degree of the self-deception described, illustrations are abundant, and there are few so fortunate as to escape altogether the influences, alike imperceptible and untoward, of erroneous pre-conceptions, upon the powers both of observation and ratiocination. Hence originate the "false facts" with which medicine abounds, and that *experience* which is worse than valueless. The practice of recording observations, and submitting them to careful analysis, is of inestimable value, not only in the greater number, and better character of the results developed, but in cultivating a habit of mind which precludes error by withholding judgments, and accepting only as truth results elicited by adequate, trustworthy investigations. The whole philosophy of this subject is admirably embodied in the sentiment of Rousseau, selected by Louis as the motto for his work on Fever:—" *Je sais que la verite est dans les choses, et non dans mon esprit que les juge, et que moins je mets du mien dans les jugements que j'en porte, plus je suis sur d'approcher de la verite.*"

After the incomparable work of Louis giving the results of his researches on Continued Fever compared with other acute diseases, and subsequent reports by several distinguished observers, it may perhaps appear a gratuitous, if not a presumptuous undertaking to make choice of cases of this affection, as a subject for analytical study. The elucidation, however, which the disease has already received, renders it more interesting, and scarcely less important, to prosecute continued researches. Were the results to be the same as those previously obtained, the labor would by no means be uselessly bestowed, since to confirm truth, is often very desirable, although a humbler attainment than the discovery of it. But other advantages are secured by a series of analyses of cases of the same affection occurring at different periods and places, and under a variety of circumstances. If the results are not uniform, the variations are to be attributed to the disturbing or modifying influences incident to time, situation, and other circumstances; and we may thus learn what belongs to the disease *per se*, and what is due to extrinsic causes. It is at once obvious that this kind of knowledge is highly desirable. For a similar reason it were probably better that a number of cases occurring during a succession of years should be subdivided—those occurring within the space of one or two successive years being grouped together, and each group analyzed separately. In view of this consideration, I have deemed it best to analyze the cases I have already collected, without waiting longer to increase the number.

Of the fifty-two cases which have furnished the data for the examination upon which I will now enter, *fourteen* occurred in private practice, and the remainder, *thirty-eight*, were observed at the *Buffalo Hospital of the Sisters of Charity*. I shall consider these as forming two distinct groups. The cases from private practice are recorded less fully than those at the Hospital. They run through a longer period of time, the first case being observed in December, 1845, and the last in June, 1850—a space of five years. The hospital cases were all observed between August, 1848, and March 1850—a space of eighteen months. The circumstances surrounding the patients in private practice, would, of course, be expected to differ in different cases. On the other hand, at the hospital, the patients were similarly situated. Inasmuch as the hygienic condition incident to hospitals, as well as private families, tend to modify, more or less, the symptoms and progress of a disease, frequently preventing a fair exemplification of its natural history, it should be premised that the *Hospital of the Sisters of Charity* presents in all respects a most favorable combination of circumstances for the relief of the sick. The wards, during the time these cases occurred, were not crowded, and were extremely well ventilated. The attendance and nursing could hardly be improved, and was much better suited to the welfare of patients, than in the private houses even of those who can command all the comforts of life. The personal attentions of family and friends, in severe illness, are often unskillful and injudicious, and money cannot purchase the discreet and faithful offices of a *Sister of Charity!*

The records of the cases in private practice were not made at the time of the daily visits; sometimes no record was made for several days, and, in a few instances, the details were noted from memory as the disease was about to terminate.

The histories of the cases in the hospital consists of daily records, (with occasional exceptions of a day or two,) made by myself at the bedside, at the diurnal visit, which was usually in the morning. I can therefore vouch that they are as accurate as I had the ability to make them. I must confess, however, that while arranging the facts for examination, I had occasion to regret that the records had not contained some particulars of which frequently no mention is made, either positively or negatively. In this respect the histories are far less complete than I could wish them to be. This, however, does not affect the accuracy of the analysis in so far as the facts are recorded.

The phrase *Continued Fever* is used, generally, in a generic sense, em

bracing two or more species of febrile disease. The distinctions between what have been termed *typhus*, and *typhoid* fevers, have, of late years, occasioned much discussion by medical writers, and practitioners are not agreed whether to regard the different types thus distinguished, as really and essentially distinct forms of fever, or not, some contending that they are different febrile affections, others that they are essentially identical. The views which the writer has hitherto entertained on this point are, that cases of continued fever admit of being nosologically discriminated after this division, but that there do not exist sufficient grounds for regarding the two forms of disease as radically and essentially distinct; in other words, that continued fever is one disease, and that the divisions which it may be convenient to make, are based on differences which are nosological, rather than pathological, the uniformity of which, even for nosological purposes, not being perfectly reliable. The distinctions between Typhus and Typhoid fever, and the subject of diagnosis, will come up more appropriately hereafter. The facts contained in the cases under examination will be studied with reference to the points just mentioned, and, for this end, it becomes necessary to discriminate beforehand, between those cases which have claims to be considered as cases of *Typhus*, and those which are properly cases of *Typhoid* fever. Applying the rules of discrimination as laid down by those writers who maintain that the two forms of disease are essentially distinct, and rejecting as indeterminate all cases in which there exists any room for doubt as to the type, the result is as follows: of the *fourteen* cases in private practice *twelve* are distinctly cases of *Typhoid* fever; one was a case of *Typhus*, and one is *doubtful* from absence of sufficient data in the record. Of the *thirty-eight* hospital cases, *eighteen* were cases of *Typhoid*, and *twelve* were cases of *Typhus* fever; *eight* admitted of some doubt.

Another basis for a division of cases in order to institute comparison of the facts belonging in each, is the issue of the disease. Of the *fourteen* cases in private practice *four* were fatal. Of the *thirty-eight* in hospital practice *thirty-one* recovered.

Of the fatal cases in *private* practice, in *two* the disease was of the *Typhoid* type, in *one* the type was considered doubtful, and the only case of *Typhus* in this group proved fatal.

Of the fatal cases in hospital, *two* were of the *Typhus* type, *four* were of the *Typhoid* type, and in *one* the case was doubtful.

The ratio of mortality between the hospital cases, and those in private

practice will be observed ; the former being about $13\frac{1}{2}$, and the latter $22\frac{1}{2}$ per cent., showing, in so far as a comparison of these two groups of cases go, a superiority in the hospital management of the disease.

Something should, perhaps, be said concerning the correctness of the diagnosis in all the cases set down as cases of continued fever. In places where both remittent and continued fevers prevail, practitioners sometimes confound the two, and unless the reader can have faith to believe that this, or any other error of diagnosis has been avoided in the present collection of cases, he can, of course, attach little or no importance to the results of the analysis. It is obviously difficult for the writer to furnish evidence of his competency to discriminate, practically, the disease under consideration, from other affections. The reader must estimate, as he best may, the grounds for reliability on this score. This assurance, however, may be given, that the utmost care has been taken to admit into the collection those cases only which, in the writer's opinion, were undoubtedly cases of continued fever, all recorded cases which seemed to him in the least degree doubtful, having been rejected.

In proceeding now to present the results which may be developed by the study of the cases under consideration, together with such considerations as, in connection therewith may be suggested, it will be most convenient to follow the order pursued in the analytical process already performed, adopting as many different sections as there are classes in which the events, symptoms, etc. of all the cases have been distributed.

SECTION FIRST.

Age, Sex, Occupation, Civil Condition, Nativity, Habits, Season, Constitution and previous health of the Patient, Period of residence in this Country and in this City, Duration of the disease before coming under observation.

Age.—The ages of the private patients are given in all but two cases, i. e., in *twelve* cases. Of these *ten* are cases of *Typhoid* fever. The maximum age of these cases is *thirty-four*, the minimum age, *seventeen*. The mean age is $23\frac{7}{10}$. Of these *ten* cases, *two* proved fatal. The ages in these *two* cases were 21 and 34. The only case of *Typhus* in this group of private cases proved fatal, the age being 25. The only case indeterminate as to its being *typhoid* or *typhus*, proved fatal, the age being 40.

In the *hospital* cases, the ages are given in all but three cases. Of *seventeen* cases of the *typhoid* type in which the ages are given, in *one* case the age was 45; in *one* case the age was 35; in *one* case, 24. In the case of the youngest patient the age was 13. In the other cases, 13

in number, the patients were between 17 and 24 years of age. In six cases the age was precisely 20. The mean age is 22 2-17.

Of *ten* cases of *typhus* in which the ages are given, in one case the age was 50. In *four* cases the age was 20, which was the minimum. The mean age is 26½.

Analysis of large collections of cases of *typhoid fever* by Louis, Chomel, Jackson, and others, have established the existence of certain laws as respects age in this disease. The vast majority of cases occur between the ages of 15 and 30. Cases occurring after 40 are extremely rare, although they are occasionally met with. *Typhus fever*, on the other hand, evinces far less regard for age.

The results given above, correspond very closely with those obtained from observations by the authors just named, on a larger scale. In the collection of hospital cases analyzed by Dr. Jackson, of Boston, embracing *two hundred and ninety-one* cases, the average age was found to be about 22½ years, which, it will be perceived, is the same, with a slight fractional difference, as in the analysis of the hospital cases just made.

The cases of *Typhus* in our collection exhibit a higher maximum, and a greater mean. The latter, however, is less than it would seem to have been in the observations of others. Prof. Jenner, of the University College, London, in a late report on this subject, states that of forty-three fatal cases of *typhus*, nearly *one-third* were more than *fifty* years of age. —[*Amer. Journal of Med. Sciences, No. for July, 1850.*]

It remains to compare the fatal cases with those in which recovery took place, with respect to age. The observations of Louis go to prove that the average age of those who recover is less than of those who die with *typhoid fever*.

Of *sixteen* cases of *Typhoid* in which the ages are given, 4 proved fatal, and 12 recovered. Among the fatal cases was the case presenting the maximum of age (45), and also the case presenting the minimum age (13).

The mean age in the cases that proved fatal, is 24½. The mean age in the cases in which recovery took place is 21 8-12.

Of *eleven* cases of *Typhus*, in which the ages are given, *two* only proved fatal. Among the fatal cases was the case of the maximum age, 50, and the mean age, is 35. The mean age in the cases that recovered is 25 5-9.

The above comparison refers to the hospital cases.

In the group of *private* patients, of 8 cases of *Typhoid*, in which recovery took place, the mean age is 22 6-8. Of two fatal cases, the age in

one was 21, and in the other, 34, giving a mean of $27\frac{1}{2}$. In the only fatal case of typhus the age was 25.

In so far as our cases afford results, then, they sustain the deduction of Louis, that the disease is less likely to prove fatal in proportion to the youth of patients.

Sex.—In the cases of Fever analyzed by Louis, and Jackson, by far the greater number were of the male sex. Some instances, however, are mentioned by Dr. Bartlett, in his work on Fever, of collections of a large number of cases in which the number of females preponderated.

In the cases which I have preserved, the number of males very much exceeds that of females.

Of the *fourteen private* patients, all but two were males. Of the *hospital* patients, the number of males affected distinctly with *typhoid* fever, was 17; of females, 1. The number of males affected with *typhus* fever was 10; of females 2. Of the cases in which the diagnosis was indeterminate, five were males and three females.

The disparity in the hospital cases might perhaps be accounted for by the fact that a larger number of males resort to such institutions than females. This explanation, however, will not apply to cases in private practice: and, hence, in so far as from the small number of this group of cases, any inductions are admissible, the inference is, that females are much less likely to be attacked with continued fever than males.

In the cases analyzed by Dr. Jackson, of Boston, the average age of females affected with fever was found to be somewhat higher than that of males. The number of female patients in my collection is too small for comparison on this point.

Occupation.—With reference to the *occupations* of the patients, no relations of the disease to any particular kinds of employment are developed. Of the patients treated in hospital, *three* were mariners, *six* were mechanics of different trades, *fifteen* were laborers, *one* was a waiter, *one* a clerk, *one* a porter, *one* a foreman in a steam mill, *one* a domestic, (female), *one* a chamber-maid, *one* a steamboat cook, and *two* were Sisters of Charity. Of the cases in private practice, *four* were soldiers recently enlisted, *two* were students, *one* a physician, *one* a maker of patterns for castings, *one* a laborer, *one* a housewife, *one* a female without occupation, *one* a male without any particular employment.

Analysis of much larger collections of cases have failed to show the existence of any tendency in particular vocations to favor the development of the disease. That the hospital patients were all in what are generally

considered the humbler walks of life, will not be deemed singular, when it is considered that it is for the relief of this class that the institution is chiefly designed.

Civil Condition.—Of the *fourteen* patients in *private* practice, *one* only was married, a female 34 years of age.

Of the *thirty-eight hospital* patients, *one* only was married, *one* was a *widow* and one a *widower*. This, however, which might at first seem a striking result, proves little or nothing respecting any causative influence derived from the unmarried state, when it is considered that the average age of the patients is between twenty-two and twenty-six years, and also that, in so far as the hospital cases are concerned, comparatively few married persons, with any disease, enter the Institution.

Nativity.—Of the patients in *private* practice, *eight* were Americans, *three* were Germans, *one* was an Irishman, *one* an Englishman, and in one case the nativity is not stated.

Of the *Hospital* cases, the nativity of the patients with *typhoid*, is as follows: Americans, one; Irish, seven; Germans, ten. Of the patients with typhus as follows: Americans, one; Irish, eleven; Germans, none. Of the doubtful cases as regards the type of the fever, as follows: Americans, two; Irish, five.

I had doubted whether it were worth while to take the trouble to ascertain the nativities, but the result, in one point of view, is certainly curious. Of twelve patients affected with typhus, all but one were Irishmen. The reader is doubtless aware that Dr. Lombard, of Geneva, Switzerland, and others, have contended that true typhus is of Hibernian origin, and that its appearance in other countries is owing to its importation from Ireland. The result of the present analysis is not given as sustaining this idea, for the recency of emigration is obviously involved, and with respect to this point, the facts have not yet been examined; but the singularity of the coincidence, if it be nothing more, is worthy of note.

Habits.—In the *hospital* cases, the records, with very few exceptions, are defective in information respecting the *habits* of the patients.

In *eight* of the cases in private practice, the habits were good. In four the habits were unknown. In one the attack had been preceded by dissipation, and in one the patient was not intemperate, but irregular and licentious.

Season.—Observations have appeared to show that while the *typhus* type of continued fever is unaffected by season, i. e., liable to prevail equally at any portion of the year, the *typhoid* type, on the other hand,

manifests a predilection for the autumnal months, although it is by no means restricted in its occurrence to the latter. In New England, where typhoid fever is the predominant febrile disease, it is commonly termed the *autumnal* or *fall* fever. The dates of the cases under analysis, were as follows:

Private Cases—Typhoid.—1845, December, *one* case; 1846, January, *one* case, October, *two* cases; 1847, September *two* cases, October, *one* case; 1848, August, *two* cases, September, *one* case; 1849, September, *one* case; 1850, June, *one* case. *Typhus*—1847, November, *one* case. Thus *eight* out of twelve cases of typhoid fever occurred during the months of *September, October, November* and *December*.

Hospital Cases—Typhus.—1848, August, *one* case, November, *one* case; 1849, March, *two* cases, June, *three* cases, July, *one* case, August, *one* case, October, *one* case; 1850, January *one* case, February *one* case, making 12. *Typhoid*—1848, October, *one* case, November, *one* case, December, *one* case; 1849, March, *one* case, May, *one* case, June, *one* case, July, *one* case, August, *two* cases, September, *five* cases, October, *one* case, November, *two* cases, December, *one* case, making 18.

Thus, *nine* out of *eighteen* cases of *typhoid* occurred during the months of *September, October, November,* and *December, five* occurring in the month of *September*; while of the twelve cases of *typhus*, only two occurred during these months, one in *October* and one in *November*.

After the month of *December, 1849*, up to the time when my period of service at the hospital ceased, which was *April 1st, 1850*, not a single case of typhoid fever was received. During this period, *two* cases of typhus are recorded, and two cases in which the type is left indeterminate.

The result of the analysis, therefore, goes to sustain the correctness of the observations, above mentioned, respecting the relations of the two types of continued fever to the seasons.

Constitution and health at the time of attack.—In every one of the cases in *private practice*, the *constitution* of the patient might be pronounced good, i. e., none were suffering under chronic disease, or general ill-health. The same is believed to be true of the *hospital cases*. In many cases it is so stated in the records, but in the cases in which this statement does not appear, it is a fair inference that, were it otherwise, the fact would be recorded. In no case is it recorded that the patient possessed a feeble or broken constitution. In so far as my observations go, then, the conclusion is, that persons not possessing good constitutions, are not apt to be affected with continued fever.

The state of health at the time of attack is known in all the cases occurring in *private practice*. In all but two cases it was good. In one of the excepted cases the patient had a short time before had *Pertussis*, from which, however, he had recovered; in the other case, the patient was reduced by lactation, and was laboring under *stomatitis materna*. In the *hospital cases*, the health is stated to have been good in seven cases of *typhoid*, and four cases of *typhus*. It is not stated to have been poor in any case, and in nearly all of the cases in which no mention is made of this point, it may fairly be inferred that the health was good, from the patients having been arrested by the disease while pursuing their customary avocations, all of which would require for their performance good health, and from other circumstances of the history. Hence, it seems proper to conclude that, in so far as the present collection of observations affects this point, persons in good health are most apt to be attacked with continued fever.

By the *state of health at the time of attack* is meant, here, the condition prior to the first development of the *prodromic* symptoms of the disease. The latter, which by some are considered to constitute a stage of the disease, and by others regarded as preliminary to it, will form a subject of inquiry under another division.

Period of residence in this country, and in this city.—The period of residence in the country, and town, where cases of continued fever are observed, has important bearing upon several questions relating to the etiology of the disease. If a patient have recently arrived from a foreign country, it becomes a subject of inquiry in how far may the production of the disease be due to circumstances affecting him before his arrival. Among the circumstances which may be suspected of being involved in the origin of the disease in these cases, are endemic and epidemic influences to which he may have been exposed prior to his embarkation, the confinement and hardships of the voyage, contagion, moral influence of change of country, etc.

Cases in Private Practice.—One of the cases was observed in a neighboring place, and is omitted. Of the remaining *thirteen* cases, *eight* had resided in this city for a period less than six months. Of the other *five* cases, *one* had resided in the city about a year, *one* about two years, and the remaining *three* had resided for a longer period. In *four* cases the patients had recently emigrated to this country. Among the latter is included the single case of *Typhus* in the group of private cases.

Hospital Cases.—Of the *Typhoid* cases the period of residence is

recorded in *ten* cases, and nothing on this point stated in *eight* cases. In *seven* of the *ten* cases the patients had recently emigrated to this country. In *nine* of the *ten* cases, the patients had recently come to reside in this city. The periods in the cases respectively, as recorded, are as follows:—six weeks in the country, and four weeks in the city; ten days in the country, and three days in the city; thirteen days in the country, and ten in the city; eight weeks in the city; a few months in the city; five weeks in the city; three and a half months in the country, and eight days in the city; recently in the country; a year in the country, and four months in the city.

In the single case in which the patient had not recently come to reside in the city, the patient had been in the country fifteen months, and in the city twelve months. This was the longest period of residence in any of the *Typhoid* cases in which the records contain information on this point.

Of the *Typhus* cases, the period of residence is stated in *ten* cases, and the histories are defective on this point in *two* cases. Of these *ten*, the patients had recently arrived in this country and place, in *eight* cases. Of the two remaining cases, the patient in one case was a native, and permanent resident; in the other case, the patient had been a resident of the city for ten years. The periods in the *eight* cases respectively, as recorded, are as follows;—in the country six months; five or six weeks in the country; in the country a month; just arrived in the country; in the country a week; recently in the country, and six weeks in the city; twelve days in the country, and four days in the city; just arrived in the country and city.

Duration of the disease before coming under observation.—In *nine* of the cases in private practice, the disease was observed from its commencement. Of the remaining (5) cases, *one* came under observation on the *fourth* day after the attack, *two* on the *seventh* day, *one* on the *fifth* day, and *one* on the *tenth* day. Of the fatal cases, *two* came under observation at the commencement of the disease, *one* on the *seventh* day, and *one* on the *fifth* day. Of the *Hospital* cases, four are deficient in information on this point. Of the remaining thirty-four cases, five only came under observation at the commencement of the disease; and by the commencement of the disease I mean not to include the prodromic symptoms, which, of course, nearly always exist for a greater or less period before application is made for medical aid, or the relief of an hospital. The duration of the disease prior to observation, varied from *two* to *twelve* days; in the greater number of cases, the period was between *four* and *eight* days. Before coming to the

hospital, some had received no treatment, others had been attended by regular Practitioners, and some by Empirics.

The only point of interest under this head, which occurs to me, is to ascertain if there seems to exist any connection between the duration of the disease before coming to the hospital, and the fatal result. Of the *two* fatal cases of *Typhus*, *one* came under observation on the *fifth* day, and *one* on the *fourth* day. Of the *four* fatal cases of *Typhoid*, *one* came under observation on the *eighth* day, *one* on the *eleventh* day, *one* on the *fifth* day, and *one* on the *first* day. Of the cases of doubtful type, the only fatal case came under observation on the *eighth* day. The mean duration in these cases is about *six* days, which is about the average duration in the cases which did not prove fatal.

SECTION SECOND.

The Access, its duration and symptoms. Circumstances supposed to have been concerned in the production of the disease.

By the term access I mean to designate that period during which the disease appears to be forming, commencing with the first symptoms of disorder, and ending when the disease becomes fully developed. By some this period is regarded as a *stage* of the disease, sometimes called the forming stage; others regard it simply as presenting precursors, preceding and ushering in the disease, or the prodromic symptoms of the disease, but not intrinsically belonging to it. Without stopping to present arguments for either of these views, I will proceed to examine the cases under analysis with reference to this period. The chief points of inquiry which possess interest and importance, relate to the *duration* of the access, and to its *symptomatology*.

Duration.—In the *twelve* cases of *Typhoid* fever occurring in *private* practice, the records are deficient in information respecting the *access* in *two* cases. In the remaining *ten* cases, without an exception, the period of access embraced several days; but in most of the cases the precise period is not stated. The record in *six* cases, simply states that it extended over several days; in *one* case, it is stated *four* or *five* days; in *one*, *two* or *three* days; in *one*, *three* or *four* days; and in *one* the patient had been in ill health for three weeks.

In the single case of *Typhus* in this group, the period of access was *two* days.

The indefiniteness of these records denotes a feature of the access of the disease which writers on the subject have remarked, *viz.*, the difficulty of

fixing, with precision, the precise date of its commencement. Patients when asked respecting the time when they first began to feel ill, generally reply "several days," "three or four days," etc., apparently not being able themselves to decide on the particular day when they ceased to feel well. In such instances the symptoms come on gradually and imperceptibly. On this account it is not practicable always to determine the exact duration of the access.

Nor is it an easy matter to decide on the termination of the access. What denotes the commencement (or the second stage) of the disease? This question is not readily answered. The symptoms of the access are generally continued, increased in degree, but not changed in character; nor are any new characters developed which may serve to indicate, with certainty, the termination of the access. My rule has been to pronounce the access passed, and the fever established, when the patient is compelled to take to his bed, i. e., when he feels no longer able to sit up. This rule is somewhat arbitrary, and is by no means perfectly accurate. Some patients, from choice or necessity, will keep about much longer than others. Much will depend on differences in mental disposition as respects apprehension, prudence, etc., with regard to this point. But, with these objections, I know of no single event, or group of symptoms, more available, and in the majority of cases, more correct than this.

In *hospital* cases it is much more difficult to ascertain the duration of the access than in private practice, patients frequently being received after the disease is more or less advanced, when they are unable to give a correct account of its origin and progress. On examining the histories I have collected, I find them very deficient in information on this subject. Of the *eighteen* cases of *Typhoid*, the information is very indefinite in *twelve* cases; in *four* cases, the disease appeared to have come on with a sudden attack. Of the remaining *two* cases, in *one* the access was of several days' duration, and in the other, *four* days. It is to be presumed that in all instances in which it could be ascertained that the disease came on suddenly, the fact was recorded; and that when the records are silent on this point, the disease came on, as is usual, with an access of several days' duration.

Of the *twelve* cases of *Typhus*, the records are defective in *five* cases; the attack was sudden in *one* case, the access was "several days" in duration in *four* cases, *five* days in *one* case, and *seven* days in *one* case. Of the *eight* cases of doubtful type, the attack was sudden in *one* case; the access was "several days" in duration in *three* cases, two or three days in

one case, and in *one* case the patient had been ill three weeks, (symptoms not ascertained,) and had recovered sufficiently to go to work, when he was suddenly attacked. In *two* cases, the records contain no information.

Of the *three Typhoid* cases proving fatal, the disease came on suddenly in *one*, and the duration of the access was not ascertained in the remaining *two* cases.

Of the fatal cases of *Typhus*, the attack was sudden in *one*, and the duration of the access unknown in the other case.

In the fatal case among those of doubtful type, the access was of several days duration.

The only conclusion to be drawn from the foregoing rather unsatisfactory analysis, is, that generally the access extends over several days, but that the attack, in a small proportion of cases is sudden—a conclusion according with general experience.

The data do not warrant a comparison of the two types in order to ascertain in which is the disease most likely to commence suddenly, and in which the average duration of the access is longest; nor is the comparison of the fatal and not fatal cases, in these respects, of any value.

Symptoms of the Access.—The records of the cases are far from being comprehensive as respects the symptomatology of the access. This is, in part, because sufficient pains were not taken in noting the symptoms, and in part, because in a large proportion of cases, (more especially those in hospital) it is difficult, and, indeed, in many instances impossible to obtain a full account of the history prior to the patients coming under observation. In the cases in which the history embraces some mention of the symptoms of the access, generally the circumstances only which are striking, or occasional in their occurrence, are noted, those being omitted which are constant or quite common. I do not, therefore, deem it worth while to count the number of times each particular symptom is mentioned, with a view to determine the relative ratio of its occurrence, but I will simply give a list of the symptoms, with brief comments. They are as follows:

Cephalalgia, a frequent symptom in the cases of both Typhoid and Typhus; *anorexia*, an almost constant symptom; *chills*, in a few cases, with *rigors* more rarely, oftener *chilly sensations or slight shivering*; *pain in limbs*; *pain in loins*, not so frequent, or prominent symptom as the last; *sense of debility*; *lassitude*; *cough* in three cases of Typhoid, in no case of Typhus, complicated with soreness in the chest in *one* case, and with pains beneath the sternum in one case; *Diarrhœa*, in one case of Typhus, (a fatal case.)

and in four cases of Typhoid ; *nausea*, and in *three cases vomiting*. Of the three *fatal* cases of Typhoid, the following are the symptoms of the access noted, which are more or less peculiar to these cases ; in *one case delirium at night* ; and in one case *tremulousness in the muscles of the face* as in mental agitation, in *one case vomiting and diarrhœa*.

Of the the three fatal cases of Typhus, in two the symptoms of the access were not noted, and in the remaining case the symptoms were, *cough, headach, pains beneath sternum, anorexia, moderate thirst, vomiting and diarrhœa*.

Circumstances supposed to have been concerned in the production of the disease.

In obtaining the previous history of cases coming under observation pains were not taken to inquire concerning the circumstances which might seem to stand to the disease in some causative relation. In the majority of cases nothing is recorded on this subject. Occasionally, however, the patients themselves mentioned circumstances to which they attributed their illness. The instances of this kind, and the supposed causes, and the type of the disease, are as follows :

Excesses of dissipation, continued for several weeks, to which the patient had previously been unaccustomed. (Typhoid.)

A catarrh, or mild bronchitis, accompanied by cough and soreness in chest, in two cases. (Typhoid.)

Bathing in cold water. (Typhoid.)

Exposure to cold and wet on the day prior to the attack, the patient becoming much chilled. (Typhus.)

Drinking cold water when heated. (Typhoid.)

Falling into the canal, and working all day in wet clothing, the patient being obliged to take to his bed on the following day. (Typhoid.)

Several of the patients thought their illness came from "taking cold," but without their having had any symptoms which constitute what is called a "cold," and the attack not succeeded any notable exposure. Many evidently referred their illness to cold under the common impression that most diseases thus originate.

Of those who had recently arrived in this country, the inquiry was generally made, whether sickness prevailed on ship-board ; in two instances only was it ascertained that this was the fact. Sometimes the answers to the inquiry were unsatisfactory, but in several instances the patients were certain that no febrile disease existed during the passage.

In the great majority of the cases, no cause for the disease is assigned.

In the few instances given in which causes were specified, it will not, of course, be supposed, from their being embraced in their histories, that much importance is attached to them. It is not probable that the circumstances mentioned by the patients as causes, really were so, or, at most, they are only to be regarded, as possibly, exciting causes. And so far as my observations go, continued fever frequently, if not generally, occurs without being preceded by any events which, even in the estimation of patients, stand to the disease in the relation of cause; and, hence, it is a fair presumption in those instances in which an attack follows any of the common causes of disease, that their agency, if they have any, is of a subordinate kind.

In three cases facts connected with the development of the disease seem to have a bearing upon the question of *contagion*. One of the cases referred to, occurred in private practice. Twenty days before the development of the disease, a brother of the patient arrived in the city on a canal boat, and was transferred, first to a hotel, and, two days afterward, to the hospital. He was attended by me both at the hotel and hospital, but a record of the case was not made. The character of his disease was not determined until after his removal to the hospital, when it proved to be a case of Typhoid fever, with the characteristic eruption, etc. While at the hotel the two brothers occupied together a small bed room, and the then well brother, not only remained during the night, but officiated as an attendant throughout the day. The type of the disease, developed twenty days afterward, was typhoid; the case was very mild, and unusually short in its duration, terminating, with a critical perspiration, on the fifth day. A few rose spots were observed on the chest and abdomen. In view of the circumstances just mentioned, it seemed fair to *suspect* that the disease was communicated from one brother to the other.

In the other two cases in which a contagious principle seemed to be involved, the patients were *Sisters of charity* at the hospital. To the two *Sisters* who had fever, had been assigned the care of all the fever cases, (with the exception of three cases) during the preceding winter, up to the time of their illness. They, alone of all the inmates of the house, became affected with fever. The first that became affected with fever, (sister H.) was attacked in February. She was attacked early in lent, and had conformed to the rules of abstinence prescribed by the Catholic Church for this season, continuing her laborious duties as before. This is mentioned as a circumstance to which perhaps, something is due in accounting for the development of the disease at that time. The disease was mild, attended

with some delirium, no abdominal symptoms, without an eruption, terminating in convalescence on the *ninth* day. The case is included among those of *doubtful type*.

The second (sister N.) was attacked about a month afterward, in March, 1850. Her duties, in consequence of the illness of sister H., had been for the previous four weeks much increased. This case was milder than the preceding, and shorter in duration. It was unattended by abdominal symptoms, and an eruption. It is also included among the cases of *doubtful type*.

That thus the two persons who were especially brought into close and continued proximity to the fever patients, should have contracted fever, no other case of fever being generated within the Institution, certainly can be less rationally explained by the law of probabilities, than by the supposition of contagion.

Assuming that these instances do exemplify the operation of a contagious principle, the facts accord with the views now generally entertained respecting the communicability of Continued Fever, viz., that the disease may be diffused by contagion, which differs in intensity at different times and places, but that generally the disease is not developed except after long continued exposure to the contagious miasm, or under circumstances in which the miasm is greatly concentrated, and, even then, co operating causes seem often to be required to determine the attack.

It is supposed by those who recognize *Typhoid* and *Typhus* as distinct types of Continued Fever, that the latter is either exclusively, or much more liable to be communicated than the former. The first of the instances just stated, assuming the supposition of contagion to be correct, would establish the communicability of Typhoid. The two latter instances cannot be considered to have any bearing on this point, inasmuch as the patients had been brought into contact with both types of the disease.

(*To be continued.*)

ART. II.—*Surgical Cases.* Reported by FRANK H. HAMILTON, M. D., one of the attending Surgeons to the "Buffalo Hospital of the Sisters of Charity."

Varicose Veins — Radical operation.—Thomas Mountfield, aged 45: admitted November 17, 1849. He has ulcers on both legs, with varicose veins. The veins are most enlarged on the right leg. In 1834, one of the veins of the right leg ulcerated and bled copiously, since which time the ulcerations have been only occasionally healed. Mountfield is a bel-

lows-maker, and he is much of the time obliged to cease labor on account of the ulcers. Habits temperate.

With horizontal posture, stimulating unguents, diet, &c., a cure of the ulcers was effected, and he was dismissed December 15.

February 23.—Mountfield returned for the purpose of having an operation made, the ulcers having again opened. During the five subsequent days, he was kept in bed, and subjected to a preparative regimen. On the 28th I operated on the two principal veins below the knee by *excision*; exposing the veins by an incision of an inch in length and then completely excising the portion of vein exposed. One of the veins was cartilagenous. No ligatures were used, but the wounds were closed and the bleeding arrested by a small compress over each wound, with straps and a roller moderately tight.

March 1.—The bandages have been rather painful during the night. Ordered to be loosened. No hemorrhage. Absolute diet. At 2, P. M., of this day had a chill. Took at 10, P. M., proto. chlor. hyd., grs. x. jalap. pulv. grs. xv.

March 2.—Has sympathetic fever. Leg very tender, and edges of wound unhealthy. ℞—Cataplasma ulmi Americanæ pulv., etc.

March 3.—Removed all dressings except the cataplasm. *Phlebitis* has commenced.

From this time forward the wounds refused to heal, phlebitis and angi-leucitis extended up the leg to the middle of the thigh, followed by superficial erysipelas and then deep, diffuse phlegmon, extensive suppuration, &c., a violent irritative fever being the accompaniment. The treatment was at first soothing and cooling. When, however, the phlebitis, &c. continued to extend, it was intercepted by girdling the limb above the line of its march with successive circles of nitras argenti and tinct. iodidi. These applications arrested its upward progress. Cataplasms of ground elm were continued to the sores, and quinia, wine, &c. were administered internally. Abscesses continued to form, and a feeble irritative fever persisted in spite of tonics, good diet, &c., for many months, and the leg was not sound, and his health restored for a long time afterwards. In short, he has escaped death most narrowly, but the varices are effectually cured!

If we enquire for the cause of phlebitis, &c. in this case, it must be looked for, I think, in the constitution of the patient and in the peculiar nature of the operation, to which phlebitis is notoriously incident, rather than to any peculiarity in the mode of operation or treatment. The operation was made according to an approved method as performed.

by Signor Prima and others, and for which the advantage of greater safety has been claimed by its inventor. The patient was subjected to a rigid preparative treatment. The dressings of the wound were simple and light—the pressure being no greater than was necessary to restrain a hemorrhage from veins with the limb horizontal and at rest, and as soon as, in consequence of a slight swelling, this became painful, it was loosened. The chill, which occurred within twenty-six hours after the operation, indicated thus early the unfavorable issue, and the subsequent treatment cannot, therefore, be held responsible for the phlebitis. We conclude that it is only another illustration of the danger of any attempt to close varicose veins.

Varicose veins — radical operation.—F. W. Van Orman was admitted to the hospital as a private patient February 6, 1850, for the purpose of being operated upon for varices. Age, 23. Occupation, master of a lake vessel. Has had varicose veins, and a large, troublesome ulcer on the same leg, for several years. His habits are temperate.

Mr. V. had subjected himself, under my directions, to a moderate diet for several days before he came to the hospital. I detained him three days in the hospital under regimen before I operated.

February 9.—I operated in presence of the class, by *tying* three veins, each in a different mode. First, according to the original method of M. Velpeau, by passing a pin under the vein, and crossing a ligature above it in the manner of the twisted suture formerly used for hare-lip. Second, according to Chaumette and others, by passing a ligature under the vein, by means of an armed needle, and then tying the ligature over a small compress upon the vein. (“Mediate or indirect ligature.”) Third, by *exposing* the vein and then ligating it.

The ligatures were all removed within three or four days. No inflammation of consequence followed, and the patient was dismissed, cured, on the 18th of February.

Labium leporinum; operation.—Infant child of Mrs. Rights, aged five months. Entered hospital December 14, 1849: has double hare-lip; extends through alveoli on both sides; the central piece of the alveoli containing the rudiments of four teeth, is carried forward upon a prolongation of the septum nasi, until it is on a line with the spine of the nose. It is covered by a narrow and short piece of integument, which is continuous with the tip of the nose. I operated before the class by dissecting up the narrow slip of integument, and leaving it attached to the end of the nose, with the view of making a column of it at some future time. *I then*

made a partial section of the piece of septum beyond the nares, and upon which the projecting alveolus was situated, and I was now able to bend it down so as to be in place on a line with the remaining alveoli, and so as to nearly fill up the chasm in the jaw. This part of the operation is, I believe, original with me.

The bleeding after the section of the bone, was quite free, but it was soon arrested by cold water and snow. I now abraded the edges of the chasm, in the soft parts, and brought the lips together with two interrupted sutures and with straps. On the 23d I removed the last suture, and on the 24th erysipelas commenced, which was not arrested until a week from this date. The union, however, was perfect, and was not disturbed by the inflammation.

March 23, 1850.—The child was again presented, and the edge of the septum having formed a very excellent column, I had no need of the piece of integument which had been left, and therefore cut it away. The line of the jaw was perfect, with only a slight fissure on each side of the central piece.

Labium leporinum ; operation.—Private patient. Augustus Herbold, aged nine months: single fissure on right side of raphe.

I operated, June 13, 1850, by excising the edges of the fissure, with scissors, and closing the wound with interrupted sutures. In this case the strain on the sutures was very slight, and I did not think it necessary to employ straps.

I removed the sutures on the fourth day, and substituted straps, which latter were continued a week longer. The cure is complete.

Fracture of os frontis ; Compound. Death. Patrick Mathews, admitted Feb. 20, 1850 ; aged 28. Fell from a scaffold 20 feet high, striking probably upon his feet, and then pitching forward upon his forehead. The malleolus internus of the right leg was broken. The os frontis was fissured over the right brow, accompanied with a considerable wound of the integument. When admitted, he was suffering under symptoms of concussion. In consultation, it was agreed that trephining was not indicated. The next day consciousness had partially returned ; he was bled sixteen ounces, moderately cathartised, and other antiphlogistic remedies were employed. On the 23d, his symptoms indicated depression, and the plan of treatment was changed. From this time until his death, which took place four weeks after, his mind wandered, and at length it became fatuitous. He had no paralysis, except of his sphincters. He was uniformly worse under the operation of a cathartic. Enormous bed sores formed ; his legs and feet

became œdematous, and his evacuations were involuntary. The treatment was chiefly tonic and stimulating, with occasional counter-irritation. Protochlor-hyd. was also given during two weeks, in small doses. No autopsy was obtained.

Fracture of os frontis; compound. Recovery.—Thomas Bluett, aged 22; admitted March 19, 1850. He was thrown from a wagon, his forehead striking a stone, and a wheel passing over the back of his head. The scalp was torn in both places, but chiefly over the right eye. A fracture, with very slight depression, was found under the wound over the occipital bone. This wound was closed with one suture. The fracture of the os frontis was more depressed, yet slightly: the fissures extended in several directions; he was in great pain, being conscious, and answering questions correctly. I removed with Heys' saw a small portion of bone, but was unable to introduce the levator. I then applied an instrument made like a dentist's screw for extracting stumps, by fastening it into the center of the depressed bone, and then lifting carefully, by which the bone was immediately brought to its level. Such an instrument is often useful, but it ought not to be used where much force is requisite, or where we have reasons to suppose that the inner plate is separated from the outer, lest we lift the outer plate, and leave the inner depressed. During the succeeding week, symptoms of violent cerebritis supervened, which were subdued with difficulty by three liberal bleedings, cathartics, &c. At a later period, bed sores occurred, with involuntary evacuations, for which a moderately sustaining regimen was prescribed. Copious discharges of matter took place from the wound, and one piece of bone exfoliated. The right eye was destroyed. He was dismissed in May. In July, four months after the accident, a slight discharge continues from the wound, but he is in other respects well.

Fracture of the os frontis; compound. Recovery.—June 5th, 1850, M—— W——, aged nine years, (private patient, in counsel with Dr. John Trowbridge,) fell about ten feet, upon the corner of a broad stair, and fractured her skull over the left brow, producing also an extensive wound in the integuments. She walked into the house alone, and described to her mother how the accident occurred. One hour after, she was nearly comatose, doubtless from effusion of blood under the cranium. The skull was broken extensively, and while a large piece was depressed to the depth of a quarter of an inch, another portion, constituting most of the superciliary ridge, was raised, and hung down over the orbit. The depressed bone was elevated, after removing several small pieces with forceps and with the

elevator. The superciliary ridge could not be replaced without removing a small portion with the saw. Owing to the elasticity of the bones at this age, it was found impossible to bring all the pieces smoothly into place, yet it was predicted that in a few years, perhaps in a few months, no deformity would exist. A cooling regimen was immediately adopted. Soon after the wound was dressed, she vomited freely. Next morning she was conscious, and has from that time steadily convalesced. Scarcely any inflammation followed the injury, which we ascribe in part to the lotions of cool water which for several days were kept constantly upon the wound. July 15th, she is well, except a slight discharge from a small orifice at the seat of the original wound. The eyes are uninjured, and the slight irregularity of the forehead is even now scarcely observable.

A comparison of these three cases shows these several points of agreement, and of difference. The frontal bone was broken in each case; slightly in the first, more in the second, and by far the most in the third. The first was intemperate; we took his bottle from his pocket at the hospital; the second was temperate, but of full habit; the third was in good habit, and, it is unnecessary to say, perfectly temperate. The first was twenty-eight years old, the second twenty-two, and the third nine. The first died in a month; the second escaped death barely, and he did not leave the hospital under two months; the third walked within a fortnight, and was well in three weeks.

Penetrating wound of the abdomen. Recovery.—Thomas Shepard, a mulatto boy, aged 16 years, admitted June 15, 1849. Was stabbed by a sailor about eight hours before admission, with a butcher knife. Knife entered on the left side, at the anterior edge of the quadratus lumborum, about midway between the last rib and the crest of the ileum. It was dressed by Drs. Ring and Gray; has bled some, both before, and since the dressing. Lies best on his back; restless. ℞. opium gr. i, every four hours until he becomes quiet.

June 16.—Has taken four grains of opium; is more quiet; hæmorrhage slight. ℞. opium gr. i, every eight hours. Diet, rice water.

June 18.—Dressed the wound; looks well. ℞. ol. ricini fʒss. Discontinue opium.

20th.—From this time the bowels were moved every day or two, and he was sustained with broths, wine, &c. On the 3d of July, we discovered a large plug of fibrin gradually pressing its way out of the wound, the mouth of which was stretched to its utmost capacity. On the 4th, we drew it out, and found it quite firm—two inches in length by three-fourths

of an inch in diameter. The wound now rapidly granulated, and healed without an untoward symptom.

It is impossible to say whether the knife penetrated an intestine or not. It is not improbable that it did, as the wound was not less than four inches in depth, and its direction toward the viscera. If so, he was saved from fæcal effusion, and death, by the opium. He had, we were informed by his mother, a strong hæmorrhagic diathesis, which determined the early use of good diet, wine, &c.

Penetrating wound of the abdomen. Death.—August 24, 1849, Michael Rush, private patient, aged 20 years, was stabbed with a jack-knife, about four inches below the umbilicus, and to the right. The wound was on the surface, about one-quarter of an inch in length, from which a piece of omentum of the size of a hen's egg protruded. It had already been out more than an hour, and it was too completely strangulated to permit of its return with safety. I therefore applied a ligature of harness maker's silk around the mass, close to the wound, and then cut off the omentum outside of the ligature, and reduced what remained. Opium in full doses was prescribed, and complete rest enjoined. He died in twenty-four hours, from acute peritonitis. The autopsy disclosed a wound of intestine; fæcal effusion; peritoneal inflammation; serous and fibrinous effusions. This man walked some distance after the infliction of the wound, during which time we think the escape of fæcal matter into the cavity of the peritoneum occurred.

The two last cases are recorded without having full notes before me, but I believe they are, in all essential points, correct.

Congenital Hypertrophy of a toe—Amputation, &c.—May 18, 1849.—Jacob Argus, (private patient,) aged ten years, a native of Chictawaga, in this county, a healthy lad. His parents are Prussians and remarkably healthy. At birth, the second toe of his right foot was noticed to be longer than the other toes; and from this time it continued steadily to enlarge both in length and breadth until the present time. It is sometimes painful when he walks much upon it, but never very troublesome. At this time it measures seven inches in circumference, and projects three inches beyond the other toes.

It has the feel of firm adipose structure; its color is natural, and the skin healthy. Its great and increasing size render him anxious to get rid of it.

May 18.—I removed the toe by amputation, carrying the incision to near the proximal end of the metatarsal bone, and separating the bone at

this point with a chain saw. My object in dividing the bone at this point, was to get beyond the diseased structure, and also to permit the adjoining metatarsal bones, which were separated an inch or more, to approach each other. The incisions upon the plantar surface were double elliptical, including a large projection of hypertrophied structure extending nearly into the center of the foot. The bleeding vessels were numerous, but only two or three required the ligature. The wound was closed by sutures and straps, and by the roller the contiguous metatarsal bones were brought nearly together without the pressure becoming painful.

On examination of the amputated member, I found that the metatarsal and phalangeal bones were hypertrophied in the same relative proportion with the soft parts; and the cellular texture had degenerated into a light colored, fibrous mass, holding in its cellules, whitish fat granules. The tegumentary textures were somewhat thickened and firm.

I regard it as a species of the elephantiasis Græcorum, or Barbadoes leg, although it lacks many of the features of that disease as it appears in the tropics. It is probably a more strictly local disease than the latter, and is, therefore likely to be more amenable to local treatment. The result, at least, in this instance, has shown the correctness of the practice.

The wound healed in a month, and on the 22d of June, 1850 — more than a year after the operation, I have examined the foot, and found it sound, with no disposition to reproduce the malady. I have a cast of the foot, taken before the operation, and one taken a year after, which may be seen in my museum at the Buffalo Medical College.

ART. III.—*Application of Collodion in Small Pox, to prevent pitting.*
By S. B. BRINKERHOFF, M. D.

LINESVILLE, Pa., July 9, 1850.

DR. FLINT—*Dear Sir:* Having pursued a treatment of my own, as far as I know, (yet it may have been practised by others, and not have met my notice,) in two cases of small pox, I have thought perhaps it would not be doing wrong to state the facts to you. I regret that the notes I took of the cases at the time, have by accident been misplaced, and I shall have to state the facts from memory. The first case occurred in a person who had never been vaccinated to his knowledge. The second had been vaccinated, but I could not discover any scar as showing it had worked well, or at all. I will not state particulars as to the cases, more than that I considered them both cases of virulent small pox.

The treatment was as follows: As soon as the eruption showed itself, I applied with a fine varnish brush, to the face and neck, a coating of collodion, except the upper eye-lids, and a small place on the neck. The place on the neck I left to see if there would be any scars, should the patient recover. I applied the mixture every day for four days, then every second or third day, as I thought required. One on the 21st, the other on the 23d day, was discharged as convalescent, with directions should any scattering eruptions appear, to apply the mixture over them. There was scarcely a single scar left on either patient over the parts where the mixture was applied, but in each case, where there was no collodion used, as the spot on the neck and other parts, was pitted as much as I ever saw in my life.

I pursued this practice from a suggestion in your lectures, while upon this disease, stating that covering the face, &c., with gold leaf had been considered by some as a preventive against scars being left, by excluding the air from the affected part. The gold leaf is an expensive mode of treatment, while the collodion is cheap, and easy of application.

Should you think this worthy of notice in your Journal, you are welcome to do so, if not, just as well.

Yours, sincerely,

S. B. BRINKERHOFF.

AUSTIN FLINT, M. D.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

From an article in the Western Journal of Medicine and Surgery, entitled "*Two Lectures on Water, Delivered to his Class, at Lebanon, Tennessee, by F. H. Gordon, M. D.,*" we select the following extracts.—EDITOR BUFFALO MEDICAL JOURNAL.

Fomentation is a mode of applying warm water, by dipping flannel cloths in it, and applying them to the surface. It is an old remedy, and is much used at this day. But it is the least efficient of all the modes, because there is but little force in the application, and the cloths cool so rapidly as to require frequent changing. Yet from its convenience, it answers a valuable purpose in many instances, where a more powerful effect is not demanded.

The poultice is a much more potent antiphlogistic, and in subduing local inflammation it is deservedly popular. Yet there are frequent failures in securing the good effects of this remedy, because of the injudicious manner of its application. The poultice, to do great service, must be *large, light, and frequently changed*. It must be large, because a small poultice cools too speedily; it must be light, because otherwise its weight would oppress the patient; and it must be frequently changed, to keep up a temperature as nearly uniform as possible. To accomplish all of these objects, wheat bran is the best material for a poultice, and should be managed in the following manner: Suppose you have a case of peritonitis, put ten quarts of wheat bran in an iron pot; gradually add water, and stir till the whole mass is *merely moist* or damp throughout. Now heat it, and stir occasionally till it is quite hot. Put half of this in a bag not less than eighteen inches square, (a pillow case is a suitable bag for this purpose,) spread the bran out, and apply the poultice, as hot as the patient can bear it, to the surface of the abdomen. At the end of twenty minutes have the remainder of the bran ready in another bag, and apply it the instant the former poultice is removed. Return the cool bran of the former poultice to the pot, to be ready to be reapplied at the end of twenty minutes more; and thus continue to change the poultices every twenty minutes, so as to keep up a temperature as nearly uniform as possible. Poulticing in this way for four or five hours will do more in subduing inflammation than a little and badly managed one could in a week. In fact, a little poultice is the mere semblance of a remedy; but when all of the conditions here named are observed, the poultice is powerful. I have named wheat bran as the best material; but poultices are made of various substances, many of which are supposed to possess some peculiar virtues, apart from the water

they contain. But I suppose that the remedial agency of all or nearly all poultices is due to the water alone, which they contain. The tansy, hot ashes, cedar leaves, yarrow, corn mush, and wheat bran are only means of absorbing warm water, and holding it in contact with the surface. And we frequently see poultices medicated with oak ooze, vinegar, and other means, under the belief that these means have some agency in reducing the inflammation. Such medications of course do no injury, and I never object to them, but gratify the friends of the patient by allowing any such medication, provided the poultices are made large and hot, and are changed frequently; for my reliance is on the water in the poultice. * * *

The stream is the most valuable form of applying warm water for the cure of local inflammation. I have been familiar with it for years. It had been used by surgeons to relax the muscles in reducing luxations; but I am not aware of its having been employed for the cure of local inflammation before I introduced it into the practice. For this latter purpose I am acquainted with no remedy which can equal it in value. I do not wish to overrate it, or to appear to exaggerate its powers, for this would prevent a proper estimate from being placed upon it. Yet, to those who have not tried the remedy, the whole truth will look like exaggeration. Cups, leeches, blisters, and the lancet, do not equal it. The stream is a mode of applying warm water which complies with all of the conditions requisite to make it efficacious. It is *more constant*, may be *longer continued*, and applied with *more force* than any other application of water. The stream may be conducted almost without variation of temperature or force for hours, and, falling *only upon the suffering point*, it does not prostrate the patient like the bath, and hence may be continued long enough to greatly subdue the local inflammation before there is much depression of the vital powers. The *force* of the stream is an item of peculiar importance, because it is incomparably greater than that of any other mode of using warm water. It is a well known fact, that *fluids in motion* manifest their peculiar powers in a much higher degree than when at rest. The sultry air of a summer's day, which almost melts us by its quiescence, may, when put in motion, make hail, and chill the body. Electricity, which gently pervades our bodies at all times, and stimulates all of the vital movements without misrule, if disturbed in its equilibrium, and transmitted through the organs, with its incalculable velocity, will produce instant disorganization and death. Caloric, which pervades all of the tissues, and without the vital functions could not be performed for one moment, does no injury while latent or in gentle motion; but when a large amount passes in or out of any point of the surface in a short time, destruction is the instant result. In like manner, water at rest in contact with the body will abstract or impart caloric according to its temperature, in so gradual a manner as to cause very striking results; but when a swift current of water strikes the surface, its effects are manifestly in proportion to its velocity. Hence the stream of warm water as far surpasses in virtue the vapor bath and fomenting cloths, as the gentle breeze which wafts a feather is transcended by the tornado which uproots the sturdy oak.

But this, like all other remedies, may lose more than half of its virtues by being badly applied; it is, therefore, important that some special directions be given for its employment. Your object is to cause a *constant stream to fall with force upon the suffering point*. You may accomplish all

of these objects by pouring with two pitchers, alternately, so as never to stop the stream longer than is necessary to exchange an empty pitcher for a full one. Or two coffee-pots or tea-pots will answer the same purpose. But the syphon is far the best instrument. A leaden tube from two and a half to three feet long, or a bent tin tube, or two joints of cane mitred together, secured with twine, and made air-tight, will answer the purpose. The outer leg of the syphon must be from two to four inches longer than the one placed in the water. The diameter of the instrument must be from one-eighth to three-eighths of an inch, according as the patient is prostrated or vigorous. To understand the application of this instrument, particularly on the trunk, suppose you wish to apply the stream in a case of peritonitis. Take every thing from the bedstead down to the cords; fold the bed-clothes, and place them on the cords so as to make two beds of them, of equal height, with an interval of two inches between them, about where the umbilicus of the patient will be when he is placed upon them. Now place the patient, supine, upon the beds, with his umbilicus over the interval, and a pillow under his head. Two blankets must cover the patient above and below the umbilicus, so as to leave a space of four inches between them. Pass two silk handkerchiefs around the body of the patient, one just above and the other just below the umbilicus, with their ends hanging through the cords to conduct the water. Have the clothes of the patient well pulled up, so that they may not get wet. A tub is placed under the bed to catch the water. A plank rests on the head and foot board, on which a bucket of warm water is placed. One end of the syphon is put into the bucket and made steady by a weight tied to it, and the other end presents over the umbilicus of the patient. By applying the mouth to the outer end and sucking, the stream begins, and ought to be adjusted to make it fall upon the most tender point. It should fall five or six inches, from the end of the syphon to the skin of the patient, or as far as it can without making a spray. The water should be tempered in a separate vessel, and poured in the one which feeds the syphon, so as to keep it full and cause the stream to be uniform in temperature and force. The stream should fall constantly in the same place, because its effects will thus be greater than if moved frequently. The length of time consumed in using the stream must vary from one to five hours, according to the strength of the patient. Mustard plasters on the hands and feet will enable him to stand it much longer than without them. The rule is to continue the stream till the patient's pulse becomes small and frequent; and the higher the temperature of the water the longer it can be tolerated without faltering of the pulse. What should be the temperature of the water? At the beginning it should be about ninety degrees, Fahrenheit, and gradually increased till it reddens the skin slightly; this will be between one hundred and one hundred and fifteen degrees, according to the excitability of the patient. It is usual for patients to enjoy the stream finely for the first hour or two, but afterwards they become restless and insist on moving. They must be carefully instructed never to move themselves, for their clothes and bed would get wet. By rolling up a towel and placing it under one shoulder, and another under the hip of the same side, the patient will be made to rest for a while, and when he again becomes tired, place the towels under the opposite hip and shoulder. In this way you may make him contented till the bathing is ended, and all means should be employed

for this purpose, for the effects of the stream increases almost in fourfold proportion with the time. In fact the last hour appears to accumulate as much good as the three preceding ones.

The first effect of the stream is on the point where it falls, subduing the inflammation for a small space around. The reduction gradually extends to more remote parts, as may be proven by gentle pressure being gradually tolerated progressively outward from where the stream falls. And, in the case of peritonitis before us, the legs can be completely extended without pain.

The second effect is upon the muscular, circulatory, and nervous systems. General relaxation ensues; the pulse loses its hardness, becomes full and elastic, and ultimately, is small and frequent, retains its softness. At first, the patient is soothed; all pains cease and he falls to sleep, but afterward awakes and becomes restless and weary of his position. These constitutional effects are so gradual, that you may almost, and sometimes entirely, subdue the local inflammation, before the patient becomes so much depressed as to require the discontinuance of the remedy. This is the excellency of the stream—that it acts with power enough *upon the point of disease* to subdue the inflammation in part or entirely, with but temporary depression of the general energies. The general febrile symptoms moderate and disappear while the stream flows, with as much certainty as can be expected from the lancet, and yet without ultimate loss of strength. It thus often does away the necessity of bleeding, and may be used in cases of great prostration, where no one would think of using the lancet. To a considerable extent, the warm stream supercedes the painful practice of blistering, relieving the disease without it; and where it does not subdue the local lesion entirely, it always diminishes it greatly, if continued long enough, and being on the decline, the blister plaster may be at once applied, with full prospect of good effects. And to speak in general terms, in all cases of local inflammation, the patient gets well sooner by the aid of the stream, than he could possibly without it, and suffers incomparably less during his illness. Unlike bleeding, purging, blistering, and all other powerful means of subduing inflammation, the warm stream may be repeated as often as may be needed. After all that has been said, need it be urged that we have no remedy so potent in reducing local inflammation? Let it be well tried, in a variety of cases, and you will be astonished at its effects. To see so pleasant a remedy relieve the most intense agony, remove all soreness, subdue the fever, and restore the pulse almost to its normal characteristics, in a few hours, almost passes belief. And though you will not be fortunate enough to see this picture universally, yet you may often be gratified with the sight.

You ask, in what cases is the stream applicable? I reply, *in all cases where there is local inflammation*, no matter what may be the name of the disease, whether acute or chronic, the stream will do good, and the nearer it can be made to fall on the point of the most intense suffering the better. It is true, that its effects will be more striking in acute disease, but it will be serviceable in chronic also. In mild or slight cases, I do not hesitate to direct the stream upon the suffering point.

I have watched this remedy in its extensive application, for many years, and could adduce from memory and my notes, a very long list of cases, tending to show its value; but must be content to bring forward compara-

tively few of them, taken promiscuously from the mass in my possession, to which I ask your patient attention.

The cases are omitted.—EDITOR BUFFALO MEDICAL JOURNAL.

The Obstetrical Extractor. A paper read before the Chicago Medical Society. By JOHN EVANS, M. D., Prof. of Obstetrics in the Rush Medical College, etc., etc.

This is the title of a memoir communicated for the North Western Medical and Surgical Journal, by Prof. Evans, one of the editors of that periodical, and also issued as a separate publication. Prof. E. submitted his new instrument at the late meeting of the American Medical Association, explaining the principles of its adaptation, and replying to such objections as were suggested by different members. The mode of its application, also, at the same time, was demonstrated with the manakin. Like most discoverers, or inventors, he is sanguine in the belief that the *Extractor* will be found to be greatly superior to the forceps in most cases in which mechanical aid is required to effect delivery, and we sincerely hope that these expectations will be realized. Leaving for others more conversant with such matters than ourselves, critical investigation of the merits of the instrument, we transfer to our columns all the article referred to, except the plates, and what relates thereto, which, of course, we are obliged to omit.—EDITOR BUFFALO MEDICAL JOURNAL.

The idea of laying hold of the head of the child, to exert extractive force upon it, must have occurred to the first intelligent accoucher in attendance upon a tedious or obstructed labour. And yet we have no account of any successful means having been devised for fulfilling this plain indication, which so frequently occurs in the practice of every Obstetrician, up to the time of the Chamberlains. And since the discovery and universal adoption of the obstetrical forceps, which, by general consent, are not proper to be used in one-fourth the cases in which this kind of aid is indicated, the whole profession has, strangely enough, seemed to be satisfied with efforts at improving them, without seeming to think a better contrivance possible or desirable. So much so has this been the case, that when two years ago, I proposed to devise an instrument that would be simpler, safer, and more general in its application, I was met on every hand by the reply, that the forceps, properly constructed, were all that was wanted. But the multiplicity of modifications through which the forceps have gone, shows that there is great deficiency in the instrument.

Since the greatest obstacle in the way of improvement, is a want of a proper understanding of the errors and deficiencies under which we labor, I will, before describing the instrument I have invented, and used as a means of extracting the head of the child in difficult labor, direct attention to some of the numerous defects of the forceps. For, notwithstanding they are the greatest triumph of obstetrical art, if not the most useful

instrument ever invented for the relief of suffering humanity; still, they fall far short of fulfilling all the indications for a resort to extractive force in parturition.

The size of the forceps, with their wide, rigid, permanently curved clamps, renders their introduction difficult; in all cases painful—often dangerous—and under many circumstances, entirely impossible. The dangers of lacerating the vagina, os tincae and perinæum, and of destroying the child, from compression of the head, where much force is required, are familiar to all, and universally admitted.

A large number of cases evidently demanding such assistance as they give, are allowed to go on hour after hour, and sometimes, even days, without relief, from a horror of applying the forceps; which can only be the result of their danger, and the pain necessarily inflicted by them.

The forceps are not applicable until the head has engaged in the superior strait, and according to British authors, not until it has descended into the cavity of the pelvis, and are not applicable until the os tincae is dilated, under any circumstances. Hence, they are useless in many cases of hemorrhage and convulsions, demanding speedy delivery.

They interfere with the natural rotation, if applied high up in the pelvis, and thus render the delivery more difficult.

The rules for the application of the forceps are so complex and varied, that they are learned and understood with difficulty, and the cases in which their use is recommended occur so rarely, that few practitioners are prepared to use them, when the emergency requiring it comes.

Certainly, under these circumstances, and in view of the great amount of suffering endured in consequence of the want of an instrument to fulfil the indications in these and other cases, it will not be out of place to give the results of my efforts to furnish the desideratum.

The Obstetrical Extractor was exhibited to my class in Rush Medical College during the winter of 1848-9; but, by waiting to test its applicability, it has not been made more public until now.

The principle upon which it operates is plain and simple, being that of placing a band or fillet around the head of the child above its largest diameter, and fixing the ends near together, by steel fingers, so that it cannot be drawn off. From this band straps pass down to the vertex and out through the os externum, to be grasped by the hand, and upon which the extractive force is exerted.

The band or fillet is applied after the manner of passing the ligature around a polypus by Gooch's double canula, as will be more readily understood after describing the instrument.

The Extractor consists of two blades or fingers of steel that are exactly alike; each 11 inches long, 5 inches of which is handle and six inches finger; a sliding ring; a silk band passing from one finger to the other, 9 inches long by 1 inch wide, and 4 straps of silk braid passing from it downward between the fingers, 18 inches long, and one-third of an inch wide. Between these straps, and attached to the inferior margin of the band is a net work two inches wide, made of silk braid.

The handles are five-eighths of an inch wide, and three-sixteenths thick, uniform and straight the entire length, so that a sliding ring can be passed over both when their edges are brought together, so as firmly to fix them side by side.

The other part forms the finger which has a curvature to fit the convexity of the child's head. It is one-fourth of an inch thick at the junction with the handle, for here the greatest strain comes on the instrument, but rapidly becomes thinner toward the end, until it is as much attenuated as it can be, to allow of two strong hinge joints in it. It tapers also in width from five-eighths of an inch at the handle to three-eighths at the end, where it is terminated by a ring, the hole through which is one-half of an inch in diameter. The edges of the fingers are rounded, and the hinges made so that they present no roughness. They allow flexion enough to adapt the finger to any part of the head upon which it may rest at the time, and extension enough to make it as straight as is desirable in the introduction of the instrument. The slide holds the handle and fingers parallel and firmly together, so that their ends cannot separate.

The band or fillet is firmly attached to the end of each finger at the ring, and by a few drilled holes below it. This is the main part of the instrument, as the steel fingers are only used to pass it around the head and hold it there.

The net work is designed to keep the band from slipping over the chin of the child, and to keep the straps in their places.

The straps are firmly sewed to the margin of the band nearest the handles, and pass down so as to reach some distance below the ends of the handles. These may be tied together at their ends, to form a loop in which the hand may hold when pulling to extract the child.

The thread or fine braid is to draw the band into the rings at the ends of the fingers, so that it will be carried entirely up during the introduction of the instrument.

To prepare the Extractor for application, draw the band through the rings, and tie the threads holding it to the ends of the handles; put on the slide, extend the fingers, adjust the straps, and dip the whole in oil.

To introduce it, pass two fingers of the left hand to the presenting part of the head of the child, or to the os uteri; take the instrument as prepared, by the handles, holding the straps in place, parallel with the fingers of the instrument, and pass it within the uterus, with the palmar surface of the fingers next the head. Then as it is passed up, flexion of the fingers will take place from traction on the straps, and the pressure of the uterus, so as to keep them in contact with the head, as if grasping it. With the fingers of the left hand, examine and adjust the end of the instrument, so as to give it a right direction, then gently press it up until the curve fits upon the head. When the end with its bunch of silk passes over the greatest diameter of the child's head, the resistance gives way, and it becomes free.

In this process there is no danger of injuring either mother or child, if done with moderate prudence, as the end of the instrument is protected by the bunch of silk band drawn into the rings. Next the threads that hold the band thus, are loosened, and the slide taken off. One handle is now grasped and gently carried around the head from its fellow to the opposite side. As the finger goes round, it pays out the band at the very place desired. Care must be had that none of the straps are pulled at this stage of the process, lest they draw the band too far down. The other handle is next grasped, and carried in the opposite direction until it meets its fellow. During this part of the process, two points should be attended

to, to facilitate the application. First, to relieve compression between the head and any part of the pelvic walls, the former may be pressed upward. And when the instrument is passing over the occiput, if passed far up, the end of it may strike against the child's neck, and make an obstruction. If this occurs, slightly withdrawing the handle will obviate the difficulty.

When brought together, the slide is to be placed upon the handles again, and the straps adjusted with the fingers, so as to be about equidistant from each other, gathered together and passed through the staple upon the sliding ring, and drawn down tightly. Thus applied, the slide on the handles holds the fingers of the instrument parallel to each other, and fixes firmly the ends near together so that the band cannot give or slip off the head.

The instrument thus applied, is ready for the application of extractive force, which may be made as occasion requires, either directly, obliquely downward, upward, or to either side, by pulling more forcibly the opposite straps. If the head needs its position altered, it gives us the best possible power for accomplishing it.

Although I have had but limited experience in the use of the *Extractor*, so far it has proved itself to be all that I could have expected. My greatest fear, before using it, was that it would be difficult of application, and especially in passing the fingers from one side of the head to the other; but in this, even when the head has been pressed down tightly in the pelvis, I have met with comparatively little difficulty, as the whole process of application required but a minute, and scarcely caused an extra pang of suffering to the patient. But as the test of experience can scarcely be claimed for the *Extractor*, we will look at it philosophically, and see what indications it will probably fulfil.

As it is capable of being extended beyond a right line, it can be passed round the bulge of the head at any point in the maternal passages, and from its small size, flexibility, and smoothness, with great facility.

These traits will also enable the operator to apply it even before the os uteri is dilated larger than the size of a dollar. The bunch of silk braid at the extreme end of the instrument, made by the band being drawn into the rings, makes a perfect protection from its being pushed against the uterus or child in a way to inflict injury.

The parts applied to the head, being a kind of silken net work, are in no danger of injuring the child.

The application of force involves but little compression of the head, which is left free to be moulded to the shape of the passage.

The straps being so soft and yielding, and their adaptation to the head being so close, there is no danger of contusion or laceration resulting from them. Even when the os uteri is but slightly dilated the straps would be in no danger of injuring it.

The head is so completely grasped by the instrument, that we can apply our force so as to make but little pressure on the mother's parts, in advance of the head.

There is no precise place on the head where the instrument must be applied, as there is no danger of its injuring any part. The small size of the instrument (its weight entire is 8 ounces) renders it convenient; its simplicity understandable and safe, and its cheapness commends it espe-

cially to those who have not been able to procure the forceps, on account of their price. The extractor will probably not cost over 3 or 4 dollars when its manufacture is systemized.

The cases to which it is applicable are:

1.—All of those in which the forceps are now recommended, excepting when the head may be so firmly locked between opposite points of the pelvis that it cannot be moved by compressing it upward.

2.—All labors protracted in the second stage, where it is possible to deliver with safety to the child. Being applied with little pain or danger there can be no excuse for allowing a patient to suffer the agonizing throes of labor, hour after hour, without progress, as is done in almost all such cases now, should the physician have the Extractor at hand.

3.—In labors obstructed at the superior strait of the pelvis, it will be especially applicable, for the higher up the head is at the time, the easier will be the application of the instrument. This will be manifest if we consider how readily it can be passed up by the side of the head when the fingers are extended, and that the only obstacle likely to interfere with its application is in passing the fingers from one side to the other, of the head. The higher the head is up, the more loosely it floats, and of course, the less resistance of this kind will be offered.

4.—Cases requiring speedy delivery at any time, either before labor has commenced, (for the os uteri toward the full time is always dilatible enough to allow of its application,) or during its progress, as is sometimes desirable in convulsions, hemorrhage, and the induction of premature labor, to prevent craniotomy.

By applying the Extractor, immediately after the separation of the placenta, in placenta previa, as recommended by Prof. Simpson, probably the head may be kept so compressed against the os uteri as to prevent fatal hemorrhage, and possibly, sometimes deliver so speedily as to save the child.

In cases of prolapsus of the funis umbilicalis, the fold may be completely returned by placing it between the ends of the fingers of the Extractor, when it will rest upon the bunch of silk band as prepared for introduction, by which it can be carried entirely above the head and held there until the head is delivered.

The difficulty of describing an instrument that is entirely unique, may prevent me from making myself understood without exhibiting it, but I think no one will fail to see its plausibility, upon an examination of the Extractor itself.

That it will fill most of the indications pointed out, I have no doubt, but extensive experience must be the test of the range of its usefulness.

Chicago, April 15, 1850.

Case of Recovery from the passage of an iron bar through the head—

Our readers will recollect the account of the passage of an iron bar through the head, and the recovery of the patient, which was copied into this Journal several months ago. In the *American Journal of the Medical Sciences*, for July, 1850, we find a communication on the case from the

pen of Henry J. Bigelow, M. D., Prof. of Surgery in Harvard University, which we copy, excluding some letters and affidavits relative to the circumstances attending and following the accident, together with the notes of the daily symptoms of the case.—EDITOR BUFFALO MEDICAL JOURNAL.

The following case, perhaps unparalleled in the annals of surgery, and of which some interesting details have already been published, occurred in the practice of Dr. J. M. Harlow, of Cavendish, Vermont. Having received a verbal account of the accident, a few days after its occurrence, from a medical gentleman who had examined the patient, I thus became incidentally interested in it; and having since had an opportunity, through the politeness of Dr. Harlow, of observing the patient, who remained in Boston a number of weeks under my charge, I have been able to satisfy myself of the occurrence and extent of the injury, as of the manner of its infliction. I am also indebted to the same gentleman for procuring at my request the testimony of a number of persons who were cognizant of the accident or its sequel.

Those who are skeptical in admitting the co-existence of a lesion so grave, with an inconsiderable degree of disturbance of function, will be interested in further details connected with the case; while it is due to science that a more complete record should be made of so remarkable an injury.

The accident occurred on the line of the Rutland and Burlington Railroad, on the 13th of September, 1848. The subject of it, Phineas P. Gage, is of middle stature, twenty-five years of age, shrewd and intelligent. According to this statement, he was charging with powder a hole drilled in a rock, for the purpose of blasting. It appears that it is customary in filling the hole to cover the powder with sand. In this case, the charge having been adjusted, Mr. Gage directed his assistant to pour in the sand; and at the interval of a few seconds, his head being averted, and supposing the sand to have been properly placed, he dropped the head of the iron as usual upon the charge to consolidate or "*tamp it in.*" The assistant had failed to obey the order, and the iron striking fire upon the rock, the uncovered powder was ignited and the explosion took place. Mr. Gage was at this time standing above the hole, leaning forward, with his face slightly averted; and the bar of iron was projected directly upwards in a line of its axis, passing completely through his head and high into the air. The wound thus received, and which is more fully described in the sequel, was oblique, traversing the cranium in a straight line from the angle of the lower jaw on one side to the centre of the frontal bone above, near the sagittal suture, where the missile emerged; and the iron thus forcibly thrown into the air was picked up at a distance of some rods from the patient, smeared with brains and blood.

From this extraordinary lesion, the patient has quite recovered in his faculties of body and mind, with the loss only of the sight of the injured eye.

The iron which thus traversed the skull weighs thirteen and a quarter pounds. It is three feet seven inches in length, and one and a quarter inches in diameter. The end which entered first is pointed; the taper being seven inches long, and the diameter of the point one quarter of an inch; circumstances to which the patient perhaps owes his life. The iron is un-

like any other, and was made by a neighboring blacksmith to please the fancy of the owner.

Dr. Harlow, in the graphic account above alluded to, states that "immediately after the explosion the patient was thrown on his back, and gave a few convulsive motions of the extremities, but spoke in a few moments. His men (with whom he was a great favorite) took him in their arms and carried him to the road, only a few rods distant, and sat him into an ox cart, in which he road, sitting erect, full three quarters of a mile, to the hotel of Mr. Joseph Adams, in this village. He got out of the cart himself, and with a little assistance walked up a long flight of stairs, into the hall, where he was dressed."

The leading feature of the case is its improbability. A physician who holds in his hand a crowbar, three feet and a half long, and more than thirteen pounds in weight, will not believe that it has been driven with a crash through the brain of a man who is still able to walk off, talking with composure and equanimity of the hole in his head. This is the sort of accident that happens in the pantomime at the theatre, but not elsewhere. Yet there is every reason for supposing it in this case literally true. Being at first wholly skeptical, I have been personally convinced; and this has been the experience of many medical gentlemen who, having first heard of the circumstances, have had subsequent opportunity to examine the evidence.

This evidence is comprised in the testimony of individuals, and in the anatomical and physiological character of the lesion itself.

The above accounts from different individuals, concur in assigning to the accident a common cause. They are selected as the most complete among about a dozen of similar documents forwarded to me by Dr. Harlow, who was kind enough to procure them at my request; and which bear the signature of many respectable persons in and about the town of Cavendish, and all corroborative of the circumstances as here detailed. The accident occurred in open day, in a quarry in which a considerable number of men were at work, many of whom were witnesses of it, and all of whom were attracted by it. Suffice it to say, that in a thickly populated country neighborhood, to which the facts were matter of daily discussion at the time of their occurrence, there is no difference of belief, nor has there been any doubt that the iron was actually driven through the brain. A considerable number of medical gentlemen also visited the case at various times to satisfy their incredulity.

Assuming the point that the wound was the result of a missile projected from below upwards, it may be asked whether the wound might not have been made by a stone, while the bar was at the same moment thrown into the air. It may be replied in answer, that the rock was not split, nor, as far as could be learned, disintegrated. Besides, an angular bit of stone, would have been likely to have produced quite as much laceration as the bar of iron; and it is in fact possible that the tapering point of the latter divided and repelled the soft parts, especially the brain, in a way that enabled the smooth surface of the iron to glide through with less injury. And assuming the only possible hypothesis, that the round bar followed exactly the direction of its axis, the missile may be considered as a sphere of one and a quarter inches diameter, preceded by a conical and polished wedge.

The patient visited Boston in January, 1850, and remained some time

under my observation, during which he was presented to a meeting of the Boston Society for Medical Improvement, and also to the medical class at the hospital. His head, now perfectly healed, exhibits the following appearances.

A linear cicatrix of an inch in length occupies the left ramus of the jaw near its angle. A little thickening of the soft tissues is discovered about the corresponding malar bone. The eyelid of this side is shut, and the patient unable to open it. The eye considerably more prominent than the other, offers a singular confirmation of the points illustrated by the prepared skull described below. It will there be seen that the parts of the orbit necessarily cut away are those occupied by the levator palpebræ superioris, the levator oculi, and the the abducens muscles. In addition to a ptosis of the lid, the eye is found incapable of executing either the outward or upward motion; while the other muscles animated by the motor communis are unimpaired. Upon the head, and covered by hair, is a large unequal depression and elevation. A portrait of the shaved head is given in the plate; and it will be seen that a piece of cranium about the size of the palm of the hand, the posterior border lying near the coronal suture, its anterior edge low upon the forehead, was raised upon the latter as a hinge to allow the egress of the bar; and that it still remains raised and prominent. Behind it is an irregular and deep sulcus several inches in length, beneath which the pulsations of the brain can be perceived.

In order to ascertain how far it might be possible for this bar of an inch and a quarter diameter to traverse the skull in the track assigned to it, I procured a common skull, in which the zygomatic arches are barely visible from above; and having entered a drill near the left angle of the lower jaw, passed obliquely upwards to the median line of the cranium just in front of the junction of the sagittal and coronal sutures. This aperture was then enlarged until it allowed the passage of the bar in question, and the loss of substance strikingly corresponds with the lesion said to have been received by the patient. From the coronoid process of the lower jaw is removed a fragment measuring about three-fourths of an inch in length. This fragment in the patient's case might have been fractured and subsequently reunited.

The hole now enters obliquely beneath the zygomatic arch, encroaching equally upon all its walls. In fact, it entirely occupies this cavity; the posterior wall of the antrum being partially excavated at the front of the hole, the whole orbital portion of the sphenoid bone being removed behind, as also the anterior part of the squamous portion of the bone, and the internal surface of the zygoma and malar bone laterally. In the orbit, the sphenoid bone, part of the superior maxillary below, and a large portion of the frontal above, are cut away, and with these fragments much of the speno-maxillary fissure; leaving, however, the optic foramen intact about a quarter of an inch to the inside of the track of the bar.

The base of the skull upon the inside of the cranium presents a cylindrical hole of an inch and a quarter diameter, and such as may be described by a pair of compasses, one leg of which is placed upon the lesser wing of the sphenoid bone at an eighth of an inch from its extremity, cutting the frontal, temporal and sphenoid bones; the other, half an inch outside the internal optic foramen.

The calvaria is traversed by a hole, two-thirds of which is upon the left,

and one-third upon the right of the median line, its posterior body being quite near the coronal suture. The iron freely traverses the oblique hole thus described.

It is obvious that a considerable portion of the brain must have been carried away; that while a portion of its lateral substance may have remained intact, the whole central part of the anterior lobe, and the front of the sphenoidal or middle lobe must have been lacerated and destroyed.— This loss of substance would also lay open the anterior extremity of the left lateral ventricle; and the iron, in emerging from above must have impinged upon the right cerebral lobe, lacerating the falx and the longitudinal sinus. Yet the optic nerve remained unbroken in the narrow interval between the inner wall of the orbit. The eye, forcibly thrust forward at the moment of the passage, might again have receded into its socket, from which it was again somewhat protruded during the subsequent inflammation.

It is fair to suppose that the polished conical extremity of the iron which first entered the cavity of the cranium prepared the passage for the thick cylindrical bar which followed; and that the point, in reaching and largely breaking open the vault of the cranium, afforded an ample egress for the cerebral substance, thus preventing compression of the remainder.

Yet it is difficult to admit that the aperture could have been thus violently forced through without a certain comminution of the base of the cranium driven inwards upon the cerebral cavity.

Little need be said of the physiological possibility of this history. It is well known that considerable portions of the brain has been in some cases abstracted without impairing its functions. Atrophy of an entire cerebral hemisphere has also been recorded.

But the remarkable features of the present case lie not only in the loss of cerebral substance, but also the singular chance which exempted the brain from either concussion or compression; which guided the enormous missile exactly in the direction of its axis, and which averted the dangers of subsequent inflammation. An entire lung is often disabled by disease; but I believe that there is no parallel to the case in the Hunterian collection of a lung and thorax violently transfixed by the shaft of a carriage.

Taking all the circumstances into consideration, it may be doubted whether the present is not the most remarkable history of injury to the brain which has been recorded.*

* The iron bar has been deposited in the museum of the Massachusetts Medical College, where it may be seen, together with a cast of the patient's head.

Case of premature labor complicated with prolapsus of the uterus. By SAMUEL TYLER, M. D., of Fredericks, Md.

To S. S. PURPLE, M. D., Editor of the N. Y. Journal of Medicine.

Dear Sir:—The following case may not be without some interest to your readers, as it fully establishes the fact of the power of the muscular fibres of the uterus, and its ability to expel the foetus or other contents, independently of assistance from any other source whatever. I am aware, of course, that at present there are few, if any, who deny its muscularity, if I be allowed the term, but it has not been many years since a distinguished

teacher of obstetrics, in this State,* denied entirely the fact, and maintained that labor was effected by the agency of the muscular parietes of the abdomen.

On the 19th of March, 1846, at 2 o'clock, P. M., I was called to Mrs. M. I found with her a female midwife, who stated that the patient had had slight labor pains for several hours, and that there was a mass of fleshy substance between her thighs, which had "come down" early in the morning, when the patient was engaged in some work about the house. Upon examination, by the touch and by ocular inspection, I found, to my surprise, that it was the uterus entirely without the vagina, and containing a five months' foetus; I could distinctly handle the lateral ligaments. To return it was impossible—the pains had somewhat subsided under the influence of opium, which had been administered by the midwife. Her pulse being full and active, I took sixteen ounces of blood from her arm, and administered twenty grains of powdered secale cornutum; in about twenty minutes the longitudinal fibres commenced contracting firmly, producing dilatation of the "os uteri;" they would cease, and the circular fibres would commence, producing an appearance of hour-glass contraction; the contraction of each lasting about a minute, by the watch. After the second contraction of the circular fibres, the womb was perfectly quiescent for about five minutes; when, as it were, by a concerted action of both sets of fibres, the foetus, a male, was expelled and received in my hand, the secundines following in about ten minutes. I administered an anodyne, and requested that she be kept perfectly still until ten o'clock, P. M., when I visited her again, and was enabled to return the womb some distance up the vagina; administered another anodyne, and saw her at nine o'clock, A. M., the next day; she had rested well, and I found the womb nearly "in situ;" she kept in a pretty comfortable condition for four days, when puerperal fever ensued, with which she was ill about three weeks, but recovered perfectly, and has since been delivered of a fine child. I should have remarked that she had had four children previous, and is a woman of very delicate frame. I made the above notes of this case at the time, but my professional engagements have prevented, or rather caused me to neglect its publication until now.

REMARKS BY THE EDITOR.

The foregoing is the history of an exceedingly interesting case. Our home periodical literature, so far as our reading extends, records the particulars of two similar cases. In the first volume of the Medical Repository, for 1797, Dr. Thomas Archer, of Hartford-Town, Md., records the particulars of "A singular case of Difficult Parturition successfully treated." The following is a condensed account of the case. The patient, a Mrs. E., aged 30, had been in labor four days with her first child. Her pains had been considerably forcing, but the intervals between them long, the waters had been gradually discharging for two days. The os uteri was not dilated to more than the size of a cent, and it formed a thick, rigid cartilaginous ring, not yielding or becoming softened by the pain. Her constitution was robust and strong, and her pulse was marked by tension or convulsive action. She was bled to eighteen ounces, and gentle laxatives, with emollient clysters, were ordered; oleaginous injections into the

* Richard W. Hall, of the University of Maryland.

vagina, and the vapor of hot water were advised, as means for relaxing the os-tinca. She took a dose of opium and stramonium, to procure rest and remove unprofitable pains. She was then left in the charge of a midwife until the following day in the evening.

At this time there was no perceptible alteration in the dilatation of the os uteri. The child's head could now with difficulty be felt through the os uteri, and there was no increase in the dilatation. A few pains protruded the uterus with its contents, without the os externum! The child, from certain evidences, was believed to be dead, and great fears were entertained for the patient's safety. Death appeared as the closing scene of every plan I proposed; in fact, her situation required such immediate assistance that there was scarcely a moment for deliberate thought. By means of a candle held by the midwife, without the knowledge of any other of the attendants, with a spear pointed lancet I made three incisions in the neck of the womb, each about two inches in length; viz., one leading towards the urethra, one towards the perineum, and the other towards the left labium. The pains, although not strong, immediately expelled the child. The uterus, after the secundines separated and came away, with no difficulty, contracted and returned, with but little assistance, to its pristine situation. Her recovery was as easy as is the case in any natural labor; she was up and walking about the room in three weeks after her delivery. At the age of fifteen she had a prolapsus uteri, which was reduced after it was washed in a strong decoction of oak bark and dusted with powdered resin.

The second case referred to will be found reported by Dr. Gardner, of this city, in volume twelve, new series, of the American Journal of Sciences for 1846. The patient, a Mrs. Potter, aged 35, was in labor at the full time with her fourth child. After her last confinement she had prolapsus uteri. Within the last four months of her present gestation, the uterus has descended so much, that from three to four inches have been always extruded beyond the vulva. In this condition the parts were found about twelve hours after the commencement of labor. The patient now began to show symptoms of exhaustion from the continuance of pains which did not allow a moment's rest, and various other symptoms began to show themselves indicating convulsions. Delay was considered no longer justifiable; the neck of the uterus was still unobliterated, and the os uteri was dilated to only a moderate degree. The vertex of the child lay in the hollow of the sacrum—the presentation was the third of Baude-locque. The time for active interference having arrived, the short forceps were applied—not however without much trouble, owing to the difficulty of inserting the blades through the narrow orifice of the uterus. In conjunction with a stronger pair, and traction, the head was dislodged and came down, bringing with it the os uteri. Great difficulty was now experienced in withdrawing the forceps. The uterus presented an external mass eight inches in length and five in diameter. The os was two and a half inches in diameter. By means of a bistoury an incision was made two and a half inches in length, soon after which a renewal of pain resulted in the expulsion of a live female child, not, however, without tearing up the incision an inch and a half more. The afterbirth was delivered without difficulty or hæmorrhage; the uterus remained external but contracted down to one-half its previous size. Twelve days after delivery, the uterus

having diminished much in size, was returned with but little difficulty. She had a slow and protracted recovery, followed by prolapsus of the uterus.

Such is the history of two cases of a rare and unpleasant complication of labor. Various writers on obstetrics have made mention in their works of this complication. Among others, Smellie, Blundall, and Meigs, may be particularly mentioned.—*N. Y. Journal of Medicine*.

Practical views of Medical Education.—The undecided state of public opinion in regard to some of the fundamental points in a course of medical education, including among other things the portion of the term of pupilage proper to be spent in attendance on lectures, is thought by the undersigned, to justify a further consideration of the subject. In some of its relations, this subject has been already discussed, in the Transactions of the American Medical Association for 1849, in two reports, pages 353 and and 359, to which the reader is particularly referred. The following condensed, but more general view of the subject of medical education, is now respectfully submitted to the members of the Association.

Boston, July 10, 1850.

1. Medical instruction should be adapted to the power of students to receive and retain what is communicated to them, and should be confined to what is important to them in subsequent life.

2. In modern times the constituent branches of medical science are so expanded, that they are not acquired by any physician in a life-time, and still less by a student during his pupilage. The same is true even of many individual branches. It is not, therefore, to be conceded that "a scheme of scientific instruction should embrace the whole science, and no part should be omitted;" nor that "a well-digested plan of lectures embraces all that is to be known and taught." Medical science has at this day become so unwieldy, and contains so much that is unnecessary, at least to beginners, that the attempt to explain to students the whole, is likely to involve the result of their learning but little.

3. In Chemistry, at the present time, a thorough adept is unknown. No man living knows all the recorded facts, or all that is to be known and taught in that science. Organic chemistry alone fills large volumes, though yet in its infancy.

4. In *Materia Medica* there are some thousands of substances and their compounds, which possess what is called a medicinal power. Yet it is not probable that any physician effectively reads the one half, or remembers one quarter, or employs in his yearly practice one tenth, of the contents of the common dispensaries.

5. In Pathology, so complicated and various are the conditions attendant on the individual forms of disease, and their relations with idiosyncrasy, temporary condition and external agency, with organic lesions and functional disturbances, that few of the most experienced pathologists can be said to understand their whole science, or to be always competent to its successful application.

6. In Etiology, the theoretical literature of causes has spread itself out to an extent, which is burdensome and unprofitable. It is true, that "man, from his nature, is subject to suffering disease and death;"—but it

is not equally apparent, that "the causes by which these conditions are produced, are ascertainable." We know nothing of the vehicle of cholera or influenza, nor is it probably in the power of any physician, by any art, or application of his knowledge, to produce in any common healthy man, a case of common pneumonia, or of acute rheumatism,—of diabetes or Bright's kidney,—of hypertrophy or of cancer,—or even of a common boil, or wart.

7. In Therapeutics, many hundred volumes exist, such as would not have existed, could a knowledge of the cure of diseases be made so easily tangible, that it could be spread before the student in the three or five years of his pupilage.

8. In Anatomy, general and special, microscopic and transcendental;—in Physiology, with its intricate ramifications;—in Surgery, of which several subordinate specialities constitute distinct living professions; it is not to be admitted that the means or time of any ordinary course of lectures, can furnish full and complete instruction. Certainly it must be difficult to arrange a course of lectures on any of the extensive sciences which now constitute medicine, if it be indeed true, that "the teachers are not justifiable in suppressing any portion."

9. It is the business of lecturers in medical schools, to condense and abridge the science which they respectively teach, to distinguish their essential and elementary principles, to sift carefully the useful from the superfluous, and to confine the scope of their teachings, as far as possible, to what is true and profitable, and likely to be remembered and used by their hearers. It is unfortunately too true, that "in an extended system of instruction, there is much the student will not master, much that will have escaped his attention, much which he ought to know, that he has not learned." The remedy appears to be, to teach him well what he can and should master, and briefly to point out to him the sources, fortunately abundant, from which he may obtain the rest.

10. Much injury is done to the cause of true learning by medical assumption, amplification and exaggeration, by premature adoption of novelties, and tenacity of theories, personal or espoused. Students, in all former years, have spent much time in learning, what has afterwards cost them both time and trouble to unlearn;—in acquiring, not merely the truths of science, but the crude announcements and plausible doctrines of sanguine or ingenious men. How much time has been wasted in our distinguished seminaries, in acquiring, the visionary, and now neglected, theories of Rush and Broussais!

11. The most commonly exaggerated branch of medical science is therapeutics. Enlightened physicians well know, that many diseases are incurable, and that others are subject to laws of duration, which cannot be interrupted by art. Yet students often return from medical schools persuaded that their instructors know how to cure a large part of these diseases, and if others are less fortunate it is attributable to their own fault.

12. Medical teachers should keep pace with the progress of their respective sciences. Yet in their haste for the promulgation of novelties, they should not omit to give the proper consideration to the older and more settled principles of science. Medical men are liable to commit the error of adopting premature opinions, unsound practice and inconvenient changes of language and nomenclature, some times for a love of display,

and sometimes for a want of self reliance, and a fear of being thought behind the literature of their time.

13. The length of a course of lectures is not the measure of its value to the student. A course of lectures should not outlast the curiosity of its hearers, nor their average pecuniary ability to attend. Custom in this country has generally fixed the limit of these things at about four months. A comprehensive and judicious course, confined to the enforcing of necessary points, is far more profitable than a more discursive course to a wearied and diminishing audience.

14. Lectures are chiefly wanted to impress by demonstration the practical branches of science, and they are most effective in places where the facilities for such demonstrations can be commanded. Anatomy requires extensive exhibitions by the teacher, and personal dissections by the student. Chemistry and *Materia Medica* require illustrations by specimens and experiments. Pathology needs the aid of autopsies, museums and the clinical demonstrations of large hospitals. A knowledge of Obstetrics is not perfected without apparatus and practice.* Surgery is acquired by witnessing numerous operations, surgical diseases, illustrated explanations, and by personal practice on the dead body. Physical exploration is wholly demonstrative. A knowledge of auscultation can no more be acquired from books, or abstract lectures, than a knowledge of music, or of individual physiognomy.

15. The intermediate period between lectures, should be spent by students in active and original study, approved and confirmed by regular recitations, and by such opportunities as can be commanded, for practical, personal experience. Private schools for small classes, and the private teachings of individuals, who are suitably qualified and situated, are more advantageous for two-thirds of the year, than either the fatiguing jostle of overcrowded rooms, or the listless routine kept up by the survivors of a passive class.

16. The usefulness of a medical school depends not so much on the length of its session, as upon the amount of education, preliminary and ultimate, which it requires, the fidelity with which it exacts its own professed requisitions, and the train of healthy exertion, active inquiry, and rigid, methodical, self-regulating study, to which it introduces its pupils. The longest lectures are of little use to students who want a common education, and whose medical education does not qualify them afterwards to observe, to inquire, and to discriminate. The exacted evidence of three years of well conducted study, is better than the exhibited ticket of a six months' course.

17. The subjects most important to be well taught in medical schools, are the elementary principles which constitute the frame-work of medical sciences, and the mode of thought and inquiry which leads to just reasoning upon them. After these, most attention should be given to selecting and enforcing such practical truths, as will most certainly be wanted by the young practitioner in his future career of responsibility.

18. The things to be avoided by medical teachers, are technicalities which are unintelligible to beginners,—gratuitous assumptions and citations

* To which we would add demonstrations with the living subject.—EDITOR BUFFALO MEDICAL JOURNAL.

of doubtful authorities,—prolix dissertations on speculative topics,—excessive minuteness in regard to subjects which are intricate and but little used, and therefore destined to be speedily forgotten. To these may be added controversies, superfluous personal eulogiums and criminations, and all self-exaggeration, personal or local.

JACOB BIGELOW, *Prof of Materia Medica and Clinical Medicine.*

WALTER CHANNING, *Prof of Midwifery and Med. Jurisprudence.*

JOHN WARE, *Prof. of Theory and Practice of Medicine.*

JOHN B. S. JACKSON, *Prof of Pathological Anatomy.*

OLIVER W. HOLMES, *Prof. of Anatomy and Physiology.*

HENRY J. BIGELOW, *Prof. of Surgery.*

E. N. HORSFORD, *Prof. of Chemistry.*

EDITORIAL DEPARTMENT.

Demonstrative Midwifery. Report of the Trial: The People versus Dr. Horatio N. Loomis for Libel. Tried at the Erie County Oyer and Terminer, June 24, 1850. Justice Mullett, Presiding. John Treanor, and Leander J. Roberts, Associate Justices. Reported by Jesse Walker, Esq., assisted by Mr. Frederick T. Parsons, Stenographer. Buffalo: Steam Press of Jewett, Thomas & Co. 1850.

It was our intention, as intimated in our last No., to have copied from the published report of the trial of Dr. Loomis, that portion of the testimony developing the facts connected with the demonstration of labor at the Buffalo Medical College during the last session, and also the testimony of the medical witnesses summoned on the part of the people, and for the defence. In order, however, that this subject may not encroach too much upon space which many might desire should be devoted to other matters, we have arranged for having a copy of the report sent to each of the subscribers to the Buffalo Medical Journal. This will render it unnecessary to carry out the plan we had intended.

We also announced a design of reviewing the facts, and the testimony, evolved by the trial referred to, discussing the validity of the objections made to demonstrative teaching in midwifery, and, at the same time, examining its advantages. But, we confess, we are somewhat at a loss to determine in how far it is advisable to carry the latter resolution into effect. Our indecision springs from the following considerations: The facts and the testimony appear to us to speak sufficiently for themselves, as contained in the published report of the trial. Then, as regards the merits of the subject incidentally connected with the trial, to wit, *demonstrative midwifery*, it really seems to us that they hardly call for labored argumentation, but that they at once suggest and commend themselves. Moreover, we

have every reason to conclude that with nine-tenths of the medical profession the claims of demonstrative teaching in midwifery require neither exposition nor advocacy. If we are in error on this point we are ready to be corrected, and to acknowledge that we have erred. But the subject has now been before the medical public several months, and, as yet, no one, that we are aware of, has come forward to sustain the assertion that demonstrative teaching in midwifery is *unprofessional,* wholly unnecessary, immoral, and indecent.* Of some fifteen medical Journals of the United States, at least ten have already contained decided expressions of approbation of this method of teaching; and this has not called out from the ranks of the profession, as yet, even a solitary knight to do battle in behalf of those who think it should not be practiced "*in any civilized community!*"† None of those who thought it their duty to administer a "*severe rebuke,*"‡ appear to think the attitude they have assumed worth contending for. We have invited communications on the subject from those who oppose, as well as those who approve of demonstrative midwifery, but, to our regret, the discussion is left to ourselves alone. We say "to our regret"—we have no fondness for controversy, but, under present circumstances, we are not reluctant to discuss this subject in any of its aspects or bearings; and we persuade ourselves that we are able to treat it with candor, notwithstanding, in connection therewith, as a member of the Faculty of the Medical College of Buffalo, we stand charged by seventeen of our professional neighbors with having participated in an immoral and indecent act. In view of this serious accusation, we claim that what we have written on this subject has been temperate, to say the least; nor have we any disposition to recriminate by inquiring into motives, so long as there seems no necessity for resorting to a method of self-protection so uncongenial.

In view of the circumstances just stated, we repeat, we are at a loss to know what degree of consideration we should give to the subject at this time, wishing to avoid, on the one hand, needlessly taxing the patience of our readers, and, on the other hand, resolved to be completely relieved of imputations which we cannot but regard as alike uncalled for, unjust, and ungenerous. In this state of uncertainty we purpose to pursue a middle course, noticing the facts and testimony briefly, and commenting on a few only of the points which a full discussion of the subject would involve.

* See letter of seventeen physicians of Buffalo, in the March No. of this Journal, 1850.

† See *ibid.*

‡ *Ibid.*

First, as to the *facts*. If the reader will peruse carefully the testimony of those of the witnesses who were present at the demonstration, he will find that the details, in so far as we have professed to state of them in our previous notices, are fully substantiated. On one point we have hitherto withheld entering into a specific statement. This is the degree of exposure. We have avoided speaking of that minutely, in the first place, because, in so far as our own opinion is concerned, we hold it to be a point of very little consequence. We wish it to be distinctly understood, that we advocate Demonstrative Midwifery on the ground of its intrinsic propriety and utility, and that any exposure which is useful, is, in our view, perfectly proper. But, since so much importance appears to be attached to this point by those who claim the prerogative of administering a rebuke, we preferred to delay for the evidence given under oath. And what do the witnesses testify on this point? *They all of them swear that they did not see the genitals of the patient, which were protected from view by the cautious management of the Professor of obstetrics.* We refer to the report of the trial for proof of the correctness of this assertion. The exposure was limited to the head of the child, and a small portion, perhaps, of the *nates*, or hips of the woman. We do not adduce this fact as one for which the Professor in charge of the demonstration is entitled to credit. We think with our esteemed colleague Prof. Lee, (see his testimony,) that enough was not exhibited; the demonstration was, in our opinion, defective in this respect. And we take occasion to say, that when the matter was submitted to the members of the Faculty of the College, it was expected that the exposure would be more complete. We shall not complain of the over prudence of the Professor of obstetrics, but we think the demonstration is susceptible of an improvement by which the students would have a fuller opportunity to witness the changes which the perineum undergoes in the process of the delivery.

Another highly interesting fact appears in the trial, to which we have not before alluded. It seems that at the stethoscopic examinations, several days prior to the labor, the Professor of obstetrics had predicted, from the situation of the fetal sounds, an unusual position of the fetus, viz., an anterior direction of the face. The correctness of this opinion was verified at the labor; and the fact that the prediction had been publicly pronounced, furnished an inducement to exhibit to the class the passage of the head through the vulva, that they might the better realize the accuracy of the deduction from the auscultatory phenomena. Assuming, for the moment, that, in ordinary cases, ocular demonstrations in

midwifery are neither useful nor proper for purposes of teaching, a generous mind, it would seem, could overlook an exposure so guarded as in this instance, and with the full consent of the patient, from sympathy with the desire of a teacher to test the precision of diagnosis, even were it but to indulge a little pride of professional acumen; but, still more, when, at the same time, it would serve to impress most vividly upon the mind of the student a sense of the reality and practical utility of the art of obstetrical auscultation. Let us not be understood to offer this as an apology for the demonstration in this instance. In no point of view do we concede, for an instant, that a demonstration of labor, properly conducted, for purposes of instruction, requires a word of apology.

We pass now to the medical testimony relative to the utility and propriety of Demonstrative Midwifery. On the part of the Defence, fifteen medical witnesses were examined, who testified that, in their opinions, demonstrative teaching of midwifery is *unnecessary* and *improper*. The reader, by comparing the names of these witnesses with those attached to the letter contained in the March No. of this Journal, will perceive that all but three of the former are enrolled among the latter. Now, as a commentary on the merits of the subject, we ask the reader to peruse carefully the direct and cross examinations of these witnesses. Each and all are persuaded that nothing is gained for the pupil by an ocular exhibition of the visible stage of labor. Yet there is no one of them that hesitates to testify to the great utility of drawings, pictures, and models, designed to illustrate this stage. Most valuable information can be derived by the *representation* of living parts, but they can perceive no advantages in seeing *the living parts themselves*. Not one of them recognizes the least indelicacy in studying the processes of labor in so far as they can be delineated on paper, or canvass, or imitated by wax, *papier mache*, and buckskin, and the more natural and life-like these resemblances are, of course, the better; but to exhibit the same parts and processes as nature herself displays them, is useless and improper! It is well to study by the reflected rays of the *moon*, but quite superfluous to avail ourselves of the light of the *sun*! The *shadow* cannot be dispensed with, but the *substance* is of no avail! We should be fearful of being suspected of exaggeration, if the reader were not supposed to have the report of the trial before him. He will find on reading the testimony, that the metaphors we have just used are scarcely hyperbolic. Several swear that plates and drawings are quite as useful for instruction as the real objects which they represent; one, at least, thinks they are even *better* than nature, and one witness testifies that com-

parative anatomy may furnish as useful demonstrations as the living subject! This is certainly an idea which is entitled to the merit of originality. Imagine the Faculty of the Medical College to have acted upon such a suggestion, and procured for the class an opportunity to witness the parturition, for example, of a bovine quadruped! But, seriously, does the projector of this new method of teaching obstetrics really think any useful ends are to be gained by it? then, plainly, he concedes all that is claimed for demonstrative midwifery, unless he contends that an inferior animal affords a *better* illustration of labor in the human subject, than the human subject herself!

The reader will observe that much is said by the witnesses for the defence of the importance of cultivating the sense of touch. No one denies the importance of this, but will it be deemed otherwise than absurd that because the touch is to be informed, the sight is useless? Do we not better educate one sense by associating with it knowledge derived by other senses? It is true, when a sense is lost, some of the other senses are cultivated with greater assiduity, so as to compensate in some degree for the deficiency; but will it be gravely argued from this fact, that the different senses, more particularly the sight and touch, are not instrumental in educating each the others? According to this notion, blindness would be a good qualification for an accoucheur! But their own testimony disproves their notions, for all admit the utility of plates, drawings, and manikins, and is not all the information therefrom derived addressed exclusively to the eye!

And why this distinction among the senses as regards *impropriety* in their use? If there be indelicacy in *seeing*, is there not the same, or even more, in *touching*? If the possibility of improper emotions is to be entertained, (and God forbid that it should be for a moment,) is the sense of *feeling* less capable of administering thereto than that of *sight*? Yet, with a single exception, all these witnesses testify, not only to the usefulness, but to the *propriety* of permitting medical students to make vaginal examinations for purposes of instruction.

But demonstrative teaching of midwifery *shocks the moral sense of the community*. We do not admit this except for argument's sake. Were it so, we ask if the moral sense of the community is to be taken as the guide and umpire in questions relating to proposed improvements in medical instruction? Do we defer to the moral sense of the community in other measures: if so, why are not dissections legalized by the popular will? We say we do not admit that demonstrative teaching in midwifery does

shock the moral sense of the community. We believe, at this moment, in this city, where the subject has been agitated, that the predominance of public sentiment is in favor of it; and, yet, a community would hardly be expected to be in favor of a novel mode of medical teaching which a number of medical men residing in that community had united in publicly denouncing as unprofessional, wholly unnecessary, offensive to morality and common decency.

On behalf of the prosecution, seventeen witnesses were examined, who testified in favor of the utility and propriety of demonstrative midwifery. We do not propose to analyze the testimony of these witnesses; but in commending it to the reader's attention we will simply allude to two or three points. Of the witnesses examined on either side, in the trial of this case, several had been practically acquainted with demonstrations in midwifery, and several had not seen a single demonstration. Now of all those embraced in the first category, not one testified to the inutility of this method of teaching; and of those who testified to its being unnecessary and improper, not one had any practical acquaintance with it. This is worthy of mention as a significant fact.

Again, of the members of the graduating class, now practising physicians, who witnessed the demonstration, all who were examined on the part of the people, and by the defence, testified that it had been of service to them—that they had gained information, and were better prepared by it to enter on the practical duties of the accoucheur. Now they could hardly be mistaken on this point. They testified to that which they did *know*. And in matters of this kind, so far as the authority of medical opinion is concerned, their testimony is worth infinitely more than that of older members of the profession, who may be supposed to have forgotten, in part, the difficulties and embarrassments incident to their first practical efforts in midwifery. This, as it seems to us, is an obvious truth, and in no wise disparages the respect due to age and experience in matters of a different nature.

Again, we claim that medical teachers should be expected to understand the necessities and interests of medical pupils somewhat better than those who have had little or no practical experience in medical instruction. In saying this, we assume no more than that a man who devotes a good share of his time and attention to any pursuit, ought to judge best of matters relating to that pursuit, and his opinions are entitled to additional weight from that circumstance. In view of this consideration, the reader of the trial will doubtless attach especial importance to the testimony of Professor

Gilman, of the city of New York, teacher of obstetrics in the oldest, and, we may add, without invidiousness, the *first* medical institution of this State; and to that of Prof. Coventry, also an eminent, and, we may add, a veteran teacher in the same department of medical instruction. We mention these names because the subject under remark relates to a branch of medical science which it has been the business of their lives to study and expound.

On the part of the defence, it will be observed no person connected with a medical school, or who professed to be engaged in medical teaching, appeared to testify *against* the utility and propriety of demonstrative midwifery. The reader will deem it hardly probable that such witnesses would not have been summoned, were they to be found, especially when he observes the proposition by the defendant's counsel, to introduce several *matrons* or *midwives*, who had been subpoenaed, to testify as to the necessity of exposure in teaching obstetrics! (See page 17 of the report.) This was certainly a singular proposition, and highly complimentary to the professional opinions that had preceded. The court, however, refused to recognize them as *professional persons*, and, hence, we have not the advantage of their testimony.

The advantages of demonstrative teaching in midwifery are not developed in the testimony so fully as to do adequate justice to them, the matter at issue in the trial, i. e. the libel, having little to do with an elaborate exposition of all the merits of the subject. What was not elicited by the course of examination pursued, however, the reflections of the reader will doubtless supply. It is only necessary to go back and review one's primary experience in obstetrics to appreciate the need of clinical and demonstrative instruction. The retrospections of many of our readers will speak to their minds more forcibly on this point than any language we could use. The first case of labor, at the commencement of a professional career, is not only an occasion full of embarrassment and apprehension, but often an event of momentous importance to the young practitioner, if not to others. His theoretical knowledge is to be put to a practical test; the shrewd and suspicious eyes of patient, nurse, and neighbors, are watching all his movements; he may be asked if he has ever attended on a similar occasion, and shall he equivocate or falsify? More than this, the very first case may present unusual difficulties, which call, if not for experience, at least calmness and *self-possession*. Will any one deliberately deny that under any or all these circumstances, a person who has witnessed the details of one or more labors conducted by his instructor, will have more confidence,

and be more competent to discharge his duties to the patient, to satisfy the expectations of friends and attendants, and advance his professional success? But it may be said by some of those who have entered their protest against demonstrative midwifery, we do not object to any kind of clinical teaching which is conducted without exposure. They may perhaps approve of all that was done at the medical college, except the display of the head of the child, a portion of the nates and hips of the patient for the space of from two to five minutes, the room being illuminated with one or two tallow candles! This is the objectionable feature of the demonstration, which their outraged feelings compelled them to denounce, forgetting, at the same time, to signify their approbation of all the rest, and the credit due to the Professor of Obstetrics, and the Faculty, for having supplied unusual facilities for clinical instruction, irrespective of this alledged violation of propriety!

Had we time and space, we should dwell somewhat upon the advantages of this particular part of the demonstration; although we cannot but think it would be supererogatory, if the reader will consider that it is precisely at the stage of labor when the exhibition was made, i. e., when the head emerges from the vulva, that, in ordinary cases, the services of the accoucher are required, and these services have exclusive reference to what was exhibited in the passage of the child through the *os externum*. The perineal tumour, the dilatation which the perineum undergoes, the mode and direction in which it is to be supported, the curvilinear movement of the fetal head, its rotation while the body is passing through the pelvis, etc. — all these points are not only minutely and carefully described in books and lectures, but it is always expected will also be taught by *illustrations*. And will it seriously be said that the *living* subject does not present better illustrations than *drawings*, *pictures*, and a *buckskin manikin*? Will the latter produce in the mind of the pupil as correct an idea, and as vivid, forcible impression as the appearances and operations of nature herself!

There is another view of this subject, which, if this article were not already so extended, we should consider. In all our remarks hitherto, we have spoken of demonstrations in midwifery in connection with their utility in preparing for the practical duties of the medical practitioner. But we are prepared to take broader ground than this, and to say, that, in our opinion, they are not offensive either to morality or decency, irrespective of practical instruction. We do not hesitate to avow our belief not only that such demonstrations might be witnessed by the physiologist, the philoso-

pher, the moralist, and the christian without detriment to the moral or religious sense, albeit the observer might never intend to become a medical practitioner, but that even the thoughtless and licentious man would find in them nothing to foster base or unworthy sentiments. We challenge any respectable medical man to come forward and declare that the lying-in chamber is a place in which to harbor thoughts of lewdness or frivolity. Every physician and every reflecting man, medical or not, must know that it is calculated to have a far different moral influence. We pity him, if there be such an one, who can think otherwise, and still more, one who does not blush to give utterance to such a thought—a thought which is insulting to all that is compassionate and manly in human character. Nay, more, it is insulting to Divine Providence, for there is truth as well as wit, in the following pithy remark by our friend, Prof. Ackley, of Cleveland: “It is impugning the wisdom of the Creator to say that He has ordained children to be born into the world in a way not fit to be seen.”

If to any of our readers it may seem that we have dwelt too much on Demonstrative Midwifery in this, and former numbers of our Journal, we beg, as our apology, to refer them to the letter on this subject contained in the number for March last, denouncing a measure, for which, in common with our colleagues of the Medical College, we are responsible, as *unprofessional, immoral, and indecent*. We would willingly think that the letter referred to was penned under the influence of erroneous representations as to the facts in the case, and that it was signed without duly weighing the force of the language used, and under the impression that its object was merely to disapprove of the innovation, and to dissent from the editorial opinion advanced in the preceding number of this Journal. Indeed, we may say that some of the signers of that letter have signified thus much. But the public *rebuke* has not been formally withdrawn, modified or explained. We cannot but think that a medical association would have been a more appropriate place to have preferred such an accusation, but since, in lieu of this, the *medical public* has been appealed to, we shall look to that tribunal for an acquittal. Farther than this, we have no feelings to gratify, or personal ends to attain by prolonging a discussion of the subject.

In closing this article, we would commend to the attention of the reader the admirable charge by Justice Mullett, and the abstract of the masterly argument by Hon. H. K. Smith, which are contained in the report of the trial. It is to be regretted that notes of the ingenious and successful plea of Henry W. Rogers, Esq. were not taken, so that the publication might have been still more complete. The opening speech for the defence by

Mr. Putnam, a spirited and forcible effort, is embraced in the report

Appended to the report are articles from the Medical Journals that have contained expressions of opinion relative to demonstrative midwifery, and to the action taken by members of the medical profession of this city on the subject. We had promised to copy these articles, but since they are contained in the publication of the trial, and will thus be in the hands of all our readers, their insertion in our Journal will be dispensed with. It were gratuitous to commend them to our readers.

Professor Charles A. Lee.—Biographical sketch.—Under the title of "*Sketches of Eminent Living Physicians,*" a writer in the Boston Medical and Surgical Journal, has given spirited and graphic biographical notices of several distinguished members of the medical profession in this country. The Sketches appear with the signature of *Cato*.

Our friend and colleague, Professor Lee, is the subject of the last sketch, (No. for the 10th ult.,) which we copy for the gratification of those of our readers, who desire to know something of the life, and peculiar traits of one whose name and writings are familiar to every one; and one who is honored and loved by his personal friends in proportion to the intimacy of friendship which it is their privilege to enjoy.

"The object of the following sketch, CHARLES A. LEE, M. D., was born about the year 1802, in the town of Salisbury, Ct. His father was a well-to-do farmer, and had been an officer in the revolutionary war. His grandfather, Rev. Jonathan Lee, is said to have been the first settled congregational minister in the above town. Until the age of 13 years Charles obtained his education in the common district schools, when, with the assistance of a private tutor, he began the study of the ancient languages. He afterwards became an inmate of the family of his uncle, Elisha Lee, Esq., of Sheffield, Mass., who was an early graduate of Yale College, a distinguished classical scholar and advocate, a polished gentleman and a devout christian. After enjoying the benefits of his instruction in the Latin and Greek languages, and in English literature, he was sent to Lenox Academy, a celebrated school of learning, then under the charge of the eccentric Mr. Gleason. He continued here more than a year, prosecuting his studies with unwearied diligence; devoting in fact little time to the relaxation so necessary in growing boyhood. His health became materially injured by this continued devotion to study.

"In 1817 he entered Williams College, in Williamstown, Mass., in an

advanced class, and graduated with distinguished honors in 1820. During his college studies his attention was by no means confined to the ordinary routine of the classes, but the Greek and Latin historians, as well as English literature and science, were eagerly devoured by him. He had also acquired that taste for the natural sciences which has so eminently distinguished his riper years. These studies were carried on under the direction Prof. C. Dewey. Another year spent in the city of New York, and we find him in the office of his brother-in-law, Dr. Luther Ticknor, of Salisbury, studying medicine. He graduated in 1825, after attending three full courses of lectures at in the medical institution at Pittsfield. The last two of these sessions he officiated as demonstrator of anatomy.

“After practicing a short time with Dr. Ticknor, he removed to New York city in 1826, and commenced the practice of his profession in earnest. He soon exhibited his spirit of enterprise by—in connection with Dr. James Stewart—founding the northern dispensary in that city. For four years he acted as attending physician, prescribing for more than 2000 patients per annum. He was, on retiring from these labors, appointed consulting physician, which office he continued to hold until quite recently, his private practice becoming too onerous to attend to the prescribing for so many public patients. His diligence in noting cases and making post-obit examinations was untiring. He found time, by way of relaxation, to prepare a popular and scientific work on Geology, for ‘Harper’s Family Library.’ Another school-book from his pen, on Physiology, soon after appeared, both of which have had, and now have, a very extensive circulation. The latter, with those of Coates, Smith, and others, has tended to create the taste, now so general among the people, for physiological science. The Medical Journals were continually supplied with his cases, original articles, reviews and monographs—when indeed have they *not* been, since he began to wield his ever-moving pen! Among his monographs, were an elaborate essay on the Thymus Gland, on the Medical statistics of New York city, an Essay on the diseases of Clergymen, &c. &c. &c. He also published a series of essays on the effects of the arts, trades and professions, on health and longevity, in the Eclectic Repository, of New York, which did much to awaken public attention to the subject of hygiene and medical police.

“In 1832 he was appointed, by the board of health, physician to the Greenwich Cholera Hospital, where he chiefly resided until the close of the epidemic. He was also physician to the New York Orphan’s Asylum.

“In 1840 he originated the ‘New York Journal of Medicine and the

Collateral Sciences,' associating with him the late lamented Dr. Forry ; to whom, in fact, in consequence of his other professional duties, he resigned the editorial chair, which was however resumed at the death of Dr. F. He never declined furnishing an article when requested, and indeed contributed freely to the Journal. He continued the editorship of the widely circulated Journal up to 1848. During all this time he wrote for its pages, attended to an extensive and increasing practice, and brought out in rapid succession, two large volumes of Copland's Medical Dictionary, with copious notes and additions ; and wrote some sixty lectures on General Pathology, and as many on Materia Medica. He edited Guy's Medical Jurisprudence, which he enlarged to nearly twice its original size. Paris' Pharmacologia and Thompson's Conspectus were edited, and nearly two-thirds of the matter in the last work was added, by Dr. Lee.

"In 1844 he was appointed Professor of General Pathology and Materia Medica in Geneva Medical College. Previous to this he had assisted in the organization of the medical department of the University of New York, and was appointed to the chair of Materia Medica and Therapeutics. He however declined before the institution went into operation.

"In 1847 he lectured in the Starling Medical College, at Columbus, Ohio ; and the same year was appointed to the chair of Materia Medica in Brunswick, (Maine,) Medical College, and in the University of Buffalo. His health having become impaired by these excessive labors, the intervals between the lecture terms, during the last three years, have been spent mostly in travelling. One summer was spent in the Lake country, exploring the whole copper regions of Lakes Superior, Huron, Michigan, &c. : his notes of which, are as yet unpublished. The summer of 1849 was spent in travelling over England, Ireland, Scotland, the Western Islands, and France ; a month was spent in Paris.

"Dr. Lee has a well selected library of three or four thousand volumes. His acquaintance with American literature is perhaps as accurate as that of any medical writer in our country. He has an herbarium of some 1500 species of plants, collected with his own hands. His catalogue of the medicinal plants of the State of New York, is, and will continue to be, a monument to his industry and learning. The students of Geneva College will long remember his accuracy and kindness in explaining the plants which they gathered from day to day during the present spring session of that institution. He is an honorary member, we believe, of most of the scientific societies of this country and many of those of Europe.

"The mineralogy and geology of Salisbury received attention from Dr.

Lee in an article in *Silliman's Journal*. Pereira's celebrated work on food and diet, was edited by him, and received from his pen copious notes ; and what was very creditable to him, the copy right was secured and its entire avails awarded to the author in England.

"The temperance reform acknowledges in Dr. Lee a staunch friend and able defender. His pen has produced many able articles, which have been published in Boston, Albany, and New York city, on this subject. He has also delivered many public temperance addresses and lectures.

"To Dr. Lee is due, we believe, (at least we have seen no prior publication,) the credit of first openly advocating, in the pages of his journal, the subject of a National Medical Convention. The medical schools were at that time, as Cato pretty well knows, generally opposed to the measure, but have since come into it heartily. He also, long before any general movement was made in the matter, called the attention of the profession, in several articles on the subject, to the immense adulteration of drugs in the New York and other American markets. His devotion in fact, to everything calculated to elevate his beloved profession, is unremitting.

"As a lecturer, he is clear, terse and impressive. Having plenty of matter already written, he is at no loss for material, but is exceedingly happy when, throwing away his carefully written manuscript, he lets loose the current of his unharnessed thoughts. His humor is inexhaustible and irresistible, and he can, at will, chain the attention of the class by the expression of wit, pathos, literary and scientific anecdote, or instructive detail and narrative. He is, at once, respected and loved ; the poorest and most dependent student will confide in Dr. Lee, and the most accomplished will desire to learn from him. A consistent member of the Episcopal church, his religious character is as pure as his literary and scientific attainments are brilliant. About five feet eight and a half or nine inches in height, broad shoulders, with a strong frame ; hair of a *salt and pepper* grey, separated on one side of his forehead ; a large Roman nose, dark-blue eyes, and a *well-developed chin* ; a voice which is low and gentle ; manners quiet and rather retiring, more inclined to listen than to take the lead in conversation ; moderate and judicious in the expression of his opinions, and never violent or personal ; with a gait which may be denominated a *good long swing*—and we have Dr. Lee.

"Of such men, Cato thinks the medical profession of the United States, or of the world, ought to be proud—a true and beautiful specimen of what a physician ought to be.

CATO."

Professorial Appointments.—Professorial changes continue to be the order of the day.

Prof. N. CHAPMAN, who has for a quarter of a century or more, occupied the chair of Practical Medicine in the University of Pennsylvania, has resigned, and Prof. GEORGE B. WOOD has been appointed to fill his place. Prof. WOOD has heretofore occupied the chair of Materia Medica. Joseph Carson, M. D., has been selected to fill the chair made vacant by the transfer of Prof. W.

Prof. DICKSON, late of the University of the city of New York, has been reinstated in the Professorship formerly held by him in the Medical College of Charleston, S. C.

Prof. BELLINGER has resigned the chair of Surgery in the latter institution, and Prof. GEDDINGS transferred from the chair of Practical Medicine, to fill his place. It is stated that Dr. DETMOLD, of the city of New York, has been appointed to fill the chair vacated by the resignation of Prof. DICKSON.

In the Medical College of Ohio (at Cincinnati,) all the chairs have recently been declared vacant by the Trustees. Drs. MUSSEY and LAWSON have been re-appointed, and Dr. RIEVE, (a new appointment) appointed to the chair of obstetrics. Dr. DRAKE, (as we are informed,) declines a re-appointment, and several of the chairs in that institution remain to be filled.

The foregoing was in type for our last number, and excluded by an overplus of editorial matter. Several changes have occurred since the article was written.

Dr. John Bell, of Philadelphia, has been appointed Professor of Practical Medicine in the Medical College of Ohio; and Dr. T. O. Edwards, Professor of Materia Medica and Medical Jurisprudence in the same institution. Dr. Bell is the American author of Bell & Stokes' Practice of Medicine, by which, together with numerous other publications, he has earned a widely extended, and high reputation. He has also for many years been accustomed to give medical lectures, although not before, that we are aware of, connected with any collegiate institution. He will reside hereafter at Cincinnati.

Dr. Edwards is known to the profession from his connection with the passage of the law regulating the importation of drugs, being at that time a member of Congress.

Prof. Dudley, of the Transylvania Medical College, Lexington, Ky., has recently resigned the chair of surgery in that institution, which he has occupied with distinguished ability for many years. As an operating

surgeon, especially in the operation of lithotomy, Prof. D. has a world-wide reputation. He has undoubtedly performed this operation a greater number of times, and with less mortality than any other surgeon living, or dead.

Prof. Mott, of the University of the city of New York, and the newly appointed Professor, Dr. Detmold, have recently resigned their places. The New York Medical Gazette informs its readers that Dr. Mott, being in Europe, immediately sent in his resignation on being apprized of the appointment of Dr. Detmold, to which he was opposed. Whereupon Dr. Detmold also resigned, and both resignations were accepted. We learn from the same source that applications are invited to fill the two chairs made vacant by these resignations.

Principles of Medical Jurisprudence: designed for the professions of Law and Medicine. By AMOS DEAN, Counsellor at Law, and Professor of Medical Jurisprudence in the Albany Medical College. Albany: Gould, Banks & Gould, 475 Broadway. New York: Banks, Gould & Co., 344 Nassau st. 1850. 8 vo. pp. 664.

The plan and scope of this work are succinctly set forth in the following extract from the preface:

An experience of some eleven years in teaching in this department, together with a knowledge of the wants of the legal and medical professions in regard to it, have led to the compilation of the work now offered to the public. It does not propose to add new heads, or general topics, for discussion; to deal in original disquisitions, upon doubtful or unsettled principles; or to offer mere novelties to those in pursuit of knowledge on its various subjects. The objects chiefly had in view, have been, a methodical, systematic arrangement of the topics legitimately embraced in the department; and in the treatment of each, a condensation of the knowledge now possessed; and an exhibition of it in a clear, natural, and logical order, together with such illustrations as were deemed necessary to make an application of the principles to practice. In the selection, and narration of cases, both multiplication, and minuteness of detail, have been equally avoided; the object being simply to illustrate, and facilitate the reduction to practice, of the principles embraced in them. By adopting this course, I am enabled, within the compass of a single volume, to state in concise terms, all, or nearly all, the ascertained facts and well settled principles that are important to be known, with a brief statement of the cases that serve to illustrate them; making at the same time, a reference to the sources from which they were obtained, and where also a greater number of cases, and more minute details of each may be found.

The author's experience, and his acknowledged ability commend the work to the confidence of the medical and legal professions.

For sale by Derby & Co.

Facilities for Medical Instruction at London, England.—Dr. Smith, Editor of the Boston Medical and Surgical Journal, who is travelling in Europe for professional observation, and the restoration of health, is furnishing for his readers some highly interesting letters respecting medical matters abroad. His tour, thus far, has extended only through Great Britain. In a late letter he speaks of the comparative advantages for medical instruction at London, and in the United States, as follows:

“After a careful examination, it may be said that with all the presumed and admitted advantages for medical study in this magnificent capital, the facilities in most respects are quite equal in Boston, Philadelphia and New York. A more preposterous apology for going abroad, so far as England is concerned, was never devised. It is susceptible of demonstration that American students are more industrious at home than on their arrival in Europe. At the moment of writing, it has been stated, on reliable authority, that anatomical pursuits are conducted here with extreme difficulty, on account of the scarcity and expense of subjects. When the limbs retail for ten shillings apiece, poor students become economical of *matériel*. Not an individual dying in a hospital is taken for the theatre. Prisons and work-houses are exclusively the sources of supply. If friends claim the body, the schools cannot be supplied with it.”

Drs. Jackson and Morton — Discovery of Etherization.—It is stated that the Academy of Sciences at Paris have awarded \$500 to Dr. C. T. Jackson, of Boston, for his observations and experiments on the anæsthetic effects produced by the inhalation of ether, and to Dr. Morton the same sum for having “introduced it into practice.”

A Theoretical and Practical Treatise on Midwifery, including the diseases of pregnancy and parturition. By P. Cazeaux, adjunct Professor in the Faculty of Medicine in Paris, etc., etc. Adopted by the Royal Council of Public Instruction. Translated from the second French edition, with occasional notes, and a copious index. By Robert P. Thomas, M. D., etc. With one hundred and seventeen illustrations. Philadelphia: Lindsay & Blackiston. 1850. 8 vo. pp. 765.

The treatise on midwifery by Cazeaux has acquired a high reputation, as evinced by its adoption by the Royal Council of Public Instruction, and by the demand for a second edition from which this translation has been made. The translator informs us in his prefatory remarks, that the new

and highly interesting physiological developements respecting the changes that take place in the ovary and ovulum before and after fecundation, are so described and illustrated in this work, as to be intelligible to every reader. The arrangement and description of the mechanism of labor, peculiar to Cazeaux, he thinks is calculated to simplify the subject. The practical principles inculcated are based on an extensive hospital practice. We do not profess to have given the work an attentive examination, but from the position of the author, and the favorable notices which it has received, together with the opinion of the translator just given, we have no doubt of its excellence. Its typographical appearance is highly creditable to the publishers.

For sale by Derby & Co.

A Short History of Medicine, from the earliest periods up to the revival of literature. An Introductory Lecture delivered March 28, 1850, in the Philadelphia College of Medicine, by James Bryan, M. D., Prof. of Institutes and Medical Jurisprudence.

We have neglected making acknowledgments for this interesting and instructive discourse, owing to its having been received during our absence, and mislaid. It is chiefly confined to the history of ancient medicine, a field of curious and useful research, too much neglected by physicians, albeit, our profession is so often charged with a tenacious adherence to antiquity. Prof. Bryan, in addition to his connection with Geneva College, has lately been appointed to a chair in the Philadelphia College of Medicine.

Address before the Suffolk District Medical Society, (Mass.) by John Jeffries, M. D., President of the Society.

This is a very able and elegant production. The subject is, the "relative position of the medical profession with the public." The causes affecting the estimation in which the profession is held by the public are admirably sketched, and the influences that have tended to the improvement of medical science and art are well considered.

New York Medical Gazette, and Journal of Health.—This is the title of a new periodical to be issued weekly by S. S. & W. Wood, of New

York, and edited by D. Meredeth Reese, M. D., LL. D. Price two dollars per annum, in advance.

It will be perceived that the work is designed for unprofessional, as well as medical readers. To meet conjointly the wants of both is a difficult undertaking, but we know of no one more likely to succeed in the attempt than Dr. Reese, and we heartily wish him success.

New Medical Institutions.—Two new Medical Institutions have lately been established, one in the city of New York, and one in Louisville, Kentucky.

The Faculty in the *New York Medical College* consist of Horace Green, Prof. of Medicine; Abraham L. Cox, Surgery; B. Fordyce Baker, Midwifery, &c.; John H. Whittaker, Anatomy; R. Ogden Doremus, Chemistry. The chair of *Materia Medica* and Pharmacy is not yet filled.

The *Kentucky School of Medicine*, as the new School at Louisville is called, opens under the direction of the following Faculty:

B. W. Dudley, M. D., Emeritus Professor of Anatomy and Surgery; Robert Peter, Chemistry, etc.; Samuel Annan, Pathology and Medicine; Josh. B. Flint, Surgery; Ethelbert L. Dudley, Anatomy; Llewellyn Powell, Obstetrics; James M. Bush, Surgery; Henry M. Bullitt, Physiology and *Materia Medica*.

Fatal effects from Carelessness of an Apothecary.—A Boston (Mass.) Apothecary lately put up ten grs. of the *oxy-muriate* of mercury for the *sub-muriate*, which was written for by a physician. It was taken by the patient, and proved fatal. The Suffolk District Medical Society, in view of this occurrence, have appointed a committee to consider, and report upon the expediency of dispensing with the use of Latin in writing medical prescriptions. The mistake, however, in this instance, could not have consisted in reading the prescription wrongly, as no apothecary of decent information would suppose that ten grains of the *oxy-muriate* would be prescribed for a patient. The carelessness must have been in giving the wrong article, and would probably have happened if the term *calomel* had been used.

Death of Prof. Shotwell.—We are pained to notice the decease of Prof. John T. Shotwell, of the Medical College of Ohio. He fell a victim to the epidemic cholera.

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NO. 4.

ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* By AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

(Continued from page 148.)

SECTION THIRD.—*Symptoms referable to the general aspect; Expression of Countenance; Decubitus.*—Characteristic changes wrought in the physiognomy, constitute an interesting branch of the symptomatic history of a disease. Such changes may involve various morbid effects. The physiognomy in health consists of several elements, the more important of which are the complexion, the facial muscles, and the eye. These elements, singly or collectively, being more or less modified, the general aspect, and the expression of countenance will be correspondingly altered.

In continued fever, some changes in physiognomy almost always occur; the changes, although not uniform, either in kind or degree, in some instances are striking, and in a measure distinctive.

Under the heading of this section, in the preliminary analysis, I have arranged whatever is recorded in the cases under examination. In several of the histories nothing is stated relative to this point. In these instances here, as with regard to other points, the inference is, that nothing

unusual, or striking, was observed; but there probably existed more or less of that *dullness* or *listlessness* of expression, which is very rarely, if ever, entirely absent.

Directing inquiries to the elements of the physiognomy separately, the *complexion* comes first in order. In a large proportion of the cases, the complexion was altered, the face being *reddened*, sometimes of a *dusky* hue, and occasionally slightly *livid*. These changes were evidently due to capillary congestion. They were always more marked in the cheeks, than elsewhere, and sometimes were chiefly observable in that situation. This state of the capillary vessels, as will be perceived when we come to speak of the symptoms referable to the skin, in many cases extended, more or less, over the entire surface of the body, but in some instances it was exhibited on the face when it was not on any other part of the external tegument. It was always exhibited in the face, if present in other portions of the external surface.

I cannot suppose this symptom to have been peculiar to the cases that I have observed, and hence I am at a loss to understand why it has not attracted more notice than it appears to have done with clinical observers, more especially since, in connection with the humoral pathology of fever, it would seem to deserve consideration.*

The redness of the face resembled closely the appearance which the surface presents after exposure to cold, the explanation probably being the same in either instance, viz: retarded circulation of the blood in the capillary vessels. On pressure with the finger the redness disappears, and returns after the pressure is removed. The redness returned less quickly in proportion to its duskiness or lividity, showing that the color is an indication of the degree in which the forces of the circulation residing in the capillary system are depressed. This statement is made from recollection, but is believed to be correct.

The cases of the *Typhus* and *Typhoid* types present, on comparison, differences as respects this symptom. It was present in a large proportion of the *Typhus* cases. It is recorded present in *eleven* of the *twelve* hos-

* I do not mean to be understood by this remark that this symptom has escaped observation, but only that, so far as my knowledge extends, it has received less notice than would be expected from its presumed frequency, and pathological relations. Without having taken pains to consult authorities on the point, I cannot call to mind any writer who has alluded to it more distinctly than Dr. Gerhard, of Philadelphia. [See Graves and Gerhard's *Clinical Medicine*, page 733, and also Dr. Gerhard's account of *Epidemic Typhus Fever* in Philadelphia in 1836, published in the *American Journal of Medical Sciences*.] I do not discover that this symptom is embraced in the researches of Louis.

pital cases of this type, no mention being made of it in the single remaining case.

On the other hand, in the *eighteen Typhoid* cases it is recorded present in *ten*. In some of the remaining *seven* cases, its absence being stated, and in others no mention made either of its absence or presence. In none of the cases occurring in private practice is this symptom mentioned.

The two groups of cases differed also as to the hue of redness. In the *Typhus* group the hue was dingy or dusky, and in one case distinctly livid. This is noted in *five* of the *eleven* cases. In the *Typhoid* group, on the contrary, in *one* case only, is the redness recorded as dusky, and in *one* case somewhat dusky. As a general remark, the intensity of redness was greater, in a marked degree, in the cases of typhus than in those of the typhoid type, in those instances in which a dusky tint was not apparent. In most of the cases of the latter type, in which the symptom was present, it is stated to have been either moderate or slight. Such qualifications are not given in any of the cases of *Typhus* in which the symptom was present.

It may be suspected that the capillary congestion of the face and surface generally, is dependent on the complication of pulmonary disease, in consequence of which, the free transmission of blood through the lungs is prevented, and its aëration compromised. With reference to this point, I have compared the pulmonary symptoms with the symptom now under consideration, in all the cases, and find that this connection does not uniformly exist. Several of the cases in which the congestive redness and dusky tint of the face were marked, presented pneumonic complication, denoted by cough and expectoration, accelerated respiration, and, occasionally, the physical signs of inflammation of the lungs, but these symptoms and signs were absent in other cases in which the appearances of the face were the same; and the lungs evinced notable disorder and inflammation in cases in which no capillary congestion is recorded. To speak with greater exactness of the cases of *Typhus*, congestive redness of the face, and marked disturbance of the respiration, were associated in *nine* cases. The congestive redness was present without notable disturbed respiration in two cases.

Of the cases of the *Typhoid* type, in but one case was congestive redness associated with marked disorder of the respiratory function, and in *two* cases in which pneumonitis existed, that symptom is not recorded as present.

Thus it appears that the condition of the capillary vessels is not to be attributed to a morbid condition of the lungs, or to derangement of the functional activity of the latter organs. It is worthy of note, however, that in *typhus*, in a large proportion of cases, capillary congestion is associated with pulmonary affection, while in *typhoid*, the absence of such a connection would seem to be the rule. It is highly probable that both pulmonary disorder and congestion of the surface, may be effects of the same prior morbid condition. It is not likely that capillary congestion in Continued Fever is limited to the surface, when it is present in the latter situation. Internal parts, could they be observed, might be expected to present similar appearances. The lungs, as is well ascertained, are liable to become congested during the febrile career, and the form of pneumonitis which usually occurs as a complication of fever, called *pseudo pneumonitis*, is usually supposed to result chiefly from passive engorgement of these organs. This complication, however, is thought to be far more liable to occur in *typhus* than in *typhoid fever*. The morbid condition then, whatever it be, which determines the congestive redness in the skin, is the occasion of congestions elsewhere, which may, or may not be sufficient in degree to produce a sensible physical change in the congested organs, leading to marked disturbance of their functions. In congestions, pervading the capillary system of the general circulation, the lungs are especially prone to become involved. This is probably the fact in cases of *typhoid* as well as *typhus*, in which congestive redness of the surface is observed, but not in a degree sufficient, generally, to lead to notable pulmonary complication, or marked disorder of the respiration. The latter consequences, it would be expected, should much oftener be present in *typhus*, inasmuch as in this type, not only is capillary congestion present in a much larger proportion of cases, but it exists in a much greater degree. This pathological reasoning is submitted as rationally consistent with the facts which preceded and suggested it.

And now the question can hardly fail to arise in the mind of the reader, upon what prior morbid condition *does* the state of capillary congestion depend. Is it dependent on the impairment of the nervo-muscular forces involved in the circulation of the blood, or is it incident to a morbid alteration in the blood itself? This question opens the way for a wide discussion, and would embrace topics of a speculative character. These I shall forego, as foreign to the historical plan of this memoir, and confine myself to the inquiry whether the cases in my collection can be made subservient to the developement of any farther results having a bearing on this inquiry.

If the congestion of the capillary vessels depends merely on the condition of the forces carrying on the general circulation, it would be expected that in those cases in which the former was marked, the pulse, the thermometer of the forces presiding over the movement of the blood through the heart and arteries, would denote a corresponding embarrassment of the latter. A comparison of the pulse in those cases in which capillary congestion was present, with those cases in which it was absent, is therefore desirable. It is not important to enumerate with respect to this point, but simply to ascertain if, in my collection of cases, the presence of capillary congestion is uniformly associated with symptoms denoting a degree of depression of the forces carrying on the general circulation, greater than when capillary congestion is not present. The symptoms by which a depressed or embarrassed circulation is to be estimated, relate to the pulse and the impulse of the heart. As regards the pulse, in proportion as it is frequent, feeble and irregular, the circulatory forces are to be regarded as compromised by the disease.

An examination of the cases with reference to this point, shows, that in much the larger proportion of cases in which capillary congestion is noted, the frequency and feebleness of the pulse were not greater than in cases in which no capillary congestion was observed. I counted *twenty-two* cases (including those of typhus, typhoid, and doubtful types,) in which capillary congestion was not associated with *unusual* disorder of the general circulation. In a few cases (*four*) this connection was found to exist; and in some cases, (*three*) there existed notable disturbance of the general circulation without congestive redness. In view of these results, is it not fair to arrive *par voie d'exclusion* at the inference that the source of the retarded circulation in the capillary system of vessels, in continued fever, lies in some morbid condition affecting these vessels; or their contents, more directly than through the intervention of the general circulation? That this morbid condition relates to the blood itself, rather than to the vessels, many considerations might be adduced to render probable, but this would lead us beyond the circle of our facts.

In connection with the pathological inquiries suggested by the symptom which has just been considered, it would be interesting and important to determine, to what extent, and under what circumstances, the symptom is present in other diseases than continued fever. This must be deferred for want of sufficient data for such a comparison.

I proceed now to consider elements of physiognomy other than the *complexion*. These are the *facial muscles*, and the *eye*.

The muscles of the face may present appearances representing the condition of the nervo-muscular forces generally, and they may also serve, to some extent, as indices of the mental state of the patient; in the latter point of view, however, they are generally to be taken in connection with the eye, and both together constituting the *expression* of the countenance.

As representing the nervo-muscular forces, the appearances which are found in the records of my cases are as follows: In *two* cases, *dropping of the lower jaw* is mentioned as a striking symptom. In both cases, the disease, judged by other symptoms, exhibited great severity, and in one of the cases it proved fatal. In the latter case, the symptom was associated with extreme prostration, with sliding down in bed. In the case that remains, the muscular prostration was very great.

Tremulousness of the muscles of the face, as if from mental emotions, was another symptom noted as present in *two* cases. In one of these cases, the disease proved fatal. In this case the symptom was much more marked, and was associated and tremulousness of the hands, and, at times, inability to protrude the tongue.

Abolition of the expression peculiar to the individual, was, of course, present in the fatal cases, as it generally is in different diseases, toward the close of life, entering into what is known as the Hippocratic countenance.

In speaking of the two preceding symptoms, i. e., depression of the lower jaw, and tremulousness of the facial muscles, it is not to be understood that they were present only during the last moments, for, at this period, they are not unusual, but during the career of the fever, prior to the last stage.

The *expression* in the greater number of the cases is described as *dull, heavy, stupid, vacant*, terms expressing different degrees of the same general aspect. This physiognomical appearance, in a large proportion of fever cases, is striking, and, in a measure, pathognomonic. There is a lack of expression, a deficiency, not only of animation, but intelligence, a look of stupidity and indifference, which, to the practiced observer, often suffices to reveal, at a glance, the character of the disease. This general rule, however, is not without exceptions. In *one* of my cases, the habitual expression was that of mirthfulness, and in another case, the patient was cheerful and generally smiling. In another case, the countenance during the whole career of the fever wore a marked lugubrious expression. These appearances are certainly rare. In the great majority of cases,

patients with continued fever manifest by the countenance, or in other ways, neither agreeable nor painful emotions.

The only peculiarities which I have noted, not included in the foregoing summary, are, an expression like that of a person confused, and attempting to collect his ideas; an expression of astonishment; an expression of wildness, the latter being associated with active delirium.

The *decubitus* is a point upon which the records of the case, for the most part, are silent. In a few of the cases, it is stated that the patients lay on the back, and generally, in connection, the tendency to slide downward in bed is noticed. These were invariably cases of great severity of disease, attended by extreme muscular prostration. In other instances, it is mentioned that patients voluntarily assumed a posture on the side, as an indication either of mildness in the disease, or abatement of its severity. All who have been accustomed to observe cases of this affection, have noticed that, as a general remark, the patient lies on the back, in proportion to the gravity of the disease, maintaining the same position, seldom, or never changing it of his own accord; and in cases of extreme prostration, the body yields to the force of gravitation, and, the head and trunk usually being elevated, tends constantly to slide downward, so that unless prevented by a foot board, or by the attentions of those around him, the feet will be found sometimes to protrude beyond the bed.

SECTION FOURTH.

Symptoms referable to the Nervous System. Mind. Sleep. Coma. Senses, and Sensibility. Muscular Contractions, etc.

The *nervous system*, in Continued Fever, is almost invariably, if not uniformly, the seat of some morbid phenomena. So constantly is this the case, that, as the reader need not be informed, a doctrine of the pathology of the disease, which has many supporters, if, indeed, it be not still the commonly accredited theory, attributes the primary and essential morbid changes, constituting, *par excellence*, the disease, to this portion of the organism. The symptoms referable to the nervous system are various in kind, as well as in degree. Some of the more prominent of the several sources of symptomatic events are enumerated in the heading to this Section, and I will proceed to consider them under separate divisions.

Mind. Under this division, the subject of *delirium* first suggests itself.

In much the larger portion of instances, more or less delirium is manifested during the febrile career, but cases are far from exhibiting uniformity with respect to this symptom. The delirium is sometimes *active*, that is to

say, the patient requires to be closely watched, and, possibly, to be placed under physical restraint. Oftener it is *passive* in its character, denoted by incoherent conversation, and muttering. In the lightest shades of mental aberration, the patient occasionally forgets where he is, and the circumstances of his illness, but recovers himself in a few moments. This transient aberration is most apt to occur on first awakening from sleep.

What is the relative frequency in occurrence of the two varieties of delirium just mentioned, viz., *active* and *passive* delirium; and what relations does each kind have to the different types of fever, to the issue of the disease, and to associated symptoms? These are the inquiries which are to direct the analysis of the data before us.

Of the *twelve* cases of *Typhoid* fever in *private practice*, the delirium might be pronounced active in *one* case. Passive delirium, more or less in degree, existed in *eight* cases, and no delirium was exhibited in *three* cases. In the single case of *Typhus* in this group, the delirium was not active; it was active in the single case of *doubtful type*.

In the *hospital cases*, of the *eighteen* cases of *Typhoid*, active delirium was present in *five*, the delirium was *passive* in *nine*, and *four* cases exhibited no delirium.

Of the *twelve* cases of *Typhus*, not *one* manifested *active* delirium, the delirium was *passive* in *eleven* cases, and no delirium was exhibited in *one* case.

Of the *eight* cases of doubtful type, *passive* delirium existed in *seven*, and no delirium was apparent in *one* case.

The above enumerations show that the existence of more or less delirium is the rule in continued fever of either type, but that exceptions to the rule occur in both types; that this rule obtains more generally in *Typhus* than in *Typhoid*, and it is chiefly in cases of the latter type that the delirium is active in its character. Should the latter point be established by a sufficiently large number of observations, it may sometimes be available for diagnosis, as respects the two types, at a period of the disease before other distinctive features have become developed.

In order that the above results may be appreciated, the principles upon which the distinction between *passive* and *active* delirium has been made, requires some explanation. As in every instance in which a division is based chiefly on difference in degree, the dividing line is not easily fixed, and must be determined by a rule somewhat arbitrary. Between a slight shade of aberration, and an excess of activity of delirium, there is a wide interval; but this is not the case at the point where the two extremes ap-

proximate; in other words, it is difficult to say precisely what amount of delirium ceases to be passive, and becomes active. The course I have pursued, practically, as nearly as I can define it, is this—when the incoherency has been boisterous, manifested by much and loud talking, shouting, etc., I have considered the delirium *active*, although the patient may not have required coercion. On the other hand, occasional, or even frequent efforts to get up, if the patient be at once persuaded to lie down, or to return to his bed, have not been sufficient to constitute *active* delirium, but the term has been restricted in its application to cases in which such efforts have been not only frequent, but perseveringly repeated, rendering constant supervision, or even mechanical restraint necessary.

The characters which belong, in general, to this symptom, as it is presented in continued fever, and its diversities in the different cases under examination, claim further notice than merely to distinguish them as active and passive. I will therefore devote some space to these points, without taking pains to obtain numerical results.

In the majority of cases in which passive delirium existed, the manifestations consisted in the patients talking to themselves, muttering, asking questions which were irrelevant, or inconsistent with their situation, frequently relating to business affairs, declaring that they were well, and desirous to go home, and in attempting to get out of bed. The effort to get out of bed, seemed sometimes like sleep-walking, and when patients assigned any motive, it generally differed at every separate attempt. The request to lie down was usually readily complied with, and was followed by quietude for a time, when the effort would perhaps again be repeated, and so successively, with intervals of greater or less duration. It happened several times, when patients had not given any evidences of mental aberration which seemed to require them to be carefully watched, that they succeeded in getting out of bed unobserved, and in partially, or entirely dressing themselves. An attendant is liable to be deceived by the representations of the patient, and suppose that it is proper to allow him to get up. For example, in one of the cases, on the third day, before the diagnosis had been positively made, I found the patient, at the time of my visit, sitting up, entirely dressed, saying he was quite well, that his mother had just arrived in town, and he was going out to see her. On being desired to undress and go to bed, he complied without the least reluctance. The attendant had supposed him to be as much better as he professed to be, and had not interdicted his dressing, which he was able to accomplish, not being, at that stage of the disease, greatly prostrated.

In none of the cases does there appear to have been a persistent delusion, giving a fixed subject and purpose to the conversation or movements, but on the contrary, it would seem that a constant succession of incongruous ideas characterizes the delirium of continued fever.

The delirium was observed invariably to be greater during the night, than in the day time. In some cases, the only manifestations occurred at night, the patient apparently being rational during the day.

In *four* instances, paroxysms of weeping and sobbing occurred, as in some forms of hysteria, the patients being all of the male sex. In two of these cases, the disease proved fatal, and the paroxysm occurred, in each, at the latter part of the disease. In both these cases, the cause assigned for grief was, that the patient had committed some heinous fault which would be followed by disgrace. In each of the two cases in which recovery took place, the weeping paroxysm occurred near the period of convalescence. In one of these cases, the source of grief seemed to be apprehension of death; in the other case, the patient said he would not tell why he wept. This variety of mental aberration presents a striking contrast with the kind of delirium usually present in continued fever, which does not involve any acknowledgment, or apparent consciousness, either of bodily or mental distress.

A trait somewhat peculiar to the mental condition in fever, is that the patient generally replies to questions rationally. Even in cases in which spontaneous incoherency is marked, when the attention is directed by a question, the mind appears to be capable of rational action, but immediately afterward, perhaps, relapses into its former dreamy state.

It does not follow, however, because replies to questions are consistent, that they are therefore correct. This is far from being the case. The statements given by patients with fever are often erroneous, when their intelligence appears perfect; and, hence, they are not to be relied upon for information of the previous history, or the daily events of their illness.

We are apt, also, to be deceived in estimating the mental condition of fever patients by the absence of manifestations of delirium. In some of the cases that I have observed, I had supposed, during the progress of the disease, that the intelligence was in no degree compromised, when, after recovery, patients have assured me that they had a very imperfect recollection of what had transpired. For example, in a case occurring in private practice, the patient had been attended by a homoeopathic practitioner for several days, until, the friends becoming dissatisfied, he was asked if he did not wish another physician. He requested that I should be sent for,

and daily saluted me by name, replied to questions coherently, giving no marked indications of delirium prior to his convalescence, but he afterward informed me that he had no recollection whatever of desiring my attendance, or of the fact that I visited him while the febrile career continued. The only explanation he could give of his electing my services was, that he had been accustomed to see me at church.

Of the few cases in which the delirium was *active*, it was remarkably violent in *two*. In both these cases, the efforts to get out of bed were almost incessant, requiring constant vigilance and considerable restraining force on the part of the attendants. These delirious effects continued up to a few hours of dissolution, both cases terminating fatally. In each of these cases, the type of the disease was *typhoid*.

Cases in which such activity of delirium is a prominent feature, may be considered and treated as cases of *encephalitis* by those who are not thoroughly versed in the principles of diagnosis. The early history of the disease, the state of the senses, the presence of abdominal and the symptoms so often associated with fever, etc., should, in most instances, furnish sufficient data for a correct discrimination.

Cases, however, do occur, in which, owing to defective information respecting the duration of the disease, and the previous history; to peculiarities in the cerebral symptoms, and the absence of many of the distinctive characters of fever, the diagnosis presents great difficulty. The following case will serve to illustrate this remark, and at the same time, furnish an instance of an unusual variety of mental aberration:

Frederick Brighton, German, aged 13, entered hospital September 9th, 1849. He was incompetent to give any account of his illness. From what was said by his sister, who brought him to the hospital, I gathered that he was attacked five days before his entrance, with diarrhoea and vomiting. His mother was buried on that day, having died with epidemic cholera. He had been delirious from the commencement. Had had no recurrence of diarrhoea or vomiting since his attack. It was not ascertained that he had had any medical attendance. He was actively delirious during the night after his admission; and in the morning succeeded in making his way to the verandah communicating with the ward, and endeavored to jump over the railing. At my visit he was somnolent, easily roused, but not to complete intelligence. Keeps his mouth in motion during sleep, as if sucking. (This symptom I have observed in another case.) Skin is cold and dry. Pulse 92. Tongue, which is readily and

fully protruded, is moist and furred. Says he has no pain. No dejection since his entrance, nor has he passed urine.

Sept. 11. Somnolent. Respiration heavy, with loud whistling nasal rale, numbering ten. Actively delirious during night, endeavoring to get out of bed. Easily roused so as to open his eyes, but lies with a vacant stare, respiration continuing the same. Eyes not injected; pupil neither contracted nor dilated. Skin cool. Pulse 94, and somewhat thrilling. Flies creep over the face without exciting uneasiness. Commences to reply to questions, but fails to finish sentences. Protrudes his tongue, which is covered with a white coating; indentations of the teeth on the side of the tongue. No dejection. Abdomen not distended. Several dark petechial spots on abdomen. Resists examination of the abdomen, apparently from disinclination to be annoyed. Passed urine freely yesterday afternoon.

12th. Somnolent. Respiration 12. No dejection. Urinates in bed. Active delirium at night. Skin cool. Pulse 112. Protrudes his tongue readily, which is moist and coated. Resists examination of the abdomen. Partially roused without difficulty, opening his eyes in part only, with an absence of expression.

Treatment.—Tart. ant. and pot. gr. ss. hourly. Head to be shaved and vesicated.

13th. Symptoms not materially altered. Pupils appear dilated, and do not contract on opening the eye-lids. Grinds his teeth loudly. The antimony was repeated yesterday several times without effect. One grain was given to-day with no effect. On attempting to make him protrude his tongue, (which was ineffectual,) he uttered a loud and piercing shriek. No dejection.

Five drops of croton oil were given on this day with no effect, and an enema containing two drachms of the spirits of turpentine.

14th. Died at one, A. M.

Autopsy—*Head.*—Moderate congestion of brain. No effusion into ventricles. Some effusion into arachnoid sac, but quantity not ascertained, owing to escape of blood into this cavity. Consistence of brain normal. No exudation of fibrin, or opacity of arachnoid. No abnormal consistency of cerebral substance discovered.

Abdomen.—Small intestines contained a thin, yellowish substance. Colon much distended with gas. Peyer's glands much hypertrophied without ulceration; and corresponding mesenteric bodies greatly enlarged. No lumbrici. Stomach presented several patches of ecchymosis; mucous

tunic not softened, but detached with unusual facility. Spleen small and not softened. Liver presented nothing worthy of note.

To some of my readers it may appear censurable not to have recognized the disease in this case by the symptoms, but I am bound to confess that the diagnosis was not made prior to the autopsy.

In another case (which occurred in private practice,) the delirium was of an unusual and striking character. I saw the patient, in consultation, on the seventh day of the disease. He had previously manifested passive delirium at night, but had seemed rational in the day time. He was greatly prostrated at the time of my visit, and the general symptoms denoted that the case would soon terminate fatally. In the examination of the case, I proceeded to ask several questions, to which he made no reply, but regarded me with a fixed, vacant stare. Suddenly he shouted with a loud and violent tone, "G——d d——n you, I'll kill you!" This was uttered with a piercing voice, and an expression of countenance which combined the utmost imaginable degree of terror and fierceness, rendering the language more startling and terrific than can well be described. Shortly afterward, he became calm, and replied coherently to questions, but before I left the house, he had two other paroxysms, in which, from expressions uttered, he appeared to imagine he was struggling in a mortal conflict. Although his muscular strength was so much reduced that he was unable to rise from the bed, he shouted so loudly as to be heard over the whole house, and even at a distance from it. The case terminated fatally the following day.

This patient had always been regarded as a man of desperate character, capable of almost any act of violence, and was supposed to have committed heinous crimes. The peculiar kind of delirium seemed to correspond with the character of the individual; and the inquiry suggests itself, may not the delirium of fever be influenced, in some instances, to a greater or less extent, by the habits, occupation, and moral constitution of the patient?

What relations does delirium sustain to the *issue* of continued fever?

Delirium existed in all the fatal cases in my collection. In each of the fatal cases of *typhus*, it was (of course) passive in its character. In the fatal cases of the *typhoid* type, it was active in three cases, and passive in three cases. The three cases, however, in which the greatest degree of activity of the delirium was manifested, all proved fatal. In the fatal cases of *doubtful type*, the delirium in one was active, and in the other passive.

In so far as any deductions from so limited a number of observations are admissible, the conclusions are, that although an unfavorable prognosis is not to be predicated on the fact that delirium is present in a degree sufficient to be pronounced active, yet such cases are more likely to prove fatal, and that an unusual violence of delirium should lead us to anticipate a fatal result.

It remains to propose another inquiry, viz: upon what pathological condition or conditions, is delirium dependent; in other words, what is the pathological explanation of the delirium in continued fever? This, as well as other questions, which, in the present state of knowledge, are open for discussion, and involve considerations more or less speculative, I shall only notice, in so far as facts can be made to bear upon it. With reference to the inquiry, whether this symptom may not be attributable to some one or more of the events appertaining to the febrile career, the method suggests itself of comparing the cases with each other to ascertain whether delirium is uniformly associated with any other particular symptom, or group of symptoms. But the symptom under consideration, being present in the great majority of cases, is apparently not influenced by the varied events which enter into the history of continued fever. It would seem that it must depend on something incident to that morbid condition which constitutes the essential nature of the disease. In all the cases in which delirium was not exhibited, the fever was of a mild grade, and it has been seen that it was not absent in a single case of those which proved fatal. Hence, it would follow, that the symptom depends somewhat on the severity of the disease; a conclusion which accords with the supposition that it proceeds directly from the proximate cause of the fever, and is not occasioned by any of the circumstances subordinate to the disease. In reasoning thus, it is assumed that the delirium of fever does not imply the existence of inflammation of any of the encephalic structures. That it does not, is rendered sufficiently plain by the symptoms, unless it be imagined that inflammation of these structures complicating fever, is deprived of all its diagnostic features except delirium, and this symptom we know does often occur, in other than fever cases, without involving inflammation. But positive observations have established that delirium may exist in fever, in a very active degree, without encephalic inflammation. This was the fact in two cases proving fatal, of those under analysis, in which active delirium was present. The autopsies revealed no evidences of inflammation within the cranium. In one of these cases the delirium

was a more prominent and persistent symptom than in any other case that I have observed.*

Lesions of the mucous tunic of the stomach existed in both of the cases just referred to, consisting, in one case, in patches of ecchymosis, and, in the other case, in ulcerations. The observations of Louis, however, show that active delirium may exist in fever, irrespective of appreciable evidences of gastric disease.

There are several points of inquiry, relating to the subject of delirium, which have not been noticed. Among these are the duration of this symptom, and the period in the career of the disease when it first appears. The latter is a point involved in the discrimination of the two types of fever from each other. In the *typhoid* type it is stated that delirium occurs later than in *typhus*. On each of these points the data are too defective to be of much value. This is owing, in part, to the difficulty of fixing the date of the commencement of the disease in many cases, and, in part, to omitting, in the preliminary analysis, to note the time when the symptom first appeared, and the period of its continuance. To supply this omission, it would be necessary to go through with a re-perusal of all the cases. In examining the facts as arranged, with reference to the points mentioned, I find that in *four* cases of *typhus*, and *seven* cases of *typhoid*, delirium commenced early in the disease, i. e. during the first week; but it is not to be inferred that the same might not have been true of more or less of the cases in which evidence of the fact does not appear.

In a considerable number of cases, I find it stated that delirium was exhibited early in the disease, and diminished, or disappeared for some days before the career of the fever was ended.

The manifestations of a morbid state of mind, irrespective of delirium, deserve some attention. I have copied all that is contained in the preliminary analysis relating to this subject, and will give the expressions made use of in the records to denote apparent mental conditions of the patients, stating relative proportions of cases in which they respectively occurred, in general terms, without taking pains to obtain exact numeral results. *Dullness* of the intellectual operations was the trait most frequently noted in the cases of either type of fever. The patients replied to questions after more or less delay, seeming as if lost in deliberation, or as if the mind acted

*The case referred to was reported for the Buffalo Medical Journal Vol. IV., page 487, No. for January, 1849.

very slowly; when not addressed, they took little or no notice of persons and objects around them, seldom asking for any thing, expressing no wishes, appearing indifferent, and sometimes stolid. This was the general rule, but with some exceptions. In a few cases it is stated that the patients replied to questions promptly, and the apprehension had, apparently, lost but little of its normal quickness. In by far the greater proportion of cases, no emotions, either of pleasure or unhappiness, were exhibited. To this rule there were also exceptions. In one case, the patient was much affected at being ill at the hospital, away from his friends, and shed tears; in another case, great peevishness existed, the patient constantly complaining and whining; in another case, the patient was disposed to complain, and was apprehensive as to the result of the disease. Impatience at being questioned, apparent reluctance to reply, and sullenness, were occasionally observed; so also irritability of temper, which was an exceptional trait. These remarks refer to the mental conditions after the fever became established, and during its career. They do not embrace the period of access. In two instances, one of the typhus, and the other of the typhoid type, there was a disposition to indulge in hilarity and playfulness, as if under moderate exhilaration from an opiate, or alcoholic stimulus, and in these cases the approach of convalescence was marked by diminished loquacity, and sedateness. There were strikingly exceptional instances. Usually, there was nothing like mirthfulness in the mental manifestations, and a *smile* was frequently noted as one of the harbingers of convalescence. In the case of a female patient, it is noted that she showed no reluctance to exposure of the abdomen, to look for the eruption, etc., until the time of convalescence, when the usual modesty of the sex was resumed.

In conclusion, although the mental characters, aside from delirium, present diversities peculiar to individual cases, those belonging to the majority of cases, may be summed up as follows: sluggishness of all the mental powers; disinclination to exertion, either of the attention, or the will; absence of emotional sensibility, of anxiety as to the issue of the disease, etc. And as a general remark, these traits were prominent, and early displayed, in proportion to the severity of the disease.

Sleep. More or less somnolency existed in a large proportion of the cases under analysis. By this I mean, that during the day time, and, in the hospital cases, especially at the time of the daily record, the patient was dozing, and exhibited a marked tendency to sleep. The degree of apparent drowsiness differed considerably in different cases. Generally, the sleep was not profound, nor complete, the patient lying in a state of

semi-somnolency, with his eyes closed, readily roused on being addressed, but relapsing shortly into the same state. This condition is peculiar, and in a high degree characteristic of Continued Fever, being seldom observed in other affections, constituting the symptom known as *coma-vigil*.

Somnolency was not uniformly present, but it was much more frequent in the cases of *Typhus*, than in those of the *Typhoid* type. Out of *eleven* cases of *Typhus* in which the records contain information on this point, it was absent in but a single case; while out of *twenty-one* cases of *Typhoid*, it appears not to have been present in *nine*. Hence, it would seem that the peculiar morbid condition of the nervous system upon which this symptom depends, exists almost constantly in *Typhus*, while it only exists in a little more than one-half the cases of *Typhoid*.

Of the cases of *doubtful type*, it existed in *three*, and was absent in *two*, information on this point being deficient in *three*.

In order to ascertain whether any connection existed between this symptom and the issue of the disease, I will give the facts in the fatal cases.

In the *four* cases in *private practice*, which ended fatally, the conditions were respectively as follows:—(*Typhoid*.) In *one* case, vigilance throughout the whole career of the disease, with active delirium; sleep rather heavy, when it did occur; in *one* case, somnolency, the patient being easily roused, until toward the close of life, when it became more and more difficult, and finally impossible; in *one* case, the record is defective on this point. (*Typhus*.) In the fatal case of this type, the patient was somnolent, but easily roused, up to the close of life.

In the *hospital* cases, as follows:—(*Typhoid*.) Vigilance throughout the disease in *one* case; somnolent toward the close of life, but easily roused, in *one* case; somnolent, and roused to only partial intelligence, in *one* case; part of the time somnolent, but easily roused, in *one* case. (*Typhus*.) Somnolent, but easily roused, in *one* case, up to the close of life; somnolent at first, and easily roused, but became insensible, and died in that state, in *one* case. In the fatal case of *doubtful type*, somnolency did not exist.

It would seem that an unfavorable prognosis is in no wise to be based on the presence of this symptom, when it is considered that it does not uniformly exist in the cases which prove fatal; nor, when it does exist, is it uniformly in a marked degree; and when it is also considered that it exists in a large proportion of the cases which terminate in recovery.

It is worthy of remark, that the somnolency of Continued Fever does not often eventuate in insensibility, or coma. In but *two* cases of *Typhus*, and

one case of *Typhoid*, do the histories show such a tendency. A practical consideration connected with this fact is, that the presence of this symptom need not deter us from prescribing anodyne remedies, or opiates, with the hope of substituting for the somnolency peculiar to fever, more complete, and refreshing sleep. If the somnolent condition of a fever patient denoted a comatose tendency, the use of such remedies might be expected to favor such a tendency, and would, therefore, rationally, be injudicious. Experience, probably, will be found to sustain the conclusion, that, under these circumstances, these remedies may be administered with impunity, if not with advantage.

Coma. This term is here employed to designate a degree of stupor from which the patient could be but partially roused, or, with great difficulty roused, or, in some instances, a state of complete insensibility; and to these conditions occurring during the career of the fever prior to a few hours before death. As occurring shortly before death, they are more properly considered under the head of *mode of dying*, in which connection they will be referred to hereafter.

Considered under these limitations, *Coma* occurred in but a very small number of the cases analyzed, viz., in two cases of *Typhus*, and in two of *Typhoid*. In three of these cases, the disease proved fatal. In the case in which recovery took place, the comatose condition was accompanied by loud, stertorous respiration, constituting what may be termed *apoplectic coma*. As this event is of rare occurrence in fever, and one which cannot but be regarded as rendering the situation of the patient imminently critical, some farther account of this case may be interesting.

William McDonald, Irish, laborer, single, aet. 27, entered hospital June 6th, 1849.

The person who brought him to the hospital, stated that he had been ill ten days, and that he had arrived in this country about a month previous. The patient was incompetent to give a history of the symptoms; said he did not know how long he had been ill.

7th inst. *Present Symptoms.* He lies most of the time in a dozing state, but is easily roused. Eyes are not suffused. Aspect stupid. Face presents slight dingy redness. Manifests aberration of mind by incoherent replies to questions. Does not mutter. Appears not greatly prostrated. Lies on his back, without changing his position, but moves his upper extremities frequently. Takes some notice of persons around him. Respirations 32. Sibilant rale through nares. *Has not been observed to cough.* Tongue furred, quite dry in the centre, moist at sides. Skin moist, and

temperature somewhat increased. Pulse 120, well developed. Has had two dejections since his entrance on yesterday. Has urinated freely. Abdomen and chest thickly covered with a faint, dusky eruption, which seems either to have faded, or to be not fully developed. Abdomen soft; no tenderness on pressure. Gurgling in right iliac region. Has taken, since his entrance, Sulph. Morphicæ, gr. 1-8, once.

Treatment : S. Morphicæ, gr. 1-8; Tart. Ant. et Pot., gr. 1-4; every six hours. Weak milk porridge for diet.

8th. This patient remained yesterday without any notable variation in symptoms, until 8, P. M. It was then observed that he was more somnolent. Pulse 140. Respirations 36, and labored. At 10, P. M., the respirations became stertorous, but he was still roused without difficulty. The stertor continued at intervals through the night. I visited the patient at 6 o'clock this morning. The respiration was then loudly stertorous. Pulse 146, and well developed. Skin hot. He could be roused, with difficulty, so as to open his eyes, and to endeavor to protrude his tongue, but the latter effort was ineffectual. When he opened his eyes, the right upper lid dropped considerably. He retains sufficient muscular strength to raise himself into a sitting posture, and to change his position in bed. He passed urine last night, and had three dejections in bed, unconsciously. Has not vomited. I directed the hair to be cut close to the scalp, the ice-cap to be applied, the head elevated, sinapism to the neck; (it had already been applied to the feet and hands.) Tart. Ant. et Pot., gr. ss., to be repeated in twenty minutes.

1-4 before 8, A. M.; a manifest improvement in symptoms. The stertor is nearly gone. Lies with his eyes open, and takes some notice. Protrudes partially his tongue, on being requested. Pulse diminished in frequency, and, notably, in volume and force. Skin moist. No nausea. Antimony to be continued in doses of gr. 1-4, and omitted, if nausea occur. Ice to head continued, and warm applications to extremities.

11 A. M. Patient again stertorous, but can be partially roused. Antimony discontinued. Blister, 6x4, to nucha.

At 4, P. M., symptoms are not given, but the following prescription is recorded: Iodid. Potassii, grs. v, every two hours.

9th. Stertorous respiration continued during the night, at some times increased, and at some times diminished. Since 4, A. M., respiration has been free from stertor, and tranquil, except that the expiration is almost constantly attended by a groan. He is apparently conscious this morning, but does not speak. He moves his arms when requested, and as requested, but

slowly, and with difficulty. He partially protrudes his tongue, which is covered with a thick moist coating. Pulse is 120. Had free dejections in bed during night. Has urinated freely. He lies with his eyes open, and appears to take notice. Pupils not dilated.

He has taken Iodid. Potassii, grs. v, every two hours, and ice has been almost constantly applied to the head.

Treatment for to-day as follows:—Iodide of Potassium, grs. v, every four hours. Proto-chlor. Hydrarg. gr. 1, hourly. Continue ice-cap. Essence of beef has been given frequently, and is to be continued.

10th. Passed a restless night, but without stertor. Had one dejection in bed. Takes notice of persons around him. Respiration now easy and tranquil. Protrudes his tongue, and essays to speak, but cannot enunciate distinctly. Tongue covered with a thin white coating. He appears to comprehend whatever is said to him.

Treatment: Sulph. Quiniæ, grs. ij, every four hours. Cont. essence of beef.

P. M. Has had three dejections. They occurred in bed, but he has cognizance of them, and endeavors to get out of bed at that time. No stertor. Lies awake; urinates freely. Pulse (figures not legible) well developed.

Omit Quinia, and give, *per enema*, Tinct. opii, ℥j. Apply blisters behind ears. Spts. ether nitrosus, ℥j, every four hours. Omit essence of beef.

11th. Rested well part of the night, and part of the time was restless. This morning, aspect more intelligent, appears to comprehend all that is said to him, but cannot control the muscles sufficiently to enunciate distinctly. Respirations 40; skin hot. Pulse 120, well developed. Tongue dry at tip. One dejection this morning, pretty large. Got up to defecate. Urinates freely. Blisters have vesicated well. Ice is still applied to the head. Has some carphologia.

Treatment: Sup. Tart. Pot., ℥ss. Nit. Pot., ℥ij. Aquæ, Oj. To be drank during the day.

12th. Restless early part of night, but after taking Tart. Ant. and Pot., gr. ʒ, became more quiet. This morning quite as comfortable as yesterday. He is now sitting in defecating chair. Tongue cleaning. Manifests more intelligence, and can enunciate better than yesterday. Four dejections since yesterday morning. Secretion of urine abundant.

Treatment: P. Doveri, grs. iij. Tart. Ant. and Pot., gr. ʒ, every four hours. Omit Sol. Sup. Tart and Nit. Pot.

13th. Aspect better. Tongue improving. One dejection. Skin less hot. Pulse 108. Respirations tranquil, and 32.

Treatment: Tart. Ant. and Pot., gr. $\frac{1}{2}$, every four hours.

14th. Had an uncomfortable night. Respirations accelerated and labored. *Cough troublesome.* Aspect as good as yesterday. Seems bright; understands what is said, and raises himself briskly in bed. Respirations 30; expiration attended by a sibilant nasal rale. Pulse, 120, well developed. No dejection. Urinates freely. Tongue moist and cleaning. Continues to enunciate imperfectly.

Cont. Treatment.

15th. Good night. Aspect improved. Pulse 108, less developed. Skin perspiring. Tongue cleaning. Respirations 36, and not labored. Enunciates better. No dejection for 48 hours. *Cough has become a troublesome symptom. Expectorates considerable puruloid looking matter.*

16th. Restless night. Respirations short, frequent, and labored. Has expectorated considerable muco-purulent matter. Skin warm and moist. Pulse 112. Had large dejection this morning, moulded, and natural in appearance. Respirations 24, and now easy.

Treatment: Carb. ammoniæ, and Tart. Ant. and Pot., if respirations become accelerated or labored.

17th. Aspect better. Comfortable night. Skin cool. Pulse 100. Copious muco-purulent expectoration continues. Respirations easy.

18th. Symptoms continue the same. Distinct relative dullness on percussion over the left side, posteriorly, at the inferior angle of the scapula, with a loud mucous rale; on right side, posteriorly, sonorous rale. He continued to improve, but exhibited some delirium until the 25th, when it is noted that the pulse were 140, temperature of surface increased, copious purulent expectoration continuing, and respirations 32.

27th. Aspect and symptoms improved, but still some delirium at night; copious purulent expectoration. Pulse 108. Skin cool, and perspiring. Chest dull on percussion on the left side, laterally and posteriorly, and anteriorly at inferior third. Respiration over dull portions, tubular. No rales perceived.

On the 29th, he was up and dressed.

July 1st, he was walking about, and from this date, he convalesced rapidly.

Remarks. The history of this case is given in detail, on account of the occurrence of coma, with apoplectic stertor, and the recurrence of this condition, in a somewhat paroxysmal manner. The investigation of the case, I regret to say, as is now apparent, was incomplete, at the time these

symptoms became developed. The chest should have been examined at that time, for, although no cough was noticed anterior to, or in connection with the coma, it is not improbable that physical exploration would then have revealed the existence of pneumonitis, the existence of which, subsequently, was apparent. The occurrence of a muco-purulent expectoration, which became at once a prominent symptom, together with cough, is presumptive evidence that pneumonic inflammation had been present for some time, for it would not be expected that a purulent formation would have accompanied the inflammatory attack from its very onset. Assuming that pneumonitis existed, and its characteristic symptoms masked by those appertaining to the brain, it becomes an interesting question whether the cerebral affection may not have been, in a measure, dependent upon the pneumonic inflammation. It would be improper to discuss this question, inasmuch as the data must be assumed.

In this pathological aspect, the case is one of those which enforces the importance of physical exploration of the chest, under circumstances in which pulmonary disease may be present, with an absence of rational symptoms directing inquiry in that direction, the attention, as in this instance, being absorbed with prominent cerebral symptoms, which, as is well known, occasionally mask affections of other organs.

(To be Continued.)

ART. II.—*Cataract and Amaurosis produced by Stramonium*, Reported by Prof. F. H. HAMILTON.

Mrs. ——— of ———, aged 48, admitted to the Hospital as a private patient, Oct. 23, 1849. When eighteen years old, Mrs. ——— had inflamed eyes, from which she entirely recovered. At the age of 36, a pain commenced in the right eye-ball, which continued at intervals during the four succeeding years. The pain was "heavy," and accompanied with a feeling of fullness and pressure in the ball. This sensation was generally induced by fatigue or excitement, and would subside after a few minutes of quiet. Vision was not in the least degree impaired during this time, except that when the pain existed, the flame of a candle was surrounded by a halo.

During one of these paroxysms of pain, on the 18th of Dec., 1841, she prepared and used as follows :

Dry powdered leaves of stramonium, ℥ j
Hot water, f℥ vii j.

Having steeped this a few minutes, she took a single tea-spoonful of the

tea. Immediately after taking it, she was inclined to lie down, and did so. At the end of an hour she got up and felt rather weak, and the lady with whom she was boarding, remarked that her eyes looked very strangely. She now took a second tea-spoonful, which was followed by an increase of weakness, &c. At the third hour, she took the third tea-spoonful, and immediately staggered and fell upon her bed, and felt as if she was dying. She still, however, persisted in taking the medicine; and at the fourth hour, she took the fourth tea-spoonful, immediately after which she became completely blind and paralyzed, and soon lost all consciousness. In this condition she remained four hours, when her consciousness returned, but she had not power to move, or to open her eye-lids. Forty-eight hours after she lifted her lids with her fingers and found she was still totally blind; the light only producing a sensation like the pricking of needles. Those who looked at her eyes now, said there was a "white film over them." Three months from this time, she saw a little light from the side of her eyes, but her eyes continued to pain her, and the light again disappeared. A second time a feeble vision returned, and then finally, about six months after the taking of the Stramonium, her sight became completely and permanently extinguished.

It appears from this record, that Mrs. — had been threatened with either amaurosis or cataract in one eye for four years previous, showing the existence of a strong predisposition to disease in the organ. It ought, also to be mentioned that on the morning of the day on which she took the stramonium, she had also taken a morphine powder, with valerian and camomile tree; yet, if the account given by herself is correct, (Mrs. — is a very intelligent and highly respectable lady) of her symptoms immediately preceding each tea-spoonful of the stramonium infusion, no one will doubt but that both the cataract and the amaurosis were produced by the stramonium. It is probable, also, that the cataract was complete at the end of 48 hours, when the friends remarked that she had a "white film over the eyes."

Mrs. — came to Buffalo for the purpose of having an operation made upon the cataracts. I assured her that such an operation could not possibly give her vision, as there was a complete loss of sensibility in the retina. But, Mrs. — said, she had her "dark days," and her "light days." On the dark days there was a pitchy darkness before her, and on her light days the darkness seemed to be tempered and less intense. These changes were not dependent upon health, or weather, and occurred indifferently, during the night or the day. She wished me to remove the lenses, and then she could be resigned. I therefore depressed them both in the

presence of the Glass. The operation was successful, but the retina remained insensible.

ART. III.—*Notes on Epidemic and other diseases which have prevailed in the United States, since their first settlement.*—By C. A. LEE, M. D.

(Continued from page 974, vol. v.)

In the winter and spring of 1761, a severe influenza prevailed over the Northern and Middle States, and the West India Islands. It began with a severe pain in the head and limbs, a sensation of coldness, shiverings succeeded by great heat, running at the nose, troublesome cough, and bilious symptoms. In May it seemed to run into a malignant fever; many aged people fell victims to it; the lungs becoming loaded with a thick, viscid phlegm, attended with cough, great prostration, laborious respiration, pains in the chest, præcordia and limbs, under which they speedily succumbed. (Webster.) In the summer of the same year, an infectious bilious fever prevailed at Charleston, S. C. The summer of 1762, was extremely hot and dry, scarcely any rain having fallen from June to September; the bilious fever proved very malignant and fatal in Philadelphia.

The summer of 1763 was wet and unhealthy. The Indians on the Nantucket, were attacked with "bilious fever," in August, and between that time and February following, their number was reduced from 358 to 136. Thirty-six only recovered out of 258, who were attacked. The disease is said to have "commenced with high fever, and ended in typhus, in about five days." It appeared to be infectious only among the Indians; for no whites were attacked, although they associated freely with the diseased. Persons of a mixed blood were attacked but recovered. None died, except of full Indian blood. The Indians on Martha's Vineyard were attacked with the same disease in December, of the same year, and out of 52 patients, 39 died. (Phil. Trans. and Lond. Mag., 1764. Hist. Coll, vol. 3, 158.) Severe anginas prevailed during several of the following years, in various parts of the United States; in 1772, epidemic catarrh was quite rife, and was followed by the measles, which proved very fatal, 90 children having died of it in Charleston, S. C. A typhoid fever swept off some 50 or more inhabitants of Cape Cod. The measles was succeeded by fatal anginas, and an epidemic croup which carried off many children, in some places, scarcely a single person recovered, who was attacked. The dysentery was very prevalent in 1773, in New Haven and East Haven, Connecticut, and Salem, Massachusetts, and put on a peculiarly malignant type. Cynanche maligna, would seem to have been very general over a consid-

erable part of the United States, and very fatal, also, for several years, about this period; while croup, measles, scarlet fever, and dysentery, were also extremely prevalent and destructive.

During the year 1776, the dysentery prevailed over every part of the country, and very fatal to the American troops at New York, and Ticonderoga—Of 13,000 men, one half only were fit for duty. It was a general opinion that the disease spread by infection, and was carried by the soldiers returning to their homes. It had, however, proved very fatal in former years, as from 1749 to 1753; and in 1775, 100 of the inhabitants of Danbury, Connecticut, were swept off by this disease, in a population of about 1000. Typhus fevers were rife in 1778, and an "infectious bilious fever" prevailed in Philadelphia, during the summer and autumn, after the British left the city. The winter of 1780 was one of the coldest ever known in the United States. Webster says, that "not only all the rivers, but the harbors and bays in the United States as far South as Virginia, were fast bound with ice. Loaded sleds passed from Staten Island to New York; the Sound between Long Island and the main land was frozen into a solid highway, where it is several miles in breadth. Chesapeake Bay at Annapolis, where the breadth is $5\frac{1}{2}$ miles, sustained, also, loaded carriages."—(On Pestilence, vol. 1. p. 266.) Dr. Rush states, (vol. I, 123.) that catarrhal complaints were very common among children during the spring of 1781, and the bilious remittent fever was epidemic in Philadelphia, during the summer and autumn, accompanied by such acute pains in the back, hips, and neck, as to obtain the name of the "*break-bone fever*." The influenza, also, swept over the country during the spring of the same year; beginning with a severe pain in the head, prostration of strength, coldness and chills, &c. The cough was troublesome. The disease usually terminated about the thirteenth or fourteenth day—although sometimes on the eighth or ninth. The same disease pervaded the eastern hemisphere the year following. The scarlatina appeared in Philadelphia in August, 1783, and in September became epidemic, and extended over New England, in 1784, where it continued to prevail with more or less violence for five or six years continuously. The measles and cynancha maligna were also, very prevalent during the same period. The Dysentery and the usual autumnal fevers were not, however, as general or fatal, as they had been in previous years. Canine madness began to rage and spread in 1785, in all parts of the Northern States, and many of the inferior animals became infected,—especially foxes, wolves, and horned cattle. The newspapers in every part of the country abounded in cases of hydrophobia, while at the

same time, malignant scarlatina was extremely rife and fatal. About the same period the ravages of the wheat insect, (weevil?) erroneously called the "Hessian Fly," were very destructive. It seems to have been introduced into the United States about the year 1776; first appearing on Long Island; then spreading over New Jersey, traveling at the rate of about 20 miles annually; till in 1785, it became so destructive to the grain, that the English Government issued a proclamation, dated June 25th, 1788, prohibiting the importation of American wheat. The Agricultural Society of Pennsylvania, however, in a report published at the time, (*Museum*, vol. 4. 244,) express the opinion that it is "the *plant* alone, which is injured by the insect; that the grain is sound and good, and that the insect is not propagated by sowing wheat, which grew on fields infected by it." The scarlet fever and hydrophobia continued their ravages throughout New England and the Middle States during the year 1786; the former, after sweeping away whole families of children.

Noah Webster describes a remarkable halo that appeared in Connecticut in May, 1789, and states, that a clergyman wrote an essay on the occasion, full of gloomy predictions, regarding the halo as certain evidence that "the arm of the Lord was extended in wrath over our land."

In the spring of 1789, there was something like a famine in the Northern States. In Vermont, the people found a new kind of food "in tad-poles, boiled with pea-straw." "Four potatoes sold for nine-pence." The crops, especially corn, had failed, to a great extent, during the preceding year, and "the cod fish were sickly." Scurvy and fluxes were consequently very rife, and cattle perished in large numbers. Hydrophobia again raged, and in Maryland there was great mortality among horses.

In the fall of 1788, the bills of mortality for New York and Philadelphia show that the measles prevailed extensively, and we find that this disease became epidemic over the Northern States the same year. The influenza, or epidemic catarrh, also, became epidemic, though the precise time and place of its appearance are not ascertained. We first hear of it in New York and Philadelphia, in September, 1789. Dr. Rush thought it was brought to Philadelphia by the members of Congress, who returned from New York in October. It soon spread over the whole country, appearing in Connecticut about the middle of October. It was generally regarded as highly infectious. It invaded the wilderness, attacked the Indians, spread over the ocean, seized the mariner a hundred leagues from land, invaded the West Indies about the same time it did the Northern States; in short, it overspread America, from the 15th to the 45th

degree of latitude, in six or eight weeks, proving, as usual, very fatal to the aged, and those of debilitated habits and broken constitutions. It seemed in some places, to replace scarlatina; in others, to alternate with it; in some, it raged coincidentally. One marked feature of both these diseases, at this period, was the bilious character they assumed, which continued to mark the prevailing complaints for several subsequent years. Noah Webster informs us, (*On Pestilence*, vol. I, p. 290,) that the scarlet fever did not spread over the country in 1790. "It was little known," he remarks, "in the Northern States, till two years after—this is among the proofs that this disease does not depend on infection for its propagation. If infection was its only, or principal means of propagation, the fomites existed in great abundance, in particular places, in 1790, and sufficient to have spread it over the United States. But a disease, however infectious, will not spread far in an atmosphere that will not generate it. Indeed, scarcely a year passes, in which sporadic cases of scarlatina, or anginas, of other kinds, do not appear in particular places, *but they never spread without some uncommon concurrence of causes.*" This latter, is a very true remark. What these causes or circumstances are, we are, as yet, to a great extent, ignorant; but that they exist, no one can doubt. To them, in connection with a pre-existing *virus*, [or *contagious principle*, we must always look for a true explanation of the prevalence of epidemic diseases.

A second epidemic influenza raged in the Spring of 1790, pervading, as before, all parts of the country, but more violent than that of the preceding year, sweeping off large numbers of consumptive persons, and hard drinkers. Mr. N. Webster notices as a remarkable fact, that 14,000 shad were caught in a single draught, by some fishermen, at the Narrows, near New York, in April, of this year, viewing it as a sure precursor of pestilence, along with meteors, volcanic eruptions, hurricanes, severe cold and heat, earthquakes, aurora borealis, &c., &c. "It will be noticed," he remarks, "that the pestilential fever, which has prevailed for many years past, first appeared in New York, in the autumn succeeding this singular draft of fish." (*Læ cit.*) "It may," he adds in a note, "excite surprise that there should be supposed a connection between an uncommon abundance of fish and pestilence. But the theory that resolves this into the unusual power of excitement is rational. The state of the elements that causes pestilence, always produces unusual numbers of insects; and often the human race is more prolific than at other times." During the autumn of 1791, bilious remittents were uncommonly prevalent in Philadelphia, according to Dr. Rush, assuming an inflammatory form, requiring repeated bleedings. The

yellow fever, also, carried off more than 200 persons, along the banks of the East River in New York. In 1792, the same disease ravaged Charleston, S. C. Scarletina became epidemic, also, in New England and the Middle States, and swept off multitudes of children. The influenza prevailed extensively, also, the same year. The scarlet fever, at that period, is described as coming on with "slight influenza, stinging pains in the jaws and limbs, soreness in the muscles of the neck, with a slight fever." In addition to the above, a very fatal form of dysentery prevailed in some of the Northern and Middle States, in the summer and autumn of 1793, which in Coventry, Conn., is said to have proved fatal in every case attacked. A severe "lung fever" also, prevailed at Weathersfield, and some other places. In August, 1793, commenced that very fatal yellow fever, so graphically described by Dr. Rush, which, in a few weeks, carried off more than 4,000 victims, being preceded by influenza, scarlatina, and mild bilious remittents. The weather, during its prevalence, was very hot, dry, and sultry. (See Rush's works.) The scarlet fever prevailed extensively in Connecticut, in 1794; in the spring of 1795, it broke out in Boston, and continued to prevail in Massachusetts and New Hampshire, in 1796. It seems to have been about four years in extending from New York to Maine, some 400 miles. The same rate of progress, though in an opposite direction, was noticed in 1735.

On the 10th of June, 1794, the "bilious plague," or yellow fever, made its appearance in New Haven, Conn. Its origin has given rise to much discussion and dispute. The general belief was that the seeds of it were imported in a sloop from the West Indies. This, however, is denied by Webster, who traces its causes—1st, to the sickly state of the oysters on the Connecticut coast for several months preceding; 2d, to the multitude of caterpillars, which overrun New Haven, in the summer of 1794; 3d, to damaged pickled codfish, which lay exposed to the sun during the recess of the tide; 4th, to the garbage of shad and decaying clams. "So noisome," says Webster, "was the air of the place for some time before the fever appeared, that the proprietor of the wharf desisted from his usual morning visits before breakfast. The putrefaction of flesh, from 30 years' observation, I can testify, will not always produce disease. But in a pestilential state of air, the dissolution of flesh is unusually rapid, and the acid evolved, peculiarly noxious. In such circumstances, putrescent substances of all kinds appear to be powerful auxiliary causes of disease. The condition of the elements accelerates putrefaction, and that putrefaction in turn increases the deleterious quality of the air."—(Loc. cit.) Have we any right to assume "a

pestilential state of the air," independent of any local causes? We should like to be informed what this "condition of the elements" is, which is always pre-supposed to exist, in advance of epidemics!

In July, 1795, the yellow fever broke out in the city of New York, and raged extensively along the low streets on the East River, in the region then and now called "the Swamp," and in the narrow alleys. The higher grounds in the centre of the city, and the western side of the island, were healthy as usual, and the disease, when carried to the healthy portions of the city, did not spread. The citizens mostly remained in the city. The deaths reached 730; 500 of whom were foreigners, chiefly Scotch and Irish. Then, as now, the excessive mortality of New York was due to the immense influx of foreigners, not seasoned to our climate, and whose habits of life are not regulated by proper hygienic laws. Dr. Treat, the health officer of the port, was one of the earliest victims. The disease was evidently infectious, where local circumstances were propitious, the same as the cholera, typhus, dysentery, &c., are at the present time. A few cases of yellow fever occurred at a small village called Mill River, near Fairfield, Conn., the same season, which were traced to infection from New York.

It is hardly necessary to state that the bilious remittent and intermittent fevers prevailed extensively in our new settlements, both at the south and west, and annually decimated the population. New York State, which was then just being brought under cultivation, bore the reputation of a very unhealthy country. Every new settler, almost, became subject to an attack of some form of malarious disease, and in many parts of the State, during the autumnal months, there was great distress and suffering, there not being a sufficient number of well persons to attend upon the sick. Few of us at the present day reflect at what an immense expenditure of strength, and health, and life, the broad fields of New York have been brought to their present state of cultivation. We, the degenerate and more effeminate sons of a hardier race, are reaping the rewards of their toil and labor, little mindful at what a sacrifice they were purchased. At that period, the bronchocele was also a very common complaint in our State, especially in the regions adjacent to the lakes; to what causes it should be attributed, we are entirely undecided.

(To be Continued.)

ART. IV.—*Modus Operandi of Medicines, Chemically Considered.* No. 2.
By M. M. RODGERS, M. D.

The manner in which medicinal substances produce their curative effects in a pathological condition of any organ or system of organs, and also their

pathogenetic effects in a state of health, is very little understood. And although this knowledge may not be indispensable to the successful administration of medicines in the cure of disease, yet in the practice of an art which professes to be founded upon inductions from the exact sciences, it is desirable, if possible, to trace the connection between every effect and its ultimate cause. The explanation given by authors, of the *modus operandi* of therapeutic agents, falls far short of any thing satisfactory; it is at best, only what relates to their remote effects, and does not reach the ultimate action or final relation which exists between them and the fluids and tissues of the body.

There are three modes, according to authors, in which the general operation of medicines may be explained. 1. It is said, "they produce their effects by actual contact with one or more tissues." But let us go a little farther back, and inquire, how do they act by contact? If we can find what the immediate chemical relations between them and the tissues and fluids are, we shall then have a point from which we can pursue them, step by step, until we arrive at the most remote change produced. When an acid and an alkali are mixed together, effervescence is produced,—and what caused the effervescence? It is caused by the union of the two constituents of the compound. But how was this union produced? By chemical affinity. But again, what causes chemical affinity? Here we must resort to conjecture, and say, perhaps it is caused by cohesion, which is itself caused by a particular internal molecular arrangement,—or, perhaps, by the molecules of each body being in opposite electrical states. But here the explanation ends, and we are still in the dark. And it is true of all investigation, that a limit is set, beyond which we can never pass. We may trace one effect to its legitimate cause, and this cause to some other more remote, which is still but the effect of some cause more deeply hidden, until all beyond is conjecture, and we must link the chain of our reasoning to the Great First Cause.

2. It is said "medicines act by an impulse conveyed by the nerves, through an impression made elsewhere." But here it is only assumed, (not proven,) that the impression is made elsewhere than on the nerves. How do we know whether the primary impression is made upon the nerves or other tissues? The nerves, instead of serving as mere conductors to impressions, may have suffered some lesion, or chemical change, which may increase or diminish their power of generating or conducting any impression whatever. This supposition does not assist the explanation at all.

3. "Medicines act by continuous or contiguous sympathy; or by that

which is excited by mere continuity and proximity of parts." Now, to say that a medicine operates by sympathy, is merely to give a name to our ignorance. Sympathy is not a physical or chemical action between particles of *matter*, but a hypothetical term, implying some metaphysical action or condition. No such therapeutical force can be proven to exist in the system: and when a distant organ feels either the pathogenetic or curative effect of a medicine, it must be from a chemical or electrical action upon one or more elements of some tissue or fluid, which is felt along the course of the tissue to the organ in question.

We have now given a synopsis of the modus operandi of the entire *medicamenta*: we may now give briefly the explanations of authors, of a few particular classes of medicines.

Tonics produce an augmented action of the circulation, temporary strength, and, finally, fever, when taken in a state of health; but this condition is followed, after a short time, by collapse and debility. In both health and disease, they tend to dry up the secretions and excretions, and thus act as astringents; in this way they arrest night sweating, diarrhoea, and other excessive discharges. In this way a reaction is produced upon the current of the circulating fluid, which causes the tide to set back upon the system, and prevent depletion from morbid action. In this way, also, arterial blood is economized for nutrition, while the *vis medicatrix nature* restores health.

Febrifuges are antiperiodics in their action, and, to some extent, are all tonics; they are supposed to terminate periodical diseases by imparting temporary strength and stimulus to the system, which interrupts their paroxysms, while the morbid functions are restored to healthy action by nature herself.

Nauseating medicines restrain hemorrhage by causing faintness, which relaxes muscular energy, and thus lessens the force of the circulation, and allows coagula to form at certain points and close the bleeding vessels.

Purgatives mostly operate by stimulating the muscular coat of the intestines, and thus increasing peristaltic action. The purgative effect of mineral waters is supposed to depend on the large quantity of water which holds in solution small quantities of mineral salts. The same salts dissolved in any water, has the same effect. They are supposed to act by the stimulus of distention; so that their operation does not depend on the substances they contain, nor on their peculiar combination.

Now these explanations are perhaps correct enough so far as they extend,—but they do not show the ultimate relation between the medicine

and any particular substance or tissue of the organism. They give the aggregate of a series of chemical and mechanical processes which had occurred primary to them,—so that they do not explain the specific effect of any medicine or class of medicines.

We shall now consider the manner in which all medicines must be primarily related to the different chemical elements of the body, and attempt to show that there are only two ways in which every article of food, or medicine, whether solid, liquid, or gaseous, must act when taken into the system. It will then be apparent that what we call the specific operation of medicines, is not really any part of their action, but only the manifest consequences of preceding chemical action: that tonics do not directly impart tone to muscular fibre; that cathartics do not directly increase peristaltic action; that emetics do not act by producing nausea; that febrifuges do not cure fever by interrupting the paroxysms, &c. The only two modes in which any substance can act upon the system as a medicine, are by chemical affinity and electricity; mechanical effects often result from these, but are no part of the primary action. These are the only means by which elementary changes take place in bodies, whether organic or inorganic. This will be more apparent when we consider the conditions necessary for the development of either of these forces. Electrical action may be excited between bodies either moist or dry; between gases, liquids, and solids: chemical action can only occur when moisture is present, excepting by high heat, or slow oxydation. When two certain substances, both dry, are brought in contact, electricity may be excited; when two certain substances, one or both moist, are brought in contact, electricity, or chemical affinity, must, one or both, be developed. Whenever this is the case, a change of elements must take place between the two bodies; the old union is broken up, and new ones are formed; so that these elements have different relations to each other, and to all other elements: in the system, a long series of chemical changes may follow this first separation and reunion, and part of these changes may be manifested as the effects of medicines.

One obstacle to our understanding the modus operandi of medicines, is, that the changes which follow their passage into the system, are concealed almost entirely from our view. Another is, that the elementary composition of the fluids and tissues is not constant in quantity or quality. But the greatest difficulty probably consists in the mixed and complicated nature of medicinal substances, and especially those from the vegetable kingdom. Their numerous elements are all compatible in the vegetable during its life; and all so combined as to allow the full development of the

organism, the perfect performance of the vital functions, and the consummation of its design. But when vitality ceases, the juices evaporate, volatile matters escape, the organic elements undergo metamorphosis, and new compounds are formed. So that the chemical character is different in the dead plant, and the constituents variable, and their union unstable. In consequence of this weakness of affinity, between the elements of organic bodies, their equilibrium is easily overcome by any disturbing force; so that the chemical character of any vegetable medicine is no index of its operation on the system: we cannot predict what changes it will undergo, and what new compounds will be formed, when it meets with the acids, alkalies, salts, and gases, in its course. Opium, for example, is a complex substance, among the elements of which are, the alkalies, morphia, narcotine, codeine, thebaine, and narceine, besides tannin, extractive and coloring matter, &c. Some of these substances have a strong affinity for others which are held in solution, in unstable combination, by the fluids of the system. So that we must commence with these first changes, in order to trace the operation through all its windings until it leaves the system. And in order to do this, we must know the relative affinities of the elements of the medicine to each other, and to those of the organism; we can then trace the effects of separation and re-union, and the counteracting effects of one upon another, until no farther chemical change takes place.

But from the complex character, feeble union, and wide range of affinities, of all organic compounds, we must consider this almost impracticable. The only way, then, to study the operation of medicine, in accordance with chemical laws, is to use those of the most simple and well known composition,—such as acids, alkalies, salts, oxides, alcohol, ethers, and vegetable proximate principles.

There are several forces besides chemical affinity and electricity, which are supposed to exert some influence on the operation of medicines,—they are, vital principle, animal heat, magnetism, mental action, idiosyncrasy, &c., most of which must be considered only hypothetical.

Galvanic, or electro-galvanic currents, may be developed by the action of free acids, often present in the stomach, upon the mineral elements of salts and oxides: these currents possess electro-positive and electro-negative power, and tend to decompose and revive elementary combinations. They may also cause decomposition in some of the tissues of the body, since their affinities are weak, and they all contain, in their normal state, more or less mineral matter. In this way the action of medicines is modified in some cases to an important extent. Without farther general

remark, we will proceed to the consideration of the specific action of some medicinal substances according to our theory.

Operation of Alcohol. It is a principle in natural philosophy, that all bodies, in passing from a rarer to a denser state, evolve heat. Alcohol is lighter, and less dense than water; when it is taken into the stomach, it mingles with gastric juice, which is mainly water holding in combination muriatic acid,—this fluid is much heavier than alcohol,—consequently when the union occurs, the dense fluid in becoming rarified, gives off several degrees of latent heat. This accounts for the first calorific effects felt in the stomach. This heat stimulates arterial action by its tendency to expand the blood and other fluids and tissues,—this expansion, while it augments the bulk of the circulating fluid, diminishes the calibre of the vessels, thus making a quicker motion of the blood necessary,—this increased velocity increases the friction between the blood and the sides of the vessels, and consequently increases the heat. Alcohol, by its tendency to coagulate the albumen of the blood, causes in this way obstruction to the circulation, and this is followed by swelling of certain parts from capillary congestion; this capillary congestion reacts upon the larger vessels behind the congested point, causes them to become distended and enlarged, and thus to exert an injurious pressure upon the nerves lying near them,—when this congestion extends to the brain and spinal cord, sensation begins to diminish, motion becomes irregular and involuntary, the senses fail,—and if the congestion continues long, complete apoplexy, stupor, and sometimes death follow. This is intoxication; and when it terminates in resolution, or passes off without any serious injury to the system, all the functions are gradually restored to health. It may be thus explained: part of the alcohol passes from the circulation, with the excretions, while a part is oxydized in the capillaries, to produce animal heat, which has partly in this way been greatly increased. After part of the elements of alcohol have been disposed of in this way, the residue are left in the form of carbon and water, and pass off as effete matter.

Alcohol, then, acts as a *stimulant* upon the vital powers, and particularly on the muscular system, by furnishing to the different tissues, through the arteries, an increased supply of highly oxygenized blood: it acts, afterwards, as a *sedative*, by producing pressure upon the brain and nerves by congested vessels, and also by deteriorating the quality of the blood. The blood in the congested capillaries, and, finally, of the lungs and whole system, becomes surcharged with carbon, and deficient in oxygen. Thus the vitalizing agent, oxygen, is cut off, and the powerful sedative agent, car-

bonic acid, furnished in excess. That this is the explanation of the modus operandi of alcohol, is proven by the pathogenetic effects,—by post mortem appearances, by analogy, and by the treatment necessary to cure poisoning by this medicine.

(To be Continued.)

ROCHESTER, August, 1850.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

On Croup. By JOHN WARE, M. D., Prof. of the Theory and Practice of Medicine, Massachusetts Medical College.

We had intended to have condensed these articles by Prof. Ware, but, on looking them over for that purpose, we have been unable to bring our mind to strike out any part of them. We have, therefore, determined to insert them entire, notwithstanding they will occupy nearly, or quite all the Eclectic Department of this Number of the Journal. In our opinion, we could not select any contributions to practical medicine more interesting and valuable than these several papers. The final conclusions respecting the treatment of Croup, at which the author arrives, will be found to conflict directly with the views generally entertained by practical physicians. This, however, is no reason why the observations and reasoning upon which Prof. W. bases these conclusions should not be carefully and candidly considered.

The several articles have recently appeared in the Boston Medical and Surgical Journal.

EDITOR BUFFALO MEDICAL JOURNAL.

I.—*Contributions to the History and Diagnosis of Croup.*—Read before the Boston Society for Medical Improvement, in 1842.

Every physician who has much practical acquaintance with disease, will have observed that there are great differences of character among the cases to which he finds it convenient, in accordance with the custom of medical men, to give the general name of *croup*. He finds that a certain portion of these cases—and by far the larger portion—yield readily to the means which he employs, and very often to the ordinary domestic remedies of mothers and nurses. He has indeed reason to believe that a considerable number of them would spontaneously subside if left to themselves. On the other hand, he finds that there are some cases, fortunately but few in proportion to the whole, which exhibit throughout their course, a character of obstinacy that bids defiance to treatment, and which, with few exceptions, pass on to a fatal termination uninfluenced by any remedies he can employ.

Different views may be taken of the nature of these cases. It is believed by some that the former are not, for the most part, essentially different from the latter; that the difference is more in degree than in kind, or that the difference in the severity and result depends in difference of management; that the favorable character and course of the former, are merely owing to early and judicious treatment, and the fatal event of the latter to the inefficient or too tardy application of remedies. A long, and I trust a faithful examination of this disease has, however, satisfied me that this opinion is not correct. I have been led to believe that there is an original and essential difference in these cases; that those of the first kind are pathologically different from the second; that the former, even if they terminate fatally, which happens in some rare instances, do not terminate in the same way, or at least do not exhibit the same morbid conditions; and that no variety or deficiency of treatment will cause a case of the one kind to assume the character of the other.

I do not, however, mean to imply, that all the cases to which I refer, are capable of being classed under two varieties. Among those which I have characterized as the more mild and tractable sort, we still find great differences in the mode of attack, course, and mode of termination, and also in the degree in which they appear to be influenced by remedies. The object of this paper is to endeavor to contribute something towards determining the nature and extent of the distinctions referred to. With this view I have made an examination of all the cases of croup of every kind which have occurred during the last twelve and a half years, in my own practice, and of this examination I now submit the results. Upon certain points relating to the severer form of the disease, I have included the examination of a number of other cases, extending over a period of twenty-five years, witnessed partly in my own practice, partly at dissections, and partly in consultations.

It should be first observed, that, in noting cases in order to this inquiry I have set down as croup, all those which in the common language of the profession are included under this name—viz., all those which, at any stage of their progress, present a fair question of diagnosis; all those in which is heard that shrill, sharp, ringing cough, which is regarded as the cough of croup, accompanied by a distinct embarrassment of respiration, however slight, and by some affection of the voice. It follows of course, that many very slight cases must have been included among those on which these remarks are founded—cases which yielded or subsided almost at once. Yet it is right that these should form part of the materials of our examination. When we are in search of means of diagnosis, our attention should be directed to all those cases which have, at any period of their progress, exhibited symptoms that give rise to a well grounded suspicion of their character. Although many cases which excite the apprehension of severe croup on their first attack, pass away very readily, and by their result show themselves to have been of very moderate severity; yet, on the other hand, it is to be recollected, that many cases, which at last terminate fatally, do not at their beginning, exhibit symptoms at all more severe, or excite apprehensions at all more serious, than those which have so readily subsided.

Of the cases to which this inquiry relates, that occurred during the period extending from Jan. 1830, to July, 1842, the number is 131. For the convenience of examination, these may be divided into four classes. I do

not intend by this arrangement to express the opinion that they constitute four distinct diseases. I would not even be understood to assert positively, with our present amount of knowledge, that they are not different manifestations of the same disease. The purpose now is to speak of them as groups of cases distinguished by certain differences in their symptoms and course, which may or may not be connected with an essential difference in their nature. These classes may be designated, with a view to their probable character and for the purpose of referring to them more intelligibly, by the terms membranous, inflammatory, spasmodic, and catarrhal. Of the whole number there were:—

	Cases.	Deaths.
Of Membranous Croup.....	22	19
Inflammatory “	18	0
Spasmodic “	35	0
Catarrhal “	56	0
	131	19

In the first class are included those cases in which there is reason to believe that a false membrane has been actually formed, lining the larynx and trachea.

In the second class, those cases in which the symptoms are for the most part, of the same character as in the first, but in which there is reason to believe that no membrane has been formed. The grounds for the opinion formed of the nature of these two classes will be stated subsequently.

The terms applied to the third and fourth classes, require no particular explanation.

The symptoms on which we depend for the diagnosis of croup, relate to the cough, the voice, and the respiration.

In the early stage of the first form of croup, the cough is by no means peculiar. In the advanced, it assumes a somewhat different character. In the early period it is sharp, shrill, ringing; it does not vary from that which we hear in the other forms, except perhaps that in some of the less formidable cases it is much louder and more violent at the beginning, than it is in those which prove ultimately more alarming. In the latter period it becomes less loud and ringing, but is equally sharp—it often becomes almost inaudible, bearing the same relation to a common cough, that a whisper does to the common voice. The cough, then, affords no certain means of distinguishing this form of croup at that period of it in which the diagnosis would be most valuable.

Of the state of the voice, nearly the same remark may be made. In the advanced stage of a case it is sufficiently characteristic. It becomes a sharp, and almost inaudible whisper. But early in the disease it is not always affected at all; and, if it be, cannot with certainty be distinguished from the hoarse voice of common catarrh.

The condition of the respiration affords us far more important information. In the early period of the disease, however, when we most need means of diagnosis, it is not a symptom which always attracts attention, even from the physician; much less from others who are around the patient. The common description of the breathing in croup, does not apply well to the beginning of the membranous variety. It seems rather taken from cases of a less dangerous kind, in which the breathing is from the first, loud

harsh, suffocative ; attended with great efforts, and much loud coughing; creating great alarm, and calling at once for efficient means of relief. But the breathing in membranous croup does not excite attention in the very commencement of the disease. It is comparatively quiet and unobtrusive. Its true character is not at once to be detected, but only by a careful and accurate observation. The patient has not the ordinary aspect of difficult breathing; in fact, the breathing is not difficult at the very first. He probably experiences no distress. There is no real deficiency in the performance of the function, and no obvious embarrassment. There is only a little effort in drawing in the air, and a little more force exercised in its expulsion, whilst the amount of air admitted and expelled is fully equal to the necessities of life. This perhaps would not be noticed on a casual glance at the patient, but will be at once perceived on attending to the muscular movements subservient to the function, which are—to use an expressive French term—somewhat exalted. It is indicated very soon, also, by a slight dilatation of the nostrils, and a little whizz or buzz accompanying the passage of air through the rima glottidis. This sound is distinguished either by placing the ear near the mouth of the patient, or by applying the stethoscope on the back of the neck, or directly upon the upper part of the larynx.

This at its very beginning is the essential respiration of membranous croup, and it affords far more aid in diagnosis than either the cough or the voice. It is not, however, always found as pure as has been described. It is often mingled with, and obscured by other sounds. Thus the disease is often attended by paroxysms of irregular and spasmodic breathing, accompanied by violent muscular efforts and great distress, and of course producing other and more obvious sounds than those described. There is often also present in the air passages, either above or below the glottis, a quantity of mucus, giving rise to a constant or occasional rattling, which seems to mask the proper sound of croup. These adventitious sounds, being also as frequently heard in the other forms of croup, are therefore of no service in diagnosis. Generally there are intervals of relief from these superadded symptoms, especially immediately after vomiting or bleeding, but the essential breathing of the disease will be found to be unchanged and unmitigated in these intervals of ease; although the apparent relief may be so considerable as to give rise to strong, but fallacious hopes of recovery.

We occasionally hear, in cases of considerable enlargement of the tonsils, a kind of breathing which closely resembles the early breathing of croup. Usually in such patients the respiration is loud, sonorous, unequal, and irregular, but in a few it is quiet, steady, with a muscular effort occasioned by a mechanical obstruction like that in croup. The distinction between them can, however, be readily made, by attending carefully to the seat of the obstruction, which is above the rima glottidis in the one case, and at it in the other; by the sound of the cough and voice, which are not croupy, and by the fact that the obstruction varies in degree and sometimes vanishes, with change of position.

I have endeavored to describe the respiration as it exists in its slightest appreciable degree, at the earliest period of its manifestation. As the disease advances, it becomes very strongly marked, whilst the condition on

which its peculiar character depends, viz., a mechanical narrowing of the orifice through which the air passes, becomes much more obvious.

The muscular effort, in the latter stage, becomes very strong, both in inspiration and expiration. During inspiration, whilst all the muscles concerned in it are in the highest state of activity, the mechanical impediment against which they act, is often strikingly displayed by the falling in of the soft parts about the neck and clavicles, at the epigastrium, and between and along the lower edge of the ribs—the air not passing in through the narrowed opening of the glottis so rapidly as the dilation of the chest by the increased muscular effort would render necessary. The expiration is chiefly characterized by the amount of force employed to expel the air. In health the expiration is easy, and accompanied by little effort. Where there is an unusual obstruction, the mere tendency to collapse of the lungs would be sufficient for the expulsion of the air, as we see in the dead body; so that the walls of the chest have merely to follow up this contraction, without adding to its force by any muscular effort. But in croup, this is not enough; and we often find that the air is blown out forcibly against the mechanical resistance occasioned by the disease. We find the same strong contraction of the muscles concerned, especially of the abdominal muscles, which is observed when air is blown out forcibly through a narrow passage.

This is the proper breathing of croup; becoming more and more intense as the disease approaches its termination, till the whole life of the individual seems, as it were, to concentrate itself in this one effort. The patient in this extreme condition seeks, by a multitude of changes of place and position, to find some alleviation of his agony; the cough, and with it the voice, have become nearly extinct; and his inarticulate appeals and beseeching looks for relief to those from whom he is accustomed to look for it, constitute one of the most touching scenes which we are called upon to witness in the practice of medicine. Happily the extreme suffering usually, though not always, subsides towards the close of life, and death takes place at last with comparative ease.

In the advanced stage of croup, the breathing is often modified by circumstances other than the mere mechanical obstruction at the upper part of the larynx. After a certain period the false membrane is in some places separated from its adhesion to the mucous surface, by the secretion of pus. The passage of air to and fro, and the efforts of coughing detach it partially from its adhesion, and break it up more or less into shreds, which however still adhere at one of their ends. These rugged portions of membrane, mingled with the pus, move up and down the air passages, causing some variety in the sounds and also in the actual difficulty of breathing. Death is sometimes very suddenly produced by a collection of this material into a mass which becomes impacted in, and thus plugs up, either the upper or lower part of the larynx. This at least, from the state in which the parts are found on dissection, would appear to be the mode in which death takes place.

The respiration may also be modified in croup from a congestion or inflammation of the lungs, which occasionally supervenes. The embarrassment of respiration has also sometimes appeared to be increased by an accumulation of air in the lungs, which arises from a deficient balance between inspiration and expiration. Owing to the greater ease with which we can make extraordinary and continued effort of inspiration than we can of ex-

piration, a greater quantity is admitted than can be readily expelled, before the suffocative feeling of the patient impels him to a new effort for relief.

But although there may be a combination of the respiration of this disease with that produced by other affections of the throat or lungs, yet the respiration of croup is in its nature and character essentially distinct from them. In them the difficulty of breathing, and the unusual muscular effort, may arise from a variety of causes, producing great varieties in the modes of dyspnœa; in croup, the one essential condition is the mechanically contracted state of the passage through which the air passes, and all the peculiarities of the dyspnœa proceed from this condition. In one particular, the breathing of asthma resembles that of croup, viz., in the intensity of the effort by which the current of air is made to move in both directions against a mechanical resistance; but the point of the resistance, and consequently the other circumstances of the function, prevent the resemblance from extending to other points.

The *first* form of croup, then, is distinguished by the cough, the voice, and by a peculiarity of the respiration which I have attempted to describe, and which, for the sake of distinguishing it in this essay, may be called *intense*.

In the cases of the inflammatory croup, which constitute the *second* form of the disease, the condition of the voice, cough and breathing, are precisely the same as in the cases of the first class. There is no certain way by which, so far as these symptoms are concerned, cases of the one kind are to be distinguished from those of the other. The cases enumerated among the second class, were of all degrees of severity, but not one of them was fatal. Cases, however, of croup which terminated fatally, and in which no membrane was found on dissection, are recorded upon the best authority. To those we shall have occasion to advert hereafter. In addition to the symptoms proceeding from the character of the cough, voice, and respiration, I have noted, in a few examples of this form of the disease, a tenderness of the larynx on pressure.

As cases of this class are then usually favorable in their termination, whilst those of the first are usually fatal, the diagnosis between them, in the early stages especially, becomes of very great importance, both as regards prognosis and treatment. Of the means by which this distinction may probably be made, and of the grounds for believing these two to be essentially distinct diseases, and not different states or conditions of the same disease, I shall take occasion to speak, after considering the other two classes which have been enumerated.

The *third* includes certain cases, generally designated as *spasmodic croup*, and sometimes as *spasmodic asthma*. The attack is always sudden, and usually occurs after the subject has been, for some time, asleep. Very often it occurs in the evening, during the first sleep of the child, before its parents have retired to bed; but perhaps as frequently at a later hour of the night, or very early in the morning. The patient wakes in great distress for breath. His inspiration is attended with great effort; it is loud, ringing, shrill, somewhat resembling the hooping inspiration of hooping cough, but louder and more sonorous. The expiration is comparatively quiet and easy. The voice at the same time, is hoarse and broken, and there is a loud, hoarse barking cough, which closely resembles that of the preceding kinds, and indeed alone, would not serve as a mark of distinction from them. These

cases seem occasionally to arise from indigestion; but more frequently we can trace their occurrence to cold, especially as they have been often preceded for a few days by symptoms of catarrh. When left to themselves, they will usually subside spontaneously, but from their suddenness and violence, they cause great alarm, and call for immediate assistance. They rarely fail to yield to an emetic or venesection, leaving behind them for a longer or shorter period, rarely for more than twenty-four hours, some hoarseness and some degree of the croupy sound of the cough, with a little huskiness or stiffness of breathing. At no period is there any proper *intensity* of respiration.

These cases, from their suddenness, the time of the attack, the great violence of the first symptoms, and the consequent alarm which they create, produce a stronger impression on the minds of common observers, and even of many practitioners, than those of the other kinds. This mode of attack is most closely associated in their minds with the term croup; and it is regarded as tending, if not checked, to terminate in the same state of things with cases of the first class. So far as the cases before us are concerned, however, this never happens, and of the whole number included under this examination, no one proved fatal.

The *fourth* class includes cases not falling under either of the above, and yet frequently presenting a very close resemblance to them. The subjects usually exhibit at first the symptoms of common catarrh. After a few days, the voice becomes hoarse; the cough becomes croupy, and there is tightness, oppression, and some approach to the croupy sound of respiration; there is, however, no intense or exalted action of the respiratory muscles, and no indication of that mechanical impediment to the current of air which exists at the rima glottidis in the two first forms of the disease. Still the resemblance is quite close enough to cases of the same forms, in their earlier stage, to occasion some anxiety, and there is also sometimes a sudden attack of dyspnoea, with loud, shrill and sonorous breathing, which imitates the symptoms of the third form, and is perhaps to be regarded as an attack of the same kind.

The cases of this form yield gradually, the croupy character wearing off in a few days, and leaving behind simply catarrhal symptoms. I suppose them, from the mode in which they come and go off, to be properly a catarrhal inflammation of the mucous membrane covering the organs of voice. We frequently observe that the catarrhal affection of the same membrane which occurs in the first stage of measles, is accompanied by the same croupy symptoms as those which have been now described—going off with the other catarrhal symptoms. In a few instances the attacks of this form of croup have terminated in severe bronchitis, or in inflammation of the lungs themselves. But among the 56 cases included above, there was no one fatal.

Having thus described these several forms of this disease, and stated in general what seemed to be their nature, the question now arises as to the justice of the distinction which has thus been assumed to exist. Is there any sufficient ground for such a distinction? Are these different cases different diseases? Are not the favorable ones, which constitute so large a proportion of the whole number, similar in their nature to the more severe; but either of less severity in their origin, or else modified and controlled in their course, by the influence of treatment? These questions, it

is obviously of great importance, to the prognosis and treatment of the cases in question, to be able to answer correctly. If we can, with regard to a large proportion of them, confidently predict from the outset a favorable issue, the practitioner and the friends will be saved much unnecessary anxiety, and the patient many annoying and debilitating remedies.

I proceed, therefore, to state the grounds for a belief that the first form of croup is a disease essentially distinct from all the others, and that it depends on a peculiar pathological condition to which they have no tendency. Whether there be any equally marked distinction between the other forms, it is not of the same practical importance to determine; and as we have no sufficient materials for a satisfactory inquiry into this question, our attention will be confined to the evidence for the distinct character of the first form.

Every physician is familiar with an affection of the throat, both in adults and children, consisting in an inflammation of the mucous membrane, of that peculiar character which produces the effusion of a layer of coagulable lymph, or false membrane. The connection of this affection of the throat with croup, was long since pointed out; and it is well known to practitioners among us, that this complaint, known familiarly, though inaccurately, under the name of "ulcerated sore throat," often accompanies or is followed by croup, and that croup thus connected is peculiarly fatal in its character. This circumstance in the history of croup, was many years since strongly impressed upon my mind by an eminent practitioner in this neighborhood.* I was in consequence led, in all cases of croup, subsequently to this period, to make a careful examination of the fauces, with the view of determining exactly the extent to which this visible affection of the throat was connected with the more important disease.

Two causes prevent the completeness of these observations. We are very apt, in making record of cases, especially of those which appear of a slight degree of severity, to omit the *noting* of negative facts, even when they have been actually the objects of attention. Hence, although I have very rarely failed to examine the fauces in any case of supposed croup, I have often in the lighter cases, and sometimes in the severer, failed to note their condition. The second cause of incompleteness is the impossibility in some patients, from their terror and consequent resistance, of getting such a view of the parts as would authorize us to pronounce decidedly what their state is. Notwithstanding these circumstances, the state of the throat has been noticed and recorded in a sufficient number of cases, to afford very fair materials for inference.

With a view to this examination, I may include a considerable number of other cases, besides those which constitute the particular subjects of inquiry in this paper, which have been noticed at other times, or in the practice of my friends. Including these cases with the 22 above referred to, I have memoranda, more or less complete, of 39 cases of what I have denominated membranous croup. The state of the fauces was observed and noted in 33, and of these, in 32 a false membrane was present; most frequently, and sometimes only on the tonsils, sometimes on other parts also, as the palate, uvula, and pharynx. In one case, no such membrane was present; but it was found to exist in the larynx after death. In three of

* Dr. William J. Walker.

these 33 cases, recovery took place; all the others were fatal. In 14, an examination was made after death, and the usual appearances were found to exist in all of them.

On the other hand, I have memoranda of 109 cases of what I have classed as the other forms of croup, and of these the state of the tonsils and fauces was noted in 45. In no one was there such a condition of the parts as was found to exist in the membranous form. In three cases there was indeed, a thin, slight exudation on the tonsils, of the color and appearance of starch, like that which is sometimes seen on the edge and surface of the tongue. This I apprehend to be a formation of an entirely different nature from that which exists in the other class of cases. Of the 45, 12 were of the second, 11 of the third, and 22 of the fourth class.

From this statement, it seems probable that the appearance of a false membrane upon the tonsils or other visible part of the throat, in a case of croup, may be regarded as a pretty certain diagnostic sign that it is the membranous form of the disease; and its absence as a pretty certain indication that it is one of the other forms. Still there will be exceptions. There will be cases in which the membrane is formed in the larynx, although it has not appeared in the throat; and there may be those in which a membrane exists in the throat, unaccompanied by a similar condition of the air passages. Of the former, I have recorded one example; of the latter, none. How frequent such exceptions will be, must be determined by more extensive observation. If they are not more frequent than they have been among the cases here recorded, the observation of this symptom will afford a sufficient safe guide, since of 75 cases in which it was looked for, and the result noticed, it failed as a diagnostic sign in but a single instance.

The question now presents itself, what are the grounds for believing that the two forms of the disease which I have distinguished as membranous and inflammatory, are not the same in different degrees or in different stages? and may not pass one into the other? The grounds are—

1. The very great preponderance of fatal results in the membranous croup, and a similar preponderance of recoveries in the inflammatory, and the evidence which exists that in the few cases of recovery from the former, the membrane has been formed, and in the few cases on record of death from the latter, that a membrane has not been formed—afford strong reason for believing that the diseases are essentially different.

2. The formation of a false membrane does not seem to require either an advanced stage or a very intense degree of the inflammation from which it proceeds. It is rather the result of a peculiarity in the kind of inflammation, than of any period or degree of it. It appears to be a very early product of the inflammation, if it be not indeed almost contemporaneous with it. It resembles in this respect the similar effusion taking place on the serous membranes, which in them occurs very early, and has ever been supposed to be the first act of inflammation. In the common inflammation of the tonsils which is accompanied by this symptom, a layer of lymph is observed to be effused over the surface of the part as soon as any signs of disease exist.

3. The circumstances attending recovery from simple inflammatory croup differ materially from those which accompany recovery from membranous croup. In the former, the amendment is rapid and speedily completed.

There is left behind only a moderate soreness of the larynx, and, in the worst case, some hoarseness. There is at no time any copious or solid expectoration. In the latter, recovery is slow, unequal, and accompanied by phenomena which must necessarily attend the separation of the membrane, and the process through which the diseased mucous surface must go in order to its restoration to a healthy condition. The natural cure of the disease takes place by the occurrence of the suppurative inflammation upon the diseased surface, by which the false membrane is thrown off, and the mucous membrane then gradually returns to its natural state. In examinations after death, we usually find that this process has begun in the trachea, the membrane being there separated and often broken up into shreds, whilst the inflamed surface is covered by a layer of pus. Above, in the upper part of the larynx, around the glottis, the false membrane usually remains closely adherent. It is obvious that recovery might always take place, could the parts be spared long enough from their functions to go through the necessary steps—and it is also obvious when it does take place, that it must be accompanied by a copious expectoration of pus, and of the membrane either in pieces, if firm enough, or else broken up and partially dissolved by the pus. Now these appearances do not accompany recovery from even the severest cases of the inflammatory croup, whilst they do accompany recovery from well-marked cases of the membranous form.

Of the three cases of membranous croup which are noted as having recovered, there are but two of which I have such an account as would justify me in presenting them as fair examples of the processes through which the parts pass in recovery. These were both of the most decided character, and had arrived at that stage of the disease in which we expect a fatal event to occur almost from hour to hour. In the first of them, six days elapsed before any sensible mitigation of the symptoms, and even then the progress to recovery was very slow, and apparently doubtful. Improvement was attended by a copious muco-purulent expectoration, in which it is true no large pieces of membrane were ever detected, but of such a consistence and appearance as would favor the belief that the membrane had escaped in a comminuted or partially dissolved state. After the probable removal of the membrane, there was for some days a bloody expectoration, the voice did not return, and it was indeed many weeks before it resumed its natural tone.

In the second case, a considerable portion of the membrane was spit up in a tubular form, after a violent fit of suffocative cough, and this was followed by the rejection of smaller pieces, mixed with a muco-purulent, at first, and then a bloody expectoration. There continued an entire loss of voice for more than a week, and for at least ten weeks after recovery, it had not regained its natural tones.

The contrast is very striking between the protracted character of these recoveries, and the speedy return to health of all those who labored only under the other forms of the disease, however severe.

The observations to which the preceding remarks relate, were all made in this city and its immediate neighborhood; how far they correspond to the disease as it appears in other places, must be left to others to judge. So far as they go, they appear to me to justify the following conclusions:—

1. That the only form of croup attended with any considerable danger

to life, is that which is distinguished by the presence of a false membrane in the air passages.

2. That the existence of this membrane in the air passages is in a very large proportion of instances, indicated by the existence of a similar membrane in the throat.

3. That this affection differs not in stage or degree, but in kind, from all the other cases which are commonly known by the same name, and that the latter have no tendency to become converted into or to terminate in the former.

As my intention has not been to write a complete history of croup, I have omitted all such notices of the symptoms, cause, morbid anatomy, &c., of the disease as have no direct bearing on that point in its character which it was my desire to illustrate. It may not be amiss, however, to record, in connection with this paper, a few circumstances with regard to its history, which have been incidentally determined from an examination of the cases before us.

Croup is often regarded as a disease which attacks suddenly and violently. This is only true of the milder forms. Genuine or membranous croup is commonly rather gradual in its approach, and consequently often insidious. It supervenes often on the common sore throat of children; and in such cases, though its development is frequently rapid and apparently sudden, yet a careful examination of the past history of such a case will generally satisfy us, that although it may have had a sudden outbreak of violence at the time it was supposed to begin, yet that it had really been coming on for several days. Of 30 cases in which I have had an opportunity of determining the mode of attack, in only two could it in any proper case be called sudden, although in many, the attention of friends was called to it quite unexpectedly, by a rapid increase in the violence of the symptoms. A sudden and violent attack is, therefore, to be regarded as affording a favorable indication of the character of the case in which it occurs. The unexpected manner in which croup sometimes steals upon the common sore throat of children, should lead always to the careful inspection and watching of such cases. It is true that but a very small proportion of them do terminate in this way; but as it is the only considerable source of danger, and the only way in which they are likely to have a fatal termination, the possibility of such a course of things should not be overlooked. No case of this kind can be regarded as entirely safe from such a result. The danger is even not confined to childhood. Two of the above-named cases of fatal croup, occurred in females of 12 years of age, in which it had supervened on this affection of the throat.

The membranous croup also sometimes occurs as a sequel to the affection of the throat in scarlatina. The most common primary affection of the throat in this disease, is of the same kind with that denominated the ulcerated sore throat, viz., an inflammation, with an effusion of false membrane upon the parts inflamed. When croup supervenes upon this, the case is usually very rapid, and invariably fatal. Of the cases above enumerated, two were of this character. A third occurred to me, not enumerated among them, in which there was no symptom of croup during life, the patient apparently dying from affection of the brain, but in which the usual appearances of croup were found after death. The subject of this was a young man 17 years of age. These cases all occurred between

eight and ten years since. None have been observed during the more recent periods of the prevalence of scarlatina.

Croup varies considerable in its duration; I mean its duration after its characteristic symptoms are fairly developed and there is reason to believe that the membrane is formed. Of 23 cases,

	1	continued 1 day from distinct croupy symptoms.
6	"	2 to 2½
9	"	3 to 3½
3	"	4
1	"	5
1	"	9
1	"	11
1	"	19

Nineteen cases, or more than three-fourths, therefore, were of four days' duration, or less. * * * * *

II.—*Treatment of Croup.* Read before the Boston Society for Medical Improvement, by JOHN WARE, M. D., Nov. 11, 1844.

The history of the case of croup reported at the last meeting,* which I had an opportunity of witnessing during its progress, has confirmed me in an opinion I have, for some time, been disposed to entertain, that the methods of treating this disease in common use, require a careful re-consideration. This opinion is connected with, or perhaps has proceeded from, certain views concerning the distinctive character of various forms of disease which ordinarily are included under this one common appellation, and which I have formerly communicated to the Society. It is not too much to say, that the received mode of treating these cases, which, so far as I know, is very much the same for all their varieties, has come down to us by a sort of tradition, from our predecessors. It is true that in single cases and by particular individuals, there have been occasional variations from the established practice; still in the main, emetics and bleeding, blisters and calomel, have been the principal remedies. The depleting, reducing and perturbing methods is that on which dependence has been chiefly placed.

That this treatment may be applicable to a very considerable proportion of the cases which pass under the common denomination of croup, I am not prepared to deny. Those which in a preceding communication have been classed as inflammatory, spasmodic, and catarrhal, certainly recover under its influence, and apparently with greater speed than if left entirely to the resources of nature. So far as my experience has gone, however, it has appeared to produce no impression upon those in which there is satisfactory evidence that a membrane has been formed.

These cases, I should repeat the opinion expressed in the paper just referred to, are essentially of a distinct nature from the others, and constitute but a small proportion of those which are usually regarded as croup.

* This was the case of a child with membranous croup, communicated by my brother, Dr. Charles Ware, of this city, in which the anodyne treatment was mainly employed, and in which the membrane was separated and thrown off. Every thing promised favorably for recovery so far as croup was concerned, but the patient died ultimately by the rapid supervention of inflammation of the lungs.

They are not aggravated cases of the same kind as the others—cases which have gone on to an ulterior stage of disease—but in their origin and conception different. The inflammation which is essential to them is peculiar in its character; the effusion of false membrane is not the result of an advanced stage of it, but is one of its early results—is perhaps the first visible act of its existence; as there is much reason to believe that it is of serous membranes. It has been common to describe the stage of effusion in croup, as preceded by one of longer or shorter duration—a formative stage. If I am right in the views taken of the character of the disease, this distinction is made by making up its history from different sets of cases—going to one for the history of the first stage, and to another for the history of the second. The same confusion of diagnosis has given also an apparent success to means used for treatment. Where all the different cases which have been referred to, are grouped together as examples of the same disease in different stages or degrees, the proportion of recoveries will not appear discouragingly small. If we were to class together, as cases of consumption, all those in which there was cough and expectoration, as is done by those who profess to cure this malady, we should have no reason to be disheartened with regard to its curability; and, in the same way, so long as we class all cases together as croup, which have a croupy cough and some difficulty of breathing, the amount of mortality will not be greater than in other acute diseases of children. A more accurate diagnosis will, I am convinced, put an end to our complacency on this point. Membranous croup unquestionably does sometimes come to a favorable termination; but recovery is comparatively so rare, it forms so much the exception, that, admitting the distinctive character of the disease, it is difficult to conceive that the treatment has any thing to do with the recovery. Where, under any given method of treatment, but one case out of six or eight recovers, one must be very sanguine indeed to attribute much influence upon the result of the remedies.

The question then properly arises—if the mode of treating croup commonly adopted does no good, are we sure that it does no hurt? This is a question we are far too unwilling to put to ourselves. What will happen if nothing be done? This should always be the first thought of the physician, in each individual case. Till he knows this, he cannot know with certainty what effect his treatment has; and just in proportion to the amount of his knowledge of the natural history of disease, and of the time and mode of its natural termination in recovery or death, will be his power of judging of the influence of treatment upon the result.

Now when we examine the cases of recovery of membranous croup which actually take place, and compare them with the condition of the parts in those which are examined after death, we find very clear evidences of a tendency in the disease to go through a certain course of changes which will terminate in health. The false membrane is effused, and, at the same time, the mucous membrane is thickened and congested. After a time, a process of suppuration is established upon the surface of the mucous membrane, underneath the false membrane, which of course separates the latter from the former, so that it lies loosely upon it, whilst between them is a layer of pus. If the membrane thus thrown off be thick and strong, it is expectorated in distinct pieces, sometimes of a considerable size; if it be thin and less firm, it is either converted partially into pus, or

else is broken up into smaller shreds and mixed with the pus so as not to be distinguished from it, except by very careful examination, and thus it is all gradually thrown up. The diseased membrane does not free itself from the false membrane over its whole surface at once. Those portions from which the false membrane has separated, are left in an inflamed and irritable state—the expectorated membrane and pus are often tinged with blood, probably from the fact that by the violent effort of coughing some portions are torn off from the mucous surface before the purulent process had effected a complete separation. The cough, then, with more or less expectoration, and a hoarseness, in some cases amounting to an incapacity for speaking except in a whisper, continue for some time—the affection of the voice for several weeks. The parts are at length, however, perfectly restored.

In cases which prove fatal, we find evidences that the same succession of changes is taking place; that an effort has been making to bring about the same result. It is in fact from the examination of the progress which has been made in fatal cases, that we are enabled to judge what is the exact condition of the parts, and what the processes through which they go, in those which recover. Thus in some portions of the organ affected, we find the false membrane very closely adhering to the mucous, whilst the latter is reddened and thickened. This especially occurs at the top of the larynx. Lower down the false membrane is more or less extensively loosened from its adhesion—usually irregularly so—whilst a layer of pus lies between it and the mucous membrane. In some places the effused coat has been entirely separated, and has been either spit up, or else is found loose, enveloped in pus, in some part of the passage; whilst the surface to which it adhered is red, swollen and besmeared with pus. Thus we trace every where distinctly the existence of a process, the tendency of which is obviously to bring about recovery; but death has taken place before it has been completed. It takes place in different steps of the process. Sometimes quite early, before any separation has taken place, the patient apparently dying from the diminished aperture of the air passages from spasm and inflammation. Sometimes later, when the separation has taken place below, but not at the top of the larynx. At other times the membrane separates in considerable quantities, becomes collected into considerable masses, and produces suffocation by being wedged in at the bifurcation of the trachea or at the very top of the larynx. There are other cases in which recovery is also obviously taking place from croup, but in which death occurs from the supervening of secondary disease in the lungs.

Croup, when once established, can then only be recovered from, by going through with this regular course of changes. These are essential to it. When once this process has begun; when the false membrane has been fairly effused, the parts can no more recover without them than the eruption of small-pox can be cut short in its progress. A rational method of treatment, then, is that which will promote the necessary changes. And what do we need? 1. To prolong life, to prevent suffocation, in order to give time for the required process to be completed by the efforts of the organs themselves; and 2. To use means which will promote and hasten this process—which will aid the system in the work which she is aiming to perform.

Now are the usual means likely to answer these purposes? Have they

answered these purposes? That emetics and bleeding sometimes relieve violent turns of dyspnoea, must be admitted; yet that they actually prevent suffocation in many cases, admits of very great doubt. But do they contribute at all to those changes upon which alone we can depend for actual recovery? There is no evidence that they do; whilst on the contrary there is reason to fear that they may interfere with them, may retard them, may prevent them. If, then, these remedies be at best of doubtful efficacy, is it not right, in so formidable a disease, to make the trial whether other measures may not be more successful? At any rate, if other means are not more successful, they may at least be less tormenting to the patient, and inflict a less amount of unnecessary suffering.

It is to be remarked of the case which has suggested these observations, that the subject of it rejected all remedies, so that it was in fact a case left very much to the resources of nature. Still, so far as the morbid condition in which croup consists is concerned, recovery was very fairly taking place, and would have been complete, except for the occurrence of a secondary affection. I may say also of the very few cases which I have seen completely recover by the expectoration of the membrane, that they were not the subjects of very active perturbing treatment, especially after the first stages had gone by, but were left a good deal to palliatives—to mild, soothing applications. It would seem worth while, therefore, to make the attempt of treating the disease without the persevering use of the heroic remedies by which it has been ordinarily encountered; that we should—not perhaps leave the disease wholly to nature—but trust it at least to such remedies as will not interfere with that regular course by means of which nature is always attempting to give relief.

III.—*Further Remarks on the Treatment of Croup.* Read before the Boston Society for Medical Improvement, Feb. 20, 1845.

Some remarks were presented to the Society a few months since, on the treatment of croup, including suggestions concerning the management of that form of the disease which is attended by the formation of a false membrane in the larynx and trachea. A case of the disease has since occurred to me, which seems to be worthy of notice in connection with those remarks.

The subject was a male, 5½ years of age; of pale and delicate aspect, and slender habit. He had not been perfectly well since an attack of scarlatina, two years ago; since then, he had been frequently liable to colds, with severe coughs. He had enlargement of the submaxillary glands and of the tonsils.

He was first seen on Sunday eve, Feb. 9, 1845. The account given by his parents was, that he had had a cough with a croupy sound—a sound with which they were familiar—for ten days past; but with it no trouble in breathing; that to day, however, his voice had become hoarse, and that he had several turns of hard, suffocative breathing. The cough and respiration were at this time distinctly those of croup, though at the time of the visit there was no distress. There was false membrane on the tonsils. He had taken an emetic of ipecacuanha and a dose of castor oil.

He was directed to take, once in three hours, 1½ grains of Dover's powder and half a grain of calomel—to sponge the neck frequently with warm

water, and to apply to it this liniment—R. Olei oliv., ℥j.; aquæ potass., ℥ij.; ungu. hyd. fort., ℥j. M.

Feb. 10th.—The night had been easy, upon the whole, though there had been several turns of distress. During one of these he took two drachms of wine of ipecac., with free vomiting. The symptoms of membranous croup were perfectly well-marked, but there was no distress. The liniment was continued, a flax-seed poultice was applied to the neck, and the powders continued every two hours; to be suspended, however, if he became fully opiated.

During the day the voice became quite extinct; and the cough lost the loud and ringing sound which it presents in the early period of this disease. The breathing became more labored, and was accompanied by greater muscular effort, both in inspiration and expiration. Still he was not distressed, owing apparently to the influence of the opium. The air entered the lungs well. There was much sound of loose secretions in the larynx and trachea, but no expectoration, except of a little frothy mucus. It having been found difficult to keep the poultices in contact, the parents substituted boiled mullein leaves, which were assiduously applied. At the same time the patient was made constantly to inhale the vapor from a boiling decoction of the same plant, and this was persevered in uninterruptedly for several days.

It is not necessary to follow up a detailed history of the case. These measures were continued without change for several days, i. e., the poultice, the liniment, the inhalation, and the calomel and opium in sufficient quantities to keep him under a moderate narcotism.

On Feb. 12, Wednesday, there had been no distress of breathing; but its croupy character still continued; there had been no return of natural voice; but the sound of the cough had changed, and was like that of common catarrh—quite loose. Through Wednesday and Thursday there was much rattling of loose matter in the larynx and trachea, and it was coughed up in considerable quantities. Portions of the sputa were mixed with blood, and false membrane was detected in detached pieces enveloped in mucus and pus. One portion of it was of considerable size and distinctly tubular. The fits of coughing, especially when masses of false membrane were ejected, were suffocative, and the sputa were dislodged with difficulty. On Thursday there were still a large thick patch of false membrane on the tonsils. He was occasionally delirious. The pulse were about 120; the respiration varied from 12 to 20, and continued distinctly croupy, though without any distress. He was extremely prostrated.

On Saturday the respiration had lost the croupy character, but there was still a loose rattling sound in the air-passages, and the voice was unchanged. This day, for the first time, he manifested a little appetite, and his tongue became clean. He had continued occasionally to throw up pieces of false membrane.

On Monday, Feb. 17, he appeared perfectly well, except as to strength and voice. By considerable exertion he could make a slight approach to proper voice, but for the most part he spoke in a whisper.*

* This patient has had no return of the disease to the present time, March, 1850. His voice was not perfectly restored for many weeks.

The important point to determine in connection with this case, is how far recovery depended upon the treatment. The treatment consisted—

1. In the absence of all reducing, depleting, and disturbing remedies.
2. Keeping the patient under the full influence of opium, combined with calomel.
3. Constant external application of warmth and moisture, and of a mercurial liniment slightly stimulating.
4. Constant inhalation of watery vapor.

It is too much to say, that the recovery in this case was to be attributed, with any thing like certainty, to the mode of treatment employed. It may have been only one of those coincidences which so frequently mislead us in studying the effects of remedies. Still, as the expectoration of the false membrane has not been a very common occurrence under my observation, and recovery not universal, even where it has taken place, it will be at least useful to notice the circumstances which have accompanied a favorable case.

On the supposition that the successful result may have been connected in some degree with the treatment, I should be disposed to attribute it to the following circumstances:

1. To the absence of all such measures as tend to irritate the parts inflamed, and thus to interfere with the natural process of restoration—especially vomiting. That vomiting gives relief to the paroxysms of bad breathing in croup, will not be doubted; and so does it give temporary relief to the distress of an inflamed stomach. But relief of a symptom is not the cure of disease, and does not always tend to its cure. It is not in accordance with what we know of the effects of remedies in other inflamed parts, that concussion, motion, &c., should allay their inflamed condition. Vomiting relieves inflammation of some parts, and some kinds of inflammation; but in this case the parts inflamed are mechanically disturbed by the act, and it has, so far as we can judge, no probable influence upon that peculiar condition which constitutes the disease.
 2. To the absence of all depressing and debilitating remedies—as bleeding, purging and vomiting, considered in their effects upon the system. Such means may be beneficial when we expect resolution of an inflammation. But where the successful issue of the disease depends upon its going through with a certain course of changes, as in croup, they are as likely to interfere with, as to promote them.
 3. To the relief of the spasmodic contraction of the rima glottidis, which seems more or less to accompany its mechanical diminution by the effused membrane, and to aggravate very much the difficulty of breathing. It is probably upon the suspension of this spasmodic condition, that the temporary relief produced by vomiting chiefly depends, and especially vomiting by means of tobacco.
 4. To the influence of external warmth and moisture in promoting the suppurative process, by which alone the false membrane can be safely separated.
 5. To the constant inhalation of watery vapor. This may have promoted the separation of the false membrane by keeping it from becoming dried by the constant passage of air—and by rendering it pliable and soft, so as to be easily managed and expelled by the organs in the act of coughing.
- These considerations lead to the belief that this method of treating croup

is at least worthy of trial. But even should it not prove more successful, it is certainly vastly more comfortable than the ordinary method. The patient, whose case has been recorded, suffered very little after the first day, even before the extrication of the membrane. Indeed, taking the disease altogether, it was not attended by more distress than accompanies the average of the acute affections of children.

IV.—*Additional Remarks on the Treatment of Croup.* Read before the Suffolk District Medical Society, March, 1850.

Since the occurrence of the case described in the foregoing paper, I have had from various circumstances, fewer opportunities of witnessing cases of croup than in former years, and only five of this form of the disease have fallen under my notice. The three first of these were treated in the method pursued in the case above related.

The first case was that of a male, four years old, who was taken with membranous sore throat, accompanied by high constitutional irritation, Oct. 14, 1845. No croupy symptom occurred till Oct. 18, when they were manifested in a perfectly distinct manner. On the 20th and 21st, patches of false membrane with bloody sputa were raised—and one piece of four inches in length. The raising of the latter was accompanied by a severe and suffocative paroxysm of coughing. On the 22d he died, eight days from the commencement of the disease, and four from the access of croup. The suffering in this case was very considerable, but far less than I have been accustomed to witness in cases of croup treated according to the ordinary method.

The second was that of a female, four years of age, taken with croup on the 8th of Nov., 1845. No depleting or reducing remedies were employed. Patches of membrane, and one piece of considerable size, were brought up on the 10th and a few following days. She never suffered much, improved steadily, and on the 15th seemed well in all respects except the voice, so that on the 16th I did not see her. On the 17th, there was a return of all the croupy symptoms, including the appearance of lymph upon the tonsils, and she died on the night of the 19th, eleven days after the first seizure. During no part of the disease was she suffering from dyspnoea very intense for any continued period.

On dissection, the usual appearances were found, and in one lung the false membrane extended for some distance into the bronchi in the substance of the organ.

The third case was a female, six years of age, who was seized with the disease Oct. 31, 1837. The onset of the disease was gradual, yet quite distinct. Nov. 2d, the symptoms had become quite severe, and Nov. 3d, there was bloody expectoration, and pieces of membrane were spit up. Pieces of membrane continued to be found in the sputa for several days, and she was very comfortable and breathed with tolerable ease, yet never losing the distinct croupy sound of respiration and voice. On the 8th, she became rapidly worse, but without distress, and died on the 9th, quite easily, ten days from the first attack of the disease.

It will be admitted, I think, that these cases, especially the two last, exhibited certain differences from the common course of this disease, which indicated a favorable influence from difference of treatment.

In all of them the membrane was thrown up in considerable quantities.

In all of them the disease was attended by very much less distress than is usual in croup, and, in two, there was so decided a mitigation of symptoms following the separation of the membrane, as to lead to considerable hope of a favorable termination.

In two, at least, the disease was prolonged to at least twice its average duration under the usual treatment.

In the two other cases, to which reference was made, the same general course of treatment was followed, with the addition of the introduction of a sponge wet with a solution of the nitrate of silver, into the larynx. In each of these cases the application was made as early in the disease as I became satisfied of its distinct character. It was repeated morning and evening. It decidedly gave relief to the breathing soon after each application, and both cases ultimately recovered perfectly. For the suggestion and adoption of this valuable addition to our means of treating this formidable disease, we are indebted, as is well known, to the enterprise of Dr. Horace Green, of New York. The profession, I think, owe to him a large debt of gratitude, for the energy and perseverance manifested in the introduction of this remedy, and I am the more disposed to render this tribute to him because so many attempts have been made to detract from his merit in relation to it.

I am well satisfied from what I have now seen of this method of treating croup, as compared with that which has been followed for so many years, that it has the advantages which were pointed out in one of the preceding papers. It is a disease which I would treat without depletion—except perhaps by a few leeches—without vomiting, without purging, without blisters, without antimonials, ipecac., and all those other nauseous remedies which have been usually resorted to. I would trust to opiates, perhaps calomel, emollients, and the local application of the nitrate of silver.

I ought to add that many of my friends in the profession have informed me of cases in their practice, treated on these principles, which have recovered in a favorable manner. Among them I would refer to Dr. Fisher, Dr. Henry G. Clark, Dr. E. H. Clark, Dr. Buckingham, and my brother, (Dr. Charles Ware,) of this city, Dr. Cotting, of Roxbury, and Dr. Spooner, of Dorchester.

EDITORIAL DEPARTMENT.

The Moral Influence of the Study of Practical Anatomy. Rev. Dr. Putnam and the Medical Profession.

In the late effort to obtain a commutation of sentence in the case of Dr. Webster, the Rev. Dr. Putnam, the spiritual adviser of the prisoner, and his advocate before the Governor and Council, speaking in extenuation of the mutilation of the remains of the murdered man, used the following language:—

“That he could devise such a plan of disposing of the body, and then go through it with all the horrid and disgusting operations incident to it, has produced in all minds an impression unfavorable to his character. He cannot complain that it should. Nothing can ever remove that impression.”

“There is but one word of abatement; and because there is no other, it ought to be remembered, viz.: that, to a man of medical education, a lifeless body has not the sacredness which it has to others: to him it is a mere subject to be handled and cut with no more awe or sensibility than the butcher has in dealing with the meat of the shambles. To the habitué of a medical college, the process of dismemberment is associated with the jokes and levities of student life and conversation; and can never be to him the same thing that it would be to another person of no greater moral feeling or principle.”

Error and truth, in almost every human production, are more or less blended. Seldom is a truth uttered without some error, or an error without some truth. In the above quotation there is both truth and error; but the general impression left on the mind of the reader is, without a doubt, both unjust and ungenerous toward the medical profession. So it was considered by medical gentlemen in the city where the remarks were made; and in the Boston Daily Advertiser we find an article, written with much ability and fervor, vindicating the medical profession from the imputations which the quotation appeared to convey, and claiming for the study of anatomy as much exemption from *brutalizing* influences, as pertains to theological pursuits. We would copy the article were it not for its length, and we should do injustice to the writer by the publication of a fragment only of it.

In the same paper, of a subsequent date, the Rev. Dr. Putnam communicates a reply to the article just referred to; and we have seldom read a production which evinced a more candid and manly spirit. As it is short, we shall copy it entire, with the conviction that its perusal will afford our readers the same pleasure which we have derived from it. Under excited feelings, and with the mind bent on a particular object, language may inadvertently be used expressing far more, and perhaps admitting of a widely different construction from that which the author intended to convey. But, unfortunately, persons are not always ready to see the error into which they have been betrayed, and make amends for even an involuntary injustice. To such persons the careful consideration of the letter by Dr. P. may be properly commended.

For the Boston Daily Advertiser.

MR. EDITOR,—In the Daily Advertiser of the 11th (Aug.) inst., a correspondent has offered some strictures on a brief passage taken from the remarks lately made by me to the Committee of the Executive Council in support of Dr. Webster's petition for a commutation of his punishment. He thinks the passage injurious and ungenerous towards the medical profession. Other physicians—friends, whose candor and kindness I cannot doubt—have intimated the same thing to me, though without the bitterness and severity of your correspondent. I am, therefore, constrained to believe, that I have unwittingly done injustice to others and to myself, in what I said in that connection. I perceive the infelicity of my language and its liability to be misunderstood. Instead, therefore, of defending it word for word, I will endeavor to state now what I really did mean to say, and what I did not.

I was suggesting a slight extenuation—all that it admitted of—of Dr. Webster's conduct in disposing of the dead body of Dr. Parkman. His familiarity with the dissecting-room, I thought, must have prepared him to dismember the body, without the nervous horror and physical faintness that would be felt by another man of the same moral grade, but a stranger to medical dissection. I did not mean that the study of anatomy would make a man any more likely to commit a homicide; but I suppose I may say that *if* a man practised in anatomy *should* commit a homicide, with or without malice, and *if* he should decide upon the concealment of the body as necessary or expedient, the bare process of dismemberment would not be so revolting, so impossible a thing to him, as to another of the same degree of moral sensibility and principle. The surgeon or anatomist, of the tenderest feelings, can operate upon a human body, living or dead, with a steadiness of nerve and a cool self-possession of mind, which is only to be had by practice. What would become of the beneficent art of surgery, and the noble science of anatomy, if he could not? I could not make an incision into a human body, living or dead, without fainting away; that fact does not prove me to be humane or moral. The beloved and trusted physician of my family could do it without a qualm. Can any body suppose that I consider that power, which he has acquired by practice, as

derogatory to his humanity or his virtue? the blessed power by which he relieves the suffering and prolongs the life of me and mine!

That the study of medicine, with the anatomical investigations so essential to it, is "demoralizing" in its influence on character, is no sentiment of mine. I never said it nor thought it; and I should not suppose it would be inferred from what I did say, on the occasion referred to. On the contrary, unless I have been singularly fortunate in my medical associations, physicians, as a class, are rather remarkable for humane feelings and tender sympathies. There are among them, certainly, as many men whom I love and revere for all that is most exalted and beautiful in human character, as in my own profession, or any other profession or calling whatever. And it has never yet occurred to me to imagine, that their ability to operate on a living body, or to dissect a lifeless one, with a courage, a steadiness of hand and composure of feeling, which I could not command, is any disparagement of their religious character, or their humane and social virtues.

I fully concur with your correspondent in all he has said, so well, of the nobleness and dignity of anatomical science, and the importance of the medical profession. I only regret, that, either through my fault or his own, he should see fit to place his sound and eloquent remarks in sharp antagonism with me and my words—words spoken in another connection, for another purpose, and with no idea of such a bearing as he finds for them. I hold the medical profession in as high honor as he does—in as high honor as I hold my own, or any other. I have no prejudices, *narrow* or broad, against it, or the admirable sciences which underlie it. I hold Physiology to be as legitimate a science as Theology,—indeed, a splendid branch of it. I assume for clergymen no superiority in virtue, or the sentiments and sympathies of humanity, over physicians. I agree with your correspondent, and am happy to confirm his remark from my own observation, that the physician enters the chamber of sickness, and the scenes of pain, death, and sorrow, with as soft a tread, as gentle a voice, and with as warm a current of affection in his heart, as the clergyman.

The physician and the clergyman, in many of their duties, find their paths run side by side, through scenes of anguish and woe. Each has his part to do. There is no rivalry, and no occasion for mutual disparagement or distrust. They should honor and help one another, as brethren and fellow-laborers in the noble and blessed service of humanity. If the tie that should unite them has been in any degree weakened by any word of mine, honestly misunderstood, or carelessly perverted, I wish that word recalled, as false to my thought, and justified by no feeling that I ever for a single moment entertained.

GEO. PUTNAM.

STERLING, July 12, 1850.

The New Hampshire Journal of Medicine. Edited by EDWARD H. PARKER, A. M. M. D.—This is the title of a new Medical Journal of thirty-two pages, the first number of which was issued in August. It is to be published monthly, at Concord, N. H. The subscription price one dollar per annum.

Address delivered before the Class of the Baltimore College of Dental Surgery, at the conclusion of the last regular lecture of the Course, for the Session of 1849-50. By CHAFIN A. HARRIS, M. D., D. D. S., Professor of the Principles and Practice of Dental Surgery.

Address delivered before the Graduating Class of the Baltimore College of Dental Surgery, at the Tenth Annual Commencement. By ELISHA TOWNSEND, D. D. S.

Valedictory Address delivered before the Graduating Class of the Baltimore College of Dental Surgery, at the Annual Commencement for the Session of 1849-50. By S. P. HULLIHEN, M. D., D. D. S.

Address to the Society of the Alumni of the Baltimore College of Dental Surgery. By JAMES ROBINSON, D. D. S., Dentist to the Royal Free Hospital, &c., London, Eng.

The occasion which called forth the above excellent Addresses, was the Annual Commencement of the Baltimore College of Dental Surgery. This institution, which has been in existence about ten years, is in a highly flourishing condition. So much is it esteemed by the dental profession, not only of the United States, but of other countries, that pupils from abroad are to be numbered among the classes which it annually assembles. How long will it be before the medical schools of our country shall receive a similar tribute to *their* excellence? A quarterly periodical, devoted to dental science and literature, has also been established at Baltimore for several years, published, heretofore, under the auspices of the *American Society of Dental Surgeons*, but, hereafter, as we are informed, to be under the sole direction and editorial management of Prof. Harris. This truly ably conducted Journal, together with the Dental College, have done much to advance the science of dentistry, and to elevate the character of the dental profession. We believe it is conceded that this branch of surgical art has been carried to greater perfection in this, than in any other country; and that, as a profession, dentistry occupies a higher position here—a flattering evidence of the ability and character of a large proportion of American dental practitioners.

The several Addresses whose title pages are given, are alike creditable to the authors, and honorable to the profession of dentistry in this country.

Sleep psychologically considered, with reference to Sensation and Memory.
By BLANCHARD FOSGATE, M. D., Physician to the New York State Prison, at Auburn. Published by George P. Putnam, New York, 1850.

The author of this volume is a physician of eminence, and well known as a scholar. He has contributed many excellent articles to the *American*

Journal of Medical Sciences, at Philadelphia, and to other medical and literary journals. In the present work he has well sustained his reputation, and has made a valuable contribution to American literature.

The subjects discussed are Nervous and Mental Action, Sleep, Mesmerism, Somnambulism, Incubus, Trance, and Catalepsy. Upon each of these separate topics he is original, ingenious, and often conclusive. He is, however, a convert to both Phrenology and Mesmerism, and he has interwoven enough "facts" from each into his arguments and speculations, to create occasionally a doubt as to the stability of the structure which in part depends upon them. Aside of this, however, the book has so many and such unequivocal merits as to commend itself to every intelligent or metaphysically inclined reader.

F. H. H.

Demonstrative Midwifery.—We received, just as we were making up the Editorial matter for this Number, a letter from a distinguished member of the medical profession, a portion of which we cannot resist the temptation of inserting, relating, as it does, to a subject with which, by this time, our readers are somewhat familiar. The letter was not written with a view to publication, and we have only a presumption of the author's willingness that we should make any editorial use of it. Under these circumstances we feel obliged to withhold the name of our highly respected correspondent.

The anxieties and trials of the first case of Midwifery, so graphically and humorously portrayed, in the following extract, are sufficiently common, and hundreds of the most eminent of the Profession could make (if they would) confessions of a similar experience. The following is the portion of the letter referred to:—

" August 26th, 1850.

" I do hope and trust that this method of teaching obstetrics, now fairly begun, will not be abandoned. You have no doubt heard frequent confessions of the embarrassment and fright endured by young men in the conduct of their first case, and perhaps might add one more to the number from your own experience. I shall never forget mine. I was left alone with a poor Irish woman and one crony, to deliver her child. The "*dolores concussentes*" were in full vigor, and I thought it necessary to call up before me every circumstance I had learned from books, as at all possible in this critical moment. I must examine, and I did—but whether it was head or breech, hand or foot, man or monkey, that was defended from my uninstructed finger, by the distended membranes, I was as uncomfortably ignorant, with all my learning, as the fœtus itself that was making all this fuss. To add to my confusion, there were the poor patient and her friend "doctoring" me every minute, and striving to extort

an opinion as to when the woman "would get to bed." But, in spite of my learned awkwardness, she did "get to bed," and the child was born. Fortunately, it was a natural labor. I had no difficulty in tying the cord and removing the infant. And then to hear the blessings showered upon my skill, and the prayers for the mother that bore me! It would have done your seventeen friends good. I assure you, there was no exposure in this case, for if the poor woman had been as nude as a model artist, I could have seen nothing then.

"However, as I said, the child was happily born, and I was overpowered with compliments, lavished with all the volubility of two Milesians. The nurse ran for a half pint, that "the docthur might dhrink the baby," while my doctorship sat in state, grinning from ear to ear, that I had escaped the commission of murder, and in danger of exploding with conceit at my wonderful achievement. But my triumph was of short duration. The nurse began to hint it was time for the after-birth to come away; and I, careful to ascertain that the mystical fifteen minutes had elapsed, made an attempt to bring it, but it would not come. Here was a fix—hemorrhage, hour-glass contraction, inverted uterus, and what not, if I used too much force, and retained placenta with its train of consequences, if I should fail in bringing it away. I pulled all I dared, and the woman became more significantly urgent every moment, until I was in danger of being sacrificed to the cause of "modest" science, when, happily for all parties, my preceptor dropped in, and with five words of *clinical demonstrative* instruction, every difficulty was removed, and I was enabled to complete the delivery. I took some pains to inquire of my fellow students during the remainder of my term of pupilage, if they, in their first case, had succeeded in bringing away the placenta without assistance—the answer was invariably, no, unless the uterine contractions had been sufficient to expel it from the os externum without manual aid.

"Now, if in those days we had had the benefit of Dr. White's *clinique*, what a deal of mental suffering would have been saved to me, and what a serious risk to the poor woman—nay, more—one woman, without the slightest risk to herself, would have been sufficient for the initiation of twenty students; whereas, under the old system it required twenty for the same purpose, and with twenty times the risk.

"If my opinion with regard to this matter, is of any consequence to you, I would, by all means, urge you to persevere. If more things could be physically demonstrated in the various departments of our profession, how incalculably would the science be advanced, and humanity benefitted! Show every thing you can to your pupils. The more numerous the "*oculis fidelibus subjecta*," the fewer will be the "*demissa per aures*." So far from abandoning the system of demonstrative midwifery, I would exhibit every stage of labor as well as the last, as far as is possible. It strikes me a speculum might be contrived that would exhibit every step of parturition, from the first perceptible dilatation of the os tincae to the final expulsion of the foetus. I make this suggestion for the benefit of Dr. White, who is, of course, much more competent to decide upon its practicability than myself.

Very truly, yours, &c.,
 _____"

"Prof. A. FLINT, Buffalo, N. Y."

Theory of the Production of Males and Females.

By SILAS HUBBARD, M. D.

To the Editor of the Buffalo Medical Journal.

SIR,—Many have been the theories of generation which have been either proved to be, or are now regarded as erroneous, and are merely mentioned matters of history. Among these by-gones, are all the ancient theories of the causes of the production of males or females: but as this subject still occupies the serious attention of very many respectable physicians, I may be excused for offering the following new and original theory, viz: that males are begotten from one to ten days before, and females from one to ten days after the courses of the mother. In proof of this observation, I shall now merely say, that it has invariably held true in all the cases I have had the means of knowing, which are half a dozen.

August 10th, 1850.

Injection of the Stomach in Chronic Gastritis.

MR. EDITOR: *Sir*,—I am now making an experiment, that, so far as I know, is new. I am treating a case of chronic gastritis by injecting the stomach with a solution of the nitrate of silver. I began with one grain, of the strongest, to one ounce of water. I intend to increase the strength as long as the patient can bear it; it does well so far. I introduce it by means of the stomach-pump,—I pump the stomach full, and after it remains a few minutes, pump it out again. This, I think, is new—but it must prove serviceable. Will you please give this a corner in your journal?

Yours, respectfully,

M. M. RODGERS, M. D.

ROCHESTER, August, 1850.

A Universal Formulary: Containing the methods of preparing and administering officinal and other medicines. The whole adapted to Physicians and Pharmacutists. By R. EGLESFELD GRIFFITH, M. D., Philadelphia. Lea & Blanchard, 1850. 8 vo., pp. 567.

“The design of this work (to quote the Author’s words) is to present a compendious collection of formula, and pharmeceutic processes, with such additional information as may render it useful to the physician and apothecary; and the principal aim has been to select materials most generally applicable, and of practical utility.”

In the introduction are presented tables and observations on the weights

and measures employed for pharmaceutical purposes in the United States, and in foreign countries; and, also, a copious vocabulary of abbreviations and Latin terms employed in writing prescriptions. The latter are now but little used by practitioners, and, in our opinion, the sooner they fall into complete disuse the better. Still, as they enter more or less into medical literature, it is convenient and important that they should be known, and hence it is desirable to be able to refer to a vocabulary occasionally to refresh the memory. The author has added to the introduction a few observations on the management of the sick-room, and on the administration of remedies, which are judicious and appropriate.

The Formulary occupies about one half the volume. It is arranged alphabetically according to the pharmaceutic names adopted in the United States Pharmacopœia. In each formula the English names for the articles are used, and the quantities are expressed in words, instead of the usual signs. The author thinks that this change, which has been made in France, should be adopted by other countries, believing that it would tend to obviate mistakes in writing prescriptions. We are disposed to concur in this opinion, and can see no occasion for regrets, if the "abbreviated cabalistic terms" now used, were to be discarded.

Following the Formulary are a list of incompatibles; a posological table, or table of doses; a table of pharmaceutical names which differ in the United States, and the London, Edinburgh, and Dublin Pharmacopœies; officinal preparations and directions; poisons; index of diseases and their remedies; index of pharmaceutical and botanical names, and a copious general index.

From the foregoing statement of contents, the reader will perceive that the work is calculated, in several points of view, to prove convenient and useful to the medical practitioner. And, from its intrinsic value, the work derives an interest from the fact that it was the last effort of the author to render useful service to the Profession of which he was an industrious and distinguished member, his decease being almost coincident with its publication. Dr. Griffith made several valued contributions to medical literature, among which is a work on medical botany, which is highly esteemed. As remarked by a cotemporary, he was emphatically a working man, and his loss is much to be deplored.

Homœopathy repudiated in Law.—A case recently occurred in the city of New York, in which the legal merits of homœopathy came up for judicial decision. An Italian opera-singer sued the manager of the opera-

house for a balance of \$100, for his services. To this a defence was made that the plaintiff had incurred a forfeiture of that amount, by failing to perform on a certain evening, and not giving notice of illness under the certificate of the Physician to the establishment, as the regulations required. The plaintiff proved by his own attending physician, who was a member of the regular profession, that on the night referred to, he was really ill and unable to perform. It appeared in evidence that the Physician to the opera-house, whose certificate of illness was required by the regulations of the establishment, was not a regular practitioner. Under these circumstances, Judge Lynch decided in favor of the plaintiff, on the ground that it was not proved that Dr. Quinn, the opera-house doctor, "was a doctor, or that he had taken a degree as Doctor of Medicine, or that he was authorized by the Medical Society, or had a regular license to practise, which was necessary in order to constitute him a doctor." The Judge adds: "So far as there is evidence on the subject, it went to show that Dr. Quinn practises upon principles of homœopathy, and that such practitioners are not recognized by the faculty of medicine, nor by a majority of the public, as regular practitioners." Such being the case, the Judge thought the evidence of illness afforded by the testimony of the attending regular physician sufficient to exculpate from the forfeiture, without imposing on the plaintiff the necessity of getting the certificate of the homœopathic practitioner, although the rules of the house required the latter.

Assimilated Rank in the Navy.—At the semi-annual meeting of the Erie County Medical Society in the State of New York, held in the city of Buffalo, June 12th, 1850, it was,

On motion of Dr. Austin Flint,

Resolved, That this Society recommend to the members of the medical profession of this county, for their signatures, the memorial to Congress in behalf of the medical officers of the navy, praying for an assimilated rank, believing that the action on the part of our National Legislature asked for, is due, not only to the medical department of the navy, but to the character of the medical profession generally.

Resolved, That this resolution be published, and that copies be transmitted to the Representatives in Congress from this District, and to the Senators from this State.

From the regular minutes,

JNO. S. TROWBRIDGE, Sec.

Visit of Prof. Liebig.—It is stated in some of our exchange Journals, that Prof. Liebig is soon to visit the United States, and will probably give a series of lectures in various cities.

Correspondence between Dr. C. J. B. Williams and a Homœopathic Practitioner.

The following correspondence displays on the part of Dr. Williams a line of conduct which every medical man who has at heart the honor and usefulness of the Profession, will not fail to pursue on similar occasions. The courteous tone of his letter, as well as the uprightness of the position he assumes, is worthy of praise. EDITOR BUFFALO MEDICAL JOURNAL.

The Letter.

“~~7~~ Street, Friday, 22d February, 1850.

“DEAR SIR,—I am very desirous of having your opinion in a case of suspected disease of the heart. The patient is the Hon. Mrs. —, at present residing with Lady —, — Square. Will you have the goodness to inform me at what hour on Monday it would be convenient for you to see Mrs. —?”

“I think it right to state that Mrs. — has been for many years a convert to homœopathy, and that I, as you possibly may have heard, practise that system of treatment. I mention this, because you may have some objection to meet a homœopathic physician in consultation, and I should much regret if I were the means of inducing you to do any thing distasteful to you, in ignorance of the above facts. I may, however, mention that it is as a matter of diagnosis rather than of treatment that your opinion is desired, and that my friends, Sir — — and Dr. —, have seen the case with me on former occasions. I remain, dear Sir, your very obedient servant,
“————.”

“To Charles J. B. Williams, Esq., M. D., etc.”

The Reply.

“7 Holles Street, Cavendish Square, 23d, 1850.

“DEAR SIR,—I am obliged to you for your courtesy in wishing to have my opinion on the diagnosis of the case of the Hon. Mrs. —, and for your candor in apprising me that she is under homœopathic treatment; but under these circumstances I must beg you to excuse my attendance.

“Believing, as I firmly do, that the so-called ‘homœopathic system’ is an entire fallacy, and therefore calculated to do much injury to those on whom it is practised, I consider it to be my duty to do nothing that can, directly or indirectly, countenance or aid it; and it appears to me, that to meet a homœopathic physician in consultation, and to assist in the diagnosis of a case professedly under homœopathic treatment, would have such an effect.

“I need scarcely add, that I have no personal feelings in the matter. And hoping that you will soon return to the legitimate domain of rational medicine,

“I remain, dear Sir, yours faithfully,

“To Dr. —.”

“C. J. B. WILLIAMS.”

London Lancet.

New Anthelmintic Remedy.—Dr. Budd, of Kings' College Hospital, London, Eng., has lately employed with success for the expulsion of tænia, a new remedy called *Kousoo*, [*Brayera Anthelmintica*, of the natural order *Rosaceæ*.] A report favorable to the anthelmintic properties of this plant, was several years ago adopted by the Academy of Medicine of Paris, and, also, by the Academy of Sciences.

The parts of the plant used, are the flowers reduced to a fine powder, and given suspended in lukewarm water. The dose is not given in any of the accounts which have as yet fallen under notice. Several cases are reported in the *Medical Times*, in which it appeared to exert a prompt and efficient action in expelling tænia, after other remedies had been administered without effect.

Poisoning with Pie Plant.—The Northern Lancet states, that a family in Keeseville, N. Y., were taken alarmingly ill shortly after eating, as *greens*, the leaves of common Rhubarb, or pie-plant. A servant in the family died the following day; all the others recovered.

In the first Vol. of this Journal an instance is given in which a whole family were made ill from the same cause. Physicians should disseminate the liability of serious consequences from the free use of this plant.

University of the City of New York.—The Chair of Practice in this Institution has been filled by the appointment of Prof. Elisha Bartlett, of Louisville, Ky., who has signified his acceptance. As a writer and teacher, Prof. B. has few, if any superiors, and the University of the City of New York is fortunate in securing his services. The Chair of Surgery is not yet filled. It is understood that Dr. Mott's separation from the Institution is to be final. Since this article was in type, we learn that Prof. Gross, of Louisville, has been invited to fill the Chair of Surgery, and has accepted.

Dr. S. Hanbury Smith, Professor of Medicine in the Starling Medical College, and the Editor of the Ohio Medical Journal, has received the appointment of Superintendent of the Ohio State Lunatic Asylum, at Columbus, in the place of Dr. Awl, resigned.

Catalogue of Medical Department of the University of Buffalo.—In the Catalogue of this Institution for 1849-50, recently issued, the name of a member of the Class was inadvertently omitted. The name referred to is J. S. Jamison, of Canistota, Steuben Co., N. Y.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* By AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

(Continued from page 214.)

Senses and Sensibility. This division will embrace several sub-divisions. Under the head of *Sensibility*, the occurrence of *pain* in different parts of the body will form the chief topic of inquiry. The morbid conditions of the different senses, but, more particularly, the *eye* and *ear*, will next claim attention.

The part of the body most frequently the seat of painful sensations in Continued Fever, is the *head*. I will examine the cases, therefore, first, with respect to this symptom.

Cephalalgia. Of the cases in *private practice*, this symptom is not mentioned in *seven*, and was present in all the remainder, *viz.*, *seven*.

Three of the cases in the records of which this symptom is *not* mentioned, came under observation, respectively, on the fifth, sixth and seventh day after the fever was established. In the remaining *four* cases, the fever was observed from its commencement.

In the *seven* cases in which the symptom was present, it was not uni-

form in degree. It existed in a marked degree in but *two* cases—in the other cases being more or less prominent. It continued from *two* to *four* days, except in *one* case, in which it continued *seven* or *eight* days. In the latter case, it was moderate in degree, and the fever was of a very mild grade of intensity.

In the single case of *Typhus* in this group of cases, which was observed from its commencement, the presence of this symptom is not stated.

In *three* of the *fatal* cases in this group, nothing is mentioned of the presence of this symptom. In the remaining *fatal* case, it existed at the commencement of the disease.

Hospital Cases. Of the cases of the *Typhoid* type, cephalalgia is noted as present in *four* only. In *thirteen* cases, nothing is stated relative to this symptom.

In *ten* of these *thirteen* cases, the febrile career had continued for a period not less than *five* days, before coming under observation. In *one* of these cases, the fever had continued but one day before observed; and in *one* case, it was observed from the commencement, i. e., so soon as the patient was compelled to take to his bed. In *two* of these cases, the duration of the disease before coming under observation could not be ascertained.

In none of the *three fatal* cases, in this group, is this symptom stated to have been present. *One* of these cases entered on the *second* day—the *two* others, several days after the commencement of the febrile career.

In none of the cases of this group in which the symptom was present, did it continue beyond the fourth day.

Of the cases of *Typhus*, cephalalgia is noted as present in but *two*. In *one* of these cases, it existed for the first day only, the case being observed from the time the patient took to his bed. In the other case, it lasted *two* days, the previous duration of the febrile career not being ascertained. Neither of these cases proved fatal.

In *eight* cases of this group, nothing is noted relative to the presence or absence of this symptom. In *two* cases, it is noted that no pain existed. In *two* of the cases in which nothing is stated relative to the symptom, the fever was observed from the commencement of the career.

Of the cases of *doubtful* type, cephalalgia existed at the early part of the disease, in *two*; of the remaining *six* cases, in *two* it is stated that this symptom was not present, and in *four*, nothing is said relative to it. In *one* of these *six* cases, the fever was observed from the commencement of its career; and in *one*, the patient entered on the *second* day. In the other *four* cases, the duration of the fever prior to observation was, in *one*

case, *three* days; in one, *five* days; in one, *eight* days; and in one, *six* days. In the single *fatal* case in this group, nothing is stated relative to cephalalgia, the case coming under observation on the *eighth* day.

These results show, *First*—that cephalalgia, in Continued Fever, is not a symptom incident to the disease after the first few days of its continuance. *Second*—that this symptom is present during the first four days, in a certain proportion of cases, but not invariably. The numerical proportion of cases in which it is present, the cases under examination do not afford the means of estimating, so many of them coming under observation after the period in the disease had passed to which it is limited, and the details of the previous history often being imperfectly obtained. *Third*—it is not a symptom which appears to have any connection with the issue of the disease. *Fourth*—in degree, the symptom is variable, occasionally being intense, but, in the majority of cases, not severe or prominent.

It will, of course, be understood that this symptom is now under consideration as it is presented during the febrile career, *after* the access of the disease. It is much more uniformly present during the access than subsequently.*

In general, it is obvious that this symptom possesses small importance, either in a pathological or diagnostic point of view, in Continued Fever.

* I wish to state that, in conducting the present analytical examinations, my rule is, to obtain results, and draw deductions, before referring to the works of Louis, and others, in order to ascertain the correspondence or disparity of these results and deductions, with those therein contained. When I express general conclusions with respect to Continued Fever, I always mean those only which the data before me authorize me to draw. If these conclusions differ from those arrived at by other observers, and by means of an analysis of a larger number of cases, then it becomes a question how the difference is to be explained. This question, however, it does not enter into the design of this report to discuss. Nor, as will be observed, do I make it a point to state always how my results and conclusions do compare with those previously obtained by others. I have done so, thus far, in some instances, but not uniformly. My reason for not devoting much space to these topics is, that to give them adequate consideration would swell too much, the size of the present report. Besides, the report purports to consist only of observations based on an examination of cases that have passed under my own observation. I am led to introduce this note in the present connection, by having noticed, *after* this Section was written, that the facts developed by the researches of Louis, respecting *Cephalalgia*, differ considerably from those I have presented. In his collection of cases, this symptom was found to exist in a much larger proportion of cases, and to have a longer duration. Why this disparity exists, I am not prepared fully to say, but perhaps it may be explained by supposing that in making inquiries relative to this symptom, Louis embraced the period of the *access* of the disease, which, in the plan I have pursued, was excluded. This would also account for the longer continuance of the symptom in the cases observed by him.

It is highly interesting, after having worked out results, to see how they compare with those which have already become the property of science; and one advantage of repeated analytical investigations of the same disease, consists in this comparison, with a view to investigate the causes of differences which may be found to exist.

When unusually prominent, however, and intense, it may occasion perplexity in the mind of the practitioner. This happened in one of the cases occurring in private practice. The pain in the head, in this case, was so severe for the first three days, being accompanied by increased susceptibility to light and sounds, together with considerable general nervous excitability, that I was not without apprehension lest the affection might be encephalitis. The practical importance of not confounding this affection with fever, is too obvious to require remark, and has been already alluded to under the head of *Delirium*. Fortunately, instances are extremely rare in which there exists much ground for hesitation in the discrimination.

The disappearance of Cephalalgia, even when it had been severe, after the lapse of a few days, is an interesting fact in the history of the disease. I would remark that, in observing cases, it appears often as if this fact were owing, not so much to the cessation of the morbid condition upon which the cephalalgia depends, as on the induction of a mental state, rendering the patient unconscious of suffering from the persistence of that condition.

The cases in which, after the career of the fever was established, *pain* in any other part of the body than the head, was experienced, are very few. The histories of the *private cases* contain no account of any other than cephalalgic pain. It is possible that various painful sensations may have existed, but if any had been present, except in a very slight degree, it is probable they would have been recorded.

In the *hospital cases*—(*Typhoid*)—pain in the limbs was the subject of complaint, in *one* case; in back, knees, and other joints, in *one* case; and pain "all over," in *one* case. *Typhus*—pain in the lower extremities, was complained of in *one* case. *Doubtful type*—pain in upper extremities, in *one* case; in the chest and legs, in *one* case; in the back and left hypochondrium, in *one* case; and in the back and limbs, in *one* case.

It may be remarked, that slight, unimportant uneasy sensations, which might have been mentioned, would perhaps not be recorded; so that in the instances above enumerated, the pains were probably somewhat prominent as symptoms.

In a very large proportion of the cases, especially after the first three or four days, the patients made no complaint—frequently declaring they were comfortable, and sometimes that they felt very well. The latter reply was most apt to be returned by those who were the most seriously ill. I have sometimes noticed that a patient who replies to a general question as to

his feelings, that he is pretty comfortable, on being inquired of more particularly, if he has not some pain somewhere, will apparently deliberate for a time, and afterward refer to some part as the seat of painful sensations. Oftener, after saying he is comfortable, if he be asked, whether he has not pain in the head, abdomen, back, etc., he answers in the affirmative. These observations show that the absence of suffering is due, not alone to the absence of conditions calculated to occasion distress, but to the blunted perceptions incident to the disease.

A blunted perception of morbid sensations was evidenced by various other circumstances; such as indisposition to changes of position, even when the position was such as would ordinarily produce uneasiness; creeping of flies over the face, apparently unnoticed, etc. Circumstances that will be noticed under other Sections, will serve to illustrate the same condition. So that *diminished general sensibility* may be said to belong to the symptomatology of Continued Fever. And it is probably true that, other things being equal, this symptom is, in some measure, proportioned in degree to the gravity of the febrile disease.

I pass now to the *Senses*. And first, the *Eye*.

Eye. The observations respecting the eye, relate almost exclusively to the presence or absence of vascular congestion of the conjunctiva. In *two* cases only was diminished mobility of the retina noted. The tone of expression given to the countenance by the eye, has been already considered, under the head of the *general aspect*. Yellowness of the conjunctiva existed in *one* case. In a few cases it is stated, that, while the patient slept or dozed, the eye-ball was partially uncovered; but pains were not taken to record observations with respect to this point. Increased susceptibility to light is not mentioned, except in a single instance—the instance already referred to, in which cephalgia existed in a severe degree, occasioning the suspicion of encephalitis. As respects *vascular congestion*, of the cases in *private practice*, suffusion, in a marked degree, is noted in *one* case; and in *one* case, the conjunctiva was moderately injected. Both of these were cases of the *Typhoid* type, and in neither did the disease prove fatal. In all the remaining cases, including the case of *Typhus* and the case of *doubtful type*, nothing is stated relative to the appearance of the eye. It is not stated that the eye was suffused or injected, in the instance in which there existed increased susceptibility to light.

Of the *hospital cases—Typhoid*—the eye was suffused in *four* cases. In three of these cases, the degree of suffusion is denoted by qualifying adjectives, as follow:—*slightly*, in *one*; *moderately*, in *one*; and *somewhat*,

in *one*. In *three* cases, it is noted that the eye was *not* suffused; and nothing is recorded in *eleven* cases. It is fair to conclude, that in nearly, if not quite all the cases in the record of which nothing is said on the subject, no congestion existed. Of the four *fatal cases* of this group, in *three* nothing is stated relative to the congested appearance of the eye; and in the remaining case, it was suffused.

Of the cases of *Typhus*, the eye was suffused, or injected, in *six* cases; and in no instance were qualifying adjectives used to denote a light degree of congestion. In *two* cases, nothing is stated relative to the appearance of the eye; and it was *not* suffused, nor injected, in *four* cases. Conjunctival redness was present in the *two* fatal cases in this group.

Of the cases of *doubtful type*, nothing is stated in *five* cases; it is stated that no suffusion existed in *two* cases; slight injection existed in *one* case.

In the use of the terms *suffusion* and *injection*, the former was applied to a watery appearance, with some redness, and the latter to a greater degree of congestion, in which the vascularity was more marked.

A suffused, or injected condition of the eye, thus, is present in a certain proportion of cases of either type of Continued Fever; but the numerical ratio of the cases in which it exists, is not deducible from this analysis; the fact of the absence of this symptom not being noted in a considerable number of cases, although the presumption is, that it was not present in the cases, in the records of which it is not mentioned. The above enumerations, however, show, that it is oftener met with in cases of the *Typhus*, than in the *Typhoid* type; and that it is less in degree in the latter than in the former. The symptom, when present in a marked degree, therefore, has some diagnostic value.

The inquiry arises, whether this symptom be not dependent on the general capillary congestion, which, as has been seen in a preceding Section, is exhibited, in a large proportion of cases, on the face and surface of the body generally. It would be a rational supposition that this was the case; but on comparison of cases, it appears that the two symptoms are by no means uniformly associated. Not only capillary congestion of the skin does not exist in all cases of a congestive appearance of the conjunctiva, but the latter may be absent in cases in which the former is present. Of the *two* cases in *private practice* in which the conjunctiva was reddened, nothing is stated respecting the skin in *one* case, and in the *other* case, congestion of the face co-existed. Of the *Typhoid hospital* cases, in which conjunctival congestion existed, nothing is stated with respect to the skin, in *one* case; capillary congestion of the skin co-existed in *two* cases, and it

did not co-exist in *one* case. Of the cases in which conjunctival congestion is recorded absent, capillary congestion of the skin existed in *three* cases.

Of the *Typhus hospital* cases, in which conjunctival congestion existed, nothing is stated respecting capillary congestion of the skin, in *one* case; capillary congestion of the skin co-existed in *five* cases, and was not recorded absent in a single case. Of the cases in which conjunctival congestion was recorded absent, capillary congestion of the skin was present in *four*, i. e., in all the cases.

Hence, it appears, as already stated, that the two symptoms are not constantly associated. This, however, does not prove that they may not both be effects of the same pathological condition—circumstances, which we are not able to appreciate, occasioning the manifestation of either, exclusive of the other.

Hearing. Bluntness of the perception of impressions received by the sense of hearing, as a general remark, obtains in Continued Fever, after the first few days of the febrile career, if not from the very commencement. This rule, in so far as the present collection of cases is concerned, would seem to be without an exception. In no case is it noted that, after the third day, there existed a morbid susceptibility to sounds. The only instance in which this is stated to have been the case at any portion of the febrile career, was the case already alluded to, in which unusual cephalalgia and increased sensibility to light existed. Merely dullness of hearing, corresponding with dullness as respects other senses, may be considered as belonging in the category with *diminished general sensibility*, and is probably due to a mental condition.

But in a considerable proportion of cases, the sense of hearing is affected specially, and in a degree disproportionate to the state of the mental perceptions.

Deafness, more or less, is noted in the records of *six* of the *fourteen* cases in *private practice*. Of the *hospital cases*, this symptom is stated to have been present in *five* of the *eighteen* cases constituting the *Typhoid* group; in *five* of the *twelve* cases of *Typhus*; and in *two* of the *eight* cases of *doubtful type*. Generally, in the cases in which it is not stated that the symptom was present, nothing is said on the subject. It is fair to presume that in all, or nearly all of the cases in which the symptom was discoverable, or sufficiently marked to be observed, it was embraced in the records. But it was not always easy to determine as to its existence. In cases of marked somnolency, or stolidity, and when persistent active delirium existed, it was sometimes difficult to appreciate the existence of deafness.

Hence, I do not suppose that the above enumerations possess much value in endeavoring to ascertain the precise ratio of frequency in the occurrence of this symptom. But here, as in many other instances, arithmetical accuracy is not of great practical importance. It is sufficient to form a rough estimation of the probabilities of its occurrence.

Nothing appears, on an examination of the cases, to denote a connection between this symptom and other events of the febrile career. It was present in cases of a mild as well as severe grade of intensity. It was observed in cases of both types, but oftener in Typhus. It was associated sometimes with somnolency and delirium, but was also noted in cases in which the latter symptoms were slight or absent. Of the hospital cases, of both types, which proved fatal, it was noted as present in *two*. In *one* case it was noted absent; and in the histories of the *three* other cases, nothing is said relative to its presence or absence.

It was observed that, when deafness existed, it frequently, if not generally, persisted through the febrile career; and, in some instances, continued for some time after convalescence was established.

It was by no means uniform in degree; sometimes occasioning only some bluntness of hearing—but in some cases rendering it necessary to speak quite loudly in order to be heard.

In some cases patients complained of the difficulty they experienced in hearing, and in other cases it did not appear to excite their notice.

No other facts relating to the sense of hearing were recorded, except that in *two* instances, in connection with deafness, the patients complained of a noise or buzzing in the ears.

As regards the senses of *smell* and *taste*, pains were not taken to record, or to make observations.

Involuntary Muscular Contractions. Involuntary muscular contractions were noted in so small a number of instances, that it will not be tedious to notice them and their peculiarities, individually.

Of the cases in *private practice*, involuntary muscular contractions are stated to have existed in *three*. In *one* of these, they consisted in *subcutis tendinum*, and visible twitching of the hands during sleep. The patient, in this instance, recovered. The disease, in its general features, did not present great gravity.

In *one* of the other instances, there existed marked tremulousness of the hands and the tongue, at the commencement of the disease. So prominent was this symptom that, for the first day, the affection was supposed to be delirium tremens—the character and the history of the patient being

unknown. This case ended fatally on the *fourth* day, and was of the *Typhus* type.

In the *remaining* case, there existed tremulousness of the hands and muscles of the face, when the patient was first seen on the *fifth* day. The case ended fatally on the *ninth* day. This case was of the *Typhoid* type.

Of the *hospital cases—Typhoid*—in *one* case, in which recovery took place, some tremulousness of the hands was observed, on one day. The case was one of severity. In another case, there existed *subcultur* and marked tremulousness of the hands, together with inability, on one occasion, to protrude the tongue. This case ended fatally on the *tenth* day.

Of the cases of *Typhus*, *subcultur* was noted in but *one* case. This was a case of considerable, but not very great severity, ending in convalescence on the *seventeenth* day after entrance—the patient entering twelve days after illness commenced.

In the two fatal cases of this type, in *one* the power of articulation was lost toward the close of life—the patient not being unconscious. In the other case, much difficulty was experienced in protruding the tongue—repeated efforts being required. Coma became developed in the latter case.

Of the cases of *doubtful type*, tremulousness of the muscles, toward the close of the disease, was noted in the case which proved fatal. Nothing under this head appears in the records of the other cases in this group.

The instances in which this symptom is recorded as present, are fewer than I should have estimated before the analysis was made. A symptom so obvious and important would not be likely to be overlooked or omitted in the history. It is, however, possible that it may have been present, in the form of *subcultur*, and in a minor degree, in some cases in which it was not manifested at the diurnal examinations and records, and, hence, have been omitted.

It will be observed that, in most of the cases in which involuntary muscular contractions were noted, the disease ended fatally. In view of this fact, taking also into consideration the fact that the symptom is one of rare occurrence, its presence is of bad omen, as respects the prognosis.

Carphologia. This symptom properly belongs among the aberrations of the sense of vision, consisting in voluntary motions directed toward illusory objects in the air, or on the bed-clothes. It was noted in *three* cases. In one case, of the *Typhus* type, it was observed only on one day. The disease in this instance was severe, but did not prove fatal.

In *one* case, the type was *Typhoid*, and the disease ended fatally. The case was characterized by vigilance, and very active delirium, up to a few

hours before death. It was a prominent symptom in this case, in the latter stage of the disease. In the remaining case, the type of the fever is classed as *doubtful*. The patient entered the hospital on the third day after the attack; but the fever had supervened on an illness of three weeks' duration, (the character of which was not ascertained,) from which he had but recently recovered sufficiently to return to labor. Convalescence was pronounced in this case on the sixth day after his entrance.

Pulling up of the bed-clothes, I do not find noted except in one case. It may possibly have existed, and, not being apparent at the time of the daily examination, escaped being recorded. In the case in which it was present, it was associated with Carphologia. This case was the last of the three cases mentioned above.

Prostration. By *prostration*, I mean exhaustion, or reduction of the force inherent in the voluntary muscular system. Under this head I shall make a few general remarks, based on an examination of the cases, without any enumerations. The latter cannot, in this instance, be made of much avail, inasmuch as we have no means of measuring, with precision, the deterioration of voluntary muscular power. We can only form estimates which are but rough approximations to correctness, and these must be expressed by terms somewhat indefinite. For examples, we may say, that the prostration is moderate, slight, considerable, great, very great, etc. These expressions, obviously, have relative significations, which, taken in the connection in which they are used, may be sufficiently explicit for practical purposes; but they lack that definiteness which is desirable in data for numerical results.

In a certain sense, fever invariably induces a considerable degree of muscular prostration. The event selected to indicate the commencement of the febrile career, is evidence of this fact. Patients are compelled to take to the bed. They are unable to maintain the erect posture, or to continue, except for a short time, efforts of locomotion. In saying of a particular fever patient, he is greatly prostrated—it is not to be understood that the powers of a healthy, vigorous man are taken as the standard of comparison; but that his muscular strength is reduced considerably below an average of the degree of diminution incident to the febrile state. Judged by this rule, in the majority of the cases which form the subjects of the present analysis, the prostration was not great. The patients generally were able to raise themselves in bed, to change their position by their own efforts, etc. The loss of muscular power was not so great as in many protracted chronic diseases in which the patient is obliged to keep the bed.

In a considerable proportion of cases, patients were able, with the assistance of an attendant, to get out of bed, in order to pass their evacuations, for change of bed-linen, etc. Sometimes they were able to remain sitting for some time; and, when this was not the case, it frequently appeared to be owing rather to *giddiness*, than to a deficiency of muscular force.

The loss of muscular power was often more apparent than real. There was ability to exert considerable voluntary effort, but a mental indisposition thereto. The nervous, rather than muscular force, was at fault. The strength was more oppressed than reduced. Owing to mental dullness, it was difficult, sometimes, to rouse the patient to a trial of voluntary effort; and in cases in which marked somnolency, or a tendency to coma existed, it was not easy, on this account, to form an estimate of the amount of muscular ability retained by the patient.

In cases in which the fever was of a mild grade of intensity, there existed moderate or even slight prostration, patients being able to assist themselves with very little aid from others. Extreme prostration always occurred in connection with other symptoms denoting unusual gravity of disease. The converse of this, however, was not true. In some of the cases attended with most danger, and some of those ending fatally, the muscular strength was retained in a surprising degree. In two fatal cases of the *Typhoid* type, characterized by active, persistent delirium, the muscular efforts were almost constant, and quite strong up to a few hours before death. One of these cases terminated on the ninth day, and the other on the third day after coming under observation. The mode of dying, in each, was by *asthenia*, or, perhaps, more properly, *necroemia*—the system of involuntary muscles exhibiting reduction of force to a degree incompatible with life—the voluntary muscles remaining active. This is a curious fact.

It follows from the above observations, that while an unusual degree of prostration is an unfavorable sign in Continued Fever, the preservation of considerable muscular force is not to be considered as, correlatively, a favorable sign.

In several of the cases, I have observed a fact which has an important practical bearing upon the treatment of fever, viz., an improvement in strength during the febrile career. In the histories of the cases, it is occasionally noted, that the prostration at first apparent, diminished while the disease was yet in progress. In these cases, the patients had not received proper care in the way of nursing, or medical attendance; and in some instances, according to the views of the writer, the management had been

not only defective, but injudicious. As respects the average degree of muscular force presented in a series of fever cases, much, I am persuaded, will depend on the therapeutical system pursued. This, however, is a topic which will more appropriately come up under the head of the Treatment of Continued Fever.

On consulting the Treatise on Fever by Louis, the reader will find that the results under the head of *Strength*, appear to be at variance with the foregoing. If I may be permitted a criticism upon the observations of that distinguished observer, under this head, I should say, that he does not seem to distinguish between the absolute loss of muscular power and the reluctance to exert it. If the reader will carefully peruse his remarks on this topic, I think he will discover sufficient evidence of the correctness of this criticism.

Louis gives an account of some cases in which the muscular strength was retained sufficiently for the patient to keep about several days after the fever was established. Agreeably to the rule I have adopted, these patients would not be considered to be laboring under established fever prior to the time they took to the bed. As I have before stated, this rule is somewhat arbitrary, and does not mark the commencement of the disease, with exactness, in all cases. Nevertheless, it seems to me to answer better as a point of departure for the origin of the disease, than any other event, or collection of events, that can be selected.

SECTION FIFTH.

Symptoms referable to the Digestive System. Appetite. Thirst. Tongue. Sordes. Parotitis. Nausea, and Vomiting. Alvine Dejections. Tympanites. Tenderness of Abdomen. Gurgling.

Morbid conditions of the digestive system are involved in important questions relating to the diagnosis, pathology and therapeutics of Continued Fever. That portion of the symptomatology of the disease, therefore, which is embraced in this Section, opens a field of inquiry from which interesting, if not useful, results may be hoped for. I will take up the several sub-divisions of the Section in the order in which they are enumerated in the caption.

Appetite. The importance of noting negative as well as positive facts, here, as in several other instances, being overlooked in recording the histories of the cases under analysis, the data are too defective for any statistical results. In a large number of the cases nothing is stated relative to the appetite. It cannot be doubted, however, that *anorexia* exists in the

vast majority of cases of Continued Fever; and it is to be presumed, that this symptom was present in most of the cases in which the records are silent on the subject.

In a few cases it is noted, that appetite existed—the patients declaring that they took the food given to them with some relish. In connection with this fact should be stated, what will be noticed hereafter under the head of *Treatment*, that the majority of the patients were allowed milk porridge and essence of beef, for diet, during the greater part of the febrile career. In *two hospital* cases, of the *Typhoid* type, the patients said, that the diet was agreeable; in *one* case of *Typhus*, the same is noted; and in another case of the same type, it is stated that the patient had some appetite. In all the other cases, both in private and hospital practice, either anorexia existed, or nothing appears in the histories relative to this subject. The reader may have some curiosity to know if there were any characters distinguishing the *three* cases in which food was relished, and the *single* case in which it was desired. Of the *two Typhoid* cases, *one* proved fatal. The patient, in this case, was passively delirious during the early part of the disease, but the mind subsequently became clear. He was greatly prostrated. Evacuations were passed in bed, not unconsciously, but from inability to restrain them. Death occurred, twenty-six days after the attack, by exhaustion, (*necræmia*.) In this case, anorexia existed part of the time. In the other case, the disease was of a mild grade; the patient being convalescent eight days after entering hospital, and quite well on the nineteenth day. In the *two Typhus* cases, both patients recovered. The disease, in both, was of medium severity. In both, pulmonary symptoms were prominent. In one, it is stated that food was relished through the febrile career; in the other, appetite was stated to exist only on one day—the second day after admission. No circumstances thus appear which can afford any explanation of the deviation from the general rule, as respects the appetite, in these four cases.

I would remark, that I do not feel confident that the four cases just mentioned, were the only instances in which food was relished, not being satisfied that proper pains were taken to make inquiries and notes relative to this point, during the examination of patients.

Thirst. Of this symptom, as of that just noticed, owing to want of care to record negative facts, I cannot submit any statistics as respects the frequency of its occurrence, its duration, etc. In a large proportion of the cases, the records contain no reference to thirst. I will not presume that it did not exist in any of these cases, but I am confident it was not a pro-

minent symptom in any of them during the time they were under observation, after the fever became established. In *three* of the cases in *private* practice; in *three* of the *hospital* cases of *Typhoid*, in *two* of *Typhus*, and in *one* of *doubtful* type, *thirst* is mentioned as a symptom continuing, more or less, through the febrile career. In all these cases, save two, the thirst was moderate in degree. It is also worthy of remark, that in all, save the two last referred to, the disease was of a mild grade of severity; and in *four* of the cases, very mild. In the *two* cases in which thirst was more than moderate, the disease proved fatal. One of these cases was among the number in which food was relished; and in this case, the thirst was considerable. In another instance, thirst was associated with the relish for food.

In addition to the *nine* cases in which, as just stated, *thirst* existed through the career of the fever, it is noted to have been present in the early part of the disease in several instances, *viz.*, in *four* cases of *Typhoid*, *three* of *Typhus*, and two of *doubtful* type. It will be recollected that in a large proportion of the *hospital* cases, the patients did not come under observation at the earliest stage of the disease. It is altogether probable that, in several of these cases, this symptom had existed and ceased prior to the period when the records commenced.

The cessation of the symptom is readily explained by attributing it to the bluntness of perception which usually characterizes the disease after the first few days. The morbid conditions which had occasioned the sensation of thirst, probably continue; but the faculty of perceiving these conditions, or of suffering in consequence of them, is no longer acute. This idea is confirmed by a fact above noted, *viz.*, that in most of the cases in which thirst continues through the career, the disease is of a mild grade—less obtuseness of sensibility belongs to its progress. It is also confirmed by another fact which I have very generally observed, but which is not noted in the histories, *viz.*, that although the sensation of thirst may not create sufficient uneasiness to lead the patient to ask for drink, nevertheless, when it is presented, he almost always takes it with readiness, and frequently with avidity, showing the existence of the symptom, but, as it were, latent, owing to the mental apathy peculiar to this disease.

Tongue. The morbid appearances of the *tongue* are observed with attention in all diseases; but in few, if any, are they generally regarded as possessing greater interest and importance than in febrile affections. Some practitioners attach a significancy and value to particular conditions of this organ, which close investigation probably will not warrant. Certain ap-

pearances are supposed to stand in a fixed relation to certain pathological conditions of other parts, and of the system at large; or, they are thought to foreshadow the issue of the disease, or to indicate particular methods of treatment. These notions, which it might be presumed involve both truth and error, are partly empirical and partly speculative—that is to say, in some instances, theoretical views have doubtless had more or less to do in originating them; but it is also frequently claimed that they are corroborated by experience. In either case, the analytical investigation of a large number of recorded observations, is the proper tribunal for adjudication. By ascertaining precisely the position which every appearance occupies in the natural history of the disease, its importance as a symptom or sign is determined.

There are two points of view in which the appearances of the tongue in Continued Fever are to be studied. *First*, the different appearances which are found in cases of the disease, considered collectively, their relative frequency of occurrence, their relations with other symptoms, etc. *Second*, the different appearances which are successively presented during the progress of the disease, in individual cases. In both points of view, the appearances are various.

Of the *fifty-two* cases which are the subjects of the present analysis, the histories contain an account of the appearances which the tongue presented during the febrile career, in all but *two* cases. In not one of these cases did the tongue retain a perfectly normal aspect through the career of the disease. The nearest approximation to this was in one of the cases in private practice—(*Typhoid*)—in which the disease was of a very mild grade, the patient being convalescent on the *fifth* day. In this case the tongue remained perfectly natural in appearance until a couple of days before convalescence, when the superior surface became slightly opaque, hardly enough so to be called furred, and not more than is habitually observed in many persons who call themselves well. In all the other cases morbid appearances, more or less considerable, were presented. The different appearances presented in the different cases, involve varying degrees of dryness of the surface of the organ, together with resistance to the touch; coatings varying in thickness; varieties in *color*; smoothness, fissures, etc.; aberrations of muscular contractility, or an inability to protrude it, facility of performing this act, etc. I will take up the more important of these appearances separately.

Dryness of the Surface. More or less dryness of the tongue was present in a large proportion of the cases, *viz.*, in *six* of the *fourteen* cases in *pri-*

vate practice; in *fifteen* of the *eighteen* hospital cases of *Typhoid*, *ten* of the *twelve* cases of *Typhus*, and *five* of the *eight* cases of *doubtful* type; making, in all, *thirty-six* of the *fifty* cases in which the appearances of the tongue were noted.

The dryness differed in degree in the different cases; in some being slight, in other cases extreme, and in others, moderate. In several cases, the dryness was accompanied with *hardness*, that is, the surface presented an appearance as if desiccated or baked, offering firm resistance to the touch. This was observed in *two* of the cases in *private* practice; in *three* of the *hospital* cases of *Typhoid*, in *four* of the cases of *Typhus*, and in *one* of the cases of *doubtful* type.

The dryness, in some cases, extended over the whole superior surface, and, in other cases, was limited to the centre—a moist margin, of variable width, existing on either side. The latter was observed in *one* of the cases in *private* practice; in *eight* of the *hospital* cases of *Typhoid*, in *four* of the cases of *Typhus*, and in *two* of the cases of *doubtful* type.

The dryness was in no case observed at the commencement of the febrile career. In all the cases that came under observation at the commencement, the dryness did not appear until several days had elapsed; and in every instance in which dryness was present at the time the case was first observed, the disease had existed for several days. This condition, therefore, although so frequently present in Continued Fever, does not belong to the early stage.

What does this symptom denote? in other words, what are the causes which produce it? Diminution of the secretions from the salivary glands and mucous follicles is, doubtless, a cause; but it is not, probably, the sole cause. It proceeds, in part, from the condition of the mind incident to Continued Fever. Owing to the apathy as respects painful sensations, the patient appears to experience little or no discomfort from the dryness and hardness of the tongue. He, therefore, does not move the organ sufficiently to diffuse the scanty salivary secretion over its surface. The tongue, moreover, participates in the inertia of the muscular system. It remains motionless. The somnolency which obtains in the majority of cases, contributes to this symptom. The patient lying most of the time somnolent, respiring through the mouth, (the nasal passages frequently being more or less obstructed,) the surface of the tongue is desiccated by the current of air passing over it.

The correctness of the above explanation is proved by the following considerations:—Patients very seldom make any complaint of the condition of

the tongue, however dry and hard it may be. The dryness and hardness do not come on during the early part of the febrile career; nor, (as has been seen in the preceding Section,) do somnolency, muscular inertia, and mental apathy. The former and the latter occur concurrently, at the same period in the febrile career; and, as a general remark, it will probably be found that they disappear together. Finally, the degree of dryness and hardness will be found to bear a certain degree of correspondence with the ataxic symptoms just mentioned; nor are the former present, as a general rule, in cases in which the latter are absent. In evidence of the statements last made, I have examined,—*first*, the symptoms referable to the nervous system in all the cases in which dryness of the tongue was *not* present, and the results are as follows:—of the *eight* cases in *private practice*, in which dryness of the tongue was not noted, in *one*, the tongue could not be inspected, except at first, owing to parotitis; in one, the patient died on the third day; and in one, the records contain nothing on the subject. Of the remaining *five* cases, somnolency, muscular inertia, and mental apathy, were not marked in *one* case, and were almost absent in each of the other cases, all of which were extremely mild as respects the grade of severity of the disease. Of the *hospital* cases, in the two instances of *Typhoid*, in which dryness of the tongue was *not* noted, active delirium, followed by coma, existed in *one* case, the patient dying on the sixth day; in the other case, ataxic symptoms were absent. In the *two* cases of *Typhus*, in which dryness of the tongue was *not* noted, in *one* case, the tongue could not be inspected, owing to parotitis, and in the other case there existed hilarity and playfulness, without either somnolency, muscular inertia, and mental apathy. Of the *three* cases of *doubtful type* in which dryness of the tongue was *not* present, in each case the nervo-muscular symptoms just mentioned were absent. *Second*, I have examined the symptoms referable to the nervous system in the cases in which dryness and hardness of the tongue existed, and find in all but *two*, (in which the records are defective,) evidences of the presence of somnolency, muscular inertia, and mental apathy—in other words, of ataxic symptoms. As the histories do not state the degree of these symptoms, I am unable to ascertain whether, in that respect, they corresponded with the degree of dryness which the tongue presented.

In conclusion, then, we are warranted in saying that, although dryness and hardness of the surface of the tongue denote deficiency of the secreted fluids poured into the mouth, they are to a considerable extent dependent

on the condition of the mind and muscular system, and may be considered, to some extent, as measuring the disorder of the latter.

Coating. In *forty-seven* of the *fifty* cases in which the appearances of the tongue were noted, it was more or less *coated* during the febrile career. Of the remaining *three* cases, (all of which were of the *Typhoid* type,) in *one* there does not appear to have been any coating; in *one*, there was none for the first two or three days after the case came under observation; but the tongue could not be inspected afterward, owing to parotitis, affecting both sides; and in *one*, there was merely a slight opacity, hardly sufficient to be called a coating. Under the head of *Coating*, I mean to include all instances in which a morbid secretion, or exudation, was deposited on the superior surface of the tongue. It is common to distinguish a *furred* or *frosted* tongue, from one which is *coated*, the latter being applied when the morbid deposit is of an appreciable thickness. For the sake of convenience, however, I have used the term *Coating* in a sense broad enough to embrace the former as well as the latter. The coating varied much in the different cases as respects thickness. All of the histories do not contain precise information on this point, but, in the majority of instances, it is stated either that the tongue was *thickly coated*, or *thinly coated*, or merely *furred*. The enumerations with regard to these three modifications of coating are as follows:—Of the *eleven* cases in private practice, in which coating is noted, it is simply stated, that the tongue was *coated* in *three*; it was *thickly coated* in *four*; it was *thinly coated* in *two*, and *furred* in *two*. Of the *eighteen* hospital cases of *Typhoid*, it was *coated* in *four*; *thickly coated* in *two*; *thinly coated* in *six*; *furred* in *five*, and no coating existed in *one* case. Of the *twelve* cases of *Typhus*, it was *coated* in *five*; *thickly coated* in *three*; *thinly coated* in *two*; *furred* in *one* case, and the organ could not be inspected, owing to the existence of *parotitis*, in *one* case.

Of the *eight* cases of *doubtful type*, it was *coated* in *four*; *thinly coated* in *two*; *thickly coated* in *one* case, and *furred* in *one* case. The cases of the different types of Continued Fever presenting respectively these differences in thickness of the coating, are not uniform in number, but the discrepancy is not such as to warrant the conclusion that either modification is more likely to occur in the *Typhus*, than in the *Typhoid* type. Not less disparity would, very likely, be found on comparing different collections of cases of the same type, than in the comparison, in this particular, which these cases afford of the two types. We should not expect to discover in the varieties of this symptom any traits distinguishing one type of the fever from the other, inasmuch as these varieties are based on differences in degree

merely, not in kind, and, moreover, are common enough in other, and, indeed, in almost all affections.

There seems to be no room for the inquiry whether the coating has any connection with any other particular symptoms, or group of symptoms, belonging to the natural history of Continued Fever. Occurring, as the symptom does, in almost every case of Continued Fever, it must be dependent on some one or more of those morbid changes which constitute the elements of the febrile state. Farther than this, in the present state of knowledge, we are unable to carry the explanation of this, as well as various other symptoms appertaining to the disease under consideration.

The only remaining points of inquiry which occur to me under this head are, *first*, to examine the histories of the few cases in which the tongue was not coated, in order to see in what respect they differed from the others; and, *second*, to compare some of the cases in which the tongue was *thickly coated*, with some of those in which the coating was *thin*, in order to see if the degree of coating affords, to any extent, a criterion of the severity of the disease.

As already stated, in but *three* cases is the absence of coating noted, and in *one* of these cases the tongue could not be inspected after the first two or three days, owing to parotitis. The patient, in this case, at the time she was attacked with fever, was suffering from *stomatitis materna*, and the tongue presented a characteristic reddened, excoriated appearance. One of the remaining cases was the case in which the tongue preserved its normal appearance save a very slight opacity. This case was distinguished for its mildness, and short duration. In the other case, the tongue was reddened, glazed, and somewhat fissured. The patient, in this case, had been ill eleven days before entering the hospital. It is highly probable that there had existed coating of the tongue, which had exfoliated prior to his entrance. The case was one of medium severity, and was not distinguished by any peculiar features. Convalescence was pronounced on the fourteenth day after his entrance. It will be observed that the circumstances appertaining to each of these cases were such that the latter are hardly to be considered exceptions to the general rule, as regards the uniform existence of more or less coating of the tongue in the present collection of cases of Continued Fever.

With regard to the second point of inquiry, I have examined and compared cases sufficiently to ascertain that the thickness of the coating cannot be considered as denoting the gravity of the disease, or the danger of the patient. Whether, in the larger proportion of severe or fatal cases,

the tongue may be thickly, or thinly coated, I am not prepared to say—the present collection of cases is too small to afford statistical results adequate to the settlement of this question. But that, if any such law exists, the exceptions are sufficiently numerous and striking to render its application to individual cases nugatory, my cases afford sufficient evidence. Of *two* of the fatal cases of *Typhoid*, the tongue was merely *furred* in *one*, and, in the other, at first furred, and subsequently thinly coated. In *one* of the fatal cases of *Typhus*, it was thinly coated. On the other hand, in several cases in which the tongue was thickly coated, the disease was mild. In the fatal case of *Typhus* occurring in private practice, in which death occurred on the morning of the fourth day, the tongue presented a normal aspect, except that it was tremulous, for the two first days, and became thickly coated on the third day.

It appears pretty obvious that very little importance is due to the degree of coating on the tongue in any of the practical relations, or bearings, in which the symptoms of disease are to be considered.*

The coating of the tongue, in the majority of cases, appeared early in the disease, preceding *dryness*. The tongue, however, was not thickly coated at first, in several cases in which it became so at a subsequent period. It was frequently at first simply furred, and in the progress of the disease became thickly coated. As a general rule, the coating became thinner toward convalescence, and gradually disappeared—leaving the appearance of the organ healthy. In a few cases, however, the tongue continued somewhat coated after convalescence was established.

The color of the coating was generally white, or dirty white, sometimes yellowish. In *seven* cases it became of a dark color. *Four* of these were cases of *Typhoid*; *two* in *private practice*, and *two* in *hospital*; *two* were cases of *Typhus*, and *one* was a case of *doubtful* type. In *one* of these cases the disease proved fatal, but in the other cases the disease was either mild, or of a medium grade of intensity; so that there is very little, if any, foundation for the idea that the dark, or black color of the coating, in cases

* The foregoing results relating to *dryness of the surface of the tongue*, and *coating*, appear to present a striking disparity on comparison with those developed by the researches of Louis. He found in a considerable number of cases, the tongue natural, or nearly so. This was true of grave and fatal cases as well as of those of a mild grade. The appearances which the tongue presents are so obvious, and offer so little room for error of observation, that, with every disposition to distrust my own ability as an observer, I cannot admit the supposition of incorrectness, in the histories I have collected.

In the cases analyzed by Dr. Jackson, the tongue appears to have been more uniformly altered than in the cases analyzed by Louis. Dr. Jackson says, "with few exceptions the tongue was noted as coated, or furred."

of fever, denotes extreme danger. This idea is entertained by some, who regard the coating as representing a perverted state of the fluids, and the dark color as denoting putrescency.

Exfoliation of the coating during the career of the disease, occurred in three cases—one case being of the *Typhoid*, one of the *Typhus*, and one of *doubtful* type. In the first of these three cases the tongue became subsequently dry and hard, and another coating succeeded. In the two other cases the tongue, directly after the exfoliation, was moist, and of natural color, and the occurrence of a second coating is not mentioned. In each of these three cases the disease was mild.

A *scabby* appearance was noticed in a few cases, viz., two cases of *Typhoid*, and two of *doubtful* type. This appearance was owing apparently to dryness and coating co-existing, the morbid deposit on the tongue becoming cracked and subdivided into small portions, which were partially detached.

In two cases, one of *Typhus*, and one of *doubtful* type, the coating was in discrete patches; but in all the other cases it was either diffused over the whole superior surface, or extended across the base, and, more or less, toward the tip, or covered a margin on each side of the whole length of the organ.

The pathological explanation of coating does not fall within the scope of this report, but the reflection arises, how little we know of the causes of this symptom in any, and all of its varieties—in other words, of the morbid changes of which the symptom is the immediate effect. Its mysterious character is perhaps less appreciated on account of its being so common. But that a morbid product should be deposited on the surface of the tongue, without any other appreciable evidences of disease of that organ; that the presence of this product should so often denote the continuance of disease, and its disappearance signalize the occurrence of convalescence, is certainly a curious fact, and one which, in the present state of knowledge, cannot be accounted for. All that is known of this symptom, as a symptom, or sign, has been obtained by observing its association with other events, and in all diseases, as well as in Continued Fever, its special significance is quite limited.

Redness of the tongue. The superior surface of the tongue was reddened in seven cases of the *Typhoid*, and in one case of *doubtful* type. This appearance was not observed in any of the cases of *Typhus*. In one of the cases of *Typhoid* the redness was associated with *stomatitis materna*, or the sore mouth of nursing women, under which the patient was laboring

when attacked with fever. In *two* of the remaining cases, the disease proved fatal. In one of the fatal cases, the disease was characterized by active delirium. Vomiting was not present, nor diarrhœa. The only gastric symptom was thirst, which was, at first, considerable. At the autopsy, in this case, there were found punctated redness and softening of the mucus membrane, together with several ulcerated patches. The papillæ of the tongue were unusually prominent in this case. In the other fatal case, vomiting, and other gastric symptoms, are not mentioned, and diarrhœa did not exist. Moderate tympanites and abdominal tenderness were present. At the autopsy the stomach was not examined. In the *four* remaining cases the disease was not severe, save in one case. Symptoms indicative of disorder of the digestive tube, in these cases, respectively, were as follows:—In *one*, moderate diarrhœa—no vomiting. Convalesced on the tenth day after entrance, and left the hospital on the sixteenth day. The redness of the tongue, in this case, continued after convalescence was established. In *one*, neither vomiting, nor other symptoms of unusual gastric disorder were present, and diarrhœa did not exist. In *one*,—the same as in the last case. This case was tolerably severe. In *one*, no vomiting or other gastric symptoms, and diarrhœa was not prominent. In the case among those of *doubtful* type, no gastric symptoms were present, but moderate thirst, at first. Diarrhœa did not exist, and distension and tenderness of the abdomen were absent.

The fact that in no case of *Typhus* was redness of the tongue present, is perhaps worthy of note.

I have given this summary of the other symptoms referable to the digestive system in the cases in which a reddened tongue was observed, because the idea still prevails, to some extent, with practitioners, that this appearance denotes either inflammation, or notable irritation of the mucus membrane, lining the digestive canal, and more particularly the stomach. In so far as symptoms are concerned, the above instances afford no evidence in support of this hypothesis. The fatal case in which the stomach was examined, it is true, was characterized by gastric disease which was not indicated by gastric symptoms during life. The researches of Louis, however, have established, conclusively, that in fatal cases of Continued Fever there does not exist any constant connection between the appearance of the tongue, and the existence of lesions of the stomach.

Tremulousness of the tongue. Tremulousness was observed in *seven* cases, viz., in *three* of *Typhoid*, and in *four* of *Typhus*. *Three* of these were fatal cases. In the other instances in which this appearance was

noted, the disease was not of very great severity. Nor was this appearance associated with singultus, tremor of other muscles, or marked disorder of the nervous system, in any case excluding the fatal cases. In *one* of the cases the tongue continued somewhat tremulous during convalescence, and even at the time the patient left the hospital, six days after convalescence was established, remaining, also, somewhat reddened. A tremulous state of the tongue, then, cannot be considered as representing a condition of the muscular system in general, nor as an indication of the degree to which the nervous system is affected. And although, as it would seem, it is apt to occur in cases which prove fatal, its presence need not affect very materially the prognosis, since it also occurs in cases in which the disease is of a medium grade of intensity.

Difficulty of protruding the tongue. This existed, to a greater or less extent, in *six* cases, viz, in *two* of *Typhoid*, *three* of *Typhus*, and in *one* case of *doubtful* type. In two of the *Typhoid* cases the difficulty was not great. The patient only did not readily succeed in the effort to protrude the tongue. In the other *Typhoid* case the patient was wholly unable to accomplish the protrusion, but this inability existed during one day only. This was the day before the decease; and on the day of the decease he succeeded in protruding the tongue without great difficulty. In this case, also, there was loss of the power of articulation for the last two days of life. The patient could be roused to make an effort to speak, moving his lips, but uttering no articulate voice. In the *two Typhus* cases the difficulty of protruding the tongue was great. One of these cases was characterized by apoplectic coma, and there existed great difficulty in moving the upper extremities, as well as the tongue. In the other case the patient exhibited unusual somnolency, which eventuated in coma. This case ended fatally. In the case of *doubtful* type, there existed inability to protrude the tongue on the first day of entrance, unusual feebleness of intelligence co-existing. The patient could not be made to reply to questions. The ability to protrude the tongue was recovered on the following day, and, at the same time, there was improvement in the state of the intellect. In all the cases, save one, the difficulty in protruding the tongue, corresponded, in degree, with the diminished power of exerting acts of the will to produce voluntary motions; and in the excepted case, the difficulty was inconsiderable in degree. It would seem, therefore, that this symptom, in so far as it may possess any special significance, is more a criterion of the condition of the mind, than of the muscular force.

A remark suggests itself in this connection which is not limited in its

application to cases of Continued Fever. It is, that patients whose intelligence is so far compromised that they cannot be made to reply to questions, may often be induced to protrude the tongue, or to make an effort to do so. The mental apprehension seems sometimes not to extend beyond the request to perform this single act. I know of no way of accounting for this curious fact except that, as the inspection of the tongue always enters into the daily examination of cases of disease, it becomes so associated with the visit of the physician, that, if a patient retains mind enough to recognize his medical attendant, he almost instinctively apprehends the request to protrude the tongue. It requires, from the association just mentioned, less mental effort than to understand and comply with other requests.

More or less hesitancy in protruding the tongue was frequently observed, although pains were not taken to note observations concerning this point in the histories. It appeared, often, as if the patients took time for deliberation before exerting the act of volition, and the protrusion was made very slowly, as if the organ were not fully under the control of the will. This, doubtless, proceeds from the same mental condition which, when greater in degree, leads to difficulty in performing the act, or entire inability to accomplish it. Instances were also noticed in which, when the tongue was protruded, the patient seemed to forget to withdraw it, and it was necessary to request him to do so.

A few appearances were noted in addition to those already considered, which I will mention, in order to embrace *all* that the histories contain with respect to the tongue. The tongue itself became *fissured*, or *cracked*, in *four* cases, all of the *Typhoid* type. In *one* of these cases, the fissures did not heal for several days after convalescence was established. In all, the *fissures*, or *cracks*, occurred while the surface of the organ was dry and hard. The superior surface was noticed to be remarkably *smooth* in several (*five*) cases; and in *three* of these cases it presented a *glazed* appearance. The *smoothness* was apparently owing to dryness, the tongue being clean, and the *glazed* appearance to dryness with a thin stratum of coating.

Finally, in one case, indentations of the teeth on the sides of the tongue were noted

Variations in the volume and form of the tongue, if they existed, were not recorded.

The different appearances of the tongue found in the cases collectively, having thus been considered, it remains to direct attention to the second

point of view under which this class of symptoms are to be studied, viz., the different appearances which are successively presented in individual cases. This view of the subject has been, in a measure, anticipated in the foregoing remarks. To present the facts under this view accurately and comprehensively, it would be necessary to give an account of the succession of appearances in almost every case distinctly; for, very few cases are to be found in which precisely the same appearances are presented, occurring in the same order of succession and time. But this minuteness of detail would not subserve any interesting or important purposes, since it is, obviously, impracticable to deduce any fixed laws regulating the succession of appearances, or to arrive at any practical results. It will suffice, therefore, to dismiss this branch of the subject with a few general remarks. The cases differed considerably as respects the number and kind of appearances which, respectively, they presented during the febrile career. In some cases the tongue remained simply furred; in others, it was furred at first, and thickly coated, subsequently; in others, it was furred, or thickly coated, early in the career, becoming more or less dry subsequently, and sometimes alternately moist and dry at different times. In a smaller proportion of cases the various other appearances occurred—in some cases one only of these appearances, and in others several—viz., tremulousness, redness, fissures, etc. All this, indeed, would be inferred from the account previously given of the various appearances under distinct heads. And it would be sufficient to say, under this division of the subject, that the analysis reveals nothing with reference to the number and succession of appearances in individual cases, which has any marked bearing on the distinctions between the two types of fever, or the relations of the tongue to other symptoms; moreover, the number and succession of appearances do not differ in any striking particulars in the cases which proved fatal, from some of those which ended in recovery.

In conclusion, with respect to the appearances of the tongue in Continued Fever, judging from the results of the present analysis, their value, as symptoms, cannot be considered very great. Here, as in the case of other symptoms, it does not enter into my plan to inquire to what extent similar appearances are to be observed in other diseases than Continued Fever. This, it is obvious, is a very important inquiry in order to determine in how far the phenomena described are peculiar to the disease under consideration. With regard to this comparison, the most accurate and extensive collection of recorded observations are doubtless to be found in the researches of Louis. But as regards the class of symptoms just considered,

every practical reader is already aware that those which have been enumerated are not, in an eminent degree, distinctive of Continued Fever, although, probably several of them are oftener found in connection with that disease than in most other acute affections. As a general remark, however, they do not possess any very special significance; in other words, they have not in the present state of knowledge very important bearings on the diagnosis, pathology, prognosis, or therapeutics. The results of enumeration and comparison in their application to this class of symptoms, are rather negative than positive. But here, as in other instances, by tending to disprove some errors which are more or less in vogue, these results are of scarcely less value, than if they disclosed important practical truths.

(*To be Continued.*)

ART. II.—*Life of Dr. John Ellis Marshall, being a report made by a Committee appointed for this purpose by the Buffalo City Medical Association.* Communicated by resolution of the Association.

GENTLEMEN,—The Committee appointed by a resolution of this Society to prepare a brief Biography of the late Dr. John Ellis Marshall, would state, that the delays and embarrassments naturally incident to the performance of a duty of this kind, have prevented the possibility of an earlier report. Indeed, so many years have elapsed since the death of Dr. Marshall, and so few of his cotemporaries now remain among us, we feel confident that many circumstances of his useful life have escaped our notice. While we regret this loss, and the more especially because it renders this intended tribute of respect less complete, and deprives us all of much that was doubtless worthy of imitation in his example, it ought to admonish us that hereafter if we would desire to perpetuate the lives and services of our deceased co-laborers, we must secure the record before time has drawn over them her impenetrable veil.

John Ellis Marshall, the only child of Thomas and Sarah Marshall, was born March 18th, 1785, in Norwich, New London County, Connecticut. His mother dying when he was only one month old, he was adopted into the family of Daniel Ellis, of Franklin, Connecticut, and educated by him as one of his own children.

At the age of sixteen years he was attacked with a pulmonary affection which required him to spend a year or more abroad for his health. When he returned he was so far recovered as to enable him to resume his studies of the higher branches of the English, and of the classics, under the tuition

of the Rev. Dr. Samuel Nott, of Franklin, a brother of Dr. Eliphalet Nott, President of Union College. His venerable and distinguished preceptor is still living and preaching, at the age of ninety years and upwards, in the same county in which he then lived.

He commenced his medical studies when twenty years old, under the instruction of Dr. Philemon Tracy, an eminent practitioner, of Norwich, and father of the Hon. A. H. Tracy, late member of Congress from Western New York. Dr. Peter Allen, of Kinsman, Ohio, a polished gentleman, and a physician of distinguished reputation, informs the writer, that Dr. Marshall commenced the study of medicine on the same day, and in the same office with himself: and that they remained together during their three years of pupilage, and were licensed to practise at the same time, on the third day of August, 1808.

"Many changes having since taken place in the science of medicine," says Dr. Allen, "I will mention our course of reading and the principal authors in each branch. On Anatomy we read Chesselden; on Physiology, Haller; on Theory and Practice of Medicine, Cullen, Boerhaave, Brown, Darwin, and Townsend; on Surgery, Benjamin Bell, with De Sault on Fractures and Dislocations; on Obstetrics, Denman; on Chemistry, Lavoisier. We read also Cullen's *Materia Medica*, and the *Dispensatories of the day*." "Changes," indeed, have occurred! Such as while they clearly indicate the rapid advance of our science, must also serve to illustrate the brevity and vanity of the most exalted literary reputation. It is scarcely half a century since these authors were ultimate and often sole authority in the several departments in which they wrote, and now not one of them all can be regarded as standard, or will make any pretensions to authority when not sustained by the numerous, and *perhaps* wiser generation which has succeeded them. Like venerable Fathers, they repose at ease upon our shelves, objects of love and respect, but of whose opinions upon most matters of importance we are wont to say, and possibly without having ever consulted them, "they are much behind the age."

"Dr. Tracy," continues Dr. Allen, "gave the most unwearied attention to our studies, and frequently took one of us with him in his visits to the sick; and it is presumed that Dr. Marshall would have attributed much of his success in after life to the attention and instruction of our excellent preceptor."

"Two years of the three in which we were together, we occupied the same room and bed, and of course I had a good opportunity to become acquainted with the character of Dr. Marshall. As a student, he was thorough and industrious; his judgment sound and discriminating, and

he passed over nothing lightly, nor without a clear and complete comprehension of its meaning. Although not then a professor of religion, he was conscientious in the discharge of what he considered duty, and he scrupulously avoided what he believed was wrong. His manners were conciliating, and his conduct always marked by discretion. I do not know that he ever made an enemy, or gave momentary offence."

This is the testimony of one who knew him, at that time, more intimately than any other person now living, and who thus early, in the character and habits of the student, was able to predict the future respectability and success of the man. Integrity and talent devoted to any particular pursuit, is an investment from which the owner never fails, sooner or later, to receive profitable dividends.

Having been examined by the Censors of the County of New London, Conn., as has been already stated, with his friend Dr. Allen, in August, 1808, he was declared qualified, and was licensed to practise medicine and surgery under the diploma and seal of the "Connecticut Medical Society." A few months after, he opened an office in Oxford, Chenango county; but although his success and prospects were exceedingly flattering, he soon became dissatisfied, and tempted by the fertility and rapid settlement of the western portion of the State, he removed, in October following, to Mayville, Chautauque county, where he resumed and continued the practice of his profession with eminent success, for six years. Here, also, he was married, in September, 1810, to Ruth, daughter of Orsamus Holmes, Esq., a gentleman of high respectability, and one of the earliest pioneers of Chautauque county. Mrs. Marshall has survived her husband, and still lives among us, esteemed and respected no less for the many feminine virtues which adorn her character, than for the rank once occupied by her deceased and lamented partner.

Having received an appointment as Surgeon to the Second Regiment of the New York State Militia, on the 15th of April, 1812, during the war with Great Britain, he was ordered by Col. McMahan, Dec. 20, 1813, to join the regiment at Buffalo, to which he promptly responded. After about five months service on this frontier, his regiment was disbanded and he returned home. But on the 1st of August, 1814, he was again ordered to report himself speedily, the next day, at Buffalo, at the house of Mr. Perry, the old rendezvous of his regiment. The season and the locality, together with the privations and exposures of a camp life, now conspired to produce among both officers and privates of the regiment a great amount of sickness, especially fevers, diarrheas and other similar diseases

incident to miasmatic localities. During the whole of August and September, the hospitals were crowded, and many went home on limited furloughs. Upon Dr. Marshall alone, as senior officer, devolved a great portion of the hospital duties, and after about four weeks of unremitting toil, worn down with labor and care, he also fell sick, and removed on a furlough of twelve days to his home in Mayville. He did not return, however, for several weeks, and he was even then in a very feeble state of health, yet he resumed his duties and continued at his post until in the fall following, when his regiment was discharged. It is believed that for many years afterwards he suffered in the spring and fall from a recurrence of biliary derangements, the origin of which was distinctly traceable to this campaign.

On the first organization of Chautauque county, in February, 1811, Dr. Marshall was appointed under Daniel D. Tompkins, then Governor of this State, County Clerk; which office he held, and the duties of which he continued to perform, except when absent with his regiment, until his removal to Buffalo in March, 1815.

It was in this latter place, where a wider field was presented for the display of his talents, and for the appreciation of his skill, that he arose to the highest distinction in the ranks of his profession. No man has acquired in our city, a more solid and extensive reputation as a practitioner of medicine, nor a more enviable name as a citizen. As evidence of the first, the writer can refer to the universal testimony of those who were his cotemporaries, and who still survive him; by *all* of whom, we believe, his memory is held in respect, and his example presented as worthy of imitation. Nor can we avoid arresting attention to this fact, as proof, if proof is needed, that elevated rank in our profession does not necessarily invoke upon its possessor either the jealousy or enmity of his brethren, and that no one receiving the continued reproofs and censures of his fellow-citizens and professional rivals may cherish this as welcome evidence, that he has either talent, integrity, reputation, or any other truly enviable distinction. As evidence of the estimate placed upon his professional attainments abroad, we may mention that he was made an honorary member of the "Medical Society of the City of New York," and also in 1838, of the "Medical Society of the Geneva College," as a mark, writes the Secretary, "of the respect in which your professional reputation is held by the members of this Society."

Dr. Marshall received also, at different times, various appointments under the State and municipal governments, which sufficiently indicate his

position as a citizen. In February, 1818, he was appointed Clerk of Niagara county, then embracing the present county of Erie. He held, also, in 1832, the office of Health Physician to the city.

On Saturday evening, Dec. 2, 1838, Dr. Marshall was attacked with the sickness which, on the following Thursday, terminated fatally. His disease was a pleuro-pneumonitis, and its progress was both rapid and severe, being accompanied in most of its course with great physical suffering. Yet his death was calm and cheerful. He died with the dignity of a man, and the confidence of a Christian.

In reviewing the character of Dr. Marshall, with a view to present a summary of his most striking traits, it has occurred to us that it will be comprised in a single extract from the discourse preached at his funeral, by the late Rev. Dr. Hopkins, which we shall therefore quote:

“Time would fail me were I to attempt to dwell on the habits of integrity, punctuality and scrupulous exactness, which distinguished him in all the business of life. And while these severer virtues are mentioned, it may not be forgotten that his heart was the seat of the more soft and winning virtues. Amiable and kind in the social circle, he gathered the full confidence and affectionate esteem of those to whom he became known.

“Perhaps I cannot better complete the sketch than by saying that his character possessed great *harmony* and *consistency*. It is frequently the case that some one trait is cultivated at the expense of another; so that, in the same individual one excellence is more marked by its prominence above others, or by its contrast with them. In Dr. Marshall, however, the chief elements of a good character as a man and a citizen were admirably blended. And, as when the different colored rays of light combined in their proper relations and proportions constitute that mellow ray which is at once beautiful and useful, so in him, those several traits which compose the character of the good citizen and useful man seemed to be so harmoniously blended that it would be difficult to distinguish their boundaries. Firmness and candor, seriousness and cheerfulness, humility and dignity, not only appeared in him to be reconcilable virtues, but each appeared to reflect a glory on the other. These produced great symmetry of character, and great evenness of conduct.”

Committee, { J. TROWBRIDGE,
FRANK H. HAMILTON.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

On the treatment of Dropsical Affections, by Injections of Iodine, &c.
By DANIEL BRAINARD, M. D., Prof. of Surgery, in Rush Medical College.
In the Nos. of this Journal for Jan., 1848, and Feb., 1849, I related a case of Spina Bifida, treated by injections of Iodine and Iodide Potassium in solution.

The number of injections used in that case was fifteen, and the time required for the cure ten months, viz.: from December, 1847, to October, 1848. The strength of the solution used varied from $\frac{1}{2}$ gr. Iodine, and gr. j. Iode Pot. to the f. oz. Dist. Water to gr. jv. Iodine, and gr. vj. Iod. Pot. to the f. oz. of water.

The cure was perfect and permanent, the patient remaining in good health to the present time.

This, the first of my published cases, was not, by any means, the first case of dropsical effusion in which I had used injections. As early as Dec., 1845, I threw into the peritoneum gr. xv. Iod. Potass, dissolved in f. oz. j. dist. water, after having, as perfectly as possible, evacuated the fluid, by tapping. An acute, smarting pain was produced, which subsided in a few minutes, and no sign of inflammation followed. The patient returned home, and I lost sight of him.

Since that time, Dr. F. Hagemann, of this city, injected the peritoneum of a dropsical patient twice, at my request. I have not notes of the case; it was one of incurable organic disease, but the injections produced no inflammation.

During the past winter I injected the abdomen of a patient laboring under general dropsy, as well as ascites from disease of the heart, with gr. jv. Iodine and gr. viij. Iod. Pot., in solution without drawing off the fluid. It was followed by no signs of inflammation, but the fluid in the peritoneum was absorbed, and a great amelioration of symptoms followed.

These were all supposed to be hopeless cases of disease in which the injection was used, not with the expectation of cure, but to ascertain the danger which there might be of producing inflammation. The result, in that respect, was perfectly satisfactory. I can now also add two other cases treated in a similar manner; one by Dr. M. Clure, of Dundee, Ill., and another by Prof. Mussey. These gentlemen have stated to me that they have recently used the injection without inflammation resulting. In the Am. Journal of Medicine, for Oct., 1847, p. 491, is the report of a case of ascites, cured by Mr. Leriche, of Lyons, in this manner. The injection was strong, and made, in this instance, after all the fluid had been drawn off.

This practice seems to have been tried with success a long time ago. Pereira speaks of it, and refers to the philosophical transaction of 1744.

Samuel Cooper, in his *Surgical Dictionary*, says he has seen two fatal cases from the practice. As they were both, as far as can be judged, from the injection of alcohol, after the fluid was evacuated by tapping, it is only surprising that one of the patients should have survived for a long time. Such cases do not authorize us to reject the practice when properly conducted, or justify the conclusion that it is necessarily dangerous.

It may, then, be considered an established fact that injections of this kind may be made, with suitable care, without danger of producing inflammation. It remained to be shown that dropsical effusions could be cured without such inflammation or adhesion of the sac in which it was situated.

A man applied to me during the past winter, with a large hydrocele, of eighteen months' standing. Injected f. dr. j. of a solution of Iod. Potas. and Iodine, gr. j. of the latter to gr. xij. of the former, to the oz. of water, without drawing off any fluid, the solution being thrown in through an exploring canula. This was subsequently repeated twice, and at the end of six weeks scarcely any trace of effusion remained, the man having continued his labor, and felt but slight tenderness in the organ.

Three cases of Spina Bifida have recently fallen under my care, all associated with hydrocephalus, in a greater or less degree, and rendered, therefore, unfavorable for perfect cure. I did not, on this account, neglect the opportunities they offered to test the effect of the Iodine treatment.

CHICAGO, April 12, 1849.

Case I.—I was called to see an infant, delivered by a midwife about an hour previously, affected with a Spina Bifida, situated upon the lower part of the lumbar division of the spine, which had been ruptured during labor. It seemed to have been about the size of the closed fist. At the point where it was ruptured, the sac was formed by little else than the membranes, being almost transparent over a space of one inch in length and two lines in breadth. The child was cold, and breathed with difficulty. I laid open the sac to the extent of the transparent part of its coverings, and saw the opening into the spinal canal, which was small—not large enough to admit the point of the little finger. Folding the collapsed sac together, I placed a compress upon it, and a band around the body, and directed the child to be warmed by artificial heat.

13th. Found the serum which had escaped and dried upon the compress, had closed the opening, and that the sac was distended with serum. Child warm, and has taken food.

14th and 15th. Remains in the same state.

16th. The sac has opened and discharged serum slowly. Child shows signs of spasmodic action. I re-opened the sac, and touching the surface around the opening into the spinal canal, with Nit. Arg., again folded the sides together and applied a compress, and bound it with as much force as was judged safe.

17th. Sac much inflamed; discharge of reddish serum. Child restless and feverish, but takes the breast. The discharge continued colored slightly, and gradually became offensive till the 25th, when I injected the sac with the following solution: Iodine, gr. jv.; Iod. Pot., grs. xij, to the oz. of distilled water, most of which escaped, but some was doubtless retained by the compress.

27th. Discharge less offensive and yellowish; sac much contracted, and of a deep red color. Child appears well, and takes the breast freely.

30th. Injection of oz. ss. of sol., as before; discharge purulent; less offensive.

May 2d. Injected it with sol. Sul. Copper, gr. j. to the oz. of water. Child well; discharge thin and purulent.

May 4th. Repeated the same. Child well; tumor subsiding.

6th. Repeated the injection.

May 12th. Opening closed. Child appears in health; head enlarged—bones separated.

During the whole of this time the child grew, and seemed in perfect health until

June 1st. Child died suddenly, of convulsions.

This case undoubtedly resulted fatally, from closing the fistulous opening in the back too soon, but it illustrates, in a striking manner, the favorable effect of injections into the spinal canal, and the little danger of inflammation to be apprehended from their use.

I was not at the time aware of the danger of keeping compresses over such an opening, and held, in common with the whole profession, the erroneous opinion that the danger in cases of inflammation of the meninges of the brain and spinal cord was greatest where an opening existed.

This is an error which has recently been pointed out by Dr. Thompson, of Columbus, Ohio. Far from closing such an opening, the sac should be punctured in cases of Spina Bifida, when violent inflammation of the membranes results from treatment and injections used, if the discharge is profuse and offensive.

Case II.—Spina Bifida, complicated with Hydrocephalus, treated with injections of Iodine.

Feb. 12, 1850. Was called to see a female child, three months old, born at seven months, excessively feeble at birth, and affected with Hydrocephalus and Spina Bifida. The tumor occupied the lumbar region; was $3\frac{1}{2}$ inches in length, $2\frac{1}{2}$ in breadth, and $1\frac{1}{2}$ in elevation; elastic, fluctuating, semi-transparent.

I made a puncture with an exploring trochar 2 lines from the base of the tumor, and without drawing off the fluid injected 2-3 of a dr. of the following solution: Iodine gr. j.; Iod. Pot., gr. iij.; Aq. Dist., f. oz. j.; making 1-12 gr. Iodine, and $\frac{1}{4}$ Iod. Pot. thrown in.

Feb. 13th. Tumor tense and hot; pressure on it produces twitching of the limbs. Directed the child to be laid upon its face, and evaporating lotions applied to the part.

14th. Child appears well; tumor tense.

17th. Tumor flaccid, and reduced one-fourth in size. Injected 1-6 gr. Tannin in 2-3 f. dr. j. Dist. Water.

18th. Child restless and feverish; tumor hot, tense. Applied evaporating lotions, and gave a purge of Castor Oil.

19th. Child appears well. Applied solution of Gun Cotton to tumor.

March 2d. Tumor which had been flaccid and greatly reduced in size, commences to increase. Injected Sol. Tannin, as before, and with the same result.

7th. Injected gr. ss. Iodine, and gr. j. Iod. Pot. in f. dr. j. Distilled Water. But slight symptoms of inflammation resulted. Tumor diminishing rapidly in size, and becoming firmer, and of a flesh color.

10th. Repeated injection. Soon after the injection, twitching of the

members occurred; the solution was therefore drawn off, and the sac partially filled with Dist. Water. The twitching immediately subsided. The child took the breast, and appeared in its usual health, and continued so for eighteen hours. At the end of that time, in the night, the mother noticed the child to be unwell, and, lighting a candle, found it in convulsions, of which it died. Such was the account given by the parents, who were sleeping in the bed with the child. Some circumstances prevented me from giving implicit credit to the account.

Prof. Davis assisted at the examination of tumor, twelve hours after death. The opening into the spinal canal was entirely closed. The sac contained f. dr. j. clear serum; its walls were thick and firm, without any effusion of lymph or unnatural vascularity. The head was tense and had increased rapidly in size for a few days before death. Dr. Davis coincided with me, in the opinion that there was no sign of irritation about the tumor, to account for the death. On the contrary, the effect of five injections in four weeks, in diminishing the size of the tumor, thickening its walls and closing up the opening into the spinal canal, without symptoms of inflammation, furnish the best evidence which could be desired of the efficiency and safety of the treatment. The Tannin was twice substituted for the Iodine solution, under the supposition that its astringency would be more effective in preventing effusion; it was found, however, to produce more inflammation, with less tendency to diminution afterward, and was therefore discontinued.

It may be stated that, from the time of the first injection, the child, which had before been puny, began to thrive, and grew rapidly up to the time of its death.

III.—Case of Spina Bifida and Hydrocephalus, treated with Iodine injections.

Sept. 20, 1849. Saw a female child of Mr. R., aged 4 days, affected with cleft spine and Hydrocephalus.

The cleft of the spine was situated at the lower part of the lumbar region, was about 3 inches in length and $1\frac{1}{2}$ in breadth; the abdomen was prominent, particularly on each side, the divisions of the spinal column, movable, showing that the want of union was not confined to the processes, but extended to the bodies of the vertebræ.

There was a fluctuating tumor situated over the fissure, about $\frac{3}{4}$ of an inch above the surface of the surrounding skin. Its covering was then of a blueish color, semi-transparent, and seemed composed of the thin membranes of the spinal cord, and the cuticle alone. Indeed, this latter was wanting over a considerable surface, where an ulceration of the size of a shilling existed.

The head was of large size, measuring 19 inches around the frontal and parietal protuberances. The upper part of the parietal and occipital bones was wanting, and the head bulged out into a very irregular, tense, and perfectly elastic sac. The other bones were imperfectly ossified and separated. The head was hot, covered with large veins, and increased rapidly in size. The right foot was affected with a Talipes varus, and the movement of the lower extremities was feeble.

The urine dribbled away, and excoriated the skin extensively.

Applications of Nit. Arg., in solution, were directed for the ulcerated

spot until it was nearly healed. Cold water was applied to the head to keep down the heat.

Oct. 9, at 12½ o'clock, the treatment was commenced as follows:—

A very minute puncture was made with the point of the lancet, through the skin, half an inch below the tumor of the back, and a small sized exploring trochar passed from it obliquely upward into the sac. Without allowing the escape of more than a few drops of fluid, f. dr. ss. of a solution of Iodine, of the following strength was injected. Iodine, gr. j. Iod. Pot., gr. ij.; Aq. Dist. f. oz. j. Making 1-16 gr. of Iodine and ½ gr. of Hyd. Potas. in the injection. The puncture and surface of the tumor were then covered with a solution of Gun Cotton. No pain was felt except at the moment of puncturing the skin, and from the application of the liquid plaister. I was assisted by Professors Evans and Herrick, and Dr. Meek, editor of the N. W. Journal of Medicine.

10th. 12 o'clock, M. Found the child had been restless from the time of the operation until 6 o'clock, P. M., when it became quiet; seems well. Veins of head less turgid than since its birth.

11th. Child appears well; head less tense. Applied adhesive liquid to tumor and continued cold water to head.

12th. Head tense; increased in size ¼ inch. Tumor of back flaccid, flattened and slightly inflamed.

13th. Tumor of back diminishing; head increasing rapidly. Ulceration of back quite cicatrized.

In consequence of the rapid increase in the size of the head, I determined to inject it without waiting, as I had intended to do, for the cure of the back to be completed. This was accordingly done with the assistance of Drs. Evans and Rutter. The point chosen was that most projecting on the summit and left side of the head.

About ½ dr. of fluid was allowed to escape. The same quantity of the solution (of the strength before given) injected, and the puncture covered with adhesive solution. Child seemed to suffer no pain except from puncture and application of ether upon the point.

14th, 15th, 16th. Head hotter, and increased in size; appears well.

16th. 8 o'clock, P. M. The puncture of head commenced leaking, which was stopped by Dr. Herrick with collodion.

17th, at 4 o'clock, A. M., leaked again, and was stopped by a compress and bandage. Head flaccid; child irritable, sleepless, and starting.

19th. Head filling. To avoid danger of leaking from former puncture, 1½ oz. of fluid was drawn off by a very oblique puncture on the right side.

20th. Child restless; affected with apathæ. Head hot.

21st. Child appears well; head natural.

31st. Head, which was stationary, has commenced to increase; measures 20 inches in circumference. It was injected at 5 o'clock, P. M., with f. dr. jss. of the solution. The puncture was made obliquely upon the summit of the right side.

Nov. 1st. For 24 hours, no effect was perceptible. At the end of this time, it commenced to be restless and sleepless, twitching often, and declined the breast. Evaporating lotions were applied to the head; a cathartic of Castor Oil administered.

2d. The child was much relieved, and took the breast. Head sta-

tionary—the whole of the back part of it covered with a red blush which had gradually spread from the puncture.

3d. Child restless. Head hot and red at the back part.

4th. Heat less; redness almost gone; head diminished in circumference, 3-4 inch in 24 hours.

5th. Child restless; head flaccid, diminished in size. Applied a band of India rubber, 4 inches broad, about it, to compress it.

6th. Has slept well from the time of application of band. Head diminishing.

7th. Continues to diminish. India rubber exchanged for a band of cotton.

From 7th to 24th, pressure continued; little change in head; remains stationary, at about 19 inches in circumference.

24th. Threw in 3-4 of a dr. of solution; no effect.

Dec. 2d. Injected f. dr. jss. of solution. Head increasing; no effect.

15th. Head soft; diminished in size $\frac{1}{2}$ inch in 24 hours.

At the first injection, but little fluid was allowed to escape. It was transparent, and showed, under the microscope, some epithelium cells. At the last operation, about one oz. was withdrawn; it was of a straw color; gave a slight cloud on application of heat, and was gelatinous on evaporation.

18th. Injected f. dr. jss. of the following solution: Iodine, iij.; Iod. Pot., gr. vj.; Aq. Dist., oz. j.; no effect.

30th. Threw in f. dr. jss. of the following solution: Iodine, grs. jvss.; Iod. Pot. gr. xij.; Aq. Dist. oz. j. No effect.

1850, Jan. 5th. Head is increasing rapidly; measures 22 inches in circumference.

Injected gr. ij. Iodine; grs. v. Iod. Pot. in f. dr. ij. Dist. Water. The fluid drawn off this time amounted to oz. iij.; was clear: exhibited, on standing, a filamentous appearance, like an exceedingly slight coagulum, and yielded, with starch, no trace of Iodine.

9th. Child was restless, and head slightly hotter than usual, after the last injection.

12th. Child well; head flaccid. Drew off oz. xij. fluid, and injected the same quantity as before, of the solution.

15th. Child slightly restless after injection, but soon became quiet, and exhibited no effect except a free secretion of tears and mucus from the eyes.

18th. Injected gr. jv. Iodine, and Iod. Pot., gr. x. in f. dr. vj. water.

19th. No effect except the secretions from the eyes.

22d. Child in perfect health; appears unusually bright, and is very active, moving its hands actively, and its feet slightly. Tumor of back reduced to a level with the surrounding skin. Urine retained naturally, and the excoriations healed.

26th. Drew off oz. xvj. fluid, and injected gr. viij. Iodine, and gr. xxjv. Iod. Pot., in water, f. oz. j.

Scarcely any effect was produced by this injection. The head was stationary for about four days after the injection, and then gradually increased. Compression by adhesive straps, bandages and Gun Cotton were tried perseveringly, without effect. Head 24 inches in circumference.

February 4th. Drew off f. oz. xxjv., and injected gr. xij. Iodine, and

gr. xxxvj. Iod. Pot. in oz. j. water. No effect was produced at the moment; but in the evening, 12 hours after the injection, there was twitching of the limbs and vomiting. The matter vomited colored the starched dress of the child of a dark color. The dress, diapers, bed and handkerchief used on it were impregnated with the Iodine odor for about three days after the injection, but the most careful tests, by Prof. Blaney, with starch, after adding chlorine water, never detected any trace of Iodine in the fluid drawn from the head.

19th. For several days the head remained stationary, but has commenced to enlarge. Injected gr. x. Iodine, with gr. xxx. Iod. Pot., after evacuating oz. xxjv. fluid. Head in this and other cases, when much fluid was drawn, supported with adhesive straps and bandages.

Effect the same as in the last instance; cold lotions were applied to the head, and purge of Castor Oil given.

March 5th. Head increasing. Drew off xxjv. oz. fluid, and injected the same quantity as before. Applied a laced cap to the head.

The symptoms resulting from this injection were but slight.

9th. Drew off vj. oz. fluid, and repeated the same injection. This quantity reduced the head to the same size as before, by which it appears that $1\frac{1}{2}$ oz. serum formed daily.

Remark. During the treatment for the last two months, the child has grown rapidly; the face, which was triangular and thin, has filled up, is fat and firm; the skin, from being pale, is become ruddy; the limbs are firm, and the skin of the head, from having been glistening, smooth, with but little hair and large veins ramifying over it, is covered with a thick growth of hair, and appears very natural; the bones of the opposite sides touch, and the head is 20 inches in circumference; the bones being thick and firm. The tumor of back is entirely obliterated, and the skin in its former position becoming firm and of natural color.

10th. No effect; head paler than natural.

11th, 12th. Child restless.

15th. Is in good health. Drew off f. oz. vi., and injected gr. vij. Iodine, with gr. xxj. Iod. Pot. in oz. j. of water.

20th. No effect from last injection; used gr. x. Iodine, with thrice the quantity Iod. Pot. in the oz. water. Fluid drawn, oz. vj. A few bubbles of air probably passed in with the injection.

21st. Has had twitching. Head pale.

27th. Drew off the same quantity, and injected as before.

28th. Little effect; dress colored by matters vomited.

April 3d. Repeated the injection with gr. xij. Iodine, and gr. xxxvj. Iod. Pot. to the f. oz. of water. It produced vomiting and purging of matters which colored the clothing. It is probable the materials used were not pure, as they were not procured from the usual shop, and had a different effect from any former injection. Since the first commencement of the treatment it had been noticed that immediately after tapping, or while the head was diminishing in size, the child was wakeful, nervous, and affected with muscular twitching, on the slightest cause, while, on the contrary, it was quiet and had a tendency to sleep, when the head was full and increasing in size. Recently this tendency to somnolence has greatly increased—so much so that it could be at times scarcely roused. It was evident the disease was approaching its natural termination.

11th. Threw in gr. ix. Iodine, with gr. xxvij. Iod. Pot. in ℥. j. Dist. Water, after withdrawing oz. vj. liquid. Slight twitching of the limbs the next day.

17th. Repeated the same quantity, after withdrawing ℥. viij. liquid.

After about 12 hours had elapsed, the child was affected with severe and dangerous symptoms, twitching, vomiting of matters which colored the clothing dark. These symptoms continued 48 hours, and were the worst the child had experienced since the commencement of the treatment.

The analogy supposed to exist between Spina Bifida and Hydrocephalus had at first given rise to the expectation that the treatment of the latter disease, by injections of Iodine, would be found efficient in its cure. This hope, after a fair trial, had in this case been abandoned, but the treatment was still continued, in the hope that its obvious effect in retarding the increase of the head might enable the bones to close at a size which would allow of health and intelligence in the child.

The gradually increasing severity of the symptoms, following each injection, and the tendency to somnolence and diminished activity and intelligence of the child, suggested the propriety of suspending their use.

Accordingly, on the 2d May, I drew off viij. oz. fluid, without any injection, the tension of the head and somnolence rendering such relief necessary. For two or three days, this afforded relief, but fluid accumulated with unusual rapidity, so much so that during my absence, in attendance on the National Medical Convention, at Cincinnati, the head attained a great size, the child became somnolent, and Prof. Herrick was called on to tap the head, which he did, May 12th, to the extent of vj. oz., with but partial relief.

It was in this condition that the head was found on my return, May 14. It was greatly increased in size, hotter than usual, covered with large, branching veins, and the child somnolent.

Notwithstanding the approaching fatal result of the case was apparent, yet the rapid aggravation of the symptoms under the treatment by tapping alone as compared with that by tapping and injection, induced us to resort again to this latter, as a palliation; accordingly, on the 15th, I threw in gr. vj. Iodine, with thrice that quantity Iod. Pot. in oz. j. water. The amount of fluid previously drawn off was oz. xij.

No perceptible effect was produced for 12½ hours, when the child commenced vomiting and purging, fell into a convulsion and died immediately.

EXAMINATION AFTER DEATH.

The back, in the situation of the former tumor, was found smooth, firm, with but slight traces of disease. A cure of the Spina Bifida had evidently been effected.

On laying open the cranium, two-and-a-half pints of clear fluid was discharged. This was contained, principally, in the enlarged ventricles, there being but a thin layer on the surface. The arachnoid membrane was in a perfectly natural state, thin, polished, delicate; without any thickening or opacity. At the point where all the punctures but two had been made, the two layers of the arachnoid were closely adherent to each other. So that all the injections had been made into the ventricles, and not, as I had thought probable, into the sac of the arachnoid. The deception, in this respect, arose from the extreme tenuity of the ventricular walls, which

over the upper part of the cerebrum were scarcely a line in thickness, and at points composed of the investing membranes of the organ, the cerebral substance having entirely given way, so that the canula could be easily felt, when in the ventricle, by the finger on the outside of the head.

Descending to the lower surface of the cerebrum, the walls of the cavity became thicker. The optic thalami, and corpora striata, although separated from each other, were entire, and, with the exception of flattening, in their natural state. The corpora quadragemina, medulla oblongata, and cerebellum, were well developed and healthy.

The internal surface of the immense cavity, was composed of white nervous tissue, firm and smooth, except at points where there were slight elevations, without any sign of lymph effused or unusual vascularity. Indeed, the whole substance of brain and its membranes, seemed less vascular than natural. The corpus callosum, extremely thin but not divided, formed the roof of the cavity. The *iter ad infundibulum* was dilated, but the *aqueduct of Sylvius* seemed not pervious without pressure. On the floor of the cavity lay the choroid plexus hypertrophied, and resembling a congeries of mucous follicles, rather than the natural condition of the membrane.

Between the cerebrum and the cerebellum was found a cyst of the size of a small plum, containing a serous fluid. On opening this, there was found a small surface, resembling entirely that of the choroid plexus in the large cavity.

This circumstance, together with the perfectly healthy appearance of the arachnoid, induced Prof. Herrick, who assisted me in this examination, to believe that the common opinion, that *internal* chronic Hydrocephalus originates from the arachnoid membrane, in so far, at least, as this case is concerned, is erroneous. He thinks the diseased choroid plexus was the source of the fluid. On examining that tissue with the microscope, his opinion (in which Prof. Davis concurred) was confirmed, for he found in this tissue, a great number of cells, arranged in groups, or in form of racemes attached to a common foot-stalk.

I am under the deepest obligation to Prof. Herrick, for assistance during the treatment of this case. I was also assisted by Profs. Evans, Blaney, Davis, Dr. David Rutter, and several other medical friends.

The inferences which I think may be legitimately drawn from these cases, are:

1. Injections into cavities affected with Dropsies, may be made without great danger; the violence of the symptoms will depend upon the strength of the injection, the quantity of the fluid present, and the duration of the disease. Old cases, when the membrane is thickened, or when the quantity of fluid is great, or when the surface has gradually become accustomed to the presence of the fluid, are those in which there is least danger to be apprehended.

2. Spina Bifida is generally a curable disease by such a treatment. Still I would by no means assert that success can be always expected. It is sometimes associated with mal-formations or incurable disease. The internal surface of the sac is often inlaid, in a great part of its surface, by the trunks of the sacral nerves. A puncture of one of these, whether in tapping, injecting or acupuncture, may be followed by serious results.

This frequent presence of nervous trunks, is a fatal objection to excision of the coverings of the tumor, to fetch the edges together by stitches.

In addition, Erysipelas or convulsions may occur in such cases, from a simple puncture.

Still, in general, I have no doubt the treatment will be found, as compared with any other known method, safe and efficient

The following are the circumstances to which I would particularly call attention as necessary to its safety and success.

Using not more than 1-32d part of a gr. Iodine, and three times as much Iod. Pot. at the commencement, dissolved in Dist. Water, and *be sure it is of perfect purity, and the solution recently made.*

As long as it produces moderate inflammation, do not increase the quantity.

Make the puncture 1-4th inch from the base of the tumor, in sound skin ; it is much safer.

Do not evacuate the tumor, or, if it escapes, replace the fluid drawn off by Dist. Water.

If spasms should occur, the fluid might be withdrawn and Dist. Water injected.

The child should be laid upon its face, and if heat and tension occur, cold be applied to the part and head.

As soon as the tension is past, collodion should be applied and re-applied as long as it continues to diminish in size. When it ceases or increases, a new injection should be made.

3. The cases of ascites treated by me thus far, in this method, were such as offered favorable opportunities for testing injections in reference to the inflammation they might produce ; it was necessary first to remove an erroneous opinion concerning their danger. I regretted, afterwards, not having continued their use, with the object of effecting a cure.

It is probable that inflammation has little or no connection with the cure of Dropsies by injection. It is only necessary, in such cases, to change the elements of the fluid, in some considerable degree, and the tendency to effusion ceases, the action of absorption becomes more rapid. It might be supposed that the case here narrated, of Hydrocephalus, militates against this opinion. We have already shown that it is most probable that the fluid in that case was the result of secretion from a morbid glandular structure in the choroid plexus, and not, by any means, an exhalation from the surface of the arachnoid membrane. If it were otherwise, and Hydrocephalus were analagous to ascites, anasarca and other serous effusions, the want of success in curing it would not militate against its utility in other cases. To any one who saw the child at the commencement of the treatment, and observed the rapidity with which the head increased, there could be no doubt that, without treatment, the term of its life could be but a few days or weeks, at farthest. That the treatment cured the back, removed, in a great degree, the paralysis, and modified and retarded the progress of the disease in the head, will afford encouragement for applying it in any case less certainly fatal than that disease.

The effect of these injections, in filling and saturating every part of the system with the medicine, should not be overlooked. In the hydrocephalic child, the most violent symptoms followed effusion of the Iodine into the stomach. There was no salivation, as is the case when the medicine

is thrown into the veins. Several years since, having occasion to treat a vascular tumor, of a doubtful nature, I threw into it a small quantity (the notes are not at hand) of Iodine and Hyd. Potash, in solution. In a few seconds the patient experienced a bitter taste in the mouth, and a free salivation which lasted a week. It was composed simply of enlarged veins, and the medicine had entered the circulation.

Convinced of this fact, the operation was repeated with the same result. In the case of injections into the head, on the contrary, the effect seemed to be to invigorate nutrition, and fatten the child, which grew, notwithstanding the effects following each operation. The importance of being acquainted with the action of other substances than those used thus far, when introduced into the cavities and tissues of the body, the effects they may produce in other diseases, such as ovarian cysts, anasarca, &c., might, with propriety be considered here. They will, however, naturally occur to the reflecting mind, and I shall defer their further notice until I can communicate some facts calculated to throw light on the subject.

CHICAGO, May 22d, 1850.

Autopsy revealing the Absence of Gall Bladder. By D. B. TRIMBLE, M. D., of Burlington, N. J.—Under the impression that whatever will tend to enlighten us, in relation to the often obscure symptoms of disease, is worthy a place in the records of medical science, however humble may be the effort, I send for insertion in the Reporter, an account of a post-mortem examination of some interest.

The subject of examination was a woman, aged fifty-five years, whom I was called upon to attend, on the sixth of January.

I found her complaining of pain in the epigastric region, nausea, occasionally vomiting, slightly furred tongue, pyrosis, anorexia, and constipation. All these symptoms had been progressing for several months, though she was still able to attend to her household duties. Firm pressure on the epigastric region gave her pain, but not acute. Her pulse was frequent, though not full, or tense. As her disease progressed, she twice, at considerable intervals, and for some hours, complained of pain in the shoulder; her appetite became more and more depressed; emaciation increased rapidly, ascites supervened, and death released her on the 24th of June.

From the symptoms, I considered it a case of chronic gastro-enteritis, approaching the stage of ulceration, and the treatment was in accordance with these views. The autopsical examination, however, showed that my diagnosis was partially wrong, but at the same time proved that no medical efforts would have relieved her.

On the evening of the 24th, accompanied by my friend Dr. Gauntt, I proceeded to the investigation, and on opening the abdomen, found it filled with serous fluid, of a reddish color, in quantity about one gallon. The colon, and the small intestines, were lying exposed, and the only appearance of an omentum was a strip about an inch in width, attached to the stomach. We next examined the intestines, commencing at the rectum, which was filled with ash-colored fæces, and inflamed. The cæcum, though less inflamed than the other intestines, had bright red patches throughout its coats; the colon, jejunum, and ileum, showed strong evidences of inflam-

mation, being of a dark mahogany color, and the coats of the small intestines much thickened. The mesenteric glands were discolored, but not enlarged. The duodenum was much contracted in size, and, about that portion of it where the pancreatic duct, and the ductus choledochus penetrates it, was adherent to the liver and pancreas. The *stomach* presented no signs of disease. The liver was diminished in size, the stomach being uncovered by the left lobe; the right lobe was also smaller, and the portion attached to the pancreas together with that viscus, were of a cartilaginous character. No *gall-bladder*, or any vestige of one, could be found, after the most careful investigation. Upon opening the cartilaginous portion of the liver, a gall-stone was found imbedded in the ductus choledochus, just at its entrance into the duodenum. It measured in its long circumference two and three-quarter inches; in the small circumference two inches, and weighed one hundred and nine grains. It is of a dull red color with patches of a white crystalline coating. The uterus was healthy in appearance, though filled with a semi-organized substance. Between the insertions of the round ligament, and the fallopian tube of each side, was a tumor, on the left side about the size of a large pea, and of an osseous consistence; on the right as large as a hazel-nut, and cartilaginous. They were so situated, that the slightest pressure on them would close the orifices of the fallopian tubes. She had been married twenty-five years, but had not conceived. Could these tumors, by their mechanical effect, be the cause? The ovaria were healthy.

She had suffered for some years with the usual symptoms of indigestion; but had not, at any time, very acute pain in the *right* hyochondriac, or epigastric regions; at least none that could cause a suspicion of the passage of a calculus of the ordinary size.

From the absence of the gall-bladder, no cystic bile could have been secreted; and from the situation of the stone, no bile could pass into the duodenum, thus seriously deranging digestion and nutrition, producing the inflammatory condition of the intestines, resulting in death, and the *cause* of which, no art could remove.

The questions arise, what became of the omentum and gall-bladder?—or was this one of those abnormal formations mentioned by authors, where those organs are absent? If there was originally no gall-bladder, how was the calculus formed? or did its size produce ulceration and rupture of the bladder, and its subsequent absorption? These are interesting questions, but difficult of solution.

Upon mentioning the foregoing case to a medical friend from Philadelphia, he informed me that he recently had a case of calculi in the gall-bladder and ducts, resulting in death in fifteen hours, in which there was no pain in the *right* hypochondrium; but intense pain in the *left*, extending to the epigastrium. Two of the most eminent of the physicians of that city, saw her with him, and were not led by her *symptoms* to suspect her disease, though one of them suggested that such might be the cause. The post-mortem examination disclosed the gall-bladder filled with small calculi, one of which, not larger than a small pea, had closed the passage of the cystic duct. These cases show the obscurity of the diagnosis in biliary calculi, and should lead us to suspect their presence when efficient means are unavailably used to relieve affections of the stomach and bowels, when timely resorted to.—*New Jersey Med. Reporter.*

Homœopathia.—Anecdote.—We copy the following from a communication in the New York Medical Gazette:—

We conclude our notice of Homœopathy by relating an incident of sober truth, which not long since is said to have occurred at the Springs, and only withhold the name of our informant, and of the watering place, that the parties may not be recognized. A merchant in a neighboring city, whose business brought him in contact with the article called "Pearl Barley," filled his vest pocket with a handful of the grains when about to leave home, and amused himself by chewing them on his journey. On his arrival at his hotel at the Springs, he found a number of invalid acquaintances, who were wont to discourse on the merits of physicians, one of whom was a sturdy believer in Homœopathia, and loud in praises of the little pellets of sugar of milk. Taking from his pocket a grain of the Pearl Barley, he placed it upon his tongue in the presence of the company, and united his testimony to the value of these pellets, which the very fine grains he used closely resembled; he, moreover, informed them that he carried these treasures in his pocket, and could spare them to any who might have occasion to try Homœopathy. Very soon one and another of these invalids, ladies and gentlemen, availed themselves of the courteous proposition, and applied to the merchant for one of his little pills, to each of whom he gave a grain of Pearl Barley, accompanied by an eulogy upon Homœopathy. In every instance the patients were relieved, and in some cases the effects were wonderful; so that the astonishing virtues of the little pills became the theme of continual conversation, and converts were as numerous as patients, so that the stock in the gentleman's pocket was soon exhausted. But at length the merchant broke the charm by announcing at the public table, in the presence of these restored invalids, that he had only humbugged all parties; that his little pills were only fine grains of Pearl Barley, of which a bushel might be had for a less number of shillings than it would take dollars to fee a Homœopathist. The result of this disclosure was most disastrous, like that of Dr. Haygarth's substituting painted wooden tractors for Perkins' magnetic metallic ones; for the patients forthwith relapsed by the return of all their maladies, and before sundown the Homœopathic cures exploded.

Fatal Mistakes.—A number of instances have recently occurred in which, by mistakes of apothecaries in putting up wrong medicines, life has been sacrificed. In a recent instance at Boston, Corrosive Sublimate was sent instead of Calomel, as prescribed, and the dose of ten grains was taken by the patient, with a fatal result, as is supposed, and the apothecary has been arrested for manslaughter. His conviction and punishment, however, will not restore the victim to life; but it will not be in vain, if it shall lead to measures which may prevent the repetition of similar mistakes.

Upon this and the like cases, we remark that there are in all our cities numerous drug stores kept by parties who are wholly ignorant of the business, and dependent on drug clerks whom they employ, and who often are mere boys, knowing little more than their employers. These stores are ever changing hands, and the new proprietors go into them to learn the business of preparing and selling medicines, having often had no previous knowledge of drugs. That blunders, which may or may not be fatal, must occur under such circumstances, must be obvious.

Again: In many drug stores the business of putting up prescriptions is very carelessly done—the most inexperienced clerks being allowed to attend to it; which is another source of mistakes.

The remedy for these sources of the mischief lies first with the civil authorities, who ought to protect the health and lives of the community, by prohibiting ignorant and unqualified persons from selling drugs, or doing business as apothecaries, and this under heavy penalties. The popular outcry against monopolies ought not to prevent legislation on so important a subject as the dispensation of drugs; for there can be no doubt that there are but few detections of the fatal mistakes made by such persons as those here alluded to. And yet all these might be prevented by empowering the College of Pharmacy, or some other competent authority, to examine and license all persons claiming to keep apothecary shops, or to dispense medicines of any kind.

But a second remedy lies with the physicians, who should direct all their prescriptions to be procured of some competent apothecary, known to be such. While a third remedy is in the hands of the people themselves, who should in no case purchase medicines except at some reputable store, the proprietor of which is known to have been educated to the business, and reliable from his character and habits.

But there is another aspect of this subject which demands grave consideration by our profession. It is said that mistakes are often made by reason of the loose and careless manner in which prescriptions are written, often scarcely legible, and so abbreviated as to be read with difficulty even by well instructed apothecaries. This matter should be reformed altogether, and will in part remedy the evil.

But still another view of the subject has been taken by the public press, which is worthy of consideration. From time immemorial our profession have written their prescriptions in *Latin*, and employed technicalities in this language in writing their prescriptions. In former days, when a knowledge of the Latin tongue was universally possessed by all physicians and apothecaries, the safety of this course was unquestionable. *Sed tempora mutantur, et nos mutamur cum illis.* In these days of *progress*, when the discovery has been made by "Young Physic" that doctors and apothecaries have no need of Latin, or any other dead language; and that men can do very well in either capacity without a knowledge of even their mother tongue; and when even bold ignorance is often the passport to popularity in the art of healing, a change would seem to be demanded by "the spirit of the age."

It is now urged so clamorously, that even medical authority has been invoked, and has responded affirmatively, that all our prescriptions should be written in the English language, and so plainly as to preclude all possibility of mistake in the article or quantity ordered. And it may have become necessary thus to conform to the degenerate days which have overtaken us, when scholarship is no longer to characterize the profession. Our old physicians will find it awkward and unseemly for a time; since they have been trained in a different school, and at a time when no prescription was ever written in English by any regularly bred physician—for all such were required to write in good medical Latin; and none but those who had entered the profession by climbing over the wall, or in some other clandestine way, were ignorant of this language. And the same

might then be said of all reputable apothecaries; for in those days, a knowledge of Latin and Greek was deemed an essential pre-requisite to reputable standing in either department.

But it is not so now; "'tis true a pity, and pity 'tis, 'tis true." Hence, a necessity would seem to be laid upon us all, to write all our prescriptions so that any ignorant apothecary may read and prepare them. This has been deemed, heretofore, the only protection we had against falling into the hands of ignoramusses in the business; for by the use of Latin technicals we have sought to secure the attention of qualified persons to put up our prescriptions: but this must be relinquished, it seems, and in obedience to "manifest destiny."

Be it so: but will this prevent fatal mistakes, such as those which have recently occurred? Suppose in the case at Boston, "*Calomel 10 grains*," had been written, instead of "*Sub Mur. Hydrarg. grs. X.*" Does anybody believe that the mistake of sending half a scruple of *Corrosive Sublimate* would not have occurred precisely as it did? The apothecary either knew better, or he did not; for no intentional wrong is alledged. If he did not know what the receipt called for, how came he to be in an apothecary shop in any capacity? If he did know any better, then he is inexcusable, for the prescription in this case was correctly written. By the *old* nomenclature, calomel was called a "Sub-Muriate of Mercury;" and corrosive sublimate, either a "Muriate," or an "Oxy-Muriate." By the *new* nomenclature, the former is written "Proto-Chloride," and the latter "Per-Chloride;" and these terms ought to be familiar as household words to any tyro in a druggist's store. That they are not so is a shame and disgrace, though, as in this case, so in others, the mistakes are not always to be ascribed to ignorance, but to haste, sheer carelessness, or a recklessness of human life. To write prescriptions in English will not remedy the mischief, though it will remove all pretext for blaming the profession.—*N. Y. Med. Gazette.*

Prize offered by the American Medical Association.—The following resolution, appended to the Report of the *Committee on Medical Literature*, was adopted by the Association at the meeting in Cincinnati in May last.

Resolved, That the sum of ONE HUNDRED DOLLARS, raised by voluntary contribution, be offered by this Association for the best *experimental* essay on a subject connected either with PHYSIOLOGY, or MEDICAL CHEMISTRY, and that a committee of seven be appointed to carry out the objects of this resolution: Said committee to receive the competing memoirs until the first day of March, 1851; the author's names to be concealed from the committee; and the name of the successful competitor alone to be announced after the publication of the decision.

Dr. FRANCIS G. SMITH, Philadelphia, Chairman.

Dr. ALFRED STILLE, Philada. Dr. JAMES MOULTRIE, Charleston, S. C.

" FRANKLIN BACHE, " " ROBERT BRIDGES, Philada.

" L. P. YANDELL, Louisville, Ky. " WASHINGTON L. ATLEE, Philada.

In accordance with the above resolution, the Chairman gives notice that the sum of *one hundred dollars* is secured, and will be paid over to the successful competitor, or, if preferred, a gold medal of equal value bearing a suitable inscription.

The competing memoirs must be transmitted to the chairman, free of

expense, and should be designated by some appropriate motto; the author's name accompanying it in a sealed packet, designated in like manner. The successful essay will become the property of the Association, and in case no paper of sufficient merit is offered, the time will be extended for another year.

After the decision of the committee, the sealed packet containing the author's name will be opened in the presence of the Association.

Medical Journals throughout the country are requested to give publicity to the above notice, and to aid in furthering the wishes of the Association in this respect.

FRANCIS G. SMITH, M. D., Philada., *Chairman.*

Vicarious Menstruation.—In the June number of the *Western Lancet*, Dr. B. F. Richardson, of Cincinnati, reports a case in which at each regular menstrual period the discharge takes place from the anus instead of the vagina, and continues about as long, and in quantity is about the same as the normal menstrual discharge. The fluid does not coagulate. Its discharge is usually preceded by pain and uneasiness in the lower part of the bowels. From careful and minute inquiry, Dr. R. is satisfied that the discharge was not hemorrhoidal, and that there was no reason to suspect a recto-vaginal communication. A very singular case of vicarious menstruation was brought to the notice of the Westminster Medical Society by Dr. Rogers, last year, in which blood oozed from the tips of the fingers. —*Southern Med. and Surg. Journal.*

Proceedings of the Fifth Annual Meeting of the Association of Medical Superintendents of American Institutions for the Insane.

The Association of Medical Superintendents of American Institutions for the Insane, convened at the Tremont House in the city of Boston, on the 18th day of June, 1850, at 10 o'clock, A. M.; the President, Dr. WILLIAM M. AWL, in the chair, and Dr. KIRKBRIDE, Secretary.

Present, Dr. *James Bates*, of the Maine Insane Hospital, Augusta.

Dr. *Andrew McFarland*, of the New Hampshire State Asylum, at Concord.

Dr. *William H. Rockwell*, of the Vermont Asylum for the Insane, Brattleboro, Vt.

Dr. *Luther V. Bell*, of the McLean Asylum for the Insane, at Summer-ville, Mass.

Dr. *C. H. Stedman*, of the Boston Lunatic Hospital.

Dr. *Edward Jarvis*, of the Dorchester (Mass.) Private Asylum.

Dr. *George Chandler*, of the Massachusetts State Lunatic Hospital, at Worcester.

Dr. *N. Cutter*, of the Pepperill (Mass.) Private Institution.

Dr. *Isaac Ray*, of the Butler Hospital for the Insane, at Providence, R. I.

Dr. *John S. Butler*, of the Connecticut Retreat for the Insane, at Hartford.

Dr. *N. D. Benedict*, of the New York State Lunatic Asylum, at Utica.

Dr. *C. H. Nichols*, of the Bloomingdale Asylum for the Insane, New York.

Dr. M. A. Ranney, of the New York City Lunatic Asylum, on Blackwell's Island.

Dr. Henry W. Buel, of Sandford Hall, (Private Institution,) Flushing, N. Y.

Dr. H. A. Buttolph, of the New Jersey State Lunatic Asylum, at Trenton.

Dr. Thomas S. Kirkbride, of the Pennsylvania Hospital for the Insane, at Philadelphia.

Dr. J. H. Worthington, of the Friends Asylum for the Insane, at Frankford, Pa.

Dr. William S. Haines, of the Philadelphia Lunatic Hospital, Blockley.

Dr. John Fonerden, of the Maryland Hospital for the Insane, at Baltimore.

Dr. John M. Galt, of the Eastern Asylum of Virginia, at Williamsburg.

Dr. William M. Aul, of the Ohio Lunatic Asylum, at Columbus.

Dr. S. Hanbury Smith, of the Ohio Lunatic Asylum, at Columbus.

Dr. R. J. Patterson, of the Indiana Hospital for the Insane, at Indianapolis.

Dr. J. M. Higgins, of the Illinois Hospital for the Insane, at Jacksonville.

Dr. Edward Mead, of the Chicago Private Retreat for the Insane, (Ill.)

The minutes of the last meeting having been read, the President announced in a feeling and appropriate address, the death of three members of the Association since its last meeting: Dr. Samuel B. Woodward, the first President of the Association, and formerly Superintendent of the Massachusetts State Lunatic Hospital,—Dr. Amariah Brigham, Superintendent of the New York State Lunatic Asylum, and Vice President of the Association,—and Dr. McNairy, Superintendent of the Tennessee Hospital for the Insane.

The Secretary reported that, as instructed by the Association, he had invited the Boards of Trustees or Managers of all the institutions for the insane, in the United States and British Provinces, to attend its meetings, and had received letters in reply from the Boards of Managers of the Maine Insane Hospital, Massachusetts General Hospital, Boston Lunatic Hospital, Friends Asylum, Pa., Maryland Hospital and Eastern Asylum of Virginia. On motion of Dr. Bates, it was

Resolved, That each member of the Association be authorized to invite such gentlemen to attend its sessions as he may deem proper.

Dr. Bell stated that in consequence of a full and well-written notice of the life and professional labors of our late associate, Dr. James Macdonald, of N. Y., having appeared in the American Journal of Insanity, he would suggest the adoption of that notice, instead of preparing another, specially for the use of the Association, which was approved.

The President stated that in obedience to the instructions of the Association, he had, soon after the last meeting, selected a subject for a report for each member, to all of whom due notice had been given, and from most of whom he had received answers accepting the duties assigned them.

An invitation from the Board of Trustees of the Boston Lunatic Hospital, to visit that institution, to-morrow at 4½ P. M., was read and accepted.

On motion of Dr. Bell it was

Resolved, That in order to enable the members of the Association, while performing the regular business that may come before the meeting, so to

arrange their sessions as most satisfactorily to apportion their time, and be able to enjoy the hospitality that may be extended to them,—a business committee be appointed, who shall at the commencement of each morning session, report the papers to be read, and other matters to be attended to during the day. Drs. Bell, Bates, and Kirkbride, were appointed the committee.

On motion of Dr. Rockwell, it was

Resolved, That a committee of three be appointed to prepare names to fill any vacancies that may exist in the offices of the Association. Drs. Rockwell, Benedict, and Kirkbride, were appointed the committee.

Dr. Rockwell, from the committee to fill vacancies in the offices of the Association, nominated Dr. Luther V. Bell, as Vice President, in place of Dr. A. Brigham, (deceased,) which nomination was confirmed, and Dr. Bell duly elected Vice President of the Association.

An invitation from Drs. Cutter and Howe to visit their institution at Pepperill, Mass., was read, accepted, and referred to the business committee.

Dr. Stedman tendered to the members of the Association, in behalf of the Boston Society for Medical Improvement, an invitation to visit their cabinet, also, one to visit the museum of the Medical College of Harvard University, which were accepted.

Dr. Jarvis tendered invitations to the members, in behalf of the Boston Museum of Natural History, the Boston Atheneum, and the Perkins Institution for the Blind, to visit those institutions, which were accepted.

Dr. Rockwell read a paper on the diet and dietetic regulations for the insane; which, after discussion by the members generally, was laid upon the table.

A letter was received and read from the librarian of the Massachusetts Historical Society, inviting the members of the Association to visit the Society's rooms during their stay in Boston, which was accepted.

Drs. Beck and Wing took seats with the Association as members of the Board of Managers of the New York State Lunatic Asylum.

Dr. Galt read a paper on the organization of Hospitals for the Insane, and Dr. Higgins on the subject of Resident Superintendents of Hospitals for the Insane. Then adjourned to 4 P. M.

AFTERNOON SESSION.

The Association met agreeably to adjournment.

The papers read by Drs. Galt and Higgins were called up for consideration, and the whole subject was fully discussed by the members generally, after which the reports were laid upon the table.

Dr. Bates read a report from the standing committee on the Medical Treatment of Insanity, which, after discussion, was laid upon the table.

An invitation from the librarian of the Boston Atheneum, for the members to visit the rooms during their stay in the city was read and accepted. On motion of Dr. Bates, adjourned to 9 A. M., to-morrow.

SECOND DAY.—MORNING SESSION.

The Association met agreeably to adjournment. The minutes of yesterday's proceedings were read and adopted.

Dr. John R. Allen, of the Kentucky Lunatic Asylum, Dr. John Waddell, of the Provincial Lunatic Asylum at St. Johns, New Brunswick, and

Dr. James Douglass, of the Quebec (Canada) Lunatic Asylum, appeared and took their seats as members of the Association.

Charles Edward Cook, and Otis Clapp, Esqrs., also took seats with the Association as members of the Board of Trustees of the Boston Lunatic Hospital. Dr. Kirkbride, from the committee on business, made a partial report, as required by the resolution of yesterday.

Dr. Ray read a report from the standing committee on the Medical Jurisprudence of Insanity, containing a project for a law regulating the legal relations of the insane, and which had been examined by, and received the sanction of, high judicial and legal authority; after the reading of the paper, on motion of Dr. Kirkbride, it was

Resolved, That the committee on business be instructed to have provided forthwith for the use of the members, one hundred copies of the foregoing project of a law, and that the same be made the order of the day for the first session of the Association to-morrow morning.

Dr. Bell from the committee on business made a full report on the objects to be attended to by the Association during the day.

Dr. Bell read a paper on the use of narcotics in the treatment of insanity; after a full discussion of the subject by nearly all the members, the paper was laid upon the table.

Dr. Fonerden read a paper on the Modification of the Brain by habits, which, after discussion, was laid upon the table.

On motion of Dr. Kirkbride, adjourned to meet at the Boston Lunatic Hospital, at 4½ o'clock P. M.

AFTERNOON SESSION.

The Association, after assembling, proceeded, under the guidance of Dr. Stedman and the Board of Trustees, to visit the Lunatic Hospital and other public institutions at South Boston.

After coming to order for business, Dr. Ranney read a paper on Insanity, as it occurs among the pauper emigrants at the Lunatic Asylum on Blackwell's Island, near New York. After discussion, the paper was laid on the table.

A letter was read from Dr. Fremont, informing the Association that a paper, prepared by him, in reference to the past and present condition of the Insane in Canada East, would be presented to, and read before the Association by his colleague, Dr. Douglass.

On motion of Dr. Galt, adjourned to meet at the Tremont House at 9 o'clock, to-morrow morning.

THIRD DAY.—MORNING SESSION.

The Association met agreeably to adjournment.

The minutes of yesterday's proceedings were read and adopted.

Dr. Kirkbride, on behalf of the business committee, *moved*, that the consideration of Dr. Ray's project of a law for regulating the legal relations of the Insane, which was made the order of the day for this morning, be deferred for the present, owing to the late period at which the printed copies have been placed in the hands of the members, which motion was agreed to.

On motion of Dr. Allen, it was

Resolved, That the Hon. Mayor of the city of Boston, be requested to furnish us, for publication, a report of his eloquent address, delivered at

South Boston on last evening; and also, that the President of this Association be requested to furnish, for the same purpose, his appropriate address in reply.

Resolved, That the Secretary furnish each of the above named gentlemen with a copy of the preceding resolution.

An invitation to visit the University of Cambridge, and the Observatory, was received, and accepted for 11 o'clock to-morrow.

An invitation from the Mayor and public authorities of the city of Boston, asking the members of the Association to visit the harbor and Bay, and to inspect the public institutions in the vicinity, to-morrow afternoon, was received and accepted.

The Association, on motion of Dr. Bell, resolved to visit the Massachusetts General Hospital, on the invitation of Dr. Hayward, at 3½ o'clock, and the M'Lean Asylum for the Insane, on his own invitation, at 4½ o'clock this afternoon.

Dr. Galt read a paper on the Medico-legal relations of the Insane, the discussion on which, on motion of Dr. Bates, was deferred till the project of a law, prepared by Dr. Ray, shall come up for consideration.

Dr. Worthington read a paper on the use of baths in the treatment of Insanity, which, after discussion, was laid upon the table.

Dr. Kirkbride, from the standing committee on the construction of Hospitals for the Insane, read a report on that subject, which, after discussion, was laid upon the table.

On motion of Dr. Ray, it was

Resolved, That the standing committee on the construction of Hospitals for the Insane, be requested, previous to the next meeting of the Association, to prepare a series of resolutions or propositions, affirming the well-ascertained opinions of this body, in reference to the fundamental principles which should regulate the erection and internal arrangements of American Hospitals for the Insane.

Dr. Jarvis commenced reading a paper on the Comparative Frequency, Curability and Mortality of Insanity in the two sexes; after proceeding for some time, on motion of Dr. Bell, the further reading of the paper was deferred till the next session.

On motion of Dr. Allen, adjourned to meet at the M'Lean Asylum, at 4½ o'clock P. M.

AFTERNOON SESSION.

Having previously visited the Massachusetts General Hospital, the Association met agreeably to adjournment, at the M'Lean Asylum, under the care of Dr. Bell, and guided by whom, they visited and examined the different parts of that excellent institution.

Having come to order for business, Dr. Jarvis concluded the reading of his paper, commenced this morning, which, after discussion, was laid upon the table.

Dr. Bell, after referring to a paper read by him, before the Association last year, relative to a somewhat peculiar form of mental disease, moved that a committee, consisting of Drs. Ayl, Kirkbride, and Douglass, be appointed to visit a case of the disease then under his care in the Asylum, and to report the result of their observations, which was agreed to.

The committee having examined the patient, reported, that it was a

well marked case of the form of the disease alluded to, and although not often seen in institutions in the interior, is frequently met with in those near large cities, where cases manifesting much mental disturbance are commonly sent at once to a Hospital for the Insane.

On motion of Dr. Ray, adjourned to meet at the Tremont House, at 8 o'clock to-morrow morning.

FOURTH DAY.—MORNING SESSION.

The Association met agreeably to adjournment.

The minutes of yesterday's proceedings were read and adopted.

Dr. Bell, from the committee on business, made the usual report as to the proceedings of the day.

Dr. Douglass read a paper prepared by his colleague, Dr. Fremont, on the past and present condition of the Insane in Canada East. After discussion, the paper was laid upon the table.

Dr. Galt read a paper on Water Closets, which, after discussion, was laid upon the table. The Association then proceeded to the consideration of the project of a law regulating the legal relations of the Insane, and after a full discussion the further consideration of the subject was postponed until the next session.

On motion of Dr. Bates, adjourned to meet at 9 P. M.

EVENING SESSION.

After visiting the University of Cambridge, and the Observatory, the Association passed the afternoon as the guests of the Corporate authorities of the city of Boston, in an excursion down the Harbor and Bay, in examining the public institutions in that vicinity, and in partaking of the sumptuous hospitality provided on the occasion, and then met for the transaction of business, agreeably to adjournment.

Dr. Bell offered the following resolutions, which were unanimously adopted, viz :

Resolved, That this Association has felt, beyond the power of adequate expression, the profound solemnity, which has been thrown around us, on occasion of its present meeting, by the loss of two of its members so prominent in the history of its organization, as well as in the records of the provision for the Insane in this country, and with still more sensibility, in view of the exalted personal worth, the amiable, cheerful and communicative manners, and pure, self-sacrificing lives of the deceased

Resolved, That the deep and general regret which filled the mind of the whole philanthropic community, of an entire section of country and circles where they were best known, uttered in a thousand forms of expression, leaves us in no doubt that their virtues, merits and devotion to great public duties have been appreciated, in a degree commensurate with their just claims, and leaving neither place nor necessity for any long drawn eulogium.

Resolved, That notwithstanding the full justice which has been done to the public and private character of our distinguished friends, we still feel that the members of this Association, more intimately and fully acquainted with their peculiar traits of service and sacrifice in our specialty, ought not to be satisfied without a more particular testimonial of our feelings and opinions, as to our deceased brothers; we therefore earnestly and respectfully request that Dr. Chandler would prepare for the next meeting of the

Association, a biographical sketch of the late Dr. Woodward, and that Dr. Nichols perform the same duty as regards the late Dr. Brigham.

On motion of Dr. Kirkbride, it was

Resolved, That Dr. Allen be requested to prepare an obituary notice of our late fellow member, Dr. McNairy, of the Tennessee Hospital for the Insane.

On motion of Dr. Bell, it was

Resolved, That the same course be adopted in reference to papers to be read before the Association at its next meeting, as was agreed upon last year.

On motion of Dr. Allen, it was

Resolved, That this Association regard with deep interest, the progress of the magnificent project, which has been and continues to be urged by Miss D. L. Dix, on the consideration of Congress, proposing the grant of a portion of the public domain, by the Federal Government, the proceeds of which are to be devoted to the endowment of the public charities throughout the country, and that it meets with our unqualified sanction.

The subject of a project for a law regulating the legal relations of the Insane, being again under consideration, on motion of Dr. Bell, it was

Resolved, That the same be re-committed, and that the committee report to the next annual meeting.

On motion of Dr. Allen, it was

Resolved, That a committee be appointed to prepare resolutions of thanks to the various public bodies and institutions, official and private citizens, to whom the members of the Association have been indebted for so much of the pleasure of their very gratifying visit to Boston. Drs. Allen, Kirkbride, and Benedict, were appointed the committee.

Dr. Kirkbride tendered to the Association an invitation to hold its next meeting in the city of Philadelphia, when, on motion of Dr. Bell, it was

Resolved, That when the Association adjourns, it will adjourn to meet in the city of Philadelphia, on the third Monday of May, 1851, at 10 o'clock, A. M.

On motion of Dr. Bates, adjourned to meet at 8 o'clock to-morrow morning.

FIFTH DAY.—MORNING SESSION.

The Association met agreeably to adjournment.

The minutes of yesterday's proceedings were read and adopted.

Dr. Kirkbride offered the following resolution, which was unanimously adopted, viz.:

Resolved, That the members of this Association have visited and examined, with great interest and satisfaction, the McLean Asylum for the Insane, under the care of Dr. Bell, and the Boston Lunatic Hospital, under the care of Dr. Stedman, and desire to express to these gentlemen our sincere thanks for their marked courtesy and attention, for their bountiful hospitality, and for their steady and unwearied efforts to promote our comfort and pleasure during our very gratifying visit to the city of Boston.

Dr. Allen, from the committee appointed last evening, reported the following series of resolutions, which were unanimously adopted, viz.:

Resolved, That the grateful acknowledgments of this Association be tendered to the Mayor, Common Council, and the citizens of Boston, for the

flattering reception we have met at their hands, and their lavish hospitalities which have been tendered to, and enjoyed by us, and for the pleasure afforded us in a general examination of the public institutions under their control.

Resolved, That our thanks are due to the Trustees of the public institutions of South Boston, for polite attention and liberal hospitalities during our visit to their institutions, and to the Trustees of the Massachusetts General Hospital, for similar kindness and attention.

Resolved, That our thanks are also due to Drs. Hayward and Townsend, Surgeons, and Mr. R. Girdler, Superintendent of the Massachusetts General Hospital; to Messrs. Harris and Sibley, Librarians, and other officers of Harvard University, and to the Messrs. Bond, of the Observatory, for attentions while visiting the institutions under their charge; and to the officers of the Boston Society for Medical Improvement, Boston Museum of Natural History, Massachusetts Historical Society, Boston Atheneum, and Perkins' Institution for the Blind, for invitations to visit their several institutions, and to the Rev. Lewis Dwight for valuable documents and other attentions.

Resolved, That our acknowledgments are due to Messrs. Tucker and Parker, the proprietors of the Tremont House, for the ample and elegant accommodations they have afforded us without charge, for the transaction of the business of the Association.

Resolved, That the Secretary be directed to furnish his Honor, the Mayor of Boston, with a copy of the preceding resolutions.

On motion of Dr. Allen, it was

Resolved, That the thanks of this Association be tendered to the President, for his able and impartial administration of his arduous duties, and to the Secretary, for his efficient discharge of the laborious functions of his office.

The Treasurer reported, that, after paying all the demands against the Association, there remained a balance of twenty-three cents in his hands.

On motion of Dr. Stedman, it was

Resolved, That the Secretary be instructed to furnish a copy of the proceedings of the Association, to the Editor of the American Journal of Insanity, and to the editors of the various medical journals in the United States and Canada, for publication in their respective periodicals.

On motion of Dr. Smith it was

Resolved, That a committee of three be appointed by the chair, whose duty it shall be to take into consideration the whole subject of publishing, and to report their views to the Association at its next meeting. Drs. Smith, Allen, and Kirkbride were appointed the committee.

On motion of Mr. Benedict, adjourned to meet in the city of Philadelphia, on the third Monday of May, 1851, at 10 o'clock, A. M.

THOMAS S. KIRKBRIDE, Sec'y.

EDITORIAL DEPARTMENT.

The Clergy and the Medical Profession.—Much has been said, of late, respecting the mutual relations of the clerical and medical professions. Custom and medical ethics prescribe the gratuitous rendering of professional services to the families of those exercising the ministerial office. But in return for this substantial testimony of respect on the part of practitioners of medicine, clergymen are often found among the most active, as well as influential, of the enemies and detractors of medical science. Hence, it is sometimes urged that they should be placed on a par with other patients as respects remuneration for professional attendance. Resolutions to this effect, indeed, have, we believe, been adopted by some local societies. We cannot concur in the propriety of this course as a retaliatory measure. "Two wrongs do not make a right." If the clergy have any valid claims on us for gratuitous services, it does not weaken the force of those claims that the value of our services is not appreciated, or receives an ungrateful return. If physicians were in the habit of declining to serve all except those who placed a proper estimate on the resources of medical art, their practice would be far more exclusive than it now is. The rule of refusing remuneration from clergymen may be a bad one, but, if so, it is for other reasons than that some of the clerical body patronize quacks, give certificates of the value of secret remedies, and officiously engage in electioneering in behalf of homœopathy, Thomsonism, etc. We concede their right to do this if they can reconcile it to their own consciences. We incline to the opinion that it would be better for both professions if the clergy were expected to pay for medical services. Both parties would thereby be more independent, and we are not sure but it would conduce to a higher estimate of medicine, inasmuch as what costs nothing is apt to be deemed worthy of little esteem. We have introduced this topic for the purpose of quoting some remarks thereupon by a clerical writer. We take them from the North-Western Medical and Surgical Journal, and they are credited by that periodical to a religious paper called the Puritan Recorder. We copy the remarks in order to accord a hearing of the clergy

in their own defence, and also because, with our esteemed contemporary, we think they contain *sense* as well as *humor*.

"Of late there seem to be symptoms of unhealthy feeling on the part of the medical gentlemen. It is even rumored, that, in some of their Conventions and Societies, they are agitating the project of a breach of the peace with the ministers of the Gospel, because a few of these have soiled their cloth by patronizing and puffing some of the patent nostrums of the day. We have no apology to make for such unministerial conduct, which is highly indiscreet and reprehensible. But this we will say, that, for one respectable and educated clergyman who has stultified himself by being thus mixed up with quackery, at least an hundred have abhorred the contact of 'the unclean thing.'

"Why should the whole body of ministers in regular standing, be made responsible for the errors and follies of a few of their brethren, who, in such matters, are under no ecclesiastical control? Have any of the ecclesiastical bodies, large or small, taken action in the premises? Has the General Association of Massachusetts recommended to the churches the use of Brandreth's Pills? Has the General Assembly of the Presbyterian Church (Old School.) espoused the cause of Sands' Sarsaparilla; or has the New School Assembly taken up the claims of the rival syrup of Townsend? Have the Episcopal bishops given vogue to the big-bellied bottles of Mrs. Kidder's Cordial? Has the General Conference of the Methodist brethren indorsed for the virtues of the Thompsonian hot-drops? Is the American Unitarian Association responsible for Homœopathic doses in medicine as well as in religion? Or has the Baptist communion pledged itself to Hydropathy for any but spiritual purposes? If no such ecclesiastical absurdity has been perpetrated, why is an entire body of unoffending men charged with the extravagances of a few visionary or eccentric brethren, who have ignorantly misapplied their official influence to the support of imposture?

"Here we are bound to warn the gentlemen of the medical profession, that, if they declare war with the ministers on such grounds, they may yet be obliged to drink out of their own measure. We would inquire whether none of them have been known to patronize quack preachers, empirical reformers, and other religious Jack-puddings, who are vending their noxious nostrums in theology and morals, poisoning and stupefying the minds of the people, and spreading the ravages of spiritual death? Is it not a fact, that very many regular-bred physicians haunt the shops of the most notorious theological impostors, and go abroad to bring their pernicious wares into general use? And now what propriety would there be in preaching up a crusade on the part of the clergy against the physicians, as a class essentially radical and faithless?"

In connection with the foregoing, we copy the following article from the Boston Medical and Surgical Journal. It contains an eloquent, and, as we hope, a just tribute to the character of the medical profession, by a member of the clerical body:—

"*The Beloved Physician.*"—A discourse delivered in Norfolk, Conn., by the Rev. Joseph Eldridge, at the funeral of the late Benjamin Welch, M.

D., of the same place, has been received. It seldom falls to our lot to read eulogies pronounced by the clergy upon one of the medical profession, and particularly one that possesses so much character and truthfulness as the one now before us. A just appreciation of the medical profession by the clergy is not common; and when we see that the physician's services are sometimes requited, and that there is one of the clerical profession who has honor and intelligence enough to award him merit, and disseminate just sentiments among the people, we must confess that it is a powerful stimulus to increased efforts in our profession. Some portions of the discourse are so truthful and applicable, that we quote freely from it, hoping that others, besides medical men, will read it, and give it their consideration. In speaking of the duties of physicians, he says, "The services necessarily impose upon him severe exertions, bodily and mental, such as subject him to many privations, and much hardship. He must unavoidably be irregular with respect to food and sleep; he has no command of his time, is subject to every body's call; when summoned, he must go, whether fresh or weary, whether it be night or day, whatever be the state of the roads or the weather. He cannot be excused from rising from his bed to-night, after having just comfortably deposited himself in it, because he entered no bed last night, nor, it may be, the night previous. With the most painful and distressing scenes he must be daily conversant; he must pass large portions of his time in sick rooms, discharging disagreeable offices; must be familiar with wounds and diseases, with the sufferings of the sick, and the mortal agony of the dying. *His services are not requited, and many of them are such that they cannot be.* He is expected to be no respecter of persons; he is every man's servant; he is commanded as readily by those who have no means of compensating him, as by those of the amplest resources. *After much reflection upon the subject, I am settled in the conviction that more gratuitous labor is performed by physicians than by any other class in the community.* It has come to be a sort of common law that they must do it. If they should decline visiting a sick family, on account of its poverty or inability to pay for the service, many would hold up their hands in astonishment and horror, who themselves would not render the slightest assistance in the very same case. Even those who have the ability and intention to pay their other debts, are often content to suffer the honest demands of their medical attendants to run along indefinitely. *The per centage of unrequited labor performed by this profession is very large.* Why should it be so? Their time, their strength, and their skill, are their own. Nevertheless, much of their time, strength, and skill, are given away. Then there is much in their services which cannot be requited, for which money is no adequate remuneration. For the weary miles they travel, for the time spent with the sick, for the sacrifice of sleep and physical comfort, they may perhaps be paid—pecuniary compensation may possibly cancel claims for such services. But what shall we say of their frequent solitudes for the sick in critical cases; their overwhelming anxiety when precious and valuable lives hang on their decisions? *when, in the struggle with disease, they find their efforts baffled; what shall we say of this wear and tear of sensibility and feeling? Will a few dollars and cents cancel such debts? They are not cancelled—they never can be.*"

Homœopathic Statistics—Very erroneous impressions obtain, to some extent, respecting the diffusion of the homœopathic delusion in other countries. It is thought by some that this mode of practice has secured a permanent foothold in Europe. This is asserted as an inducement for the credulous to become converts to the doctrine, the tendency to follow in the wake of European currents of opinion on some subjects being an obvious weakness of a portion of our countrymen. The following facts, communicated for the London Medical Times, exhibit the numerical ratio of homœopathic practitioners to the population, and the regular profession in Great Britain:—

1st. *Of London.*

The population of London amounts to about..... 2,200,000
 The number of medical practitioners, practising in London, whose names appear in the "London Medical Directory," is..... 2,571
 The number of homœopathic practitioners, practising in London, according to the accredited "list," in the *British Journal of Homœopathy* for January, 1850, is..... 48
 Of these 48 homœopathic practitioners, 22 are not in the *London Medical Directory* at all; and of the 26 which remain, 10 are graduates in medicine, and 16 are surgeons or surgeon-apothecaries.

Of the ten graduates, 6 appear to have the Edinburgh degree, 1 Aberdeen and Paris, 1 Aberdeen and Turbingen, 1 Aberdeen, 1 Erlangen.

2d. *Of the Provinces.*

According to the *Provincial Medical Directory*, there are of medical practitioners, practising in the provinces..... 8,327
 According to the "homœopathic list," already referred to, there are, of homœopathic practitioners, practising in the provinces, 52
 Of these 52 homœopathic practitioners, 16 are not in the *Provincial Medical Directory* at all; 4 are in it, but their qualifications are not vouched for by the Editor of the Directory; and of the remaining 32 whose names appear in the Directory, 18 are graduates in medicine, and 14 are surgeons or surgeon-apothecaries.

Of the 18 graduates, 13 possess the Edinburgh degree, 3 St. Andrews, 1 Glasgow, 1 is an Est. Lic. Lond. Coll. Ph.

There appear, therefore, to be in Eng'land, about 10,898 medical practitioners; but suppose that we make a liberal deduction from this number of 898, as practitioners of doubtful license, and make the number of legalized practitioners 10,000, instead of 10,898; then out of this number appears the insignificant proportion of 28 graduates in medicine, and 45 general practitioners, who call themselves homœopaths, and who profess to practice as such.

By the foregoing statistics it is apparent that sugar pellets are not in very general repute in Great Britain. They are just now much more in vogue in this country. But the truth, probably, is, that having traveled to England before visiting the United States, the system has had its day, and is now in the sear and yellow leaf of whatever popularity it may have

had heretofore in that kingdom. That it will have its decline and fall in this country in a few years, it requires but little shrewdness to foresee. But it may be doubted if this result will denote any abatement of the spirit of quackery. The same credulity and love of the marvelous which have fostered homœopathy, and other impositions, will remain. A portion of every community will still insist on being duped; and, doubtless, the fertility of invention will be adequate to supply a worthy successor to the fictions of Hahnemann.

We have no disposition to scold or fret about homœopathy. Like other dispensations of Providence, it has its specific objects to accomplish, and some of these may possibly, in a measure, compensate for the sacrifice of life and health incident to its career. It furnishes some illustrations of the progress and results of diseases uninfluenced by medication. These would be truly valuable if confidence could be placed in the competency and integrity of homœopathic practitioners. It may perhaps exert some effect in preparing the minds of people not to consider medical advice and the administration of active remedies as indissolubly connected as cause and effect. To remove this popular error, has for many years past been the aim and tendency of medical practice. The latter is sometimes attributed mainly to homœopathy. It would be nearer the truth to reverse the proposition. Another reflection which should reconcile us somewhat to the absurdities of homœopathy is, that if this delusion did not exist, the credulity of the public might be exposed to some other form of deception more disastrous in its consequences. Finally, there is another consideration which applies to this, as to all the phases of medical quackery. This is, that although the world may not grow wiser by experience, individuals do. On the principle embodied in the homely apothegm, "a child once burned, ever after dreads the fire," they who once discover the folly or danger of a popular medical delusion, are not apt to be duped a second time. These persons thereafter will be found to be the firmest friends of legitimate medicine. This consideration may prove a succedaneum for practice temporarily lost, and should also dictate the line of policy to pursue with those who choose to make trial of a fashionable humbug.

We trust that we have not laid ourselves open to the charge of being an apologist for homœopathy by these remarks. Regarding it as one of the entities of the day for which we are in no wise responsible, we have indulged a few of the thoughts suggesting themselves at the moment of writing, which pertain to the cultivation of a philosophical endurance of those of the evils and absurdities of human life which it were bootless to attempt to remedy.

*Transactions of the Medical Association of Southern Central New York,
at the Annual Meeting, held at Elmira, June, 1860.*

A copy of the above publication was received some time since, and would have been noticed ere this, but for the pressure of other matters. We have had occasion before to advert to the success attending the organization of the Profession in the Southern central counties of this State. The Transactions for the present year betoken increased zeal on the part of those who have originated the enterprise. The plan pursued by this association is an excellent one, viz, to appoint a committee for each county to report on the endemical and epidemical diseases of the county during the year. If local associations existed in all parts of the country, and this plan were to be generally adopted, the materials for a comprehensive history of the diseases of successive years would be supplied—and in some such way only can this desirable end be attained. The American Medical Association appoints annually a committee to report on the Epidemics, etc., of the country, but how are the data for such a report to be obtained? We can see no mode so effectual, as the plan just referred to; and this furnishes not the least important of the numerous inducements to form and support local societies for scientific objects.

The Transactions of the Medical Association of Southern Central New York for the present year, make a pamphlet of eighty octavo pages. They consist, *first*, of an address by the President, Pelatiah B. Brooks, M. D., on the diseases of the kidneys and urinary organs; *second*, Report on the endemics and epidemics of Cortland county, by Frederick Hyde, M. D., chairman, embracing communications from Dr. H. P. Eels, Drs. Smith and Woodward, Dr. G. W. Bradford, and Dr. Caleb Green, all of whom were associate members of the committee of which Dr. Hyde was chairman; *third*, Report on the endemics and epidemics of Chemung county, by Wm. Woodward, M. D., chairman; *fourth*, Report on the endemics and epidemics of Tioga county, by Dr. H. N. Eastman, chairman. To the foregoing papers are appended a brief sketch of the life of the late Dr. Rowland Wilcox, by T. H. Squire, M. D.; and the proceedings of the annual meeting of the Association.

As a whole, the productions just enumerated are highly creditable to their authors, and to the Association. Their circulation will, we hope, incite members of the Profession in other sections of the country to unite for similar objects.

Report of the Majority of the Committee on Medical Societies and Colleges on so much of the Governor's Message as relates to the Cholera. March 25, 1850.

It will be recollected that the Governor of this State, in his annual message, recommended the Legislature to take some action to secure "the benefit of the combined experience of scientific and learned men throughout the State with respect to the origin, the cause, the progress, and the treatment of all malignant and infectious diseases."

The subject was referred by the Senate to the Committee on medical societies and colleges, of which Dr. C. D. Robinson was chairman. The majority of the Committee made a report embracing communications treating of the several points embraced in the passage of the Governor's message quoted above, by Drs. W. P. Buel, A. Vache, and Professors Reese and Clark—all of the city of New York. These papers are valuable contributions to the literature of Cholera, and the Committee, by eliciting them, have conferred a favor on the Profession and Public. Several resolutions were reported by the Committee, providing for hygienic regulations with a view to preventing the development and spread of this terrible epidemic. It is perhaps due to the adoption of effective measures to remove exciting or predisposing causes, that the cities and towns of this State have suffered so little from the disease during the past summer.

Report of the Committee of Internal Health on the Asiatic Cholera, together with a Report of the City Physician on the Cholera Hospital. Boston. 1849.

This Report is worthy of perusal and preservation. It contains the histories of thirty-three fatal cases of cholera, with the post-mortem appearances. The latter are given with admirable precision, and great minuteness. Appended to the Report are descriptions of the localities in which the disease prevailed, accompanied by daguerreotype views, which convey to the mind of the reader a vivid apprehension of the fact that the special cause of cholera derives its efficiency mainly from filth, want of ventilation, over-crowding, intemperance, and other circumstances appertaining to insalubrity, and neglect of the laws of health.

A map of the city also accompanies the Report, showing the locations in which all the cases at the hospital, and all the fatal cases elsewhere, originated. The course of the epidemic as thus delineated, in Boston, as in

other cities, furnishes an almost insuperable difficulty in the way of attributing its diffusion to contagion.

Another fact stated in the Report militates strongly against the doctrine of the personal communicability of the disease. The hospital was situated near the scenes of the greatest ravages of the epidemic. But while several cases originated in its vicinity before it was occupied by patients from other parts of the city, no cases occurred in the houses upon the same square, and the cases which did subsequently occur in the neighborhood were limited to the houses in one direction which were occupied by the most miserable of the population of the city, living in a most miserable manner. Persons residing under better circumstances in the opposite direction from the hospital building, were entirely exempt from the visitation of the malady. These circumstances show that the collection of cholera patients in a hospital did not, in this instance, constitute a focus of contagion; nor can we call to mind any instances that have been reported in which the disease has appeared to spread from cholera hospitals. The conclusions with regard to the various modes of treatment adopted are negative in their character. Patients were treated with narcotics, stimulants, emetics, calomel, quinia, tannic acid, ether, cathartics, venous injection, external heat, the wet sheet, bleeding, etc.; but the comparison of cases in which different remedies were employed does not furnish results upon which a recommendation of any particular plan of treatment can be based. In the great majority of instances patients were admitted in a state of complete collapse, and the large ratio of fatality under different methods of treatment, go to confirm, what we fear must be admitted as a truth, that under such circumstances active remedies are seldom successful, and perhaps not infrequently injurious. In its incipient stage cholera is perfectly manageable, and requires only simple remedies, with proper hygienic regulations; but after this favorable period has passed, the question of life and death turns on the degree of the blood lesions that have occurred, and the amount of the recuperative energies of the organism. Perturbating treatment under the latter circumstances is, to say the least, questionable.

The reporter states that an invitation was extended to several homœopathic practitioners to test the vaunted efficacy of infinitesimal doses in the treatment of the disease. None, however, were willing to come into the hospital upon equal terms with the city Physician, and take charge of an equal number of patients.

The remarks on the pathology of the disease, based on the autopsical histories, are sensible, but the interest of the subject, at this time, is per-

haps not sufficient to devote to it space enough to do justice to the views presented in this portion of the report. There is one point, however, to which we will briefly advert, inasmuch as the ideas generally entertained respecting it, exert, with many practitioners, a controlling influence in the election of remedies. We refer to the biliary secretion. It has been generally supposed that an attack of cholera is uniformly attended by suppression of bile. On reference to the autopsies contained in this Report, it will be found that bile was frequently found after death in the duodenum and stomach, almost always being present in the gall-bladder, and generally expressed without difficulty from the small ducts in the interior of the liver. The theory which refers the disease to congestion of the portal circle, dependent on suppression of the bile, appears to us wholly inadequate to account for the phenomena, assuming the facts to be precisely as they are taken for granted. But it would seem from the observations contained in this Report, that the liver is even less affected than other glands—certainly less than the renal organs. The supposed efficacy of calomel in this disease is rationally explained by its supposed action on the liver. In view of the facts just referred to, some other explanation must be sought for. That the bilious stools, as they are termed, occur in cases in which mercurials have not been given, our own observations have afforded abundant evidence.

In concluding this notice of this able and truly valuable Report, we should not omit to state that it was submitted to the Mayor and aldermen of Boston, by Dr. Henry G. Clark, city Physician, having been prepared by him in conjunction with Dr. Charles E. Buckingham, John C. Dalton, and Henry W. Williams.

Medical College Circulars.—We quote the following paragraph from our esteemed contemporary, the *Western Lancet*, simply stating at this time that it expresses views which have suggested themselves to us in reading not a few of the many Circulars with which we are favored. If we are not mistaken, the honor of the profession requires a reform in the tone of these annual missives. One way to bring this about is for Editors of medical journals to review those circulars which are obnoxious to criticism.

Medical College Circulars.—We wish to call the attention of our friends, the Professors of Medical Colleges, and also the Editors of Medical Journals, to the style of communication which has for the last few years crept

into the circulars of our medical schools. It is not necessary to allude to the origin of this practice, or to designate one college more than another, for all are more or less implicated in it. It appears to us that the style of these documents might be much improved, and made to conform more to professional propriety than they do at present, with advantage. We see no reason why a body of men, collectively, ought to say that about themselves, that would be improper for any one to say about himself as an individual. Professional etiquette and propriety prohibit puffing and laudatory notices of one's self. Ought it not to be equally binding on an association of medical men. We have thrown out these few thoughts without intending to cast censure, but for reflection; if we are wrong, we do not object to being set right.

(*Miss Blackwell, M. D.*)—The movements of this estimable lady, and intrepid pioneer in the cause of female medical education, will continue to be a subject of interest with the Medical Profession. (A private letter has been transmitted to us by a mutual friend, which we are not at liberty to insert in full, by which we learn that she ~~has~~ continued to prosecute her studies in Paris, up to July last. The disease of one of her eyes, contracted from a patient under her observation, has proved a serious calamity, the sight being nearly destroyed.

In July she was at *Grafenburg*, at the hydropathic establishment of *Priessnitz*, partly to try the effects of his system upon herself, and, partly, to study the effects of his system upon the numerous patients congregating there, with a view to ascertain what success is really attained, and to determine how much is to be attributed to the therapeutic action of water, and how much to the general hygienic conditions under which the patients are placed.

She states that she has received a courteous invitation to pass several months in London, every facility for attending the hospitals and schools having been promised; and that it is her intention to avail herself of this opportunity to institute a comparison between French and British practice.

Medical College of Ohio—H. Willis Baxley, M. D., of Baltimore, Md., has been appointed to the chair of anatomy made vacant by the decease of Prof. Shortwell. Dr. Baxley has been for several years past connected with the Washington University of Baltimore. He sustains a high reputation as an anatomist and lecturer.

Cholera in Buffalo.—Our city has, Providentially, escaped a visitation of Epidemic Cholera during the past summer. Single cases have occurred from time to time, but the disease has not prevailed to an extent sufficient to entitle it to be called an epidemic. We hope to give hereafter some account of the few cases that have been observed.

Dysentery has been the disease most commonly met with in practice during the summer, as it is at the present time. In general this disease has not assumed a severe form, but in some instances it has been characterized by hemorrhagic discharges which have proved rapidly fatal. Dysentery has prevailed, as we are informed, more in the vicinity of Buffalo than in the city.

Cholera in Cincinnati.—The Western Lancet states that the cholera has entirely disappeared from Cincinnati. Dysentery and diarrhœa prevail, but not to a greater extent than frequently is the case at this season. The deaths from cholera during the months of June, July, and August, amount to *fourteen hundred*, as certified by the Board of Health.

Cholera in Chicago.—We clip from the *Daily Journal* the following statement of the total mortality, presuming that it is as nearly as possible correct:—

“The total number of deaths in July, 240; total in August, 466.

Deaths from Cholera from the 23d of June, the time when the epidemic made its appearance, to the 1st of August, 158; from the 1st of August, to 1st of September, 283, making a total of deaths by Cholera during the season, of 441.

Thus the whole mortality of the city, from the 23d of June to the 1st of September, a period of 69 days, is not far from 624, making an average of about nine per day, which, taking the population at 27,000, gives one death daily to every 3,000.”

Buffalo Medical College.—The preliminary term of instruction in this Institution commences on the 2d inst., and continues to the first Wednesday in November, the date of the commencement of the regular term. This gives a preliminary term of *five weeks'* duration. The regular term continues *sixteen weeks*.

Louisville Medical College.—Daniel Drake, M. D., has been elected to the chair of medicine in this institution, made vacant by the resignation of Prof. Bratlett. The chair of surgery is not yet filled.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* By AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

(Continued from page 282.)

Sordes. The presence of *Sordes* on the teeth, or lips, or on both, was noted in *fourteen* cases, viz., in *six* cases of *Typhoid*; (*two* cases in private, and *four* in hospital practice;) in *six* cases of *Typhus*, and in *two* cases of *doubtful* type. In *six* of these cases the déposit was confined to the teeth; in *six* it extended to the lips, and in *two* cases its situation is not mentioned. In all the cases in which it was observed on the lips, it also existed on the teeth. In *five* of the cases in which *sordes* was present, the disease proved fatal. In *eleven* of the cases the tongue was either dry, or dry and hard. In *thirteen* of the cases, the tongue was more or less coated; and in the remaining case the tongue could not be inspected, owing to Parotiditis. In no case in which *sordes* was present, could the disease be called mild, and in most of the cases, exclusive of those which proved fatal, it was severe, and attended by considerable prostration. In the other cases the grade of intensity was not far from medium; and in the latter cases the *sordes* was not abundant.

This symptom very rarely occurs in the early part of the febrile career. In *one* of the cases, however, it appeared, and the deposit was abundant, on the third day. At the same time that the sordes appeared on the teeth and lips, in this case, the tongue, which for the two previous days had been natural in appearance, became thickly coated. The rapidity and abundance of the deposit in this instance, show, pretty conclusively, that it involves some change in the character of the secretions of the mouth. This case was of the *Typhus* type, and ended fatally on the fourth day.

In so far as the changes in the salivary fluids giving rise to sordes are concerned, they do not belong intrinsically to the disease, or, at all events, if they uniformly take place, they are very unequal in degree, inasmuch as the deposit is observed only in a limited number of cases. Its presence is to be regarded as denoting gravity, since, in three-sevenths of the cases in which it was observed, the disease proved fatal, and in no instance in which it occurred was the disease mild, but generally it was severe. Nevertheless, it was absent in some fatal cases, and by no means uniformly present in cases which were of a severe grade. There seems to be no foundation for the idea, which has been entertained, that it is specially significant of adynamia. Nor does it indicate, more than a dark color of the coating of the tongue, a putrescent state of the fluids. Co-existing, as it does, with more or less coating of the tongue, it seems fair to infer that it proceeds from a cause common to both; and being also found generally in connection with dryness of the tongue, we may presume that it is, in a measure, owing to the mental condition which, as has been seen, induces, in part, the latter condition.

Hæmorrhage from the Gums. Oozing of blood from the gums occurred in *two* cases, both of the Typhoid type, one in *hospital*, and the other in *private* practice. In *one* case (private practice) the disease was mild, unattended by delirium, accompanied by mild diarrhœa, tongue thinly coated, convalescence dating from the twenty-first day. The hæmorrhagic effusion occurred in the latter part of the disease, blood exuding pretty freely, so as to color constantly the saliva, and to form incrustations on the lips. In the other case the disease proved fatal on the tenth day. The hæmorrhagic effusion occurred on the sixth day, forming bloody incrustations on the lips. He complained of soreness in the throat, and a sense of dryness in the mouth. Diarrhœa did not exist in this case. The tongue was coated, at times somewhat dry, and tremulous. In neither of these cases did hæmorrhage occur from the bowels, or from any part except the gums.

An *herpetic eruption about the mouth* was observed in *one* case. Lest

the occurrence of this symptom may suggest a suspicion that the disease was, in this case, *remitting* fever, in which herpetic eruptions in that situation are apt to occur, it may be stated that no doubt could exist as to the diagnosis, the characteristic *macula*, together with other distinguishing traits, being present.

Parotiditis. In *five* cases, the parotid gland on one, or both sides, was the seat of inflammation, proceeding to suppuration in all but one case, and in the excepted case death took place before sufficient time had elapsed for the suppurative process to be completed. In *two* of these cases the disease was *Typhoid*, in *one*, *Typhus*, and in *two* the type was *doubtful*. One of the cases occurred in *private*, and the other cases in *hospital* practice. In the *first* case in which this complication occurred, the patient entered the hospital, March 29, 1849. Prior to this case I had never met with this complication in Continued Fever. *Two* other cases occurred at the hospital in November, 1849; and the remaining hospital case was in December, 1849. The case in private practice occurred in May, 1850.

In *two* of these *five* cases the disease proved fatal.

It is due to the importance of this intercurrent affection to devote to each of these cases some special consideration. In the *first* case, the patient entered the hospital five days after taking to his bed. The parotid of the right side began to swell on the second day after his entrance. It became greatly enlarged, hard, painful, with erythema of the surface, and proceeded to suppuration, opening below the ear, and also into the *meatus auditorius*. A large quantity of pus, and sanguinolent fluid, was discharged through these two orifices. The discharge of purulent matter continued during the career of the fever, and for several weeks afterward. A portion of cellular tissue about the size of the end of a finger, sloughed, and came away through the orifice below the ear. Owing to the swelling, and the pain in moving the jaws, the patient could not, for several days, open the mouth sufficiently for the tongue to be inspected. Convalescence was dated on the fourteenth day after his entrance. This was a case of *Typhus*, presenting the characteristic eruption of that type. *Diarrhœa*, tenderness, and meteorism were not present. *Passive delirium*, with getting out of bed, *somnolency*, *some subsultus*, and on one day *carphologia*, were noted in the record of this case.

In the *second* case, it is not stated whether the patient had been confined to the bed before entering the hospital. On the *seventh* day after his entrance, swelling of the left parotid commenced. The swelling was considerable, accompanied with redness, soreness, and pain. In this, as in all

the other cases, before suppuration was accomplished, the swelling was remarkably hard, or resisting to the touch. Eight days after the commencement of the swelling, fluctuation being apparent, an incision was made, and a large quantity of pus evacuated. In two or three days afterward, another and distinct collection of pus was opened. The discharge continued, but gradually diminished, and at the time of the decease, the swelling was much reduced, and the quantity of pus small. This case terminated fatally on the twenty-third day after entrance. Diarrhœa, meteorism, and tenderness were absent in this case. Delirium, and other ataxic symptoms, were not prominent. The disease did not seem to be of a severe grade of intensity, and death, apparently, was determined by the affection of the parotid. This case is included among the cases of *doubtful* type. No autopsy was had. In the *third* case, the affection of the parotid commenced four days after convalescence was pronounced. Properly the affection in this instance should be classed among the events of convalescence, or the *sequelæ*. The patient had entered the hospital twelve days before the date of convalescence. He had not taken to his bed before entering the hospital, but had been ill for about twenty days, making strong efforts to keep about, and resist yielding to the disease. The swelling, in this case was large, tender, hard, and painful, and the surface reddened, and it proceeded to suppuration. An incision was made when fluctuation was discovered, but the abscess also ulcerated and discharged through the *meatus auditorius*. Diarrhœa, meteorism, and tenderness were not present in this case. Delirium was also absent. Eryepelas of the face succeeded the parotiditis. The patient was quite well forty days after his entrance. This case, also, is embraced among the cases of doubtful type.

In the *fourth* case, swelling of the right parotid commenced on the *sixth* day after the patient entered the hospital, and *ten* days after the attack. The patient was a female. The swelling was large, somewhat livid, with redness, tenderness, and pain. On the day succeeding that on which the swelling of the *right* parotid commenced, the *left* began to enlarge, and it soon became greatly swelled, reddened, painful, and tender. The inflammation on both sides proceeded to suppuration. On the *twelfth* day, fluctuation being apparent in both sides, an incision was made, giving exit to a copious purulent discharge. A portion of cellular tissue protruded through the orifice on the left side, and sloughed away. There was no discharge into the *meatus*, in this case. There was no diarrhœa in this case, but moderate tenderness, and meteorism. Delirium was slight. Moderate somnolency existed. It was difficult to fix the day of convales-

cence. She sat up on the twenty-fourth day after her entrance, and the last record was made on the twenty-eighth day, when there was still some discharge from the abscesses. This case was of the *Typhoid* type.

In the *fifth* case, the fever had existed five days before the patient received medical attention. In this case the patient was a female. She was suffering from nursing sore mouth when attacked with fever. On the *seventh* day after the attack, the right parotid began to swell, and became greatly enlarged, hard, reddened, and painful. Death occurred on the ninth day, the affected parotid still being greatly enlarged, and resisting to the touch. This case was of the *Typhoid* type, and occurred in private practice. It was characterized by mild diarrhoea, tenderness, meteorism, passive delirium, somnolency, eventuating in coma, the patient dying in the latter state.

Parotiditis is certainly a much rarer complication of Continued Fever than would appear from the proportion of instances in which it was present in this collection of cases. It is not mentioned in the researches of Louis, nor is it alluded to in the treatise by Bartlett. In Christison's work on fever, and in Copland's dictionary, I find it referred to, as an event which occasionally occurs toward convalescence, in some cases, proving critical, and therefore desirable. These views are not sustained by the facts in the cases that have come under my observation. In all, save one, the affection occurred before convalescence was to have been expected, and in the excepted case it occurred after convalescence was declared. In all the cases it was a serious complication, not only adding to the sufferings of the patient, but increasing the severity of the disease, and, in one instance, it appeared to be the determining cause of a fatal issue.

That this affection should have occurred in *five* out of *thirty* cases coming under observation between the dates of the first and last of the cases in which this complication existed, is a remarkable fact, of which I can offer no explanation. It may possibly be suspected that a contagious influence was transmitted from patient to patient; but there is no ground for this hypothesis. The specific form of parotiditis was not prevalent at the hospital during the period these cases transpired, nor, except in one instance, were the patients brought into contact with any other patients laboring under this complication. It occurred in two patients in the same ward, successively, with a few days' interval. In the other two hospital cases, the patients were not in wards in which any case had occurred, and the case in private practice had no connection whatever with the cases at the hospital. The fact can only be considered as exemplifying what Syden-

ham and others have remarked—that fever, at different times and places, may be characterized by peculiar and various local tendencies, and science is no better prepared to explain their occurrence now, than at any past period in medical history.

Nausea and Vomiting. Nausea and vomiting occurred in a very small proportion of the cases, viz., of the cases in *private practice*, in *three*; of those in hospital, *Typhoid*, in *three* cases; *Typhus*, in *one*; *doubtful type*, *two*. The vomiting, in all these cases, was slight, and in all it occurred early in the career of the fever, as follows:—of the private cases, in *one* instance it occurred once before the case came under my observation, and may have been owing to remedies; in *another* instance remedies were rejected for the first few days; in the *third* instance slight vomiting early in the disease. Of the *hospital* cases, *Typhoid*, in *one* instance, it is noted that the patient vomited early in the disease; in *another* instance the patient vomited on the day of attack and not afterward, and in the *third* instance remedies were rejected on the first day. In the single case of *Typhus*, the patient vomited two or three times early in the disease. Of the *two* cases of *doubtful* type, in *one* the patient vomited once on the second day, and in the *other* case vomiting occurred several times during the first few days.

Vomiting occasionally occurred after the administration of antimony, but the instances in which this symptom was obviously the result of that remedy, are not included in the above. It is to be recollected that the present enumeration embraces only those instances in which vomiting occurred after the febrile career was established. This symptom occurs in a larger proportion of cases, if the access be included. It is also to be borne in mind that in a considerable number of the hospital cases the febrile career had continued for a greater or less period before the patients came under observation. In some of these cases vomiting may have occurred, and the fact not have been noted in the *previous history*.

I should state that in the great majority of cases in which the occurrence of vomiting is not noted to have occurred, no mention is made of this symptom in the histories. It is, however, quite improbable that in any of these cases it should have occurred after the patient came under observation, and not been embraced in the record. Certainly this could not have been the case if the symptom had been in any degree prominent.

In one of the cases in which vomiting occurred, the disease was fatal. The stomach, in this case, at the autopsy, was found to be normal in size, the mucous membrane of a dingy brown color, notably thickened, and somewhat softened, without redness or vascular injection. The vomiting, in

this case, occurred only two or three times, and in the early part of the disease. It was not a prominent symptom. In two fatal cases in which the stomach exhibited lesions, ulcerations existing in one case, vomiting is not noted to have occurred.

The foregoing facts are chiefly important in a negative point of view, that is to say, they go to show that nausea and vomiting rarely occur in Continued Fever, and when they are present, they are not prominent as symptoms, and are confined to the early part of the febrile career. Negative results of this kind are sometimes scarcely less valuable than those which are positive. In this instance the infrequency of vomiting is important to be taken into account in the discrimination of Continued Remitting Fever, the symptom occurring much more frequently in the latter disease.

Pains in the stomach, and other gastric symptoms, inclusive of tenderness on pressure over the epigastrium, are not mentioned in any of the histories. It is possible that had attention been directed carefully to these points, something might have been observed worthy of being recorded. Any evidences of gastric disorder, however, which were prominent, would not have been likely to have escaped notice.

Alvine Discharges. Questions relating to the nature of the distinction between the two types of Continued Fever, (Typhus and Typhoid,) and to the practical discrimination of each from the other, involve especially morbid conditions of the intestinal tube and their symptomatical expressions. The facts, therefore, embraced under the heads which remain to be considered under this Section, are to be studied with care in each type separately, and the results compared.

Typhoid. Cases in Private Practice. Of the *thirteen* cases, diarrhoea was present in *twelve*. It was mild in degree, or slight, in *seven* of these cases. It was severe and persistent in but *one* case. It was confined to the early part of the career of the fever in *four* cases. It followed the operation of cathartics in *four* cases.

Cases in Hospital. Of the *eighteen* cases, diarrhoea was present in *nine*. It was mild, or slight in degree in all but *two* cases, and *one* of these cases was fatal. It was limited to the early part of the career of the fever in *three* cases, and in *one* of these cases continued but for a single day. It occurred in the latter part of the disease in *one* case. It continued, more or less, through the career in *five* cases.

Typhus. Diarrhoea followed a cathartic in the single case of Typhus in private practice.

Hospital Cases. Of the *twelve* cases, diarrhoea was present in *four*.

The discharges were thin, but not too frequent in *one* case not included in the above. In all the cases it was very mild, being present but a single day in *one* case. It was limited to the early part of the career of the fever in *three* cases. It occurred in the latter part of the career in *one* case.

Adding the cases in private practice to those in hospital, diarrhœa was present in *twenty-one of thirty-one* cases of *Typhoid*; and in *five of thirteen* cases of *Typhus*. Moreover, in degree it was more uniformly slight in the cases of *Typhus*. This comparison, then, goes to show that diarrhœa occurs more frequently in the *Typhoid* than in the *Typhus* type of fever, and that, although mild in degree in the great majority of cases of both types, it is more uniformly slight in *Typhus*.

I should define the sense in which I have used the term *Diarrhœa*. I have not applied the term to express the character of the dejections, so much as their frequency. The character of the dejections, as will be seen in an appropriate connection, were in a large proportion of cases not ascertained. They may have been thin in some cases in which their frequency was not much, if at all increased, and in these instances diarrhœa would not be noted to exist. The patient was not considered to labor under diarrhœa while the dejections numbered only one, and occasionally two in the twenty-four hours. This explanation will serve, in some measure, to explain the greater frequency of this symptom in the cases analyzed by Louis, a fact which I find on reference to his work since the foregoing results were written. Louis evidently considers liquid dejections as constituting diarrhœa. Were it practicable to apply this rule to my cases, the number of instances in which this symptom was present, would, probably, be greater.

The reader will have observed that the proportion of instances of diarrhœa is greater in the cases in private practice than in those in hospital. Why is this? The only explanation which offers itself is, that, in most of the cases in private practice, cathartic, or laxative remedies were prescribed, while they were administered very rarely in the cases at the hospital. The latter fact is important to be taken into account in the consideration of the abdominal symptoms in the latter cases. It will be noticed that in four of the cases in private practice the diarrhœa followed, and apparently was occasioned by cathartic remedies. The practical considerations connected with this point will come up more appropriately under the head of the *Treatment of Continued Fever*.

With a view to the comparison of the fatal cases with the average of those not fatal, as respects this symptom, I will examine the former to

ascertain in how many instances diarrhœa was present. Of the *two* cases of *Typhoid*, and the single case of *doubtful* type, in *private practice*, proving fatal, diarrhœa was present in all. It was a prominent and persistent symptom in *one* case; moderate, and easily relieved in *one* case, and the degree is not stated in the remaining case. It was present, after the operation of a cathartic, in the case of *Typhus* in this group. Of the *four* fatal cases of *Typhoid*, of those in *hospital*, diarrhœa was present in *two*. In *one* of these cases, it existed early in the disease, and was followed by obstinate constipation, resisting several drops of Croton oil. In another of these cases costiveness existed.* In *one* case only was the diarrhœa considerable and troublesome. Of the *two* fatal cases of *Typhus*, in *hospital*, diarrhœa was not present in either case. The dejections were *thin* in one of these cases, but not too frequent.

The connection of this symptom with lesions of the intestinal tube is an interesting point of inquiry, but this will be more appropriately considered in connection with the latter subject. *Costiveness*, more or less, was present in *five* cases of *Typhoid* occurring in *private practice*. In *one* of these cases bloody stools took place; in *one*, the costiveness was followed by diarrhœa on the administration of a cathartic; in *one* the costiveness followed diarrhœa present in the early part of the disease; in *one* it existed throughout the career, except that a cathartic excited hypercatharsis; in another case it continued, but cathartics occasioned profuse discharges; in another case it continued until the fourth day, and was followed by severe and persistent diarrhœa. *Costiveness* was present in *five* of the *Typhoid* cases in *hospital*. In one of these cases, it followed diarrhœa, and in another case it was followed by diarrhœa.

In *four* cases no dejection occurred for *three* days, and in *two* cases none for *four* days.

Of the *Typhus* cases in *hospital*, and in *private practice*, *costiveness* existed in *three*. In one of these cases no dejection occurred for several days, and in *two*, none for *four* days.

The character of the evacuations, as already stated, in a large proportion of cases, was not observed. This was owing to the difficulty of preserving them distinct, and retaining them until the time of the daily visit. In *two* of the cases in which the records contain information on this point, the dejections are stated to have been moulded, and natural in appearance. These cases were of the *Typhoid* type. In *six* cases of the same

* This case is reported in full under the head of Delirium.

type, the evacuations were thin and yellow, and in one case thin and of a brown color. Of the cases of *Typhus*, in a single instance only was the character of the evacuations noted, and in this case they were thin and yellow.

Hemorrhage from the bowels occurred in two cases, both of the *Typhoid* type. In both cases, the disease terminated in recovery. One of the cases occurred in *private practice*, and the other in *hospital*. In the former case, the discharge of blood occurred on two occasions, castor oil having been given a few hours previously to each time. The hemorrhage was pretty copious, and attended by considerable exhaustion, but on each occasion it speedily ceased after the administration of opium and the acetate of lead. Moderate tenderness over the abdomen existed in this case, but no tympanites. Costiveness existed throughout the disease in this case, and laxatives were not ventured upon except in the two instances referred to. In the other case, copious bloody evacuations also occurred on two occasions, which ceased speedily after the exhibition of enemata of morphia and tannin. They were attended by considerable prostration. The dejections at other times were thin and yellow. No abdominal distension or tympanites existed in this case, but extreme tenderness, especially in the right iliac region.

The dejections were passed in bed, more or less frequently in six cases of *Typhoid*, (two in private practice and four in hospital,) and in four cases of *Typhus*, (all in hospital.) Of the *Typhoid* cases marked by this event, two were *fatal*; and in but one of the *fatal* cases of *Typhus* is this event stated to have occurred.

Evacuations in bed may be involuntary from paralysis or relaxation of the sphincter muscle; they may arise from unconsciousness, an unconscious act of volition, if this expression be allowable; or they may be due to indifference. On examining the histories of the cases in which this event is noted, I cannot discover that it is to be explained, in either instance, by the first of the causes just mentioned. It did not occur under circumstances of muscular prostration in which this explanation is admissible. In two instances it occurred in connection with a comatose condition, in which the second explanation would perhaps apply. In the remaining cases, it was apparently owing to mental indifference. The patients did not appear to appreciate the impropriety of the act, or have any concern for the consequences; although they were easily roused, and manifested in their replies to questions more or less intelligence. Nor were they annoyed by the contact of excrement with their persons. In two of the cases the de-

lirium was active. In none of the other cases, excluding the cases of coma, was somnolency a marked symptom, but in all more or less, delirium was present. In several of the cases, the patients passed evacuations in bed only occasionally, and in two or three instances, only once or twice during the continuance of the disease.

It was found practicable to prevent this accident many times by carefully watching patients, asking them frequently if they did not desire to evacuate the bowels, and occasionally placing them on the defecating chair, even if they expressed no disposition to have a dejection.

Tympanites. This symptom was considered present only in those cases in which there was obvious distension of the abdomen, as well as resonance on percussion. This signification should be defined, since it is stated that, by French writers, the term (or its synonym *meteorism*) is applied to all cases in which resonance is present, whether the abdomen appear distended or not.

Typhoid. Cases in private practice. In ten cases, nothing is stated on this point. In the remaining ten cases, *tympanites* existed in three. Of these three cases, it was moderate in degree in two, in one case existing only on one day, and it was extreme in one, which was a fatal case.

Cases in Hospital. The histories of all the eighteen cases contain information on this point. *Tympanites* was present in twelve cases. It was moderate in all cases save one, and was considerable in the latter case. *Typhus.* In the single case in private practice, *tympanites* was not present. In the twelve hospital cases, (the histories of all of which contain information on this point,) *tympanites* was present in eight. Of these cases it was moderate in degree in three, slight in four, and in one case the degree is not stated. Adding together all the cases of *Typhoid* on the one hand and those of *Typhus* on the other, *tympanites* thus was present in sixteen of the twenty-eight cases of the former type in which this symptom was mentioned; and in eight of the thirteen cases of *Typhus*, proportions not very far from being equal.

As respects degree, there does not seem to be any ground for distinction between the two types. It would, perhaps, be fairer to compare the hospital cases of *Typhoid* and *Typhus*, excluding the cases in private practice, since all the cases in the latter group, save two, were of the former type. In this view, *tympanites* occurred in a ratio exactly equal in the two types, viz., 12-18 of the cases of *Typhoid*, and 8-12 of the cases of *Typhus*, i. e., in both 4-6. These results are not what I had anticipated. I had supposed this symptom was considerably more frequent in its occurrence in the cases of *Typhoid* than in those of *Typhus*.

I know not how to explain the comparative infrequency of this symptom in the cases in private practice, except by reference to the fact which was adduced in explanation of the greater frequency of diarrhœa in the latter group of cases. The inquiry arises, what connection has *tympanites* with *diarrhœa*? With reference to this inquiry, I have examined the cases in which *tympanites* was present, in order to ascertain in how many of these cases diarrhœa co-existed. In the *four* cases in private practice in which *tympanites* was present, more or less diarrhœa existed in all. In the *Typhoid hospital* cases, diarrhœa co-existed with *tympanites* in *eight*, and *tympanites* was present without diarrhœa in *four*. In the *eight* cases of *Typhus* in which *tympanites* existed, in *all* diarrhœa did *not* co-exist. In one case it is noted that the discharges were thin and yellow, but not too frequent.

These results are curious. In so far as these cases afford data for statistical inferences, it would seem that, in the *Typhoid* type, *tympanites*, in the majority of cases, is accompanied by diarrhœa, but that the former is present without the latter in all cases of *Typhus*! Of course the number of observations is much too limited to authorize such an induction; nevertheless, the contrast is too striking to be considered wholly fortuitous.

With respect to the concurrence of *tympanites* and diarrhœa, it should be added, what will suggest itself to the reader as a corollary of the above facts, that diarrhœa, in both *Typhoid* and *Typhus*, but (according to the above data) oftener in the latter, is present without *tympanites*. This symptom was present in all the *fatal* cases of *Typhoid*, save *one*; and in *each* of the fatal cases of *Typhus*, save *one*. Of the *Typhoid* cases, it was extreme in *one* case, moderate in *two* cases, and the degree is not stated in the remaining *three* cases in which the symptom was present. Of the cases of *Typhus*, it was moderate in one case, and the degree not stated in the other case in which the symptom was present.

Abdominal tenderness. Typhoid. In *two* of the histories of the cases in *private* practice, there is no allusion to tenderness on pressure over the abdomen. In the remaining *eleven* (*Typhoid*) cases, more or less tenderness was present in *eight*. The tenderness was considerable in degree in *one* case, and either moderate or slight in the remainder (*seven*) cases. Of these cases, the tenderness was either confined to, or more marked in the *right iliac* region in *four* cases, and in the other cases the situation is not stated. In the *Hospital Typhoid* cases, information is contained on this point in the histories of all. Of the *eighteen* cases, more or less *tenderness* was present in *eleven*. The tenderness was either moderate or slight in all

but *three* cases. It is noted as especially marked in the *right iliac* region in *two* cases, and in *both iliac regions* in *four* cases.

Typhus. Slight tenderness existed in the single case of *Typhus* in *private* practice. Of the Hospital cases of *Typhus*, information on this point is contained in all the histories. Of the *twelve* cases, tenderness existed in *five*. In all it was slight in degree. The situation of the tenderness is not noted except in one case. In this case, it existed around the umbilicus ; it was present at the early part of the disease, and disappeared during the febrile career. Bringing the two types into comparison as respects this symptom, it was present in *nineteen* of *twenty-eight* cases of *Typhoid*, and in *six* of *thirteen* cases of *Typhus*, showing a considerably larger proportion in the former.

In order to ascertain if there exist any constancy of connection between this symptom and that last considered, viz., tympanites, I have examined the histories of all the cases in which tenderness existed, and enumerated those in which tympanites co-existed. Tenderness with tympanites existed in *ten* cases of *Typhoid*, (*three* in private practice and *seven* in hospital,) and in *four* cases of *Typhus*. In other words, tympanites did not co-exist in *ten* of the cases of *Typhoid* in which tenderness was present, and in *ten* of the cases of *Typhus* in which tenderness existed, tympanites was absent.

On comparing now the number of cases in which tenderness was present in both types, with the number of cases in which tympanites existed, it is obvious, not only that tenderness exists without tympanites in a certain proportion of cases, but that tympanites is present without tenderness in a certain proportion of cases. Thus there exists no relation of dependency between these two symptoms, since either may be present without the other.

It remains to inquire respecting the connection of tenderness with diarrhœa. Of the *Typhoid* cases in which tenderness was present, diarrhœa co-existed in *ten*, (*five* in private practice and *five* in hospital.) Tenderness was present without diarrhœa in *nine* cases, (*three* in private practice and *six* in hospital.) Of the cases of *Typhus*, tenderness co-existed with diarrhœa in *two*. Tenderness was present without diarrhœa in *four* cases. Diarrhœa existed without tenderness in *four* cases of *Typhoid*, and in *two* cases of *Typhus*.

These results show that the two symptoms, *tenderness* and *diarrhœa*, are not connected by any kind of mutual dependency ; and if they spring from the same cause or causes, circumstances may occasion the development of either symptom to the exclusion of the other. In the *fatal Typhoid* cases, tenderness was present in *four* cases, it was not present in *two*

SECTION SIXTH.

Cutaneous Eruptions. The cutaneous eruptions incident to Continued Fever furnish, agreeably to the views of some distinguished observers, striking traits of distinction between the two types denominated *Typhus* and *Typhoid*. I shall, therefore, study the eruptions in the cases grouped after difference in type, separately, and compare the two types as respects the results. It should, however, be premised that in arranging the individual cases into the divisions of *Typhus* and *Typhoid*, considerable importance was attached to the characters, &c., of the eruption.

Typhoid. The characters, etc., which are said to distinguish what is called the *Typhoid eruption* are as follows:—It is generally limited to the chest and abdomen, but occasionally extends to the extremities. It may be copious, particularly over the chest and abdomen, but often it is the reverse, the spots being few in number. The eruption is of a rose red color, [rose spots, *taches roses*,] the spots are oval, appearing somewhat elevated, the redness momentarily disappearing on pressure.

In the histories of the *Typhoid* cases in which an eruption was present, the appearances, etc., were generally described, but in some instances, it is simply stated that it was marked by the *Typhoid* characteristics. I may fairly assume that in all cases in which, variations from these characteristics were present they were noted, and that when the presence of the eruption is stated without special description, it exhibited the distinctive characters above mentioned.

A rose eruption was present in *nine* of the *twelve* cases in *private* practice, and in *fourteen* of the *eighteen hospital* cases, i. e., in *twenty-three* of *thirty* cases. In all the cases in which it was not stated as present, the fact of its absence was stated. It was present in a degree to be called copious in *five* cases in *private* practice, and in *two hospital* cases, i. e., in *seven* cases. In the other cases the number of spots varied from four or five to fifty. In one case only were they so few as four or five. In one case there were only five or six. In one case there were only ten. In all the others there were over fifteen. The eruption extended to the upper and lower extremities in *three private*, and in *one* of the *hospital* cases; in all *four*. It extended to the face in *one* of the *private* cases, and was associated with erythema of the face in that case.

The date of the development of the eruption can be ascertained in only a few of the cases. In many of the cases the disease had commenced

small and faint, and confined to the inferior part of the chest on either side, the redness disappearing on pressure. In the *third* case, the eruption appeared on the sixth day, and was copious, extending over the upper and lower extremities. It was of two kinds. The larger number of spots appeared somewhat elevated, and the redness disappeared on pressure. Other spots were not elevated, and the redness did not disappear on pressure. The eruption continued five or six days, gradually fading until it disappeared (which is the usual course with the eruptions in Continued Fever.)

In the remaining *five* cases no eruption was present. In the *single* case of *doubtful type* in *private* practice, an eruption had existed prior to the case coming under my observation, but the characters were not ascertained. This was a fatal case. In the other fatal case two or three rose-spots were observed, and no autopsy was had in this case.

No inquiries occur to me suggesting comparisons of the eruption with other associated symptoms in individual cases.

SECTION SEVENTH.

Symptoms referable to the Respiratory Apparatus. Cough. Expectoration. Pain in Chest. Pneumonitis. Aberrations of Respiratory Movements. Epistaxis. Singultus.

Cough. More or less cough was present in *ten* cases of those in the *Typhoid* group, *five* being in *private* practice, and *five* in *hospital*. Of these *ten* cases, the cough was moderate, or slight, in *seven*, and a prominent symptom in *three*. It was present only during the early part of the febrile career in *three* cases. It was protracted into convalescence in *one* case. It occurred in connection with *pneumonitis* in *two* cases. In one of these cases the existence of that complication is demonstrated by recorded physical signs; in the other case physical signs are not stated, but the existence of the complication is evidenced by the rational symptoms.

Of the *Typhus* group more or less cough was present in *eleven* cases. Of these *eleven* cases, the cough was moderate, or slight, in *four*, and a prominent symptom in *seven*. It was present only during the early part of the febrile career, in a single case. It was protracted into convalescence in *two* cases. It occurred in connection with *pneumonitis* in *five* cases, in *three* of which that complication is determined by recorded physical signs; in the other *two* cases it is evidenced by rational symptoms.

The periods in the career of the fever of which the *pneumonitis* commenced in the cases of either type, are not given in the analysis preliminary to this Report, save in one instance, and the importance of deter-

mining this point is perhaps not sufficient to compensate for the trouble of re-perusing the histories of the cases in which this complication existed. In the case just excepted, the pneumonitis became developed twenty-seven days after the admission of the patient. Convalescence which was apparently postponed by the occurrence of this secondary affection, was not pronounced until the thirty-second day after his admission. The case referred to was in the *Typhoid* group.

It is not improbable that in some cases, in both groups, slight cough may have occasionally been present and escaped notice. In most of the cases in which the histories do not show that it was present, nothing is stated on the point, but it is not probable, and indeed hardly possible, that this omission could have occurred in any instance in which this was in any degree a prominent symptom. In *six* cases of *Typhoid*, and *six* of *Typhus*, more or less expectoration is stated to have accompanied the cough, but I am not sure that in all the other cases in which cough was present, it was wholly unattended by expectoration. In *two* cases of *Typhoid*, and *three* of *Typhus*, the expectoration is stated to have been muco-purulent. As respects the existence of pneumonitis, I am not satisfied that this complication did not exist, to some extent, in other cases than those in which it is now practicable to determine it to have been present. In general, physical exploration was not practised in cases in which the symptoms did not point to the existence of this complication; and it is certain that pneumonic inflammation may be present without being denoted by rational symptoms, even cough and expectoration being absent.

The foregoing results, however, suffice to show that cough and expectoration are symptoms incident to Continued Fever, without forming a necessary element of the disease; and that pneumonitis becomes developed in a certain ratio of cases, the average frequency of both, in so far as the present collection of cases is concerned, being indeterminate. But these results, imperfect as they may be, are interesting and striking, when considered and compared in the different types. It will be perceived that while cough was recorded present in only *one-third* of the cases of *Typhoid*, it was recorded present in *eleven-thirteenths* of the cases of *Typhus*. Admitting that these results are not perfectly exact, it is fair to conclude that it was present in a much larger proportion of cases of the latter, than of the former type. Moreover, it was a prominent symptom in only *one-fifth* of the cases of *Typhoid* in which it was present, and in more than *one-half* of the cases of *Typhus*. Pneumonitis was present in but *two* of the *thirty* cases of *Typhoid*, and in *five* of the *thirteen* cases of *Typhus*. These comparisons

show a liability to pulmonary affections, considerably greater in the *Typhus* than in the *Typhoid* type of Continued Fever.

Of the *six fatal* cases of *Typhoid*, the presence of cough is not mentioned in the histories of *five*, and in the single remaining case, it was not a prominent symptom.

Of the *three fatal* cases of *Typhus*, it was present in all. It was a prominent symptom in only one case; it was present only in the commencement of the disease, in one case, and it was moderate in degree in the remaining case. It would thus seem that the presence of cough, and its prominency as a symptom, need not affect unfavorably the prognosis in either type of Continued Fever.

Pain in the Chest. This symptom is recorded present in but *one* case of *Typhoid*, and in this case it accompanied the development of pneumonitis on the twenty-seventh day after the admission of the patient. It was recorded present in *one* case of *Typhus*, occurring in connection with cough at the early part of the febrile career, and situated beneath the sternum.

Aberrations of Respiration. Aberrations of respiration, more or less in degree, and differing in kind, appear in the histories of *nine* of the *thirty* cases of *Typhoid*, (*four* in *private* practice, and *five* in *hospital*), and in *ten* of the *thirteen* cases of *Typhus*, thus showing a great preponderance in the latter type of the disease. In *eight* of the histories of the cases in the *Typhoid* group, it is recorded that the respiration was unaffected, and nothing is stated on this subject in the histories of *thirteen*. In the histories of the *three* cases of *Typhus* in which some aberrations of respiration were not noted, nothing is stated on the subject. The varieties of disorder in the respiratory movements, and the number of cases in which they were noted, are as follows:—*increased frequency* in *three* cases of *Typhoid*, and in *eight* cases of *Typhus*; *diminished frequency* in *three* cases of *Typhoid*, and in none of *Typhus*; *sighing respiration*, in *two* cases of *Typhoid*, and in *three* cases of *Typhus*; *panting on slight exertion* in *one* case of *Typhoid*, and *one* case of *Typhus*; *stertor* in *two* cases of *Typhus*; *catching* (inspiration shortened and quickened) in *one* case of *Typhoid*, and in *four* cases of *Typhus*; *irregularity in rhythm* in *three* cases of *Typhus*; *sibilant nasal rale*, in *four* cases of *Typhoid*, and in *two* cases of *Typhus*; *dilation of the alve nasi*, in *one* case of *Typhoid* and in *four* cases of *Typhus*. These variations were, of course, more or less associated in different cases. It should also be stated that the aberrations of respiration occurring toward the close of life, in the cases which proved fatal, are not included, since they are, under such circumstances, incident to the

mode of dying, and do not, strictly speaking, belong to the career of the disease.

It is of some interest to determine the relative frequency of these several kinds of aberration in cases of the two forms of Continued Fever; but a more interesting point of inquiry relates to their connection with the ulterior morbid conditions upon which they are dependent. Some of the symptoms that have been enumerated, are expressions of disorder of the nervous system, others are occasioned by the state of the pulmonary organs, and some may involve either or both. To bring the results of the analytical investigation to bear on this point of inquiry, a collection of cases might be divided into two groups, in the one group pulmonary disease being present as a complication of the fever, and in the other group this complication being absent. The two groups should then be studied and compared with respect to the presence of the several symptoms just mentioned. Symptoms present without the evidence, physical and rational, of pulmonary disease, it would be fair to refer to a nervous origin; on the other hand, those present in cases in which a pulmonary complication existed, might be due to it, or they might, still, originate in the nervous system; but if they exclusively existed in the group distinguished by the presence of pulmonary complication, they should be considered incidental to the latter. This interrogation of results is but imperfectly available in the present investigation, because, from neglect of physical explorations, it cannot be positively determined in what cases pneumonic complication was absent. I will, however, under distinct heads, institute, so far as practicable, a comparison in the manner pointed out, regretting that the data are not more satisfactory.

Increased Frequency of Respiration. In four of the cases of *Typhus* in which this variety of aberration was present, *pneumonitis* undoubtedly existed. Of the remaining four cases of *Typhus* characterized by this symptom, in all cough co-existed. In one case the cough was accompanied with sanguinolent expectoration. In another case the respirations were much accelerated (56 per minute the maximum) with labor, and panting on exertion. In this case pneumonitis may be suspected to exist. In another case, muco-purulent expectoration accompanied the cough, and occasionally the expectoration was streaked with blood. Dilatation of the *ala nasi* also co-existed. Either pneumonitis or bronchitis must have existed in this case as a complication. In the remaining case, the cough was a prominent symptom. Thus, all the *Typhus* cases attended with accelerated breathing, were complicated with pulmonary disease.

Of the three cases of *Typhoid* in which the respiratory movements were

show a liability to pulmonary affections, considerably greater in the *Typhus* than in the *Typhoid* type of Continued Fever.

Of the *six fatal* cases of *Typhoid*, the presence of cough is not mentioned in the histories of *five*, and in the single remaining case, it was not a prominent symptom.

Of the *three fatal* cases of *Typhus*, it was present in all. It was a prominent symptom in only one case; it was present only in the commencement of the disease, in one case, and it was moderate in degree in the remaining case. It would thus seem that the presence of cough, and its prominency as a symptom, need not affect unfavorably the prognosis in either type of Continued Fever.

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Aberrations of Respiration. Aberrations of respiration, more or less in degree, and differing in kind, appear in the histories of *nine* of the *thirty* cases of *Typhoid*, (*four* in *private* practice, and *five* in *hospital*), and in *ten* of the *thirteen* cases of *Typhus*, thus showing a great preponderance in the latter type of the disease. In *eight* of the histories of the cases in the *Typhoid* group, it is recorded that the respiration was unaffected, and nothing is stated on this subject in the histories of *thirteen*. In the histories of the *three* cases of *Typhus* in which some aberrations of respiration were not noted, nothing is stated on the subject. The varieties of disorder in the respiratory movements, and the number of cases in which they were noted, are as follows:—*increased frequency* in *three* cases of *Typhoid*, and in *eight* cases of *Typhus*; *diminished frequency* in *three* cases of *Typhoid*, and in none of *Typhus*; *sighing respiration*, in *two* cases of *Typhoid*, and in *three* cases of *Typhus*; *panting on slight exertion* in *one* case of *Typhoid*, and *one* case of *Typhus*; *stertor* in *two* cases of *Typhus*; *catching* (inspiration shortened and quickened) in *one* case of *Typhoid*, and in *four* cases of *Typhus*; *irregularity in rhythm* in *three* cases of *Typhus*; *sibilant nasal rale*, in *four* cases of *Typhoid*, and in *two* cases of *Typhus*; *dilation of the alve nasi*, in *one* case of *Typhoid* and in *four* cases of *Typhus*. These variations were, of course, more or less associated in different cases. It should also be stated that the aberrations of respiration occurring toward the close of life, in the cases which proved fatal, are not included, since they are, under such circumstances, incident to the

this symptom be taken in connection with these results, it can hardly be doubted that it is due to a morbid condition of the nervous system. It is to be borne in mind that the co-existence of pulmonary disease in a certain proportion of cases, does by no means establish any connection between a symptom present in such cases, but also present in other cases in which pulmonary disease is absent. To prove such a connection the symptom should be present exclusively in cases characterized by pulmonary disease. I have been accustomed to regard frequent sighing as of bad omen in fever. This idea is, in some measure, sustained by the fact that in *two* of the *five* cases in which the symptom was noted, the disease proved fatal.

Panting on slight exertion. This was noted in *one* of the above cases of *Typhoid* in which *sighing* occurred, unconnected with any marked symptoms of pulmonary complication. In the other case cough and other pulmonary symptoms were prominent, and it is highly probable that pneumonitis existed.

Stertor. In both of the cases of *Typhus* in which this symptom occurred other than as an immediate forerunner of death, pulmonary symptoms were prominent, pneumonitis existing in one case, if not in the other. This, however, obviously, is not a symptom due directly to the condition of the lungs.

Inspiration shortened and quickened. In the single case of *Typhoid* in which this aberration is noted, the existence of secondary disease of the lungs is indeterminate from the history, no pulmonary symptoms being recorded, while cerebral symptoms were prominent. In the *four* cases of *Typhus* cough and other pulmonary symptoms were prominent, and in *three* of the cases pneumonitis was marked. In *two* of the *five* cases presenting this aberration, the disease proved fatal. This symptom I have been accustomed to regard as an expression of a morbid condition seated at the nervous centre, rather than in the pulmonary organs. The correctness of this view is not disproved, although it is in no wise sustained by the above results.

Irregularity. Of the *three* cases of *Typhus* in which this symptom was noted, in *one* case the history affords no evidences of the existence of pulmonary complication. In the *two* other cases pneumonitis co-existed. This, like the preceding aberration, is generally attributed to a nervous origin; and the fact that, in one of the three cases in which it was present pulmonary disease did not co-exist, suffices to show that it is not necessarily dependent on the latter.

*Dilation of the *alæ nasi*.* In the single *Typhoid* case in which this was noted, the data are insufficient to determine either the presence or absence

accelerated, the recorded data are quite insufficient for determining the presence or absence of pulmonary disease in one case. Nothing is stated with respect to cough. The respiration was catching, with dilation of the *alae nasi*; coma and stertor succeeding. In another of the cases nothing is stated relative to cough. The respiration in this case was moderately accelerated, the maximum being 24. In the remaining case *pneumonitis* was evidenced by cough and rusty expectoration.

In conclusion, then, with respect to this symptom, in *all* but *two* of the *eleven* cases in which it was present in both types, it was accompanied by evidences of pulmonary complication; and in the two excepted cases the data are too defective for any definite inferences. I should state that in speaking of acceleration of the respiration as a symptom, I mean a degree of acceleration sufficient to attract attention. The histories of all the cases do not embrace an enumeration of the respiratory movements. When these did not appear to be increased they were not always enumerated. Hence, it may be that a slight increase above the average normal frequency may have existed in some instances when it was not sufficiently apparent to excite notice.

Diminished Frequency of Respiration. In none of the *three* cases of *Typhoid* in which this symptom was present, does the history afford evidence of pulmonary complication. In *one*, it is stated that no cough existed, and in the other *two* cases, nothing is recorded on that point. In the first case the respiration is noted to be somewhat slower and deeper than in health. Cerebral nervous symptoms, in this case, were not prominent. In one of the remaining cases cerebral symptoms were prominent, eventuating in coma. This case proved fatal. In the other case, the respiration was but slightly retarded, and more heavy than in health. In this case, about the average of cerebral symptoms were present. In so far as these few and imperfect observations go, diminished frequency of respiration is not to be attributed to morbid conditions of the lungs; and this accords with the view of this symptom generally entertained.

Sighing. By this I mean occasional, or frequent sighing, not a continuously suspicious respiration. Of the *two* cases of *Typhoid* in which this symptom was noticed, in *one*, cough was present at the early part, but in a trifling degree. In the other case cough did not exist. Of the *three* cases of *Typhus* in which the symptom was present, in *one*, cough and sanguinolent expectoration was present; in *one*, slight cough had existed, but did not continue up to the time that the case came under observation; and the remaining case was complicated with *pneumonitis*. If the character of

this symptom be taken in connection with these results, it can hardly be doubted that it is due to a morbid condition of the nervous system. It is to be borne in mind that the co-existence of pulmonary disease in a certain proportion of cases, does by no means establish any connection between a symptom present in such cases, but also present in other cases in which pulmonary disease is absent. To prove such a connection the symptom should be present exclusively in cases characterized by pulmonary disease. I have been accustomed to regard frequent sighing as of bad omen in fever. This idea is, in some measure, sustained by the fact that in *two* of the *five* cases in which the symptom was noted, the disease proved fatal.

Panting on slight exertion. This was noted in *one* of the above cases of *Typhoid* in which *sighing* occurred, unconnected with any marked symptoms of pulmonary complication. In the other case cough and other pulmonary symptoms were prominent, and it is highly probable that pneumonitis existed.

Stertor. In both of the cases of *Typhus* in which this symptom occurred other than as an immediate forerunner of death, pulmonary symptoms were prominent, pneumonitis existing in one case, if not in the other. This, however, obviously, is not a symptom due directly to the condition of the lungs.

Inspiration shortened and quickened. In the single case of *Typhoid* in which this aberration is noted, the existence of secondary disease of the lungs is indeterminate from the history, no pulmonary symptoms being recorded, while cerebral symptoms were prominent. In the *four* cases of *Typhus* cough and other pulmonary symptoms were prominent, and in *three* of the cases pneumonitis was marked. In *two* of the *five* cases presenting this aberration, the disease proved fatal. This symptom I have been accustomed to regard as an expression of a morbid condition seated at the nervous centre, rather than in the pulmonary organs. The correctness of this view is not disproved, although it is in no wise sustained by the above results.

Irregularity. Of the *three* cases of *Typhus* in which this symptom was noted, in *one* case the history affords no evidences of the existence of pulmonary complication. In the *two* other cases pneumonitis co-existed. This, like the preceding aberration, is generally attributed to a nervous origin; and the fact that, in one of the three cases in which it was present pulmonary disease did not co-exist, suffices to show that it is not necessarily dependent on the latter.

Dilation of the alæ nasi. In the single *Typhoid* case in which this was noted, the data are insufficient to determine either the presence or absence

of pulmonary disease. In the *three Typhus* cases, cough existed in all. It was prominent in *one*, and the expectoration was streaked with blood; it was moderate in *one*; and in the *two* remaining cases, *pneumonitis* existed.

Sibilant nasal rale. This symptom has no special reference either to the nervous system, or pulmonary organs, but is probably dependent on the state of the nasal passages.

The foregoing examination of cases with a view to determine the connection subsisting between the several aberrations of the respiratory movements, and the secondary affections of the pulmonary organs occurring in Continued Fever, may not seem to furnish results sufficiently numerous and explicit to compensate for the labor which it has cost. I should perhaps hardly deem them deserving of the space which they occupy in the report, were it not that, possibly, they may serve to suggest the application of a similar method of comparison in a larger series of cases with the facts relating to the pulmonary system more satisfactorily recorded.

The reader may be surprised that in connection with the subjects embraced in this Section, I do not adduce the appearances presented on dissection of the pulmonary organs, in the instances in which the disease proved fatal. The reason for this omission will appear in the Section devoted to the Autopsical Observations, where it will be perceived that the post obit histories of the fatal cases are very imperfect as respects the pulmonary organs.

Epistaxis. Epistaxis was noted in *eight* of the cases of *Typhoid* (*three* in *private* practice and *five* in *hospital*). In *three* cases it is noted that this symptom did not occur. In the remainder nothing is said either of its absence or presence. In all the above eight cases the hemorrhage occurred from the nostrils, more or less in quantity. In a few cases (*three*) not included in this enumeration, it was observed that sputa detached from the posterior nares were tinged with blood. This may have occurred in other instances and have escaped observation. It is also not impossible that epistaxis may have occurred in some of the cases not under observation from the commencement of the disease, the fact not being ascertained, although, generally, if not universally, pains were taken to inquire whether this symptom entered into the previous history. In most of the cases in which nothing is stated relative to this point in the history, it is fair to presume the symptom did not occur. Here, as in other instances, the importance of recording negative facts was not sufficiently borne in mind. As respects the period in the career of the Fever when epistaxis was observed,

in *all* but *three* instances this is stated in the preliminary analysis. In these three cases it is simply stated that it occurred early. In the other cases the periods were respectively as follows :—In *one* case, on the 8th and 10th days; in *one*, on the 8th day; in *one*, daily, up to the 7th day, and sometimes profusely; in *one*, on the 10th day; and in *one*, on the 26th day. The ratio in which this symptom was observed in many cases is very nearly the same as in the 303 cases of Typhoid Fever analyzed by Dr. Jackson. Of the quantity of blood which escaped, and the precise number of times the hemorrhage took place, the histories do not contain information. Pains were not taken to ascertain the former more especially. Generally speaking, it was slight, and I am quite certain that no appreciable effect upon the progress of the disease was produced by it. In none of the cases in which epistaxis occurred, did the disease prove fatal; and *two* of the *three* cases in which it was noted that this symptom did not occur, were fatal cases. Of the cases of the *Typhus* group, epistaxis was noted in but *two*. In *one* of these it occurred several times early in the febrile career; in the other case it occurred once early, and once afterward. In one of these cases the disease proved fatal. In none of the cases of Typhus is it noted that the sputa from the posterior nares were tinged with blood. In *three* of the cases in this group the absence of this symptom is noted. In the other cases nothing is stated on the subject. Comparing the foregoing results in the two groups, this symptom occurs oftener in *Typhoid* than in *Typhus*, the ratio being a little more than one in four of the former, and one in six and a half in the latter type.

Singultus. This symptom was observed in *two* cases, *one* of *Typhoid*, and the *other* of *doubtful* type. In the former case it was a troublesome symptom for several days. The disease proved fatal, but this symptom ceased toward the close of life. It was unaccompanied by involuntary movements of the voluntary muscles. In the other case, it occurred on the day after the admission of the patient, four grains of opium having been administered the night previous. The respiration was, at the same time, somewhat labored, and the inspiration shortened and quickened. This case became complicated with pneumonitis.

In the foregoing enumerations and comparisons with respect to the symptoms referable to the respiratory system, it will here be observed that the cases of *doubtful* type (with the single exception in the preceding paragraph) have not been included. It is probably not of much importance to present the facts in the histories of these cases belonging to this class, but since it has been done hitherto, it will render the report defective to omit

them, and as the readiest mode of accomplishing the object they are arranged in the following tabular form:—

1	2	3	4	5	6	7	8	9
Cough prominent. Accelerated respiration. Probably Pneumonitis	Respiration slightly accelerated. Nothing stated as to Cough, etc.	Slight Cough continued into convalescence. No aberration of respiration stated.	No Cough or expectoration. Respiration accelerated <i>et</i> <i>vasi</i> dilate	Moderate Cough. Expectoration streaked with blood. Short and catching respiration FATAL.	Slight Cough. Respiration unaffected.	Cough and expectoration. Respiration labored and catching. Physical signs of Pneumonitis. Epistaxis several times.	No Cough. Respiration unaffected. Epistaxis several times.	Slight Cough and substen. and patens. Respiration normal. Sputa from posterior nares tinged with blood.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Physiological and Therapeutical Effects of Tannic Acid. By Dr. ALISON.

As an astringent, I have found tannic acid exceedingly efficacious, certainly as much so as any other agent, vegetable or mineral, that I have ever employed. It has equalled the salts of lead, copper, and zinc, without producing any of those poisonous effects which are liable to follow the free use of the salts of the first two metals.

Internal Use.—In the *chronic bronchial catarrh* of weakly and elderly persons, unconnected with disease of the heart or great blood-vessels, and attended with copious and debilitating expectoration, the administration of tannic acid by the mouth, in doses of one, two, and three grains, two or three times daily, has greatly and gradually abated the secretion, relieved the frequent cough, and improved the strength of the patient. In the second stage of *pulmonary consumption*—viz., that of softening, when bronchial catarrh has been present to a large extent, weakening the patient, causing frequent cough, and disturbing sleep, the same results have followed, and have greatly contributed to the comfort and welfare of the sufferer. But in pulmonary disease, the greatest amount of benefit has obviously been derived when large cavities have been present in the lungs, the walls of which have thrown out large quantities of purulent matter, occasionally mixed with blood. In such cases the discharge has been effectually controlled, and the rate of tear and wear of the system obviously restrained, without the induction of oppression or other evils.

In *chronic diarrhœa*, which had resisted the ordinary treatment by chalk, opium, and regulated diet, and was not dependent on obstructive disease of the heart or liver, tannic acid, in a solid form, has proved of surprising efficacy. In cases of severe disease, depending on irritable mucous membrane, I have not known of one failure; and of those examples connected with chronic inflammation and disorganization of the mucous membrane, only two proved beyond the influence of this remedy. These two cases occurred during the last autumn, while cholera was prevalent, and the dis-

ease of the mucous membrane was extensive. The complaint, in one of the examples, was of long standing, and the patient had been addicted to habits of intemperance. But it was not tannic acid only that failed; the salts of copper, iron, lead, and zinc, in large doses, proved to be of no more avail. In this form of disease tannic acid was administered in the form of pill, in combination with opium.

In *leucorrhœa*, unconnected with inflammatory action, I have found tannic acid efficacious in restraining the discharge, and in increasing the strength of the patient. The aqueous solution, combined with a small proportion of dilute nitric acid, was the form usually employed in these examples of disease. In *menorrhagia*, not dependent on a plethoric state of the system, or on local congestion, it was also serviceable, administered in the same form.

The excessive *sweating in phthisis*, and in other diseases running on to a fatal termination, has been usefully restrained by the use of tannic acid, combined with dilute nitric acid; and the habitual cold damp upon the skin of soft, weakly constitutions, has been corrected by the same means. I have had no opportunity of testing the virtues of this remedy in the *hemorrhagic diathesis*; but I am strongly disposed to believe they would be found very considerable, conjoined with other suitable means. I believe it would prove serviceable in *albuminuria*, dependent on chronic disorganization of the kidney, and not associated with obstructive disease. When the egress of albumen results, as I believe it often does in no small degree, from reduced tone and elasticity in the organ, and is not (as in a great majority of cases) a wholesome outlet necessary for the relief of the circulation, tannic acid offers the promise of benefit. Such a case, however, I have not lately met with, and consequently have not had an opportunity of testing the treatment.

Local application.—In the form of aqueous solution, used as a gargle, tannic acid has been most useful in correcting relaxation of the throat. Sponginess and hemorrhage of the gums have been greatly controlled by a lotion of tannic acid, and by the application of the dry powder. By this means loose teeth may be retained for a time, and the impediment to articulation thereby prevented, which would result from their removal.

In *prolapsus ani* I have prescribed tannic acid, dissolved in water, as an injection. This remedy is particularly indicated when the disease is associated with great relaxation of the solids. Applied to *hemorrhoidal tumors*, free from inflammation, in the form of a fine powder, mixed with lard, it would doubtless prove more efficacious than galls, the usual remedy. It is assuredly due to the tannic acid which it contains, that *uva ursi* proves serviceable in *catarrhus vesicæ*.

In *gonorrhœa*, chronic or about to become such, tannic acid, applied externally as a lotion, has proved serviceable. In the latter mode it has induced no smarting, although the parts have been tender, and though it has been applied with little intermission for several days. It is as a *local astringent* that tannic acid produces the most obvious effects, as Dr. Garrod has remarked.

Of tannic acid as an astringent, I have merely further to say, that it is of special excellence as an external application to the skin, when such a remedy is required. I have found it of extraordinary efficacy when reduced to a fine powder, mixed with lard, and applied to the skin. The

small and faint, and confined to the inferior part of the chest on either side, the redness disappearing on pressure. In the *third* case, the eruption appeared on the sixth day, and was copious, extending over the upper and lower extremities. It was of two kinds. The larger number of spots appeared somewhat elevated, and the redness disappeared on pressure. Other spots were not elevated, and the redness did not disappear on pressure. The eruption continued five or six days, gradually fading until it disappeared (which is the usual course with the eruptions in Continued Fever.)

In the remaining *five* cases no eruption was present. In the *single* case of *doubtful type* in *private* practice, an eruption had existed prior to the case coming under my observation, but the characters were not ascertained. This was a fatal case. In the other fatal case two or three rose-spots were observed, and no autopsy was had in this case.

No inquiries occur to me suggesting comparisons of the eruption with other associated symptoms in individual cases.

SECTION SEVENTH.

Symptoms referable to the Respiratory Apparatus. Cough. Expectoration. Pain in Chest. Pneumonitis. Aberrations of Respiratory Movements. Epistaxis. Singultus.

Cough. More or less cough was present in *ten* cases of those in the *Typhoid* group, *five* being in *private* practice, and *five* in *hospital*. Of these *ten* cases, the cough was moderate, or slight, in *seven*, and a prominent symptom in *three*. It was present only during the early part of the febrile career in *three* cases. It was protracted into convalescence in *one* case. It occurred in connection with *pneumonitis* in *two* cases. In one of these cases the existence of that complication is demonstrated by recorded physical signs; in the other case physical signs are not stated, but the existence of the complication is evidenced by the rational symptoms.

Of the *Typhus* group more or less cough was present in *eleven* cases. Of these *eleven* cases, the cough was moderate, or slight, in *four*, and a prominent symptom in *seven*. It was present only during the early part of the febrile career, in a single case. It was protracted into convalescence in *two* cases. It occurred in connection with pneumonitis in *five* cases, in *three* of which that complication is determined by recorded physical signs; in the other *two* cases it is evidenced by rational symptoms.

The periods in the career of the fever of which the pneumonitis commenced in the cases of either type, are not given in the analysis preliminary to this Report, save in one instance, and the importance of deter-

mining this point is perhaps not sufficient to compensate for the trouble of re-perusing the histories of the cases in which this complication existed. In the case just excepted, the pneumonitis became developed twenty-seven days after the admission of the patient. Convalescence which was apparently postponed by the occurrence of this secondary affection, was not pronounced until the thirty-second day after his admission. The case referred to was in the *Typhoid* group.

It is not improbable that in some cases, in both groups, slight cough may have occasionally been present and escaped notice. In most of the cases in which the histories do not show that it was present, nothing is stated on the point, but it is not probable, and indeed hardly possible, that this omission could have occurred in any instance in which this was in any degree a prominent symptom. In *six* cases of *Typhoid*, and *six* of *Typhus*, more or less expectoration is stated to have accompanied the cough, but I am not sure that in all the other cases in which cough was present, it was wholly unattended by expectoration. In *two* cases of *Typhoid*, and *three* of *Typhus*, the expectoration is stated to have been muco-purulent. As respects the existence of pneumonitis, I am not satisfied that this complication did not exist, to some extent, in other cases than those in which it is now practicable to determine it to have been present. In general, physical exploration was not practised in cases in which the symptoms did not point to the existence of this complication; and it is certain that pneumonic inflammation may be present without being denoted by rational symptoms, even cough and expectoration being absent.

The foregoing results, however, suffice to show that cough and expectoration are symptoms incident to Continued Fever, without forming a necessary element of the disease; and that pneumonitis becomes developed in a certain ratio of cases, the average frequency of both, in so far as the present collection of cases is concerned, being indeterminate. But these results, imperfect as they may be, are interesting and striking, when considered and compared in the different types. It will be perceived that while cough was recorded present in only *one-third* of the cases of *Typhoid*, it was recorded present in *eleven-thirteenths* of the cases of *Typhus*. Admitting that these results are not perfectly exact, it is fair to conclude that it was present in a much larger proportion of cases of the latter, than of the former type. Moreover, it was a prominent symptom in only *one-fifth* of the cases of *Typhoid* in which it was present, and in more than *one-half* of the cases of *Typhus*. Pneumonitis was present in but *two* of the *thirty* cases of *Typhoid*, and in *five* of the *thirteen* cases of *Typhus*. These comparisons

and having been introduced into the chest, the suction could be applied immediately, and all the fluid usually discharged, without any change of the apparatus, save the turning of the piston-pipe, above described, in order to discharge the fluid when the barrel was full.

Dr. B. had seen the operation performed five times during the last three months. All the patients were immediately more or less relieved; two had tubercles at the time, one of whom has since died. Two cases were perfectly successful, the patients being, at the time of the operation, very ill, with pulse over 120, night sweats, &c. One recently operated on with the greatest relief was still under treatment. Finally, he knew of one case treated by Dr. Wyman, who is now well, and who undoubtedly would have died had the operation not been performed.

The pain of this operation is, comparatively speaking, nothing, and the patients are generally not at all troubled by it. The wound, of course, closes instantly on the removal of the canula, and no air can enter the chest while the apparatus is in operation.

Dr. Bowditch concluded by remarking that operations had been done from time immemorial, upon the chest in cases of effusion, but he believed that they were generally considered as a last resort. In Guy's Hospital Reports, cases are given of the use of the trocar, but Dr. B. believed that to Dr. Wyman was due the credit of having first proposed the use of the *exploring canula with suction applied thereto*.

Dr. Bowditch has arrived at the following conclusions:—

1st. The operation is perfectly simple, but slightly painful, and can be done with ease upon any patient in however advanced a stage of disease.

2d. It should be performed forthwith in *all* cases in which there is a complete filling up of one side of the chest.

3d. Dr. B. had determined to use it in *any* case of even *moderate* effusion lasting more than a few weeks, and in which there should seem to be an indisposition to yield to the ordinary modes of treatment.

4th. Dr. B. would urge the practice of puncturing in this way upon the medical profession *as a very important measure in practical medicine*; for he believed that, by this method, death may be frequently prevented from ensuing either from sudden attacks of dyspnoea or subsequent phthisis, or, finally, as in two cases he had seen, from the gradual wearing out of the powers of life from inability to absorb the fluid, even when all the organs, save the pleura of the affected side, were healthy.

5th. Dr. B. was likewise inclined to the belief that this operation if generally adopted, would sometimes prevent the occurrence of those very tedious cases of spontaneous evacuation of purulent fluid, and those great contractions of the chest that occur after long-continued effusion and the subsequent discharge or absorption of a fluid.—*Am. Jour. of Med. Sciences.*

Hydrometra. Case reported by Dr. D. H. STORER.

The patient was a large and very fleshy woman, thirty-five years of age, and weighed about 230 pounds. In June, 1849, I saw her for the first time, and received the following history: that she had been married three times, and had borne five children by her first husband, but none since. Previously to the last two years, she had suffered but little at her menstrual periods; but since then the pain at such times had been intense, and,

mode of dying, and do not, strictly speaking, belong to the career of the disease.

It is of some interest to determine the relative frequency of these several kinds of aberration in cases of the two forms of Continued Fever; but a more interesting point of inquiry relates to their connection with the ulterior morbid conditions upon which they are dependent. Some of the symptoms that have been enumerated, are expressions of disorder of the nervous system, others are occasioned by the state of the pulmonary organs, and some may involve either or both. To bring the results of the analytical investigation to bear on this point of inquiry, a collection of cases might be divided into two groups, in the one group pulmonary disease being present as a complication of the fever, and in the other group this complication being absent. The two groups should then be studied and compared with respect to the presence of the several symptoms just mentioned. Symptoms present without the evidence, physical and rational, of pulmonary disease, it would be fair to refer to a nervous origin; on the other hand, those present in cases in which a pulmonary complication existed, might be due to it, or they might, still, originate in the nervous system; but if they exclusively existed in the group distinguished by the presence of pulmonary complication, they should be considered incidental to the latter. This interrogation of results is but imperfectly available in the present investigation, because, from neglect of physical explorations, it cannot be positively determined in what cases pneumonic complication was absent. I will, however, under distinct heads, institute, so far as practicable, a comparison in the manner pointed out, regretting that the data are not more satisfactory.

Increased Frequency of Respiration. In *four* of the cases of *Typhus* in which this variety of aberration was present, *pneumonitis* undoubtedly existed. Of the remaining *four* cases of *Typhus* characterized by this symptom, in all cough co-existed. In *one* case the cough was accompanied with sanguinolent expectoration. In another case the respirations were much accelerated (56 per minute the maximum) with labor, and panting on exertion. In this case pneumonitis may be suspected to exist. In another case, muco-purulent expectoration accompanied the cough, and occasionally the expectoration was streaked with blood. Dilation of the *ala nasi* also co-existed. Either pneumonitis or bronchitis must have existed in this case as a complication. In the remaining case, the cough was a prominent symptom. Thus, *all* the *Typhus* cases attended with accelerated breathing, were complicated with pulmonary disease.

Of the *three* cases of *Typhoid* in which the respiratory movements were

accelerated, the recorded data are quite insufficient for determining the presence or absence of pulmonary disease in one case. Nothing is stated with respect to cough. The respiration was catching, with dilation of the *alae nasi*; coma and stertor succeeding. In another of the cases nothing is stated relative to cough. The respiration in this case was moderately accelerated, the maximum being 24. In the remaining case *pneumonitis* was evidenced by cough and rusty expectoration.

In conclusion, then, with respect to this symptom, in *all* but *two* of the *eleven* cases in which it was present in both types, it was accompanied by evidences of pulmonary complication; and in the two excepted cases the data are too defective for any definite inferences. I should state that in speaking of acceleration of the respiration as a symptom, I mean a degree of acceleration sufficient to attract attention. The histories of all the cases do not embrace an enumeration of the respiratory movements. When these did not appear to be increased they were not always enumerated. Hence, it may be that a slight increase above the average normal frequency may have existed in some instances when it was not sufficiently apparent to excite notice.

Diminished Frequency of Respiration. In none of the *three* cases of *Typhoid* in which this symptom was present, does the history afford evidence of pulmonary complication. In *one*, it is stated that no cough existed, and in the other *two* cases, nothing is recorded on that point. In the first case the respiration is noted to be somewhat slower and deeper than in health. Cerebral nervous symptoms, in this case, were not prominent. In one of the remaining cases cerebral symptoms were prominent, eventuating in coma. This case proved fatal. In the other case, the respiration was but slightly retarded, and more heavy than in health. In this case, about the average of cerebral symptoms were present. In so far as these few and imperfect observations go, diminished frequency of respiration is not to be attributed to morbid conditions of the lungs; and this accords with the view of this symptom generally entertained.

Sighing. By this I mean occasional, or frequent sighing, not a continuously suspicious respiration. Of the *two* cases of *Typhoid* in which this symptom was noticed, in *one*, cough was present at the early part, but in a trifling degree. In the other case cough did not exist. Of the *three* cases of *Typhus* in which the symptom was present, in *one*, cough and sanguinolent expectoration was present; in *one*, slight cough had existed, but did not continue up to the time that the case came under observation; and the remaining case was complicated with *pneumonitis*. If the character of

employer, with his name and residence, in his own chirography and orthography, which is on file:

New York, Nov. 25th, 1848.

Mrs. W.'s Examination—

Pressure on the Brain, look as if there would be too much water on the brain, if not removed, Bronchial tubes are weak which effects the Eppagladis no disease on the Lungs Great weakness of the Palmonary arteries which arises from the inflamary state of the blood also it has caused a tubucle to form on the Heart, Great weakness in the Heart caused palpitation the blood is in a very bad state which if not removed will cause scrofula disease of the blood, Liver topid and dry muscles swelled, Kidneys are not diseased but are weak caused by the overflowing of the gall on them, Weakness in the womb, Spine is diseased which effects the head and limbs causing a heavy feeling of the whole system.

Rx. Balsam 5 drops every night for one week, Mullien 3 leaves simmered in half Pint Milk drink frequently Cancalagua one teaspoonful three times a day in a little water for one week one gale powder to-night and one Fever powder in the morning.

DR. C. GRATTAN,

137, Grand-street, New-York.

On the Treatment of Acute Rheumatism. By GEO. L. UFSHUR, M. D., Norfolk, Va.

A celebrated physician being once asked what he considered the most successful treatment of acute rheumatism, answered, "six weeks," thereby plainly intimating his opinion of the intractable nature of the disease. While I am ready to admit that there is *some* truth in this opinion, and that rheumatism now and then assumes a form which might well entitle it to a place among the *opprobria medicorum*, I am not at all prepared to entertain the notion that early and judicious treatment has no power, in a majority of cases, to alleviate its pangs, or shorten its duration. On the contrary, I believe that rheumatism is as amenable to proper management as any disease of equal severity.

Within the past five years, I have accurately noted the history and treatment of about *thirty* cases of acute articular rheumatism, from among which the twelve cases just detailed are indiscriminately taken.* In these, it will be seen, the average duration of the disease was $5\frac{1}{2}$ days; the longest being 14, and the shortest 2 days. *Croton oil* was used in 11, *colchicum* in 6, *quinine* in 6, *blood-letting* in 2, and *iodide of potassium* in 2. I desire to say a word or two upon each of these agents.

Of *Croton oil* as a purgative in acute rheumatism, I am prepared to speak in the highest terms. Cathartics have always stood in the front rank of remedies in this disease, but I am disposed to believe that the efficacy of *Croton oil* does not depend entirely upon its cathartic properties; it possesses a power over the disease beyond these, and apparently not dependent upon them, for other cathartics, which act as powerfully and as promptly, producing similar watery stools, do not bring a like amount of relief to the patient. I do not say that it is a *specific*, for I am not a be-

* The cases are omitted.—EDITOR BUFFALO MEDICAL JOURNAL.

of pulmonary disease. In the *three Typhus* cases, cough existed in all. It was prominent in *one*, and the expectoration was streaked with blood; it was moderate in *one*; and in the *two* remaining cases, *pneumonitis* existed.

Sibilant nasal rale. This symptom has no special reference either to the nervous system, or pulmonary organs, but is probably dependent on the state of the nasal passages.

The foregoing examination of cases with a view to determine the connection subsisting between the several aberrations of the respiratory movements, and the secondary affections of the pulmonary organs occurring in Continued Fever, may not seem to furnish results sufficiently numerous and explicit to compensate for the labor which it has cost. I should perhaps hardly deem them deserving of the space which they occupy in the report, were it not that, possibly, they may serve to suggest the application of a similar method of comparison in a larger series of cases with the facts relating to the pulmonary system more satisfactorily recorded.

The reader may be surprised that in connection with the subjects embraced in this Section, I do not adduce the appearances presented on dissection of the pulmonary organs, in the instances in which the disease proved fatal. The reason for this omission will appear in the Section devoted to the Autopsical Observations, where it will be perceived that the post obit histories of the fatal cases are very imperfect as respects the pulmonary organs.

Epistaxis. Epistaxis was noted in *eight* of the cases of *Typhoid* (*three* in *private* practice and *five* in *hospital*). In *three* cases it is noted that this symptom did not occur. In the remainder nothing is said either of its absence or presence. In all the above eight cases the hemorrhage occurred from the nostrils, more or less in quantity. In a few cases (*three*) not included in this enumeration, it was observed that sputa detached from the posterior nares were tinged with blood. This may have occurred in other instances and have escaped observation. It is also not impossible that epistaxis may have occurred in some of the cases not under observation from the commencement of the disease, the fact not being ascertained, although, generally, if not universally, pains were taken to inquire whether this symptom entered into the previous history. In most of the cases in which nothing is stated relative to this point in the history, it is fair to presume the symptom did not occur. Here, as in other instances, the importance of recording negative facts was not sufficiently borne in mind. As respects the period in the career of the Fever when epistaxis was observed,

in *all* but *three* instances this is stated in the preliminary analysis. In these three cases it is simply stated that it occurred early. In the other cases the periods were respectively as follows :—In *one* case, on the 8th and 10th days; in *one*, on the 8th day; in *one*, daily, up to the 7th day, and sometimes profusely; in *one*, on the 10th day; and in *one*, on the 26th day. The ratio in which this symptom was observed in many cases is very nearly the same as in the 303 cases of Typhoid Fever analyzed by Dr. Jackson. Of the quantity of blood which escaped, and the precise number of times the hemorrhage took place, the histories do not contain information. Pains were not taken to ascertain the former more especially. Generally speaking, it was slight, and I am quite certain that no appreciable effect upon the progress of the disease was produced by it. In none of the cases in which epistaxis occurred, did the disease prove fatal; and *two* of the *three* cases in which it was noted that this symptom did not occur, were fatal cases. Of the cases of the *Typhus* group, epistaxis was noted in but *two*. In *one* of these it occurred several times early in the febrile career; in the other case it occurred once early, and once afterward. In one of these cases the disease proved fatal. In none of the cases of Typhus is it noted that the sputa from the posterior nares were tinged with blood. In *three* of the cases in this group the absence of this symptom is noted. In the other cases nothing is stated on the subject. Comparing the foregoing results in the two groups, this symptom occurs oftener in *Typhoid* than in *Typhus*, the ratio being a little more than one in four of the former, and one in six and a half in the latter type.

Singultus. This symptom was observed in *two* cases, *one* of *Typhoid*, and the *other* of *doubtful* type. In the former case it was a troublesome symptom for several days. The disease proved fatal, but this symptom ceased toward the close of life. It was unaccompanied by involuntary movements of the voluntary muscles. In the other case, it occurred on the day after the admission of the patient, four grains of opium having been administered the night previous. The respiration was, at the same time, somewhat labored, and the inspiration shortened and quickened. This case became complicated with pneumonitis.

In the foregoing enumerations and comparisons with respect to the symptoms referable to the respiratory system, it will here be observed that the cases of *doubtful* type (with the single exception in the preceding paragraph) have not been included. It is probably not of much importance to present the facts in the histories of these cases belonging to this class, but since it has been done hitherto, it will render the report defective to omit

other gentlemen who have written upon the subject, in leucorrhœal cases—purulent or muco-purulent. I find inflammation, engorgement, induration, excoriation, patches of aphthæ, epithelial abrasion, and granulation often enough, but very seldom what I could call Ulceration, in non-malignant and non-syphilitic cases.

The clearest description I can find of the so-called ulceration, is in Dr. Bennet's work, (pages 102 and 103,) which I take the liberty of extracting, and certain expressions in which I have italicised: it occurs under the heading—

"Inflammatory Ulceration.—Inflammation may exist for years in the cervix and its cavity, without giving rise to any other anatomical changes than those which have been enumerated. This, however, is seldom the case. The mucous membrane lining these regions, and more especially that portion of it which is near the os, appears to be *peculiarly liable to take on ulcerative action.* Consequently, the existence of inflammation *in the great majority of instances* is soon followed by the manifestation of the ulcerative process. Ulceration generally appears first round the os, and just within the cavity of the cervix. Many different forms or species of ulceration are described by Continental writers, but, in my opinion, without necessity or advantage. An ulceration occupying the cervix uteri may present all the various modifications which suppurating surfaces offer in any other part of the body, from the minute granulations of a slight abrasion, to the livid vegetations of an unhealthy sore; but these modifications of the ulceration require in reality no division or classification."

"When an abrasion or excoriation only is present, the cervix is generally of a vivid red, and the granulations are often so minute, that it is at first difficult to ascertain whether the mucous membrane is abraded or merely congested, or to perceive the limit of the ulceration when once it has been ascertained to exist. The doubt, however, may be solved by lightly touching the suspected surface with the nitrate of silver. The abrasion immediately assumes a much whiter hue than the region which is merely congested, and its margin becomes well defined and evident. *An abraded or excoriated condition of the mucous surface is generally the form under which ulceration presents itself in the cavity of the cervix,* granulations of any size being seldom met with in this region.

"In its more decided form, ulceration of the cervix uteri is susceptible of presenting every possible variety. The granulations may be firm, of a vivid red hue, scarcely bleeding on pressure, or they may be large, fungous, livid, and bleeding profusely on the slightest touch. These fungous ulcerations are generally connected with torpor of the local circulation: when they are present, the congestion of the vagina and cervix is often very great, of a livid venous character, and the non-ulcerated surface of the cervix may present dilated varicose veins."

Now, this portrait, the general fidelity of which I admit, does not appear to me to warrant the profuse use of the term "ulceration," which in itself, and when applied to other parts of the body, has a definite and unmistakable meaning. It cannot truly be said, that mere abrasions or excoriations caused by irritating leucorrhœal discharges are "ulcerations." We do not call the excoriation which occurs from similar causes in other parts of the body "ulceration." We do not call granulations upon mucous membranes "ulcerations," whether they secrete mucus or pus, or whether

they bleed or not upon mechanical irritation. If we analyze this important passage closely, we shall find that granulations at the os and cervix uteri is the strongest fact testified in support of the theory of "ulceration." Dr. Bennet states, that "an ulceration occupying the cervix uteri may present all the various modifications which suppurating surfaces offer in any other part of the body, from the minute granulations of a slight abrasion to the livid granulations of an unhealthy sore." But, immediately afterwards, he negatives this by adding, "An abraded or excoriated condition of the mucous membrane is generally the form under which ulceration presents itself in the cavity of the cervix, granulations of any size being very seldom met with in this region." Thus, it is evident, that even in the absence of granulations, Dr. Bennet calls "an abraded or excoriated condition" a "form" of "ulceration." From his own descriptions, it is evident that Dr. Bennet classes abrasions, excoriations and granulations together, as forms of ulceration, a proceeding which it appears to me is utterly opposed to all sound pathology. In this way we may explain the great frequency with which ulcerations of the os and cervix uteri is declared to be met with.

If we consider excoriation or abrasion as genuine ulceration, probably no woman ever passes through life without suffering from this form of disease. In the virgin uterus, the circulation is frequently modified by the recurrence of menstruation, ovarian irritation, mental emotion, the varying conditions of the bladder and rectum; and in constitutional ailments, the vaginal and uterine secretions, in common with the other secretions of the body, are frequently depraved. Excoriation and abrasion of the mucous membranes are easily accounted for under such circumstances. Menstruation alone, in the turgidity of the uterus and ovaria, before the catamenial flow is established; in the exudation of blood from the surface of the uterus; and in the perforation of the peritonæal membrane for the elimination of the ovule from the ovary, trenches very nearly upon pathology. The slightest divergence from the ordinary function merges into disease.

In married women, and those who have borne children, other prejudicial causes in addition to these are in operation—such are the mechanical irritation of coitus, the risk of lacerations of the os uteri during the passage of the child in parturition, and the state of the uterine orifice which obtains after labor, and the return of the organ to quiescence. After labor, the orifice of the uterus does not contract smoothly, so as to leave the os uteri regular and even, but it becomes puckered and contracted unevenly. In irritable conditions of the mucous membrane of the uterus and vagina, or in a morbid state of the utero-vaginal secretions, these folds or corrugations are very liable to be chapped or excoriated, and I believe this is often mistaken for ulceration. All these, and other causes which I might enumerate, explain the frequency with which the os uteri deviates, in color, volume, and secretion, from the strictly healthy standard. In fact, we may compare the upper part of the vagina to the fauces, which is seldom found perfectly healthy in any subject who may be examined. Some of the indurations and enlargements of the os and cervix uteri appear to resemble enlarged tonsils, and, like them, to increase in size without any amount of active inflammation.

The granulations which are sometimes found surrounding the os uteri—which may secrete mucus or pus abundantly, and which may bleed on

being roughly handled—are, I have no doubt, the result of inflammation; but they resemble *the granular state of the conjunctiva*, rather than the granulations of a true ulcer, the granular os uteri offering no edges or signs of solution of continuity, by which we might satisfactorily declare it to be an ulcer. The *granular os uteri* would be a more correct designation, in such cases, than “ulceration” of the os uteri. Some of the so-called ulcerations appear to be nothing more than patches of thickened epithelium, or portions of the os and cervix, from which the epithelium has been melted away by acrid or irritating secretions. We can imitate this condition of the parts by the slight application of the nitrate of silver—sufficient to affect the epithelial covering, but not sufficient to injure the mucous membrane beneath.

It appears to me that we can neither receive the existence of excoriation or abrasion; of granulation or fungous growths; the secretion of pus or muco-purulent matter; as affording undeniable evidence of the existence of “ulceration” of the os and cervix uteri. We must try ulceration in this part of the body by the same tests which we apply to ulcers in other parts of the economy. We must look for a solution of continuity, with a secreting surface, separated from the healthy structures, having defined edges, everted or inverted,—for an ulcer, in fact, in the common pathological meaning of the term. We find ulcers having these characters in the air-passages, mouth, stomach, intestines, bladder, and other mucous surfaces. There is no mistaking the characters of an intestinal ulcer after dysentery, and there ought to be no mistake about an ulcer of the uterus. Indeed, in the corroding ulcer of the uterus we unfortunately see that this organ is but too capable of taking on all the qualities of ulceration, in the degree only equaled by its extraordinary vitality, the organ being scooped out, or eaten away, in a comparatively short space of time. Cases are also met with, in which the os uteri has been destroyed by the sloughing ulceration and loss of structure, sometimes following the application of the more powerful caustic agents. We are, however, called upon, by the unlimited believers in uterine ulceration, to admit that ulcerative disease may exist for years, in its common form, without any perforation, excoriation, serious loss of substance, or altered configuration. Whether we test the so-called ulceration of the uterus by ulceration occurring in other mucous surfaces, or in the uterus itself, under undoubtedly ulcerative disease, the distinctive characteristics are wanting in the great majority of cases; and they certainly are not found, unless I am most egregiously mistaken, in the enormous proportion of 222 cases of ulceration to 300 cases of promiscuous uterine disease.

In all that I have said, I do not wish it to be supposed that I question the frequency of irritation, chronic inflammation, and sub-acute inflammation, in connection with leucorrhœa. Recent writers would, however, treat leucorrhœa merely and solely as a symptom, not as an independent disorder. But I am well assured that it is often the disease itself, or at least all of it that we can appreciate; and that the irritable or inflammatory condition is excited secondarily, and mainly, by the morbid leucorrhœal secretion. Some change in the innervation or nutrition of the organ occurs; or it sympathizes with a malady in some remote organ, and the secretions are consequently depraved. These depraved secretions irritate the surfaces with which they come in contact, and produce the visible signs

of irritation or inflammatory action. We see these discharges sometimes inflame and excoriate even the external integument, but we should never dream of saying that the inflamed condition of the skin was the essential part of the disorder. The same observation applies to the uterus. Thus it is not pathological, nor useful, always to consider leucorrhœa as a mere symptom; and the old plan of astringent injections, though sometimes mischievous, cannot quite be dispensed with, for in some, even profuse leucorrhœas, an astringent injection, by arresting the utero-vaginal discharges, does more than any other plan to soothe inflammatory conditions, or rather, to suspend their causes.

Notwithstanding the use of the speculum,—notwithstanding the use of lamps and glasses, there is often considerable difficulty in ascertaining the precise condition of the cavity of the uterine cervix, engorged as it is, and deep in color from irritation, or other disease, and from the interruption to the circulation in the uterine organs which is almost necessarily dependent on the introduction and expansion of the speculum within the vagina. But in the dead subject no such difficulties exist, and it might certainly be expected, since leucorrhœa is a malady so very common, that uterine ulceration would be frequently revealed by post-mortem examinations. The only place in which, so far as I am aware, post-mortem examinations have been conducted in considerable numbers, with special reference to the determination of the frequency or infrequency of ulceration of the os and cervix uteri, is at St. George's Hospital. For several years past, the condition of the uterus has been examined with great minuteness and accuracy in the dead subject at this hospital.

Mr. Pollock, one of the lecturers on Anatomy at St. George's Hospital, informs me, that for more than three years during which he was Curator to the hospital museum he examined the uterus internally and externally, in all the subjects in the dead-house. During this time, upwards of 100 women died in the hospital annually. In each case, the uterus was laid open, and carefully inspected. Mr. Pollock only detected actual and unmistakable ulceration in four cases. Of these, three were scrofulous subjects, and scrofulous ulceration existed in other parts of the body; and in one of them the ulceration involved the vagina extensively as well as the os uteri.

Mr. Gray, who succeeded Mr. Pollock as Curator, informs me that during his curatorship he examined the bodies of 180 women, who died of all diseases in St. George's Hospital, with a distinct view to ascertain the proportion of cases in which ulceration of the uterus existed. These examinations were also conducted with great care and minuteness. Out of the 180 subjects, distinct ulceration of the os and cervix was found in only three instances. Slight abrasions, discolorations and granulations were frequently observed, and this accords with the observations of Mr. Pollock. One or two other curators to St. George's Hospital, besides Mr. Pollock and Mr. Gray, have arrived at the same results. It is only by pathological investigations of this kind that we can arrive at infallible results.

But, it may be asked, why bestow so much pains on proving that abrasion, excoriation and ulceration are not *ulceration*? Why dispute as to terms? Simply because a name rules treatment, and because the name of "ulceration" being first given, and heroic treatment not without danger, is frequently resorted to, where milder local applications or constitutional

treatment would be equally efficacious. After Mr. Abernethy wrote his celebrated work on the Constitutional Treatment of Local Disease, his idea was pushed to its extreme, and local remedies were often most improperly neglected. Now, in all that relates to the uterine organs, the doctrines of Mr. Abernethy are in danger of being entirely refuted, and we are in some risk of utterly neglecting constitutional treatment and of being entirely absorbed by local applications. This we cannot do without impeding the improvement of the treatment of this class of affections. When a patient is told she has an ulceration of the womb, she often thinks of an ulcer of the leg, or the cheek, &c., and is proportionably frightened, because of the importance of the organ which is the seat of the presumed disease.

There is nothing women will not submit to, to be freed from such a malady. At the present time a veritable uterine panic affects the upper and middle classes of society, and every woman with the slightest ache or discharge, is not satisfied until the peccant organ has been ocularly inspected. I do not believe that this state of things, or its inevitable results, will conduce to the dignity and respectability of our profession. I do not hesitate to affirm, so far as I have eyes to observe and judgment to weigh facts, that much exaggeration prevails respecting the frequency of this same ulceration of the os and cervix uteri—an exaggeration which should be calmed, so that the legitimate methods of examination may lead, not to a suspicion of our profession, but to real improvement in the diagnosis and treatment of uterine disease as it actually exists. We cannot safely repudiate either the local or the constitutional treatment of uterine disease. I have seen cases in which the local ailment has been as far as possible cured; nevertheless, the constitutional symptoms remained unrelieved. I have seen others, in which judicious constitutional treatment has cured the local malady without any topical treatment whatever. But in the combat against disease, we require both constitutional and local weapons; and any views which disparage either the one or the other, must cripple the resources of our art.—*London Lancet.*

DR. BENNET'S REJOINDER.

On the frequency of Ulceration of the Cervix Uteri, and on the Pathological value of the term Ulceration. By J. HENRY BENNET, M. D., Late Physician-Accoucheur to the Western Dispensary, etc.

To the Editor of The Lancet.

SIR,—In the August No. of THE LANCET there has appeared a paper of Dr. Tyler Smith, previously read at the Westminster Society, on the "Supposed Frequency of Ulceration of the Os and Cervix Uteri," to which I trust you will allow me briefly to reply.

I would firstly remark that the title of Dr. Smith's paper ought not to have been on the "presumed" frequency of ulceration of the cervix, but rather "On the Pathological meaning of the term Ulceration, as applied to solutions of continuity of the organ in question." The, in reality, erroneous title of Dr. Smith's paper implies a negation, on his part, of the frequency of the lesions which I have described in my work on Inflammation of the Uterine Organs, under the generic head of Ulceration; whereas such a negation is not to be found in any part of his paper. On the contrary, he

admits, in a dozen places, the probable frequency of these lesions, but differs from me in thinking that the term ulceration ought, in sound pathology, to be applied to them.

This is a most important feature in the discussion raised by Dr. Smith; inasmuch as by thus correcting the title of his paper, and making it really agree with the contents of the latter, we reduce the question at issue to what it actually is, to one of *words only*—viz., are the lesions I describe in my work to be considered forms of ulceration, or are they not?

Before I proceed to the examination of this question, I must observe, that although evidently written with a very different purpose, Dr. Tyler Smith's Essay, from first to last, substantiates and supports my views. By a process of mental reasoning and reflection, if not by clinical experience, he has clearly become a convert to my doctrines respecting uterine pathology and so fully appreciates the influence of the various causes of inflammatory disease to which the uterine organs are exposed, at the various periods of uterine life—causes which I have developed at length in my work, and which he rapidly enumerates—that he absolutely states, "*If we consider excoriation or abrasion as genuine ulceration, probably no woman ever passes through life without suffering from this form of disease.*" Surely this is out-heroding Herod. Although my *clinical* experience has enabled me to say that out of 300 women presenting decided uterine symptoms, and observed in the course of two years' special practice at a large public institution, 222 offered some form or other of ulceration, (*including abrasions and excoriations.*) I should be very sorry indeed to make such a sweeping assertion as the above; uterine lesions even of this description, not existing long, without making known their presence by local or constitutional disturbance at least, in the immense majority of cases. Were this assertion true, we then should find nearly every other female we meet laboring under uterine disease, as, I hear, some practitioners facetiously state to be the case at present. I leave Dr. Tyler Smith, however, to reconcile his theory of what ought to be the state of uterine pathology with what it is clinically found to be, and will only add, that, for my part, although convinced of the frequency of inflammation, and of inflammatory ulcerative lesions of the cervix in women presenting uterine symptoms, I still have great confidence in the power of nature to carry through her operations, generally speaking, without hurt or accident. I consequently decidedly think that the very great majority of women pass through all the trials of uterine life without being affected either with abrasions or excoriations. Inflammatory lesions are no more *the rule* with the uterus than with the brain, the liver, or the lungs, although they may be as frequently, or even more frequently, the exception.

With regard to the main question at issue, the proper interpretation of the term ulceration, in applying it generally to all solutions of continuity of the cervix and its cavity, whether slight or considerable, I have followed both the highest authority and the bent of my own mind. I have always considered divisions and distinctions in the nomenclature of disease as useless, and obstructive to progress, unless founded on some real and therapeutically important difference. Thus, were I to write a treatise on diseases of the skin, I should throw aside, without scruple, one-half of the forms of disease now admitted, because they are merely founded on a species of botanical consideration of the visible appearance of the disease, and

are, in reality, only different modes of manifestation, and different stages of development, of a malady identically the same, and requiring the same treatment.

Applying these principles of general pathology to the ulcerative lesions produced by inflammation of the cervix and its cavity, instead of describing abrasions, excoriations, and luxuriant ulcerations, as distinct morbid conditions, (which they are not,) I have embodied them all in the general term ulceration, adding, by way of explanation, "that ulceration occupying the cervix uteri may present all the various modifications which suppurating surfaces offer in any other part of the body, from the minute granulation of a slight abrasion to the livid vegetations of an unhealthy sore."

That I am warranted in applying the term ulceration even to a mere abrasion, the result, not of physical violence, but of inflammation and of morbid vital action, must be evident to all who are acquainted with the classical literature of the profession. "Ulceration," says Samuel Cooper, "is the process by which sores or ulcers are produced in animal bodies." J. L. Petit defines an ulceration, or ulcer, "as a solution of continuity, from which is secreted pus, or a puriform, sanious, or other matter." Boyer states, that "an ulceration is a solution of continuity of the soft parts, more or less ancient, accompanied by a purulent secretion, and kept up by some local or internal cause." Any of these definitions apply quite as truly to a mere abrasion or excoriation, secreting pus or sanies, as to the chronic excavated *cutaneous* ulcer, which Dr. Tyler Smith most unaccountably expects to find on the cervix uteri.

Indeed, it is with extreme surprise that I find Dr. Tyler Smith saying, that, to be able to apply the term ulceration to the cervix uteri, "we must look for a solution of continuity with a secreting surface, separated from the healthy structures, having defined edges, everted or inverted,—for an ulcer, in fact, (query, a cutaneous ulcer?) in the common pathological meaning of the term." Had not Dr. Smith commenced by stating that he is in the habit of using the speculum, I should be inclined to think that he was reasoning from analogy only; for the fact is, and he ought to know it, if his opinions are the result of practical experience, that the form of ulcers thus described is scarcely ever met with on the neck of the uterus, except as the result of syphilitic chancre or of corroding ulcer. Owing to the tenuity of the mucous membrane lining the cervix, and its cavity, the margin of an inflammatory ulceration is scarcely ever, if ever, either everted or inverted. So much is this the case, that it is generally most difficult to say where the ulceration finishes, until, by the application of the nitrate of silver, the margin of the sore, or the point where the epithelium finishes, be revealed. In my work (see page 103) I have distinctly stated, and now repeat, that "whatever the character of an inflammatory ulceration of the cervix, the ulcerated surface is *never* excoriated; it is always on a level with or above the non-ulcerated tissues that limit it, and its margin never presents any abrupt induration. Owing to this circumstance it is always impossible to determine by the touch the precise point at which the ulceration terminates."

Thus Dr. Tyler Smith's criticism of my position respecting the frequency of uterine ulceration, is founded, on the one hand, on a frivolous negation of the term ulceration to abrasions and excoriations, these lesions being strictly and legitimately within the pale of all classical definitions of ulcera-

tion; and, on the other, on the establishment of a visionary ideal of uterine ulceration, drawn from chronic cutaneous ulcers of the leg, such sores or "ulcers" not being met with at all, in reality, on the cervix uteri.

In the course of the above argumentation, I have not alluded to the French writers on uterine diseases, because I did not require their assistance to establish my position. I may now, however, add, that all who have written in France on the subject of uterine inflammation, have applied the word ulceration to the lesions to which I apply it. Some have called abrasions and excoriations, exulcerations, or granular ulcerations, which I have not thought proper to do; but this is the only difference between me and them. I have thus not only reason and pathology on my side, but also antecedents, and the example of my seniors—men of the first eminence and talent, such as Lisfranc, Gendrin, Jobert, Duparcque, &c.

Dr. Tyler Smith, in common with all those who are now *vainly* endeavoring to arrest the strong current of professional feeling towards a correct and sound uterine pathology, appears to think that I and those who adopt my views are discarding all constitutional treatment, and directing our attention solely to local lesions. This, however, is a most unwarranted "petitio principis." I would ask—Where is the proof? Certainly not in my work on Uterine Inflammation, for a large portion of it is devoted to the minute and careful investigation and elucidation of general and constitutional symptoms, to which I strenuously and continually direct the attention of my readers. Nor have I observed any evidence, in the practice of those medical men, converts to the modern doctrines, with whom I come in contact, that constitutional means of treatment are by them neglected.

Dr. Tyler Smith lays great stress on the results of the post-mortem examination, at St. George's Hospital, which seem to me, on the contrary, to completely corroborate my views. He states, that only seven instances of ulceration of the cervix (excavated with distinct edges, I suppose) have been found on the post-mortem examination of the females who die annually in the hospital. In the same paragraph, however, he is obliged to confess that slight abrasions, discolorations, and granulations, were frequently observed. The severe ulcerations alluded to appear to have been scrofulous sores, the result of the softening and emptying of the tubercular masses, or malignant ulcerations.

I cannot conclude these remarks, without entering my protest, in the most forcible and energetic manner, against a statement made by Dr. Tyler Smith, at the conclusion of his paper. He says:—"At the present time, a veritable uterine panic affects the upper and middle classes of society, and every woman with the slightest ache or discharge, is not satisfied until the peccant organ has been ocularly inspected." I have no hesitation whatever in stating that this assertion is a libel on our country-women, which I trust has only escaped from Dr. Smith in the hurry of composition. The change that has taken place in the arguments brought forward by those who adhere to the prejudices and errors of days which we shall soon be able, I firmly believe, to call past, is truly remarkable. Five years ago, when I published the first edition of my work, I was greeted by the observation, that English women were too modest and reserved to submit to physical examination, and that I should only destroy my professional prospects by advancing views which required it. Now, however, that English women suffering under uterine disease are beginning to become aware

that there is relief to be found, and that they need no longer be handed from practitioner to practitioner, in a vain search after health; now that they show themselves willing to control agony of mind and pain of body, for the sake often of those who are dear to them, an odious accusation is thrown in their teeth, and they are told that they are ready, nay, *anxious*, to submit to uterine examinations, nearly without a cause or pretext. I can only say, that I meet with *no such females*, either in the higher or the lower ranks of my country-women, and that I blush for those who thus asperse them.

I am, Sir, your obedient servant,

HENRY BENNET.

Phosphate of Ammonia, and its employment in Gout and Rheumatism.

Dr. Samuel Edwards, in a paper read before the *Westminster Medical Society*, after giving a history of this salt, and its mode of preparation, remarked with regard to its physiological effect, that "in the ordinary dose of ten grains it sometimes occasions a slight degree of nausea, accompanied with heat of the epigastrium; immediately after which, if the surface be kept warm, it acts as a stimulating diaphoretic, and also a diuretic." The proximate cause of gout and rheumatism he considers in the light of "blood diseases, arising from morbid matter circulating in the blood, originally formed in the primary and secondary assimilating processes," and consequently disturbing the parts to which it is attracted. He considers the two as varieties of the same disease.

From a number of experiments Dr. Edwards had arrived at the conclusion that both gout and rheumatism had lithic acid for their essential cause; that it was supplied by the nitrogenized elements of the food as well as the changing tissues of the body; and confirmed the fact that uric acid existed in the blood. On the physiological action of this remedy in these diseases, Dr. Edwards says:

On being taken up into the system, and coming in contact with the uric acid, or urate of soda, it becomes decomposed, a phosphate of soda and urate of ammonia would be produced; thus exchanging a very insoluble for a very soluble salt; but this is not all, for Baron Liebig has shown that the phosphate of soda has remarkable effect upon uric acid, rendering it soluble with facility in water. By these means, therefore, the combined and free uric acid existing in the system in these diseases will be dissolved and capable of easy elimination by the kidneys. Dr. Edwards then remarked that he had used the phosphate of ammonia in almost every variety of gout and rheumatism, and almost always with the most beneficial effects. He had warded off attacks by its early employment. Before using it he generally prefaced it by a purgative of calomel and colocynth, or some other, and in acute articular rheumatism adopted the usual local and general antiphlogistic treatment. It had been found extremely useful in those cases bearing a resemblance to neuralgic disease. Another important fact to which Dr. Edwards drew attention was, that in fifteen cases in which he had used this remedy in *acute rheumatism*, in no one had *heart* symptoms accompanied it. He also described its solvent power as great in uric acid

gravel, and asked, might it not be available in uric acid calculi? He had given it a comparative trial with phosphate of soda and benzoic acid, and found it far more useful in its effects. In some few cases of gout he had used a lotion of it, with good and soothing effects, especially where a concretion of urate of soda appeared to be forming.—*Dublin Medical Press*, June, 1850.—*From N. Y. Register of Med. and Pharmacy.*

Incontinence of Urine in Children. By J. SIMON, Esq., F. R. S.

Irritability of the bladder in children usually takes, with more or less completeness, the form of *incontinence of urine*: the child wets its bed. Whenever this symptom is presented to you, if you proceed to examine the urine, (as in every such case you should do,) you may pretty confidently expect to find copious crystals of lithic acid. This condition of the urine in children is very far from painless; and in severe cases the symptoms cannot at first sight be distinguished from those of calculate. The child makes water very often, and a little at a time, doubles itself up, and cries with the pain of each effort, and pinches and pulls its prepuce, just as it would with stone in the bladder. The pain experienced is a severe scalding in the urethra, and sometimes this passage will be so much irritated as to inflame and secrete pus. There was recently a case under my treatment which, though not one of incontinence of urine (for it was in an adult,) will yet serve to show the manner of dealing with such inconveniences, generally as depend on the passage of crystals of lithic acid in the urine. The patient, W. M., aged 22, had for two or three years suffered occasionally with symptoms, which made it probable that he had a calculus lodged in his left kidney; but the immediate cause of his admission to the hospital was the circumstance of his then habitually passing lithic acid gravel, occasionally mixed with blood. His urination was frequent and painful; his pulse was feeble, and he was of little muscular power; his skin acted fairly; his tongue was white and coated; his bowels a little constipated. I ordered him five grains of Plummer's pill every night till his tongue was quite clean, and then changed the treatment; giving him quin. disulph. gr. ii. twice a day, and potass. bicarbon. half a drachm, five hours after his chief meal. He left the hospital after a month's stay, quite free from uneasiness in his urinary organs, and materially improved in general health.

This case will illustrate the sort of treatment which I generally pursue in similar instances of chemical derangement of the urine. If the tongue is coated, and if (as is usually the case with children) the intestinal secretions are unhealthy, I give hydrarg. c. creta, or some other preparation of mercury, till that evil is remedied; I then commence the exhibition of alkalis, giving usually a single large dose daily, after the completion of the digestion of the chief meal of the day; and almost invariably I find it highly advantageous to give quinine twice a day during the same period. In my hands it has answered far better than any preparation of iron, and especially so in the combination I have mentioned. I give it usually before breakfast and before dinner, and the alkali, in copious solution, five hours after the latter meal. Extreme attention to the quantity, quality, and simplicity of the diet, is essential.

With this treatment you will seldom, I think, have occasion to resort to blistering over the sacrum, and other measures of a similar nature, which have been recommended for the cure of incontinence of urine in children.—*Lancet*.

Successful removal of an Ovarian Tumor by the large abdominal section.

By Dr. VAN BUREN, of New York.

In this case, which is stated to be the second successful operation of the kind which has occurred in this city, the patient was 21 years of age, unmarried, and the tumor of five years growth, and still increasing in size. It was purely a fibrous growth of the left ovary, and connected with the uterus by the broad ligament only, which was very much attenuated and elongated. The incision by which its removal was effected, was nearly thirteen inches in length, and the tumor weighed over seven pounds. The omentum was partially adherent to its surface, and on detaching its connections with the knife, three arteries (of the omentum) were divided, which required ligatures, both ends of which were cut off, the knots being returned into the abdomen. The pedicle of the tumor was surrounded by a solitary ligature, the ends of which were brought out of the external wound. This was closed by the "*Carlsbad insect pins*," and the twisted suture, the favorite method of Dieffenbach, of Berlin, in cases where certain union by the first intention was desired.

The case progressed favorably; not a solitary bad symptom occurred which was not readily controlled, and after a month's confinement to the bed, the patient was allowed to resume her ordinary occupations.

It should have been mentioned that the patient was suffering from an extreme degree of *procidentia uteri*, a consequence of the pressure exerted by the tumor from above. This was reduced before the operation, and retained in its place after the patient's recovery by a globular caoutchouc pessary, about two and a half inches in diameter, which answered its purpose admirably.

Dr. Van Buren attributes the successful result of his case, 1st, To its exceedingly promising character from the great mobility and slender attachments of the tumor; 2d, To the employment of chloroform during the operation, by which all shock was prevented; and 3d, By the free use of opium to obviate peritonitis.—*N. Y. Jour. of Med.*, March, 1850.

Who are the Regulars? In variety of doctors, New York is beyond all England, and the distinction, if we could enumerate them all, would be multitudinous and amusing.

1. Regular graduates, M. D's., who claim to be both Physicians and Surgeons.
2. Honorary graduates, M. D's., many of whom are guiltless of any education of any sort.
3. Fictitious M. D's., who may or may not be licentiates, and yet they claim the title and wear it.
4. Licentiates of any county or state societies, who when honest, prefix the letters Dr. to their names; though many of these fictitiously assumed to be graduates.

5. Homœopaths, all of whom sign their names M. D., whether with or without authority.
6. Hydropaths, ditto.
7. Magnetic and Mesmeric doctors, some of whom claim the title.
8. Paw doctors, who profess to cure by friction with the hand.
9. Indian doctors, who profess to have knowledge of aboriginal remedies.
10. Cancer doctors, who promise to cure cancer without the knife, and who apply arsenic and other caustic plasters.
11. Seventh-son doctors, whose "*larning cum by natur.*"
12. Thompsonian doctors, who use lobelia, cayenne, and steam.
13. Natural bone setters, who cure by dislocating and reducing all the bones at pleasure.
14. Botanical doctors, who claim to reject all minerals, and rely for remedies on *roots and yarbs.*

And besides all these we have so-called oculists, and aurists, lung, liver, kidney, and urine doctors, dyspepsia doctors, and a numerous class of Lock Hospital, or secret doctors, with pile and corn doctors, *et id genus omne*, whose name is Legion; so that surely in this great city no one need suffer without doctoring, or being doctored, in every conceivable variety.

Thus far no mention has been made of the tribe of blood doctors—and pill doctors, and panacea doctors, and bitters doctors, and consumption doctors, and the clergymen's sore throat doctors; though many of these are veritable M. D's. But our limits will not permit us to enlarge at present, though we are sensible very many of the doctors have not yet been named. Indeed, some of them, it would be a shame even to name, so shameless is their craft.

Our British brethren, however, have nothing to boast of in the way of exemption from quackery. So far from it, we are indebted to England, especially to London, for the worst examples among us; but it is fair to presume that many of them "left their country for their country's good."—*N. Y. Med. Gazette.*

Simple Method of Testing Quinine.—Our worthy friend, Charles Augustus Smith, Pharmaceutist, of Cincinnati, has published the following plan for detecting the stearic and margaric acids and spermaceti in sulphate of quinine, by means of chloroform. Six grains of the suspected salt are agitated in a test-tube with a fluid drachm of chloroform for two minutes; the sulphate of quinine is then dissolved out by dilute sulphuric acid, the solution separated from the chloroform, which is then washed with distilled water, and suffered to evaporate gradually on a piece of paper. The fatty matter, if present to the extent of ten per cent., will be found on the paper, which will itself have a greasy stain upon it.—*Ohio Med. and Surg. Journal.*

Prize Essay on Nostrums.—The committee of the New York State Medical Society, on prize dissertations, reported at its last session the reception of one essay on the subject for which the prize was offered last year. This being not in accordance with the spirit and requirements of the reso-

lution under which it was offered, they recommended the passage of the following resolution, which was adopted:—

Resolved, That a prize of twenty dollars be offered for the best essay “on the pernicious influences of nostrums and secret remedies on the health and morals of the community.” Said essay to consist of not less than sixteen pages, or more than twenty of the Transactions, to be adapted for popular, rather than professional instruction. The essays to be transmitted to the Secretary of the Society, before the 1st of January, 1851.—*N. Y. Med. Journal*.

Statistics of Amputations. The Boston Medical Journal contains an invaluable paper, by Dr. George Hayward, exhibiting in detail the statistics of the amputations of large limbs, performed at the Massachusetts General Hospital from 1822 to 1849, inclusive—a period of twenty-seven years.

The following summary of the whole is thus stated:

“It appears, then, from these tables, that the whole number of amputations of large limbs that have ever been performed at the Hospital, is 146, on 141 patients; of this number, 32 died.

“Eighty-five had their limbs removed in consequence of disease, of whom 10 died.

“Fifty-six in consequence of injury; of whom 22 died; being one in 8½ of the former, and more than 1 in 3 of the latter.

69 patients had the thigh amputated—	19 died.
50 had the leg removed below the knee—	10 died.
11 had amputation above the elbow	— 1 died.
11 “ below “	— 2 died.

141

32

“The ages of the patients were as follows:

Under 20 years of age, 26, of whom	4 died.
Between 20 and 30	56, “ 11 died.
“ 30 and 40	28, “ 10 died.
“ 40 and 50	18, “ 5 died.
“ 50 and 60	7, “ 1 died.
“ 60 and 70	4, “ 1 died.
Over 70	2, “ 0 died.

741

32 “

We observe that Dr. Hayward renders his frank tribute to conservative surgery by the concession that “*amputation is often performed, when it might have been avoided,*” and adds: “It is difficult in many cases to decide on the best course; but the operation should not be done *without the clearest evidence of its necessity*, for it is a hazardous and painful one, and, even when perfectly successful, leaves the patient in a mutilated state.” He expresses the fear, that amputation is often resorted to earlier than it should be, particularly in recent injuries; and thus accounts for its greater fatality, and rebukes hasty surgery. These sentiments do equal honor to his head and heart.—*N. Y. Med. Jour.*

TAPE-WORM.—[As the new Anthelmintic Koussou is now being tested extensively in Europe, our readers will be gratified with the following clinical lecture on the subject. Our correspondent, who, in view of the enormous price of this remedy, wishes attention directed to the cheap and efficacious substitute found in Sp. Terebinth, will perceive that the new agent succeeds after the turpentine has failed. Whether a perseverance in the latter remedy might not have been completely successful, may be highly problematical.]—*New-York Medical Gazette.*

Report of the Cases Treated by Koussou, in King's College Hospital. By Dr. BUDD.

GENTLEMEN,—We have lately been employing in the hospital, a new remedy for tape-worm, the "Koussou." Our stock of it is now exhausted, and it is right that you should know the amount of evidence that our experience has furnished in its favor. "Koussou," otherwise called "Brayera Anthelmintica," from Dr. Brayer, who first made its virtues known in Europe, is a tree, growing in Abyssinia, which has been classed among the *Rosaceæ*, and which is said to attain the height of an oak, and to bear large bunches of very small flowers, varying from a pale green to a rose color. The flowers, which are the medicinal part of the plant, are used by the natives of Abyssinia as a remedy for tape-worm, which is very common among them. The medicine comes to us a brownish powder, looking very like jalap, smelling very like scammony, and slightly bitter and somewhat nauseous to the taste. It can, I believe, be procured at present only from a single druggist—M. Boggio, pharmacien, 13, Rue Neuve des Petits Champs, Paris—who charges the enormous price of forty francs, that is, thirty-five shillings, for each dose of it, which weighs four drachms and a half, and is contained in a well-stopped bottle.*

Some time ago I obtained four doses of it through the kindness of my friend, Dr. de Mussy. I let Dr. Marshall Hall have one of these, and gave the other three to patients who came to the hospital with tape-worm. The medicine having proved successful in each of these three cases, six more doses of it were got from Paris, on account of the hospital. Three of these have been used by myself; the rest by my colleague, Dr. Todd. Nine doses of the medicine have been given by us, and not one of them has failed to kill and expel the worm.

The medicine has been given in the morning, before breakfast, which is the best time for administering all remedies for tape-worm, as the small intestine which the worm inhabits, is then more empty than at other times; the worm, consequently, less likely to be sheathed and protected from the action of the drug by the other contents of the bowel.

The powder has been infused for ten minutes in three-fourths of a pint of hot water. The infusion has then been stirred, and the whole drank.

One of the patients under the care of Dr. Todd, a woman advanced in pregnancy, vomited about half of the dose; but what remained in the stomach destroyed the worm. Two of the other patients had a slight feeling of nausea for ten minutes or a quarter of an hour after taking the

* Mr. Hooper, Druggist, 7. Pall-mall East, has obtained some of the Koussou from Paris, which he sells to hospitals for 16s. and to the public for 20s. a dose.—M. Delluc, Broadway, New-York, has a supply, which he sells at \$10 the dose.

medicine. Another was several times purged by it. Another, the woman to whom we first gave it, had headache after taking it, and ascribed it to a diuretic effect. The rest felt no uneasy sensations whatever from the medicine.

In the cases that fell under my own care, I ordered the patients to live sparingly, and to take a seidlitz powder, or a dose of castor oil, the day before taking the Kouso, for the sake of emptying the bowels, and so leaving the worm more exposed. This is a precaution which it is well to take before administering any of the remedies for tape-worm. They all act directly on the worm, and to take effect must, of course, be brought in contact with it.

I also ordered the patients to take a seidlitz powder, or a dose of castor oil, after the Kouso, to carry the medicine down to the worm, and to expel the dead or enfeebled worm from the bowels. This expedient also is equally applicable to other remedies for tape-worm, and especially to turpentine, which, if it remains long in the stomach, or passes slowly through the bowels, gets absorbed and irritates the kidneys and bladder, producing strangury and bloody urine, and may not reach the worm in quantity sufficient to kill it.

Tape-worms are very tenacious of life. They are seldom voided entire without the aid of medicines that act especially upon them. Single joints often come away, and pieces two or three feet long are often then voided, but it very seldom happens, unless after medicine, that a portion comprising the head is thus passed. This portion remains and grows again, so that a person is often plagued with the parasite for years. One of our patients, Sarah Wheeler, had been so plagued for sixteen years, during which she told us that she had seldom gone a week without passing joints of the worm. She had taken turpentine, and the bark of the pomegranate root; and some years ago, when she was at Fort Beaufort, at the Cape of Good Hope, she took the root of a plant used by the natives as a remedy for tape-worm, which, she says, is called there "Cacay." She describes this root as being round, and when scraped, white like a turnip, and sweet to the taste. This medicine had very little effect. The turpentine and the pomegranate brought away large pieces of the worm, but the head and the portion near it remained, and the worm grew again. The worm was expelled after the kouso in different portions, on the 11th and 12th of April, and she has since had none of the symptoms which she attributed to it. She is still (May 31st) in the hospital, for prolapsus of the uterus, which she has long had, but she feels convinced that the worm is entirely destroyed.

Another case that shows more strikingly still the tenacity of life of the tape-worm, is that of Samuel Payne. He first passed joints of the worm seven years ago. In September last, he was brought into the hospital with cholera, and on the day of his admission, passed a portion of the worm two yards in length. He remained in the hospital three weeks, on account of the cholera. Some time after this he again passed joints of the worm, and to get rid of it, came to the hospital as an out-patient. Turpentine and castor oil were given him, and brought away a long piece of the worm. The ailments which he attributed to the worm were much relieved for a time. They then became again more severe; joints of the worm were again passed, and in January last he came for the second time to the hospital to be rid of the worm. Turpentine and castor oil were given him, as before-

a long piece of the worm was again expelled, but the creature was not destroyed; so that here the worm had existed seven years, had kept its place during the terrible commotion and flooding of the intestines in cholera, and had escaped destruction by two doses of turpentine. Payne took the kousoo on the 3d of May, and the next morning voided the worm, which was ten yards long.

In all the cases in which the kousoo had been given in the hospital, the head of the worm, or the taper portion near the head, has been found, so that there is reason to suppose that the creature has been entirely destroyed. The action of the medicine is very speedy. In one of the cases, in which the bowels were slow to move, the worm was expelled, in different portions, in the two days succeeding that in which the kousoo was taken; in all the other cases it was expelled the same day, and in several of them after the lapse of only three or four hours.

The result of our experience then, is, that the kousoo is a very effectual remedy for the tape-worm, and that in its action it is both speedy and safe.

I have had little experience of the male fern. But the kousoo is certainly much more efficacious than the pomegranate; and much more efficacious, as well as less disagreeable, and safer, than turpentine. There is a considerable demand for it at the hospital, which, on account of the cost, we cannot satisfy. I trust, however, now that its efficacy is known in this country, which has commercial relations with every part of the world, that a fresh supply will soon be brought here, and that we shall have it at a reasonable rate.

From one of our patients two worms, one very large, and the other much smaller, were expelled by the kousoo. It sometimes happens that as many as three or four of these worms are found together; but such an event is very uncommon. The tape-worm requires no mate, and generally leads a solitary life; and, hence, has been called by the French *ver solitaire*, the solitary worm.

In the cases which fell under my own care at the hospital, notes were taken of the ailments which the patients attributed to the worm. Most of these persons had some unpleasant sensations referred to the stomach and bowels, such as a feeling of sickness or nausea, especially on first getting up in the morning; pain, or a sense of sinking at the epigastrium; flatulence; a feeling at times as if the worm were moving in the bowel. It is worthy of remark, that in none of them was there diarrhoea. The appetite was very variable—in some, much impaired, in others, craving.

Two or three of the patients had a troublesome cough, and one, annoying palpitation; which were ascribed to the worm. Some of them complained also of unpleasant sensations in the head; giddiness or drowsiness felt during the day, and especially after meals; and almost all complained, more or less, of weakness, and lassitude, and incapacity for work.

The most constant of the symptoms were a feeling of lassitude, a sense of nausea on first getting up in the morning, and drowsiness felt during the day. But symptoms such as these, are of very little use in diagnosis. Persons who have tape-worm generally discover the fact by passing of joints. The worm seems to grow rapidly; and the tail-joints, as they attain their full development, drop off, and are voided with the other contents of the bowel. Probably no person suffers very long from tape-

worm without voiding some of these joints. The existence of the worm cannot be safely inferred, and the remedies for it cannot be given with a fair chance of success, unless joints have been passed. It is a common belief that while joints are dropping, the worm is feeble, and that the medicines which kill it act more surely soon after joints have been passed.—*London Lancet*.

Aphtha. By J. YALE WARE, MASS.—The following simple prescription has proved a specific in my hands in many hundred cases of aphtha. I learnt it of Dr. Eli Ives, of New Haven. R. Ipecac. gr. vi; Tinct. Opii; Es. Meneth. pip. aa. gtt. iv; boiling water xxvi tea-spoonfuls. Sweeten with loaf sugar. Dose, a tea-spoonful every two hours. At the same time apply to the tongue equal parts of a powder of borax and loaf sugar, which the child will carry over the mouth. I have never known the above to fail in any case of infant's sore mouth. It generally cures in two days. Occasionally, in delicate subjects, the disease returns again, when the remedy needs repeating.—*Amer. Jour. Med. Sciences*.

EDITORIAL DEPARTMENT.

TO THE MEDICAL PROFESSION.

The undersigned, Chairman of the Standing Committee on *Practical Medicine*, appointed by the American Medical Association, May, 1850, respectfully solicits the co-operation of members of the Medical Profession in furnishing materials for the Annual Report in May, 1851. The duty of this Committee, as defined by the Constitution of the Association, is to "prepare an Annual Report on the more important improvements effected in this country in the management of individual diseases; and on the progress of Epidemics; referring, as occasion requires, to medical topography, and to the character of prevailing diseases in special localities, or in the United States generally, during the term of their service." In order to fulfill the objects thus expressed, the requisite data must be supplied by medical practitioners in different sections of the Union. This is more particularly true with reference to the "*progress of epidemics*" and "*the character of prevailing diseases in special localities.*" Communications, therefore, are particularly desired from persons residing in places in which Epidemics have prevailed, or in which prevailing diseases have been marked by special characters during the present year. Epidemic Cholera and Dysentery are known to have prevailed more or less extensively in different parts of the country during the past summer. Facts bearing upon the

features peculiar to the present season, the production, diffusion, mortality, treatment, etc., of these diseases, will be acceptable. It is requested that Communications upon these or any of the subjects coming under the cognizance of the Committee, be transmitted to the undersigned by the first of March, 1851.

All contributions with which the Committee may be favored, will receive due attention and acknowledgment.

AUSTIN FLINT.

BUFFALO, New York, Nov., 1850.

Editors of Medical Journals will confer a favor by copying the above.

A brief History of an Existing Controversy on the Subject of Assimilated Rank in the Navy of the United States. By W. S. W. R. Philadelphia. 8 vo., pp. 108.

Our readers are probably aware that an effort is being made by the medical officers of the navy to obtain an *assimilated rank* in the service. The force of this expression may not be fully understood by all. We select, therefore, in explanation, from the work, the title page of which prefixes this article, the following extract:—

Assimilated rank is the relative position of members of staff corps to officers of the line, and to officers of different staff-corps.

As authority to command is not inherent to rank of any description but originates exclusively from the instructions or orders of the Commander-in-chief of the army, that is, the President, assimilated rank cannot confer a right to command in the line. A colonel of engineers cannot command a major in the line, nor a medical officer, who may be a major or captain in assimilated rank.

Consuls enjoy assimilated rank as captains in the navy; but the possession of this kind of rank does not confer upon them any right to command junior captains, commanders or lieutenants; nor have those officers in the navy any right to command a consul. But it serves to regulate points of etiquette, precedence, &c., on occasions of ceremony between consuls and officers of the navy on foreign stations.

The effect of assimilated rank may require further illustration. A captain in the line commands a company, every member of which is bound to obey his orders; without such obedience, the functions of the office of captain in the line could not be performed. An assistant-surgeon, who has assimilated rank as a captain, has no authority in virtue of his assimilated rank to command in the line: he cannot act as captain and direct the duties of the company under any circumstances whatever; not being of the military profession, technically speaking, not in the line of succession in the grade of captains by the rule of seniority, or any other rule, he can never occupy a position to discharge the official duties of a captain. The right

of the medical officer to command is limited to the immediate control and direction of those of the company who are in need of medical attention, and such persons are bound to obey him; and he is properly responsible for all that relates to the care and management of the sick and wounded of the company, as well as the attendants upon them. From the nature of the case, he must have this degree of authority in order to discharge the duties of medical officer, and therefore his official commission confers it upon him. His assimilated rank adds nothing to the measure of his authority, which he derives exclusively from his commission. But his assimilated rank, as a captain, entitles him, in his official and social relations with members of the military community, to receive the same signs of respect, which are conventional, as a captain in the line; and on occasions of ceremony, such as military reviews, parades, marches, funerals, &c., and also, when serving as a member of a military council, or as a member of a board of survey, or a military court, his assimilated rank entitles him to take a place among those of the grade of captains, which is determined by the dates of their several commissions as captains, the earliest date taking precedence. Assimilated rank, no matter how high a grade it may attain, does not confer authority to command those possessing *lineal* rank in any grade, not even in the lowest; but those of an *inferior lineal* rank have no authority to command those possessed of *assimilated* rank of a higher grade; that is, cadets, second lieutenants, first lieutenants of the line cannot rightfully command or take precedence of those who have assimilated rank as captains. Nor can those who have *lineal* rank as captains take precedence and command of those whose *assimilated* rank as captains is of older date, especially when in the presence of a common superior—a major of the line, for example. Hence it is that assimilated rank is a conservative, or protective rank; without it, the officers of the medical, or any staff department of an army, might be made subordinate even to privates, and find no redress in law for such indignity.

It has long been a subject of complaint with naval medical officers that, not possessing an assimilated rank, their position has been imperfectly defined, and, in consequence, the rights and privileges due to the corps are not always accorded. The end sought for is, that Congress shall take such action in the matter as shall leave no room for doubt or cavil on the part of secretaries, commanders, and subordinate officers. For this end petitions have been transmitted, not only from the medical officers of the navy, but from the National Medical Association, and numerous local societies in different parts of the Union. The nature of the application is such, that it concerns, not alone the medical branch of the navy, but the whole profession of the United States. A similar source of complaint heretofore existed in the army, but, through the persevering exertions of the present Surgeon General, an assimilated rank has at length been obtained. It is to be hoped that the efforts of our brethren in the navy will be equally successful.

The whole subject is fully discussed in the work referred to, the initials being of those of Dr. Ruschenberger, Surgeon, U. S. Navy—a name well known for many valuable contributions to medicine and natural science. The courteous tone which pervades the discussion by Dr. R., denotes the true gentleman.

Memorial of Dr. John Bell, and Rejoinder of Prof. S. Jackson.—Some weeks since we were favored by a Memorial addressed to the Trustees of the University of Pennsylvania, by Dr. John Bell. It appeared from the Memorial that Dr. B. had been an unsuccessful candidate for the vacancy in the Medical Department of the University occasioned by the resignation of Prof. Chapman. It was evidently written under a poignant sense of injustice for which the author held certain members of the Society responsible, attributing to them the exertion of undue influence, in opposition to his appointment, with the Board of Trustees. The publication of the Memorial must have been deemed unfortunate by the friends of Dr. B., more especially as he did not confine himself to a rehearsal of his supposed wrongs, but endeavored to retaliate by personal attacks with that most dangerous of all weapons, *satire*—a two-edged sword which is apt to wound those who wield it. Whether Dr. B. has just occasion for feeling aggrieved, we do not presume to judge. In the choice of a Professor, the Managers of the University of Pennsylvania would be expected to select from the list of candidates, the one whose labors as a teacher, would, in their estimation, be most likely to prove successful. The interests of the School and of the Faculty, of course, dictate this policy, and not less, also, the interests of Medical Education. If, in pursuing this line of conduct, the result conflicted with the supposed personal claims of any individual, whatever chagrin that individual may have felt, it would certainly have been wiser and in better taste, to have brooded over it in silence, than to seek to enlist the public in his private feelings of disappointment and resentment. We are constrained to say that our sentiments of great respect for Dr. Bell, led us to regret deeply that some judicious confidential adviser had not stepped between him and the printer, and placed his veto on the publication of a document which, in the sober moments of the author, will be deprecated by himself as sincerely as by those who esteem him most.

Within a few days we have received a rejoinder to the Memorial of Dr. B., with the initials of the name of Prof. Samuel Jackson, appended. The latter retorts in the mode of warfare which had been unwarily provoked,

and few writers have the art of directing satire and ridicule with greater effect. His pen is a paichan of the first magnitude. But Dr. Johnson would perhaps have said of this effort, as of one of the productions of Goldsmith, "It is a foolish thing, exceedingly well done." Personal quarrels are scarcely less defensible when carried on by the artifices of rhetoric, than when the combatants resort to the bowie knife and pistol, and the latter has the advantage of settling disputes more speedily.

Respecting, as we do, in common with the medical profession, both the distinguished Professors who have been betrayed by excited feelings into an attitude toward each other of intellectual pugilism, we concur most heartily in the wish expressed by our contemporary, the New-York Medical Gazette, that the "old warriors will bury the hatchet and smoke the calumet of peace."

Ship Fever, so called; its History, Nature, and best Treatment. The Fiske Fund Prize Dissertation for 1849. By HENRY GRAFTON CLARK, M. D. Printed by order of the Rhode Island Medical Society. Boston: Ticknor, Reed & Fields. 8vo., pp. 48.

This, as the title page imports, is a publication of the Essay which gained the Fiske Fund Prize for 1849. The author, from his position as Port Physician of the city of Boston, has enjoyed ample opportunity for studying the disease of which the essay treats, having had, as he states, personal familiarity with nearly two thousand cases. Cases of Ship Fever, so called, in so far as the observations of Dr. Clark have extended, are cases of *Typhus* as distinguished from *Typhoid*. He has found no difficulty in practically making the distinction between these two forms of fever, adopting as the basis of discrimination, the diagnostic points laid down in the Treatise on Fever by Dr. Bartlett.

Dr. Clark is of opinion that Typhus "is very clearly different from any fever which has hitherto been known in this country"—an opinion, probably, much too broadly and positively expressed. He regards it as "an entirely distinct disease" from the Typhoid fevers of France and New England. The same criticism, in our opinion, is applicable to the latter opinion. The conclusion adopted in view of the autopsical examinations made by the author is, that "Peyers glands are usually unaffected in any form of ship fever." The records of the appearances of the intestine in three cases are given as illustrations. It would have been more satisfactory if the observations with reference to this point, in a larger number of

cases, had been submitted; or even if the author had stated the number of dissections upon which the above conclusion is predicated.

Two-thirds of the mortality of the cases falling under the 's observation have been owing to secondary affections, which may be classed under two forms, viz.:

1. General dropsy, which is often accompanied by swelling and sloughing, or suppuration of the parotid and other glands, and occasionally by suffocative œdema of the glottis.

2. A diarrhœa, or dysentery, which is usually dependent upon inflammation of the ileum and colon.

The number of instances in which the affection became developed among those brought into contact with the sick, sufficed to show the contagiousness of the disease. Facts also were observed which appeared to establish that the disease is infectious as well as contagious. With proper attention to cleanliness and ventilation, however, its communicability, in either mode, may be restricted within very narrow limits.

The remarks on Treatment are judicious, but contain nothing new.

We have thus given a condensed summary of the several points which the investigations of Dr. C. tend to establish. We have read the essay with interest, but we could not forbear wishing that, in connection with conclusions, more of the data had been furnished.

Report of Committee on the Poor Department of Erie County, N. Y.—
By the published proceedings of the County Supervisors at their late sitting, we observe that the Report of the Superintendents of the Poor shows an improvement in the rate of mortality at the Poor-house during the last year. The number relieved was 1,540, and the number of deaths 155, making a ratio of a fraction over *one* death in *ten* of the whole number received. We take pleasure in chronicling this improvement, which is doubtless owing to some increase in the accommodations, together with more careful and skillful attention to hygienic provisions. There is still, however, abundant need for further amelioration of the condition of the sick at the poor-house. A stirring Report on this subject, and on the evils of the present pauper system, was made by the Committee on the Poor Department (Dr. Baker, of Colden, being a member of the Committee,) and the Board of Supervisors adopted a resolution to purchase a farm and erect buildings more extensive and appropriate than those now occupied.

The mortality among those relieved at their homes in the city, continues

to be truly appalling, being, for the past year, 518. We have no data for determining the number of sick persons among this class of paupers. Assuming the ratio of deaths to be the same as at the poor-house, the number of cases calling for medical treatment during the year, is *five thousand one hundred and eighty!*

Practical Illustration of the Concours in France.—Dr. Warren, in his published Address, gives the following account of the mode in which Dupuytren achieved success at the *Concours* for the place of Chief Surgeon of the Hotel Dieu:—"In the evening after the *Concours*, Dupuytren visited the house of a friend of his, of high political importance, and, rushing into his room, struck his hands upon his head, exclaiming, "I am lost, Roux will triumph!" His friend endeavored to pacify him; inquired into the circumstances which had caused his discouragement, and discovered that his depression was produced by the apprehension that the supporters of M. Roux were more powerful than his own. His friend then told him, "Obey me, and you shall still be victorious. Go this moment to Madame B.; her influence is all-powerful. She has a good opinion of you, and will be flattered by your application, and gratified at mixing in a court intrigue. Throw yourself at her feet, supplicate her to exercise her power in your favor, and never leave her till she has given her promise to aid you." Dupuytren obeyed. He visited the lady, obtained her promise, and was subsequently declared the successful candidate. This occurrence is a practical commentary on the infallible impartiality said to characterize the mode of election by *Concours*." The *Concours* has been proposed as the mode of electing Professors in the Medical Institutions of this country: could a more striking instance of favoritism be found, under our present system, than that by which the most distinguished of modern surgeons obtained his appointment!

Speculum Vaginae.—Quite a warm discussion appears to be going on in Great Britain respecting the use of the Speculum in the treatment of vaginal and uterine diseases. We have selected for the Eclectic Department of this No. of our Journal two articles on the frequency of the affections requiring the use of this instrument, one by Dr. Tyler Smith, and the other, a rejoinder, by Dr. Bennet. Dr. Bennet, as our readers need not be informed, is the author of a work on uterine affections, which has led to the

more frequent use of the Speculum than formerly with British and American Practitioners. We had supposed it was generally conceded that in this particular the practice of medicine had received an important improvement, and that many females are now promptly and effectually relieved, who, under the old system of treatment without the advantage of ocular investigation, were doomed to an indefinite continuance of distressing ailments. Certain are we that we have derived great assistance from the use of the Speculum since Dr. Bennet's book appeared in this country; and we have some cases to report, when our leisure will permit, which will serve to illustrate its great value in the management of uterine diseases. An impartial reader of the discussion reported in the last No. of Ranking's Abstract, (Report on Midwifery, etc.,) can hardly repress suspicion that the ardor of opposition on the part of Drs. Robert Lee and Ashwell, both authors of works on the Diseases of Females, springs more from personal feeling toward Dr. Bennet, than from that excessive regard for delicacy which they affect. The views of Drs. Acton, Locock, and the Editor of the Report just referred to, will, we think, commend themselves to the good sense of most medical readers.

Demonstrative Midwifery.—The October number of the American Journal of Medical Sciences contains an article over the signature of C. M., purporting to be a review of the Report of the trial of the People vs. Loomis, in which the writer animadverts on the case of Demonstrative Midwifery at the Buffalo Medical College during the session of 1849-50. As a solitary instance in which that subject has been discussed, in a medical Journal, by one totally opposed to the plan proposed and exemplified by the Professor of Obstetrics, and Faculty of the Buffalo College, we have every disposition to transfer the article to the Eclectic Department of our Journal. We have not done so solely because the subject has already engrossed a considerable share of our columns of late, but upon receiving an intimation that it will be acceptable to a fair proportion of our readers, or that it is a matter of justice to the opponents of demonstrative midwifery, we will give the article an insertion in our next number. While the opinions of any who may choose to take exceptions to an expression of our views on this, or any other topic, with as much freedom and frequency as seems to us proper, will not give us much concern, provided we are unconscious of indulging in personalities or any other breach of decorum, we

desire to consult the interests and tastes of our readers so far as to avoid indulging a tedious partiality for subjects which may chance to engage our attention.

The article referred to, we understand, in this region is currently reported to be from the pen of Prof. C. D. Meigs. This, however, is assuredly an error, which it is but due to Professor M. should be corrected. The author is presumed to be Dr. Caspar Morris, of Philadelphia.

To the Editor of the Buffalo Medical Journal.

My name appeared as one of Seventeen Physicians who signed a Card which was published in the March number of your Journal, disapproving of the course pursued by the Professor of Obstetrics in a case of Demonstrative Midwifery at the Medical College in this city.

That protest styled the demonstration as "unprofessional in manner, and grossly offensive alike to morality and common decency." After hearing the evidence before a Court of Justice as to that transaction, I am satisfied that the conduct of the Professor *was not* such as deserved censure. Therefore I choose to withdraw my assent to such an imputation of his character and motives.

WILLIAM RING.

Buffalo, 1850.

Consultations with Irregular Practitioners.—The Erie (Penn.) Gazette, contains an able address to the public, by a Committee appointed by the County Medical Society, giving the reasons for a resolution adopted by the Society prohibiting consultations with irregular practitioners. The Committee consisted of Drs. Flint, Beebe, Vosburg, Miller, and Perkins. The merits of the subject are, of course, well understood by the Profession, but very imperfectly by a portion of the public; and hence the propriety of explaining the line of conduct which it appears to have been necessary for the Erie County Society to prescribe for its members.

Statistics of Homœopathy in New York.—In our last No. we published statistics of Homœopathy in Great Britain. In a late No. of the Medical Gazette we find a list of the Homœopathic practitioners in the city of New York. The whole number officially announced by the *Hahnemann Academy of Medicine* is *thirty-five!* The number of regular practitioners in the city of New York is 875. The numerical proportion of Homœopathic

practitioners to the Medical Faculty in that city, therefore, is *one-twenty-fifth*; and the proportion to the population is *one* for every 14,000! The letters of M. D. are affixed to all the names announced by the *Hahnemann Academy*, but the Editor of the Gazette surmises that in most instances the title is assumed. Empirics of all complexions are apt to testify their real estimation of the respectability of the Profession they affect to despise, by stealing its insignia.

Dr. Warren's Address before the American Medical Association.—The Address delivered by Dr. Warren at Cincinnati, at the opening of the session of the American Medical Association, has been published by John Wilson, of Boston, Mass., in beautiful style. The Address was delivered *memoriter*, and not furnished for publication with the Transactions—an omission to be regretted. The printed Address contains much that was not spoken, and, altogether, it does the author better justice in its present form, than as delivered *viva voce*.

Transactions of the American Medical Association. Vol. III.—The volume of Transactions of the American Medical Association for 1850 has been received, too late for notice in this No. beyond the announcement of its publication. Copies may be obtained on application to Messrs. Lea & Blanchard, Philadelphia. Societies which have been represented in the Association will be furnished copies for the members on the following terms:—3 copies of Vol. III, in paper covers, for \$5.; 3 copies of Vols. I, II, and III, for \$12.

Ranking's Half-Year Abstract of the Medical Sciences. No. 11. *January to July, 1850.*

Braithwaite's Retrospect of Practical Medicine and Surgery. *Part the Twenty-first.* 1850.

These popular periodicals continue to be issued punctually, and are promptly reproduced in this country. They are too well known to require aught from the journalist save the announcement of the appearance of the successive numbers.

The work by Braithwaite is issued by Daniel Adee, 107 Fulton-st., New York. Ranking's Abstract is republished by Lindsay & Blakiston, Philadelphia.

Materia Medica and Therapeutics: with Ample Illustrations of Practice in all the Departments of Medical Science, and very copious notices of Toxicology, suited to the wants of Medical Students and Practitioners. By THOMAS D. MITCHELL, A. M., M. D., Prof. of the Theory and Practice of Medicine in the Philadelphia College of Medicine, etc., etc., etc. Philadelphia: Lippincott, Grambo & Co., successors to Grigg, Elliott & Co., 14 North Fourth-street. 1850. 8 vo., pp. 738.

This volume, as the author states in his *Introduction*, contains the substance of his Lectures on *Materia Medica* and *Therapeutics* in the Medical Department of Transylvania University for eleven successive winters. It differs from other treatises on the same subjects in being divested of dry details relating to the natural, botanical and chemical history of medicinal articles. In place of these the author gives the fruits of his own reflections and experience in the use of remedies, at the same time gleaned from the observations of others from various sources, thus rendering the work eminently practical in its character. This feature will serve to commend it to the attention of the Practitioner. In so far as we may judge from a partial examination of its merits, it deserves a place in every medical library. Prof. M. adopts the alphabetical arrangement of the articles of the *materia medica*. For sale by Phinney & Co.

A Treatise on the Diseases and Physical Education of Children. By JOHN EBERLE, M. D., late Prof. of the Theory and Practice of Medicine in Transylvania University, etc., etc., etc. Fourth edition, with notes and large additions by THOMAS D. MITCHELL, A. M., M. D., Prof. of the Theory and Practice of Medicine in the Philadelphia College of Medicine, etc., etc., etc. Philadelphia: Lippincott, Grambo & Co., successors to Grigg, Elliot & Co., No. 14 North Fourth-street. 1850. 8 vo., pp. 768.

Eberle on Children not many years since was a standard work on infantile diseases. It is now, for the most part, supplanted by treatises which are more in conformity with the present stage of the progress of practical medicine. There is, however, much contained in that work which is valuable, and while it cannot be recommended as a text book, it may be read with advantage by the practitioner who is familiar with the modern literature of the diseases of children. To adapt it to the existing condition of medical science Prof. Mitchell has appended a *Sequel* of over three hundred pages. The latter has also been published separately for the convenience of those who already possess the former editions of Eberle's work. For sale by Phinney & Co.

Medical Jurisprudence. By ALFRED S. TAYLOR, F. R. S., Licentiate of the Royal College of Physicians, etc., etc., etc. Second American from the third London edition. With notes and additions by R. EGLESFELD GRIFFITH, M. D., etc. Philadelphia: Lea & Blanchard. 1850. 8 vo., pp. 670.

The first English edition of this valuable work appeared in 1844, and in the interim two large editions have been sold. The present is the second American, printed from the third English edition, the latter having been in a great measure re-written and much valuable matter added. The American Editor, the late lamented Griffith, a short time before his death added numerous notes and observations. The work thus purports to embrace all "that is important or useful in Legal Medicine, and all facts in relation to it, to the close of 1849." For sale by Phinney & Co.

Of the Causes, Nature, and Treatment of Palsy and Apoplexy: of the Forms, Seats, Complications and Morbid Relations of Paralytic and Apoplectic Diseases. By JAMES COPLAND, M. D., F. R. S., etc., etc., etc., etc., etc. Philadelphia: Lea & Blanchard. 1850. 12 mo., pp. 326.

This monograph is chiefly a re-publication of the articles on palsy and apoplexy contained in the Medical Dictionary by the author. It contains a digest of facts and opinions relating to the important diseases of which it treats. For sale by Phinney & Co.

In addition to the foregoing, several new publications have been received, which will be noticed in our next number.

New Medical Journals.—The following two new Medical periodicals have recently been commenced:—

The Western Medico-Chirurgical Journal; Edited by I. F. Sanford, M. D., and Samuel G. Armor, M. D., Professors in the Iowa State University. This is a monthly of 32 pages. The terms are *two dollars per annum, in advance.*

The New-York Register of Medicine and Pharmacy; Edited by C. D. Griswold, M. D. Published semi-monthly, each number containing 16 pages. Terms, *one dollar per annum, in advance.*

We welcome these new candidates for favor, and tender to the Editors our best wishes.

Dr. Mott.—Dr. Mott has returned from Europe, and accepted the appointment of Emeritus Professor of Surgery in the College of Physicians and Surgeons, in the city of New York. It is expected that he will lecture occasionally at that Institution during the lecture session now in progress.

Prof. Gross.—Prof. Gross passed through this city, recently, on his route to the city of New York, to commence his professorial labors in the Chair hitherto occupied by Dr. Mott. No one can doubt the ability of Prof. G. to sustain himself in his new position, and none who know him can wish his shadow less than it is.

Louisville Medical College.—Prof. Paul F. Eve, of Augusta, Ga., has been appointed to the Chair of Surgery, in this Institution, made vacant by the resignation of Prof. Gross. The College is fortunate in the selection. We have not learned who succeeds Prof. Eve in the Georgia Medical College.

Buffalo Medical College.—The Regular Session of this Institution commences on the 6th inst. About 70 students have been in attendance during the preliminary term.

Massachusetts Medical College.—Dr. E. N. Horsford has been appointed Professor in Chemistry in this Institution.

Trial of the Boston Apothecary.—The Boston apothecary, Wakefield, who dispensed ten grains of corrosive sublimate for calomel, has been tried for manslaughter, and acquitted. It appeared on evidence, that the poison was probably not the cause of death.

Errata in the Report on Continued Fever, by the Editor.—In stating the ratio of mortality in the hospital and private cases of Continued Fever, in the Report by the Editor of this Journal, page 136, No. for August, 1850, a material error in figures inadvertently escaped notice, which the reader is requested to correct. For the numbers 13½ and 22½ per cent., ad 18 8-19 and 28 4-7 per cent.

BUFFALO MEDICAL JOURNAL

AND

MONTHLY REVIEW.

VOL. 6.

DECEMBER, 1850.

NO. 7.

ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* BY AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

(Continued from page 346.)

SECTION EIGHTH.

Symptoms referable to the Circulation. Pulse, Capillary Congestion.

The points of interest in this section, relate, for the most part, to the pulse, and the circulation in the capillary vessels of the cutaneous surface. As respects the pulse, the question arises, how shall the facts contained in the histories be presented? In frequency, and other characters of the pulse, no two of the cases are precisely alike, and, therefore, they cannot be enumerated in classes. Moreover, in individual cases, the pulse was in no instance entirely uniform throughout the career of the disease, and frequently varied, not only daily, but at different periods of the same day. In order to exhibit, as concisely and comprehensively as practicable, the facts which the histories contain, the readiest and best method is to arrange them in a tabular form. I shall adopt this method, assigning a distinct table, respectively, to the cases of *Typhoid*, *Typhus*, and those of *doubtful type*. These tables will, in general, embrace all the facts with re-

spect to the circulation embraced in the histories. To this there are few exceptions, arising from an omission to include all the details in the preliminary analysis. In the private cases, it will be observed, that the facts are expressed in general terms, with less regard to details and precision. This arises from the symptoms in these cases not having been noted daily at the bed side, as has been before stated. In the hospital cases, usually, the frequency and character of the pulse, etc., were daily recorded, but to this rule there were occasional exceptions. Generally speaking, so soon as convalescence was declared, this particularity in recording was no longer continued. In the majority of cases the pulse, etc., as well as other classes of symptoms, were only noted once daily, generally in the forenoon. The histories, therefore, do not embrace all the variations which may have occurred during the day, except in some instances. When this is the case, in the tables which follow, the letters A. M. and P. M., are attached to the figures denoting the frequency of the pulse. When these letters are not attached, it is understood that observations were made only in the forenoon; and the numbers following each other, without these letters, express the frequency of the pulse on different days.

TABLE EXHIBITING THE PULSE, ETC., IN TWELVE CASES OF TYPHOID IN PRIVATE PRACTICE.

	1	2	3	4	5	6	7	8	9	10	11	12
In early part of disease, 80 in the morning, and at evening from 90 to 109. Larger than normal, but not full nor hard. For several days before convalescence, normal, and no exacerbations appear.	Moderate febrile movement. Details not stated.	Moderate febrile movement. Details not stated.	Moderate febrile movement. Details not stated.	On the 10th day (the 1st of observation) 100, feeble and intermittent. Became more developed, and regular, and less frequent. Two diurnal exacerbations.	Ranged from 100 to 120 in early part of febrile career. In later part from 80 to 90.	In early part, 120. Gradually abating during latter part. Moderate exacerbations in the afternoon.	Ranged from 88 to 95. Moderate exacerbations.	Ranged from 86 to 92. No exacerbations. Pulse well developed.	Ranged from 80 to 100, and rose to 120, 100. Dis-tinct evening pillary con-gestion.	At first to 100, and rose to 120, being below one day. Evening exacerbation well developed and at first large and strong. <i>Fatal case.</i>	Ranged from 80 to 100. Dis-tinct evening pillary con-gestion. <i>Fatal case.</i>	Pulse 6th day, (first of observation) 120. Rose to 130. Ca-140, and 150, and became ex-tinct. <i>Fatal case.</i>

TABLE EXHIBITING THE PULSE, ETC., IN EIGHTEEN HOSPITAL CASES OF TYPHOID.

	1	2	3	4	5	6	7	8	9
Ranged from 80 to 90. No regular exacerbations	Moderate for several days, from 88 to 96; then from 100 to 120. <i>Fatal</i>	Ranged for several days, from 88 to 96; then from 100 to 120. <i>Fatal</i>	Ranged for several days, from 88 to 96; then from 100 to 120. <i>Fatal</i>	At first, 125. Third day, 108. Fifth day, 96. Sixth day, 80. Capillary congestion confin'd to the face. Mean frequency, 102 1-4.	120. 120 small. 80. 96. 98. *note below. Mean frequency, 96 4-5.	84. 96. 96. 72. 86. 72. Mean frequency, 84 1-3.	108 well developed. Fourth day 120, and rather feeble. 106, moderately well developed. 112. 128. 108. 100. 84. Even'g exacerbations. Ca-100 swell. Mean frequency, 109 2-9.	128 well developed, and somewhat thrilling. 120 less thrilling. 120 moderately thrilling. 124 thrilling. 104 soft. 104 soft. 92 soft. 100 swell. Mean frequency, 109 1-5.	Third day 88, and somewhat thrilling. 86 soft. 96 somewhat thrilling. 104 thrilling. 84. 80. 72. Mean frequency.

* The numbers in this case and the subsequent cases, denote the frequency at the successive positions, taking no notice of some days, in some of the cases in which the frequency was not noted.

TABLE EXHIBITING THE PULSE, ETC., IN TWELVE HOSPITAL CASES OF TYPHUS.

	1	2	3	4	5	6	7	8	9	10	11	12	
Very small and irregular. Second day mostly developed. Third day and but little accelerated. 4th day, 96. 5th " 80. 6th " 80. 7th " 56. Capillary congestion of surface, somewhat livid.	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean frequency at A. M. 110 4-13.	100 A. M., 120 P. M., (well de-veloped.) 120. 120 small 120. 120 soft and compressi-ble. 130 A. M. 112. 120 A. M. 120 P. M. 106. 120 toler-ant (thrilling) 140. 120 P. M. Well de-veloped thro-ughout whole career. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118	120 soft, weak, and occasional-ly intermit-ting. 120 soft and compressi-ble. 120 soft and compressi-ble. Day before decrease 120. Capillary congestion of surface by develop-ment and re-gular. 104. 108 A. M. 108 P. M. 110 A. M. 104 P. M. 100. 104. 100. Capillary congestion of surface. Mean fre- quency, 118

TABLE EXHIBITING THE PULSE, ETC., IN EIGHT HOSPITAL CASES OF DOUBTFUL TYPE.

1	2	3	4	5	6	7	8.
104 soft and compressible. 50. 52 and somewhat irregular. Capillary congestion of surface. Mean frequency, 78 2-3.	104 rather small and feeble. 120. 108. 116. 100. 80. Capillary congestion of surface. Mean frequency, 106 2-7.	120 soft and compressible. 112 small and feeble. 96 well developed. 96. 88. Capillary congestion of face. Mean frequency, 104 2-5.	92. 84 somewhat thrilling. 100. thrilling. 100 thrilling. 108. 104. 112. 120. 120 thrilling. 112 quite small 120 small and thrilling. 46 thrilling. <i>Fatal</i> 23 days after admission. Complicated with Parotiditis. Capillary congestion of face. Mean frequency, 109 1-13.	128 full and thrilling. 120 compressible jerking. 84. 72. Capillary congestions of face. Mean frequency, 101.	120 extremely small and feeble. 108 more developed. 86. 84. 96. Mean frequency, 101 1-5.	Maximum 120. Not noted daily. Moderate capillary congestion of face and extremities.	96 well developed. 96. 96. 92. 90. Capillary congestion of face, and upper extremities Mean frequency, 94.

The foregoing tables suffice to show, in the first place, that there exists considerable diversity in the frequency, and other characters of the pulse both in different cases of Continued Fever and in different periods of the febrile career in individual cases.

It is probably never the case that the pulse remains wholly unaffected throughout the febrile career. A near approximation to this is apparently presented in the table of Typhoid hospital cases, No. 13. In this case the pulse was but 72, except on one day, when it was 96—the mean frequency being 76. This patient, however, had been confined to the bed eleven days before entering the hospital, during which time it is altogether probable that the pulse had been more or less accelerated.

The numbers given in the tables do not furnish a perfectly accurate representation of the condition of the pulse, since, generally, observations were made but once daily, and various transient causes are liable to affect the pulse at the time of the daily examination. To render this portion of the histories complete, the pulse should have been enumerated in each case several times daily, and the mean result taken, together with the variations.

The variations which in most of the cases are apparent on different days, in part, doubtless, denote fluctuations incident to the progress of the disease; but they are also, in part, to be referred to extrinsic circumstances affecting the condition of the patient, among which are to be included the influences of remedies.

On comparing the frequency of the pulse in the two types, *Typhus* and *Typhoid*, it is found that, it is considerably greater in the majority of cases of the former than of the latter. The average mean in *thirteen* of the hospital cases of *Typhoid* is 95 7-13; while in *nine* cases of *Typhus*, the average is 105 4-9. The fatal cases are excluded from this calculation. Greater frequency of the pulse, then, characterizes *Typhus*, a result which accords with the observations of others. Instances of a high maximum are oftener presented in *Typhus*. In the table of *Typhus* cases, exclusive of the cases that proved fatal, the pulse had a maximum of 140 in two cases in which recovery took place, and it was 186 in another case. In the cases of *Typhoid*, on the other hand, the highest maximum was 128, and this was attained in but a single case. Two of the fatal cases presented a greater frequency, 180.

An examination of the tables will also show that instances of feebleness, and irregularity of the pulse occurred oftener in cases of *Typhus* than in *Typhoid*.

In general terms, I should say, that the frequency of the pulse is a

pretty good criterion of the severity of the disease. This opinion, however, is not based on a careful examination of all the cases in order to compare the pulse, as regards frequency, with the grade of severity. This would require not a small degree of labor. But if this be a general rule, it is not without exceptions, for, in some of the cases in my collection, the pulse numbered 120, when, in other particulars the disease was mild, and milder than in other cases in which the frequency of the pulse was less. It did not exceed 120, the day before the decease in one of the fatal cases, and in another, in so far as data were preserved, it fell below this number. In a case which has recently fallen under my observation, not included in this collection, the pulse, two days before the fatal issue, was but slightly accelerated. There can be no doubt if the pulse exceed 120, except for a transient period, that the case is of dangerous severity; and the danger increases in more than a geometrical ratio if it rise above this point.

A deception occurred in one of the cases of *Typhus* (No. 6) which demands a passing notice. The pulse had been daily noted as "small," or "very small," and, on one day, "scarcely appreciable." This symptom, although in other respects the disease did not appear to be of a severe grade of intensity, was regarded as denoting danger, and stimulants were directed more freely than would have been the case had not the pulse presented this character. On the ninth day, I discovered that there was a bifurcation of the radial artery, one branch pursuing the usual course, and the other winding over the radius, the former being much the smaller of the two. From the position of the bed, I had always examined the pulse in the arm in which this peculiarity existed. The same anomaly I have met with in several other instances. The neglect to discover it in this instance affecting materially the treatment, may serve to suggest the importance of looking for a similar explanation in cases in which smallness and feebleness of the pulse are marked.

It will be observed on reference to the tables, that a few cases presented a minimum in the number of the pulse considerably below the normal average. In No. 16 of the hospital *Typhoid* cases, the pulse, on one day, was noted 64. In No. 1 of the *Typhus* cases, it was, on one day, 52. In No. 1 of the cases of *doubtful type*, it was on one day 52. In all these cases this reduction in the number of the pulse occurred at the commencement of convalescence, and was of transient duration. In none of these cases was there reason to suppose that the pulse, in health, was habitually slower than the average. Marked slowness of the pulse, then, occasionally characterizes convalescence in Continued Fever. Oftener the pulse con-

tinued somewhat accelerated after convalescence, by other symptoms, was fully declared. Generally speaking, a return to a normal state, as respects frequency and other characters of the pulse, is one of the best criterions of a favorable termination of the disease.

With respect to *exacerbations of fever*, in a large proportion of the cases, observations having been made but once daily, the histories are silent. In some instances, however, it is noted that exacerbations were present, and in a few instances the data suffice to show that they were absent. The only conclusion to be drawn from these limited facts is, that they may, or may not occur during the career of Continued Fever. To determine the probability of their occurrence, statistics much more extensive than the present collection of cases furnishes, are requisite.

The presence of capillary congestion of the face, extremities, or entire surface of the body is noted, in the tables, among the symptoms referable to the circulatory system. This symptom, it will be perceived, is present in a much larger proportion of the cases of *Typhus* than in those of the *Typhoid* type. The pathological significance of this symptom has already been commented on (section third.) On examination of the pulse in the cases in which this symptom is noted, in the tables, the fact already stated (page 32d) may be verified, viz., that it does not depend on causes affecting the general circulation, i. e., the circulation through the heart and arteries, since it does not sustain any relation to symptoms denoting that the forces carrying on the general circulation are compromised in a special manner. In other words, in the cases in which capillary congestion is noted, the pulse does not invariably denote an enfeebled or disordered circulation, but, on the contrary, in some of the cases in which the capillary congestion was marked, the arterial circulation was less disturbed, as indicated by the pulse, than in other cases in which that symptom was not present. The occasion of the capillary congestion is, therefore, obviously inherent in the capillary system, and, perhaps, in the blood itself. In the remarks on this point in section third, it was stated that pulmonary congestions are said to be more liable to occur in *Typhus* than in the *Typhoid* type of Continued Fever. This has been confirmed by the results developed in the preceding section. A comparison of these results with the greater frequency of capillary congestion of the surface in cases of *Typhus*, favors the supposition that the same condition of the capillary vessels, or of the blood, may affect internal organs; and that the greater frequency of pneumonitis, in *Typhus*, is in this way susceptible of explanation.

SECTION NINTH.

Symptoms (Exclusive of Eruptions) referable to the Skin.

The Symptoms (exclusive of Eruptions) referable to the skin, may be distributed into those relating to *sweating and moisture*, to *dryness*, to *temperature*, to *congestive redness*, to *Erysipelas*, and to *gangrene*. The phenomena belonging to these several classes are commingled, more or less, in individual cases; and, hence, they are to be considered, not only under separate heads, but as they are presented together in combination, succession, or alternation, during the progress of fever. I will proceed, first, to inquiries respecting each of the above divisions distinctly, taking them up respectively in the order enumerated.

Sweating and moisture. In a considerable number of the cases of both types, sweating, more or less free or profuse, occurred either once, or oftener, during the febrile career. It was so noted in fourteen of *twenty-seven* cases of Typhoid, in which the histories contain information on this point. Of these *fourteen* cases, *seven* were in *private* practice, and *seven* were in *hospital*. These cases are exclusive of the instances in which sweating occurred either coincident with, or shortly before convalescence, or as a precursor of death. In a certain number of cases the termination of the disease, either fatally, or in convalescence, was characterized by this event, but as occurring under these circumstances it will be more appropriately considered in a subsequent section. In *eight* of the *fourteen* cases, sweating occurred more than once during the febrile career; in *seven* of the *eight* cases it occurred several times, and in the remaining case twice. The periods of the disease at which it occurred, were varied; in fact, among these fourteen cases, each day of the febrile career would probably be represented. Perspiration in a lesser degree, constituting what was distinguished in the histories as *moisture* of the skin, occurred, in *ten* of the *twenty-seven* cases of Typhoid, viz.: in *four* of the cases in *private* practice, and in *six* of those in *hospital*. In these cases the skin was either moist almost constantly during the febrile career, or, generally, the moisture was present more or less frequently for hours, or days, in the course of the disease. It will be perceived, by the above enumerations, that sweating occurred in a larger proportion of the cases of *Typhoid*, than merely moisture of the skin—a curious fact, worthy of passing notice with a view to its possibly being found to possess some significance in connection with other facts.

Sweating occurred in a proportion of the *Typhoid* cases in *private* prac-

tice larger than in those in *hospital*, being present in seven-ninths of the former, and in only four-ninths of the latter group. As this disparity is not susceptible of explanation by reference to difference in circumstances in the two groups, it probably shows that, in the proportion of instances in which this symptom is present in Typhoid Fever different collections of cases should not be expected to exhibit uniformity.

Of the *twelve* hospital cases of *Typhus*, sweating was noted in the histories of *five*. It occurred several times in *one* of these cases; once only, in *one* case; and *twice* in *two* cases. *Moisture* of the skin was noted in but *two* cases. Thus, as regards sweating, results do not show a marked difference, in the two types, in the frequency of its occurrence; but simply *moisture* of the skin was less frequently observed in *Typhus* than in *Typhoid*, occurring in the ratio of 1 to 6 in the former, and 1 to 3 in the latter.

Of the *eight* cases of *doubtful type* sweating was noted in *four*. *Moisture* is noted in a single case; and in one case the history is deficient in information respecting the symptoms referable to the skin.

Adding together the cases of the three groups above mentioned in which the condition of the skin was noted, the sum total is 46, and of this number sweating occurred in 23, i. e., in precisely one-half.

The sweating was not in all cases equal in degree; pains, however, were not taken to determine differences in this particular with exactness, which, it is obvious, would be difficult, if not impracticable. The terms *free*, *profuse*, *copious*, and *bathed in sweat*, were frequently employed in the histories to express the quantity of secreted fluid present on the surface, but in other histories it is simply stated that sweating had occurred, or was observed at the time of the examination. In *one* case of typhus it is stated that the hands were corrugated from the profuseness of perspiration. This has been styled the *washer-woman's sweat*, and has been thought to be a fatal sign. In the case in which it was noted the disease ended favorably. In *one* case of Typhus, in which sweating occurred twice; on the first occasion it was quite profuse, and limited to the forehead and face; on the second occasion, it was copious on the face, and extended, but in a less degree, to the chest and upper extremities. As a general remark the sweating during the career of the Fever was not attended, or immediately followed by any marked abatement of the severity of the disease, but, on the contrary, judged by the symptoms, the effect was the reverse. This may be enumerated as the rule, and I am not aware that there were any exceptions to it. No exceptions were noted in the preliminary analysis of

the cases, and in several instances it is stated that this event was not connected with any improvement in the symptoms.

The foregoing results, it is believed, conflict with the ideas generally entertained by practitioners, respecting the frequency with which sweating occurs in Continued Fever, and the degree and the kind of importance to be attached to it, illustrate the influence of theoretical or preconceived notions in frustrating the lessons of experience, unless that experience be based on observations recorded and analyzed. It is a current belief that sweating generally proves salutary when it occurs in Continued Fever—a belief originating, probably, in the ancient humoral hypothesis of a specific morbid material being eliminated through the cutaneous secretions; and apparently sustained by the instances in which this symptom is regarded as critical from its occurring concurrently with convalescence. Its correctness, however, is disproved by the facts which numerical investigations disclose, showing the large number of cases in which the event occurs, and, at different periods of the disease, without affecting, apparently, in a favorable manner, its character or progress; showing, moreover, as will be seen hereafter, that it occurs during the febrile career, without improvement, in a larger proportion of cases, than that in which it signalizes convalescence. If this view of the subject be correct, the propriety of pursuing measures, during the career of Continued Fever, to produce sweating is more than questionable.

It would be interesting to ascertain upon what ulterior conditions, or circumstances, appertaining to Continued Fever, sweating is dependent. With reference to this point of inquiry, it would be important to establish any relations subsisting between this event, and other phenomena belonging to the natural history of the disease. I have examined the histories of several of the cases characterized by sweating, in order to discover a clue to some connection of this kind, but without success. It occurs, not only at different periods of the disease, but apparently irrespective of the circulation, the temperature of the skin, and other symptoms. The antecedent morbid condition, or conditions, upon which it depends, and the circumstances involved in its production, are unknown. All that can be said of its causation is, that it is an event incident to the progress of Continued Fever, belonging in the category with other incidental events, such as acceleration of the pulse, coating of the tongue, somnolency, etc.—all of which are not more rationally explicable than it.

This fact should be mentioned, viz: in the majority of the instances in which sweating occurred, it took place during the night, or, commenced during

the night, and was continued into the day. It is not noted in any of the histories that it was preceded by exacerbation of fever, but this may have been true of some cases without having been observed, the examination of the patients, and records of symptoms, being made, for the most part, in the morning, and the occurrence of exacerbations, therefore, not always ascertained.

It remains to inquire respecting the occurrence of sweating and moisture in the cases which ended fatally. In *nine* of the fatal cases included in the above enumerations, sweating was noted in *four*, a proportion, it will be observed, not far from that in which it is found in the cases terminating in recovery. Hence, this event has no particular bearing on the prognosis. Of these *nine* cases, *moisture* was noted in *three*, which is also not very far from the proportion which it bears in the cases which recovered. It is to be borne in mind, that in these enumerations, as well as in those which have preceded, sweating and moisture at, or toward the close of the career of the disease, are not included.

Dryness. The skin, if perfectly normal, is neither moist nor dry, but presents a medium state, conveying to the touch a sensation which is best described by applying to it, figuratively, the term *mellow*. It has been seen, that in Continued Fever, deviations from this condition are apt to occur in consequence of increased secretion from the sudoriferous glands, causing moisture, or sweating.

Deviations of an opposite kind may occur, in consequence of diminished secretion from these glands. The surface then communicates to the touch a sensation of dryness. Dryness of the skin, more or less, occurs in the great majority of the cases of Continued Fever. Of the *thirty* cases of *Typhoid*, I find but *four* in which it appears from the histories that this condition of the skin was entirely absent during the febrile career. This was true of but *one* of the *twelve* hospital cases of *Typhus*. In several of the cases, however, the records do not afford positive information on this point—viz: in *ten* cases of *Typhoid*, and *one* case of *Typhus*. This defect in the histories of these cases probably was owing to not always appreciating the importance of noting the presence of a symptom so familiar and almost universal as this. Had the skin remained moist, or mellow, throughout the disease, in any of the cases, I am quite sure the fact would have been embraced in the histories.

The occurrence of dryness was very variable, as respects the period or periods of the disease when it was observed; its duration; and also its relation to other conditions of the external surface. In *five* cases, it ap-

peared early, and persisted through the greater part of the febrile career. In other cases, it was not observed until the disease had made more or less progress ; and, generally, it was present at intervals, alternating with other cutaneous symptoms. It was so irregular in these particulars, as also in its degree of intensity, that it would be difficult to condense, by means of classification, the facts contained in the histories of all the cases. The brief and very general account contained in the foregoing paragraph, will suffice for the historical portion of this division of the symptoms referable to the skin.

Dryness of the surface would seem to be a more *consistent* event in continued fever than moisture or sweating, since observations show that all the glandular secretions of the organism are diminished in this disease. It is, however, as little susceptible of rational explanation as the opposite conditions already considered. Like most of the phenomenal manifestations of fever, it is so connected with the primary, essential, morbid changes, in which the disease consists, that while these remain undiscovered, we can hardly hope to penetrate completely its causation. We can only study it in its various relations.

As a symptom, it obtains so invariably in continued fever of both types, and also in other affections, that it is of small or no value in diagnosis, and has very little pathological significance.

Temperature. Heat of the skin existed, more or less, in most of the cases. In a few instances, however, the temperature did not appear to be increased beyond what belongs to a state of health. The heat was associated with moisture, and, more rarely, with sweating ; but generally, dryness co-existed. In some cases the heat continued pretty steadily throughout the disease, and in other cases it occurred at intervals, and was of variable duration. That peculiar pungent or acrid heat called *Calor Mordicans*, which is regarded by some writers as characteristic of the *Typhus* type, was noted in but *two* cases, one of the *Typhus*, and the other of *doubtful* type.

The increase of temperature was of different gradations, from a slight elevation above the natural warmth, to a degree to which, in a hyperbolic sense, the term *burning* might be applied. Coldness of the surface was not noted in a single case.

Of temperature, as of dryness, it may be said, and for similar reasons, that the conditions of the skin in this respect are of little account in diagnosis, and have no special pathological significance.

When associated with dryness, heat may in part be explained by the di-

minished evaporation. It is not, however, wholly accounted for in this way, but it involves a positive increase in the disengagement of caloric, as is evidenced by the fact that it co-exists in some instances with moisture, and even sweating. To inquire concerning the rationale of its production under these circumstances, would be to travel beyond the limits of the facts before us.

As a general remark, applicable to the divisions previously considered, as well as to the subject of temperature, it may be said that in estimating the severity of the disease, and the prognosis, by the symptoms referable to the skin, no very definite or positive inferences are to be drawn from any of the deviations from the state of health, considered by themselves; but a return or approximation to normal conditions, other things being equal, must, of course, be regarded as favorable.

Gangrene. Gangrenous ulceration of the skin occurred in but a single case, which was of the *Typhus* type. It appeared on the tenth day after the admission of the patient into hospital. The parts affected were the Sacrum, and a small space over the Scapula. In the latter situation the affection was slight, and, of short continuance. Over the Sacrum a considerable eschar followed, leaving a large and troublesome ulceration, which did not heal for several weeks after convalescence was pronounced. The patient, finally, completely recovered. This case in other respects did not exhibit unusual severity.

In a very few instances (three or four, as near as can be determined by recollection) some erythema over the Sacrum, or nates, was observed which disappeared on the application of astringent washes, and the use of air cushions, without proceeding further.

It has been noticed that in this disease, blistered surfaces are apt to degenerate into troublesome ulcerations. The opportunities afforded in the present collection of cases for verifying this point were extremely limited, vesication very seldom entering into the treatment. In the few instances in which blisters were applied, no such result followed.

Erysipelas did not occur in any case during the febrile career. It became developed in one case, succeeding Parotitis, after convalescence was established. It was situated on the face, and was treated by the application of ice to the part affected. It was of short duration, and did not attain a great degree of severity.

Having thus considered, under distinct heads, the several divisions of the symptoms referable to the skin, it remains to give some account of these symptoms as they were associated in individual cases. In many of

the cases all the varieties of the foregoing cutaneous phenomena were presented, either in combination, succession, or alternation, during the career of the disease. Not only were variations observed in the same case on succeeding days, but uniformity frequently did not exist at different periods of the same day. Since the laws regulating these fluctuations are but little understood, and very little pathological or practical importance belongs to them in the present state of knowledge, they are chiefly interesting as comprising a part of the natural history of the disease. The readiest method of exhibiting them as contained in the history of all the cases, would be to arrange them in a tabular form, in the mode adopted in other sections. This I have done for convenience of examination and reference in writing the foregoing remarks. It will, however, answer every purpose, and economize space, as well as the patience of the reader, to select a few cases from those of each type as illustrations, giving the symptoms referable to the skin, which were noted on each day during the career of the Fever. This plan will therefore be adopted.

Typhoid.

Case 1.—1st day, skin cool ; 2d, warm and moist ; 3d, cool, and bathed in perspiration ; 4th, warm and moist ; 5th, moist ; 6th, perspiring freely ; 7th, perspiring profusely ; 8th, warm and mellow ; 9th, warm ; 10th, warm and dry ; 11th, perspiring copiously ; 12th, warm and moist ; 13th, warm and mellow ; 14th, perspiring profusely ; 15th, warm and moist ; 16th, warm and mellow ; 17th, cool and moist ; 18th, cool and moist.

Case 2.—1st day, skin hot and dry ; 2d, dry and warm ; 3d, warm ; 4th, warm and mellow ; 5th, warm and dry ; 6th, warm and mellow. Deep congestive redness of face and extremities.

Case 3.—1st day, skin rather hot, perspired at night ; 2d, warm and mellow ; 3d, hot and dry ; 4th, warm and moist, and at night profuse perspiration ; 5th, rather hot and dry ; 6th, perspired at a. m., and profusely at p. m. ; 7th, hot and bathed in perspiration, dry before decease, this case proving fatal.

Case 4.—1st day, skin hot and dry ; 2d, warm and moist ; 3d, hot and dry ; 4th, cool ; 5th, warm and mellow ; 6th, warm ; 7th, warm and dry ; 8th, moist ; 9th, warm and dry ; 10th, warm ; 11th, warm and dry ; 12th, warm and dry ; 13th, warm ; 14th, warm and dry ; 15th, moist, 16th, warm and dry ; 17th, warm and dry ; 18th, warm and mellow. Congestive redness of the face.

Typhus.

Case 1.—1st day, skin hot, and heat acrid ; 2d, cool ; 3d, normal temperature and dry ; 4th, cool ; 5th, cool ; 6th, warm and very dry ; 7th, mellow ; 8th, cool ; 9th, copious perspiration, confined to forehead and face ; 10th, warm and dry ; 11th copious perspiration on face, extending in a moderate degree to the upper extremities, suderina, deep congestive redness, somewhat livid.

Case 2.—1st day, warm and moist ; 2d, dry, and temperature increased ; 3d, cool ; 4th, warm and mellow ; 5th, mellow, temperature somewhat increased ; 6th, cool and moist ; 7th, cool and moist. Dingy congestion of face and extremities, at first considerable, gradually diminishing, but not disappearing entirely until after convalescence was pronounced.

Case 3.—2d day, cool and dry ; 3d, noted that skin presented no change. Congestive redness. Fatal on the third day.

Case 4.—1st day, cool ; 2d, cool ; 3d, warm and dry ; 4th, warm, and at p. m., hot ; 5th, moist ; 6th, moist and cool ; 7th, warm, and at p. m., moist.

Doubtful Type.

Case 1.—1st day, hot and dry ; 2d, hot and dry ; 3d, warm and dry ; 4th, warm ; 5th, dry and rather hot ; 6th, dry and rather hot ; 7th, face covered with perspiration, and remainder of the body dry ; 8th, hot and dry ; 9th, hot and dry ; 10th, hot ; 11th, dry, but less burning heat ; 12th dry, not hot. Some congestive redness of face. This case proved fatal.

Case 2.—1st day, hot and dry ; 2d, dry, acrid heat, (calor mordicans;) 3d, warm ; 4th, moist, perspired freely during the night ; 5th, moist. Congestive redness of face. Parotiditis and erysipelas occurred, after convalescence was pronounced in this case.

The enumeration of days in the foregoing cases is dated from the time the patients came under observation.

(*To be Continued.*)

ART. II.—*Case of Ante version of the Uterus during Parturition.* By WILLIAM TREAT, M. D.

Having met with a case of this accident recently, and not calling to mind any previous reading of the liability of its occurrence, after taxing mother-wit to a successful issue, I was tempted to refer to such authority in

Obstetrics as I had near at hand, and Blundell is of the number, without finding mention made of anteversion under any circumstances, save in James Burns, wherein a brief paragraph appears. After speaking of retroversion, he adds:

“The Uterus is also sometimes anteverted, that is, the fundus is thrown forward so as to compress the neck of the bladder, and its mouth is turned to the sacrum. Of this accident I have never seen an instance during gestation, and from the nature of the case it must be very rare; but I have met with it, from enlargement of the fundus uteri, in the unimpregnated state.” He adds, in a note, that, “Baudelocque relates a case from Chopart where it, (anteversion,) was produced in the second month of pregnancy by the action of an emetic.”

The curious reader may find other cases elsewhere related, undoubtedly, but I have not leisure or inclination for the present to seek further. Believing the case I have recently had in charge possesses sufficient interest, on suggestion from others, I am induced to write a brief description for publication.

October 25-6th—At night I was called, as by previous arrangement, to see Mrs. H——, aged 28, a thick-set, short-waisted woman, about 5 feet in height, in labor with her fourth child. When making the first vaginal examination, although the pains were not very active, the progress of the labor was easily determined. After an hour's delay, in the act of making a second examination, an unusual quantity of amniotic fluid was discharged before the finger had reached the membranes. Immediately the presentation of the child was ascertained to be that of the head, with the face towards the sacrum, and with it, the os uteri was found to be dilated two or more inches in diameter. As the pains were not long protracted or of frequent occurrence, I left the bed side and reclined upon a lounge, not for sleep, but to “but to bide my time,” and, about a half an hour after, again repeated the examination,—the woman upon the left side, in bed,—when to my surprise although the woman was suffering from much parturient pain the uterus could scarcely be reached, and, when circumscribed by the finger, the mouth had disappeared as though occluded. Placing my left hand upon the abdomen for the purpose of pressing its walls in such way as to bring the uterus down and nearer to the reach of the index finger of the right hand, the difficulty was immediately ascertained; *the fundus of the uterus was deflected over the pubes at least half the length of the gravid uterus!* and the child thus thrown upon its back was at each contracting effort of the uterus not only “making haste slowly,” but with an uphill and retrograde movement!

The patient was now placed on her back, and an effort immediately made to replace the uterus by seizing the fundus during the intermission of pain, and sliding it under the abdominal parieties, upwards and forwards to a place where it was at right angles with the body of the patient. At this point the resistance, it was thought, could not safely be overcome by reason of the super-imposed viscera, resisted by their proximity to the diaphragm, the head and shoulders of the woman having been previously depressed to overcome gravity. Now it was that the anterior edge of the dilated os uteri, at each uterine contraction, could be felt, as also a portion of the vertex of the presenting head. Yet with each contractile act was the uterus, in greater or less degree, renewedly anteverted, although effort was made sometimes with one hand, oftener with both, upon the abdomen to suppress it. After some delay, finding the pains becoming more violent and frequent, and the labor making little or no progress, the bandage usually designed for after purpose was applied by passing one end under the back, and there fastened, while the other was brought round and forward over the abdomen and held by an assistant in such a manner as to make firm and continuous pressure upon the fundus uteri in a line upwards and backwards. Thereafter, with but little more difficulty, the head and body of the child performed a gradual evolution and advanced, as in natural labor, and thus relieved me, after only six hours detention, from what for a time indicated a necessity for instrumental aid. With this happy result terminated my chief interest in this, if not novel, certainly precocious exhibition of "a distinguished stranger" in the art of "grand and lofty tumbling." The nurse soon announced the weight at $9\frac{1}{2}$ lbs.— $\frac{1}{4}$ lb. less than its immediate predecessor.*

The cause of this unlooked for and unexpected position of the uterus, during labor, is doubtless attributable to the short body and by consequence great rotundity of the female,—bearing as she did, a large child with an excess of amniotic fluid,—the sudden discharge of the latter depriving the uterus of the requisite support from the abdominal muscles, now not in full tone, all of which combined to facilitate the over-turning of the matrix and its contents.

Buffalo, Nov. 6th, 1850.

*While upon the subject of midwifery, having here incidentally mentioned the weight of the child, I am tempted to believe that by way of "big babies," as in every thing else, a Yankee's word for it, in "the States" the women "beat all creation." I am led to this remark by long observation, after having read and kept in mind the statistical tables of weight made with the "greatest accuracy" at the Matron's Hospital, Paris, wherein of 7,077 cases, it is stated, only three weighed 10 pounds, eighty-two from 9 to $9\frac{1}{2}$ lbs., 463 from 8 to $8\frac{1}{2}$ lbs., and about one-half weighed only from 6 to $6\frac{1}{2}$ lbs.

ART. III.—*Cases illustrative of effusion within the Arachnoid Cavity, as a cause of Sudden Death, after Scarlatina.* By AUSTIN FLINT, M. D.

The two following cases appear to the writer to exemplify the occurrence of effusion within the cavity of the arachnoid, as the cause of sudden death. In both cases this conclusion is based solely on the phenomena exhibited before death, there having been no post mortem examinations. They are reported not as containing *evidence* of arachnoid effusion, but as *illustrations* of the points involved in the diagnosis of that event, assuming as correct the principles deduced from observations communicated for the March number (1850) of this Journal. In connection with these cases the attention of the reader is invited to the article just referred to. The first of the subjoined cases was inadvertently omitted in preparing that article. The second case has fallen under observation at a subsequent date. As an occasional, and, in a certain sense, an accidental element in the progress of different diseases, proving the determining cause of a fatal termination, the occurrence of this lesion has, in the opinion of the writer, in several instances been apparent in cases that have been observed since that article was published; and, if the views of the writer on this subject be correct, every practitioner will have no difficulty in finding illustrations. The two cases now reported, however, appear to present the occurrence of the lesion isolated in a striking manner from other obvious elements of disease, and to exemplify, in consequence, more vividly the circumstances involved in the diagnosis.

CASE 1. June 15th, 1847, I was requested, at night, to visit a patient in consultation with Dr. J. B. Pride, of this city. I found, on arriving at the house, the patient, a young girl aged 18, unconscious, with tracheal rattle, cold extremities, and evidently in *articulo mortis*. *The pulse, however, had considerable volume* (the frequency is not recorded.) *The respiration was irregular and convulsive, several respirations preceding each other, followed by a long interval.* She lived about two hours after I first saw her. Dr. Pride gave me the following history:—She has been in delicate health for some time. On the day but one preceding her death, she was attacked with Scarlatina. The disease appeared to be mild. There was moderate Pharyngitis, and the eruption was abundant on the day and evening of her death. Dr. P. had visited her at ten o'clock on that evening, and found her sitting up. She reported pretty comfortable, and anticipated feeling much better the next day. Her pulse was considerably accelerated, enumerating 120. There existed much pruritus of the skin.

Dr. P. prescribed a few grains of Dover's powder. At eleven o'clock, her mother, who had not retired, observing that shortly after conversing she became unconscious, with the eye-balls upturned, became greatly alarmed, and sent in haste for Dr. P., who resided but a few rods distant. On the arrival of Dr. P., he found her awake, declaring she was comfortable, and expressing surprise that her friends were alarmed. She was not sensible of any unpleasant change in the symptoms. Dr. P. remained to observe the patient for a time. After talking jocosely for a few moments, ridiculing the fears of her mother, she dropped asleep, and Dr. P. noticed that her respiration became heavy and stertorous. He was told, however, that this was not unusual. She awoke several times spontaneously, and remarked herself on her audible breathing. At length Dr. P. observed a slight convulsive tremor, and rolling upward of the eye balls. He immediately attempted to arouse her, and with partial success; but she speedily lapsed into a state of complete insensibility. *The respiration now became irregular and rattling as it was when I saw her*, which was about half an hour after the development of the coma. *Deglutition was impossible.* Dr. P. had resorted to the application of sheets dipped in hot water, and sinapisms, but with no benefit.

CASE 2. A child of Mr. D. S. R., between 4 and 5 years of age, had mild scarlatina, in June, 1850. It was accompanied by moderate pharyngitis, and considerable enlargement of the submaxillary glands. The child convalesced in a few days so as to be about the house, the submaxillary glands, however, remaining swelled, and my visits were discontinued. Subsequently I was called to prescribe for the enlarged glands, and I directed the Iod. Potassii. The child continued pretty well, appetite good, was out of doors, and, owing to the feeble, health of the mother, did not, perhaps, receive that degree of care, as regards exposure, etc., which otherwise would have been bestowed.

On the 7th July, the father informed me, at a casual meeting in the street, that the limbs, abdomen, and face of the child appeared bloated. Suspecting that the renal function might be at fault, I requested him to bring me a vial of the urine, which he did on the following morning. On testing the specimen with nitric acid, a copious deposit of albumen was thrown down. At evening of the same day I was requested to visit the child. He had continued to be up and about, playing out of doors; had been bright during the day, and had accompanied his parents in the afternoon, in taking a ride. The abdomen and limbs were enlarged in bulk, but the latter did not pit on pressure. The face also ~~seemed~~

swelled. *The respiration, since evening, had become much accelerated, and was now very rapid, panting.* I thought effusion into the chest might have taken place, but, on percussion, no physical evidence appeared of this as the cause of the disordered respiration. *The child did not appear to suffer from dyspnœa, although he was extremely restless.* His muscular strength was not prostrated, being able to sit up. The pulse was much accelerated, and tolerably developed. I directed a warm half bath, and a solution of the sulphate of magnesia in hourly doses, until free cathartic operation. I am free to state that I did not anticipate the sequel. The deglutition was not impaired.

In about an hour afterward, I was summoned in haste, and on my arrival at the house, I found the patient already dead.

The disordered respiration had continued unabated, but not increased, until a few moments before the messenger was despatched, when it suddenly became extreme, and death took place in less than half an hour after this change occurred. To use the father's expression, the child seemed to "*choke to death.*" *A neighbor informed me, of her own accord, without any questions, that she felt the heart beat at the precordial region, after the respiration had ceased.*

Remarks. I have italicised, in the foregoing histories, the passages which relate to points of special importance in the diagnosis. These points are disordered respiration, eventuating in apnœa, the action of the heart in the first case, persisting and well developed after the patient was evidently moribund; and, in the second case, continuing after the cessation of the function of respiration. The circumstances upon which the diagnosis is based in these two cases, then, are as follows :

In the first case, coma suddenly and unexpectedly developed, not preceded by any aberration of the intellect, nor by any cerebral symptoms, but for a short period by heavy and stertorous respiration during sleep; death by apnœa, the deglutition affected simultaneously with the fatal disorder of the respiration. This group of circumstances, as it seems to the writer, denotes that the morbid agency which proved the immediate cause of death was, in the first place, cerebral, and, in the second place, produced its fatal consequences by suspending the functions of the medulla oblongata—that portion of the cerebro-spinal centre which sustains direct relations with the movements of respiration and deglutition. The same group of phenomena has been shown to be produced by hæmorrhagic effusion into the arachnoid cavity, (see No. of Journal for March, 1850, for cases.) Effusion of serum into the same cavity, it would be fairly inferred by analogy,

would produce the same phenomena, and be followed by the same result. But the presence of serous effusion in this cavity has been proved to exist in cases in which sudden death, under similar circumstances, has taken place. (See *ibid.*)

In the second case, notably disordered respiration, suddenly developed, without coma, and deglutition preserved; the disorder of respiration suddenly increased, producing apnoea and sudden death, the impulse of the heart persisting after the cessation of respiration. An analysis of this collection of phenomena, as in the first case, leads us to the medulla oblongata as the source of the fatal apnoea.

It is to be observed, in this case, that the embarrassment in respiration, although great, was unattended by dyspnoea, or a painful consciousness of a defective performance of this function. This is a distinctive feature of disordered respiration, tending to apnoea, proceeding from compression of the medulla oblongata. The sense of the want of respiration (*besoin de respirer*) is impaired, in proportion to the diminished motive acts for carrying on the function—hence, the absence of that extreme distress attending defective hæmatisation from affections of the lungs or heart. This is a diagnostic point to be borne in mind.

In the last case, the existence of albuminaria, accompanied by the accumulation of serum in the abdomen and cellular tissue, favors the supposition of serous effusion into the arachnoid.

In both cases the fatal affection might with propriety be nosologically included under the term apoplexy; and, if the pathology of the writer be correct, they are probably instances of serous apoplexy.

The cases are interesting from the fact of the sudden death occurring in connection with scarlatina.

They are reported, with the foregoing few remarks, of necessity hastily penned, with a view to solicit the attention of pathological observers to the subject of serous effusion within the arachnoid cavity as a cause of sudden death, and as a mode in which a fatal termination occurs in various diseases.

Buffalo, November 18th, 1850.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Report of the Committee on Medical Education. Submitted at the meeting of the Am. Med. Association, May, 1850.

The Committee on Medical Education, respectfully submit the following
REPORT :

The subject of Medical Education has been thoroughly discussed in the Association, the profession, and the journals during the last three years. Three formal reports have been laid before the Association, containing a full account of the general condition of medical education in this country and abroad, representing its defects, and recommending various measures of reform. The Committee feel, therefore, that the Association is in possession of all the facts essential to a correct comprehension of American medical schools, and that they may safely omit from the present report, many of the more special subjects which have occupied the attention of their predecessors.

The records of the discussion thus far embrace, as in most cases where practical subjects have been submitted to abstract investigation, a somewhat intricate intermingling of *facts* and *opinions*.

The *facts* include a statistical history of the schools, professors, students, and graduates; their situation, quantity, kind and means of instruction; preliminary and ultimate requirements, mode of examination, and other important particulars.

The *opinions* relate chiefly to conclusions thought to be deducible from the *facts*. So far as they have assumed a definite form, they assert, first in general terms, that the system of medical education in this country is defective. Second, that it is so because the number of schools is too great, their situation (in many cases) bad, their instructors too few, their students and graduates too many, the time devoted to instruction too short, the quantity too limited, the quality too superficial, the bestowal of honors too profuse, and the mode of bestowal too unrestricted; all this involving, inevitably, a depreciation of the profession, and an impairment of the dignity, honor, and usefulness of the healing art.

The position of the Association, in relation to this subject, has been uniformly one of forbearance and conciliation. It has recommended that certain customs of the schools should be altered and amended. These recommendations refer mainly to the preliminary education of pupils, the requirements exacted from candidates for graduation, extension of the term of study, the manner in which the test for professional honors shall be applied, the mode in which teachers shall be appointed, and some incidental advice as to particular subjects of study.

The schools have been disposed to meet the efforts of the Association in a spirit of cordial co-operation. Some of them have attempted to carry

into practice its most important recommendations; and almost all have manifested a desire to contribute to the improvement of professional education. In no instance has any one of them, so far as the Committee can learn, assumed a hostile aspect towards the Association or its objects. Whatever reproaches may have been uttered against them for deficiency or delinquency, they have reason to feel proud that their relation to the Association has been, from the first, one of harmony and good will; that they have identified themselves with it, not to thwart or defeat its great designs, but to further their fulfilment; not merely for their own honor and their own interest, but for the advancement of science and the good of their fellow men.

The chief purposes of the Association in connection with the subject of medical education, have been—

- 1st. To elevate the character of preliminary preparation.
- 2d. To secure to the student, during the portion of his pupilage passed in the public schools, competent and complete instruction.
- 3d. To provide for the application of a severer test of qualification for admission into the profession.

The Committee propose, therefore, to confine themselves to a concise consideration of these three points.

1st. *Preliminary Education.*

Neither public nor private teachers of medicine can deny that many of their pupils come to them imperfectly and insufficiently prepared. Aside from the deficiencies which immature age almost necessarily implies, it cannot be gainsayed that a considerable number of medical students in this country enter upon their professional education with a limited and inadequate amount of proper preliminary training. The fact admitted, the question follows, Where is the fault, and where lies the remedy? A full solution of this question would impose upon the Committee a more difficult duty than they desire or design to fulfil. They only suggest that the blame and the remedy should be equally shared by the *profession*, the *schools*, and the *student*.

The *profession*, occupying an intermediate relation to the student and the *schools*, should abstain from offering to the student any enticement or encouragement not based upon a right knowledge of his character and capacity.

The *schools* should have the firmness to reject all importunity not sustained by real and appreciable qualification. With reasonable allowance for difference of original capacity, means, and opportunity, it should be their habit, as it is their duty and their interest, to see that the reputation and usefulness of the profession, of which they are, in some sense, the recognized guardians, receive no detriment through the admission of the incompetent and unworthy.

The *student* should be told, frankly and plainly, by those who have the best right and opportunity to do it, that nothing can compensate for lack of physical, intellectual, and moral fitness for his vocation; that his position in a profession for which he has none of the essential pre-requisites, must ever be a false one, and that he has no just claim to its "honors and immunities," without faithful, continued, industrious application. The burden is a common one, and should be equally borne. So far as the *schools* are concerned, the best boon that could be bestowed upon them, would be

to ensure to them intelligent, tractable, and ambitious pupils. No public teacher would reject such a gift. His work is attractive or repulsive, in exact proportion to the degree of mental correspondence between himself and those who listen to his instructions. To suppose that the schools do not desire the greatest amount of ability and culture in those who seek their halls, is as absurd as to suppose that the recruiting officer would prefer to enlist dwarfs, cripples, and idiots. There is not a teacher of medicine, from the St. John to the Sacramento, who would not confess that all the irksomeness of his daily duties, and all the doubts of his final decisions, are dependent upon the intrinsic and essential unfitness of the ignorant and the indolent; and that all the pleasures of a position which, however eagerly the ambitious aspirant may covet it, often involves much self-sacrifice, spring from his relations to the well-trained, the industrious, and the intelligent.

The Association has attempted to obviate this difficulty by urging upon the profession and the schools certain recommendations, which as yet have not been fully complied with. Neither of the parties most interested seems willing to assume the responsibility. The student dislikes to have his self-complacency offended; the practitioner fears to hazard the good will of the student; and the school, it is feared, is often too anxious, lest the portal of some active rival should be found of easier access than its own. Hence, with the defect generally admitted and deplored, it does not appear that hitherto much has been done towards applying any efficient remedy.

[Dr. Blatchford, of Troy, N. Y., a member of the Committee, states in a note appended to this report, that "local medical societies, in States where an organization has been effected, have, however, to a limited extent, adopted the plan of a preliminary board of examiners, a certificate from which is required of the pupil before entering the office of any one of the members as a student."]

2d. The Association has endeavored to secure to the student competent and sufficient instruction, during that portion of his pupilage which is passed in the schools.

The charge has been somewhat pertinaciously repeated that the student, after entering the schools, is often imperfectly and insufficiently taught; that there are schools badly situated, scantily officered, and meagrely equipped—unable, in short, to fulfil their promise to provide a proper course of public instruction. The Committee are not in possession of the name of any particular school against which, justly or unjustly, this charge has been openly and directly alleged. If there be a single school fairly deserving such an imputation, it is hoped that no false compassion will interpose to shield it. After three years of consultation and discussion, it surely cannot be deemed unreasonable that this Association should discriminate between those schools which have its confidence and those which have not.

Practically, the Association has advised an increase in the number of instructors, and an extension of the period of instruction. It is not known that either of these recommendations has been generally adopted; as to the last, it would seem that the original design of the Association, which, as the Committee apprehend, contemplated a lengthening of the term of lectures without materially increasing their number (on the ground that too many were crowded into too short a time,) has been somewhat misunderstood.

Since, of late, it has been argued that the *completeness* of a course of lectures necessarily depends upon its duration, and that this is an indisputable advantage of the six over the four months' course. The schools also appear to differ in their understanding of this point; some of them having increased both the term and amount of instruction, while others have extended the former alone. Thus, in the institution selected by the Committee of 1849 "as a model of the schools of our own country, whose rules and requirements are believed to be quite as full and extensive as those of any other institution, and whose trustees and professors seem to be as well disposed as any others to accede to the general wish of the profession, as expressed through this Association, to favor proper measures of reform"—although the lecture term is one of six months, the aggregate amount of instruction, as reported, is really less than in another school whose course continues but four and a half months. The Committee would suggest, however, that it should be fairly stated, by those schools in which the session is prolonged, whether there is, or is not, a proportional increase of the amount of instruction.

3d. The Association has tried to provide for the application of a more rigid test of qualification for admission into the profession.

It has proposed that the profession itself should have a voice and a vote in deciding upon the worthiness, or unworthiness, of those who desire to enter it; and that, for this purpose, there should be boards of examiners, composed of teachers and practitioners. Aside from any inclination or disinclination in the schools to consent to this arrangement, one thing must be yielded, that in this country admission to all the liberal professions must always remain comparatively easy. The nature of all our institutions supposes this. They must conciliate the public good will, upon which they are dependent for existence and patronage, by a liberal exercise of their powers and privileges. This is especially the case with new institutions in new States. It may be deemed doubtful, therefore, whether any uniform plan of medical education, or any uniform method of regulating admission to the profession, can be established throughout the Union. The Northern and Middle States, having a dense and wealthy population, accustomed to educational institutions, and able as well as willing to sustain them, are in a very different condition from that of the newer communities of the west and south-west. To insist that regardless of these differences, all shall conform to an arbitrary standard, would be to insist, in the judgment of the Committee upon something utterly impracticable. A disparaging comparison has been drawn between the system of medical education in other countries and in this. Admitting that American and European medical schools differ in the amount of exact training, rigid requirements and restraining discipline considered essential to the highest grade of intellectual education, it does not follow that it is practicable or desirable to assimilate them in all respects. There is no central power in this country able to originate or enforce a uniform system of laws regulating matters pertaining to general or professional education; and it is probable that an attempt to establish such a system would at once excite suspicion and opposition. Indeed, it is notorious that the friends of the only institution in the country* that approaches the best foreign schools in thoroughness of scientific and

* The United States Military Academy.

practical instruction, have been repeatedly called upon to repel the assaults of political jealousy.

The Committee have no wish to excite profitless discussion. They do not deny that American medical education may fall short when compared with that of the best European schools. Yet, after all the reproaches that have been uttered, and all the delinquencies that have been alleged, it does not appear that more severely disparaging epithets have been applied, rightly or wrongly, to an American degree, than were so strongly bestowed upon certain foreign degrees some twenty years ago; or that, even now, there is more content in the profession in Great Britain, with its Royal Colleges of Physicians and Surgeons, or in France, Austria, and Prussia, with their stern restrictions as to what shall or shall not be done, and who shall and who shall not do it, than at present exists in the United States.

The Committee have forbore from a more elaborate examination of the subject entrusted to them, from a conviction that it would be impolitic to multiply issues upon which the profession and the schools may differ. As to present action, they would recommend that the Association await the result of its advice to the "various schools of medicine to meet at Cincinnati before the next (this) annual meeting, and recommend and present a plan for elevating the standard of medical education to this Association."

Having fully and freely reiterated its opinions and wishes upon the subject, the Association has a right to expect from the schools an equally frank expression of theirs.

By a resolution passed at the last annual meeting, this Committee were directed to inquire into "the expediency of establishing a school, or schools, of pharmacy in the respective States, for the special purpose of preparing persons for the business of apothecaries; and also the expediency of adopting a rule that no physician ought to patronize a druggist or apothecary who deals in secret or patent remedies."

Schools of pharmacy already exist in several of the Atlantic cities. In New York and Philadelphia, these institutions have acquired a deserved reputation for capacity, industry, and usefulness; and there can be no doubt that the organization of similar schools throughout the country would be productive of great benefit. At present, the occupation of "apothecary" is open to all who choose to follow it; and there is scarcely a town or a village, from Eastport to San Francisco, in which one cannot buy drugs of all varieties of quality, and poisons of all degrees of intensity, from persons of all grades of ignorance and knowledge.

The expediency of adopting a rule that "no physician ought to patronize a druggist or apothecary who deals in patent or secret remedies," depends, in the opinion of the Committee, entirely upon the probability of its general observance. If, in the judgment of the Association, the sale of a single pernicious nostrum is likely to be checked by such a rule, a true regard for the interest of the public and the profession obviously prompts its immediate enactment, and its enforcement by all the pains and penalties the Association may have the power to inflict.

JOSEPH ROBY, *Chairman.*

NOTE.—The author of this report deems it proper to make the following explanation.

As chairman of the Committee, he prepared the report, and, in consequence of his inability to attend the meeting, transmitted it to Dr. Drake,

of Cincinnati, with a request that it might be laid before the Committee before it was presented to the Association. The names and residences of the whole Committee were stated in the communication to Dr. Drake, and the report was sent to him as Chairman of the Committee of Arrangements, he having given notice publicly that it was his duty to receive the reports of committees whose members might not be present.

It was the intention of the writer to leave the whole subject in the hands of the Association. The report of 1849 had almost completely exhausted all information in relation to the condition of medical education abroad; the statistics of American medical schools; the requirements of the army and navy boards; and the laws regulating the practice of medicine in the several states. It was not thought needful or desirable to go over this ground anew. Regarding the powers of the Committee as discretionary, it was hoped that the Association would not consider an omission of matters, which had been so fully treated of in previous reports, disrespectful to itself or a dereliction of duty in the Committee.

In the report, as read to the Association, an unintentional error existed which has been corrected. There was no motive or wish to do wilful injustice; the meaning of the writer was that but a limited number of the colleges of pharmacy "seem to be in active operation." In the transcription, the word "none" was unwittingly substituted for "few." The original opinion with regard to these institutions has since been fortified by the fact that, at the fourth decennial convention for reviewing the Pharmacopoeia of the United States, which met at Washington on the 6th of May, 1850, but three of the whole number of schools and colleges of pharmacy in the country were represented; viz., the New York, Philadelphia, and Cincinnati Colleges.

Pulmonary Gangrene. By MORETON STILLE, M.D., of Philadelphia.

In the thirteenth volume of the Prague Quarterly Journal of Practical Medicine, may be found an article upon Pulmonary Gangrene, by Dr. Fischel, physician to the Insane Asylum of the city of Prague. It is one of those valuable contributions to medical literature, which, from being in a language not generally understood, is not available to the mass of English readers. As, however, it relates to a subject rarely submitted to extended investigation, and contains many interesting observations, we propose to make use of it as a basis for the remarks we have to offer on this subject, and compare the opinions of the author with those of other writers. The disease treated of, although a rare one, is at the same time of much pathological interest, and on account of its infrequency, is not as well understood as it ought to be. Reports of the nature of that prepared by Dr. Fischel deserve to be made generally known, for it is by such laborious investigations that the bounds of our science are enlarged, and a wider field of profitable study reclaimed.

In the space of six years, viz., from 1839 to 1845 inclusive, there were made, at the Prague institution for Morbid Anatomy 3437 autopsies. Of this number, 3102 came from the General Hospital, the Lying-in and Foundling Hospitals, and 335 from the Insane Asylum. The cases of pulmonary gangrene among the former were 55, among the latter were 25,

which is a proportion among the sane of 1.6, and among the insane of 7.4 to every 100 autopsies. This excess of cases among the insane was constant in each year, a fact which seems to preclude the idea, that the great mortality from gangrene of the lung was due to an epidemic influence. The form of insanity was melancholy in 12, epilepsy in 5, mania in 4, and in 4 idiocy.

Dr. Fischel considers, that this predominance of the cases among the insane, shows a greater liability to the disease upon their part; an opinion which is in harmony with the views of Genest and Guislain. He thinks also that its more frequent occurrence among those suffering under the depressing forms of mental disease, confirms the idea of its being dependent upon an impairment of the nervous energy, but does not venture to locate this supposed lesion, as some have done, in the pneumogastric nerves. Its exciting cause in the majority of his cases was insufficient and improper food. Others enumerated were loss of blood, abuse of ardent spirits, great physical exertion, and prolonged venereal excess. This statement, gives, of course, but a very imperfect view of the etiology of gangrene of the lung. The persons who came under Dr. Fischel's observations were insane, and confined in a public charitable institution, their energies were exhausted by poverty, excess and actual disease, as well as by prolonged confinement, and voluntary abstinence from food. It is not difficult, therefore, to perceive how all these causes may have concurred to produce a condition of the system favoring the development of gangrene. It would not be correct, however, to regard them as the ordinary causes of the disease as it occurs in persons not under the same unfavorable hygienic circumstances. If its incidental development in the course of diseases of a typhous character be excepted, it will be found that it usually occurs under the influence of exposure to cold and dampness, during periods of depression following great excitement either of a mental or bodily nature. It cannot, indeed, be denied, that there is a certain kind of constitution in which it seems to be more readily developed than in others, but it is very difficult to describe it. Schonlein says, that it attacks principally young people of delicate skin and florid complexion who have given themselves up to an intemperate course of living. Canstatt collected twenty-two fatal cases, in sixteen of which the constitutional strength of the patient was noted, and twelve of them are described as being remarkably healthy persons. *Med. Klinik. Bd. 3.*) Dr. Gerhard says that "gangrene occurs in exhausted subjects, either affected with diseases calculated to weaken the powers of the system, or enfeebled by a life of intemperance, but there are exceptions to this rule. (*Am. Journ.*, vol. xviii, p. 301.) It is probable, that these statements, which appear to be somewhat at variance with each other, (and more of the same kind might easily be cited,) result from the frequent want of correspondence between the apparent muscular strength and the constitutional power of resistance to disease. While the first remains but little impaired, the foundations of the latter may have been long secretly undermined; cases exemplifying this truth are of daily observation, especially in our large hospitals, both in the medical and surgical practice; it being no uncommon event, that men of large stature and powerful frame, but of intemperate habits, succumb rapidly under injuries or diseases, apparently of a trifling character. Gangrene occurs in such persons most readily, being at one time the conse-

quence and at another the cause of phlebitis, or the index of a general or of a local deficient vitality.

Dr. Fischel makes the usual division of pulmonary gangrene into two kinds, viz., the circumscribed and the diffused. Although Cruveilhier (*Path. Anat.* t. i. liv. ii.) thinks that both varieties are equally common, we believe he is the only writer of any authority who expresses such an opinion. Lawrence out of sixty-eight cases met with diffused gangrene only six times, and Laennec only twice in twenty-four years. In the Report for the year 1848 of the Vienna Institute of Morbid Anatomy, by Dr. Lauthner, we find that 1069 post mortem examinations were made in the year, and of this large number there were only five cases of gangrene of the lung, one only of which was of the diffused kind. In Dr. Fischel's eighty cases there were but four diffused. Dr. Fischel's results confirm also the general impression that the disease occurs preferably in the lower portion of the lung, and chiefly in that of the right side; except, indeed, where it is consecutive to tubercular disease, when it may occur, of course, in any part of the organ so affected.

We have not met with any description of the forming stage of pulmonary gangrene, drawn from actual inspection of the lung, with the exception of a case reported by Dr. Gerhard, (*loc. cit.* Case VIII.) In this case, which was one of diffused gangrene, the death of the patient took place at a very early period in the disease; the texture of the lung was not broken down, although infiltrated with serum, diminished in consistence, and of a gangrenous odor. The later stages of the gangrenous process are well known, and may be easily recognised, but, in the absence of any very positive knowledge of its mode of commencement, it is usual and convenient to ascribe it to inflammation. It is very certain, however, that not being an ordinary result of inflammation, it must depend upon something extrinsic to this process, but not essential to it. If inflammation does not really include (as it is sometimes made to do) every possible morbid process, it is far more simple to confine it within the limits of stasis and exudation, and to regard the phenomena of resorption or of organization of the effused fibrin as processes of reparation, complimentary to, but not parts of the inflammation. There really exists no such thing as too violent inflammation in this sense; it is by its effusion that it becomes excessive or dangerous, since it cannot, in the ascending scale, go beyond the limit of an effusion of a more or less plastic material. When the blood vessels have been relieved by its discharge, the phenomena which follow are those of repair, and this process is more or less complete, according to the tissue affected, and the constitutional strength of the individual. The sentiment cannot be too often repeated, paradoxical as it may seem, that inflammation is, *essentially*, a conservative process, and that when, by interference with the functions of organs essential to life, it becomes lethal, it is so, either by its extent, or by the feeble recuperative power of the patient. If gangrene supervene upon inflammation, it is therefore to be attributed, not to the violence of this, but to causes which, although difficult to appreciate, are known to act by impairing or destroying the constitutional power of resistance. The engorgement and hepatization which are found around the gangrened portion of the lung, cannot be regarded as manifesting a prior stage of the process of mortification, but are rather an indication of an effort made by nature, to throw up a barrier against the extension of the disease, by which the de

tissue is circumscribed, and the source of the contamination isolated from the rest of the organ. It is the same process, essentially, which is seen in gangrene of the extremities, the progress of which it is designed to limit; when it does not occur, it is plain that no reliance can be placed upon the recuperative powers of the system. We may seem to be begging the question if we assert that gangrene is never a result of pneumonia, but as the direct anatomical proof of the fact cannot be procured, it is fair to this as a matter of general experience, that so common a disease as is pulmonary inflammation, equally rare is gangrene of the lung, and that if it be a consequence of violent pneumonia, the fact is a very remarkable one that it should occur so seldom. Indeed, all authors who have given particular attention to this point, agree that it does never succeed to a frank pneumonia; which is certainly equivalent to an admission, that if it depends at all upon inflammation, it is assuredly not due to an excess of it. Dr Hodgkin (*Lectures—Mucous Membrane*) says, that it “generally, if not always, seems to require some peculiarity in the constitution of the individual, rather than merely to depend on intense inflammation;” also, “it sometimes affects small spots scattered through the substance of the lungs; which quickly lose their vitality, without the precurrence of the ordinary symptoms of inflammation of the lungs.” Genest and Grisolle do not think that inflammation alone can produce it. The latter has never seen it follow a well characterized pneumonia. (*Trait prat de la Pneumonie*, p. 355.) Dr. Gerhard (*loc. cit.*) says, “In every instance in which the patients were seen in the early stages of the disease, I could distinguish the humid rhonchi, indicating bronchitis, or at least the secretion of liquid into the bronchial tubes. There was, in no instance, bronchial respiration, dull sound on percussion, or other unequivocal evidence of pneumonia; and in one case only was there even reason to suspect that pneumonia may have preceded gangrene.” Laennec (*Dis. of Chest*, p. 207) says, that “it can scarcely be ranked among the terminations of pulmonary inflammation, and still less can it be considered as the consequence of its intensity.” The opinion of some eminent pathologists, particularly of Carswell, Piorry, Cruveilhier, and Schroedervan der Kolk, that the obliteration or obturation of the arteries of the lung, is the proximate cause of pulmonary gangrene, is hardly tenable, and that for reasons analogous to those we have offered above. This obturation, it is reasonable to suppose, is the consequence and not the cause of the death of the pulmonary structure, and depends possibly upon an inflammation of the minute pulmonary vessels, in consequence of the reception of the corrosive products of the gangrened portion of the lung, as well as upon the effusion of plastic lymph in the structure around them, by which they are compressed and their canals effaced. Dr. Fischel justly observes, that this inflamed state of the pulmonary vessels, has never been found as a primary affection. But however easy it may be to assign valid reasons against these views, it is by no means equally so, to say what is the proximate cause.

Dr. Fischel, indeed, in the spirit of the German “*Krasenlehre*,”—or doctrine of crases,—feels satisfied with referring it to a hypnosis, or to a deficiency of fibrine in the blood; but this is not a sufficient explanation for one very plain reason, viz., that this condition of the blood exists in several other pathological states of the system, in which, nevertheless, no gangrene occurs, or is dreaded. The view advocated by Genest, and Dr. Law, of Dublin, that it is a consequence of pulmonary apoplexy, seems to have much in

its favor; for it is not difficult to conceive, that the effused blood, if it remain in the lung, may become putrefied by the action of the air, and become the source of the disease in that part with which it is in immediate contact. We believe that gangrene may originate in this manner, but there are many cases which cannot be explained by it. Hæmorrhage from the lungs is not unfrequently one of the first signs observed, yet it is more common in the progress of the disease than at its beginning,—a result of its progress, not the force which sets it in motion. The opinion of Dr. Stokes, always of great value, and particularly so in relation to this disease, is that the accidental putrefaction of blood effused in the lungs, cannot be reckoned even as an ordinary cause of pulmonary gangrene." He has not seen any cases of the change from one of these diseases to the other; and is of the opinion that where a pulmonary clot does become putrid, "the change is in itself a proof of gangrenous disposition pre-existing."—*Dub. Journal*, Feb., 1850.

While it is of practical importance that the idea of the dependency of gangrene of the lung upon inflammation should be combated, and its relation to pulmonary apoplexy considered as exceptional, it is not necessary for a correct knowledge of its treatment that an acquaintance with the exact nature of the process should first be possessed. For, knowing that all those causes which depress the nervous energy and vitiate the blood, may take part in its production, and that it appears, at times, to originate in consequence of the deprivation of sufficient nutriment, a rational treatment can be founded thereupon. In Dr. Fischel's cases, this last cause played an important part, and a large and interesting portion of his paper is taken up with a narrative of the means by which he sought to remedy it. We do not propose to enumerate these, nor the remedies recommended by various writers, as we would thereby trespass too much upon the space allotted to us. We have desired to call attention merely in a general way, to the anatomical characters of this formidable disease, and would take the liberty of referring the reader, for a minute account of the same, to a translation of Rokitansky's description in Copland's Medical Dictionary. In order to understand the interesting process by which, in favorable cases, a cure is accomplished, we might bring together the results of the observations of Dr. Fischel, and the accurate clinical reports of Dr. Gerhard, before referred to. By such a comparison it will be seen how beneficial is the result of the inflammation surrounding the gangrened portion of the lungs; the extension of the disease being limited by an exudation of plastic lymph around the dead tissue. This exuded fibrin forms a wall of various thickness around the cavity left by the separation of the slough, and after the latter has been removed by expectoration, the walls of the cavity, in course of time, gradually contract and approach each other, and sometimes finally unite, forming a dense cicatrix in the spot formerly occupied by the gangrenous eschar.—*Medical Examiner*.

A Remarkable case of Poisoning with Lead, extending over a period of nearly four years. By EDWARD MURPHY, M. D., of New Harmony, Indiana.

Mr. R.—, merchant, aged 42, of medium height, and rather stout habit of body; of bilious temperament, and sound intellect; has always

enjoyed good health, and no hereditary liability to disease; has always been temperate, and a close but active business man. During September, 1843, had a slight attack of autumnal fever of short continuance, and throughout the following winter had been often afflicted with pains in his abdomen, which disturbed him a good deal.

About the last of February, 1844, was confined to his bed for several days, with excessive, intermitting abdominal pain, and obstinate constipation of his bowels; but, he thinks, without fever; and was treated by his physicians for an attack of acute peritonitis. The constipation was very obstinate, and only yielded after several days, to very large doses of medicine. But, I consider it impossible, that acute inflammation within the abdomen should have continued so long as this attack did, without producing some organic change among the abdominal viscera.

After an imperfect recovery, Mr. R. went to Louisville on business, during the following March, where he was again attacked with the same symptoms—though not quite of the same severity—and was attended by a distinguished physician, who pronounced his disease to be abdominal neuralgia, stating, that it was a rather frequent complaint among mercantile men in that place, and prescribed accordingly. He also gave it as his opinion, that his former attack was the same disease and not peritonitis. Since that time, Mr. R.'s complaint has been considered neuralgia, and treated as such.

From that time up to the 22d of February, 1846, he has been suffering almost constantly, with excessive pain in his abdomen, radiating from thence to all parts of his body, often of very great severity; obstinate constipation of his bowels, accompanied often with nausea and vomiting, (the patient attributing the nausea and vomiting to the *very* large doses of opium which he was sometimes obliged to take;) was frequently confined to his bed; he lost flesh and strength, notwithstanding a constant good appetite, and had a bloated though anæmic countenance. He had very much the appearance of a person in cachexia from malignant disease. There was a dirty yellow color of the skin, and a yellow discoloration of the albuginea oculi simulating jaundice, the whole time. Sometimes during this period, he became affected with slight paralysis of the extensor muscles of the fingers of the right hand, with the exception of the index, which rendered him unable to write; his vision became imperfect; there was great mental prostration, approaching hypochondriasis,—indeed, he was totally unable to do business, throughout the greater part of this period, from mental imbecility, sometimes being unable to perform the minutest calculation, or to attend his customers, who generally considered him insane; was very irritable the whole time.

About this time, Mr. R. was attacked with what was thought to be apopleptic fits, having had four or five, and on the 24th, I was called in consultation. He was confined to his bed, very pale and feeble; sensible, although very weak in mind; would give an answer, in relation to his case, and immediately forget that he had done so; sometimes became alarmed at persons present, and again was much terrified at absent imaginary enemies, who were conspiring against him—a state resembling delirium tremens; speech, faltering and hesitating; sight, defective. His face was frequently affected with choreic convulsions, when he would complain of severe shooting pains through his body, and of which he was in constant

dread; tongue, soft and broad; pulse, feeble, but almost natural as respects frequency; bowels, constipated; stomach, very irritable; chest, perfectly sound; sounds and rhythm of the heart natural; nothing unusual in the appearance of his urine; although very feeble, would sit up for a short time when desired. Considering it impossible that an individual should have had four or five fits of apoplexy in two or three days, without any lesion to the brain or symptoms denoting such, and on carefully interrogating his family—the physician in attendance not having seen him in a fit—I made out his attacks to be of an epileptiform character—being preceded by the horrid scream of epileptics, accompanied with evident convulsions. I advised opening the bowels by active purgatives, opiates, nourishing diet, blister to the nuchæ, and sulph. quinia, when the bowels were well opened, and took my leave, after assuring his family that I did not consider his present attack to be apoplexy, but probably a part of his old complaint, and gave an unfavorable prognosis.

Mr. R. remained in nearly the same state, but without another fit, until the 3d of March, when I was again called in and associated in the treatment of his case. By persistence in the above remedies, to which was added wine and brandy, he very gradually recovered to his late state of health. When so far recovered as to be able to sit up, his defective vision became almost complete amaurosis, which continued some time, then gradually disappeared, but was not entirely recovered from; the patient was fully of the opinion, that it was caused by the quinia he had been taking, although never more than six grs. in twenty-four hours, and with no idiosyncrasy as to its action. There was also at this time increased paralysis of the right hand, the left also becoming slightly paralysed.

From the 16th of March, at which time my attendance ceased, up to January, 1847, when he placed himself in my hands for treatment of fistula of the anus, complicated with fissure, he continued to have the same attacks, of greater or less severity, with only short intervals of repose, being nearly worn out with constant suffering and bad health. As opium was his only relief, he generally prescribed for himself throughout the lengthened period of his sickness, except when his attack was unusually severe. - After the cure of his fistula, his disease returned with greater severity, and of a more alarming appearance than ever.

On the 10th of June, in the absence of his regular physician, I was again consulted. Mr. R. was confined to his chamber and almost to his bed, the mere wreck of his former self. Scarcely able to sit up, weeping from excruciating pain, and in such a state of mind as to express a wish to commit suicide, and indeed he was afraid he should do so. His face, pale and wan, was marked by the deepest despair, from extreme suffering, imploring me strongly for relief; wrists entirely dropped from complete paralysis—being perfectly helpless, and unable to straighten either hand, unless by the aid of the opposite arm, and requiring all the care of an infant, in being fed, washed, &c., yet, a comparatively good gripe with his hands. His extremities were dwindled away to the mere sheaths of the muscles; his abdomen seemed to be the centre as usual, from which his pain radiated, and it was with the greatest difficulty that I could persuade him, after a careful examination, of the non-existence of organic disease there. The slightest touch of the skin over the umbilicus, and indeed over other parts of the body, produced such terrific pain as almost to throw him

into convulsions, producing all the effects of an electric shock; while the greatest pressure over the same place, gave him no uneasiness, but rather relief: his bowels were always constipated, unless moved by medicine—was the constipation produced by the large quantity of opium which was taken, or did it depend on paralysis of the muscular tunic of the intestines? there was sometimes vomiting of a greenish watery fluid; tongue, flat and broad; pulse, very feeble, and more frequent than natural; his cachectic appearance was that of a person in the last stage of malignant disease; appetite, comparatively good; his suffering was much more intense during the night than the day, unless relieved by excessively large doses of opium. From the balls of both thumbs, which were much atrophied, excruciating pains would arise, shooting with great severity up his arms and shoulders, to the back of his neck and head; the shoulders were affected with constant pain, especially the deltoid muscles, which also were slightly paralyzed. The pain in his lower extremities was also very severe, commencing in the soles of his feet, which were so sore that he dreaded to touch the floor with them, and shooting up the limbs to the lumbar region with dreadful suffering. There was also at this time a new source of suffering, shooting pain through his testicles, of such severity, as almost to produce fainting; indeed, to see him in his suffering, was the most heart-rendering sight I ever witnessed, and I was greatly astonished to see how any human being could so long survive so much and such constant misery.

I stated to Mr. R., which I had done several times before, though not when attending him, that he presented in the strongest light, all the symptoms of poisoning with lead, and had it been possible that he could in any manner have been exposed to its influence, I should have no hesitation in attributing all his suffering and bad health to that cause. But, Mr. R. was a merchant, and in no way liable to be acted upon by lead or any of its salts in his business. There were no lead pipes or utensils used about the house; nor, had he taken it in any form as medicine during his whole life. The autumn before the commencement of his sickness, he built a new store and repaired his house, which were painted in the usual manner; and this was the only exposure to the influence of lead to which he could refer. I however considered, that this could not be the cause in itself, as I thought it impossible that its influence could have extended over a period of nearly four years.

His case seemed perfectly hopeless, and I firmly believed he would never leave his chamber again alive. As *all* the remedies recommended for neuralgia had been exhausted without any benefit, and as he had taken so much medicine from time to time, that his stomach gave way almost at the bare mention of it, I felt very much at a loss what to devise. I, however, advised Mr. R. to submit to an alterative course of mercury as a last resort; giving him to understand that I considered neuralgia, convulsions, and various anomalous affections, might depend upon a cachectic state of the body, from poison either taken into, or generated within it, and preventing its proper nutrition; and which might be controlled or removed by a course of mercury, as constitutional syphilis and malaria often were; at any rate, it was possible that it might produce a new action in his system. This he dreaded very much, and offered a great many objections, which I removed; but he declined it for the present.

June 17th. To-day Mr. R. consented to take mercury. I gave him a

one gr. blue pill, four times a day, with an occasional aperient, and continued the opium to relieve his suffering. I applied blisters over various parts of his spine, which increased his pain so much, that I was obliged to heal them directly. This treatment was continued about three weeks, with an occasional rubbing in of mercurial ointment over his abdomen, when a considerable improvement was manifest. Treatment continued.

July 15. Was summoned to Mr. R., when I expected another unfavorable turn in his disease had taken place, but was agreeably disappointed in finding him much relieved and improving, and down stairs. The statement which I had before repeatedly made to him, that he presented all the phenomena of poisoning with lead, made a very strong impression on his mind, so much so, that it constantly occupied his mind, and just brought to his recollection that he had been in the habit for many years of chewing lead, and that this habit extended so far back, that he was unable to date its commencement. Formerly, being very fond of his gun, he frequently took hunting excursions, on which occasion he always had a piece of bullet or shot in his mouth; when in the store, he seldom ever passed by the box containing the shot, without putting some in his mouth to chew. But, what he most liked from its agreeable taste, and of which he chewed a great deal, was the lead lining of tea boxes; besides, he considered that the pressure of the teeth on the metal enabled him better to bear his pain. I immediately replied, that the cause of all his suffering and bad health was perfectly clear, and at once assured him that he might yet be a sound man. I at once examined his gums for Dr. BURTON'S symptom, and found the blue line over four or five teeth. I considered the case fairly made out, and never felt so much rejoiced as at that moment, to think, that an individual, after such a prolonged period of suffering and bad health, whom all considered as beyond recovery, and almost in the grave, should by this discovery be yet restored to health and usefulness. Not so my patient, however; he was very skeptical of my prognosis, not conceiving it possible that his disease could have originated from what to him appeared so slight a cause. I assured him that his case always appeared a very strange one to me, and I was always astonished to think that a healthy individual as he had always been, should have been reduced to such a protracted state of bad health, without any organic disease, unless from some evident cause, which had at last been discovered; that it was now rendered almost certain, that his first attack, which was considered acute peritonitis—and many subsequent ones, were attacks of lead colic. Further, that his attack of autumnal fever, from which I date the commencement of his disease, had probably produced a debilitated state of the body, rendering it more susceptible to the influence of minute portions of the metal; also, that nearly every symptom, which writers have laid down as indicating poisoning with lead, had in his case been repeatedly and severely manifested; and the only reason why he did not before recover, was the continued renewal of the poison, whenever he was present where it could be obtained. Also, that we were now in a fair way of proving it, the cause being discovered, would in future be avoided, and he would continue well. I pointed to the present amelioration of his disease from the treatment he was pursuing, as a favorable indication that it depended on some removable cause, as idiopathic neuralgia of such long standing was seldom benefitted by any treatment. From all this, it will be seen that I had to urge a number of reasons to convince my patient

of the real nature of his case, but without convincing him. I added acid sulph. aromat. to the former remedies, and by the 1st of August, his pains had nearly entirely subsided, his bowels were acting naturally, and he left off medicine, even opium, for the first time since the commencement of his sickness. I ordered splints to his wrists and hands, which gradually recovered their natural state.

Mr. R. entirely recovered his health in every respect, and has continued well up to the present time, February, 1850, being again a *strong, active* business man. *He thinks* that the extensor muscles of his wrists and fingers are not *quite* so strong as before his disease commenced, which is probably true, as muscles which have long been inactive, require frequent and strong exercise to recover their proper tone, which cannot be given to these muscles; their function being merely extension, they cannot be exercised to any extent; however, the defect is very slight indeed.— *Western Lancet*.

Successful extraction of the Child by the Cæsarean Section, after the Death of the Mother. By GEORGE HARLEY, Esq., House Surgeon in the Edinburgh Maternity Hospital.

Catharine Davidson, aged 39, unmarried, was admitted into the Royal Maternity Hospital, Edinburgh, in the beginning of March, 1850, being in the seventh month of her second pregnancy, and complained of shortness of breathing in ascending stairs, and of symptoms of dyspepsia. Had menstruation last on 12th August, 1849, and expected to be delivered about the middle of May.

Being much troubled with swelled legs, and her face having a puffy, unusually pale, appearance, her urine was tested ten days before her death, and showed no appearance of albumen on the application of heat or nitric acid, separately and combined.

Thursday, 25th April, 1850.—About 25 minutes to 11, a. m., I was summoned to the kitchen by the matron, as one of the patients was in danger of suffocation. My first inquiry was, "had she lately taken any food?" The answer was in the negative. On reaching the kitchen, I found Catharine Davidson sitting on a chair, in the washing house, close to the kitchen door, supported by two of the other patients, with eyes prominent, lips becoming blue, respiration very hurried, with great effort and heaving of the chest; her countenance altogether denoting the most intense anxiety. All that she could say was, "I am gone! O, doctor, save me!" Hearing a distinct half crowing, half gurgling sound in her throat, I immediately ordered her dress to be loosened, and her stays torn off. She was now spitting up a brownish, glairy, semi-fluid matter.

We supported her, and got her up stairs to ward No. 4, having to stand still on the way several times, to allow her to recover her breath. As soon as she sat down in bed, I ran and prepared an emetic of ζ ss. sulphate of zinc, which she willingly swallowed. Her symptoms gradually, though quickly, becoming more urgent, I proposed performing laryngotomy; she laid herself upon her back, and the operation was quickly completed. On bringing her head forwards over the bed, white frothy mucus ran out of her mouth, and some from the wound. There not being a tracheotomy tube in the house, I cut about three and a half inches off a full-sized flexible male catheter, and inserted it into the wound; but finding the mucus did

not come readily through it, it was immediately withdrawn. I then repeated the emetic, as the former dose seemed greatly to have assisted expectoration, and in less than five minutes above eight ounces of white frothy mucus were collected in a vessel. The patient now became quite livid in the face, her eyes seemed starting from their sockets; and after tossing about her arms, and giving one or two gasping inspirations, she fell backwards insensible. The symptoms still simulating those of spasmodic closure of the glottis, or some other obstruction at the larynx, and thinking that the artificial opening was not large enough to admit sufficient air to carry on the respiration, or allow her to expectorate freely, we pulled the bed to the nearest window, and I immediately proceeded to perform tracheotomy; but before the operation was much more than half finished, all attempts at respiration ceased.

The matron, a most intelligent person, having had her hand on the pulse, watching its decline, told me that it had ceased to be perceptible for some minutes. On touching the wrist, I found she was perfectly correct; and, on putting my ear to the chest, I heard no pulsation at the heart. A single glance at the patient's face showed evidently that she was dead; the frothy mucus was running out at the mouth and nostrils, the eyes were fixed, and the pupils dilated.

All hope of saving the woman being lost, my next thought was to save the life of the child; so I again snatched up the bistoury, ripped down the patient's dress, and instantly made an incision in the mesial line, through the parietes of the abdomen, commencing a little above the symphysis pubes, keeping close to the right side of the umbilicus, and terminating a little above it. The uterus then appeared, and I proceeded to make incisions in it, to avoid wounding the child. In making these incisions none of the intestines came in the way of the knife, and there was very little or no bleeding from the wounds.

When the cavity of the uterus was reached, the liquor amnii escaped. I put the two first fingers of my left hand into it, laid the back of the knife against them, and cut downwards and outwards. One side of the nates now appeared. I then put the right hand into the uterus, caught hold of the first thing that came in the way, which happened to be a leg, and withdrew the child without any difficulty, the uterus not contracting round the neck.

The child, on extraction, looked beautiful and clean, as if it had been carefully washed; it was to all appearance quite dead, no pulsation being felt either at heart or cord. I dashed cold water on its chest, gave it a rub, and then put my mouth to its mouth, depressed and pushed back the larynx, held the nostrils with the one hand, and pressed on the chest with the other, after each time that I filled the child's lungs with air.

After a few minutes I stopped to take breath, and during that time I applied friction and aqua ammoniæ to the breast; and on using artificial respiration for some minutes more, the child's heart began to beat, and the pulsations in the cord became distinctly visible; a legature having been put upon it, the child was cut away. It was a male, weighing 6 lbs. 12 ozs. and measuring 18½ inches. Shortly afterwards I put my hand into the uterus, and peeled the placenta from the back part and right side of the organ, as it would not come away by pulling at the cord; it weighed 1 lb. 4 ozs., and the cord measured 20 inches. The wound was stitched up, and moved into the delivery room, where the post-mortem examination

Not more than twenty minutes elapsed from the time the patient was seized in the kitchen till all was over.

Autopsy, twenty-four Hours after Death. Present—Dr. Thompson; Mr. Rolston, surgeon; and Mr. Harley, house-surgeon.

External Surface.—Countenance livid, lips bluish-purple, eyes prominent, pupils dilated; an incision $8\frac{1}{2}$ inches long, extending from a little above the symphysis pubis, keeping to the right side of the umbilicus, and terminating about an inch above it.

Pulmonary Organs.—Lungs pale and white at the margins, which were emphysematous, touching each other opposite the 2d and 3d ribs; six ounces of serum in the left pleura, as much, if not more, in the right, which was not measured, as some blood from the incisions made in opening the chest, had mingled with it.

On incision being made into the lungs, they were seen to be quite full of mucus, which exuded on pressure; the crepitation was very slight, in consequence of the œdema; a few hard substances like tubercles were found throughout their substance.

Cardiac Organs.—On opening the pericardium, four ounces of serum were found in it; it was otherwise healthy. Heart rather pale, weighing 14 ounces. Ventricles contracted, without any blood in their cavities; left auricle dilated; mitral valves cartilaginous, and scarcely admitting the point of the fore-finger. The corpora aurantii on the aortic valves were very much hypertrophied, preventing complete closure of the aortic orifice, and consequently permitting regurgitation. Right side of heart natural.

Larynx and Trachea both quite healthy; no vestige of any lesion, except the laryngotomy wound; epiglottis and vocal cords healthy.

Abdomen.—*Kidneys* full size and quite healthy. *Spleen* healthy and of usual size. *Liver* not examined. *Intestinal canal* quite normal. *Uterus* not contracted, the wound in its anterior surface quite distinct. Some urine being drawn off by the catheter, was found to be slightly coagulable by nitric acid, but not by heat, till nitric acid was also added. It was not changed by caustic potash and heat.

Examination of Head, twenty-nine Hours after Death.—Present—Dr. Keiller and Mr. Harley.

Skull thick, with dura mater adherent to it; no extravasation external to dura mater. Superior longitudinal sinus empty. On raising the dura mater, a quantity of serous effusion was found beneath the arachnoid membrane, and a large quantity flowed also from the spinal canal. *Brain* quite healthy, rather anæmic than otherwise; arteries empty, and choroid plexuses very anæmic; ventricles containing rather more than the usual amount of serum. Nothing could be detected in the brain to account for death.*

From information subsequently received from the relations, it was found that, when 14 or 15 years of age, she had been confined to bed for a fortnight or three weeks with an attack of acute rheumatism. Four years ago she had had a severe cold, and had always complained more or less of colds since, together with shortness of breath on going up stairs, and occasional slight palpitation; during the past winter her cough had been more troublesome than usual.—*Edinburgh Monthly Jour. of Medical Science.*

*Serous effusion in this arachnoid was probably the immediate cause of death. See Original Communications, Art. No. III.—*Editor Buffalo Journal.*

*Notes on the Purification and Properties of Chloroform.** By WILLIAM GREGORY, M. D., Professor of Chemistry in the University of Edinburgh.†

1. Chloroform has been prepared both from alcohol and wood-spirit. The latter has been used for the sake of cheapness; but as it is a mixture of several liquids, all of which do not yield chloroform, it gives an impure product, in a proportion which varies much, but is always below that obtained from alcohol. There is, therefore, not only no advantage, but the contrary, in using wood-spirit, which is not, after all, much cheaper than alcohol.

2. But the chloroform from these two liquids, *when fully purified*, is quite identical in all its properties. Its smell, density, boiling-point, and action in the system, are in both cases exactly the same. That from alcohol is no doubt more easily purified than the other, but it also contains certain volatile oily impurities, which must be removed before it can be safely used. The peculiar oils which adhere to both kinds of chloroform are not identical, or at least, not all identical, but they are of analogous constitution and properties.

3. Soubeiran and Mialhe have examined these oils. They contain chlorine, have a disagreeable smell, and when inspired or smelt, cause distressing headache and sickness. In the case of wood-spirit, some of its own impurities distil over unchanged, and are also found in the chloroform.

4. It is well known that many persons, after the use of chloroform, have suffered from headache, nausea, and even vomiting, as I have more than once seen. Headache and nausea I have myself often experienced, when I have tried different specimens of chloroform, without taking so much as to produce the full effect.

5. Perfectly pure chloroform does not, so far as I have seen or experienced, produce these disagreeable effects. It is therefore highly probable, that when they occur, as they do with some individuals, from the use of chloroform of more than the average goodness of quality, they depend on the presence of a trace of these poisonous oils.

6. All good manufacturers of chloroform, purify it by the action of oil of vitriol, which destroys the oils, while at the same time a part of the acid is reduced to sulphurous acid. The chloroform, to remove this, is then distilled with lime or carbonate of baryta, and is tolerably pure if the process be well conducted.

7. But it is not quite pure, and contains a trace, more or less distinct, of the oils. I have found this to be the case with all the best chloroform made here up to 1849; and I have several times seen headache and sickness from the use of such chloroform, which was the best anywhere made. I must add, however, that the quantity of oils in the chloroform of the best Edinburgh manufacturers, although variable within certain limits, was always so small, that that product was fit for use, and only caused headache, &c., in a few peculiarly sensitive persons.

8. It was desirable to have a test for these impurities, as well as an easy and effectual mode of removing the last traces of them, especially as many

* Although I am alone responsible for the opinions contained in this paper, it is my duty to state, that all the experiments and observations mentioned in it have been made by me in concert with my able assistant, Mr. Alexander Kemp, of whose ingenuity and accuracy I have had constant opportunities of judging.

† Read before the Royal Society of Edinburgh, March, 1850.

sorts of chloroform not made here were far inferior in quality to that prepared in Edinburgh. One very delicate test is, that of oil of vitriol, which should be quite colorless, pure, and of the full density of 1.840 at least, as it may be obtained by Mr. Kemp's process, lately read to the Royal Society; when agitated with the chloroform, it becomes yellow or brown, from its action on the oils, which it chars or destroys. Any change of color is easily seen by contrast with the colorless chloroform which floats above. Pure chloroform gives no color to the acid. It is essential that the oil of vitriol be colorless and also of full density; for if colored, it is not easy to see a slight change on its color; and if below the proper density, that is too weak, it is not much colored by a chloroform which will render dark brown the acid of proper strength.

9. Another test, still more delicate, I find to be the smell of the oils. When chloroform is poured on the hand or on a handkerchief, it rapidly evaporates; but the oil being less volatile, are left behind; and their smell, previously covered by that of the chloroform, is easily recognized. Until very lately no chloroform was sold, or indeed known, which would stand this test, or even the former.

10. Up to 1849 the best commercial chloroform had a specific gravity of 1.480, which was considered a guarantee of its purity; but it had been obtained by chemists of specific gravity 1.494, and even 1.497. I have found that chloroform of 1.480, when once more acted on by oil of vitriol, which destroys the oils and becomes brown, may be obtained after removing the sulphurous acid, of specific gravity 1.500 at 60°. This I take to be the specific gravity of pure chloroform. Our best makers have lately, much to their credit, pushed the purification so far as to furnish chloroform even of this highest density, and also in other respects such as it ought to be.

11. There are still, however, many makers in other places whose chloroform is not so pure; and I shall now describe the method which, with Mr. Kemp, I have employed for purifying, perfectly and easily, any commercial chloroform, except one remarkable specimen—a process which will enable any medical man to purify it for himself with the greatest facility.

12. The chloroform, having been tested as above, and found more or less impure, is to be agitated with the oil of vitriol, (half its volume will be sufficient,) and *allowed to remain in contact* with the acid, of course in a clean, dry, and stoppered bottle, and with *occasional agitation* till the acid no longer becomes darker in color. As long as the action is incomplete, there will be seen, after rest, at the line of contact, a darker ring. When this no longer appears, the chloroform may be drawn off, and for greater security once more acted upon by a quarter of its volume of the acid, which should now remain colorless. It is now to be once more drawn off, and in a dry stoppered bottle mixed with a little powdered peroxide of manganese, with which it is gently agitated, and left in contact until the odour of sulphurous acid is entirely destroyed, and the chloroform has acquired a mild, agreeable, fruity smell. It has then only to be poured off into a proper phial. It will now leave no disagreeable smell when evaporated on the hand. [If the commercial chloroform, after having been *frequently well shaken* and *left for some time in contact* with the acid, has given to it only a moderate tinge of color, it is probable that it may be completely purified by the first process. To ascertain this, test a fresh portion in a tube with fresh acid, shaking well and allowing it to stand for some time. If it do not color the acid

at all, then the whole chloroform has only to be finally purified by the oxide of manganese. If the acid become colored in the test-tube, it will be as well to act on the whole chloroform a second time with fresh acid till it stands the test. Mr. Kemp has observed, in repeating this process for me, the very curious fact that, as soon as the action is complete, and the oily impurities are destroyed, but not sooner, the chloroform tested with the acid in a tube, exhibits a strongly convex surface downwards, where it rests on the pure acid, or what is the same thing, the acid becomes concave at its upper surface. The smallest trace of impurity, not sufficient to affect the density of the chloroform, we have found to render the line of junction horizontal. It is probable that this may become a valuable test of the perfect purity of chloroform; but we shall not say more on this subject until we have thoroughly examined it.] This process requires no apparatus beyond a few stoppered bottles and a *pipette*, if we wish to draw off the whole chloroform without loss, although nearly the whole may be simply poured off. The use of the oxide of manganese is due to Mr. Kemp; and on the large scale the chloroform may be filtered through a cylinder full of it. In this final purification of commercial chloroform, no distillation is necessary. Indeed, no rectification is required at all, if it be well washed with water before using the acid.

13. It may be considered as certain, that the use of chloroform thus purified, will very rarely, if ever, cause the disagreeable effects above noticed.* As to more serious bad results from the use of chloroform, so often spoken of elsewhere, it is enough to state that a large proportion of the cases must be attributed to the use of a liquid so impure as hardly to deserve the name of chloroform at all.

Postscript.—Since writing the above, my attention has been called to a paper by Dr. Wilson, on the specific gravity of chloroform, which he was not able to obtain higher than 1.498. I have therefore to add, that every specimen, whether of specific gravity 1.480, 1.490, or 1.497, which I purified as above, acquired the same density of 1.500, as ascertained by the use of a very delicate and accurate bead, (made by Lovi,) which sank at 60.°5

* Dr. Simpson informs me, that the purest chloroform he has used not unfrequently causes vomiting. On further inquiries, I find that this occurs when it is administered after a full meal. This can easily be avoided, and must not be confounded with the headache, nausea and vomiting alluded to in sections 4 and 5, which symptoms are persistent, and occurred in my experiments always with an empty stomach, the experiments being made an hour or two before dinner. Mr. Carmichael, assistant to Dr. Simpson, has mentioned to me some facts which confirm the view I have taken. At one period, for more than a week, Dr. Simpson and Mr. Carmichael were kept in a state of continual anxiety by the occurrence, in all the puerperal cases in which chloroform was used, of very unpleasant symptoms, particularly of frequent pulse and other febrile symptoms, lasting for some days. At last, after much annoyance from this cause, it occurred to Dr. Simpson that he was using one particular specimen of chloroform above the average in quality. As soon as this idea occurred, he threw away all that remained, and returned to that which he had generally used. The unpleasant symptoms no longer appeared. (I regret much that I had not an opportunity of examining that specimen; but I may add, that the maker, not an Edinburgh one, now produces chloroform of much better quality, though not yet absolutely pure.) But the striking fact is this, that Dr. Simpson and Mr. Carmichael state, that during the period above alluded to, when that one kind of chloroform alone was used by them, their handkerchiefs became quite offensive from the smell left on them, which even adhered to them after washing. There can, I think, be no doubt, that here the oily impurities alluded to in sections 4 and 5 were present in notable quantity.

and rose at 59.05 ; and also by three successive weighings with a very delicate balance. It will also be seen, that three commercial specimens had this density; I could detect no foreign matter in my chloroform; and besides, every foreign matter that is likely to occur *lowers* the density. I have no doubt that Dr. Wilson's specimens would have colored the acid and left a smell on the hand.

I may add, for the maker, that, after distilling the materials which yield chloroform, no distillation or rectification is needed. He has only to wash the heavy fluid with water till its volume no longer diminishes, and then to use the oil of vitriol as above, finishing with the oxide of manganese. Distillation with the acid is of no use, because no proper contact can take place, the chloroform distilling from the surface as it would from mercury. In testing by oil of vitriol, it is best to use some ounces of chloroform, and to shake it in a phial, because in a test-tube, the color produced, if not strong, may be overlooked.

While I acquit the makers of chloroform, who have sold an impure drug, of all desire or intention to adulterate it, I feel it my duty to point out, that the system which permits *any one* to set up as a manufacturer of this or any other potent remedy, without let or hindrance, without any test of his qualifications, without, in short, enforcing a knowledge of chemistry and pharmacy as an essential condition, is a radically bad one; and that our law, in relation to Druggists and Apothecaries, requires reformation. In fact, the evils naturally resulting from it are only neutralized, and that but in part, by the good feeling and principle of the leading manufacturers.

To illustrate this, I may remark, that some of the makers of chloroform must have been very ignorant, even of what was known and published concerning its properties; for, among the specimens I examined, and several of specific gravity below 1.480, which was long ago given as the standard, even so low as 1.347.

That this neglect proceeded more from ignorance than from intention, is, I think, plain from the fact, that a specimen labelled "Pure Chloroform" actually contained only a trace, about one-thirtieth, of that substance. I did not ascertain its specific gravity, which must have been far lower than 1.200 or 1.100—nay, possibly, under 1.000, because its impurity was so obvious in every other respect, and the quantity I had was too small; but, on examining it further, I am convinced that its origin was this:—The maker, after distilling the materials, obtained, of course, two liquids, a lighter and a heavier. He evidently *did not know* that the latter was the chloroform, and therefore threw it away, and preserved the *lighter*—a mixture of pyroxilic spirit—of its natural impurities, of the deleterious chlorinated oils and a trace of chloroform. At least, such are its characters; and it exactly resembles what would be obtained in the way supposed. But what a fearful degree of ignorance (without any evil intention) is here exhibited! And yet this maker was free to produce and sell *pure chloroform*, which was actually almost *pure from chloroform*, and loaded with deleterious agents.—*Monthly Journal of Medical Science*, May, 1850, and *Pharm. Jour.* June, 1850.

[A few weeks since, having occasion for some chloroform, we obtained a pound from a manufacturing establishment in this city. On opening the bottle the odor of the chloroform was contaminated with that of chlorine, and the stopper had on it a greenish yellow discoloration. On returning it

to the chemist with the information of its impurity, we received the following note:

"Dear Sir: We have your favor of this morning, in regard to chlorine found in chloroform, and for the same are much obliged, as it gives us some ground, other than our own opinion, for declining to make the article by the late improved process with sulphuric acid. We soon discovered that this change was likely to go on, but some of the customers wanting an article of this kind (*i. e.* made by this process) particularly, we were induced to adopt the improvement."

This chloroform bleached moist litmus paper rapidly, first reddening it. The acidity is due to hydrochloric acid, generated from the chlorine and moisture adhering to the chloroform operated on by light. Mr. Abraham, in the paper, at page 348, has arrived at similar conclusions.—EDITOR.]—*Med. Jour. of Phar.*

Treatment of Rubeola by Inunction.—By JOHN EVANS, M. D., Prof. of Obstetrics, &c., in Rush Medical College, etc., etc.

June 1, 1850.—Miss F., aged 15 years, was laboring under the symptoms that characterize a violent attack of Measles. The febrile action was strong—pain in the extremities, loins and head, severe—the eyes were injected, suffused and intolerant of light—distressing nausea was constant, and the characteristic eruption was well marked upon the face, neck and breast.

I gave Dovers powder grs. viij. every six hours with free use of warm teas.

Finding no abatement of the distressing symptoms the next morning, I determined to use inunction, and, as practiced by Dr. Schneeman in Scarlatina, directed the patient to be rubbed with a piece of fat bacon over the entire cutaneous surface.

The relief was marked by the subsidence of all the distressing symptoms in a few hours, and the application was repeated twice the next day. No other treatment was applied except the free use of warm teas. The recovery was more rapid than I had before seen in such cases, and without any disagreeable sequel.

Two other members of the same family were treated by the inunction with the same favorable result.

I have since used the plan of treatment in a number of cases and with uniform and prompt relief.—*North Western Journal*

Singular Revenge.—In a memoir of Sir B. C. Brodie, the Editor of the London Lancet, relates the following remarkable circumstance:

"Late one evening a person came into our office, and asked to see the editor of THE LANCET. On being introduced to our *sanctum*, he placed a bundle upon the table, from which he proceeded to extract a very fair and symmetrical lower extremity, which might have matched

"Atlanta's better part,"

and which had evidently belonged to a woman. "There," said he, "is

there any thing the matter with that leg? Did you ever see a handsomer? What ought the man to be done with who cut it off?" On having the meaning of these interrogatories put before us, we found that it was the leg of the *wife* of our evening visitor. He had been accustomed to admire the lady's leg and foot, of the perfection of which, she was, it appears, fully conscious. A few days before, he had excited her anger, and they had quarrelled violently, upon which, she left the house, declaring she would be revenged on him, and that he should never see the objects of his admiration again. The next thing he heard of her was, that she was a patient in **** Hospital, and had had her leg amputated. She had declared to the surgeons that she suffered intolerable pain in the knee, and had begged to have the limb removed—a petition the surgeons complied with, and thus became the instrument of her absurd and self-torturing revenge upon her husband!—*Southern Med. and Surg. Jour.*

The Application of Extension in Fractures by means of Adhesive Plaster.

[Allusion was made in the August number of the Journal to the use of adhesive plaster as a means of applying extension in fractures. Understanding that Dr. Josiah Crosby's method was not stated with perfect accuracy, a note was addressed asking for information upon the point. The following is a portion of the reply. Ed.]

My first plan was to apply two strips of fresh spread English Adhesive Plaster, one on either side of the leg, wide enough to cover at least half the diameter of the limb from above the knee to the malleolar processes, below which the straps were left floating a foot or more without plaster. A circular strap was then applied above the knee—above the calf of the leg, and another above the malleolar processes, and over these the roller bandage; but in the last case I applied the longitudinal straps from below the knee to the ankle, relying on the roller bandage without the circular straps. This is undoubtedly preferable, as the circular straps might be injurious in case the limb should become much swollen.

This was a case where considerable extension was necessary, and was continued until union had taken place, without the slightest alteration in the first dressing. On the 32d day, the roller, which was applied from the toes to above the knee, was removed in presence of several physicians, and the straps were adhering well to the limb, and no appearance of excoriation of the skin, and the patient had never uttered a complaint from the extension. His answer to the inquiry, "what the sensation was" would be, "It feels as if my leg was in the mud, and I was trying to pull it out."

I am now treating a compound oblique fracture of the tibia about three inches above the ankle joint, (the fibula also is broken,) making both extension and counter extension in a most perfect manner with the adhesive straps, and without the slightest inconvenience to the patient—no complaint from the dressings, whatever. The counter extension is made from *below* the knee, and the extension from two inches of the lower part of the leg and the foot.

I have treated two cases of fracture of the clavicle in children of two

years old, with nothing but adhesive straps, with as good success as I ever had with the old methods, and not half the trouble.

Your ob't serv't,

JOSIAH CROSBY.

[From the *New Hampshire Journal of Medicine.*]

The "Mange," communicated to three persons by a Pig. Reported in a letter to the Editor. By H. R. CASEY, M. D., of Columbia county, Ga.

I will give you the particulars of a conversation held a few days since with a gentleman of this county, and if the deduction I have drawn from the facts as reported is correct, we have presented to us (so far at least as my observation extends) a new disease of the cutaneous system—one hitherto undescribed by dermatologists.

Mr. S. asked me "if I had ever known a man to have the mange?" to which I gave a negative reply: having always understood that it was a disease peculiar to the quadruped—He then asked me "if I thought it possible for a man to catch it from a hog?" I replied, that there are a great many things regarded as impossible, which are not found to be so when subjected to the test—and that this might be one of the cases. He then proceeded to give me the following particulars:

He states that about the first of May last, having a pig badly diseased with the mange, and being desirous to cure him, he had some soap and water got and went to work on him with his hands—and that after giving him a good washing, he stripped him almost of his entire *external* with his nails. That he was entirely well at this time; but that in about three hours thereafter, he felt an itching on his hands and wrists, and an eruption which commenced spreading upwards; that about the same time, his ankles began to itch him and the eruption there made its appearance, which also spread upwards and met the eruption from above at the half-way house—the umbilicus; that it reached its height in about two weeks; that the eruption was characterized by great heat and intolerable itching, composed of small vesicles, which, though not confluent, stood close together over his entire tegumentary tissue. Thus was he at the time of his commencement with the abluion—a sound and healthy man—but in a very short time thereafter, he was transformed into a Lazarus. He thought he had contracted his disease from the pig, and went to work to cure himself, using first the soap and water. This not benefiting him, he was bled and took salts. This failing, he tried *pot-liquor*—then the grease from fried bacon—then a solution of blue-stone. He does not think that any of the means used had any control whatever over the disease, but that it seemed to pursue its course, knowing no conqueror, until it finally wore itself out, in about five weeks.

Now, from the above narrative, I can but infer that the disease in question was one identical with the mange, and that it was communicated from the quadruped to the man. And I am further strengthened in this view of the case, from the fact—that a female and the negro boy who held the pig while being subjected to treatment, became in like manner affected. The view I have taken of this case, I know to be in direct conflict with the long-established dogmas of the veterinary school, but I think I am sustained in my position from the facts of the case—and "facts are stubborn things."

By reference to the "*History of the Horse*," I find the following language. The author, in speaking of the contagiousness of the mange, goes on to say—"If the same brush or curry-comb be used on all the horses, the propagation of mange is assured; and horses feeding in the same pasture with mangy ones, rarely escape, from the propensity they have to nibble one another. Mange in cattle has been propagated to the horse—and from the horse to cattle—but there is no authenticated instance of the same disease being communicated from the dog to the horse. There is as much difference in the character and eruption of mange in the horse and dog, as between either of them and the itch in the human subject; and the itch has never been communicated to the quadruped, *nor the mange of the quadruped to the human being.*"

My only reply to the above quotation, is the presentation of the case related; and if I am not sustained in my corollary from the facts of the case, this article will go for nothing. I pretend to no familiarity with cutaneous diseases; but if I were called upon to classify the mange, I should locate it in the group *dermatoses scabienses* of Wilson, not only from the pathology, but also from the therapeia of the disease; for I find sulphur the anchor of safety to the veterinary surgeon. Nor do I think there is any thing very strange in all this; and the only reason why we have never before had the mange communicated to man arises simply, I think, from the fact, that in all probability more caution has hitherto been experienced than was in the case before us. We have examples of other diseases occurring in the human subject, the result of propagation from the lower order of animals. In the *Revue Medicale*, of July, 1845, we have detailed a case of an officer who took the glanders and farcy from a horse, and in which experiments were made by M. Andouard, to test the contagiousness of the human fluid introduced into other animals—the results of which experiments went to prove that the disease was not only communicable to man from the horse, but that the disease was again transmissible from the human subject to the quadruped. In the *Southern Medical and Surgical Journal*, Nov., 1847, we have a case of Glanders in the human subject, derived from the horse, reported as occurring in your own city. Other diseases might be mentioned occurring in the great paragon of animals, communicated from the lower order; but I have already spun out this article to a greater length than was designed at its commencement, and will conclude by merely advising those persons who may have to treat the mange in stock, to touch it lightly, and never make a curry-comb of their hands; to which injunction I know my friend F. will say amen.—*Southern Med. Jour.*

Excerpta.—In certain sects of religion there is a class known (at the south) by the soubriquet of Hard Shells—a class, whose hog-like souls no precepts of wisdom, human or divine, can lift above the mire of conceit and stupidity in which they instinctively wallow. Perhaps the practitioner of medicine has therefore no right to complain, if, in the healing art, a like species of hard cases are to be encountered in officious visits around the bedside of the halt, the lame and the blind—suggestions drop through their prolific brains with the facility of water through a sieve. If all such were to visit a menagerie, their asinine diathesis might be much improved.—*New Hampshire Jour.*

EDITORIAL DEPARTMENT.

Obstetrical Statistics.—The application of statistics to the study of the various subjects relating to health and disease, constitutes one of the most striking of the characteristics of modern medicine. In almost every department of the science the advantages flowing from numerical investigations are conspicuous. Of many of the points admitting of arithmetical demonstration, our knowledge, by this means, has been rendered full and exact, which heretofore was incomplete and dubious; certainty, in not a few instances, has taken the place of merely rational conjecture, and the false conclusions of speculation, or loose observations, have been displaced by results oftentimes not less unexpected than positive. Not only has truth been confirmed, and error disproved by statistical facts, but by the various methods of interpreting phenomena with the aid of figures, new relations have been disclosed, and new laws developed. Indeed, it has been discovered that the slate and pencil are important instruments of interrogating, as well as interpreting nature. In this way not only have fields already in possession been cultivated with greater success, but new territories have been explored and reclaimed, and the horizon which bounds the limits of our inquiries extended.

We have been led into this train of reflection by placing before us articles contained in two of our contemporaries, submitting the fruits of statistical researches in the province of obstetrics. The first of the articles referred to, may be found in the *Am. Jour. of Med. Sciences*, No. for Oct., 1850, and is entitled "Statistics of the Boston Lying-in-Hospital." By D. Humphreys Storer, M. D., one of the Physicians of the Massachusetts General Hospital. The other article is in the *Philadelphia Med. Examiner*, No. for October, 1850, and is entitled "Contributions to Obstetrics, with tabular views, and Miscellaneous Observations. By Henry A. Ramsay, M. D., Raysville, Georgia."

There are numerous points relating to obstetrics which admit of investigation by means of statistics, and respecting which, by series of observations sufficiently numerous and extended, our knowledge may be rendered more precise and comprehensive. Lying-in-hospitals, of course, offer

a more favorable opportunity for collecting materials for enumeration, than private practice, yet the latter, with a little pains, may be made available for this purpose. The 'Contributions' by Dr. Ramsay, are the results of the analysis of the records of private cases. The habit of keeping notes of this class of cases, as well as of other classes, enhances our scientific interest in the obstetrical department of general practice, and, in this point of view, amply repays the trouble which it costs. We have, of late years, made it a duty to record the details of all cases of labor occurring under our observation, and have now collected between three and four hundred for analysis at some future period of leisure, when the number has become still greater. In this, as in other branches of statistical investigation, the results of different analyses by different persons, at different times, and in different places, are desirable. They will serve not alone as additions to numerical data, but to determine variations, which may be found to exist, incident to time, locality, and divers modes of practice.

With these remarks by way of introduction, we proceed to present a summary of the statistical results contained in the two articles alluded to, taking the liberty of condensing in so far as practicable, and omitting such portions as seem to us to possess comparatively little interest or importance. The cases analyzed by Dr. Storer, are all that have occurred at the Boston Lying-in Hospital, since the foundation of the Institution. The period is not stated in the Report. The reader, however, is led to infer that it embraces from ten to twenty years.

The whole number of deliveries is 451. The number of children in these deliveries is 456. Of 331 cases, 132 were males, and 199 females.

The youngest woman delivered was 16; the oldest 47.

The greatest number of times any of the women had been pregnant, was 16, which occurred in but one instance. The number of primipara was 193.

No. of days from last menstruation.—This is an important point with reference to the duration of pregnancy. We give the author's figures:—

No. of days	279	278	277	275	275	274	273	272	271	270	269	268	267
No of cases	7	8	11	9	4	6	6	6	8	20	2	4	2
No. of days	266	265	264	263	262	261	260	259	258	257	256	255	244
No. of cases	5	9	9	3	5	4	7	4	3	2	1	4	2
No. of days	253	252	250	249	246	245	244	243	242	240	236	228	220
No. of cases	4	2	6	1	2	2	2	3	1	11	1	1	1
No. of days	215	213	212	207	195	180	162	150	120	69	57		
No. of cases	1	2	1	1	1	2	1	1	1	1	1		

Total No. of cases 201.

The average number of days at which menstruation occurred previous to confinement, was 256, or 36 weeks.

In two cases menstruation occurred during each month of pregnancy. In the instance in which the patient was in her sixteenth pregnancy, menstruation had occurred regularly during all her pregnancies. She had had 12 children, and 3 miscarriages; had been married 19 years, and nursed all her children.

The period of Quickening in 158 cases.

Days before confinement	60	83	86	90	93	96	100	106	109	110	111	113	114	116
No. of women	1	1	1	1	1	1	2	1	1	3	1	1	1	1
Days before confinement	118	119	120	121	122	124	125	126	128	129	130	131	132	133
No. of women	1	1	6	1	1	2	3	1	1	1	5	4	4	2
Days before confinement	134	135	136	137	138	139	140	142	143	144	145	146	147	148
No. of women	1	5	1	6	3	4	8	1	1	3	5	4	2	2
Days before confinement	149	150	152	153	154	156	157	158	159	160	161	163	164	165
No. of women	1	8	2	2	2	3	2	1	4	4	3	2	2	4
Days before confinement	166	168	169	171	173	174	175	176	180	186	187	188	192	199
No. of women	1	2	1	1	2	1	1	2	2	2	2	1	1	1

The least number of days in which quickening took place previous to confinement was 60—the greatest number of days previous to confinement was 199; of the former was one case, as well as also of the latter. The average number of days at which quickening occurred was 142.

A woman, aged 25, in her first pregnancy, stated “that she had never during her pregnancy experienced nausea or vomiting, nor felt quickening.”

The duration of Labor in 433 cases.

Hours in labor	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	
No. of women	12	28	23	26	41	38	27	22	23	21	12	21	13	10	10	19	5	13	6	6	2	5	
Hours in labor	24	25	26	27	28	30	31	32	33	34	35	36	37	40	41	42	45	48	53	58	70	74	88
No. of women	8	6	3	2	5	2	2	3	1	1	1	4	1	2	1	1	1	1	1	1	1	1	1

The least number of hours any woman was in labor was 1—the greatest number of hours was 88; of the former there were 12 cases, and of the latter 1. A greater number of labors occupied 5 hours than a longer time—and next in frequency were those completed in 6, 2, 7, 4, 3, 8, 10, and 12. The average number of hours was 11½.

In the 5 most protracted cases mentioned above, the mother did well in each instance, and in 3 of the cases the child was saved.

The case occupying 53 hours was one of mere inefficiency of pains.

That case which was terminated in 58 hours was rendered difficult by the presentation of “the right arm with the funis.” The membranes broke without any pain being present, and the patient was not seen until several hours afterwards, when the arm and funis were found in the vagina. The child was immediately turned, but was *still*.

In the case in which labor was delayed 70 hours before its completion, a “sudden discharge of the liquor, without obvious cause,” took place before the os uteri had become sufficiently dilated to expel the child.

In the case in which labor continued 74 hours, *the size of the child alone* appeared to be the only obstruction. The mother, twenty-five years old, had had two children, each of which, according to her account, weighed up-

wards of eleven pounds. She was in pretty good health, the presentation was natural, and she sustained her labor so well that I was unwilling to interfere—and eventually she gave birth, unaided, to a living male child, weighing twelve and a half pounds—the largest child which had been born in the institution.

In the remaining case, which occupied 88 hours, the left foot and head presented—the head considerably higher than the foot. “Some efforts were made to draw down the foot and push up the head, but finding the head most disposed to descend, the foot was supported during the pains, and the head came down.”

The time of Birth.—In 428 cases, 214 occurred between the hours of 7 A. M. and 7 P. M.; and 214 cases between 7 P. M. and 7 A. M.

These results differ from those in 280 cases published by me in the *New England Quarterly Journal of Medicine and Surgery*, in 1843, and also 440 cases published by Dr. Metcalf, in the *American Journal of Medical Sciences*, for October, 1847—in each of which a larger number of births are stated to have occurred during the night than during the day.

Weight of the Children in 406 cases.

Weight,	½lb.	1	2	3	4	4½	5	5½	5½	5½	6	6½	6½	6½	7
Male,	1		1		2	2	2	9	3	8	1	18	8	17	
Female,	1	1	1	1	2	1	3	2	6	5	9	10	9	7	14
Weight,	7½lb.	7½	7½	8	8½	8½	8½	9	9½	9½	9½	10	10½	10½	12½
Male,	11	27	15	28	7	11	2	14	2	12	5	3	1	1	1
Female,	10	25	14	24	6	11	7	7	2	3		3			

The number of males in the above table was 212, whose aggregate total weight was 1669½ pounds; and the average weight of each was 7½ pounds.

The number of females was 184, whose aggregate total weight was 1309½ pounds, and the average weight of each was 7½ pounds.

It will be observed that the weights of the above children fall considerably below those of the cases reported by me in the first volume of the *New England Journal of Medicine and Surgery*, and also of those registered by Dr. Metcalf in his paper previously referred to. I can account for this only by the fact that the great mass of his cases, if not every case reported in those communications, referred to American mothers, whereas the *vast majority* of the mothers of the children, included in the table here presented, were foreigners. That my reasoning is not fallacious, will be perceived by the following table:—

Of the 21 children which weighed 9 pounds, the mothers of 8 were Americans.

Of the 4 children which weighed 9½ pounds, 3 had American mothers.

Of the 15 which weighed 9½ pounds, 9 were children of Americans.

Of the 5 weighing 9½ pounds, 4 had American mothers.

Five of those which weighed 10 pounds, were children of Americans.

The one weighing 10½ pounds, as well as that weighing 12½ pounds, were children of Americans.

In other words, of the 54 children which weighed 9 pounds and upwards, 31 had American mothers; although three-fourths of all the children born in the hospital had foreign parents.

The 4 children weighing less than 3 pounds, were premature and *still*. Those weighing 3 pounds were born alive; one of them “soon after leaving the hospital,” which occurred ten days after its birth.”

Length of Funis.—The most common length was two feet; the next in frequency was 26 inches; and the next 23 inches. The largest was 43 inches; and the shortest $4\frac{1}{2}$ inches.

Presentations in 440 cases.

425 Vertex; 5 Breech; 2 Feet; 1 Face to pubis; 2 Hand to head; 1 Hand to face; 1 Foot and head; 1 Vertex and funis; 1 Arm and funis; 1 Placenta.

The cord was around the neck in 31 of 444 cases. It was *once* around the neck in 21 cases; *twice* in 7 cases; around the neck *twice* and passed under the right arm in one case; *thrice* around the neck in one case.

As regards the child in the 31 cases referred to, it did well in 28 cases. In several cases, it cried immediately upon being born.

In one case where it was resuscitated, the record states "funis twice around the neck, has no pulsation; child was washed in alcohol, and was well in a few minutes."

In the second case, the cord was very tightly coiled around the child's neck, so that previous to the exit of the shoulders, blood flowed freely from its nose and mouth; and the child was not perfectly resuscitated for nearly half of an hour.

Of the three still children, in which the funis encircled the neck, one was of a second labour, weighing $6\frac{1}{2}$ pounds, and nothing appears in the record to account for its death. "There was no pulsation in the cord, nor any other indication of life. The breast was sprinkled with alcohol, friction was employed, with artificial respiration, but without effect."

In the second case, the woman was thirty-seven hours in labour. Four days previous to her delivery, she was attacked *with bearing down pains followed by flooding*—and lost about a pint of blood at that time. The record does not show whether the motion of the foetus was felt after that time or not.

In the third case, the umbilical cord was around the neck, and under the right axilla. This was a second pregnancy, and the woman thirty-four years of age. The child, a male, weighed $9\frac{1}{2}$ pounds. Labour continued 28 hours, and was terminated by the exhibition of ergot.

The above cases serve to corroborate the opinion of Churchill, that when the cord is of the ordinary length, labour is not delayed by its being coiled around the neck of the child. And the experience of Cade,* respecting the foetus being destroyed by apoplexy, produced by the pressure of the funis around the neck—and also the experiments of Negrier "that the umbilical cord is both long enough and strong enough to produce strangulation in a new-born infant, by being twisted around its neck after the head is delivered."

NOTE.—As the question was asked by several gentlemen, upon the reading of this communication, "How did Negrier arrive at his conclusion?"—and as others upon perusing it may be disposed to make similar inquiries, I present the following details from the *Edinburgh Medical and Surgical*

* Reflexions et observations sur l'entablement du cordon ombilical au'our du cou du fœtus.—*Encyclographie des Sciences Medicales*. Avril, 1841.

Journal, April 1st, 1841, page 556: "A girl of bad character was accused of having strangled her child by means of the umbilical cord, before it was completely expelled from the uterus. As there was a difference of opinion among the medical men as to the possibility of the umbilical cord possessing sufficient strength or length for this purpose, Dr. Negrier performed a number of Experiments for the purpose of ascertaining the strength of the cord, and measured it in 166 cases to arrive at its average length.

"Of the 166 cases, it was remarked that in 144 the umbilical cord floated free within the uterus; in 20 it was rolled around the neck of the child: in one it was round the shoulders; and in one between the thighs, the breast presenting in this case: 98 of the umbilical cords were not varicose, and 67 were varicose. As to length, 28 were 17 inches long; 112 were from 17 to 25½ inches long, and 26 above that length.

"The resistance of the umbilical cord was ascertained by attaching weights to one end of the cord until it ruptured, the weights being always attached to the placental extremity. About one-half of the cords were passed by their middle over a round bar, and weights attached till they gave way; the other half of the number were rolled once and a half round the same bar, covered with linen, so as to bring it to the diameter of a child's neck, when it was found that these supported a greater weight than the sound cords, and generally gave way at one of the various dilatations. The mean weight which these varicose cords supported before they gave way was eight pounds Troy; the most resistant supported fourteen pounds seven ounces. The medium resistance of the non-varicose umbilical cords was fourteen pounds four ounces Troy; but one cord required twenty-five pounds to rupture it.

"Dr. Negrier next made a few experiments to ascertain what weights suspended round the neck of an adult would produce such a degree of compression as to cause unpleasant feelings of strangulation. A weight of eight pounds was suspended to a cord passed once and a half round the neck, the back of the neck being upwards. The respiration was rendered difficult, and the brain strongly congested in two minutes. Vertigo commenced soon afterwards. The respiration, however, could be continued with difficulty. When the face was placed upwards, the effects of the congestion were more rapid; the respiration was much impeded, but was still possible; but Dr. Negrier thought that death would have resulted if this position had been maintained for a quarter of an hour.

"When the experiment was made with a weight of thirteen pounds, and the face downwards, rapid congestion of all the vessels of the head took place; the eyes became injected, and filled with tears; the respiration was very laborious, but was still possible. It was, however, dangerous to continue the experiment for two minutes.

"When the same experiment was repeated, but with the face looking upwards, the strangulation was almost complete. Respiration was so impeded that Dr. Negrier thinks death would have resulted in less than five minutes.

"From these facts, he infers that the umbilical cord is both long enough and strong enough to produce strangulation in a new-born infant, by being twisted round its neck after the head was delivered. A force applied to a cord equal to thirteen pounds, would strangle an adult in five minutes, and a much less force would strangle a child.

Instrumental Deliveries.—In 451 cases, eight were delivered by the forceps and two by craniotomy.

Of those delivered by the forceps, six were in cases of the first pregnancy, one in a second pregnancy, and one in a fifth pregnancy.

Five of the eight children were born alive; of the remaining three, one patient was aged 32; this was her second pregnancy, and the child presented with the face to the pubis. After a tedious labor of twenty-five hours, a male child was delivered weighing $6\frac{1}{2}$ pounds.

In a second case, the patient was 32 years of age, and this was her first pregnancy—at the expiration of twenty-seven hours her delivery was completed—the child, a male, weighed $8\frac{1}{4}$ pounds. “It had apparently been dead for several hours.”

In the third case, the mother, aged 24, was in her first pregnancy; after a labor of twenty-eight hours she was delivered of a male child, which weighed 7 pounds.

Still-born.—I find twenty-seven cases of delivery, in which the child was not born alive.

One of those was an instance of arrested development.

Five were cases of premature delivery.

In two cases, craniotomy was employed.

Three were delivered by the forceps.

One was a presentation of an arm and the funis.

Two were presentations of a foot.

One was a presentation of the breast.

In one the mother had constant convulsions for two hours previous to delivery.

One child was putrid when delivered.

In one case no physician was in attendance.

One was the second expelled of twins.

Of the remaining eight cases, two occurred in the first pregnancy, and six in the second pregnancy.

Lacerated Perineum.—I find but four cases of *lacerated perineum* spoken of, and these were all in the first labor.

In one case, the head was delayed a long time by the resistance of the external parts; the pains in the meanwhile were vigorous, and although the perineum was firmly supported, it suffered a slight laceration when the head finally passed. The records read, “it may be well to add that the patient was dwarfish and the child rather large.” The child, a female, weighed $8\frac{1}{2}$ pounds.

In a second case, where the forceps were applied, there was a slight degree of laceration, although the perineum was firmly supported.

In a third case, the laceration extended two-thirds of the space between the vagina and anus. It was probably torn by the passage of the shoulder, which immediately followed the head, while the perineum was unsupported in consequence of the accoucher's attention being directed to the condition of the umbilical cord which was protruded by the side of the head.

The fourth case of laceration occurred in a patient whose perineum was left unsupported to test the utility or inutility of such a course.

Diseases.—In the 451 cases of confinement, six cases of peritonitis occurred; three of convulsions; two of diarrhoea; two of utero-hemorrhage; one of phlegmasia dolens; one of ascites; one of neuralgia; one of typhus fever.

Neither of the cases of *convulsions* occurred during the period of my attendance, and, as I should injure the reports by an abstract, they are copied entire from the hospital records.* All the cases of *peritonitis* terminated favorably, as well as those of *hemorrhage*; that of *phlegmasia dolens*; that of *neuralgia*; and one of the cases of *diarrhœa*. While the case of *ascites*, one of the cases of *diarrhœa*, and the case of *ship fever*, were fatal.

Child.—With respect to the children, I find but little worthy of notice.

One case of *cephalœmatoma* occurred. The mother, aged 29 years, her fifth pregnancy. She was ten hours and a half in labor, and her child, a male, weighed eight pounds. The tumor was upon the right parietal bone. The mother left the hospital a fortnight after her confinement, and nothing is known of her child since.

In one case, death followed *haemorrhage from the cord*. The child, a female, weighing seven pounds and a half, was born after a labor of thirty-six hours. "During the afternoon, blood began to ooze from the extremity of the cord. It was tied again and again without effect."

On the next morning, "the right eye was livid and œdematous. Trunk and limbs œdematous, but not discolored; also various parts, chiefly on limbs. The cellular tissue is almost of a stony hardness. The color of the skin not altered."

On the second day, "the cord has pulsated up to this time. Blood has continued to ooze from the *extremity* until to day; it now flows from the root; the cord beginning to be detached. On the third day, the child died."

The only notices of *distortions* I can find are two instances in which "the right foot was turned inwards, *varus*."

To show that it is not an uncommon circumstance for a woman to be mistaken as to her pregnancy, I would state that, during the four years I was connected with this institution, three women of excellent character entered to be confined, neither of whom was pregnant. One of them, a very industrious Irishwoman, from the fact of her menses being suppressed, and her abdomen having become enlarged, and from the belief that she distinguished "quickenings," made her usual calculations, expended all the means she could spare from her hard earnings to provide clothing for her expected offspring, and with her "permit," came to the hospital. I was called to attend her in her accouchment. She was in bed, expecting momentarily to be confined. Her pains were *false pains*—she was not pregnant.

Upon another occasion, I was called at night to attend two women who were expecting to be sick. One was in labor; the other, an American, had entered the house a few hours previously, and thought she should probably be confined during the night. While the labor of one was rapidly advancing, observing that the other was silent, I jocosely asked her what reasons she had for supposing she should be confined. She answered, "Because her doctor had told her she should be." I asked her if that were her only reason. She said it was a principal one; that her catamenia had been irregular, her abdomen had enlarged; that she had suffered much pain in the region of the uterus; and that her physician, satisfied of her pregnancy, had fixed upon the day of her delivery. I examined her, and found the uterus empty. She was suffering from a congestion of its neck, and was dismissed, to enter the Massachusetts General Hospital for treatment.

*They are omitted in this summary.—ED. BUFFALO JOURNAL.

Instances of mistaken pregnancy are sometimes sufficiently ludicrous. We were once summoned, in haste, to attend a case of labor, and on arriving at the house we met, before we entered the parturient chamber, two female neighbors who had been called to render assistance at the delivery. We found the patient primly sitting in an arm chair, presenting as spare and *collapsed* an appearance as could well be imagined within the limits of health. We were curious to inquire on what grounds she had fancied herself pregnant, and proceeded to ask if the menses had been interrupted, to which she replied that she had been perfectly regular. The only symptom of pregnancy, in fact, was, what she had imagined to be the motion of the child, but how she continued to consider this evidence sufficient, in spite of the most palpable absence of the least enlargement of the abdomen, was surprising; and not less mysterious was the process by which she had arrived at the conviction that her period of delivery was at hand, not having the slightest approach to what could be mistaken for uterine pains.

The Report by Dr. Ramsay embraces an analysis of *four hundred and twenty-three cases* in private practice. Every case terminated favorably. The author distributes them in the following tabular arrangement:—

Aggregate view, No. 1.

		Frequency.	Per cent.
Natural Labor,	- - - - -	5 cases twins, 1 in 94,*	or 1.05
Unnatural Labor,	- - - - -	8 cases,	1 in 59, or 1.69
Complex Labor,	- - - - -	21 cases,	1 in 22, or 4.45
Manual Labor,	- - - - -	3 cases,	1 in 157, or 0.63
Instrument. Labor,	- - - - -	7 cases,	1 in 67, or 1.06

Distinctive view, No. 2.

		EUTOCIA.			
Presentations.		No.		Numerical frequency.	
Vertex,†	- - - - -	429		429	in 473
Feet,†	- - - - -	3		1	in 157
Breech,	- - - - -	4		1	in 118
Face,	- - - - -	1		1	in 473
		DYSTOCIA.			
		Presentations, etc.	No. of cases.	Frequency.	
<i>Complicated, &c.</i>	{	Arm and shoulder,	- - - - - 3	1 in 157	
		Arm, foot and placenta,	- - - - - 1	1 in 473	
		Placenta previa,	- - - - - 2	1 in 236	
		Side and funis,	- - - - - 1	1 in 473	
		Convulsions,	- - - - - 4	1 in 118	
		Tumor,	- - - - - 1	1 in 473	
		Labial effusion,	- - - - - 1	1 in 473	
		Hemorrhage,	- - - - - 2	1 in 236	
		Adherent placenta,	- - - - - 1	1 in 473	
		Hour glass contraction,	- - - - - 3	1 in 157	
		Cord torn from placenta in utero,	- - - - - 1	1 in 473	
		Impacted head,	- - - - - 1	1 in 473	
<i>Manual</i> ,—Cases required version,	- - - - - 3	1 in 157			
<i>Instrumental</i> ,	{	Forceps,	- - - - - 2	1 in 236	
		Required embryotomy or cephalotomy,	- - - - - 5	1 in 94	

* A general average with Clarke, Boivin and Baudelocque.
 † Twin cases not included.

On comparison of the foregoing with the relative frequency of various forms of labor in French and English practice, as presented in statistics by Bodelocque, Boivin, Bland, Collins, and Merriman, the author deduces the following conclusions:—

1. That we do not have unnatural labors as frequently as the French and English, as far as our tables are capable of determining. 2d. That presentations have occurred more frequently to me, than to Bodelocque, Bland, or Collins. 3d. That puerperal convulsions have occurred oftener with me than with Collins or Merriman, although I have previously declared that our ladies enjoy almost an immunity, and attempted to account for it; this unique position will find some atonement by recurring to Dr. Bland's experience in 1897 cases; and it will be fully explained in an annotation at another place. 4th. It will be seen that a lamentable deficiency exists in the tables of Boivin and Bodelocque, involving points of the highest practical and statistical magnitude. 5th. The mortality among French women will strike forcibly the most casual observer, when it is remembered that the forceps are seldom used, and the perforation sacrilegiously interdicted.

Dr. R. advocates the delivery of the placenta prior to the child, in cases of placenta prævia. With respect to the delivery of the placenta in other than placental presentations, he remarks:—"In my own practice I never have a retained placenta. I invariably deliver in 15 minutes after delivery, all things being fair. I have never had any cause to regret the practice, but the experience of every week proves to me its correctness. It is seldom I see a hemorrhage after delivery among my own cases, and I attribute it to the speedy delivery of the after-birth."

He expresses a belief in the hereditary influence of labor so far as *quickness or tediousness of the process is concerned*.

We have reproduced the foregoing additions to obstetrical statistics, in part from their intrinsic value and interest, and partly with a view to their suggesting the collection of data for similar enumerations by practitioners in different sections of the country. The comparison of statistical results contributed from various quarters, with each other, will be curious and useful; and these results aggregated, will furnish a representation of American obstetrical practice, which it will be interesting to compare with the statistics of foreign countries.

On the use and abuse of Alcoholic Liquors in health and disease. Prize Essay. By WILLIAM B. CARPENTER, M. D., F. R. S., F. G. S., etc., etc. Philadelphia: Lea & Blanchard. 1850. 12 mo. pp. 204.

A discussion, by one of the first of living physiologists, of the effects of alcoholic liquors, when taken under various circumstances, in a state of

health, and of their applicability in the treatment of diseases, could hardly fail to possess interest for the medical reader. The discussion is of interest in a purely scientific point of view, as involving questions of hygiene and therapeutics; and, in these aspects, the principles of the use and abuse of alcoholic liquors are by no means so fully settled, as not to leave room for considerable differences of opinion and practice among practitioners. It is, however, the moral relations of the subject that preeminently commend it to the attention of Physicians. To consider it in these relations, it is true, does not fall strictly within the province of medical science. But practitioners of medicine cannot divest themselves, if they would, of peculiar responsibilities, in view of these moral relations. Their position is such that they must almost necessarily exert, by their opinions, example, and modes of practice, an influence either for, or against the evils of intemperance. The abuse of alcoholic liquors, to a certain extent, is correlative to their use—the former is liable to become a consequence of the latter. The latter may be affected in no small degree, by medical authority, and, hence, either for good or evil, this authority must, indirectly affect also the former. If this be a just view of the case, and that it is so can not, as we think, but be sufficiently apparent, then it behooves medical practitioners to study well the hygienic and therapeutic properties of alcohol, so that while they appreciate whatever importance it may possess, as a prophylactic or remedial agent, they may be prepared to discharge with fidelity the duties incident to the relations of their profession to the evils of intemperance. The responsibility is two-fold. We have no right to disparage alcoholic liquors as a means of preserving health, or relieving disease; but the conscience shrinks from any agency in the moral consequences flowing from an over estimate of the efficacy which belongs to them in their physical effects. The grand point, obviously, is to determine with the utmost practicable correctness and precision, the effects of alcohol in health and disease, and to deduce therefrom, in connection with the results of experience, the rules which are to guide its administration as a medicinal agent. The duty of the practitioner is discharged when he has impartially arrived at opinions on these points, after due consideration, with the light of present physiological and pathological knowledge; and when, practically, he acts in conformity with his best judgment, under a proper sense of responsibility as respects both the moral and physical consequences of his professional conduct.

After a careful perusal of the essay by Dr. Carpenter, we commend it most heartily to our professional brethren. And we think that the major-

ity of medical readers will concur in the opinion that the several questions are discussed candidly, as well as ably, and that the conclusions of the article, in the main, are abundantly shown to be in accordance with established principles, in the present condition of medical science.

We quote from the preface the following propositions, embodying the conclusions just referred to:—

In the *first* place.—That, from a scientific examination of the *modus operandi* of Alcohol upon the human body, when taken in a *poisonous* dose, or to such an extent as to produce Intoxication, we may fairly draw inferences with regard to the specific effects which it is likely to produce, when repeatedly taken in excess, but not to an immediately fatal amount.

Secondly.—That the consequences of the *excessive* use of Alcoholic liquors, as proved by the experience of the Medical Profession, and universally admitted by medical writers, being precisely such as the study of its effects in poisonous and immediately fatal doses would lead us to anticipate, we are further justified in expressing that the habitual use of smaller quantities of these liquors, if sufficiently prolonged, will ultimately be attended, in a large proportion of cases, with consequences prejudicial to the human system,—the morbid actions thus engendered being more likely to be chronic, than acute, in their character.

Thirdly.—That as such morbid actions are actually found to be among the most common disorders of persons advanced in life, who have been in the habit of taking a “moderate” allowance of alcoholic liquors, there is very strong ground for regarding them as in a great degree dependent upon the asserted cause; although the long postponement of their effects may render it impossible to *demonstrate* the existence of such a connection.

Fourthly.—That the preceding conclusion is fully borne out by the proved results of the “moderate” use of Alcoholic liquors, in producing a marked liability to the acute forms of similar diseases in hot climates, where their action is accelerated by other conditions; and also by the analogous facts now universally admitted, in regard to the remotely injurious effects of slight excess in diet, imperfect aeration of the blood, insufficient repose, and other like violations of the Laws of Health, when habitually practiced through a long period of time.

Fifthly.—That the capacity of the healthy Human system to sustain as much bodily or mental labor as it can be legitimately called upon to perform, and its power of resisting the extremes of Heat and Cold, as well as other depressing agencies, are not augmented by the use of alcoholic liquors; but that, on the other hand, their use, under such circumstances, tends positively to the impairment of that capacity.

Sixthly.—That, where there is a deficiency of power, on the part of the system, to carry on its normal actions with the energy and regularity which constitute health, such power can rarely be imparted by the habitual use of alcoholic liquors; its deficiency being generally consequent upon some habitual departure from the laws of health, for which the use of alcoholic liquors compensate; and the employment of such liquors, although with the temporary effect of palliating the disorder, having not merely a remotely injurious effects *per se*, but also tending to mask the action of other morbid causes, by rendering the system more tolerant of them.

Seventhly.—That, consequently, it is the duty of the Medical Practitioner to discourage as much as possible the *habitual* use of alcoholic liquors, in however “moderate” a quantity, by all persons in ordinary health; and to seek to remedy those slight departures from health which result from the “wear and tear” of active life by the means which shall most directly remove or antagonize their causes, instead of by such as simply palliate their effect.

Eighthly.—That, whilst the habitual use of alcoholic liquors, even in the most “moderate” amount, is likely (except in a few rare instances) to be rather injurious than beneficial, great benefit may be derived, in the treatment of diseases, from the *medical* use of Alcohol in appropriate cases; but that the same care should be employed in the discriminating selection of those cases, as would be taken by the conscientious practitioner in regard to the administration of any other powerful remedy which is poisonous in large doses.

The foregoing appear to the Author to be the conclusions legitimately deducible from the facts and arguments which he has brought forward; it will be for his Professional readers to decide how far the case which he has made out is sufficiently strong to lead them to the same results. This much; however, he would add: that, when he first entered upon the investigation, some years ago, he had adopted no foregone conclusion, and had, consequently, no temptation to make the facts square with preconceived views; that he has constantly endeavored to treat the subject as one of purely scientific inquiry, and has avoided mixing up any other considerations with those which presented themselves to him as a Physiologist and a Physician; and that, for the sake of keeping himself free from even the appearance of partisanship, he has never allied himself with any one of the Societies which have been formed to carry into practical effect the Total Abstinence principle, but has preferred to follow a perfectly independent course. He ventures to hope that on these grounds he may claim some right to being candidly heard by those to whom this Essay is more especially addressed.

For sale by Phinney & Co., and Derby & Co.

Introductory Lecture to the Winter Course of Instruction in the Philadelphia College of Medicine. Delivered, Oct. 14, 1850. By JAMES M'CLINTOCK, M. D., Professor of Surgery and Anatomy. Published by the Class.

Introductory discourses, thus far, since the “flowering season” has commenced, have been like angel’s visits in that they are few and far between. The lecture by Professor M’Clintock, in fact, is the only one that has, as yet, reached us. Usually the crop has been so abundant that we have despaired of being able to render to all parties even handed justice, and, therefore, to avoid apparent injustice by an invidious discrimination, we have been obliged to serve all alike by simply acknowledging the courtesy which did not overlook us in the distribution of copies.

As the solitary representative of this species of medical periodical literature, we will devote some space to the discourse just mentioned, not wishing, however, that this course shall be taken as a precedent for future action, provided our editorial table becomes crowded, as usual, with similar favors.

The subject of Prof. M'Clintock's lecture will appear from the following extract:—

In choosing a topic for discussion to-day, I have thought it best, instead of dwelling upon broad generalities, to select a single line of remark, upon a subject of wide interest however to you and to our whole profession. My purpose will be to correct a misapprehension which prevails in many quarters in regard to the scope of Surgery, and the relation which subsists between the medical practice of the Surgeon and that of the Physician. Many persons out of our profession imagine that the duties of the Surgeon are confined to the management of external diseases and the performance of manual operations. It readily follows from this notion, wherever it is imbibed, that a man may be a skilful surgeon without being a capable physician; and from this again it is not difficult, for the common mind, by one of those leaps of induction to which it is so remarkably liable, to infer that the skilful surgeon is not *likely* to be a good physician. This, gentlemen, is the misapprehension which I now propose to correct.

Having thus unfolded the topic of the discourse, he next develops the position which it is the object of the remainder of the discourse to advocate and illustrate. We quote as follows:—

It might suffice for my present purpose to show you, from the very nature of surgery itself, that no man is qualified to practice it without a full acquaintance with the other branches of medicine; but I shall go further, and show you on the other hand, that the ordinary routine of medical practice so often requires the aid of that branch of the profession which is technically called surgery, that no man is competent to discharge the duties of the physician fully, who is not at the same time familiar, to a certain extent at least, with both the principles and practice of surgery. It will follow, then, gentlemen, necessarily, that, as a general rule, the *good* surgeon is the *best* physician.

The postulates assumed in the foregoing paragraph are these:—a knowledge of medicine proper, is necessary to the surgeon. A knowledge of surgery is necessary to the physician. Ergo, the "*good* surgeon is the *best* physician."

We cannot entirely concur in the logic by which the lecturer reaches the above conclusion. Since he admits that a knowledge of medicine is necessary to the surgeon, as well as *vice versa*, by the same method of reasoning, we may say that the *good* physician is the *best* surgeon. The rule works equally well in both ways. The author is a teacher of surgery, and

views the subject as it may be gratifying to a surgeon to look at it. His colleague in the chair of medicine, however, we trow, will hardly hold up to the class of pupils attending the Philadelphia College, the pre-eminent importance of close attention to the lectures on surgery, as the best means of acquiring a knowledge of the branches which it is his prerogative to teach.

The converse of the position taken by Prof. M'Clintock would, in fact, be much nearer the truth. The better acquainted a surgeon is with the principles of medicine, the better qualified is he to practice surgery; because in every thing relating to the treatment of external diseases, save what concerns the use of the knife, these principles are involved, not less than in the duties of the physician. And since, according to the maxim of modern conservative surgery, *cutting* is only an alternative when *curing* fails, knowledge of the latter is much the more important of the two. We should vastly prefer to trust life and limb in the hands of the surgeon most competent to cure, than of one whose chief aim was to excel as a dexterous operator. The cultivation and application of therapeutical principles in the treatment of surgical affections is the best safeguard against pruriency in the use of the scalpel.

The ground taken by Prof. M., is not only untenable, and absurd, but, if heeded by medical students, could not fail to prejudice their interests, by encouraging them to undervalue instruction in the diagnosis, treatment, etc., of medical, as distinguished from surgical diseases.

As the lecturer justly remarks, Surgery and Medicine cannot be absolutely separated. They are different branches, with the same trunk and roots. The good surgeon must be a good physician we admit, but it is a *non sequitur* that the good physician must be a surgeon, still less that ability as a physician is commensurate with excellence as a surgeon. The latter is a preposterous conclusion, which we can hardly conceive of any person seriously entertaining, who had devoted much study to the subjects involved in medical practice.

Prof. M. cites the examples of several distinguished surgeons who were successful as medical practitioners. This we admit, but we deny that their *real* success, as practitioners of medicine, depended on their skill as surgeons. The eclat of surgical eminence has sometimes favored the acquisition of medical practice, owing to an impression, somewhat current with the public, that because a person operates adroitly, he can therefore cure diseases better than those who devote exclusive attention to the latter. We are sorry to see this popular error formally promulgated from a surgical chair in a medical college.

Demonstrative Midwifery and C. M.—In the last number of this Journal, we alluded to the publication of a notice of the trial of Loomis for libel, by a writer in the American Journal of Medical Sciences over the signature of C. M., and we offered to reproduce the article in our columns if desired. We had then given the notice but a casual examination. We have since read it with more attention, and we recur to the subject now, to express astonishment, in the first place, that any person, after reading the report of the trial just mentioned, whatever might be his predilections as regards either the subject or individuals concerned, could give as a fair statement of the facts developed therein, the distorted account contained in the article signed C. M. That account, we aver, contains gross misstatements relative to the demonstration by the Prof. of Obstetrics in the University of Buffalo.

Our astonishment, in the second place, is not less in finding, in the article by C. M., the professional conduct of the Prof. of Obstetrics assailed on the ground of unskilfulness in not rectifying the position of the fœtus in the early stage of labor. We have no fear of standing alone when we avow the opinion, that a writer who ventures deliberately to charge a fellow member of the profession with mal-practice, should be prepared with proof, clear and conclusive, to sustain the accusation. We deny that any such proof is contained in the notice by C. M., or that it is in any measure available in the present instance.

For the present we rest with this affirmation and denial, pledging ourselves, in a future No., if occasion seems to call for it, to make good our assertions, and, at the same time, to consider the validity of the arguments offered by C. M. in opposition to demonstrative instruction in midwifery.

Having offered to copy the article from the Am. Jour. of Med. Sciences, if requested, we should state that no intimation of a wish by any one to that effect has been received.

The Report of the trial has been reviewed by the New Orleans Med. Journal, the Charleston, S. C., Med. Jour., the Northern Lancet, the Louisville Journal, and the New Hampshire Med. Jour., each of which takes a decided stand in support of the course pursued by the Faculty of the Med. College of Buffalo.

Dr. I. V. C. Smith.—The Boston Medical and Surgical Journal contains a series of letters from the Editor, Dr. Smith, who is making the tour of Europe. Dr. S. is awake to every thing interesting, and we anticipate the collection of his letters in a volume, which will be read with pleasure and profit by many who do not have access to his Journal.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Report of Clinical Observations on Continued (Typhus and Typhoid) Fever. Based on an Analysis of Fifty-Two Cases.* BY AUSTIN FLINT, M. D., Prof. of the Principles and Practice of Medicine, and Clinical Medicine, in the University of Buffalo.

(Continued from page 401.)

SECTION TENTH.

Symptoms referable to the Genito-urinary System.

The facts, contained in the histories, relating to the subject of this section, are quite insignificant. In the great majority of the cases, the quantity and other characters appertaining to the urine were unobserved. This was in consequence of the difficulty of preserving, in a separate vessel, the evacuation of each patient, and retaining it for my inspection: a difficulty by no means insurmountable, but which, to my regret, was not persistingly overcome, the attempt having been commenced, but abandoned. The present collection of cases, furnishes no data for determining what deviations from the conditions of health ordinarily obtain in continued fever. In a few instances, some unusual symptoms referable to the urinary system presented themselves, attracting notice, and were consequently noted. They are as follows :—Copious lithatic deposit was

observed in *two Typhoid* cases, occurring in *private practice*. In these cases this deposit took place during the whole course of the disease.

In the history of *one* case of *Typhus*, it is noted occasionally during the progress of the disease that the urine was high colored, and without sediment, but two days before convalescence was pronounced, the secretion became more abundant, and deposited a copious lateritious sediment.

In *one* case of *Typhoid* in *private practice*, there was, for a time, inability to void urine, requiring the catheter. The inability in this case did not proceed from unconsciousness, the patient experienced fully the desire to urinate, but was unable to accomplish the act. This was early in the disease, and may, perhaps, have been owing to anodyne remedies, which, as is well known, occasionally are attended with such an effect.

In another *Typhoid* case in *private practice*, there was some difficulty in evacuating the bladder, for a day or two, which, perhaps, was due to the same cause.

In several cases, it was noted that the urine was passed in bed. This was owing to the same mental indifference which accounts for the evacuation the bowels in bed; and the two events generally co-existed.

These few and meagre observations are all that I have to report.

SECTION ELEVENTH.

Duration of the disease. Circumstances attending Convalescence. Sequela. Mode of dying. Fatality.

Duration.—In order to determine, accurately, the duration of the febrile career, it is necessary that the periods of its commencement and termination should be fixed with precision. It has been seen, in a previous section, that it is not easy to decide, in all cases, upon the exact time when the access, or forming stage closes, and the fever begins. The circumstance selected to denote the period of commencement, in the present analysis, is, confessedly, wanting that degree of precision which could be desired. It is, in a measure, arbitrary, and is adopted only because, in the opinion of the writer, it supplies, in the majority of cases, a criterion as accurate and as readily available, as any that may be fixed upon. The circumstance referred to, is a sense of disease or debility sufficient to lead the patient to desire to take to the bed. This marks the date of the fever proper, or of the second stage if the access be included among the stages of the disease.

Now, what shall serve as the index to the day of convalescence? The rule adopted by Louis, is to consider the day of convalescence that on

which the patient begins to take some solid food. In following this rule, however, it is plain that he determines the fact of convalescence, prior to the patient's taking food, from other circumstances. That is to say, when the patient is distinctly convalescent he allows solid food for the first time, and the latter is, therefore, merely an indication of his decision, not, necessarily, a criterion of the fact of convalescence. The applicability of this rule will involve the withholding of solid food prior to convalescence, on the one hand, and that it should be invariably given so soon as convalescence is pronounced, on the other hand, both of which provisions may not be in accordance with the course pursued by all practitioners. Notwithstanding these objections, I am not aware that a better criterion has been suggested. But instead of following the rule laid down by Louis, it appears to me preferable to do what it would seem Louis virtually does, viz: decide on the day of convalescence from an ensemble of circumstances. If the febrile movement, as determined by the heat of skin, acceleration of pulse, etc., have ceased, clearness of the intellect returning, with refreshing sleep, and the patient has a desire for, and relish of food, he may be pronounced convalescent. Some one or more of the above conditions, in some instances, may be wanting, and, still, the other circumstances be such that convalescence may properly be declared. Judgment, and some experience are requisite to decide correctly; and, with every qualification on the part of the observer, it will not infrequently be a matter of some doubt as to the particular day which should limit the termination of the febrile career. Different practitioners would not fix upon the same day in all cases, owing to differences in the mode of estimating the circumstances upon which the opinion is based. Perfect exactitude and entire uniformity, in short, as respects this point, are not practicable: and yet, sensible physicians, in the majority of instances, will act with sufficient correctness for all practical purposes. The exercise of judgment in a similar way, would answer equally well in designating the commencement of the disease, without recourse to an arbitrary event like that which the writer has adopted. But there is this material difference in the two instances, viz., it very frequently happens that cases do not come under observation at their commencement, and hence, it is desirable to fix upon something which will serve to mark the date at a period when we have nothing to rely upon but the history given by the patient or friends. Under these circumstances we can almost always ascertain at what time the patient took to the bed, when we are able to obtain very little information of the symptoms, and history in other respects. On the other hand, a patient does not pass from our observation.

usually, until he is considered convalescent; hence, there is not the same necessity, or advantage, in determining the time of convalescence by reference to an arbitrary standard.

It was an ancient belief, which is not yet entirely obsolete, that there is an intrinsic tendency in Continued Fever to terminate after the lapse of a certain number of days, dating from the commencement of the disease. That the doctrine of *critical days*, as these days were termed, originated in a fancied virtue inherent in the days, or numbers by which they were designated, is evident from the foregoing considerations. The difficulty of deciding the duration, to a day, in a large proportion of cases, renders it impracticable either to establish, or disprove, by observation, that doctrine. This, however, has been attempted within a few years. Dr. Welch, of Edinburgh, Scotland, enumerated the days in 690 cases, collected from different sources, and he found that, of this number, 470 ended on some of the days considered critical. Subsequently Dr. Davidson, of the same place, analyzed a collection of cases, considerably less in number, with reference to this point, and arrived at different results. Statistics by different persons, with an equal allowance of good faith, may be expected thus to differ. The termination, as well as invasion of the disease being seldom so well defined but that one of two or three days might be selected, a disposition to credit or discredit the doctrine of critical days would afford a bias which would hardly fail to determine the preference of critical or non-critical days according to the pre-conceptions of the observer.

In determining the duration of the disease, then, in the cases under investigation, rigorous accuracy is not claimed, but only an approximation thereto, sufficient for all practical purposes exclusive of the doctrine of critical days.

In about one half of the hospital cases (17 of 38) the histories do not contain a definite statement of the commencement of the disease, i. e., of the time the patients had been confined to the bed prior to entering the hospital. In many instances it was impracticable to obtain information on this point, and proper effort was not always made to obtain it. Patients were received at different periods of the disease, and hence, the space of time from the date of their admission to the date of convalescence, is not a fair representation of the duration of the disease. In a considerable number of cases, however, it may be supposed that the differences in the time that had elapsed before entering the hospital would compensate for each other, and, the cases of different groups might be considered uniform in this respect. Hence, the cases of the two types may be compared as re-

gards the duration from the date of admission. The time of convalescence was generally fixed, being determined by so marked an abatement of the symptoms as to denote that the febrile career was at an end. In a few cases, however, this was impracticable, owing to the existence of complications, continuing after the febrile career was ended, symptoms of both being so intermingled, that chronological limits of the latter were not discernible.

Another point of inquiry relates to the time of remaining in hospital, i. e., the number of days, or weeks, from the date of admission, to the date of discharge. Here various causes are operative aside from disease, and the amount of debility remaining after convalescence is established. Some patients are anxious to leave the hospital as soon as possible, and do not remain as long as prudence would dictate; others, having no better asylum, are desirous of staying as long as possible. Still, on the principle of compensation for differences, the cases of the two types may be considered uniform in this particular, and a comparison instituted.

The facts contained in the histories of the cases under examination relating to the foregoing objects of enumeration and comparison, are presented in the following tables:—

TABLE EXHIBITING DURATION, ETC., IN EIGHTEEN HOSPITAL TYPHOID CASES.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	
No. of days from time of taking to bed, to convalescence.					18		18	14						25	31	14			Total 120. Mean 20.
No. of days from admission to hospital, to date of convalescence.				8	8	12	13	12	13	14	10		15	32*	14	21	6	9	Total 187. Mean 13 5-14.
No. of days from date of convalescence to date of discharge, or date of last record.	23													30	13			7	Total 135. Mean 10 5-13.
<i>Fatal cases.</i> No. of days from taking to bed.		26																10	
Do. from admission.	4	15									6							10	
Length of time in hospital.	15	19	17	16	22	16	20	19	16		6	22		44	34	16	16		Total 258. Mean 19 13-15.

* Complicated with pneumonia.

TABLE EXHIBITING DURATION IN TWELVE TYPHOID CASES IN PRIVATE PRACTICE.

	1	2	3	4	5	6	7	8	9	10	11	12	
No. of days from time of taking to bed, to date of convalescence.	16	11	18	about 21	about 28	15	11	10	14	9	5	9	Total 149. Mean 14 9-10.
									Fatal.		Fatal.		

TABLE EXHIBITING DURATION IN 12 HOSPITAL TYPHUS CASES.

	1	2	3	4	5	6	7	8	9	10	11	12	
No. of days from date of taking to bed to date of convalescence.		Fatal. 13th day after invasion.	19	19		9		Fatal on the 7th day.		13	14		Total 74 days. Mean 14 4-5.
No. of days from admission to hospital to date of convalescence.	8	Fatal 8th day.	12	14	18	9	13	Fatal on the 3d day.	13	14	17	10	Total 131. Mean 11 10-11.
				Complicated with parotitis.					Remain'd in hospital 6 weeks. Transferred to surgical department for eschar on sacrum				
No. of days from date of convalescence to dismissal from hospital or date of last record.			16	37	5	9				16	21	11	Total 115. Mean 16 3-7.
Length of time in hospital.	8	28	51	23	22					30	38	21	Total 221. Mean 27 5-8.

TABLE EXHIBITING DURATION IN EIGHT HOSPITAL CASES OF DOUBTFUL TYPE.

	1	2	3	4	5	6	7	8	
No. of days from time of taking to bed to date of convalescence.	9	12	14			14	9	5	Total 63. Mean 10½.
No. of days from date of admission to hospital to date of convalescence.	6	10	9	Fatal 23rd day, complicated with parotitis.	5	9	9	5	Total 53 Mean 7 4-7
No. of days from date of convalescence to date of discharge from hospital, or of last record.	5	5	17		Parotitis supervened.	Parotitis supervened.	Remained in hospital. Sister of Charity.	Sister of charity	Total 89. Mean 17 4-5.
Length of time in hospital.	11	15	26		40	36			Total 128. Mean 25 3-5.

The shortest term of duration, as will be perceived by consulting the tables, was *five* days. In *two* cases, *one* being in *private* practice, and *one* among the *hospital* cases of doubtful type, this was the length of the febrile career. The history in each of these cases leaves no doubt as to the fact that the disease was Continued Fever. In both cases the fever was of a very mild grade, but the diagnostic features were unequivocal. In both instances it seemed most rational to attribute the origin of the disease to contagion. There were exceptional cases as respects duration. In no other case did the fever run its course in a less period than *nine* days, and in all but *five* instances, the duration was longer. The maximum of duration was twenty-eight days, the case occurring in private practice. This case was treated homœopathically for ten days before coming under my observation, the patient being kept on rice water for diet.

It will be observed that the average duration of the *Typhoid* cases in *private practice* is considerably less than the average of those of the same type in hospital, 14 9-10 being the mean number of days in the former, and 20 the mean number of days in the latter. I can offer no explanation of this disparity. It should, however, be considered that the cases in hospital in which the criterion for determining the commencement of the disease was ascertained, are few in number—being only six. On comparison of the average duration of the *hospital* cases of *Typhoid*, with that of the cases of *Typhus*, the latter are found to have a shorter career, the mean number of days being 14 4-5, while the mean number of days of the former, as just stated, is 20. This disparity accords with the results of analyses by others.

The disparity is less, if we compare the hospital cases of the two types as respects the duration from the time of admission into hospital to the time of convalescence. The mean number of days of the *Typhoid* cases from the date of entrance to the date of convalescence is 13 5-14, while the mean number of days of the *Typhus* cases is 11 10-11. The probable explanation of this result is, that in cases of *Typhus* the symptoms sooner assume a degree of gravity to lead the patient or friends to seek hospital relief.

Although the career of the disease is shorter in the majority of cases, in *Typhus*, patients are nearly as long in hospital before convalescence, because they enter sooner than in cases of *Typhoid*.

Again, on comparing the hospital cases of the two types as respects the duration from the time of convalescence to the date of the last record, or discharge, the disparity is in favor of the *Typhoid* type, the mean number of days being in the latter 10 5-13, while in the *Typhus* type it is 16 3-7.

This result would go to show that the system recovers sooner from the effects of *Typhoid* than *Typhus* fever, or, in other words, it shows that the latter form of fever, although shorter in its career, produces a graver influence on the organism. The length of time in the hospital in cases of the two types, corresponds with the above result, the mean number of days in *Typhoid* being 19 13-15, while in *Typhus* it is 27 5-8.

It remains to notice the duration in the cases ending fatally. Of the *five* fatal cases of *Typhoid* type, the duration of the disease in each was as follows:—after the attack, *nine* days in *two* cases; *twenty-six* days in *one* case; *ten* days in *one* case. In the other cases the dates of the invasion were not precisely ascertained. The duration after admission into hospital of the latter cases, were as follows:—*three* days in *one* case; *six* days in *one* case. Mean duration of time the cases were under observation, 8 1-5. Of the *three* fatal cases of *Typhus* the duration in each was as follows:—*three* days in *one* case; *thirteen* days in *one* case; *seven* days in *one* case. Mean duration of time these cases were under observation, 6 1-3. The mean period of duration when the disease proved fatal, is considerably greater in the cases of *Typhoid* than in those of *Typhus*, being $13\frac{1}{2}$ in the cases of the former in which the date of the attack was ascertained, and 8 in the cases of the latter type. In so far as these few cases afford data for results, then, *Typhus*, when it proves fatal, runs a shorter career than *Typhoid*, a conclusion which accords with the observations of others.

Of the *two* fatal cases of *doubtful type*, the duration was in *one*, *eight* days, and in the other, *twenty-three* days,

There are several points of inquiry connected with the subject of duration, in addition to those which have been noticed. One of these is the influence of age upon duration; another is the influence of early or late reception into hospital; the effect of seasons is another, etc. The number of cases in this collection is not sufficiently large for enumerations with reference to these questions.

Circumstances attending convalescence. The circumstances attending convalescence are of interest with reference to the pathological inquiry in how far they may be considered to have contributed to the favorable termination of the disease; in other words whether, or not, they are *critical* in their character. Of the *ten Typhoid* cases in *private practice* which terminated favorably, convalescence in *one* case was preceded by profuse perspiration for several nights; and in *one* case it is stated that convalescence was accompanied by profuse perspiration. In *one* case diarrhoea, which had existed moderately during the progress of the disease, was some-

what increased at the time of convalescence. Of the remaining *seven* cases, it is stated in *four* that no evacuations occurred which could be regarded as critical; and in *three* there is nothing stated on the subject. The occurrence of an event which was suspected to be critical, would hardly escape being recorded; and, hence, it is fair to infer that in all the cases except the *three* first mentioned nothing of the kind was observed.

In *five* of the above *ten* cases, convalescence was established by a gradual abatement of symptoms, and the histories of the remaining *five* cases do not furnish data for determining whether the disease ceased suddenly, or not. In the latter group are included the *two* cases in which perspiration occurred at, or near the time of convalescence.

Of the *fourteen Typhoid hospital* cases which terminated favorably, convalescence was accompanied by perspiration in *two*. In *one* of these cases the perspiration was profuse. In *one* case diarrhoea continued, but not increased. In *one* case, the contents of a small abscess on the thigh were discharged on the day preceding that on which convalescence was pronounced. Nothing is stated of any event which might be supposed to be critical in the histories of any of the remaining cases. In *two* cases, the occurrence of convalescence was apparently postponed by the existence of pneumonitis, and in *one* case, the same was due to parotitis.

Of the *ten Hospital* cases of *Typhus* which terminated favorably, convalescence was preceded or accompanied by perspiration, in *four*. In none of the remaining cases does the histories contain aught which could be regarded critical. In *two* cases, pneumonitis co-existed, and continued after the febrile career had ended; and in *one* case, the same was true of parotitis.

Of the *seven* cases of *doubtful type* which ended favorably, convalescence was accompanied by profuse perspiration in *one* case. Pneumonitis existed at the time of convalescence in *one* case.

In the preliminary analysis of the Hospital cases, pains were not taken to ascertain whether the febrile career ended suddenly, or by a gradual abatement of the symptoms.

The above results show a small number of cases in which there are any appreciable circumstances connected with convalescence, that may be supposed to have contributed to the favorable termination of the disease. The instances in which diarrhoea continued, are hardly entitled to consideration in this point of view. Perspiration, which occurred in *nine* of *forty-two* cases, it may at first be supposed, was a critical event. But when it is considered how large is the proportion of cases in which sweating occurs

during the career of the disease, prior to its occurrence in connection with convalescence, (viz. in *twenty-three of forty-seven*,) there does not seem to be sufficient ground to attribute to it much, if any, agency in determining the probable termination. From the frequency of its occurrence, irrespective of convalescence, it might, perhaps, be expected to occur at, or near the time of convalescence, in about one-fifth of the number of cases, by the law of probabilities, having no connection with the convalescence beyond that of coincidence in time. Assuming that there does exist some connection, other than this, it would by no means follow that the perspiration contributed to the convalescence. The former may be an effect of the latter, instead of being involved in its causation, or both may be effects of the same favorable change in the morbid actions constituting the disease. To discuss the latter points, would be to engage in speculative inquiries; but, as it seems to me, a comparison of the ratio of the occurrence of perspiration in Continued Fever, without any amelioration of symptoms, with the ratio of its occurrence, at or near the time of convalescence, suffices to disprove the supposition that there exists any such connection between these events.

Sequelæ.—Under this heading, I will include occurrences worthy of note, which transpired from the time convalescence was established, up to the periods when the cases passed from observation. The system, after the febrile career, is liable to disorder, or disease, of various kinds, and, at some times and places, it has been observed that particular affections are apt to ensue, showing a predisposition to certain sequelæ derived from the disease through which patients have just passed. No special tendency to subsequent affections characterized the cases in this collection, and, in the great majority of instances, convalescence was not retarded by any untoward event. The occurrences which were noted, are as follows:—

In the group of cases in *private practice*, convalescence was marked by unusual characters in but a single case. In the case referred to, the patient, for several days, exhibited a delirious excitation of ideas, and various delusions, the most prominent of which was, that he had acquired great wealth, that he owned steam boats, &c., and, among other extravagances, he entertained the strange conceit that he had remunerated his medical attendant with a munificent fee! This patient, during the febrile career, did not manifest marked delirium, replying coherently to questions, but he recollected very little that transpired in the course of his illness. The delirium above mentioned became developed after all the other symptoms denoted convalescence, and indeed when he was able to sit up a portion of

the day. The delusions suddenly vanished. He said he thought the matter all over, and discovered the fancifulness of the ideas, which had, for a few days, afforded him so much satisfaction. The discovery did not induce any depression, he continued to convalesce rapidly, and has enjoyed excellent health for the two years that have now elapsed since his recovery. May not the delirium, in this case, have been due to an exuberance of that which occasions the pleasurable sensations incident to returning health?

Of the *Hospital Typhoid* cases, in *one* the formation of a small quantity of pus near the rectum occurred during convalescence. The parts healed readily after the purulent discharge. In *one*, diarrhoea and indigestion occurred, which were of short duration, and were probably due to imprudence in diet.

In *one*, hysterical paroxisms occurred. In *one*, the submaxillary gland became swelled, but the swelling subsided and disappeared in a few days; and subsequently formation of pus occurred on the thigh just below the trochanter, and in the axilla. These abscesses discharged for several days, and the parts then healed, the patient leaving the hospital quite well, thirty days after convalescence was pronounced. In *one*, perspiration occurred for several nights, and on one night slight delirium.

Of the *Hospital* cases of *Typhus*, in *one* the patient seemed dull, and apparently experienced some difficulty in arranging his ideas. In *one* several furunculi on the legs appeared in succession, and, in the same case, during the time of convalescence, the gums were spongy, and bled for a few days. The latter was not the effect of mercurialization. In *one*, a furunculus appeared on the ankle, several days after convalescence, and the patient complained of pains in the loins, which were relieved by the application of a sinapism. In *one*, parotitis followed convalescence, and this was succeeded by erysipelas of the face.

Of the cases of *doubtful type*, in *one*, cough and substernal pain occurred, relieved by a blister. In *one*, vomiting, probably occasioned by imprudence in diet. In *one*, considerable febrile movement was observed on one day; and in *one*, parotitis commenced eight days after convalescence was pronounced.

The foregoing list shows nothing like uniformity in the few sequelæ which were noted, nor were any of the subsequent affections of serious import. It is to be borne in mind that the histories, for the most part, embrace only the time elapsing after the cases came under observation, up to the date of the discharge. Most of the patients were lost sight of, after leaving the hospital. In the instances, which are exceptions to this rule,

it is not recollected that any have experienced disease up to the present time.

In none of the cases did *relapse* occur. I have never witnessed what might properly be called a relapse, after the career of Continued Fever was ended. A recurrence of febrile movement from persistence of some of the complications of the disease, or supervening affections, has been observed, but not a recapitulation of the phenomena necessary to constitute Continued Fever.

I have not noticed in this connection the instances in which complications and symptoms, developed during the career of the fever, continued, retarding recovery, and modifying the symptoms of convalescence. This was the case with respect to pneumonitis in several cases, and parotitis. Deafness, congestive redness, diarrhoea, coated tongue, tremulousness of the tongue, acceleration of the pulse, etc., were observed in convalescence, as well as during the febrile career, but this has been already stated under divisions treating of these affections and symptoms distinctly.

Mode of Dying.—The mode of dying in fatal cases of Continued Fever, is not uniform. I will consider the diversities in this particular, as arranged into two kinds—first, dying by apnoea, and second, by asthenia. Death by apnoea may be occasioned by any cause acting directly to suspend respiration. The cause may be in the respiratory organs, but oftener it is situated at the cerebral centre, destroying the instinctive want of respiration necessary to maintain the involuntary movements involved in that function. Death by asthenia is occasioned by causes acting more directly on the circulatory forces, affecting the *vis nervosa* upon which the contractile property of the heart depends; or depriving the heart of its proper stimulus, the blood, either by diminution of its quantity, or impairment of its quality. These two general modes are not infrequently commingled in fatal cases of Continued Fever. Asthenia may contribute to induce a condition of the cerebral centre, in consequence of which, death occurs from apnoea sooner than it would have resulted from the former mode alone. Both modes are so conjoined in some cases, that it is not easy to decide which really determines the fatal issue. I will proceed to enumerate the fatal cases, giving the duration of each, and the mode of dying which appears most probable from the histories. The circumstances connected with the last moments of life are not always detailed, so that the mode of dying cannot be asserted with positiveness in all instances.

The fatal cases in private practice :—

- 1 (*Typhus*,) Death on the 4th day, by asthenia.
- 2 (*Typhoid*,) on the 9th day, by asthenia.
- 3 (*Doubtful type*,) 8th day, by asthenia.
- 17 (*Typhoid*,) 9th day, by apnoea.

The fatal Hospital cases :—

- 1 (*Typhoid*,) Death on the 3d day after admission, previous duration not ascertained, by asthenia.
- 2 (*Typhoid*,) on the 15th day after admission, and the twenty-sixth after the date of the attack, by asthenia.
- 3 (*Typhoid*,) on the 6th day after admission, previous duration not ascertained, by apnoea.
- 4 (*Typhoid*,) on the 10th day after admission, and date of attack, by asthenia.
- 5 (*Typhus*,) on the 8th day after admission, and 13th after date of attack, by apnoea.
- 6 (*Typhus*) on the 3d day after admission, and seventh after date of attack, by apnoea.
- 7 (*Doubtful type*,) on the twenty-third day after admission, previous duration not ascertained, by asthenia.

Fatality.—The ratio of mortality in fifty-two cases, upon which this report is based, is a fraction over 21 *per centum*. In the Hospital cases, it is 18 8-19 per cent, while in the cases in private practice, it is 29 4-7 per cent.* The ratio of mortality is a fraction greater in the cases of *Typhus*, than in those of *Typhoid*. The proportion of instances in which the disease proved fatal, is not large, judged by the average rate of mortality of this disease, and it is proper to state that the proportion of fatal cases would have been less, if all the Hospital cases of Continued Fever had been included in the collection. A few cases, which were probably cases of Continued Fever, were rejected because there seemed some room for doubt as to the correctness of diagnosis, and a few were excluded in consequence of meagreness of the histories; but every fatal case of the disease is embraced in the analysis.

* In the remarks preliminary to Section First, (page 136) the ratio of fatality is erroneously stated. The reader is requested to make the correction.

SECTION TWELFTH.

Examinations after death.

In the few examinations which were made after death, the object was not to ascertain the appearances of all the organs, in order to gather from this source a complete assemblage of negative as well as positive facts. Time and opportunity were wanting for so minute and thorough an investigation as would be requisite for that object. The more important of the vital organs, in some cases, were inspected; in some instances the dissection was limited to the display of the lesions which are most characteristic of this disease. In the *four fatal cases in private practice* no examinations after death were made.

Of the *seven fatal cases in Hospital*, examinations more or less extensive were made in *five*. The morbid appearances disclosed in these five cases, respectively, were as follows:

No. 1. (*Typhoid*.) Autopsy twelve hours after death.

Head.—Moderate adhesion of dura mater. Arachnoid membrane diaphanous; a little serum beneath the membrane at dependent portion of brain. Some effusion at base of brain, quantity could not be estimated, being commingled with blood which escaped on removing the brain from the skull. Large veins between convolutions, congested. A considerable number of red points on section of cerebral substance. Ventricles empty. Consistence of brain's substance normal.

Chest.—Slight, old adhesions over small space in right chest. Several ounces of sanguinolent effusion (say 5 or 6) in left chest. No adhesion in this chest; no lymph. Pleural membrane presented nothing to attract notice. Lungs on both sides free from morbid appearances, presenting the amount of hypostatic congestion generally found in autopsical examinations; crepitating throughout.

Heart.—Slight effusion of transparent serum. Organ rather below normal size. Left ventricle empty. Left auricle contained a small quantity of fluid blood. Right ventricle contained fluid blood, with some small, soft coagula. Right auricle moderately distended with blood, mostly fluid.

Abdomen.—Colon and cæcum greatly distended with gas, small intestines moderately so. Dividing the ileum close to the cæcum, and, lengthwise, for several feet upward, the glands of Peyer were found to be enlarged, projecting two or three lines above the surface of the surrounding mucous membrane. From fifteen to twenty patches were counted, the enlargement diminishing, progressively, upward. No ulcerations, nor discoloration

perceptible. Mucous surface appeared healthy. Numerous enlarged solitary glands visible, some as large as small peas. Mesenteric glands, in portions of mesentery corresponding to diseased glands of Peyer, greatly their enlarged. At lower portion of ileum they were as large as filberts; enlargement was less and less, in correspondence with the diminution of the size of Peyer's glands upward, and they ceased to be visible at a point corresponding to the limit of the latter. On section on the Mesenteric glands, some presented a red, and some a white color; none were found to contain pus or other fluid.

Stomach.—Presented punctuated redness, or ecchymoses. No capilli-form redness. Size normal. Mucous membrane softened. Several ulcerations varying in size and form, the largest half an inch in length, and three or four lines in width, superficial, apparently having penetrated only the mucous coat. These appearances were limited to the larger curvature. The organ at this part was easily torn, a rent occurring in removing it from its vascular splenic attachments.

Spleen somewhat enlarged, but not softened.

Liver congested. Otherwise normal.

This case was characterized by active delirium, which persisted through the whole career of the fever. The absence of the evidence of encephalic inflammation in connection with this fact, is deserving of note. A report of the case in full, together with the above autopsical appearances, is given in the Buffalo Medical Journal, vol. IV, page 487, et seq. (No. for January 1849.)

No. 2. (*Typhoid.*) The examination in this case was limited to the abdomen, and even here was cursory. The omentum was adherent to the intestines, and the intestinal convolutions were adherent to each other. These adhesions, for the most part, were easily ruptured, but in some situations they were so firm that the force required to separate the adherent parts, occasioned rupture of the tube. In several instances, in separating the parts united by firm adhesions, small quantities of purulent matter escaped.

On longitudinal section of the intestinal tube, recently cicatrized ulcerations of Peyer's glands were apparent near the ileo-cæcal valve. One of these was as large as a quarter of a dollar. The abrupt termination of the mucous membrane formed a ridge surrounding these spaces, defining their boundaries. The parts that had been ulcerated were covered with a delicate, smooth surface like serous membrane. The glands of Peyer, for several feet above the cæcum, presented ulcerated patches varying in

size and form, which were mostly cicatrized as just described, but in some of those situated higher in the tube, the healing process was not completed. The mesenteric glands were enlarged, but none larger than a large pea.

Death occurred in this case fifteen days after admission, and twenty-six days after the date of the attack.

A report of this case is contained in the Buffalo Medical Journal, vol. V, page 271.

The case was interesting in consequence of its affording a fine illustration of cicatrization of intestinal ulcerations completed, and in progress of completion. The patient died evidently of chronic peritonitis.

No. 3. (*Typhoid*.) Autopsy fifteen hours after death.

Head. Considerable congestion of the veins of the superficies of the brain, and numerous red points on section of the cerebral substance. Some effusion into arachnoid sac, precise quantity not ascertained. No effusion into ventricles. No softening of brain. Not the least opacity of arachnoid, nor effusion beneath it.

Abdomen. Colon distended with gas; small intestines collapsed. The latter contained a small quantity of thin yellowish matter. Peyer's glands hypertrophied, so as to project above the surrounding mucous surface; especially an elliptical patch near the cœcum. The ileum in the space of about three feet above the cœcum presented several elevated patches of different sizes. No ulcerations. Mesenteric glands corresponding to the lower portion of intestines, much enlarged. No lumbrici. The stomach, on its internal surface, presented several patches of ecchymoses. The mucous tunic was not softened, but easily detached from the middle coat.

Spleen small, and did not seem softened.

Liver presented nothing worthy of note.

Bladder much distended with urine. Other organs not inspected.

The history of this case from the time it came under observation, up to the termination, is given in full in this report—see page 203. It was regarded as a case of cerebral disease prior to the autopsy. The case affords another illustration of great predominance of cerebral symptoms without encephalic inflammation. It is also worthy of note in this case, that not only was diarrhœa absent, but costiveness existed in an extreme degree, resisting several drops of croton oil. It illustrates the fact that the absence of diarrhœa, and the existence of obstinate costiveness do not afford grounds for an inference that the glands of Peyer are unaffected.

No. 4. (*Typhoid*.) Autopsy, about fourteen hours after death.

Abdomen.—Peyer's glands hypertrophied, and, nigh to the cœcum, a large ulceration of the size of half a dollar. Enlargement of the mesenteric glands, greatest at the portion of mesentery corresponding to the lower portion of the ileum, and gradually diminishing in an upward direction. Hypertrophy of Peyer's glands gradually diminishing in degree, in ascending the tube. Spleen softened. Liver presented no morbid appearance. Other organs not inspected.

This case terminated fatally on the tenth day, by asthenia. The history of this case affords an illustration of the coexistence of costiveness with follicular disease and ulceration. The febrile career was not characterised by any peculiar circumstances.

No. 5. (*Typhus*.)

Head.—Moderate adhesion of dura mater to calvarium. Slight flow of serum on dividing the arachnoid. Arachnoid membrane transparent. Veins situated between the convolutions, injected. A little serum at the base of the brain. A few red points on section of the cerebral substance. No softening. No effusion into ventricles.

Chest.—The lungs anteriorly presented nothing unusual. Posteriorly, they were deeply congested, and solidified. This was greater in some lobules than in others. Some lobules emphysematous.

About an ounce of clear serum in pericardium. Left auricle and ventricle contained a small quantity of fluid blood, without coagula. Right ventricle contained a small, soft coagulum, a portion of which was white; no fluid blood. Size and consistence of heart normal.

Abdomen. Cœcum and colon greatly distended with gas, external appearance of intestines normal. On dividing the ileum, the patches of Peyer, for the space of several feet above the cœcum, were distinctly visible, numbering, in all, about a dozen. The lowest were somewhat softened. The appearance of the mucous membrane, otherwise, normal. A little thin yellowish matter, adherent to the mucous membrane. Spleen not enlarged, but softened. A few mesenteric glands at the lower portion of the mesentery, enlarged to the size of a small pea. Liver presented nothing abnormal. Stomach normal in size, mucous lining of a dingy brown color, notably thickened, and somewhat softened. No redness, nor vascular injection visible.

This case terminated fatally on the eighth day after admission, and the thirteenth after the date of the attack. The mode of dying was by apnoea. An eruption, having the typhus characters, existed over the abdomen,

chest and extremities. Diarrhœa was not present, but slight meteorism and tenderness on pressure. The respirations were accelerated, ranging from 40 to 56. Deep congestive redness of the surface existed, and the conjunctiva was injected. There was present somnolency, muttering, and toward the close of life, the power of articulation was lost. The pupils, also, were somewhat dilated, and their mobility impaired. It is sufficiently clear, from the foregoing summary of symptoms, that the case was one of *Typhus*.

NOTE.—The clinical observations on fever which have been continued, monthly, since August last, will, for the present, be suspended. The subject will be resumed and completed so soon as the writer's leisure will allow him to prepare the remainder, but this, probably, will not be the case for several months. The topics which remain to be considered are the diagnosis, the identity of the two types, and the treatment.

ART. II.—*Obstetrical Statistics, reported by DR. P. O. WILLIAMS, of Gouverneur, N. Y.*

MR. EDITOR:—I send you the results of my observations in the obstetrical cases that have come under my notice for the past three years and six months, ending the 7th November, 1850, which you can publish if you think it proper to do so; not that there is any thing peculiar in my practice, or the report of my cases, but that a Physician's diary in this, and every other branch of the medical profession, is too little attended to for his own benefit, and that of his patients; and the reported cases far too few—which might, if it were otherwise, benefit the profession. Extensive and practical reports might be made by physicians of far more capability and experience than myself.

Whole number of Cases,	216
“ “ boys,	125
“ “ girls,	91
Of these the whole number of abortions were,	5
“ “ “ girls,	3
“ “ “ boys,	2
Whole number of premature labors,	10
“ “ “ “ girls,	6
“ “ “ “ boys,	4

OBSTETRICAL STATISTICS.

The greatest weight of any boy,	14½
The least, excluding abortions,	5
Average weight of boys,	8½
The greatest weight of any girl was,	13½
The least, excluding abortions,	3
Average weight of girls,	7½
Whole number of occiput presentations,	169
" " " girls,	70
" " " boys,	99
Whole number of face presentations,	20
" " " girls,	12
" " " boys,	8
Whole number of breech presentations,	12
" " " girls,	7
" " " boys,	5
Whole number of presentations of inferior extremities,	4
" " " girls,	3
" " " boys,	1
Whole number of presentations of shoulder,	3
" " " girls,	1
" " " boys,	2
Whole number of presentations of superior extremities,	2
" " " girls,	1
" " " boys,	1
Whole number of placenta presentations (or placenta praevia),	3
" " " girls,	2
" " " boys,	1
Presentations of abortive cases not known.	
Whole number of cases of retained placenta,	3
" " " girls born,	3
Whole number of children who died just before, or just after the delivery of mother, not including abortives,	6
The causes of death were malformation of heart,	1
Unknown,	2
From injuries of mother,	2
Monstrosity,	1
Brain and medulla spinalis exposed ; occiput spinous. Transverse and oblique processes wanting. Face resembles that of a toad ; weight 8½ pounds ; and makes a fine addition to my museum.	

The whole number of mothers that suffered from phlegmasia dolens,	6
One limb only,	4
Both limbs,	2
The whole number of cases of puerperal fever,	3
" . " " peritonitis,	2
" " " hysteritis,	2
" " " " mania,	1
The whole number of cases of convulsions before birth of child,	1
" " " " " after birth of child,	1
The whole number of cases of excessive avoidable flooding,	2
All the abortions were produced by a repeated use of emenagogues.	
Of the whole number of cases one mother died ; cause, the absorption of effluvia from decomposed fœtus; inflammation of stomach, bowels, and uterus from repeated use of supposed abortives terminating in hectic and death.	
The least time that any woman was in labor, (I give the time that I was detained,) was, hours,	$\frac{1}{4}$
The longest time, hours,	36
Average time, hours,	$3\frac{1}{2}$

In no case have I used an instrument, and in no case has the mother died, except the one above mentioned.

Gouverneur, November, 1850.

ART. III.—*On the Treatment of Bronchocele by Compression.* By WM. C. DWIGHT, M. D., of Moscow, N. Y.

Although Goitre is by no means common, yet it is not so rare in some districts of our country as not to require attention.

Many cases were brought under my notice when Iodine had become the fashionable remedy, and my patients were advised in regard to its use. All the precautions were taken to have them guarded from the effects of imprudent use of this medicine, yet, more than once, I was forced to witness distress for breath, and palpitation of the heart, which I could attribute to nothing but the Iodine, and this, too, before there was any sensible diminution of the deformity. It was found, moreover, that this was an evil to be expected, as prudence is not common at the age of patients of this class. Under such circumstances, it was desirable to look about for a safer remedy, and it was determined to try pressure. To produce sufficient

pressure without impeding respiration, resort was had to the following mode of proceeding:—

Three straps of good glazed brown cambric were spread with Emp. Ol. Lini cum Plumb. Sem. Vit. Oxidi,* each of half the width of the tumor, and of length sufficient to reach from the lower edge of the scapula of one side obliquely up the opposite side of the neck and across the lower part of the tumor, passing thence onward in return to the upward direction down to the lower edge of opposite scapula, crossing like suspenders. The strap is drawn quite tightly, producing very considerable turgescence of the blood-vessels of the face. The patient will shrug up his shoulders for a few minutes until the Thyroid vessels become compressed sufficiently to enable him to breathe more comfortably, and the countenance resumes its natural appearance. Five minutes is all the time ordinarily required. The second strap is then passed in the same manner across the upper part, from half an inch, to an inch, from the first, according to the circumstances of the case, such as length of neck, size of tumor or situation and form. This strap is drawn as tightly as the first. After waiting until the countenance allows a new application, the third strap is put on in the same manner over the intermediate space in like fashion.

Ordinarily the plasters will adhere in cool weather from ten days to a fortnight, when, becoming loose and non-adherent, they ought to be removed. If the pressure has been well applied, the tumor will be found to have become slightly less, the skin somewhat reddened and tender. In such case it is prudent to wait until it assumes its natural appearance before a new application of the plasters.

The first application has in one case been sufficient, but the average has been as high as four times in each case. When the Bronchocele has become diminished to half its size at the time of the first application, it will continue to disappear without further care. The success which has attended this treatment is such as to warrant confidence. In twenty cases there has been no failure. In the first four, Iodine was used in conjunction with the plasters, and in the twelfth it was used antecedently for several weeks without diminution of the disease. In these cases the progress was no more rapid than when no Iodine was used. In two of the cases the disease returned at the end of two years each, but on a new application of the straps was immediately overcome, and although ten years have elapsed

* I prefer this plaster as I know of no other with equal adhesive property, which produces so little irritation.

ORIGINAL COMMUNICATIONS AND REVIEWS.

since the last application, all is as well yet as though there had never been any deformity. It is proper to add, also, that in both of these cases Iodine was freely taken as well as pressure used at the commencement.

Moscow, Dec. 1850.

ART. IV.—*Case of Spermatorrhœa, treated by cauterization of the urethra.*
By J. Roor, M. D., of Fon-du-Lac, Wisconsin.

T. C., a young man, aged about 22 years, came under my care while residing in Lockport, N. Y., 25th May last. He had been treated for some time previous for nervous disorders, dyspepsia, sleeplessness, worms, &c., but nothing seemed to relieve the symptoms under which he labored. The impression existed among his friends, that he was laboring under *derangement of mind*. He complained of a very uneasy sensation in the region of the epigastrium, sleepless nights, and a roaring or splashing noise in the head. He appeared to think he was laboring under a very severe form of disease, from which he would never recover. He would rise from his bed in the night, take a light, and look at himself in a mirror, declaring he looked like a dead man; in the day time he would stretch himself on a board and declare he should not move till he was laid in his coffin. He was naturally a very strong, athletic man, of a nervo-sanguineous temperament, accustomed to labor. Stated that he had been ill for some two or three months; previously in good health. His present symptoms had come gradually upon him. His countenance was pale, and haggard, and his eyes, glassy and of a wild expression; pulse quick and feeble; tongue furred; bowels costive. He had no appetite, and had been several days without taking any food. I did not carry my investigation of the case so far, on my first visit, as to come to any understanding of the case. I did not, as I should have done, make any inquiries as to the state of the sexual organs, but I have the poor consolation that others, previously in attendance, were as culpable as myself.

I saw him occasionally, till the 17th of June, prescribing some simple remedies which were of no avail, the patient remaining about the same as when I first saw him.

From a remark of his, at this time, I was led to make inquiries in reference to his sexual habits, and he confessed that he had been subject to masturbation for several years, not knowing, he said, its evil effects.

On examining the penis and testicles, they were very flaccid and diminutive in size. The lips of the meatus urinarius were red and swelled. A

thick, creamy fluid occupied the urethra. He had no erections or sexual desires. On the 18th of June, assisted by my friend, Dr. C. Hill, I applied the nitrate of silver, made into an ointment, to the prostatic portion of the urethra with M. Lallemand's port caustique. No pain followed the operation; perhaps the ointment was too weak. In about a week from the first application, I saw him again. His nights were still sleepless, though his mind was less unhappy. Some erections and sexual desires. Lips of meatus urinarius less red and tumid. On introducing a gum-elastic catheter, the urethra was found more sensitive than previously, and there was some difficulty in passing the prostatic portion. At this time I cauterized the whole length of the urethra, with the solid stick of nitrate of silver. Considerable pain was experienced, and the end of the catheter was covered with blood. No unpleasant consequences followed the operation. In three months after this, the patient was completely restored to health, his symptoms having subsided, and cheerfulness of mind having returned.

I am disposed to think this treatment, recommended by Lallemand, too much neglected. Many of those obscure cases, generally considered incurable, which are passed from quacks to regulars, and back again from regulars to quacks, might, I believe, be successfully treated, if more attention were bestowed on the abuse of the sexual organs. The treatment is simple, and if due caution be observed, safe. Without nauseating drugs or severe regimen, a *cure* can be effected by the simple application of nitrate of silver to the affected parts.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS

Case of Extraction of the Uterus. By PAUL F. EVE, M. D., Prof. of Surgery in Med. Col. of Georgia. With remarks, by C. F. MEIGS, M. D., Prof. of Obstetrics and diseases of Women and Children in Jefferson Med. Col. of Philadelphia.

To the Editor :—When Prof. Eve, of Augusta, Ga., passed through Philadelphia, on his return from the meeting of the Association at Cincinnati, he gave me a pathological specimen, which is now in my museum. This specimen consists of the uterus of a woman of color, which was removed by Professor Eve, in the hope that, by such a desperate operation, he might be able to rescue the patient from the imminent death which

seemed by no other means to be avoided. The uterus, which he removed in the manner described in his letter, has been very much changed in its external form by the ravages of cauliflower excrescence.

I do not know that any American surgeon has heretofore extirpated the entire uterus *in situ*—an operation that is said to have been first performed by M. Sauter, in Constance, in 1822.

M. Colombat de l'Iseru informs us that the operation has been executed by Sauter, by Hoelscher, twice by Siebold, and thrice by Langenbeck; four times by Blundell; once by Banner; once by M. Lizars; twice by Recamier; once by Dubled; twice by Roux, and once by M. Delpech; while this operation of Prof. Eve adds one integer to the whole number, which amounts to twenty operations, in all of which the result was contrary to the hopes of surgeons.

M. Colombat expresses the opinion that operations for the removal of the womb *in situ* ought not to be in future performed, in consequence of the disastrous summing up of the statistical records. He does not apply his objections to the cases of incurable inversion of the organ.

There are too many examples of recovery after extirpation of the inverted organ to leave any doubt on the mind as to the hopefulness of such an operation. Still, I have firm confidence in the opinions I have published in other places, as to the power of spontaneous cure of *inversio uteri*, I should hesitate long before resorting to the measures of extirpation. In my friend's operation, there is cause to congratulate him upon the skill and resolution manifested by him, and upon the very hopeful success up to a certain point.

The following extract, from Prof. Eve's communications, will show that, but for the recommencement of the original heterologue development in the vagina, the patient had, in the most remarkable manner, been rescued from death.

I send you herewith an extract of a letter from Prof. P. F. Eve; also a letter from Dr. J. A. Eve; and lastly, extracts from two letters from the surgeon.

Very respectfully, your obedient servant,

CH. D. MEIGS.

"On the 18th of April last, I removed the entire womb from a patient, who has recovered. The operation was performed at my surgical infirmary, in which I was assisted by my cousin, Dr. J. A. Eve, Professor of Obstetrics and Diseases of Women and Infants, and by Drs. Hurray, H. Campbell, Longstreet, and Montgomery, and in the presence of several others connected with the profession.

"The patient is a negro woman, twenty-eight years of age; has been married, but never conceived, as she believes. For more than three years, she has been laboring under uterine affection; at least, she has been annoyed for about that length of time by a vaginal discharge. The history of diseases among our negro population, is generally very imperfect and unsatisfactory; and this is especially true as regards uterine derangements. All we can obtain, in the present case, is, that the patient experienced great irregularity in menstruation, and had frequent hemorrhages from the vagina.

"Yours, &c.,

P. F. EVE."

We now refer to Dr. J. A. Eve's statement of the case, as he observed it before she arrived at the Infirmary, in Augusta.

AUGUSTA, April 24, 1850.

DR. P. F. EVE—Early on the morning of the 10th instant, I was called to visit Mary, the patient whose womb you extirpated on the 16th, in consultation with Drs. Murray and Cook, some eleven or twelve miles from the city.

Under the influence of morphine, which had been given before my arrival, the patient had become easy. On examination, I found a tumor of considerable size in the hypogastrium, and the whole pelvis, to the outlet, filled and blocked up with a lobulated, convoluted, incomprehensible mass, from which issued a copious and horribly fetid discharge.

As this was unquestionably carcinoma, cauliflower excrescence, encephaloid tumor, of some malignant growth, the patient's certain doom was death, after a few months, or at most a year, of miserable existence, worse than death, unless rescued by surgery, in the performance of a heroic operation which would involve the removal of a portion or the whole of the uterus.

If such an operation would ever be indicated or warranted, the age, (twenty-eight years,) the vigor of constitution, and the comparatively unimpaired health of the patient, made it proper in this case.

In consultation, I suggested to Drs. Murray and Cook, that as neither of us could take charge of, or do justice to her case, so far from our respective residences, she should be removed, as soon as practicable, to your infirmary, where she would enjoy every advantage and benefit that favorable circumstances, as well as science and art, could afford her case; and that we should all meet and confer with you after her removal to this place; to which suggestions these gentlemen cordially acceded.

I know nothing of the previous history of this case, except what has been related to us by Dr. Murray. In consultation, all the physicians present concurred in opinion with you, that the operation was one of extreme danger, and the probabilities were as many, perhaps, as a hundred to one against its success.

Before the operation, Dr. Murray and myself visited the patient, explained to her its great danger, and the very great probability that she might not survive it; telling her that, although it afforded but little hope, it was the only hope of delivery from suffering and death. We told her, further, that it rested entirely with herself to determine whether or not she would submit to the operation. Without persuasion or influence of any kind, she determined promptly and unhesitatingly to submit to the operation, terrific as it was represented to be. She is now doing well, and in all probability will return home next week.

Your sincere friend,

J. A. EVE.

Operation.—The bowels having been previously emptied, a large quantity of urine was drawn off by the catheter, which diminished considerably the hypogastric tumor, and proved the bladder to have been generally distended, as there was no urgency to micturation—in fact, the patient was unconscious of distension. About two pints were thus evacuated. Chlo-

reform was now inhaled to its full anæsthetic effects, when the vaginal tumor was seized by various forceps, but which, after large tubercular masses were torn off, was finally brought down to the os externum by the left hand. Finding it impossible to remove the firm resisting body now presented to view, it was carefully excised from above downwards, or in an antero-posterior direction, by the knife—I confess, with some suspicions at the time, it might be the uterus. One artery (now believed to be the left uterine,) throwing out blood quite vigorously, was seized, and an animal ligature cast around it. A solution of sulphate of zinc was applied to restrain further hemorrhage, which had been considerable.

There was no protrusion of the bowels, nor was the case followed by any very severe symptoms. A most rigid confinement to the horizontal position was strictly enforced for about ten days, with absolute diet, &c., &c. The bladder, it is presumed, filling up again, pushed the intestines backwards, while the opening made into the peritoneum was closed by agglutination and subsequent adhesion. The rectum was evacuated on the fourth day after the operation by warm water, and the bowels were moved freely by oil on the fifth.

In the mass removed, the uterus is readily recognized, with its Fallopian tubes, broad and round ligaments; but the os tinææ is involved in the encephaloid degeneration. The tumor in the vagina was about the size of a child's head at full term. No one, it is believed, who has examined it, has entertained the least doubt but that the entire womb was removed, and this includes, besides the gentlemen who witnessed the operation, Dr. R. D. Mussey, Professor of Surgery in the Medical College of Ohio, and Chairman on the Committee of Surgery for the past year in the American Medical Association; and my preceptor, Dr. C. D. Meigs, the distinguished Professor of Obstetrics, &c., &c., in the Jefferson Medical College, with whom the uterus has been deposited, and who has kindly insisted upon presenting the case to the profession in his own way.

During my absence at the meeting of the Medical Association in Cincinnati, the case was left under the care of my relative and assistant, Dr. A. P. Longstreet. The patient returned home on the 3d of May, visited Augusta again on the 20th, to inquire why she had had no hemorrhage (menstruation) since the operation; and, in answer to a letter, Dr. Murray writes, on the 10th of June, that he saw her "up and about" the day before, and promised to bring her, in a few days, to my office.

Fifteenth of June, two months after the operation, the patient, Mary, has called, after riding eleven miles on a loaded lumber wagon. She is much improved in flesh and appearance, and has enjoyed good health. She says there has been a slight show of blood but once since the operation, and only a moderate discharge at times of colorless fluid. But I regret to add, we have most unmistakable evidence, both ocular and by touch, of a rapid reproduction of the encephaloid disease, which, in all probability, must sooner or later destroy life.

Extract of a letter, dated—

Augusta, July 20th, 1840.

My Dear Doctor—I write to say, Mary, my *non-uterine patient, is dead.* She died on the 22d of July, having lived three months and a week after

the operation. She became cedematous, (ascites, also,) but had no hemorrhage, neither protrusion of the diseases from the os externum. I regret no post-mortem was made by the physician in attendance, and I only learned her decease incidentally at the time.

PAUL F. EVE.

Dr. C. D. MERRIS.

Am. Jour. Med. Sci.

Quackery.—We propose, from time to time, to speak of the different forms of Quackery, in the profession as well as out of it. In the Boston Transcript we find some articles upon this subject, from which we offer extracts instead of our own thoughts. It is understood that the writer is a lawyer, his *nomme de plume* being “a sexton of the old school.” In No. 113, of “dealing with the dead,” he says:

“There is nothing marvellous in the existence of quackery, if we recognize the maxim of M. Sorbier, in his *Relation d'une Voyage en Angleterre*, p. 155, *homo est animal credulum et mendax*—man is a credulous and lying animal. David said, that all men were liars; but, as this is found in one of his lyrics, and he admits that he uttered it in haste, it may be fairly carried to the account of *poetica licentia*. With no more, however, than a moderate allowance for man's notorious diathesis, towards lying for pleasure or profit, it is truly wonderful that credulity should preserve its relative level, as it does, and ever has done, since the world began. Many, who will not go an inch with the Almighty, without a sign, will deliver their noses, for safe keeping, into the hands of a charlatan, and be lead by him, blind-fold to the charnel house. Take away credulity, and the world would speedily prove an exhausted receiver for all manner of quackery.”

The following, we apprehend, gives the true explanation of the fact that educated men are so often found to encourage empiricism. We once availed ourselves of the columns of a religious newspaper to protest against a laudation of hydropathy which had appeared in the same journal. Afterwards we received a dogmatical *admonetur* from the reverend editor, which not only afforded us amusement, but convinced us that he too knew nothing about the matter.

Ignorance is the hot-bed of credulity. This axiom is not the less respectable because the greatest philosophers occasionally place confidence in the veriest fools, and do their bidding. Wise and learned men, beyond the pale of their professional pursuits, or peculiar studies, are, very frequently, the simplest of simple folks—*non omnia possumus omnes*. Ignorance must be very common, for a vast majority of the human race have not proceeded so far in the great volume of wisdom and knowledge, as that profitable but humiliating chapter, whose perusal is likely to stimulate their energies by convincing them that they are of yesterday and know nothing. Credulity must, therefore, be very common.

But our sexton continues to discourse in this way:

In a dissertation before the Medical Society, in June, 1828, Dr. George Cheyne Shattuck, after setting forth a melancholy catalogue of the trouble and perplexities of the medical profession, concludes by saying, that, “all these trials to which the physician is subjected, do not equal that which proceeds from the *uncertainty* of the healing art.” When we contrast this candid avowal from an accomplished and experienced physician, with the

splendid promises and infallible assurances of empirics—with their balms of Gilead, panaceas and elixirs of everlasting life—we cannot marvel that the larger part of all the invalids, in this uncertain and credulous world, fly from those conservative professors who promise nothing, to such as will assure them of a perfect relief from their maladies, no matter how complicated or chronic they may be—with four words of inspiring import—NO CURE, NO PAY.

I am no physician; my opinion, therefore, is not presented *ex cathedra*: but the averment of Dr. Shattuck is, I presume, to be viewed in no other light, than as the opinion of an honorable man, who would rather claim too little than too much for his own profession; who would rather perform more than he has promised, than promise more than he can perform. If the regularly bred and educated physician complains of uncertainty, none but a madman would seek for its opposite in the palace or the kennel of a quack—for the charlatan may occasionally be found in either.

The first thing to be done, I suppose, by the regular doctor, is to ascertain what the disease is. This, I believe, is the very last thing thought of by the charlatan. He is spared the labor of all pathological inquiry, for all his medicines are fortunately, panaceas. Thus he administers a medicine for the gout; the patient does not happen to have the gout, but the gravel; it is the same thing; for the physic, like our almanacs, was calculated for different meridians."

It is most refreshing to us, to find educated men thus speaking out the truth on matters concerning which they often err. We would desire to express our thanks for every blow struck in our favor by those who ought always to be with us, but are too ready to oppose. From the nature of the case, their influence is far greater than that of a physician, to whom, perhaps, not without seeming justice, it is objected that it is his craft that is in danger.—*New Hampshire Med. Jour.*

Observations on the use of Cod Liver Oil, Matico, Chloroform, Tannin and Iron. By N. W. CONDUCT, M. D.

I take the liberty of submitting to the readers of your Gazette the following paper, which contains the result of my experience in the use of some of the drugs which have come into use in the course of the last few years. The principal articles are Cod Liver Oil, Chloroform, the Matico leaf, Tannin, Subnitrate of Bismuth and Citrate of Iron.

The Cod Liver Oil I have used in about thirty cases of tubercular consumption: in many cases of chronic Bronchitis; in some old cases of chronic rheumatism; in one very intractable case of Salt rheum eruption, covering nearly the whole body; in every variety of chronic inflammation of the mucous membrane; as a local application, combined with chalk or calamine powder, and in nearly every case with marked benefit. In consumption the patients who were too far gone to be cured, have in every instance been saved the aphthous sore mouth, and diarrhoea, and also the paroxysms of hectic fever, which almost universally attends the close of life in this disease.

Three or four of the cases of branchitis had gone through the whole round of topical applications, blisters, leeches, tartar emetic ointment, Dr. H———G———'s nitrate of silver swabs, &c., &c. But the oil, with the aid of cold water bandages to the neck, has left the patients in every case apparently cured. In some cases the stomach does not tolerate the oil, unless it be given after eating and then qualified by the elixir of Peruvian bark, or by wine, or some similar adjuvant. Where phthisis has advanced to its last stage, and the patient has hectic and diarrhoea, the bismuth in scruple doses, and the cold water bandages to the abdomen are of great service as adjuvants. If, at the same time, as is usual, the patients have much thirst, I encourage them to drink all the cold water they may incline to do, as it greatly aids, as I suppose, by its solvent power in the elimination of the hus, taken from the lungs into the circulation.

In the case of salt rheum, its value, as a topical application, was well proved by using it for two or three days on one side of the face, and not upon the other; upon one wrist, and not upon the other; and by varying it in this way many times over, and always with the same result.

Chloroform I have used in about fifty cases of labor, and always with the happiest effect—when used even while ergot is given, the operation of both goes on as if only one had been given. In Hysteria, or any other form of disease where convulsions supervene, its effect is instantaneous.

Taken in the incipient stages of inflammatory disease of any kind, by its sedative action upon the nervous system, and by withholding for the time it is being used, the supply of oxygenated blood from the tissues, it does much to smother the kindling up of the flame. It has been thought unsafe to be used in diseases of the lungs, but I used a pint of chloroform, in the case of a lady who died of consumption, in the course of the last twenty-four hours of her life, and by it saved her from all the ordinary suffering which marks that sad hour. In the convulsions of infants, I esteem it of more value than all the other remedies we have before known. In idiopathic neuralgia its use, if persevered in, will, I believe, always effect a cure. From experience in my own person, I can say that it has cured me of this disease, when morphine in doses of one grain repeated every hour, for six hours, had scarcely a perceptible effect. That disease so troublesome in children, Catarrh, gives way to two or three inhalations. To relax the muscles for reducing a fracture or dislocation, it acts to entire perfection—nor to do this need the patient be made at all insensible.

The only caution required is, not to give it upon a full stomach, nor exclude entirely the atmospheric air from passing into the lungs.

Of the Matico leaf and Tannin I will only say that they are most astringent.

The Matico leaf is powder, in tincture, in decoction, or in infusion is quite as potent as alum, Acetate of lead, or sulphate of Zinc. The Tannin is scarcely at all inferior to it.

The Sub-Nitrate of Bismuth, to check diarrhoea or acidity of stomach, no matter from what cause, is of very great value, but needs to be given in doses of a scruple to a drachm, and repeated as often as occurrences of symptoms calls for it.

The Citrate of Iron is an elegant preparation, and decidedly pleasant to the palate, and at least fully equal in virtue to either of the chaly beata.—*N. Y. Med. Gazette.*

Auscultatory Sign of Enlarged Liver.—Dr. WALSHE has described in the *Lancet*, a stethoscopic indication of enlarged liver, under the name of "hepatic compression rhonchus."

"It co-exists with inspiration only, or, indeed, seems to be rather super-added to it, not commencing until the inspiration-murmur appears almost at an end. Its evolution is peculiarly slow, drawling, and (if I may be allowed the expression) lazy, being, in this respect, the exact reverse of that of the crepitant rhonchus of pneumonia. It consists of a variable (but commonly a great) number of excessively fine, dry crepitations, rather superficial than deep-seated; is rendered audible by forced inspiration only, and may be heard in front, at the side, and in the back of the right half of the chest, (least commonly in front, however,) at, or near to, the upper edge of the liver. Its existence is completely independent of any lung-affection; and I have never found it on the left side, in these cases of liver-enlargement. The characters of this rhonchal sound are so peculiar, that a mere tyro would be able to distinguish it from all varieties of rhonchus—it differs essentially, as I have just proved, from crepitant, subcrepitant, dry crackling, pulmonary rhonchi, and from pleural rhonchus. Of its mechanism, I am not prepared, at present, to offer any demonstration; but it appears to me to be most feasibly explicable as follows:—The lower portion of the lung, pressed upon by the enlarged liver, undergoes a sort of creasing, or condensation, which, in ordinary breathing, interferes with their expansion. By forced inspiration, the portion of lung implicated will readily be understood to be *uncreased*, and so conceivably a series of sounds, such as I have described, is produced. Another fact strongly corroborates this hypothesis—namely, that it often ceases to be audible, for a time, after from one to five or six forced inspirations; the lung seems to require rest and time to be again crease up. Should further experience confirm this view of its mode of production, we shall have collateral support given to the doctrine I have long taught (and which, so far as I know, has not been refuted,) that the crepitant rhonchus of pneumonia is formed, not in the air cells or capillary bronchi, but in the pulmonary parenchyma itself. I am not able, as yet, to make any positive assertion, concerning the frequency with which the rhonchus under consideration attends enlargement of the liver; but, on the other hand, I am in a position to affirm, that in no single case of notable increase of bulk of that organ which has fallen under my observation, since my first discovery of the rhonchus, have I failed to substantiate its existence. The sound may, it is true, escape detection on one or more occasions, but has never been absent for a series of days. On the other hand, I have not met with it in other conditions of disease; though doubtless, if my theory concerning its formation be well founded, it will probably be ascertained to accompany a variety of conditions, causing slight compression of the lungs."—*London Jour. of Med.*

Foreign Body in the Trachea.—In July last, Mr. J. A. Dobie, of Hanover, N. H., while making an application to his throat by means of a sponge, lost his hold upon it and drew it into his trachea. It could be perceived that the sponge moved from the bifurcation of the bronchi to the larynx. The dyspnoea at times excessive, occasionally disappeared entirely. Tracheotomy was performed by Prof. Dixie Crosby, and the sponge removed. The patient

died within forty-eight hours after the operation. The sponge is described as being about two inches long by one wide, and about half an inch in thickness—a size which we should have supposed would entirely preclude its passage through a healthy larynx,—(and such the patient's is said to have been,) or after reaching the trachea, would have prevented it from moving. The fact is interesting, considered in connection with the discussion upon the feasibility of making applications directly to the larynx.—*New Hampshire Jour.*

On a new Method of opening Abscesses, without leaving visible Cicatrices. By M. LERICHE, Physician to the Lyons Dispensary.—The inconveniences daily met with in opening abscesses by incision, or by the application of caustics, have induced me to seek a less objectionable method of effecting this object. My principal aim has been to avoid the permanent marks left by the means hitherto employed, a point of much importance when the abscess occupies the neck or bosom of the female. I shall be much gratified if I can prove to the profession, what I am myself convinced of, that if my results have not been crowned with complete success, my patients have at least been often spared the dread which always attends the use of cutting instruments.

Although the method which I propose is apparently sufficiently simple, I have not arrived at it without repeated trials, both of the use of different materials and of the mode of operating. I shall, however, give in a few words the result of my researches.

My first idea was to employ wires of iron, silver, or lead; the results were tolerably satisfactory, but their use was liable to three objections:

- 1st. The difficulty of procuring them everywhere.
- 2nd. The necessity of having a special instrument for their introduction, and the rather acute pain which the operation occasioned.
- 3rd, and lastly, the contact of a hard substance, irritating the inflamed and already painful tissues.

I also tried threads of hemp, linen, and cotten; all were liable to a serious objection, which induced me to discard them altogether; they became swollen by the moisture in which they were constantly immersed, and thus opposed the exit of the pus. I remarked, also, that their employment gave rise to a rather acute inflammation around the openings. Might not this be attributed to the facility with which these substances become altered in their nature. It also occurred when the threads were previously waxed.

Silk thread is the material on which I have decided, from its having the following advantages over the others:—1st. It is to be had everywhere. 2ndly. It is not liable to become altered during the time it is required to remain in the abscess. 3rdly. It does not absorb the moisture. 4thly, and lastly, it does not irritate the painful parts with which it is in contact. The silk thread which I use is known in the shops under the name of twist (cordonnet).

After having shaved off any hair on the tumor, the surgeon takes a curved ligature needle, passes through its eye one of the silk twist, then introduces the needle into the tumor, about six or eight lines from the most depending part, where it must be brought out, draws the thread into the passage formed by the needle, and retains it in this position by uniting the

two ends in a knot; the entire tumor is now covered with an emollient poultice, which, in this case, acts mechanically. The patient should remain as little as possible in bed, in order to favor the escape of the pus along the thread, an effect which takes place with difficulty in the recumbent position, when the abscess is situated on a part of the trunk or limbs. The poultices have also, in this case, the advantage of diminishing the inflammation which is excited, and which the practitioner must watch. The twist is to be left undisturbed for four, six, or eight days, according to circumstances; most frequently four days have sufficed. Subsequently, when thought advisable, the twist is removed, and the part is dressed with dry compresses, or, when necessary, with compresses soaked in aromatic wine. In not one of thirty-three buboes which had arrived at the stage of suppuration, and which had been treated in the manner just described, have I been obliged to abandon this plan for any other. In cases of simple buboes, that is, those in which the pus did not seem to possess specific characters, the cure has been effected in from fifteen to twenty days; in the opposite cases, when the orifices of the little openings ulcerated, it has occupied from forty to fifty days; and in neither case does the patient retain any trace of syphilitic infection.

When the tumor has been tardy in reaching the suppurative stage, the pus is sometimes contained in cellular pouches, in which case it may happen that at the time when one thread seems to have effected the cure of an abscess, another forms; under these circumstances a second thread must be introduced.

In other cases, the thread has brought on severe inflammation and caused intense pain. When this occurs it must be removed, and lightly astringent unctuous applications, such as Goulard's cerate, substituted for the poultices; to these should be added the employment of general measures, baths, regimen, &c., &c.

But the selection of the means most suitable to combat the symptoms which may have arisen must, in such cases, be left to the judgment of the practitioner.—*Dub. Quar. Journal, from Revue Médico-Chirurgicale de Par.*

QUERE.—Why would not the exploring canula, attached to a suction pump, in the mode proposed by Dr. Bowditch, to evacuate pleuritic effusion, (see Buffalo Med. Jour. No. for November) answer capitally for removing the contents of abscesses, and avoiding cicatrices? [*Ed. Buffalo Jour.*

Remedy for Ferociousness in a Dog.—In one of the Cincinnati papers, we find an account of an attack of a ferocious dog upon a little child. "The dog seized the child by the throat," we are told, "and the more he was pounded so make him let go, the harder he held on. The people broke the dog's back, and after inserting a lever into his mouth, pried his jaws open and released the sufferer, but not till her throat was mangled." There is a *sure* remedy in such cases, which should be known by every one. We hear of the case often, and it would seem that persons at these times are very apt to forget the disposition of the animal. Now, if instead of pulling upon the dog, to disengage him when his jaws are set upon anything, a sponge or cloth, wet with *strong spirits of hartshorn*, be applied to his nostrils, he will instantly relax his hold.—*Boston Med. and Sur. Jour.*

On the Employment of Tartar Emetic to Relieve Rigidity of the Cervix and Os Uteri, in cases of Parturition. By ARCHIBALD HALL, M. D., L. R. C. S. E., Lecturer on *Materia Medica*, *M. Gill. College*; Consulting Physician to the *Montreal General Hospital*, and to the *University Lying-in Hospital*, &c.

Of the various causes which tend to complicate a labor, and render it tedious, discouraging to the accoucheur, and harassing to the sufferer, few are of more common occurrence than rigidity of the os or cervix uteri—few, the removal of which is generally more readily effected, and in which timely relief obviates such unpleasant consequences.

The object of the present paper is not to propose a new method of relief under the circumstances mentioned, but to call attention to, and re-produce a means which has been strangely and inconsiderately neglected; which is not dwelt upon at all in most of our works of authority, or does not occupy that position which its merits so eminently deserve.

Rigidity of the os uteri consists in a deficient dilatation of the muscular fibres of that portion of the uterus, coupled with heat, dryness and tenderness, in proportion to the duration of time in which it has existed. If the rigidity has been of short duration, these latter phenomena will not be found to exist; but if of long duration, they will, by their presence, indicate the existence of local inflammation to a greater or less degree. This unequal or irregular contraction of the muscular fibres of the neck of the uterus, depends originally upon irritation of those fibres locally applied, which may, if not relieved, end in inflammation—which, in its turn, proves a secondary exciting cause; and in order, therefore, to obviate a serious complication in which might have been otherwise a perfectly natural labor, the earlier this source of difficulty is detected the better, as the more conducive to the practitioner's reputation, and the safety and comfort of the patient.

Pathologically examined, rigidity of the cervix uteri consists in a *spasmodic contraction* of the circular fibres of the locality specified, and is induced by causes of an irritant nature.* The cause most frequently in operation, is the early pressure of the child's head upon the whole cervix,

*Dr. W. Tyler Smith, describes the opening of the os uteri, as depending "partly upon the mechanical distension of the non-contractile tissue, and partly upon the muscular dilatation of the contractile fibres which enter into the composition of the os and cervix uteri;" and applying this fact to the explanation of rigidity of the os uteri, he shows that the latter consists either in the absence of distensibility, or of dilatability, or in both of these states combined. * * * "In numerous cases, both the muscular and mechanical forms of rigidity exist, and mechanical rigidity is itself sometimes a cause of spasmodic closure of the os uteri. The heat and irritability of the os uteri render it morbidly excitable, and the presence of the liquor amnii, or the presentation instead of exciting a reflex dilatation of the mouth of the uterus, excites it to spasmodic contraction."—*Braithwaite's Retrospect*, Vol. 19, from *Lancet*, Nov. 25 1850.

"This unfavorable state of the os uteri, [rigidity] may be discovered to exist at the very commencement of its dilatation, or may not occur until the process of dilatation has somewhat advanced; in the former case it is the result of a premature rupture of the membranes, in the latter, most probably owing to a spasmodic contraction of the cervical fibres, produced by the irritation of unnecessary and too frequent vaginal examinations, or the effect of pressure on the cervix between the child's head and bony pelvis, &c., &c."—*Dr. A. Tyler's Lectures on Practical Obstetrics in British Record of Obstetric Medicines*, Vol. 2.

or a partial pressure exercised upon the anterior wall, operating upon that portion lying between the presenting part and the pubis. Under the latter circumstance, the spasm may be partial, involving only the anterior lip of the cervix; under the former circumstance, the spasmodic action may be complete, and in the majority of cases, will be induced by the early rupture of the membranes, in either an artificial or spasmodic manner, with a general pressure of the child's head, or other presenting portion, upon the cervix, thereby compressing it upon the brim of the pelvis.

Cases, however, are recorded in obstetric works, in which the rigidity has depended upon a *cartilaginous* degeneration of the muscular fibres of the cervix, and upon cicatrization, the consequence of sloughing from injuries which that part of the viscus may at some antecedent period have received. Such cases, fortunately very rare, require a specific line of treatment by the knife. The fact is here only worthy of notice as having originated a division of this peculiar complication of labor into the two varieties of *symptomatic* and *idiopathic*.

Flowing from the idea, perfectly correct, that the irritation is likely to be succeeded by inflammation, unless quickly relieved, most obstetric works recommend, to overcome the difficulty, the practice of venesection, or the exhibition of opium.* But few authors advise a resort to tartar

*When the orifice is rigid, undilated and undilatable, we should employ to overcome this resistance, the various means I have already suggested; bleeding if the patient be phlethoric, baths, unctions on the cervix, with the extract of belladonna."—*Chailly's Midwifery, Am. Ed. 1844, page 273.*

"When there is much vascular excitement, &c., it is almost always advisable to abstract blood. The quantity must, of course, be regulated by circumstances; but as a general rule, I would say, let it be the minimum required. I think I have observed considerable benefit to result in such cases as these, from the employment of nauseating doses of tartar emetic, in conjunction with, or as a substitute for bleeding; but there certainly are circumstances under which the latter cannot safely be dispensed with. Opium has been much recommended as a relaxant, but it is a medicine, the effect in particular, we cannot accurately measure. . . . Other means, as fomentations, the introduction of tallow into the vagina, application of belladonna to the os uteri, injections of tobacco, the warm bath, &c., have been recommended; but of their effects I know nothing from experience, and *a priori* see no reasons that can sanction their use."—*The Dublin Practice of Midwifery, by H. Maunsell, M. D., 1845. Page 142 and seq.*

"Nauseating remedies, and even tobacco injections have been tried to a considerable extent, for the purpose of relaxing the mouth of the uterus, but they produce little or no good effects, and cause much suffering to the patient."—*Library of Medicine, Vol. 6, page 199.*

"For myself, in endeavoring to effect a relaxation of the soft parts, fomentations and bleeding are the remedies to which I principally confide."—*Principles and practice of Obstetric Medicine, by J. Blundell, M. D.*

"For rest of body, tranquility of mind, the abstraction of stimuli, the loss of blood, free bowels, and not allowing the soft parts to be disturbed by ill-timed and officious touching, or ill-conceived manual aid at the mouth of the uterus, have, in a thousand instances, overcome every difficulty presented by simple rigidity."—*Dewee's system of Midwifery, 1847, page 302.*

"If the patient be strong, phlethoric, and disposed to make violent straining efforts, a free depletion from the arm will be of use; it diminishes the tendency to inflammation, and produces a feeling of exhaustion in the patient which induces her to bear her pains more patiently. In order to produce such an effect, depletion may be followed by tartarized antimony, in small doses, so as to excite nausea, &c."—*Lectures on Natural and difficult parturition, by E. W. Murphy, M. D., 1846, page 128.*

The multiplication of authorities is, I think, unnecessary.

emetic; and in most of the works of authority to which I have had access, they recommend it as an *adjuvant* to the venesection, and by way of securing the effects of the bleeding upon the system. The local application of belladonna has also been advised by Mr. Dubois of Paris.†

Therapeutic agents are of value exactly in accordance with the effects which, under peculiar circumstances, they are capable of producing. This is apprehended to be an axiom in medicine, and by which the relative value of medical agents may be accurately measured. I propose now to examine these several modes of treatment, keeping the above axiom in view.

1. With regard to venesection. This seems to be the established rule of practice, and is sanctioned by every author of note. Bleeding, in its influence upon the system, is one of the most certain and powerful sedatives which we possess, and presents strong claims to our consideration, under the peculiar circumstances of the case. To be of any real value, however, it requires to be vigorously practiced, and relaxation will be found rarely to follow, until a large quantity of blood has been extracted, and syncope is on the point of supervention. There can be no question that the best effects follow its employment, as far at least as rigidity is concerned. But is this line of practice always expedient? Is it always imperiously demanded? When the os uteri exhibits heat, dryness and tenderness, such effects being the evidence of existing inflammation, the propriety of the practice cannot be questioned. But before the development of these symptoms, several hours may elapse, during which the effects of irritation alone are apparent as indicated by the mere rigidity. Under these circumstances, bleeding is *not* imperiously demanded, chiefly because it entails an unnecessary withdrawal of blood, inducive of debility, and protracted recovery—thus effecting more than we desire. The practice, however, is recommended and sanctioned by Burns, Dewees, Blundell, Ramsbotham, Chailly, Cazeaux, and a host of others.

2. The exhibition of opium is not always attended with the advantages which we might *a priori* be led to expect from its well known narcotic powers. If exhibited, full doses should be employed; but as in the case of bleeding, it is liable to effect too much—it may lull the uterine contractions, which it is generally desirable to expedite.

3. The local application of Belladonna. This was first proposed by Chaussier, who suggested its use in the form of ointment made with cerate, reasoning analogically from the influence of this medicine upon the iris. Dubois subsequently used the extract in its undiluted state. The practice is peculiarly French, and has not been followed to any extent by either British or American accoucheurs. That the application of the belladonna will produce relaxation, is admitted on all hands; but the extent of that relaxation cannot be predetermined. It may affect the whole muscular coat of the uterus, and be thus productive of alarming consequences.

4. Tartar Emetic. The prostration and muscular relaxation produced by this agent, almost naturally indicate its employment in cases of the

†Besides the employment of bleeding, tartar emetic, opium, and belladonna, advantage has also been derived from the local application of leeches, the warm bath, the exhibition of purgatives and artificial dilatation. More lately, Dr. Scanzoni advises a continued douche of warm water applied to the os uteri by a special apparatus; and, more lately still, Dr. Snow has found advantage from the employment of chloroform.

kind we are considering. Nausea having been once established, the rigidity will in a very large majority of cases be found readily to yield. Tartar emetic seems almost to exert a special influence on the cervix, for while the contractions of the fundus and body of the uterus are not interfered with, dilatation of the cervix will be found to proceed rapidly, and this the result of the re-establishment of the reflex actions existing between the stomach and the uterus, which are apparently suspended.

The few authors who have advocated the employment of tartar emetic in these cases, have generally prescribed it in doses of one-fourth of a grain repeated every three or four hours, until its influence became apparent by the gradual dilatation of the mouth of the uterus. There is certainly no worse system of midwifery than the meddling midwifery; but if, in any case, interference is demanded, whether of a manual or medicinal nature, the object should be a speedy delivery, consistent with the safety of the mother and the child. My own observation has led me to the belief that it may be safely resorted to in much larger doses, and with more prompt relief; and I cannot but view the mode of employing the medicine indicated above, as attended with a very considerable and quite unnecessary sacrifice of time on the part of the accoucheur, and of prolonged suffering on the part of the patient. I accordingly, prescribe it in one grain doses, repeated every half hour, and I have not as yet, found it necessary to exhibit the medicine in such doses more than thrice, relaxation most commonly following the exhibition of the first grain. The following cases, from rough notes in my possession will, I hope, demonstrate the value of the practice:

Case 1.—March 2, 1846. Mrs. P., (Bonaventure Street,) in labor of her first child; had been under the care of a midwife for the last seven or eight hours. Her age was 36, stout and healthy; was called about 1 a. m.; for the last two or three hours labor had made no progress; pains severe and regular; membranes ruptured about 9 p. m., the preceding evening. On examination, the perineum and vagina were found sufficiently soft and yielding; the os uteri was dilated to about the size of a half crown, was thick, firm and rigid, through which the scalp protruded to a considerable extent. The impediment being clearly the rigidity of the os uteri, I prescribed to her one grain of tartar until relaxation followed; after three doses were taken, relaxation followed, and the child was born in the course of an hour afterwards. In this case no nausea supervened until the second dose had been swallowed, and vomiting did not occur until the third had been administered. The vomiting was not urgent, and did not appear in the slightest degree impede the progress of the labor. The subsequent recovery was complete.

Case 2.—Dec. 20, 1846. Mrs. L., (Notre Dame Street.) Symptoms of labor came on about 11 p. m., and I was sent for about 3 a. m. This lady was of a delicate constitution, and had already given birth to two children, both of whom were still-born. Of the cause of this I could not satisfy myself at the time, but in both instances the labors were protracted. On examination, I found the membranes protruding through a partially dilated orifice, and could with some difficulty satisfy myself of a natural presentation. In the course of about two hours the membranes ruptured, and the child's head having descended lower, the presentation was accurately determined, left occipito-cotyloid. Every thing appearing to progress favorably, no fur-

ther examination was effected for an hour; at this period, the anterior lip of the os uteri was found forced down in front of the head during each pain, acting at the same time as a tight band across it. Several ineffectual attempts were now made to push it up during the pains; these proving abortive, recourse was had to the tartar emetic. Half a grain was immediately exhibited in a little water, and before the expiration of half an hour, the rigidity of these fibres of the cervix had yielded, the child's head was firmly forced down, and at about nine o'clock a. m., she gave birth to a living child. It is not an improbability that the two previous protracted labors of this lady were due to the same cause, and that interference was too long postponed. I have attended this lady a second time since, and the same anomalous condition of the cervix uteri occurred.

Case 3.—June 3, 1847. The husband of Mrs. O'N. (St. Joseph Street) requested me to visit his wife, who had been about thirty-six hours in labor. I found her under the charge of an ignorant woman, who claimed to act as a midwife. She was about 40 years old, had had six children previously, was of short stature, and very muscular. The membranes had ruptured, and the waters had dribbled away several hours previous to my visit; the head rested upon the brim of the pelvis; os uteri dilated to the size of a crown, hot and irritable, and feeling like a ligature applied over the presenting parts; the vagina hot and tender, and the labia considerably swollen. Found that the midwife had examined her very frequently, and had caused some pain during her manipulations. These effects might have been ascribed as much therefore to improper interference, as to the pressure of the head on the bony pelvis, compressing the cervix. Inflammation was here clearly setting in, and I abstracted about $\frac{3}{4}$ xxx of blood; she bore it well; but in the course of about 20 minutes, upon examination, finding not the least impression upon the os uteri, I gave her the tartar emetic, in preference to further abstraction of blood. After three grains had been taken, the rigidity yielded, and although the perineum, at a subsequent period, offered an obstacle, yet patience overcame the difficulty, and the child was born, although dead.

Case 4.—(Nov. 5, 1850.) Mrs. M'P., (Cheneville Street,) had been under the care of one of the most experienced midwives in the city. Her labor up to 7 o'clock, p. m., had lasted for nearly forty-eight hours, and was, in all other respects, perfectly natural. I was sent for at that hour; it was her first child. The mother was healthy, stout, and well formed, and aged about twenty-five. On examination every thing was normal, with the exception of a rigid band of the os uteri, dilated to about the size of a dollar, encircling the presenting vertex. The membranes had ruptured several hours previously, and by stethoscope, the fetal pulsations were clearly audible. In this case, one grain of tartar emetic produced such immediate and complete relaxation, that the child was born in about thirty-five minutes afterwards. In no case which has come under my observation have I witnessed such marked beneficial results following its employment.

It must be admitted, however, that cases may be met with in which its exhibition would be decidedly improper. These exceptions to the rule will be found to occur in women of delicate habit and leucophlegmatic temperament. They are cases in which the action of the remedy, if exhibited, may proceed too far; which, unable to resist the prostrating effects of the medicine, might be followed by a collapse, to which the vital powers of the sys-

tem might succumb. This is a contingency which should be sedulously kept in view; and prudence demands, therefore, some care in the selection of proper cases for its exhibition.

I have selected the above cases, and have given them as abstracts from my note book, for the purpose of illustrating the effects of the medicine, and of drawing more general attention to it. I place them before the profession with that object alone in view.

Montreal, Nov. 15, 1850.

British American Med. and Phys. Jour.

On the Treatment of Sprains of the Ankle. By M. BAUDENS.—M. Baudens observes, that judging by the frequency of the occurrence of this accident, its treatment ought to be well understood and successfully practised; but that this is in fact far from being the case, and he is therefore desirous of making his own plan of treating it, by the cold-bath and gum bandage, more extensively known.

The indications are, first, to prevent or remove inflammation, and then to secure immovability to the distended or lacerated parts, until they have recovered their power, the patient being at the same time allowed the use of the limb. For the purpose of subduing inflammation, numbers of leeches are usually applied, and then an emollient cataplasm; and M. Baudens feels convinced that it is in consequence of such treatment that degenerated sprains so often augment the number of amputations in hospitals. By free leeching of a joint, the seat of sprain, two mischievous effects are produced. In the first place, the pain, which is the first of the series of inflammation after sprain, is increased by the leech-bites, in place of being mitigated; and, in the next, the increased afflux of blood towards the part is encouraged instead of being repelled. M. Baudens, on these grounds, strictly forbids the application of leeches in all surgical maladies attended with acute inflammation, while he often derives most excellent aid from their employment in chronic inflammations; thus, by the induction of a temporary congestion, giving a fillip to the too languid action of the part. When blood need be taken in sprain, he abstracts it by venesection, although probably both the profession and the public, from the force of habit, would tax with ignorance any one who neglected the use of leeches. As to emollient cataplasms, they favor in place of opposing the afflux of fluids to the parts, while the long maceration the joint has been thus submitted to, deprives it of its elasticity, gives rise to a pasty *engorgement*, and predisposes to the formation of white swelling.

M. Baudens has pursued his own plan of treatment now for twenty years, and under it his patients have been enabled to resume their trying military duties in a very short time. He is not the first who has employed cold water in the treatment of sprain; but his originality consists in trusting to it alone, and continuing its application for so long a period. His plan of employing it, contrasted with that of his predecessors, may be thus summed up:—1. *Period of the Application.* Cold has usually been thought desirable only when it could be resorted to very shortly after the accident; but he applies it not only immediately, but also several hours or days after the occurrence, or even in chronic sprain—whenever, in fact, there is a *morbid degree of heat to abstract*. 2. The local bath has never been ordered by others for longer than five or six hours, although some

practitioners, since his first publication on the subject, have ventured to extend it to twenty-four. In certain of his cases, however, immersion has been continued for eight or ten days, and, in one example, for fourteen days; while in no case has it been less than for two. 3. *Mode of Application*.—The vessel containing the water is brought to the bedside of the patient, so that he can conveniently place his leg in it, having the heel resting on a sponge at the bottom, the leg and thigh being supported by cushions, so that the position may be maintained as many days as required. In the vessels used at the Val-de-Grace the water reaches as high as the middle of the leg, and is changed about every three hours in order to keep it sufficiently cool. Spring water is usually employed, and if the inflammation is intense, ice is added. A purgative is given, and, if indicated, one or two bleedings are resorted to. 4. *Effects*.—One of the first of these is the cessation of pain, which sometimes occurs at once, and at others in an hour or two. From the moment the foot is placed in the bath, the swelling becomes stationary, and soon after, with the heat and redness, decreases. About the fourth or fifth day the part becomes wrinkled like the hands of a washerwoman; and usually about the third or fourth day, the patient finds the water too cold, and then the limb is removed from it—the period for doing this being regulated by the patient, he being told to keep it in only as long as he derives comfort from so doing. Few of the patients suffer from any general reaction. *Gangrene* has been said to have resulted from this application, but the author has never met with such a case. The patient sometimes persists in keeping the limb in water after the dispersion of the heat and pain, and the consequence is the production of engorgement of the joint, a tense state and dark color of the skin, together sometimes with darkish lines—precursory signs of congelation in fact—on seeing which the joint should be enveloped in a fomentation of elder-flowers and poppy-heads at the temperature of the atmosphere. The objections which have been urged from the fear of producing *repercussion*, are quite theoretical and unfounded. It is in fact only the *excess of morbid caloric* that is abstracted.

Gum-bandage.—When the inflammation has been subdued, all the depressions in the vicinity of the joint are filled with wadding, and a bandage carefully and equably applied. This is well moistened, by means of a brush with very thick gum, which in a short time imparts to it almost the hardness of wood. After this has been worn for twenty-five or thirty days, it is removed, and the joint slowly and gradually exercised; for want of which precaution many patients (especially those treated by leeches and poultices) suffer all the symptoms of a sub-inflammation of the white tissues of the joints, even for years.—*Gaz. des. Hop.*, 1850, Nos. 5 and 6.

On Chorea. By Dr. LEE.—An analysis of various published cases of this disease, and of forty-two observed by himself at the *Hospital des Enfants*, leads Dr. Lee to the conclusion that there are four principal varieties of it.

1. One which has been called *sympathetic*, coincides with the local lesions of the various viscera of organic life, and especially with disease of the gastro-intestinal system and of the heart.

2. A second, which is very common, depends upon a *general* disease, and

ECLECTIC DEPARTMENT.

especially *rheumatism*. So frequent is this variety, that it constituted seventeen out of the author's forty-two cases, and thirty of seventy-four he has collected. Rheumatism indeed may not only give rise to chorea, but to a variety of other nervous disturbances, as simple convulsions, contractions, tetanic convulsions, pseudo-meningitis, pseudo-myelitis, &c.; and, in fact, there is no symptom usually referred to lesions of nervous substance, which may not be dependent upon a rheumatic affection of the joints or heart, such affection being almost always marked by the nervous derangement, and giving rise to only very slight local suffering and febrile action, especially in a chronic neurosis like chorea. When, however, the febrile reaction is intense, the neurosis is usually only developed when the inflammatory fever has undergone some remission; and a reproduction of the febrile action always induces an improvement in the nervous symptoms, except in some cases in which the disease proves quickly fatal.

3. Another form of chorea quite independent of cerebral alteration, is the so-called *essential* chorea, in which no appreciable change of structure is recognisable either in the organic viscera or the nervous system; this, like the rheumatic chorea, is a very common form.

4. The last form depends upon *cerebral* or *spinal lesion*, and is but the symptom of various cerebral and spinal affections.—*Bull de l' Acad.* tom. xv, p. 343.

Chloasma.—This disease of the skin is also known by the names, *Ephelis*, *Maculæ*, *Hepaticæ*, *Pityriasis versicolor*, *Leberflectete*, and *Liver spots*: and generally makes its appearance on some part of the chest or arms, and extends in very irregular patches to other parts of the body, sometimes covering nearly its entire surface. As far as the disease spreads, the skin assumes a dull, yellowish or brown color, sometime varying in tints. There is a very slight elevation of the cuticle in most cases, with a very fine eruption. Occasionally the itching is very annoying, though not at all constant. The patches are often covered with minute scales.

This disease is supposed to exist as a sequel to disease of the stomach or liver; but several cases have certainly come under my observation, where there was no perceptible functional derangement, either of the stomach or liver; and I am of the opinion, therefore, that it has no more connection with derangement of the stomach and liver than has *impetigo*, *lepra* or *psoriasis*.

My principal object in introducing this subject is to speak of the treatment which I think has been heretofore unsatisfactory, both to the physician and to the patient. During the early years of my practice, the cure of this superficial disease annoyed me exceedingly. In 1844, began to use the Sulphur Fume Bath as a remedy, and from that time have had entire success; and am now prepared to recommend this remedy as a specific for this disease, if there be any specific in medicine. In recent cases a few applications are sufficient; and in no case has it been necessary to apply it more than eight or ten times. If any member of the profession has a remedy as certain as this, and more easily applied, it would be highly gratifying to have it made more public.

Manchester, Nov. 1850.

New Hampshire Med. Jour.

WILLIAM GREY, M. D.

Deafness of Twenty Years Standing, caused by the presence of a Tooth impacted in the Ear.

The patient in this case, a shepherd, thirty-two years of age, applied to Dr. Neismann for his aid in reference to deafness, from which he had suffered from childhood. He had been under Dr. Neismann's care two years previously, when suffering from an attack of typhus, at which time the doctor regarded the deafness as the result of cerebral disturbance, and which he considered he would lose with returning health.

On examining the organ with a sound, it struck against a hard bony substance; and on questioning the patient, he replied, "Yes, there is a tooth there." Dr. Neismann expressed his surprise at the answer, which was reiterated; and on further inquiry he learned that, when a boy at school, a schoolfellow having removed a loose tooth for him, he jestingly inserted it into the external meatus, and it passed in beyond his reach. His hearing thereafter became impaired, and he was often quite deaf. Repeated but unavailing efforts had at various times been made for its extraction; he had suffered much pain, and lost much blood in these attempts, and had, in consequence of his deafness, been obliged to abandon his calling as a soldier, and had become a shepherd. Dr. Neismann introduced a pair of fine, long forceps into the ear, and by a rotatory motion extracted the tooth from the cavity of the tympanum. As if by an electric shock, the patient's previously dull and inexpressive countenance brightened up with joy, and he exclaimed, "Now I can hear." The tooth weighed five grains, and was coated with hard cerumen.—*Casper's Wochenschrift.*

New mode of Reducing a Dislocation of the Lower Jaw.

It is generally believed that in luxations of the lower jaw, the condyle lies in front of the transverse root of the zygoma, and is there held down either by muscular contraction, or by the resistance of the zygoma. This theory of J. L. Petit, Boyer, Sir A. Cooper, &c., has lately found an opponent in the person of M. Malgaigne; and M. Nelaton has recently brought forward some facts which confirm M. Malgaigne's opinion. According to these surgeons, the persistence of the displacement is caused by the contact existing between the coronoid process and the os malæ. Thus, it is sufficient, in order to reduce, to place the two thumbs on the coronoid processes after the patient has opened his mouth, and without taking hold of the jaw, or taking any fulcrum, to press them backwards, in order to make the condyles glide into their place. Another mode of proceeding, is to place one's self behind the patient, to take a fulcrum on the nape of the neck with the two thumbs, and place the fingers on the coronoid processes. The patient is then desired to open his mouth, and the surgeon makes a gentle pressure from before backwards on the coronoid process. Nothing more is required to bring the joint to a normal state. The method consists, then, merely in relaxing the muscles by opening the mouth, and in pressing moderately on the coronoid process to push it backwards, and a little downwards.—*Review Medico-Chirurgicale.*

A Precious Morceau.—The following anecdote is copied from the N. Y. Medical Gazette, and credited by that journal to the *American Homœopathic Journal*. The Editor of the Gazette says the trick has often been repeated in America. [Editor Buffalo Journal.

“In a first-class carriage between Manchester and London, each compartment having its occupant, a gentleman made great contortions, moaned, sobbed convulsively, applied his hand to his stomach, writhed, and exhibited all the signs of suffering. His fellow travelers were alarmed; and he gasped out to the one next to him: “My pocket—a little box—thank you—oh! dear! open—the bottle—marked ‘Cocculus,’—two globules—thank you.” The globules fairly in his mouth, there came a minute’s storm of aggravated pains, groans, moans; then the rigid features relaxed, a benign smile stole over the features, a sigh of relief, then free and frank speech again. ‘What was the matter with you, sir?’—‘Cramp in the stomach—wonderful the effect of the globules; people should never travel without them.’ Then a lecture on Homœopathy; seeing is believing; the fellow-travelers of the man so rapidly relieved of severe suffering, wished to know who he was; then a card is given to each of them, and that the card of a *doctor*.

“It is said to have so chanced, that the same person again seemed to suffer in a like manner, in the course of a journey from London to Manchester, and the same scene would have been repeated again, but one of his fellow-passengers cried out, ‘That will do, doctor——, I was present when you did that trick with the little box, some months ago.’”

On Linseed Oil in Hæmorrhoids. By M. VAN RYN.—M. Van Ryn believes, that, in general, surgical treatment is too hastily resorted to in this affection, and he wishes to bring under the notice of the profession a remedy he has found of great efficacy during twenty-five years. It consists in the administration of two ounces of fresh linseed oil morning and evening; and so rapid is the amendment generally, that the remedy is seldom continued longer than a week. Sometimes the stools are somewhat increased in quantity, but neither vomiting nor any other ill effect is produced. The only precaution the while, is the abstinence from alcoholic drinks, and too stimulating a diet.—*L'Union Medicale*.*

* We copy this article for the benefit of those who prefer any substitute to a surgical operation; but were we the patient we should entreat for the knife or the ligature rather than continue such a remedy long.—EDITOR BUFFALO MEDICAL JOURNAL.

EDITORIAL DEPARTMENT.

The Transactions of the American Medical Association, Instituted 1847.
Vol. 111. Philadelphia, 1850. pp. 499.

The volume of transactions of the American Medical Association, for the present year, is less in bulk, by nearly one half, than that for 1849. It is not to be inferred from this fact, that, as respects the progress of the medical sciences in America, the past year bears a corresponding proportion to the year preceding, nor that the volume for the present year is in value one half less than its immediate predecessor. The diminution is chiefly owing to the reports of standing committees having been restricted in their scope to American Medical Science, but, in some measure, also, to greater condensation in their preparation. There is less diffusiveness, and less irrelevant and appended matter in this volume, when compared with that for 1849; and, hence, it is really more valuable, and will, we doubt not, be more acceptable to the majority of readers. By this remark we would not be understood to imply any invidious comparisons respecting the ability displayed in the reports of 1850, as contrasted with those of 1849. The latter were, perhaps, quite as creditable to their authors as are the former. The improvement consists in greater brevity, and in an exclusive nationality. The latter quality is desirable for the sake of consistency, at least, in the transactions of an *American Association*.

The Report of the Committee on Medical Science, comes first in order.
Dr. USHER PARSONS, of Providence, R. I., Chairman.

This extends over fifty pages. It is a succinct, clear, and candid exposition of the progress of Anatomy, Physiology, General Pathology, and Therapeutics, etc., during the year; being just what should have been expected from the able and judicious Chairman of the Committee.

The Report on Practical Medicine and Epidemics follows, and occupies thirty-eight pages. Prof. I. K. MITCHELL, Chairman.

Nearly the whole of this Report is devoted to Cholera, which, as the reader will recollect, was not only the chiefly absorbing topic of Medical Literature, during the year, but the absorbing disease in another sense,

i. e., all other epileemics, and, to a certain extent, sporadic affections, having been merged into this epidemic. The report contains much interesting and valuable statistical information, mainly derived, however, from the city of Philadelphia. It is to be regretted that the observations and statistics in different sections, which had been communicated during the year in the different Medical Journals, had not been more fully embraced in the Report, so that the transactions would have embodied, more completely, all the available facts appertaining to the prevalence of the Epidemic.

The Report of the Committee on Medical Education, Prof. JOSEPH ROBY, Chairman, is brief, occupying only eight pages.

We have given this Report in full, in the Electic department of the Dec. No., 1850, of our Journal. In our judgment, it presents an able and candid statement of the position of the subject of Medical Education, as it stands before the Association, and the medical public at the present time. The views of the author seem to us to do justice to all the parties interested in the discussion of the subject—the Association—the Medical Schools—the Profession at large, and medical students. This was the impression received when we listened to the reading of the Report, and it is confirmed by its perusal. We have the less reluctance to express such an opinion, because the Report, and the conduct of its author, were, as it seems to us, harshly and unfairly treated by some members of the Association. The ground of complaint against the report appeared to be that it was not in strict accordance with the character of previous reports, and action of the Association; as if, forsooth, a Chairman of a committee is to be instructed by the opinions of his predecessors, as to the views he is to present; or, as if the Association, at its successive annual meetings, is bound to an uniform tenor of action! These positions, preposterous as they must seem to most intelligent persons, appear to be seriously assumed by some and made the basis of opposition to new propositions for legislative deliberation. The cavilings to which we have referred, were the more unbecoming, because the Chairman of the Committee was not present to defend himself, or to make explanations; and, as it appears, points which were made the occasion of personal invective, were susceptible of complete and satisfactory explanation.

Report of the Committee on Medical Literature. ALFRED STILLE, M. D., Chairman.

This report occupies *fifty* pages. The preparation of a report on Medical Literature is a delicate duty, involving, as it does, an impartial dis-

crimination, which, 'it may well be imagined, cannot easily be exercised without danger of giving offence to some, and, unless managed with *tact*, may wound the feelings of the literary aspirant, in an uncalled for degree. The Report by Dr. Stille, is executed with his usual ability and elegance, and we believe it is not open to censure on the score of either candor or fairness.

Appended is a list of all the original articles, of interest, published in American Journals during the year, arranged according to their subjects. This will prove useful for reference, and we hope the plan will be followed by the authors of subsequent Reports on Medical Literature.

Report of a Special Committee, on the means of Encouraging a National Medical Literature. Prof. W. E. HORNER, Chairman.

The following resolution embodies the conclusions at which this Committee arrived:—*Resolved*, That in the opinion of this Association, the only legitimate means within our reach, for the encouragement and maintenance of a National Medical Literature, are to increase the standard of preliminary and professional education, required of those who would enter the medical profession; to promote the circulation, among the members of the profession, of the medical journals of the day; to encourage the establishment of District Medical Libraries, and to induce every practitioner to cultivate, with care, the fields of observation and research, that are within his reach.

These several *means* are, of course, very desirable in themselves, but they would be the legitimate *ends* of a better encouragement of a National Medical Literature.

Report of a Committee on a Memorial to Congress, in favor of an International Copy-right Law. Prof. GEO. B. WOOD and Dr. ISAAC HAYS, Committee.

The Committee reported a Memorial, which was adopted by the Association, without debate. This we regretted, not because we are prepared to deny the policy of the measure, but we opine that the question has not been sufficiently considered by the medical profession to take action respecting it. Some members of the Profession, as we know, who have bestowed upon the subject considerable reflection, have been led to doubt the expediency of petitioning for a copy-right law. Moreover, as a matter for Congressional action, the subject embraces a wider scope than medical publications, and involves other considerations than those relating to Medical Literature. Hence, it is not to be expected that paramount importance

will be attached to petitions from the Medical Profession, and we would reserve our influence with the National Legislature, for occasions in which we should have more certainty of effecting something, and for objects more clearly to be desired.

Report of the Committee on Publication. ISAAC HAYS, M. D., Chairman, and Report by the Treasurer, Dr. HAYS.

The Committee on Publication reported the following resolutions, which were adopted:—

1st. *Resolved*, That the assessment for the present year shall be three dollars.

2d. *Resolved*, That those delegates who pay the assessment shall be entitled to one copy of the Transactions of the present year, and that the payment of two dollars in addition shall entitle them to two additional copies.

3d. *Resolved*, That permanent members shall be entitled to one copy of the Transactions of the present year on the payment of two dollars, and three copies on the payment of five dollars.

4th. *Resolved*, That societies which are represented at this meeting shall be entitled to copies for their members on the same terms that such copies are furnished to permanent members.

5th. *Resolved*, That the permanent members, unless present at the meeting as delegates, shall not be subject to any assessment.

6th. *Resolved*, That any delegate who is in arrears for his annual assessment, shall not be considered as a permanent member.

7th. *Resolved*, That the several committees be requested to bring to the meeting of the Association their reports correctly and legibly transcribed; and that they be required to hand them to the Secretaries as soon as they have been read."

The Treasurer's Report exhibits a balance in the Treasury of seventy-three dollars and fifty-two and a half cents.

Report of the Committee on Public Hygiene. Prof. JOSEPH M. SMITH, Chairman.

The report proper embraces *twenty-three* pages, and is devoted mainly to the consideration of the source of the poison producing Typhus Fever, which the author thinks is produced by excretions from the living human body, and more especially the animal matter entering into the composition of the cutaneous and pulmonary exhalations. In other words he refers the origin of Typhus to what has been distinguished as *idio miasmia*

The considerations which are adduced in support of this view, or rather in elucidation of it, are interesting, and striking. The subject is handled with much ability.

Appended to this report, are two papers, one on the *Sanitary Condition of Massachusetts and New England*, by Dr. EDWARD JARVIS ; the other on the *Hygienic Characteristics of New Orleans*, by Dr. J. C. SIMONS. Both papers will repay a careful perusal, and will serve to direct attention to important questions, in which is the subject of public health—a subject which has secured a degree of attention inversely in proportion to its magnitude and importance.

Report on Adulterated Drugs, Medicines, Chemicals, etc. Prof. R. M. HUSTON, Chairman. 18 pages.

Nothing but want of space, together with the expectation that the Transactions will circulate extensively among members of the profession, restrains us from inserting the greater portion of this report, so valuable are the facts which it embraces, and so particularly important is the subject to the practitioner. The evils arising from adulterations, and the sale of spurious medicines, are not generally half enough appreciated. These evils do not relate solely to the cure of disease in the cases in which unreliable articles are dispensed; but the therapeutical experience based on them must necessarily become vitiated. In the latter point of view, the injury can hardly be estimated; it must be very great. The committee recommended the following resolutions, which were adopted :—

“1st. *Resolved*, That the various State and local medical societies be requested annually to appoint boards of examiners, whose duty it shall be to procure specimens of drugs from the stores within their limits, for examination, and report upon the same to their respective societies, at least once in every year.

2ndly. *Resolved*, That the respectable druggists and apothecaries throughout the United States be requested to take active measures for suppressing the fabrication and sale of inferior and adulterated drugs, and that it be respectfully suggested to them, whenever practicable, to form themselves into societies or colleges for the promotion of pharmaceutical knowledge and general improvement in their profession.

3dly. *Resolved*, That a committee be appointed, consisting of one member from each state here represented, whose duty it shall be to collect information in regard to adulterated and spurious drugs, and report the same at the next meeting of the Association.”

Report of the Committee on Indigenous Medical Botany. Prof. ELI IVES, Chairman.

This report is quite brief, and is confined to an account of the personal experience of the Chairman, and of Dr. Bennett, of S. C., on the virtues of a few plants not before enumerated, or the medicinal effects of which have been only partially described.

Report of the Standing Committee on Surgery. Prof. R. B. MUSSEY, Chairman.

This report should have followed that on Practical Medicine, but was not inserted in its proper place in consequence of its not having been received by the Committee until after the foregoing reports were in type. The report extends over *sixty-four* pages, and, so far as we may judge, contains a faithful statement of improvements, etc., in the department of Surgery, during the year. Appended to the report are the following papers:—*On the use of Anaesthetics*, by J. D. WARREN, M. D. *On the same subject*, by W. L. ATLEE, M. D. *On the use of Chloroform*, by J. D. GROSS, M. D. *On the Result of Operations for the cure of Cancers*, by J. C. WARREN, M. D.

The Report on Surgery, with the papers appended, closes the volume of Transactions proper. In an appendix, are embraced several papers, as follows:

1. Protest of the delegates from the state of Alabama, against statements reflecting on the character of the medical profession of that state, contained in the Report on Medical Education, for the year 1849.
2. A discussion of the question, has the Cerebellum any special connection with the sexual propensity, or function of generation? By Prof. .N S. DAVIS.
3. The Obstetrical Extractor, by Prof. JOHN EVANS.
4. A brief notice of some of the Physicians of the United States, who have died within a few years. By STEPHEN W. WILLIAMS, M. D.

In connection with the Transactions, we have, in this volume, the official publication of the minutes of the third annual meeting of the Association. We observe in these a striking discrepancy with the account given by us in a former number of this Journal, relating to the action taken on the subject of Medical Education. We reserve this matter for further notice in a subsequent No.

The foregoing was in type for our last No., but excluded for want of space. We will now indicate the discrepancy in the published minutes,

to which allusion is made in the last paragraph of the preceding article.

On page 35, we find as follows:—"The Chairman of the committee of the whole reported, that they had had under consideration the preamble and resolutions of Dr. Blatchford, of N. Y., and certain other resolutions, herewith submitted, proposed by Drs. Lawson & Drake, of O., Gross, of K. Y., and Theobold, of M. D., which were recommended by resolution of Dr. Flint, of N. Y., to be referred to the standing committee on education for 1851: *and that they afterwards adopted the accompanying resolution of Dr. Caspar Morris, of Pa., offered as a substitute for the above.*" We have italicised the portion to which the error specially relates. The following quotation from page 36, contains the resolution of Dr. Morris, referred to in the foregoing extract, and reiterates the above statement: "Resolution offered by Dr. Morris, of Pennsylvania, as a substitute, passed in committee of the whole, *reported to the Association, and adopted:*

Resolved, That the recommendations of this Association, at its former meeting, in regard to Medical Education, be re-affirmed, and that private preceptors be still urged to receive into their offices only those duly qualified by previous education to engage in the study of medicine."

Now, had we not participated in the proceedings at this stage, (having offered the resolution of reference to the standing committee on educations for 1851,) we might distrust the accuracy of our recollection, but we think we can hardly be mistaken in the opinion that the above account is erroneous. Moreover, we think that the account itself, as it stands in the published minutes, shows that there is an error, in other words, that the course of proceedings, as given, is inconsistent and unparliamentary. We hasten to remark that we would not, for a moment, be thought to imply that there is any *intentional* error in the record by the Secretaries of the Association. The error, (if error there be,) doubtless arose from some confusion in the proceedings at the period to which it relates.

On the second day of the session of the Association, May 8th, after the reading of the Report by the Chairman of the Committee on Medical Education, a series of Resolutions was submitted by Dr. Blatchford, (a member of the Committee,) as a minority Report. The discussion of these resolutions, proposed amendments, substitutes, etc., occupied the Association during the whole of that day, and on the morning following the Association resolved itself into a Committee of the Whole to continue the discussion, Dr. Knight, of New Haven, being Chairman of the Committee. The discussion in Committee of the Whole continued during the morning session until a motion was made by the writer that the *Committee rise and report a re-*

commendation to the Association to refer the whole subject to the standing Committee on Education for 1851. The motion prevailed by a large majority. Dr. Lopez, Vice President, resumed the Chair, and the Report was made at once by Dr. Knight, Chairman of the Committee. These being the facts, the error consists in the statement that Dr. Morris' resolution was passed in Committee of the whole *subsequently* to the motion by the writer. That resolution, as we recollect distinctly, was offered prior to the motion referred to, and of course, by this motion was referred, as well as the other resolutions mentioned, to the standing Committee on Education for 1851.

The reader conversant with the rules of parliamentary usage will perceive that the facts, as we have stated them, must be correct. After a motion has prevailed to rise and report, in Committee of the Whole, no resolution can be adopted without a re-consideration of the motion to rise and report. It is in no wise probable that such a blunder could occur with Dr. Knight, (an admirable presiding officer,) in the chair, and with many members present who were practically familiar with the laws regulating deliberative bodies.

The *inconsistency* of the proceedings, as they are published, shows, equally, that they must contain the error pointed out. As they read, the resolutions of Drs. Blatchford, Lawson, Drake, etc., were recommended, by resolution of Dr. Flint, to the standing Committee on Education for 1851, but that afterward was adopted a resolution by Dr. Morris as a *substitute for the above resolutions*. To make a reference of certain resolutions to a committee, and then directly to adopt a resolution as a substitute for the resolutions referred, without reconsidering the motion of reference, would be absurd.

According to the proceedings as published, the Association again committed itself to its previous recommendations on the subject of Medical Education, but if these proceedings do contain the error to which we have adverted, the action of the Association on the subject of Medical Education was negative, and the whole matter is referred to the next Medical Congress. The point, thus, it will be perceived is not altogether trivial.

In making this criticism on the published proceedings, it is due to ourselves to add that we are not opposed to the recommendations of the Association for the re-affirmation of which Dr. Morris' resolution provided. These recommendations relate mainly to increased duration of instruction in Medical Colleges. We are in favor of this measure, and are ready to advocate it to the best of our ability. As respects the mode of increasing instruction

advised by the Association, viz., lengthening the term to six months, we are not so clear that this is the best that can be desired. But we reserve remarks on this topic for another time.

Observations on certain of the Diseases of young Children. By CHARLES D. MEIGS, M. D., Prof. of Midwifery and the diseases of women and children, in the Jefferson Medical College at Philadelphia, etc., etc., etc. Philadelphia: LEA & BLANCHARD. 1850. 8 Vol. pp. 214.

This work, as the author states in the preface, contains the substance of several lectures delivered in the Jefferson College, in October, 1849, during the term preliminary to the session of 1849-50. It is not designed as a systematic treatise, "seeing," as the author remarks, "that the place is already occupied with numerous valuable books, presenting a complete body of doctrines on children's diseases."

To assure the reader that he will find in this small treatise many valuable observations, is only to echo the anticipations of every practitioner who is conversant with the previous publications of the author. Having enjoyed a long and large experience, much of which, it is to be presumed, has related specially to diseases of women and children; and been for many years a distinguished teacher of medicine in its applications to these diseases, it would be strange, indeed, if his contributions to our science and art were to prove unacceptable or uninteresting. Moreover, it is gratuitous to say that there are few among those who adorn the medical profession of this country, whose writings and teachings evince greater learning, research, scholarship, and talent, than do the publications and lectures of Prof. Meigs. We admire the fervor of his style, and the high professional tone which he assumes and maintains.

But all the excellencies that have been mentioned, do not secure the mind against error, in the prosecution of scientific inquiries; and, indeed, some of the qualities which serve most to attract, and to enlist the sympathies of the hearer, or reader, are apt to mislead in the pursuit of truth. Enthusiasm, which, within just bounds, serves to stimulate exertion, and inspire zeal, by a natural and easy transition may pass into extravagance and credulity. The phenomena of science, even of the science of disease, present abundance of material for the exercise of a vivid imagination, often luring the hardy way-farer to turn aside from the tedious and rugged paths of observation, and induction, with the vain hope to anticipate results by a shorter route, and to enjoy fruits which, however pleasing to the eye, may prove to be but ashes to the taste.

In perusing the "observations on certain of the diseases of young children," we have found, with much that is interesting and useful, not a little that furnishes fair occasions for criticism. If the work has striking excellence, it has faults not less glaring. The latter will be as obvious to the intelligent medical reader, as the former. The private teacher, in putting the book into the hands of his pupil, will find it necessary to interpose a caution against receiving the peculiar views of the author, as established or accredited truth. The advice of most persons would probably be, to attend to the practical maxims and suggestions, but read with distrust the portions in which the author launches into the sea of speculation.

We had not designed, in this notice, to have directed attention to particular portions of the work, which, on the one hand, appear to us of special practical value, and, on the other hand, obnoxious to criticism. We will, however, cite a few passages in illustration of the justness of the remark just made, assuring the reader that while we do not, by any means, quote all those to which exceptions might be taken, we also leave untouched not a little that will commend itself to his approbation.

Speaking of *diagnosis*, in Chapter First, the author enforces, in his glowing style, the importance of understanding the natural language of disease, involved in the manifestations of diseased organs—a study highly important in the diseases of children, since we have not the advantage of the statements of the patient, which, as the author justly remarks, are not always an advantage, the organs, irrespective of intellect, often being more truthful than verbal descriptions of morbid sensations dictated by the consciousness of the sufferer. In exemplification of the precept just explained, he gives the following:—

"How do you do, to-day?" said I to a lady.

"I am very sick, indeed, doctor."

"How are you sick—where are you sick?"

"I have had a terrible chill, which made my teeth chatter together, followed by fever, violent headache, pains in the back and limbs, and unappeasable thirst; I am dreadfully ill.

"Have you pain in the abdomen?"

"No."

"Have you any pain in the thorax?"

"No."

"Pain in the great joints?"

"No."

"Have you not a lump in your breast?"

"No, I have not."

"Yes, you have."

"Indeed, I have not."

"No," said a witness, "I have examined it with the greatest care.

"When was it examined?"

"Just now."

"Will you let me examine it?"

"Yes."

"Well, then, does not that hurt you?" I touched the breast.

The answer was an outcry; she had a lump in her breast; she had a *weed* in her breast, and did not know it. Suppose she had been dumb and deaf—would there have been any bar to my diagnosis in her surdmu-tism?

A friend of mine had lodgings for the summer, twenty miles from the city; his daughter, two years old, was seized with a fit of unextinguishable crying; she screamed all the time that she was awake.

On the morning of the second day, as her distress continued, he became much alarmed, and resolved, accompanied by his lady, to bring his child to me. They arrived at their city residence, and sent for me by an urgent messenger. I heard the child's voice from the third story, while I was in the lower hall. My friend began to explain the nature of the case, and to set forth all his alarm for his dear daughter. I stopped him, saying, "I hear her crying now, do I not?"

"Yes, that is her voice; she is in the greatest distress."

"She is crying with the ear-ache," said I.

"Ear-ache? How do you know that, doctor?"

"Come with me to the nursery, and I will prove it to you," said I; "I know the voice of the ear-ache."

When we came into the room, the child, surprised by my presence, ceased for a moment to scream. "Now," said I, "see if I don't prove to you that she has an ear-ache." I approached her, and putting the *p*-alp of my thumb on the right meatus auditorius, I suddenly pressed the cartilaginous tube inwards upon the ear; the child merely looked surprised at my rudeness; she did not scream. "It is not the right ear," said I. I next repeated the same movement as to the left ear, and she screamed as if she would go into convulsions. "There," said I, "I have hurt her ear for you, by a slight touch, and she cries with the same voice that I heard when I was down stairs; I knew that it was the ear-ache then, and I as sure of it now, this touch is the diagnostical test." The cry was a test."

These anecdotes are sufficiently in point as illustrations of the diagnostic prominence of two isolated symptoms; but we must take serious exceptions to the inferences which the young student would be likely to draw from such relations.

Does Prof. M. really mean to inculcate the notion, that a patient having experienced a chill, without pain in the abdomen, thorax, or great joints, must necessarily have a *weed* in her breast? Does he mean to teach that his students are to do, in similar cases, as he did in that case, pronounce upon the diagnosis so promptly, and positively, and with no other data? Or, is he willing to be understood that an ear-ache is to be diagnosed from the cry of a child heard from the third story? If he is accustomed to draw conclusions in that way, as to the seat and nature of disease, he should, in fairness, have given other instances in which he failed to *guess* rightly, for, after all, it is guessing, shrewdly, intelligently mayhap, but still guessing.

The advice, by the same author, given in another work, as it seems to us conveys a safer and better maxim. "I advise the student early to come to the resolution of being cautious in giving his diagnosis and prognosis of these doubtful cases of labor. I know that there belongs to professional men a disposition to *pronounce* at once. This, perhaps, arises from a false pride, which prompts them to seem to know all things at a glance, or by mere intuition."—Meigs' Treatise on Obstetrics, page 262.

In the chapter on Cyanosis neonatorum, after assuming that the nervous mass is the creature, the *Ens*, in all living beings, the rest of the organism being "vile, common, and of less account"—which possibly might be entertained as a philosophical conceit, or a poetical license, he proceeds as follows:—

"I shall not encumber these pages with quotations from the authorities to fortify the assertion that the nervous mass is the essential *Ens*. The asseverations of a thousand philosophers would not make more or less true a proposition which commends itself to the mind, acting upon its own perception and judgment of a dogma declared to be true. Such a truth is not proved by evidence, nor established by any method of induction. It is a truth of reason—it is a truth of consciousness—it is in the same category with the cognition of our personal identity."

We leave with the reader, without comment, the idea that a physiological proposition is not to be "proved by evidence, nor established by any method of induction," but is to be classed among the intuitive truths, and placed in the same category with the belief in personal identity! This is transcendentalism *par excellence*.

In the same chapter the reader will observe a formal argumentation of the point that it is not blood that develops the nervous force inherent in the cerebro-spinal axis, but the oxygen that the blood contains. The author contends that it is not a "mere fancy in Oken to say that 'the artery is an air-tube.'" He is not content with the position that oxygenated blood is requisite to produce the nerve force, but insists that it is oxygen and not blood! This metaphysical quibble, it would seem, in the author's estimation, involves a doctrine of great importance to the medical student.

The author's attachment to Oken, by the way, he admits, with a candor which is certainly as creditable as remarkable, is so great as to prevail over the exercise of his own judgment. He says:—

"For my own part, without feeling in general bound to adopt whatever explanations or rationales of life may be presented to me by men we call authorities, I freely admit that there is in me a tendency to surrender my judgment to the dicta of Lorenz Oken, notwithstanding the salutary declaration of St. Augustin, "*Quod scimus debemus rationi; quod credimus, auctoritati;*" expressions that ought to serve as a motto and general declaration of independence for all persons devoted to scientific pursuits. But of such authorities as Oken, it might almost be correct to say, in believing after him, *quod credimus scimus.*"

He regards, in fact, the opinions of his private author as "the basis of physiological truth." He adds following the passage just quoted:—

"Taking the foregoing declarations and statements of those distinguished physio-philosophers as a basis of physiological truth, the question issues from it as to what are the nature and extent of the force with which nervous mass is endowed."

Arguing in behalf of the insufficiency of evidence of the contagiousness of Scarlatina, we find the following remarks:

"I do not conceive that we are bound to believe, because two or more persons in one company or household are seized with scarlatina within a certain time after communication held with a patient laboring under it, they acquired it from the said person—not even if the same sort of circumstance should be observed a thousand consecutive times.

"Although it may be true that the cause of scarlet fever is a miasm, exhalation, gas, or substance, which is extricated from the bodies of the sick—yet if it is true, no one has hitherto proved it to be so. The most that can be said of it, therefore, is that it is in general supposed to be contagious."

If a thousand consecutive instances of the development of a disease, within a certain time, after communication held with a person laboring un-

der it, be not evidence of contagion, we should like to know how the personal communicability of any disease is to be established irrespective of inoculation.

That the author refuses to recognise this sort of evidence, is only susceptible of explanation, on the ground that he is predetermined, in spite of any evidence, to deny the contagiousness of scarlatina. This, indeed, he distinctly confesses, with admirable frankness, in the following passage:—

“My own mind has, for many years past, been so fully made up on this subject, that I do not suppose I shall ever change it so as to believe that scarlatina is propagated by contagion. I think that I have come to this conclusion after very careful consideration of the subject, with which a medical life, so long and active as mine has been, could not but have familiarized me.”

We will content ourselves with one quotation more, which will serve to show the logical process by which the author arrives at certain notions in pathology. As a specimen of a *facile* method of reasoning, the following extract is certainly rich. The author dismisses, it will be perceived, the explanation which first suggests itself, because it is not agreeable to him to admit it. He is “hardly willing to attribute,” etc. It would seem that truths of natural science are to be determined by volition, as well as intuitive *belief*! The next explanation is difficult to be entertained; the third conjecture is therefore probable; and, after all this “it remains to suppose!” but to the quotation—the author is speaking of the pathology of swelled liver in the jaundice of infants:—

“I am unacquainted with the precise principles of the pathology of this condition; I am hardly willing to attribute it to faults of a simple excessive sanguine determination to the capillary termini of the hepatic artery; and it is equally difficult to assign as a cause for it, any affection of the hepatic veins; it is probably, therefore, a fault essentially resident in the capillary termination of the hepatic portæ. It only remains to suppose that the sanguine engorgement is to be referred to a fault in the condition of the digestive circulations which issue from the aorta—that is, by the coeliac and the two mesenterics, hence, the farther inference that the trouble in the hepatic functions is pathologically connected with some faulty action of the capillary ramifications of the three digestive arteries above named, or of the venous capillaries of their second expansions in the substance of the biliary organ.”

As already remarked, we have cited the foregoing extracts by way of illustration of the correctness of the opinion, previously expressed, that the

work is not to be recommended to medical students in unqualified terms. Books, like individuals, have always their faults. Sustaining the comparison, we would say, that in this instance, the eccentricities, which are so numerous and striking, may perhaps be overlooked in consequence of the good qualities which make it acceptable.

For sale by Phinney & Co.

Spectacles; their uses and abuses in long and short sightedness; and the pathological conditions resulting from their irrational employment, By J. SICKEL, M. D., of the Faculties of Berlin and Paris, Cilm. Prof. of diseases of the eye, etc., etc., etc. Translated from the French by permission of the author, by HENRY W. WILLIAMS, M. D., Fellow of the Mass. Med. Society, etc. Boston, Phillips, Sampson, & Co., 110, Washington street. 1850. 8 Vol., pp. 202.

As the translator remarks in his preface to this treatise, the catalogue of our medical literature contains no work, on the uses and abuses of spectacles, prior to this publication. And that, in this particular, a *hiatus* existed, no one can doubt, who has given any attention to the subject, and, more especially, one who, for reasons of a personal nature, has been obliged to bestow some thought upon it. Practitioners of medicine are often consulted respecting the propriety of wearing spectacles in cases of myopia, presbyopia, etc., and their judgment is solicited as to the choice of glasses, and other points bearing on this method of giving artificial assistance, or protection to the organs of sight. Patients have a right to look to the Profession for this kind of information; but there is reason to believe that they are not infrequently disappointed in obtaining it, if, indeed, they are not sometimes misled by the enunciation of incorrect notions.

The subject of myopia, as affecting the young, is one which concerns not alone the visual organs. The mind and character of an individual may be influenced, in no small degree, by this defect. It tends to repress the observing powers by diminution of the amount of stimulating incentives to their exercise, and thus indirectly to favor an undue development of the meditative faculties. This is a point of view in which we do not remember to have seen this disorder of vision considered, but we have no doubt of the correctness of the remark. Here, then, is an additional reason why this difficulty should receive proper attention in early life, before the natural tendency to modify the intellectual character of the individual has had time to produce mental habits and effects which it will be difficult afterward to overcome.

Myopia, in a large proportion of cases, whether congenital, or acquired in early life, (the latter being very frequently the case,) is susceptible of being remedied if discovered, and the proper measures resorted to. The latter are of an obvious character, consisting in the habitual exercise of vision upon distant objects, on the one hand, and avoiding, on the other hand, the use of the eyes in observing objects at too near a focus. Occupations and pursuits are to be selected and pursued in reference to this defect. Spectacles are not to be resorted to while there is reason to hope for success in overcoming the defect. When it becomes necessary to resort to them, certain principles are to be observed in the selection of to glasses. We quote on this topic from Dr. Sichel's work:—

“When concave glasses are judiciously selected, they ought neither to lessen or bring objects near, nor produce a too great distinctness of vision, which may sometimes even amount to oxyopia, a name given to a peculiar condition where the eye is over-excited and dazzled by the cleanness with which objects are perceived. Still less should the glasses cause a feeling of annoyance, of pressure, of pain in the eyes, of fatigue after using them for some time, or any other abnormal phenomenon. When the spectacles are laid aside, the sight should not become more confused at short distances than it previously was; nor ought a species of shade, or badly defined image, representing either the form of the spectacles or their frame, or the objects which have been observed, to remain before the eyes.”

In Presbyopia (far sightedness,) the opposite of myopia, (near sightedness,) the proper adjustment of the degree of convexity in the glasses selected is of great importance, with reference to the preservation of the powers of vision, and the prevention of other ocular affections. The rule is, to select the lowest curve of convexity that will answer the ends desired. One of the evils attending the use of glasses with greater convexity than is needed is, the ability of the eye to accommodate itself to objects at different distances is impaired. We quote the author's remarks on this point:—

“Generally speaking, every person, whether presbytic or myope, can make use of glasses of different curves; only, with the stronger numbers he sees more clearly, and is forced, if presbytic, to bring objects nearer; if myopic, to place them further from the eyes, but without thus causing them to be magnified or diminished in volume. The more powerful the glass, the less latitude it allows in the position of bodies looked at. The feebler spectacles allow this position to be varied to a certain extent without any notable change in the distinctness of the visual perception; an

evident proof that these glasses of slight curvature allow the conservation of a certain degree of the accommodative faculty. An example will make what has been said more intelligible, and will facilitate the comprehension of the conclusions which we shall deduce from it; a presbytic person who has never yet employed spectacles, but has not allowed the opportune moment for their adoption to pass by, can generally read equally well with Nos. 72, 66, and 60; he will, however, find, if he pays strict attention to his sensations, that with the last-named number he will be compelled to bring the book nearer, and to hold it more invariably at the same distance: whilst the first obliges him to place it further off, and permits him to bring it nearer or remove it, within a certain distance, without his sight becoming dim or sensibly fatigued. Those more feeble glasses, then, permit the preservation of a certain degree of the faculty of accommodation, which those of a greater curvature diminish and end by abolishing, in a manner more positive in proportion as their use is more constant. If the eye can make use of several numbers solely by changing the position of the objects, it is because it accommodates itself to the focus of the glasses. Once habituated to a shorter focus, it cannot, without difficulty, return to more feeble glasses; this difficulty, always proportioned to the power of the spectacles, may end by becoming insurmountable when this power has been exercised. Thence results the high importance of the precept we have given, always to choose the most feeble number with which the patient can distinguish clearly and without fatigue, without change of the apparent volume of the objects, or without being forced to place them at a distance very different from that which the unassisted eye admits of. A presbytic who can employ, with nearly equal facility, Nos. 72, 66, and 60, by using the last for some time would accommodate his sight to this number. The modifications which age effects in the visual organ making their inevitable progress, he will be forced to change his spectacles at a given time, and much sooner than would otherwise be requisite, because the accommodation of the sight to spectacles of too great power is soon accompanied by fatigue and a want of clearness of vision, like all too continuous exercise of the adjusting faculty, especially at short distances.

He has then again to select among several numbers of which the effect will appear to him to differ very little, as Nos. 54 and 48. But, for the same motive as before, he will infallibly choose the strongest, as being that which apparently renders him the most efficacious aid. Thus the progression is very rapid; the further it descends, the more the glasses destroy the faculty of accommodation, not only during their use, but also du-

ring the time when they are laid aside; for, bound as it were to the focus of the spectacles, during all the time they are worn, the sight no longer easily adapts itself to greater distances. Thus the presbytic who employ more feeble numbers can yet read for some time with the naked eye, and preserve all the integrity of their visual range for remote distances, whilst those who constantly read or work with powerful glasses, end by finding themselves unable to dispense with them, and often even can no longer see large objects as far as formerly.

If, then, after having seen perfectly well with spectacles of a certain number, a person uses, without reason, or for experiment, glasses of greater power, he will find, at the end of a few weeks of their employment, that, on returning to the number primarily used, he sees infinitely less well than formerly, if he can see at all; an evident proof that spectacles limit the focus, and diminish the adjustive faculty, which they may end by destroying. That which might, in this respect, be assumed *a priori*, is confirmed, in each point, by experience, which proves, at the same time, that it is of the highest importance to proceed rationally in the choice of spectacles, and to begin, according to the rules we have endeavored to establish, by those of the feeblest power, substituting others, of greater strength, by insensible degrees. Here, as everywhere, the physiological laws forbid sudden transitions, which, by their shock, produce a disturbance, which is almost always injurious to the function."

The foregoing extracts, aside from their intrinsic practical value, will serve as specimens of the style and character of the work.

For sale by Phinney & Co., and Derby & Co.

Dr. Jarvis on Insanity in the Sexes.—We have received an interesting pamphlet on the above subject, being the publication of a paper read before the Association of Medical Superintendents of American Institutions for the insane, at their annual meeting, at Boston, in June, 1850, by E. Jarvis, M. D., of Dorchester, Mass.

Dr. Jarvis has analyzed statistics collected from various sources to determine, 1st, the comparative liability of males and females to insanity, and the causes in males and females; 2d, the comparative curability in the two sexes; and 3d, the mortality of male and female lunatics.

The conclusions arrived at are, that males are more liable to insanity, than females, but this is not a universal fact, in some countries the reverse being true. The males predominate in the asylums of America, England,

Scotland, Ireland and France. The females predominate in the asylums of Belgium, among the people of Norway, and among the paupers of England and Wales.

As respects curability, statistics show a proportion of cures, in most of the countries from which data are obtained, in favor of females. In the United States, however, the preponderance is slightly in favor of males. The mortality appears to be less in females.

The author adduces statistics to show that man is more exposed to, is less frequently cured of, and fails more under the attacks of disorder of the nervous system than females. The reverse of this is perhaps the common impression.

The general inference from the statistical investigations presented by Dr. Jarvis, would appear to be that sex *per se* does not exert a very notable degree of influence in the development, or issue of insanity. Although this result is negative, it is important that it be ascertained, and the author is entitled to credit for his industry in collecting materials bearing on the subject.

Human Physiology. By ROBLEY DUNGLISON, M. D., etc., etc., etc. With nearly five hundred illustrations. Seventh Edition. Thoroughly revised and extensively modified and enlarged. In two volumes. Philadelphia: Lea & Blanchard. 1850.

It is now more than fifteen years since the publication of the first edition of "Dunglison's Physiology." During this lapse of time the work has passed through several editions, each of which has received, from the hands of the Author, revision, together with such additions as were required by the constant developments and changes incident to the progress of physiological knowledge. From the first appearance of the work, it has been highly prized by medical pupils, and by practitioners of medicine, and in most, if not all, the medical institutions of the country, it has been recognized as a text book in the department of medical education of which it treats. It has also been received with great favor on the other side of the Atlantic. So well known and established, indeed, is its character, that commendation on the part of the Journalist at this period in its history, would not only be supererogatory, but be deemed by the reader officious almost to the degree of impertinence.

In noticing the publication of the *seventh* edition the readiest and best method of doing justice to our readers, and to the author, will be to make some quotations from the preface. The author therein states that he has

bestowed more care on this, than any previous review of the work; that the present edition has been "subjected to an entire scrutiny, not only as regards the important matters of which it treats, but the language in which they are conveyed." * * * * *

"On the whole subject of physiology proper, as it applies to the functions executed by the different organs, the present edition, the author flatters himself, will be found to contain the views of the most distinguished physiologist of all periods. The contributions to the science of life, of late years, have been rich and varied; and to collect and weigh them, and to separate the most worthy and valued, has been a work of no little discriminating labor—but to the author a labor of love, inasmuch as they are subjects which he has been long accustomed to investigate, and on which he has annually to treat before the class of Institutes of Medicine of the Jefferson Medical College."

* * "On no former occasion, has the author felt as satisfied with his endeavors to have the work on a level with existing state of the Science."

For sale by Phinney & Co., and Derby & Co.

Surgical Anatomy. By JOSEPH MACLISE, Surgeon, with colored plates. Philadelphia: LEA & BLANCHARD. 1850.

We called the attention of our readers some time since to this excellent work, now in course of publication. The three first volumes are already issued, and but one volume remains to be published. It is, without doubt, the best work upon Surgical Anatomy in this country. No plates that we have ever seen, will compare with these in point of accuracy or clearness. The price for this complete work is eight dollars.

For sale by G. H. Derby, & Co.

F. H. H.

Medical Appointments.—Dr. S. G. Moses, has been elected Prof. of Obstetrics in the Medical Department of the University of Missouri.

Prof. Dugas, who has hitherto occupied the Chair of Physiology and Pathology in the Medical College of Georgia, has been transferred to the Chair of Surgery, in the same institution, made vacant by the resignation of Prof. Eve; and Dr. H. V. M. Miller, of Georgia, has been elected to the Chair vacated by the transfer of Prof. D.

Obstetrical Clinique.—An Obstetrical Clinique has recently been instituted by Prof. Bedford, at the University of the city of New York. This is a novelty, if not an innovation in medical instruction, for which we think Prof. B. is entitled to credit.

By the way, it is gratifying to see that the senseless clamor of some, on the subject of College Cliniques has died away. They are not substitutes for proper clinical, i. e., hospital instruction, but they possess a certain share of the value which belongs to the latter. Students cannot see too much. Illustration gives not only interest, but practical efficiency to medical teaching. A person may be said to have *learned* what has been rendered intelligible to his understanding, but he cannot be said to *know* the truths of natural science, until his senses have taken cognizance of the phenomena to which these truths relate. Demonstrative instruction in all the departments of medicine, is, therefore, desirable. Obstetrics has suffered more than other branches, hitherto, in this country, for the want of this advantage, but we augur favorably for an improvement from the plan adopted by Prof. Bedford.

The Race of Man; A Fragment. By ROBERT KNOX, M. D., Lecturer on Anatomy, and corresponding member of the National Academy of Medicine of France. Philadelphia: Lea & Blanchard. 1850. 12 mo. pp. 328.

We have dipped a little into this work, but we do not profess any competency to judge of its merits, and it is so foreign to subjects most congenial to our taste, that we do not expect to give it a careful perusal.

The author is known as the English Editor of Quain's Anatomy. The proposition which it is his aim to unfold and establish is, that in the character of countries, and individuals, everything is due to *race*; literature, science, art, in a word, civilization depending on it. He appears to disclaim all belief in revealed truth, and doubts whether Christianity has done much for civilization!

For sale by Phinney, & Co.

Editorial Changes.—Prof. J. P. Garvin has relinquished the editorial charge of the Southern Medical and Surgical Journal, and is succeeded by his colleague in the Georgia Medical College, Prof. L. A. Dugas. This periodical has always been conducted with signal ability, and its character will not be likely to suffer in the hands of its present editor.

General Therapeutics and Materia Medica, adapted for a Medical Text-Book. By ROBLEY DUNGLISON, M. D., etc., etc., etc. One hundred and eighty-two illustrations. Fourth edition, revised and improved. In two volumes. Philadelphia; LEA & BLANCHARD. 1850.

In this new edition of a work sufficiently familiar, remedial agents of recent introduction have been added; the number of illustrations have been greatly increased, and a copious index of diseases and remedies appended.

These improvements entitle it to continued favor.

The general tone of the author's remarks on General Therapeutics is eminently in the spirit of the modern practice of medicine.

For sale by Phinney & Co., and Derby & Co.

Death of Hon. Jonathan Hoyt, M. D.—Died, on the 25th Nov. 1850
Hon. JONATHAN HOYT, M. D., aged about sixty years.

The late Dr. Hoyt was an old resident of this county, having been a practicing physician in the town of Aurora for nearly forty years. He was a man of eccentric habits, but was much esteemed as a practitioner in the part of the county in which he resided.

He was, for several years, one of the Judges of the county.

His death occurred suddenly. He was walking along Main-st., conversing with a friend, and apparently in perfect health, when he fell upon the side walk, and died almost instantly.

Vaccine Depot—We would direct the attention of those of our readers who may be in want of vaccine virus, to the advertisement of Mr. A. I. Mathews, of this city. We are assured by Mr. M., that pains will be taken to obtain supplies from none but reliable sources.

New Year.—With this number a new year opens. To our many personal friends embraced among the patrons of our journal, and to our numerous readers who are personally unknown to us, we tender, most cordially and sincerely, the salutation, of the season, 'a happy new year!'

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ORIGINAL COMMUNICATIONS.

ART. I.—*Electro-physiology. Report on the Memoirs of M. E. Du Reymond (of Berlin,) on Electro-physiological Phenomena, presented to the Academy, (Commission M. M. Magendie Becquerel, Despretz Rayer, Pouillet Reporter.) Translated from the French of the "Comptes Rendus," (No. 3, July, 1850.)* By JAMES BRYAN, M. D., Professor of Institutes of Medicine and Medical Jurisprudence in the Philadelphia College of Medicine, Member of the American Medical Association, President of the Medico-chirurgical College of Philadelphia, Professor of Surgery in Geneva, N. Y., &c., &c.

Three classes of phenomena are observed in Electro-physiology :

1st.—Those developed in electrical fish:

2d.—Those which are the result of some external known cause, as the commotion due to the shock of the Leyden jar, to the current of the pile, &c. We denominate these, phenomena of external currents:

3d. Those which result from unknown causes, and in which we, however, observe all the electric characters. These are denominated phenomena of organic currents.

This classification has nothing definitive; it is evidently transitory, and we adopt it to avoid confusion. When the currents which we denominate here organic, shall be better studied, when their causes, now unknown or imperfectly understood, shall have been analysed, or even circumscribed with more care, we may then establish rational distinctions. At the same

time, there need be no difficulty in comprehending them under a common denomination, especially, with the explicit reservation, that the denomination of *organic currents* prejudices nothing in reference to the producing cause; it is experience and discussion which must decide whether this cause be organic or in-organic; whether it is interior or exterior, and in the latter case, the corresponding phenomena will be arranged among those of the second order.

We know, for the rest, that Nobili has used the expression of *proper current* of the frog: that Matteucci uses the *muscular current*, and that Du Bois Reymond has employed the terms, *laws of the muscular current*, *laws of the nervous current*, &c.

We will include these expressions under the general one of *organic currents*, as embracing them all: but always under the reserve above expressed.

The memoirs which Du Bois Reymond has presented to the Academy refer only indirectly to electric fishes, we will then pass by these phenomena, in order to attend exclusively to the two other branches of electrophysiology.

1st.—Phenomena of External Currents.

The electric commotion was the first phenomenon which manifested the action of electricity on living bodies. Observers were not long in discovering that this commotion might assume all degrees of energy, that it might be strong enough to kill like powder, or weak enough to produce contractions or sensations almost imperceptible.

The pile of Volta, according to its strength, produced all the effects obtained from bodies simply electrified, or from the most powerful batteries. To reappear in another form, and in different conditions, the phenomenon did not retain all its primitive complexity; only, science may hope that, possessed of new means, she will one day be able to penetrate further than this singular action, and that she will discover finally the modification which the electric forces impress upon the organic forces, to determine the irresistible and instantaneous movement which constitutes the commotion.

We will not here recount the history of the different opinions which have been held on this subject, nor the various experiments which have been performed; but it will be proper to refer, in a few words, to the principal facts which enter into the investigation which we are to make, and see their bearing on the subject.

At the origin of galvanism, and in operating upon the frog, which we will denominate *galvanic*, to indicate that it has been prepared in the manner of Galvani, Volta was among the first who conceived from the contractions, important facts, viz:

1st.—That the contraction is almost certain in *direct current*, that is, that which courses the nerves in the direction of their distribution; and that they seldom take place in *inverse currents*; or in passing from the circumference to the vertebral canal.

2d.—That the contraction observed at the first passage of the current ceases, after a short time, and sometimes that it reappears at the moment that the current has ceased by the division of the circuit.

3d.—That the galvanic frog always becomes insensible to the current, either direct or inverse, when it has continued twenty-five or thirty minutes; but that it remains sensible to the contrary current, and that it may thus recover its sensibility, if in lieu of being subjected to a contrary current, it is allowed to repose a few minutes; from which fact the name of *alterative voltaics* has been given to this phenomenon?

In 1800, M. Le Hot observed that if the *direct current* induces the contraction at the first moment of application, it is the *inverse current*, he thought, induces the contractions at the instant of breaking the circuit; that the same thing occurs in reference to the taste, viz: it is excited on closing the circuit, if the current passes from the tongue to the metal, and at its rupture, if the current passes from the metal to the tongue.

In 1816, M. Bellingeri, in confirming the results in relation to the contractions, added this important circumstance, already intimated by Pfaff, Creve, and some others, viz: that these contractions take place with the same regularity and the same force, when, in place of passing the direct current or inverse currents through a muscle or nerve, we pass then, through a certain extent of nerve which has been isolated; if the direct current be applied, it will be transmitted from the proximal extremity of the nerve to the proximal extremity of the muscle; if the inverse current be applied, the positive wire on the contrary is to be applied to that portion of the nerve nearest to the muscle, and the negative wire to that part nearest the vertebra.

In 1817, M. Marianini, by very ingenious and well directed experiments, was led to announce the following proposition, that the *direct current* induced, at the instant of contact, a contraction, and a sensation at the instant of its cessation, and that the inverse current, produced in inverse order the two phenomena of contraction and sensation; but, to the present time, the experiments of others have not confirmed this law in full. It is difficult to secure all the conditions necessary to produce the same result.

In 1829, Nobili stated that the galvanic frog, on account of great vigor, experienced contractions almost as forcible at the moment of closing the

circuit, whether the current was direct or inverse, and that it was only when it was somewhat weakened, that the law observed by Le Hot, was presented with regularity. He has stated another fundamental fact; it is that while acting on *solitary and isolated nerves*, by means of either the direct or inverse currents, with a certain degree of intensity, *tetanic convulsions* or an *electrical tetanus*, analagous, perhaps, to ordinary tetanus, with the simple condition of establishing and breaking the circuit, stroke after stroke, at short intervals; from it follows, says he, "that a continuous current tends to weaken the nerves, and an intercepted one, to excite them."

In 1844, M. M. Longet and Matteucci, in isolating the anterior root of the spinal nerves, in the horse, dog, rabbit and frog, to subject the longest portion possible to the direct and inverse currents, announce that the results obtained were exactly the reverse of those presented by mixed nerves; that is to say, that in the anterior spinal root, the contractions took place, only at the commencement of the inverse current, and at the discontinuance of the direct current.

In 1844, M. Matteucci having concluded, from some interesting experiments, that under the influence of the direct current a nerve loses sooner its sensibility, than under the influence of the inverse current of the same force, recurring later, in 1846, to this first result, and after having applied the mechanical means of experimenting, which we will allude to, he was led to make the following general proposition:

"That the electric current circulating in mixed nerves of a living animal, or recently killed, varies the excitability of the nerve; if the current is direct, the excitability is diminished and destroyed, while this excitability is preserved and augmented by the passage of the inverse current."

In 1847, M. Matteucci, continuing his experiments on the comparative effects of the inverse and of the direct currents, gives as the consequence of these investigations, this result, worthy of attention, viz: that in separating the two members of a galvanic frog, and in so disposing them, that the nerve of one shall be traversed by the direct current, and that of the other by the inverse current, furnished by a pile of *Farradis*, of 15 or 20 elements, the first becomes insensible, in conformity to the observation of *Volta*, after twenty-five or thirty minutes; then if we continue passing the current again for a few minutes, to break again the circuit, it happens that at the instant of the rupture of the member which was traversed by the inverse current, instead of being insensible, it enters into a tetanic contraction, which is suspended if the current be again passed in the same

direction, but which will persist for several minutes if the division of the circuit be continued.

Such are the series of results which appear the most important in reference to the effect of exterior electricity on organic nature. We do not say that they are incontrovertable, for some of them appear contradictory; but many of them are based upon experiments whose accuracy has been tested by different observers. While others require additional confirmation.

It was necessary to recite these facts, without which we could not explain those which are connected with what we have denominated organic currents, of which we now proceed to speak.

2d.—Phenomena of organic currents.

Every one, at the present day, is familiar with the names of Galvani and Volta. Everybody knows also, of the memorable discussion carried on by these illustrious founders of electro-magnetism, more than half a century ago.

The question was this: are the contractions of the galvanic frog due to its own, or to electricity from without. Galvani advocated the electricity of the frog. Volta, the external electricity; and it is a curious fact, well calculated to inspire those who seek truth, with prudence and modesty, even those who have the greatest penetration and most firm convictions, and these two opinions, apparently exclusive and contradictory, are probably nothing more than a mixture of error and truth. In certain cases, the contraction results from external electricity, as Volta says, but this electricity has not the origin which he assigns to it. In other cases, the contraction results, perhaps, from the electricity of the tissue, according to Galvani, but its source is unknown, and we will see, further on, the opinions which we have gathered in this matter.

As the opinion of Volta prevailed for many years, it is not to be wondered at that before the voltaic pile and its effects, the opinion of Galvani was neglected, that it was almost abandoned to oblivion; its time had not yet come; not only had it not attained confidence and support by any discovery, but it had been kept in a state of impotence disabling it from accomplishing anything.

It followed from this, that the electro-magnetic action was known, that the multiplier was invented, and finally perfected by the principle of compensation.

Then Nobili, the author of this ingenious finish, who gave to the Galvanometer a sensibility analagous to organic sensibility, had scarcely finished his apparatus when he made a most happy application of it.

He desired to know whether his galvanometer, more delicate than the galvanic frog itself, in its impressions, would indicate the most feeble electric forces. It is in this comparison of his two apparatus, of dissimilar structure, subjected, however, to a common law, affected, each of them by the same influence, that Nobili has demonstrated a new fact, which would revive the theory of Galvani. The memoir of Nobili is dated at Reggio, the 3d of November, 1827; it was soon followed by another of the 1st of November, 1829; these two memoirs must be considered among the most remarkable of those which have appeared on this subject, as well for the new truths which they contain, as for the precision with which they are described. Without making a complete analysis, we will present their general facts as follows:

1st.—The galvanic frog has a peculiar current directed from the muscles to the nerves, or from the feet to the head.

2d.—In placing successively, one after another in the same order, several galvanic frogs, we obtain a pile, whose tension increases with the number of elements, as is demonstrated by the increasing deviations of the needle of the Galvanometers.

3d.—We determine the presence of feeble external currents and their direction, by causing them to pass through a free portion of the nerve of the frog; the contractions which it induces, shows the current to be direct or inverse, according as they take place at the junction or separation of the current.

We should bear this observation in mind, to understand with what care Nobili has himself indicated the habit of the frog, which M. Matteucci afterwards, denominated 'the *galvanoscopic frog*, after finishing its preparation, and which Du Bois Reymond has since called the *rheoscopic frog*. We prefer the last name because it is less liable to be confounded with the "*galvanic frog*," which we have adopted as indicating the frog prepared after the manner of Galvani.

After these researches, of so much interest, due to the rare sagacity of Nobili, those of Matteucci come next in the order of dates, which were conducted at the same time, on the subject we are treating of, and upon electrical fishes; but as already indicated, the latter subject will not now be treated of.

M. Matteucci, during a period of ten years, beginning at 1838, has published a large number of memoirs, in which he exhibits laudable and persevering efforts, to obtain general conclusions on the phenomena of electricity, as manifested in cold and warm blooded animals.

ELECTRO-PHYSIOLOGY.

He enjoys the double merit of having instituted, in this respect, a large number of experiments which are consulted with interest, and of having contributed greatly to attract attention to these remarkable phenomena. Among the various conclusions to which he has arrived, in his labors, published in his memoirs in 1841-42-43, and in his treatises on Electric-phenomena published in 1844, there are two which appear to be the most exact of the new truths to which he has been led, viz:

1st.—That in all cold and warm blooded animals, either living or recently killed, there is a muscular electric current directed in the muscle itself *from its interior to its surface*.

2d.—That a *rheoscopic frog* exhibits contractions when its nerve is placed in contact with the muscle of another frog, or with that of a rabbit, and produces in the muscle decided contractions, either with the aid of an exterior current or of mechanical actions.

It is this last phenomenon which has since received the name of *induced contraction*.

But the first of these propositions removes immediately the question whether the *muscular current* of Matteucci, is anything else than the (current proper) tissue current of Nobili; after reiterated attempts, it appears that Matteucci has not been able to establish any well defined distinction between these two.

In reference to the second proposition, that of the *induced contraction*, it establishes a new and important fact, but it is to be regretted that Matteucci has not recognised the consequence which has been suggested to him on this subject by our friend M. Becquerel, and that Matteucci has taken care to assign it to himself after his memoir. (Annales de chimie et de Physique, 3 serie, tome vi, page 39.) This idea would doubtless have led Matteucci to more profound observations than those which he has made in order to explain *induced contraction*.

We come, at last, to the researches of Du Bois-Reymond, who commenced in the month of January, 1843, (Annales de Poggendorff,) and has continued with success up to the last communication, which he has made to the Academy. We will attempt, also, in the matter before us, to condense the chief propositions to which he has arrived by different series of experiments, which, if we may judge by those made in our presence, bear in the highest degree the stamp of great precision, marking the skillful experimenter, joined to the skillful Physiologist.

We hope to neglect nothing essential, in presenting under the following form, the laws established by du Bois-Reymond.

1.—The nerves after their section, and during their vitality, that is, during the whole time that they are capable of exciting muscular contraction, or of transmitting impressions, give origin to a current which is perceptible by the galvanometer, and which, outside the nerve, is directed from the surface, or from the longitudinal section to the transverse section.

The intensity of this current depends upon the position and the distance of the points through which the nerve is placed in the circle of the galvanometer; it is not when these points are symmetrical on account of the equator of the nervous trunk considered as a cylinder; it is maximum, on the contrary, when one of the points of contact is on the equator, and the other in the centre of the cylinder, that is to say, in the centre of transverse sections.

2.—The muscles of all animals, while they are susceptible of contraction under any influences, manifest a current analogous to that of nerves subjected to the same laws as well in the direction as the intensity.

Of which it should be remarked that certain muscles, such for example as the gastrocnemii and the triceps of the frog, present *natural transverse sections* where the muscular fibre is joined to the tendon, the muscular aponeuroses being nothing more than coverings of the natural transverse sections.

3.—In comparing these different muscles among themselves, we observe that the intensity of the current is in proportion to the amount of mechanical force exercised by the muscle, whether voluntary or involuntary; thus the fibres of the heart, which are involuntary, manifest an energetic current like the muscles of relation, which are all voluntary: at the same time the muscles of the intestines, having but feeble force, manifest a very feeble current.

4.—When a current produced in a gastrocnemius muscle of a frog is observed, and that from any external cause, electric or non-electric, we observe in the muscle repeated muscular contractions, and see in an instant the intensity of the ordinary and natural current to which it gave origin, exhibits a very remarkable diminution of intensity.

It follows that muscular contraction, whatever may be the cause, does not take place, until some considerable change in the interior electric circulation has taken place.

The rheoscopic frog, placed in contact by its nerve, and under the requisite conditions, with this muscle tetanized, exhibits itself corresponding contractions, which result from this diminution of intensity; it is observed to act convulsively if the muscle with which its nerve is placed in contact is itself in a state of convulsion; and if, on the contrary, the contrac-

tions of this muscle are distant and successive, the rheoscopic frog in some degree counts them, and measures them by distant and successive movements which correspond. This fundamental fact presents a direct explanation of the *induced* contraction of Matteucci.

The rheoscopic frog, which has only its nerve placed in the circuit, presents the same phenomena.

5.—When we observe on the galvanometer a current produced by the nervous trunk, which enters, for example, the circuit, only with half of its length, touching on one side by its transverse section, and on the other by the points of its equator, and we attempt to produce different actions on the end of the free half which is without the circuit, we see that the ordinary and natural current to which it has given birth, exhibits a diminution of intensity, analogous to that which is seen in the muscle at the moment of contraction. The actions which we induce on the free extremity of a nervous trunk, may be either a direct or the inverse current, either a cauterization, an intoxication, or a mechanical bruising.

It follows that the local actions which they transmit, either to the muscle or to the nervous centre, if the nerves be not detached [the one from the other, appear effectual in modifying the electric condition of the nerve in portions even where it is not directly affected.

6.—After having cut, near the pelvis, one of the sciatic nerves of a whole and living frog, it may be so disposed, that by each of its inferior extremities it enters into the circuit of the galvanometer, and closes it, and no electric phenomenon appears. Inducing the absorption of nitrate of Strychnia, tetanus is manifested, and is manifested only in the inferior extremity where the nerve has not been cut; at the instant that the needle of the galvanometer has a current which is without, directed from the contracted member to that which is not, and which is consequently a direct in the contracted member.

Such is the view which we can now present of the principal results of du Bois-Reymond.

Each of these propositions is nothing more, as already stated, than a general enunciation of a great number of comparative experiments, performed and arranged with care. M. du Bois-Raymond has repeated before us all those experiments which we consider the most important, and he introduced, at our request, all the modifications which we thought of.

It will be readily understood how much zeal and penetration would be necessary to appreciate the profound reasoning, in a subject comparatively new, where it may be necessary, in some sort, to create means of observation, modes of experiments, as well as experiments themselves.

M. du Bois-Reymond had, as a point of departure, the results of Nobile and those of Matteucci, previous to 1843, which we have given above. From these one may judge of the progress which he has made in this rising portion of electro-physiology, and at the same time the important direction he has given to experimental science.

Thus far we have spoken of the works of M. du Bois-Reymond exclusively, in reference to experiments on animals, and the laws which he has deduced from them; we must now speak of another observation which merits particular attention.

Every one knows that we design to speak of the current which appears to be developed in the human body, endowed with full life, at the moment of the contraction of the muscles of the arm by voluntary action.

This new fact, discovered by du Bois-Reymond, is not less positive, nor less clearly confirmed than the preceding; we add, besides, that it is not the less general; that is to say, the first person who comes, when we explain to him the mode of procedure, will without fail produce a greater or lesser deviation in the needle of the galvanometer; at the same time, the intensity of the effects seems to be due rather to the power of the will, than to the force of the muscular contraction.

We will present this fact in a distinct point of view for two reasons; because it raises an important question, and because it has been the principal object of the communications which M. du Bois-Reymond has made to the Academy. In his first note we read these words: "The object of this note is to inform the Academy of the series of experiments which have terminated by leading me to the discovery of the development of an electric current in the muscles of a living man at the moment of contraction."

"The great question which this fact solves, then, is this: in the living man, is there an electric current developed at the moment of contraction?"

We proceed to state that the production of a current is incontrovertible; that this current is demonstrated by the galvanometer with less proof than those developed, under the requisite conditions, when the muscles or the nerves are introduced to the circuit. It is already a fundamental point, but this is not all; it remains to see whether this current, of which the galvanometer proves the presence, is, in effect, developed in the muscles, and if it be the necessary result of their contraction.

Now there is here abundant material for controversy; we do not expect to decide the question; we will attempt only to discuss it, and to indicate the doubtful points. To simplify this examination, to proceed by degrees

from the more simple to the more complex phenomena, we must commence by taking a bird's eye view of the organic currents in general, in order to separate, as far as we can, that which is known from that which is unknown in their origin and cause.

We have stated that the tissue current (*courant propre*) of the frog was advocated by Galvani and opposed by Volta. They agreed in the fact, each admitted the simple contraction of the frog, that is to say, that which was produced by the contact of a nerve, and by certain points of a muscle; but, while Galvani attributed this phenomenon to the tissue current, Volta explained it by the heterogeneity of the elements placed in contact, and by the *electro-motive force*, which in his theory, is its result.

The progress of science, especially since the discovery of electro-magnetism, has by degrees brought to light a new force, suspected before by Fabroni and other experimenters, which is capable also of developing electricity and of generating currents; this force is *chemical action*. As soon as this power was well established, it immediately invaded the vast domain which the genius of Volta had attributed to the electro-motive force, and of which, step by step, it has taken real and sovereign possession.

The electro-motive force, such at least as is now admitted, has disappeared almost entirely, and with it have disappeared also the explanations given by Volta of the simple contraction of Galvani.

But in examining in its turn chemical action itself, do we arrive at this conclusion: Is this the sole cause capable of producing these electric currents? This question has long since been answered in the negative.

In admitting the existence of ordinary electricity developed by friction and pressure, there are two great classes of phenomena which belong evidently to chemical action, viz: the electric phenomena which are presented by crystals analogous to tourmaline, and by the thermo-electric phenomena.

Has science, arrived at that point, or not, at which it can enumerate, without cavil, all the several causes which give rise to the development of electricity? Can it say that every electric current is induced essentially by the causes at present known and established? We do not think so. On what ground indeed is this to be the case? The actual causes are diverse and without explanations; we know that they are effectual, but we do not know why they are effectual; we presume that there is among them a certain connection, we neither know in what this consists, nor in what it can consist; we are struck with their diversity, but are ignorant of the principle which doubtless binds them together. Now, in this ignorance, almost absolute, how can we affirm that this primitive principle shall never

manifest itself under new forms and aspects, which are at present hid from our view?

Our efforts should then have a double object; to distinguish analogous phenomena and attempt to search out their laws, and mark, as well as we can, the character of the cause which produces them: to seek new phenomena to discover new causes, or to investigate more thoroughly the causes already known.

The phenomenon above stated, so well observed by Nobili, although not new in every respect, is not the less, under the present aspect, a discovery of much importance.

In the first place because it teaches, for the first time, this fundamental fact, that the galvanic frog presents a current capable of causing a deviation in the charged needle, a regular current, in a definite direction, and dependent on the organic sensibility, in this sense at least, that it follows or marks all the phases; increasing when it increases, and ceasing when it ceases.

Secondly, because the cause of such a current, instead of disappearing with evidence of the mode of experimenting, appears to be hid in the depths of organic nature itself.

Indeed, Nobili has indicated in passing, that the tissue current (courant propre) may perhaps have a thermo-electric origin, but he has not demonstrated it, and it should be added, no experimenter has attempted to demonstrate it, so slightly are the analogies in favor of this opinion.

After Nobili, it has been thought that the tissue current owes its origin to chemical actions; but as yet this opinion does not appear to have received sufficient support. Chemical action has its infallible criterion; when invoked we are bound to show on what it is exercised and what it accomplishes. Now, in the matter before us, no one has pointed out either the elements in action or the products formed. There is indeed a necessary distinction on this point; if the tissue current is the result of a chemical action, is it not important to know whether it is an internal or external action? That is, whether it is a simple reaction of the constituent organic elements, without the influence or concurrence of any ponderable foreign agent, or whether it is an action exercised on the organic body by exterior means which are in contact with it.

In the latter case, the galvanic frog, or the organized body in general, is simply analogous to plates of zinc and copper, which do not possess in themselves the power of developing electricity, and which demand, to obtain this power, to be placed in contact with an acid or other conducting medium capable of combining with them.

In the first case, on the contrary, the organic body possesses in itself the power to generate currents; it possesses it either from its nature, or from its structure, or from the chemical reaction of its proper and constituent elements.

We are thus forced back to the discussion between Galvani and Volta; the ground is not the same, the arguments and proofs have changed character, but at bottom it is the same thought.

It should be remarked, again, that this distinction applies not only to the phenomena before us, but also to electrical fishes themselves, that is to say, in general to all the electro-physiological phenomena which are not the evident result of electricity whose source is without, and which is made to pass artificially into organic bodies.

Electro-physiology scarcely exists, it is connected with phenomena infinitely amplex, which appear to be one of the links which bind inorganic with organized nature, it must in common with the other experimental sciences appeared, with methodic and reflecting care, the ground on which it proposes to be built.

For this reason we insist upon this primary and old question: the currents which are manifested in the tissues of life, or of recent life, have they an internal or external cause, a known or an unknown cause? It is to be regretted that this question has not been approached in a manner very explicit by the above named experimentors; they would, doubtless, have conceived experiments directly to the purpose, and calculated to remove all doubts.

The following, then, may be drawn from their experiments, although they were not instituted for that purpose:

M. Matteucci, pursuing the idea of Nobili, and perfecting it, has constructed a pile by the simple juxtaposition of organic elements, without any intermedium, the two extreme elements alone, being in connection by means of fluid conductors; now the deviations by these piles appear to indicate that if the currents were due to an external chemical action, it should follow, that when the organic elements were in contact, the chemical action to explain why it was developed in the second and not in the first place.

Many other experiments must be resorted to with the same view.

In the actual condition of things, the committee have not been unanimous in forming a definitive conclusion; it is disposed to say only, that the whole of the phenomena present a great probability that the organic currents are not the effect of an external chemical action, but that it will be

well to present proofs more positive than those which have as yet been presented.

In supposing this first question resolved in the sense that it ought to be, another presents itself which is not eventful to every body, and which has already been the subject of many discussions. It is this: the currents developed, do they originate in an internal chemical action, or from the nature only of the structure of the tissue submitted to their particular forces?

Nothing is more evident than the prodigious variety of the chemical phenomena which are brought to view in an organized being at each instant of its existence. Among these, there are some which cease immediately with life, and others which continue under the influence of forces with which we are but imperfectly acquainted; these, considered as simple chemical phenomena, do they present a clear exposition? either the cause should be the same as when they touch the fluid conductor, which is different in its nature.*

M. Du Bois-Reymond, having demonstrated that the nerves give birth also to currents, whose laws he has established, it should follow, that the action of the fluid conductor, with which he had placed them in contact, should be the same on the substance of the nerves as on the substance of the muscles; and as besides, we can make piles with nerves as well as muscles; it should follow the more, that the transverse and longitudinal sections of the nerves should excite, among themselves, a chemical action, such as they exercise on the fluid conductor.

We regret not having demanded of Du Bois-Reymond the performance, before us, of the following experiments, which he has probably had occasion to perform in the course of his researches: a nerve on which a ligature has been applied, retains a sufficient amount of conductivity; it may be introduced into the circuit in two modes, either by the connection of the ligature itself, or by leaving this out, and placing the nerve in such a position that it shall touch on one side by its transverse section, and on the other by its longitudinal section. Now, should it happen that in the first case, it presents nothing, and that in the second case it exhibits an ordinary current, it would be difficult to attribute the current to an external chemical action, for it would be very difficult, of the contractability which exists in the muscles, of the sensibility in the nerves, or of other propor-

* There is an obvious obscurity in this passage which we are unable to clear up. The manuscript of the translator is literally followed, but something is evidently omitted.—EDITOR.

tions which still remain, for a longer or shorter time, according to the rank which the animal occupies in the scale of being.*

It appears to us, that, in answering, in the affirmative, in accepting this explanation as sufficient, we cause chemical action, which is so clear and precise, to descend to the rank of occult and intangible causes, we cause it to loose its essential character which it has obtained from positive analysis and its effects.

But if we cannot now hold that the chemical actions, which succeed life, explain all the persistent organic properties, can we say that they explain all the electric phenomena which are observed on the galvanometer; and which diminish with the remains of vitality. On this point, opinions are divided; no person, doubtless, holds that the chemical actions, which are developed, are not accompanied with a disengagement of electricity; but some, contented with this general idea, regard as very probable, if not as certain, that this electricity is the cause of the organic currents; others are doubtful, and wait for further analysis and study of the chemical actions, and while waiting, doubt, more or less, whether, by this means, a complete and explicit explanation of the intensity and all the other characteristics of the organic currents, will be obtained.

This is, then, but one question to be solved, a general question, in which we must not loose sight of the fact, that not only have we to seek the cause of the currents observed in the nerves and muscles, but that there are two phenomena besides to be explained, viz: the intermittent diminutive force which the muscular current undergoes during the contraction of the muscle, and the modification observed in the nervous current during the excitation of the latter.

We must, moreover, make another remark: the laws given by Du Bois-Reymond, are presented, in general, either by layers of muscles and nerves connected with a painful existence, or by detached portions whose separation has been so recent, that they still retain something of what we denominate *organic sensibility*. But are the incisions of the scalpel without influence? Does not the mutilation itself enter into the phenomena observed? The exudation of fluids, altered currents by endosmosis or otherwise, do they not take some part? These questions demand serious examination; they should be discussed and solved, concluding from a part to the whole.

Here, induction alone is powerless, we want proofs—proofs positive—to

* See note on the preceding page.

be authorized to explain the entire muscular system, and especially the whole nervous system, that which results from a separate portion of nerve and that which is the result of the scissors.

Thus, then, in fine, our opinion of the cause of the organic currents in general is as follows: their cause is unknown.

1st.—It is probable that these currents result from exterior chemical action.

2d.—It has not been demonstrated that they result from an interior chemical action; there is here a question for solution, and as it shall receive a positive or negative answer, the ulterior consequences vary.

Let us return to the current which appears to follow or result from the contraction of the arm.

These are the elements of the discussion,

1st.—If, according to the experiments mentioned in the sixteenth proposition, (of facts observed by Du Bois-Reymond,) a frog does not present a sensible current, it is that the inferior extremities tend to produce equal and opposite currents.

2d.—When, having cut one of the sciatic nerves to paralyze the limb, tetanic contractions are produced in the other, the intensity of its current diminishes in conformity with the fourteenth proposition; then the current of the limb paralyzed or condemned to repose, becomes predominant, and the galvanometer renders it visible with the direction it should take; that is to say, that it is *direct* in the tetanised member.

3d.—When both arms are placed in the galvanometer, none but accidental effects are observed, dependent, doubtless, on the cutaneous condition of the fingers, which touch the conductors; when these irregular effects have ceased, and the needle of the galvanometer has become stationary, voluntary contraction of one arm is induced; when the needle of the galvanometer is at once effected, its deviation always shows an *inverse* current; that is to say, directed, in the contracted arm, from the hand towards the shoulder.

Now, if we examine these last propositions, to see their connection, a difficulty will be found, thus; between the second and third, there is a certain analogy; there, then, is a leg which contracts artificially, here an arm which contracts voluntarily; but why is the current *direct* in the first case, and *inverse* in the second? This is an important point; it is to be regretted that Du Bois-Reymond, who has himself taken care to point out this difference, this constant inversion in the course of the current, has not felt the necessity of explaining the reason; while this explanation remains un-

made, we must doubt whether there is a necessary, or even any, connection between the third and the second experiments. According to the principles of Du Bois-Reymond, the effect of a sustained contraction, is not to induce a current, but to enfeeble and to suspend, by intermission, a pre-existing current; there must, then, be a pre-existing current, as indeed there must be, two equal and opposed, which neutralize each other, while the needle of the galvanometer is at Zero; the one must be essentially in the arm, which contracts, and it is this which the contraction enfeebles; the other, for the same reason, must be in the other arm, and this one, the contraction renders dominant. Thus the current, observed at the instant of contraction, is not developed in the arm contracted; it is, on the contrary, pre-existent in the quiet arm, and it exhibits itself only because it ceases to be entirely neutralized.

If the question should be put in these terms, it appears to us, to assimilate this experiment to the preceding; one condition must be fulfilled, that is, to demonstrate clearly that the muscles of the arm of a man, in a state of contraction, if considered as in their natural condition, are disposed in a way to induce a direct continued current, going from the shoulder to the hand, and that they induce this current according to the laws of longitudinal and transverse sections. This condition is indispensable, while if it is not answered, the experiments cannot be assimilated; we cannot, and ought not to regard the third proposition as being a consequence of the first. But let us admit, for an instant, this first difficulty to be removed, that the form of the muscles of the arm which are here engaged, that their structure, their interlacement, their absolute and relative condition, conduce to the desired conclusion, that is to say, that it suffices to apply to it the muscular current, to demonstrate that in composing the directions and intensities, we obtain as a final result, a continued current from the shoulder to the hand: will the whole question be resolved? Should we consider as certain that the third experiment is identical with the second, and that it is explained fairly by the same cause? We do not think so. There will still be doubts depending on the diversity of the conditions and of the complexity of the problem; but the time has not arrived to resort to them and to discuss their value.

In fine, the current which appears to pertain to the muscular contraction of a living man, is a very curious phenomenon. While applauding this discovery of M. Du Bois-Reymond, while, according to it, perhaps, intimate connections with other electro-physiological phenomena, of which he has

so skilfully studied the laws, we cannot admit that these connections have as yet been demonstrated in a conclusive manner.

We will not conclude this Report without making a reflection designed to encourage M. Du Bois-Reymond, to stick closer and closer to those vigorous methods which have conducted him to so many new facts. It is impossible not to remark some words which he uses at the commencement of his first Memoir, and in which he announces "that his researches border on a positive theory of the nervous agent, and of the motive power of the muscles."

While the text of his communications does not approximate the discussion of any theory, it contains facts whose correctness and importance we have attempted to point out.

The theory announced in the words quoted, we have not examined; we can only consider them as a special point of view, an abstract idea, a future thought to be placed before the Academy. It appears quite natural that Du Bois-Reymond should expect for himself great things, from the fecundity of the new career in which he has entered to investigate truth, the first approaches of which he has explored with a success worthy of praise. Thanks to the modes of observation he has conceived, he has discovered facts of a certain character, which will add much to science; but nothing demonstrates, as yet, that they are other, that is to say, truths which explain the *nervous agent and the motive power of the muscles*. If, one day in future, after long and laborious researches, these truths having been subjected to the crucible experience, remain, science may then be said to have made a bold stride onwards.

Finally, your committee have witnessed with the liveliest interest the experiments of Du Bois-Reymond. They bear testimony to the great precision of those performed before them, and add, that they expect much from researches still more exact, which will be made in this direction.

In conclusion, we propose to the Academy to thank M. Du Bois-Reymond, and to congratulate him on his diverse series of facts which he has demonstrated by experiment.

The conclusions of the report were adopted.

ART. II.—*Case of Obstipation, with Stercoraceous Vomiting, etc.* Reported by the Editor.

On the 30th of Nov., 1850, at 5, a. m., I was requested to visit Mrs. N., aged about 25, a lady with whom I had had no previous acquaintance. I

found the symptoms and history as follows:—In the early part of the night she had begun to suffer from pain in the abdomen, back, and lower portion of the chest, which progressively increased in severity, with occasional remissions, until it was deemed proper to call in medical aid. She had been engaged the day before, and for several days previously, very constantly in *quilting*. I ascertained that she possessed a highly impressible constitution, that her health had been for some time delicate, that she had suffered for a long period from leucorrhœa and menorrhagia, and, recently, from diarrhœa. The spine was highly sensitive to pressure. The pulse was not accelerated; skin cool. Friction over the situation of the pain was attended by some relief. When the suffering was severe she threw herself wildly about the bed, and exhibited hysterical symptoms. Bowels had not been moved since the day but one prior to the attack.

Regarding, from the above assemblage of facts, the pain as neuralgic in its character, I prescribed morphia, and sinapisms, and left her at 7, A. M., partially relieved. Slight vomiting occurred during this first visit.

At 9, a. m., finding that the pain persisted, and was still severe, I left directions to continue the morphia in doses of gr. 1-4, and was detained by other engagements until noon. In the mean time the pains continued with such severity that Dr. Mixer, of this city, was called in. Some uterine flowing, in the mean while, occurred. Dr. Mixer prescribed morphia in combination with the acetate of lead. The pain persisted, but was in some measure alleviated, until evening, when she became quite easy, and passed a comfortable night.

On the following morning, Dec. 1, I found her free from pain, pulse but little accelerated, complaining somewhat of nausea, which was attributed to the morphia. Rest and simple nourishment alone were advised, and I did not propose to visit her again until the next day.

At noon I was summoned, and found the pain had returned with great severity, and it was now attended by marked hysterical symptoms—violent sobbing and weeping. She had felt so well during the forenoon that she had received some visits, and became excited. It seemed fair to attribute the renewed attack, in part, to this as the cause. Morphia and hot fomentations were directed. The pain, as before, was not confined to one situation, but shifted from one point to another—now in the abdomen, now in the inferior part of the chest, and frequently in the back. The menorrhagia had ceased the day before, and she had now only the usual menstrual discharge.

The pain continued, with great acuteness, during the evening and night. She took during the night four grains of the sulphate of morphia, and, *per enema*, a drachm of *Tinct. opii*. She occasionally vomited, but this was not a prominent symptom, and did not occur save when fluids were taken into the stomach. By withholding drinks vomiting was prevented. She complained of thirst, and entreated for water. The pulse had now become moderately accelerated—about 100. Tongue moist and slightly furred. No dejection from the bowels since the attack.

On the 2d Dec., the pain continued, but with less severity. It was occasionally severe, but much of the time not very distressing. During the forenoon of this day vomiting became a prominent symptom, and she began to eject a yellowish, and brownish yellow fluid, very fetid, having distinctly a stercoraceous odor. The quantity vomited was very copious, and occasionally it contained small portions of stercoraceous substance. The vomiting at first afforded relief, and was encouraged by the patient under the impression that matter so offensive should be expelled. The vomiting of this matter continued through the day. Several quarts were ejected. No dejection occurred from the bowels. The abdomen was slightly tympanitic. Percussion elicited flatness over the right iliac region, but no distension existed in this region. The abdomen was universally tender on pressure, but not extremely so. Friction was well borne, and afforded relief. She was at evening much prostrated. The pulse became frequent, numbering 130, small and feeble. Skin warm. Pain was diminished.

Dr. White saw her in consultation on this evening, and we concurred in directing an enema of *T. opii*, ζij , and anodynes, *per orem*, if retained, sufficiently to relieve the pain.

Nov. 3d. She had passed a more comfortable night. She had experienced considerable pain, with restlessness, and vigilance, but less suffering than hitherto. Pulse 120, tolerably developed. The vomiting continued, but the matter vomited was less offensive. It had now a greenish tinge. No dejection. Skin warm. No delirium. Considerable prostration, but she retains sufficient strength to move about, change her position, etc. Abdomen not more tympanitic. Tenderness the same. The inguinal and femoral regions on this day, and previously, were explored for hernial protrusion.

On this morning I introduced a flexible tube into the rectum about 18 inches. It passed thus far with considerable difficulty, even after advancing beyond the sphincter, being detained several times by obstructions which yielded to continued gentle pressure. About a quart of warm water, con-

tingai molasses and castor oil, was injected by means of a forcing syringe attached to the flexible tube. More would have been injected but the tube did not prove sufficiently firm to resist the force of the syringe.

It should have been stated that, on the second day after the attack, i. e., Dec. 1, an enema was given with the common syringe, which was returned with a very little foecal matter; and on the day following, Dec. 2, she had several enemas which came away unaltered. *Castor oil entered into these injections.*

The injection with the flexible tube on the 3d gave no pain, and was shortly returned unchanged. The finger introduced into the rectum passed with facility, and without pain. The uterus was ascertained to be in right position.

Vomiting occasionally occurred during this day, and the patient complained of the taste of castor oil in the matters vomited. It was also stated that oil was observed floating on the fluid vomited. The latter I did not see, but the above was stated without any inquiries being made relative thereto, the patient and friends, too, having no idea of the pathological significance or importance of the facts. As no oil had been administered *per orem*, it could only have been derived from the enemas.

During the afternoon of the 3d inst. I resolved to try the effect of administering croton oil. She took two pills, each containing a drop of the oil. She retained the first pill a few moments, and severe vomiting then occurred. The second pill was rejected almost instantly, and with severe efforts of vomiting.

Dr. W. visited in consultation on this day, and we again concurred in the propriety of keeping the patient under an anodyne influence, to continue stimulating embrocations over the abdomen, and to cover the abdomen with a thin warm cataplasm.

Dec. 4th. Had passed a tolerably comfortable night. Aspect better. Pulse 100. Skin warm and mellow. Abdomen, as respects tenderness and meteorism, about the same. Resonance over the abdomen tympanitic, except in the *left* iliac region. *No dejection.*

I resolved to try again injection with the flexible tube. Procuring a strong stomach tube, it was introduced without difficulty, about six inches. I could not cause it to advance farther after prolonged efforts, and the exertion of as much force as was deemed prudent. On withdrawing the tube the point gave no evidence of having been in contact with fecal matter. Re-introducing it to the same distance and commencing the injection, in a short time the tube was pushed upward, with facility, about six inches far-

ther. About a quart of warm water, with salt and molasses added, was injected into the intestine, when the patient complained of a sense of distension, and an irresistible desire to evacuate the bowels. Withdrawing the tube the injected fluid came away, containing numerous small flocculi of mucus, tinged with fecal matter. Leaving the patient at this juncture, and returning in about two hours, I found she had passed, in the interim, a large quantity of feces, filling about two thirds of a chamber pot. The feces were of a brown color, of the consistence of mush, not containing any solid portions. The first evacuation was about ten minutes after my departure, and two subsequent evacuations had occurred, all without pain.

The vomiting had much diminished prior to the injection of this morning, and she had been able to keep a little wine and nutriment on the stomach. After the three large evacuations above mentioned, there occurred during the day three small ones. At evening she was comfortable. Pulse about 100, abdomen moderately tympanitic, still tender on pressure, and pressure, as heretofore, (which has not been mentioned,) occasions nausea and efforts to vomit. She is much prostrated, and complains chiefly of a sense of weakness. The treatment now directed was to continue the embrocations, and repeat opiate enemas should there be pain or much restlessness; to give wine, milk, and essence of beef, in small quantities at a time, often repeated.

Dec. 9. This case has progressed favorably since the date of the last record. On the 5th inst. she had a recurrence of severe enteralgic pain, which was relieved by large doses of morphia. She has suffered daily, since, from paroxysms of pain, but on each day less in degree. The pain is referred to the abdomen and back. The spine is very tender on pressure. The abdomen has been moderately tympanitic. The bowels have required enemas, but have been readily moved in that way. She has now some appetite, and gains strength daily. She has had no vomiting since the 4th inst. She is taking Sol. Sulph. Quinæ, wine, nutritious diet, and pustulation over the spine has been produced by the croton oil.

Up to this date, Jan. 1, 1851, this patient has continued to improve. She is now up and about. She has had no recurrence of vomiting or pain. She is under treatment for Leucorrhœa, and is taking chalybeates.

Remarks. The interest and novelty of this case seem to the writer to warrant the foregoing detailed report. The history shows it to have been a case of spasmodic, inverted peristaltic action, extending over the whole tract of the intestinal tube. This view of the pathology could not, of course, be deduced from the symptoms prior to the occurrence of the evacuations from

the bowels, after an entire obstruction of seven days' duration, on Dec. 4th. Up to that date, after the symptoms assumed a grave character, intus-susceptio, flexion, impacted hardened feces, or some other obstacle to the passage of the intestinal contents, more serious than inverted action, was apprehended. Indeed, this was the view entertained, and the prognosis was accordingly unfavorable. But it is to be observed that while circumstances led to this view of the case, the local and general symptoms were not so intense in degree as they usually are when an irremediable lesion exists. The tenderness over the abdomen was not so extreme, and its neuropathic character was shown by the fact that friction was well borne, and afforded relief. The tympanites was less prominent. The tenderness, moreover, was diffused over the abdomen, and even over the chest, and did not concentrate toward a particular point in the abdomen. It co-existed, too, with highly marked tenderness over the spine, and with paroxysmal pains in the back. The aspect of the patient was not sunken, the muscular strength was not so greatly depressed, and copious perspirations did not occur—these symptoms being, in some measure, characteristic of obstruction dependent on structural lesions. Delirium was not present, as it usually is in the progress of ileus dependent on such lesions. Entertaining, however, notwithstanding these considerations, the supposition that some lesion existed, the propriety of administering cathartics was carefully considered. The natural tendency to prescribe remedies of this class was restrained, under the convictions that if intus-susceptio, or any analogous lesion existed, these remedies would prove ineffectual, that they would interfere with the natural processes upon which the possibility of recovery depended, and that they would tend to exasperate the symptoms, superinducing inflammation, if it did not already exist. The correctness of this view was confirmed by the effects of the single trial of administering croton oil.

It is to be noted in the history of the case, that the symptoms evinced decided improvement prior to the occurrence of dejections from the bowels. On the morning of the day on which the latter took place, the pain and vomiting had diminished, the pulse was less frequent, etc. The occurrence of the dejections, in fact, was doubtless due, in no small degree, to the cessation of the inverted peristaltic motion. The latter stood to the former in the relation of causation, rather than of effect. And, regarding the pathology as involving functional disorder of the muscular tunic of the intestine, not obstruction from a mechanical cause, it is obvious that the measures best calculated to expedite relief, were those which would tend to allay the nervo-muscular irritability of the canal. The injection of a

stimulating fluid into the large intestine, however, was doubtless highly serviceable, proving the immediate occasion of the return of the natural peristaltic movements, the inverted action having already ceased. There would perhaps have been an advantage in the treatment of the case, in resorting to this most valuable measure earlier.

The history of this case suggests some inquiries of interest in a physiological, as well as pathological view. The peristaltic movements of the alimentary canal, exclusive of the pharyngeal and anal outlets, are supposed, by physiologists, to be dependent on that portion of the nervous system distinguished as the *sympathetic* or *splanchnic*. They are thought to be independent of the *true spinal* or *reflex* system, the latter presiding over the automatic acts involved in the ingestion and expulsion of the contents of the alimentary canal. Now, do not the circumstances involved in the history of this case render the correctness of this physiological doctrine somewhat questionable? This spasmodic inversion of the peristaltic movements occurred in connection with a well marked affection of the spinal cord—with spinal irritation, as it is usually called. The remote cause, occasioning the attack, seemed to be one acting directly upon the spinal cord, viz., the continued stooping posture, and exertion of the upper extremities involved in quilting—a species of domestic occupation which *tires the backs* even of the healthy and vigorous. The disordered condition of the alimentary canal, moreover, was associated with symptoms due directly to the morbid condition of the cord. Other pathological phenomena, frequently observed in connection with spinal irritation, go to show that the peristaltic movements are not exclusively under the control of the *sympathetic* or *splanchnic* nervous system. Every practitioner who is prepared to appreciate the pathological relations of the cord, has met with cases of obstinate vomiting springing from this source. Tympanites, or meteorism, is also a very common symptom in persons laboring under a morbid condition of the cord, showing defective tone of the contractility of the muscular tunic of the intestines. These facts appear to lead to the conclusion that if the intestinal movements are not dependent on a reflex influence derived from the true spinal system of Marshall Hall, they are, at least, liable to be affected indirectly by morbid influences derived from that source.

On consulting the "Principles of Physiology" by Dr. Carpenter, Am. Edition for 1850, since the foregoing article was in type, I find that agreeably to the views entertained by this distinguished Physiologist, the peristaltic movements of the intestinal tube may be influenced, and excited, through

the reflex spinal system, although, in his opinion, these movements, normally, are wholly due to the irritability inherent in the muscular tunic of the tube. Dr. Carpenter states that the muscular contractions of the intestine have been observed "to continue long after the tube has been separated from its nervous connections through its whole extent;" and this is supposed to prove that these contractions are independent of any reflex action, either through the spinal cord, or the ganglia of the sympathetic. Experiments on inferior animals, however, as Dr. Carpenter states, show that not only are contractions of the intestinal tube occasioned by irritation of the splanchnic ganglia, but that they follow irritation of the cord, varying their place according to the part of the cord irritated. The author adds, "from these facts it is evident, that the movements of the intestinal tube may be influenced by the spinal cord; and that what is commonly termed the sympathetic nerve, is the channel of that influence, by the fibres which it derives from the spinal system. But it by no means thence follows, that the ordinary peristaltic action of the muscles in question are *dependent* on a stimulus reflected through the spinal cord, rather than on one directly applied to themselves." * * * "The intestinal tube, then, from the stomach to the rectum, is not dependent on the spinal cord for its contractility, but is enabled to propel its contents by its own inherent powers; still we find that here, as in other instances, the nervous centres exert a general control over even the organic functions—doubtless for the purpose of harmonizing them with each other, and with the conditions of the organs of animal life," page 297.

It is obvious that the occurrence of abnormal muscular action of the intestine as the consequence of a morbid condition of the spinal cord, is explicable by application of the physiological views quoted from Dr. Carpenter's work. The reflection, however, arises, that if the peristaltic movements may be thus affected by an influence originating in the spinal cord, it is reasonable to suppose that some influence is derived from the same source in a state of health. It may not, as Dr. Carpenter remarks, follow from the fact of the irritability of the muscular tunic of the intestine being affected by irritating the cord, that the ordinary movements of the former are dependent on the latter; still, it would certainly be a fair presumption that parts thus closely connected in disease, could not be wholly independent of each other in health.

If it be true that the tonic contractility, or tonicity, of the intestinal muscular fibre may be affected by the condition of the cord, as appears to be the case when tympanites occurs as one of the phenomena associated in



spinal irritation, this fact would seem to show a more intimate connection between the intestine and the spinal system than does the occurrence of spasmodic contractions as a consequence of causes originating in the latter. The tonicity of the muscles is supposed by Dr. Carpenter, and others, to be a quality inherent in the muscular fibre, not derived from the spinal or other ganglionic centres. The fact, however, just referred to, (if it be a fact,) gives rise to the presumption that tonicity involves an innervation communicated to the muscular structures through its nervous communications. Other pathological facts might be cited in illustration of this opinion. The expression of the face due to the tonicity of the facial muscles, is abolished in paralysis affecting, or involving the portio dura. Here, not only is the power of voluntary motion lost, but the tonic contractility is more or less impaired. Would this be the case if the latter quality were inherent in the muscular fibre, irrespective of its nervous communications? It is granted that this quality may be manifested in muscles after death, and when separated from all their nervous connections. Experiments abundantly establish this to be the case, and, also, that the contractility of muscles may be acted upon, under the same circumstances, so as to produce active movements; may not these facts (which are apparently inconsistent with a relation of dependency of the muscles upon a force of innervation) be explained on the hypothesis that this force, generated within the ganglionic centres of the spinal cord and the sympathetic, and communicated to the muscles, creates, in the latter, properties which continue, for a time, after the supply of the force ceases?

Practitioners who have been accustomed to study cases of spinal irritation, must have observed that a sense of muscular debility, and lassitude, varying in degree at different times, are among the most constant of the host of symptoms appertaining to this affection. Do not these symptoms, referable, as they are, to the condition of the muscular system, denote disordered innervation incident to a morbid condition of the spinal system?

We have thrown out these ideas by way of suggesting inquiries relating to topics interesting both in a physiological and pathological point of view. Much as the functions of the nervous system have been elucidated by modern discoveries and researches, there is still open a wide field for farther developments, which may be expected to advance still more our knowledge of diseases, and their successful management.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS

On the Utility of Warm and Cold Bathing in the Treatment of Insanity.

By J. H. WORTHINGTON, M. D., Superintendent of the Friends' Asylum for the Insane, near Frankford, Pa. Read before the Association of Medical Superintendents of American Institutions for the Insane, convened in Boston, 6 mo. (June,) 1850.

It will, perhaps, be generally admitted, that the due performance of the function of cutaneous transpiration is an essential requisite to the healthy condition of the entire system. Labor, which has been the lot of the greater portion of the race since the first punishment was inflicted upon man by his Maker, seems to be the grand means appointed by nature to insure the healthy action of the skin. "In the sweat of thy face thou shalt eat bread till thou return to the ground," was the sentence pronounced against man for his first transgression, and it seems to have been imposed on him as a punishment from which there could be no escape, except at the risk of suffering and disease. To obviate the evil consequences which are likely to result from the neglect of this means of securing the healthy action of the skin, the resources of art are necessary; and among these there appears none capable of answering the purpose so beneficially as the use of the bath, under one or other of its different forms. With the advancement of luxury and refinement, the experience of all ages has proved the necessity of this means of preserving health; and the ruins of the public baths of the Romans, and other nations of antiquity, are evidences, yet remaining, of the estimation in which a healthy skin was held in those early periods.

The insane, either from physical inability, or from the want of that degree of reason which is necessary in the performance of most kinds of manual labor, as a class, must be deprived of all the benefits which accrue from this source; and, perhaps, from this cause, in great part, are subject to those chronic diseases which prove fatal to so large a proportion of the incurable insane.

The altered condition of the cutaneous surface is noticed by most writers on the subject of insanity. Almost all speak of coldness of the extremities and want of free circulation in the capillary system, in some forms of the disease. Diminution of cutaneous transpiration is manifested in many of the insane by a dry and harsh state and altered appearance of the surface, and a change in its quality is evident in the odor which is often given off by the bodies of the insane. Insanity, when it has become chronic, is apt to be accompanied by cutaneous disorders of different kinds, which are often very difficult to eradicate. Esquirol lays particular stress on the connection between insanity and eruptive diseases, and thinks it one which deserves attention. He speaks of the skin, in some cases, being in a state of *erethism* which is very remarkable, and of a renewed flow of the per-

piration being followed by a resolution of the disease. In the affection called "pe'agra," a diseased condition of cutaneous surface, sometimes accompanied with disorder of the digestive functions, ends in the gradual impairment of the intellect and senses, with a strong tendency to suicide.

The effects of the sudden suppression of perspiration by the application of cold, have long been observed under the form of fevers and dangerous inflammatory affections of different internal organs. Experiments upon the inferior animals, by coating their bodies with an impervious layer of varnish, demonstrate the injurious and violent effects produced by the repulsion into the circulation of the effete matters which it is the function of the skin to eliminate. A French physician, Fourcault, mentions the instance of a child, who was covered with gold leaf at the coronation of one of the Roman pontiffs, to represent the golden age which was to be revived by the reign of the new sovereign, who fell a victim to this act of presumption. The same author coated the skins of several Guinea pigs with gold and silver leaf, or tin-foil, so as completely to prevent the escape of the perspiration, and they all died in a short time with every appearance of asphyxia. When a smaller extent of surface was covered, the effects were less violent, and they consisted generally of a modification of some internal secretion. In some of the experiments a profuse discharge was produced from the nostrils, in others, diarrhœa ensued, and the mucous membrane, in either case, on examination, were found presenting the appearance of intense inflammation.

When these striking effects are seen to be produced by the sudden, and more or less complete suppression of the perspiration, it might be expected that in a less degree, but continued through longer periods of time, its detrimental effects upon health, though less cognizable, would be none the less real. We must look to classes of society and communities, whose habits and modes of living are such as to favor the habitual diminution of cutaneous transpiration, for evidence of the prejudicial influence exerted thereby upon health, as well as for the nature of the morbid changes it produces. The occupants of dark, damp, and ill-ventilated dwellings, operatives in manufactories, inmates of large alms-houses, convicts in prisons and penitentiaries, &c., have been especially noted for their unhealthiness, and the prevalence among them of scrofula, phthisis, &c., has generally been attributed to the influence of vitiated air upon the function of hæmatisation, and of insufficient food, combined with want of exercise upon that of nutrition. The observations of M. Fourcault, however, without denying to these causes a degree of influence in the production of the diseases named, seem to point more especially to derangement of the excretory functions of the skin as their origin or primary cause. This author concludes upon examination of all the circumstances attending the mode of life of the different classes of people enumerated above, that their unhealthiness is not so much owing to their being ill-fed and breathing a vitiated atmosphere, as to their habits of bodily inactivity, or to confinement to occupations which afford only a small amount of exercise. These views are sustained by the reports of the British Registrar General, and by M. Villirme, of France, who concur in the statement that by far the greater number of cases of phthisis, scrofula, &c., occurring among persons engaged in factory labor are presented by workmen whose occupation requires them to remain steadily in one position, and in whom, consequently, elimination

by the skin cannot be assisted by exercise. The silk weavers of Lyons, and other towns in the south of France, are thus situated, and an immense proportion of the mortality among them is caused by consumption. In other towns where the manufacturing of cloth is carried on, which requires considerable activity and strength, consumption is much less frequent.

It is scarcely necessary to refer to the work* of M. Fourcault for further evidence of the greater liability to consumption and scrofula among persons pursuing sedentary occupations, and among the wealthy, who lead indolent and luxurious lives. Indeed, the prevalence of these diseases among the latter class shows this liability does exist, even when the other circumstances, which are thought to be most favorable to their production, are entirely wanting. The effect of a life of labor and hardship in preventing the development of tubercular disease, in persons predisposed to it, is well known; and perhaps we cannot explain this exemption more satisfactorily, than by referring it to the beneficial influence upon all the other functions of the natural secretion of perspiration which is thus insured. The inference, that diminution in the quantity of fluid perspired is a principal cause of the development of consumption, is materially strengthened by the fact of the great prevalence of the disease in countries having a moist and cool atmosphere, as England, where the mortality therefrom is as high as one-third of the whole number of deaths. It has also been remarked, that individuals predisposed to phthisis may reside for a long time in a warm climate and continue to enjoy good health, but on removing to a colder one are exceedingly liable to have the disease developed. A large proportion of animals exhibited in menageries, natives of warm regions, die of tubercular diseases, and in these cases it would, perhaps, be difficult to account for the result in any other way, than by attributing it to the diminution or suppression of the insensible perspiration, caused by the colder atmosphere.

The mode in which derangement of the function of the skin acts in producing disease, may be understood on the physiological law of vicarious secretion. The mucous membranes and the skin present many points of resemblance, both of structure and function, and the former may, therefore, with good reason, be supposed to supply the place of the latter, when, from any cause, it is rendered unfit for performing its functions. On this hypothesis we would consider the increased secretion from the mucous membrane of the nostrils in ordinary catarrh merely to have taken the place of the natural cutaneous excretion, and in bronchitis the irritation of the mucous membrane would be produced by the effort to throw off from the system, through this channel, the retained matter of the perspiration. Diarrhœa, and the various forms and grades of enteritis, are caused by sudden cooling of the surface of the body, and their production might be accounted for in the same manner.

However we may attempt to explain the occurrence of inflammatory affections of the lungs, bowels, and kidneys, consequent upon exposure to cold, the belief that they are intimately connected with suppressed perspiration is warranted by the trials of M. Fourcault on animals, as well as sanctioned by common experience. It is not, however, with acute diseases

* *Causes generales des maladies chroniques, specialement de la phthisie pulmonare. Vide Am. Journal Med. Science, Oct., 1845.*

of these organs that we have to contend in the treatment of the insane. Their cutaneous functions are seldom interrupted by sudden vicissitudes of temperature. They become deranged in a more gradual manner, and hence those organs which sompathize more directly with the skin,—which are most liable to disease on the sudden suppression of its functions,—suffer more slowly, and become afflicted with various chronic disorders. It does not appear to be an unwarrantable conclusion, that in this way originate a great part of the consumption, the chronic diarrhœa and dysentery, the marasmus and dropsies, which cause such a large proportion of the deaths in Lunatic Asylums.

If the foregoing views respecting the importance of the functions of the skin, and their liability to derangement in the various forms of insanity, be correct, the necessity is obvious of attending to these functions, and of preserving them in as healthy a condition as possible, by the employment of the only means to which we can conveniently resort.

The forms of bathing which are most generally applicable in the treatment of insanity, are, the warm bath averaging from 92 to 98 deg. Fahrenheit, and the cold bath, at an average temperature of 52 deg. Fahrenheit. Independent of their direct influence upon the surface of the skin, tending to remove obstructions to the free performance of its functions, and thus promoting a healthy action of the internal organs, the beneficial effects of warm and cold bathing, in many cases of mental disorder, are such as can scarcely be obtained by any other means. The days in which it was considered necessary to treat every case of disordered cerebral circulation by a resort to depletory measures have happily passed by, and the end which was sought to be attained thereby, may be answered, in many cases of insanity, by other means, including the use of the warm bath. The diseased action of the brain, even in the early periods of insanity, is rarely of that kind which is attended with high arterial excitement. It is, at least, many degrees removed from true inflammation, whose natural termination is effusion of lymph or pus, and which requires the adoption of the most vigorous measures for the prevention of fatal or permanent disorganizations. Produced in most cases by causes which tend gradually to lower the tone of the system, experience has shown the necessity of avoiding every means calculated still further to reduce the strength. It is, then, in a condition of diminished vital activity, that we have to apply our remedies, while the brain is, notwithstanding, suffering from a too great quantity of blood circulating through its vessels; other organs, perhaps, at the same time, receiving a deficient supply. Under these circumstances, the use of the warm bath is attended with peculiar advantages. It acts by inviting the circulating fluid to the surface, and filling the capillary system of vessels, by which means the vital fluid distributed to every part of the system is equalized, and the brain relieved of its surplus, without the evil consequences resulting which are to be feared from direct depletion. In cases which are attended with considerable nervous irritability and morbid wakefulness, the warm bath, taken at bed-time, will often prove serviceable by its soothing influence in allaying excitement, and disposing to sleep when narcotics would be inadmissible or fail of their usual effects. It may act here also, by remedying a slighter degree of unequal distribution of the circulating fluid, which often accompanies this condition of the nervous system. To many persons in ordinary health, travelling is apt to be pro-

ductive of a certain degree of morbid irritation of the nervous system, attended with dryness of the skin, slight feverishness, and a defective performance of some of the secretory functions, for the removal of which the warm bath is one of the most effectual remedies. In most insane persons, these effects may be expected to be produced in a more marked degree by their journey to the Institution where they are to undergo a course of treatment, and the bath, were there no other reasons for its employment, will therefore frequently be found serviceable, immediately after the admission of the patient. The effect of the warm bath is to soothe and refresh the whole system, and at the same time to render all its functions free and of more ready performance. It is, therefore, probable that it would be found highly beneficial in those cases of high maniacal excitement, which, from their being accompanied with almost entire want of sleep and obstinate refusal of nourishment, are apt to terminate in fatal exhaustion. In such cases, the functions of the skin, as well as those of the gastro-enteric mucous membrane, are greatly perverted, the surface of the body and the extremities being in most cases cool, and either moist or excessively dry and harsh, and presenting to the eye an earthy or dusky appearance.

In no form of insanity has the diseased condition of the cutaneous surface been more frequently noticed than in melancholia. Esquirol speaks of the skin as being brown, blackish, dry and scaly, while in no form of insanity is there so strong a tendency to those chronic affections which, as has been attempted to be shown, are probably dependant on disordered cutaneous function. Of 196 deaths of patients affected with melancholia, who came under the care of this distinguished author and physician, 62 were of phthisis, 32 were of chronic affections of the bowels, and 24 were of marasmus. If there be any such connection as has been suggested, between the supposed cutaneous function on one hand, and the diseased thoracic and abdominal viscera on the other, it is evident that any means capable of remedying the former would be the most effectual in preventing the latter. In this form of insanity, therefore, it is probable that the frequent use of the warm bath would be found highly beneficial in its tendency to prevent the development of those diseases which prove fatal to so large a proportion of melancholiacs.

In a large number of insane persons there is a striking defect of the function of capillary circulation, in connection with a torpid condition of the entire system. In these cases the cutaneous surface, especially on the extremities, is constantly cool, sometimes moist and clammy. The skin is discolored, and if the blood circulating in it be temporarily removed by pressure, it is a long time in resuming its original color. There is a defective performance of the digestive functions, the bowels are sluggish in their action, and the brain seems to suffer from a deficient supply of arterial blood. This condition of the physical functions is frequently accompanied by great mental apathy, from which it is almost impossible to arouse the patients, and many of them are addicted to depraved personal habits, which tend to their deterioration of body and mind. In these cases the cold bath acts most beneficially, by calling into exercise the conservative powers of the system, and by the reaction thus produced tending directly not only to quicken the circulation in the capillary vessels, but to promote the vital activity of the whole system. In many cases of hypochondriasis and excessive nervous irritability, after the disordered condition of the

abdominal viscera has been in some measure corrected, the cold bath, by its general tonic effects and by restoring a healthy condition of the cutaneous functions, will often be found advantageous.

The cold douche, as a form of local bathing, has been too much employed in the treatment of the insane to be passed by without notice. In many cases of high cerebral excitement, the application of a stream of cold water falling from a height upon the head, has been found to be a valuable auxiliary to other necessary treatment; and when there is an unequal distribution of the circulating fluid, it will, especially in connection with the warm bath or pediluvium, often prove of service in insanity. Where there is considerable vigor of the general circulation, particularly in young persons, the frequent use of the douche is productive of a feeling of freshness and comfort which is very grateful, and which may be indicative of improvement in the state of the cerebral circulation. It is beyond the province of this report to enter further upon the subject of the so called moral treatment of insanity by means of the douche, further than to remark that its general use, as well as that of the shower bath, as a means of repression or punishment would probably be destructive, by the feelings of fear or dislike which it would inspire, of all the benefits likely to be derived from their use as therapeutic agents.

The foregoing article is from the American Journal of Insanity. Some remarks on the construction of baths are omitted.—EDITOR BUFFALO MEDICAL JOURNAL.

On the Secreting Function of the Colon. By JAS. PAUL, M. D. Read before the District Medical Society for the County of Mercer.

Although great and deserved attention is paid to the secretions in disease, both urinary and fæcal, and in a great many of the diseases to which the human frame is liable, particularly in fevers, there is no surer criterion to lead us in our prognostications, or guide us in our remedial efforts than the appearance of the excretions. I do not know if we are so thoroughly acquainted with the philosophy of the fæcal discharges as we ought to be, or that we view them altogether in the physiological bearing to which they properly belong in the animal economy.

We are, I think, too much in the habit of viewing the excrements merely as an index of the food having undergone the proper and necessary process of digestion, and when we see pieces of undigested aliment mixed up in the fæces, we naturally conclude and say that the substances consumed, whether of potato, apple, carrot, or whatever else has been partaken of, has not been digested.

Even this, however, is not without its use—for although in such cases the pressing symptoms, whether of croupy cough, nervous twitches, or convulsive spasms, are relieved by evacuating the alimentary canal of foreign and irritating substances, it yet enables us to note what portion of the digestive function is incomplete, whether the deficiency lies in the non-rendering the vegetable food into the saccharine principle, or otherwise, and so to alter the food to that which can be digested, and direct our remedial efforts to that portion of the function which is deficient.

My purpose at this time, however, is not with the function of digestion, but to direct our attention to the fæcal secretions or excretions, and to the colon, or large intestine, as a great secreting organ,

Every practitioner is more or less acquainted with the appearance of the secretions as they are passed from the body of a patient laboring under fever—the brownish watery discharges having a cadaverous or fleshy smell, the black or dark green discharges resembling blubber, or the green fat of turtle, having a highly offensive and putrid odor—and the gradual return to the yellowish watery discharges having as convalescence is established, more consistence, and the more genial odor of proper fæces. I do not intend to enter into, neither is it needful that I should, the various appearances of the fæcal discharges in disease, nor the altered appearances caused by various remedial agents.

It has been a question among physiologists of the older school, whether absorption takes place in the larger intestines? On this subject, Blumenbach has the following—"It has been inquired whether lacteals exist also in the large intestines, and their existence has been contended for from the effects of particular injections, nutrients, inebriating, &c., and also by the circumstance that the fæces if retained for any length of time become hard and dry. Although these arguments do not demonstrate the absorption of genuine chyle below the valve of Fallopius, nevertheless, it is rendered probable by the visible existence of an abundance of lymphatics in the large intestines having the same structure and function with the lacteals, for these absorb lymph from the intestines during the absence of chyle.

"But the very different structure of the internal coat of the large intestines from that of the villous coat of the small, strongly argues that they are not naturally intended to absorb chyle."—*Blumenbach*, 233.

Our present views of the transudation of liquids through animal texture, will readily enable us to comprehend how absorption may take place, and nourishment be conveyed into the system when thrown into the large intestines, and even only into the rectum by means of injections. Nor is it at all incompatible with physiological facts that absorption and secretion should go on in the same organ, and through the same texture by different sets of vessels.

The same unsatisfactory knowledge, if I may be allowed the expression, exists regarding the functions and uses of the *mesenteric glands* of the colon. Prof. Grant, treating of these organs, says: "There are nearly a hundred of these organs on the human lacteals, and about a fourth part of these belong to the colon; but the changes they effect on the fluids which are incessantly passing through them during life, and even for some time after death, or the uses to which they are subservient in the economy, are still unknown, like the functions of many other obvious parts of our most complicated and wonderful fabric."—*Prof. Grant's Lectures*, Jan. 26, 1824.

Following up the argument of the absorption of chyle, and its having been seen in the mesenteric veins, Blumenbach says: "The assertion that chyle has been seen in the mesenteric veins requires further investigation and proof; so that I cannot believe that they carry anything more than blood, being *carbonized* and destined for the formation of bile."—*Blumenbach*, 234.

* That these derangements occur in fever, is often owing to injudicious cathartization.—EDITOR BUFFALO MEDICAL JOURNAL.

Here, then, we find the blood loaded and surcharged with that principle, of which a great portion of the fæces is composed.

Having thus briefly alluded to the views generally and formerly entertained by physiologists, let us enter more minutely into the structure of that portion of the large intestines in which this most important function is situate. "A part of the fæces, however—says Carpenter,—may be derived from the secretions of the enteritic mucous membrane, and of its glandulæ; the surface of the former, with its simple follicles, probably secretes nothing but mucus; but the glandulæ with which it is so thickly studded appear to serve as the channel for the elimination of putrescent matter from the blood. There can be no doubt that a large quantity of fluid is poured out by these glandulæ when they are in a state of irritation from disease, or from the stimulus of a purgative medicine; since the amount of water discharged from the bowels is often much greater than that which has been ingested, and must be derived from the blood."—*Carpenter*, 501.

For a description of these glandulæ, allow me to transcribe from the same author the following: "The whole mucous surface of the intestinal canal is furnished with glandular follicles of a very similar character; of which some approach those of the stomach in complexity of structure, whilst others evidently correspond with the crypts of ordinary mucous membrane. An innumerable multitude of pores are easily seen by the aid of a simple lens to cover the whole internal surface of the large intestines, and these are the entrances to tubular follicles closely resembling those of the stomach, but more simple in structure. Their cœcal extremities shut against the submucous tissue; towards the end of the rectum, however, they are much prolonged, and constitute a peculiar layer between the mucous and muscular coats; the tubes which are there visible to the naked eye being erect, parallel, and densely crowded. These glands probably form the peculiarly thick and tenacious mucus of the large intestine."—*Carpenter*, 668.

And of the functions of this glandular structure, the same author observes, "Although the particular use of each variety of the intestinal glandulæ cannot yet be determined, there seems little doubt that their general function is to eliminate from the blood those putrescent matters which would otherwise accumulate in it; whether as one of the results of the normal waste of the system, or as produced by various morbid causes which act as *ferments*, and thus occasion an unusual tendency to decomposition in the solids and fluids of the body. That the putrescent elements of the fæces are not derived from the food taken in, so much as from the excreting action of the intestinal glandulæ, appears from this consideration among others; that fæcal matter is still discharged, even in considerable quantities, long after the intestinal tube has been completely emptied of its alimentary contents. We see this in the course of many diseases where food is not taken for many days, during which time the bowels have been completely emptied of their previous contents by repeated evacuations, and whatever then passes in addition to the biliary and pancreatic fluids must be derived from the intestinal walls themselves. Sometimes a copious flux of putrescent matter continues to take place spontaneously, whilst it is often produced by the agency of purgative medicine. The 'Colliquative Diarrhœa' which frequently comes on at the close of exhausting diseases, and which usually precedes death by starvation, appears not to depend so much

upon a disordered state of the intestinal glandulæ themselves, as upon the general disintegration of the solids of the body, which calls them into extraordinary activity for the purpose of separating the decomposing matter."—*Carpenter*, 670.

What I have just read is so comprehensive, and brings the subject so forcibly and powerfully to the mind as to preclude the necessity almost of entering more fully upon it.

My attention was particularly drawn to this subject by the frequent occurrence of immense quantities of the morbid and putrid discharges by stool, in tropical fevers, immediately before returning convalescence. At the commencement of the disease, the alimentary canal would be carefully emptied by repeated doses of purgative medicine, the fever would continue, watery stools would supervene; at this period the patient would take the simplest nourishment, and that in small quantities, and in many cases none at all, the stomach rejecting every particle of food exhibited—in the progress of the disease, the patient prostrated and nearly fainting on the least exertion, large dejections would occur of dark-colored gelatinous offensive matter—quarts, and I may say gallons on some occasions, are passed off at repeated operations—and although the patient at this time would be scarcely able to move or speak, yet after such evacuations he would feel more easy—a moisture appear on the surface—the critical moment being seized, and nourishment with wine or brandy exhibited—the patient slumbers, and from that time convalescence progresses.

And what is the result if this dark offensive matter is not thrown off? It is more than probable that the fever will continue, and in more favored climates a slow and dilatory convalescence may ensue, or the whole system becomes corrupted, and *in a tropical climate* putrefaction succeeds almost ere the being has ceased to breathe.

In the epidemic which has so lately made such havoc, and ruin in its course in some cities of the Union, causing such fearful mortality, the non-performance of the proper functions of the secreting glands of the intestines is no doubt a principal effect. Without entering into the manner in which the morbid poison of the cholera acts on the system, we see an abeyance of the proper secretions—of bile, urine, and fæcal discharges, and in their stead a watery secretion is ejected, even with force, from the stomach and intestines, without straining, and without pain; indeed, so offensive is the presence of this secreted fluid to the stomach and intestinal canal, that the patient can scarcely control its ejection for a few seconds. And this unusual parting with the serous portion of the blood leaves the remaining portion thick, viscid, and incapable of entering the minute or capillary vessels, and collapse is the consequence—but arrest the serous discharges, and once produce a fæcal evacuation with tinges of biliary secretion, and there is every chance of the recovery of the patient. Hence, it is obvious that the secreting organ of the large intestines is seriously affected in this formidable disease. I call it formidable from the fatality attending the visitation, but in my opinion controllable in a great majority of cases where the patient has been timely put under the care of the physician, and remedial and energetic measures have been pursued.

Every practitioner will no doubt bring to his recollection cases in which the patient, even after repeated and free evacuations, will answer to the inquiry regarding his feelings, "I am better—my medicine has acted very well—still I feel as if there was yet something to come away." Is it not

probable that this feeling, indescribable to the patient, not amounting to pain, and relieved by a copious evacuation, has been owing to the surcharged state of the mesenteric veins, and the relief the consequence of the active secretion from the *glandulae* which has been the subject of consideration.

The secretion of the liver is looked for, and the returning appearance of bile in the faecal discharges is held in great estimation, and properly so, by most, if not by all practical physicians; its proper action is absolutely necessary to recovery from disease and the enjoyment of health. It is not my object to withdraw attention from that most important organ, but to direct more particular attention to the colon as a great secreting organ; that the faeces, which in health may contain that portion of the food which has not been absorbed into the system, more especially when a superabundant quantity of aliment has been consumed, is for the most part secreted by the large intestines, from which the deleterious and disintegrated portions of the organic mass is passed away, and the system freed of much of the superabundant carbon which may not be required for the purposes of respiration. *New Jersey Medical Reporter.*

The study of the functions of the large intestine is to be commended to those practitioners, who, in the language of Dr. Stokes, (which is more significant than chaste,) derive facts for diagnosis, and the indications for treatment, chiefly from the inspection of chamber pots. Some Physicians appear to see in the varied appearances of the dejections, little else than different conditions of the secretions of the Liver. Indeed, the *Hepatic pathology* of modern days, now happily on the wane, has been mainly instrumental in giving to the alvine evacuations undue relative importance as criteria for discriminating and treating diseases. The secretions from the liver in fact constitute only an element, and, probably, a minor element in producing both the healthy and morbid appearances of the dejections from the bowels. It may be doubted if the *bilious stools* which often appear to afford so much satisfaction, have more than a remote dependence on the presence of bile. The changes in the color, consistence, odor, etc., of the feces depend mainly on the function of fecation, performed by the large intestine, with which function the bile is concerned only as one of the collateral circumstances involved. As furnishing symptoms by which to estimate the condition of the organism, and the phases of disease, of how much greater importance is the secretion of urine, than the intestinal excrement! And, yet, how seldom is the former studied and observed with care, while the latter not infrequently seems to absorb both the senses and the meditations of the practitioner! In the investigation of many diseases the alvine discharges possess but little real relevancy. In cases of diseases of the chest, or heart, for examples, how absurd for the dejections to be carefully reserved and assiduously inspected, while physical exploration is depreciated or neg-

lected! But it has fallen to our lot, as it must also to that of our readers, to witness illustrations of this absurdity. The popular mind has unfortunately become imbued with the exaggerated importance of seeing and smelling every thing that passes the bowels in all cases of sickness, without reference to the seat and character of the malady. The comfort of the medical practitioner will be in no small degree promoted by the diffusion of correct notions respecting the function of fecation, and a consequent abatement of that popular error which regards attention to this portion of the animal economy as a *sine qua non* under all circumstances. Far be it from us to advocate any neglect of the *pot de chambre*, but as an optical instrument for developing useful information, we fancy it resembles the *kaleidoscope*, oftener than the *camera obscura*.

EDITOR BUFFALO MEDICAL JOURNAL

On the Identity or Non-identity of Typhoid Fever, Typhus Fever, and Relapsing Fever. By W. JENNER, M. D., Professor of Pathological Anatomy, University College, London.

The author, at the commencement of his paper, remarks that, for many years, small pox, measles, and scarlet fever were confounded under one name, and that it was only after the publication of Dr. Withering's essay, that measles and scarlet fever were regarded as distinct affections—i. e., distinct as to their course, their symptoms, their lesions, and their causes. Typhus fever, typhoid fever, and relapsing fever are yet by many looked on as but varieties of one disease. But the writings of Dr. Gerhard, M. Valleix, and Dr. A. P. Stewart have rendered it highly probable that typhoid fever and typhus fever are absolutely distinct from each other—two species of disease, and not varieties of one affection. In the *Monthly Journal of Medicine* of the present year, the author has analyzed the course, symptoms, and lesions of structure found after death in a certain number of cases of fever, and this analysis he thinks, proves that, as regards their course, symptoms, and lesions, no two diseases can be more distinct than typhus or typhoid fever. But small pox, measles, and scarlet fever differ also in respect of their exciting cause, which, in the case of each of these diseases, is specific. In like manner, typhoid fever, typhus fever, and relapsing fever must require for their production the application of distinct specific causes, if they be distinct diseases. To inquire whether the specific cause of each of these diseases is distinct, or whether the cause of these is the same, is the author's object in the present paper. He first describes the peculiarities of the course and symptoms of relapsing fever, and of the skin eruption of typhoid fever and of that of typhus fever, on which the diagnosis of these diseases rests. He then gives three tables, showing all the instances in which two or more cases of fever were admitted from one house into the London Fever Hospital, in the years 1847, 1848, and 1849; the age, sex, and degree of intimacy of the individuals, as well as the nature of the disease under which they labored; and for the years 1848 and 1849,

of these organs that we have to contend in the treatment of the insane. Their cutaneous functions are seldom interrupted by sudden vicissitudes of temperature. They become deranged in a more gradual manner, and hence those organs which sympathize more directly with the skin,—which are most liable to disease on the sudden suppression of its functions,—suffer more slowly, and become afflicted with various chronic disorders. It does not appear to be an unwarrantable conclusion, that in this way originate a great part of the consumption, the chronic diarrhoea and dysentery, the marasmus and dropsies, which cause such a large proportion of the deaths in lunatic Asylums.

If the foregoing views respecting the importance of the functions of the skin, and their liability to derangement in the various forms of insanity, be correct, the necessity is obvious of attending to these functions, and of preserving them in as healthy a condition as possible, by the employment of the only means to which we can conveniently resort.

The forms of bathing which are most generally applicable in the treatment of insanity, are, the warm bath averaging from 92 to 98 deg. Fahrenheit, and the cold bath, at an average temperature of 52 deg. Fahrenheit. Independent of their direct influence upon the surface of the skin, tending to remove obstructions to the free performance of its functions, and thus promoting a healthy action of the internal organs, the beneficial effects of warm and cold bathing, in many cases of mental disorder, are such as can scarcely be obtained by any other means. The days in which it was considered necessary to treat every case of disordered cerebral circulation by a resort to depletory measures have happily passed by, and the end which was sought to be attained thereby, may be answered, in many cases of insanity, by other means, including the use of the warm bath. The diseased action of the brain, even in the early periods of insanity, is rarely of that kind which is attended with high arterial excitement. It is, at least, many degrees removed from true inflammation, whose natural termination is effusion of lymph or pus, and which requires the adoption of the most vigorous measures for the prevention of fatal or permanent disorganizations. Produced in most cases by causes which tend gradually to lower the tone of the system, experience has shown the necessity of avoiding every means calculated still further to reduce the strength. It is, then, in a condition of diminished vital activity, that we have to apply our remedies, while the brain is, notwithstanding, suffering from a too great quantity of blood circulating through its vessels; other organs, perhaps, at the same time, receiving a deficient supply. Under these circumstances, the use of the warm bath is attended with peculiar advantages. It acts by inviting the circulating fluid to the surface, and filling the capillary system of vessels, by which means the vital fluid distributed to every part of the system is equalized, and the brain relieved of its surplus, without the evil consequences resulting which are to be feared from direct depletion. In cases which are attended with considerable nervous irritability and morbid wakefulness, the warm bath, taken at bed-time, will often prove serviceable by its soothing influence in allaying excitement, and disposing to sleep when narcotics would be inadmissible or fail of their usual effects. It may act here also, by remedying a slighter degree of unequal distribution of the circulating fluid, which often accompanies this condition of the nervous system. To many persons in ordinary health, travelling is apt to be pro-

ductive of a certain degree of morbid irritation of the nervous system, attended with dryness of the skin, slight feverishness, and a defective performance of some of the secretory functions, for the removal of which the warm bath is one of the most effectual remedies. In most insane persons, these effects may be expected to be produced in a more marked degree by their journey to the Institution where they are to undergo a course of treatment, and the bath, were there no other reasons for its employment, will therefore frequently be found serviceable, immediately after the admission of the patient. The effect of the warm bath is to soothe and refresh the whole system, and at the same time to render all its functions free and of more ready performance. It is, therefore, probable that it would be found highly beneficial in those cases of high maniacal excitement, which, from their being accompanied with almost entire want of sleep and obstinate refusal of nourishment, are apt to terminate in fatal exhaustion. In such cases, the functions of the skin, as well as those of the gastro-enteric mucous membrane, are greatly perverted, the surface of the body and the extremities being in most cases cool, and either moist or excessively dry and harsh, and presenting to the eye an earthy or dusky appearance.

In no form of insanity has the diseased condition of the cutaneous surface been more frequently noticed than in melancholia. Esquirol speaks of the skin as being brown, blackish, dry and scaly, while in no form of insanity is there so strong a tendency to those chronic affections which, as has been attempted to be shown, are probably dependant on disordered cutaneous function. Of 196 deaths of patients affected with melancholia, who came under the care of this distinguished author and physician, 62 were of phthisis, 32 were of chronic affections of the bowels, and 24 were of marasmus. If there be any such connection as has been suggested, between the supposed cutaneous function on one hand, and the diseased thoracic and abdominal viscera on the other, it is evident that any means capable of remedying the former would be the most effectual in preventing the latter. In this form of insanity, therefore, it is probable that the frequent use of the warm bath would be found highly beneficial in its tendency to prevent the development of those diseases which prove fatal to so large a proportion of melancholiacs.

In a large number of insane persons there is a striking defect of the function of capillary circulation, in connection with a torpid condition of the entire system. In these cases the cutaneous surface, especially on the extremities, is constantly cool, sometimes moist and clammy. The skin is discolored, and if the blood circulating in it be temporarily removed by pressure, it is a long time in resuming its original color. There is a defective performance of the digestive functions, the bowels are sluggish in their action, and the brain seems to suffer from a deficient supply of arterialized blood. This condition of the physical functions is frequently accompanied by great mental apathy, from which it is almost impossible to arouse the patients, and many of them are addicted to depraved personal habits, which tend to their deterioration of body and mind. In these cases the cold bath acts most beneficially, by calling into exercise the conservative powers of the system, and by the reaction thus produced tending directly not only to quicken the circulation in the capillary vessels, but to promote the vital activity of the whole system. In many cases of hypochondriasis and excessive nervous irritability, after the disordered condition of the

clamor and opposition by awakening attention; and exciting investigation, often tend to promote the interests of science.

We transfer to our columns the portion of the article by Prof. Gilman which relates directly to the use of the Speculum.

EDITOR BUFFALO MEDICAL JOURNAL.

The speculum, we hoped, might now be allowed to take its place beside the stethoscope, a valuable means of investigating and treating disease—a means by which one of the *opprobria medicorum* was removed, and leucorrhœa made a curable disease. But it seems it is not to be so. The attack upon this instrument made in London, finds a ready echo in our own land, and we are called to fight this battle over again, and to oppose here, as in the case of chloroform, facts and experience to reasonings, theories, and declamation. The contest is not one from which the advocates of the speculum need shrink. The result is not doubtful.

The opponents of the speculum, aware that it would hardly be safe to deny outright its value, have taken other, and as they doubtless suppose, more tenable ground: they profess not to forbid, but only to limit its use; "it is of great value in proper cases, but it has been used unnecessarily and for bad motives, and we must limit its use." To this limitation some of those who know the speculum practically will accede, especially as regards malignant disease. But they rely on its use in simple inflammation and ulceration; here the speculum is as useful in the treatment as in the diagnosis of disease. How is its use to be *limited* in this class of affections? Simply by denying the alleged frequency, and some go as far as to doubt the *existence*, of simple ulceration. This is the ground taken in London by Drs. Lee and Ashwell. The proofs relied on to support this most extraordinary assertion are—1st, That in very many *post mortem* examinations of women dying of other diseases, no ulcerations were found by the various curators of museums by whom these examinations were made. The number of such examinations rises to thousands, and might no doubt be carried to tens of thousands; for, as Dr. Bennett well observes, when these examinations were made, the practical knowledge of the inflammatory lesions of the uterus did not exist in the profession. But, 2d, The opponents of the speculum refer to their own experience to prove the extreme unfrequency of simple ulceration. Dr. R. Lee has not seen a single case of simple ulceration of the cervix. Dr. Ashwell, in 1026 cases of uterine disease treated by him at Guy's, found only 25 cases of ulceration of the cervix.

This reference to facts and cases is just what the friends of progress desire: it is ground they are accustomed to tread, and over which they move with assured steps. Let it then stand recorded, that of 1026 cases of uterine disease treated at Guy's Hospital, London, only 25 are found with ulcers of the cervix. This is a very valuable fact. Let us collect other facts, and compare the experience of other men with that of Dr. Ashwell. Dr. Murphy, in the same debate, said that he had seen hundreds of uterine cases, and seven-tenths of them were inflammations and ulcerations of the cervix. Dr. Bennett, in 300 cases, found diseased cervix in 243, ulceration in 222. Here, indeed, is a marvellous discrepancy!

Dr. Murphy finds diseased cervix in seven-tenths of his cases, Dr. Ben-

nett in five-sixths, Dr. Ashwell in one-fortieth, and Dr. R. Lee never a single case! How is it possible to account for such discrepancies? Can any plausible explanation be offered other than that suggested by Dr. B.?—*They did not use the speculum.*

Is not this the obvious, the inevitable conclusion from the premises? The simple truth is—it must be—these gentlemen have not seen ulceration, because they have not looked. They do not know, because they will not learn. Now what could the advocates of the speculum desire more than this: in Guy's Hospital the number of cases of simple ulceration that are not diagnosticated, and of course not well treated, is seven-tenths less one-fortieth, or about sixty-five per cent. Here we have, in a tangible shape, the fruits of this doctrine of the abuse of the speculum. To avoid such abuse of the speculum, Dr. Ashwell will not use it, and he fails accurately to diagnosticate sixty-five per cent. of his cases. Here is an abuse indeed. But will any one charge me with disrespect to Dr. Ashwell? Let the treatment of Bennett, and other less distinguished men who think with him, be my defence, if I need one. Did not Dr. R. Lee assert that he did not believe Dr. B. had ever seen a simple ulcer of the cervix; and that after Drs. Locock and Murphy had declared that they met with them very frequently? And is not this the talk of all those who rail at the abuse of the speculum? Dr. Bennett, Dr. Locock, Dr. Murphy, the hundreds, might I not say thousands of practitioners who, in Great Britain, on the continent of Europe, and in this country, a part of whose daily business it is to see, and treat, *and cure* these ulcerations, are laboring under some strange hallucination, for to this we are driven—there is no room for mistake. I see a case of chronic leucorrhœa; the patient is incapable of exertion, her health is broken, her spirits depressed to the lowest point, for she has been for years under treatment—has tried tonics without number, and washes without end, and all to no effect; I use the speculum. *I think* I see the cervix large, red, and on either lip I *imagine* that I see an ulcer. Under the influence of this idea—supposing, nay, so far has the delusion gone with me, verily believing that there is an ulcer—I apply nitrate of silver, I cauterize this *supposed* ulcer. I do this again and again; my patient gradually improves in health—is convalescent—is well. She returns to her home, a happy, useful wife; and in a year or two I get a letter, full of gratitude and happiness—the long barren wife is a mother. Now is this case—and, like many others who devote special attention to diseases of the uterus, *and use the speculum*, my experience will supply many such—is this case a mere delusion? Have I dreamed that I saw this ulcer—that, after repeated cauterizations, I saw it growing less and less, till nothing but sound mucous membrane appeared? Have I dreamed all this? Must I, at the dictum of Dr. Lee, or Dr. Ashwell, or Dr. any-body-else, give up my own observation, my own experience? And why? Because they will not look, and do not see. But there are great moral considerations, and Dr. Ashwell says he should feel tempted to give up the treatment of the diseases of women altogether, if the speculum continued to be used as it has been. And shall I follow his example?—shall I give up the instrument that has enabled me to restore scores of women to health and happiness, because it is indelicate? Well may Dr. Locock say that the talk about the indelicacy of the use of the speculum was all nonsense. Nonsense it is—poor, paltry, but yet mischievous, most mischievous non-

sense; and, for myself, I believe that all this talk about treating the use of the speculum is little better, and most of that about its abuse is very much worse. The attempt to cast reproaches on honorable men for well-intended efforts to advance our science and improve our means of curing disease, because those attempts involve personal exposure, is much more likely to have its origin in professional jealousy than in moral principle.

P. S. Since this was written, I have seen Marshall Hall's letter. He says, "a woman on whom the speculum has been used is never the same, *morally*, she was before." Did this come from another man, I should feel it my duty to the many excellent women for whose benefit I have used it, to say, that a grosser calumny on female purity never was uttered.

The Spirometer. Clinical Lecture. By PROF. S. JACKSON.

Medical Science finds, in almost every department of knowledge, some portion of its facts or laws applicable to itself, and lays them under contribution for its own advancement, or the augmentation of its resources.

The introduction of physics into the practice of medicine, applied to the diseases of the thoracic organs, belongs to the present time, and is the most valuable improvement that has yet been made in the diagnosis of disease.

Percussion and auscultation are means of ascertaining and interpreting the physical causes of sounds which can be determined by them as belonging to the thorax and its contents.

Skill in these processes imparts a degree of certainty to the diagnosis of thoracic affections, that nearly reaches perfection; it almost equals that of ocular inspection. There is, however, this defect attending them; disease must have made some progress, and change of structure have taken place to a certain extent, before physical pathological signs, that is, alteration in the normal sounds, or production of abnormal sounds would be produced. They do not avail us in indicating the approach of disease, or its forming stages, except to a very limited extent.

Another contribution from the domain of physics has been made, by Mr. Hutchinson, to the investigation of the respiratory functions in health and disease. It consists in an instrument he has invented, by which may be measured the amount of air that can be taken into and expelled from the lungs by voluntary effort; or what he calls "the vital capacity" of the lungs. By this instrument Mr. Hutchinson believes that incipient disease may be detected before physical signs exist. This instrument he names spirometer.

On the table is an instrument of the kind. It is simple and less expensive than that of Mr. Hutchinson. It was planned by a gentleman of this city, Mr. Charles McEuen, who has been confined to his room for some months by a pulmonary affection; possessing an active mind, with a turn for philosophical pursuits, he occupies his time in scientific observations and investigations. I gave him Mr. Hutchinson's paper, published in the *Medico Chirurgical Transactions*, containing a diagram of his instrument. Mr. McEuen constructed the instrument now before you on the same principles. I think it preferable to the original.

The instrument will be seen to consist of a cylinder containing water, in

which is immersed another cylinder inverted, into which the expired air finds its way. This cylinder is counterpoised by a weight attached to a cord passing over a wheel of large diameter, and which rotates with the ascent of the cylinder, caused by the entrance of the expired air, and on which a scale indicates the amount that has been introduced.

The person using this instrument must loosen any part of his dress that may restrain the movements of the chest or abdomen. He then deliberately expands his chest to its greatest extent, and expires through the mouth piece and air-tube into the cylinder. As this rises the wheel turns round, and an index marks on the scale, in inches, the amount expired.

To understand the use of this instrument, it is requisite you should possess some preliminary information on the respiratory actions, and to what extent they influence the air in the lungs.

Inspiration and expiration are performed by muscular power, and are both voluntary and involuntary actions. The extent to which they may be carried varies in different individuals, and in the same individual at different times. They have a limit which cannot be surpassed; the lungs can never be emptied, by the most strenuous efforts of expiration.

The air in the lungs is, therefore, divisible into two portions. The first, which is a fixed quantity, is that over which the will has no control, but remains after the strongest expiration, and is contained in healthy lungs after death. Its amount must correspond with the size of the thorax. Mr. Hutchinson calls this the residual air.

The second portion is that which is controlled by the will and muscular action. This portion Mr. Hutchinson divides into three sub-portions. 1st. Reserve air, or that portion which, after an ordinary expiration, may still be thrown out by a voluntary effort. 2d. Breathing air, or the portion inhaled and exhaled in ordinary breathing, when at rest; and 3d. Complementary air, or that portion that can be inhaled, by the strongest effort, beyond the amount of ordinary inspiration.

The three last are included in, and designated by the term "Vital Capacity." It is, in fact, the highest effort of the muscles producing respiration. The spirometer measures the "vital capacity" of an individual, and, it appears to me, is the measure of the muscular respiratory power.

Mr. Hutchinson was struck with the fact, that the vital capacity had no relation to the size of the thorax. On the contrary, he found, by experiment, that persons of the largest thorax possessed a less vital capacity than others with chests much smaller.

In the course of his observations he remarked that there appeared to prevail a very close relation between the height of individuals and their vital capacity. This circumstance was the more strange and unaccountable, as height depends most commonly on the length of the lower extremities, and not on that of the chest or trunk alone.

From observation made on a large number of individuals, taken indiscriminately from various classes of society, amounting to 2150, he arrived at the conclusion, that the vital capacity is a constant quantity, and holds a close relation with the height.

From the result of direct examination, in near 2,000 cases, Mr. Hutchinson felt authorized to announce the following rule, "For every inch of height (from 5 feet to 6 feet) eight additional cubic inches of air, at 60 deg. are given out by a forced expiration."

He further states, "here is a guide for the operator, and a rule given that will enable us to compare men of different stature and conditions of health, one with another."

If this result should be found accurate, the spirometer would be unquestionably a most valuable addition, to aid the physician in deciding the state of health, in many cases, that are, by our common mode of examination, enveloped in great uncertainty.

The following table shows the relation between height and vital capacity.

HEIGHT.				TOTAL CAPACITY.	
Ft.	In.	Ft.	In.	Cubic Inches.	
5	0 to 5	1	-	-	174
5	1 "	5	2	-	182
5	2 "	5	3	-	190
5	3 "	5	4	-	198
5	4 "	5	5	-	206
5	5 "	5	6	-	214
5	6 "	5	7	-	222
5	7 "	5	8	-	230
5	8 "	5	9	-	238
5	9 "	5	10	-	246
5	10 "	5	11	-	254
5	11 "	6	0	-	262

Before making any further comment on the rule laid down authoritatively by Mr. Hutchinson, I will test by the instrument the vital capacity of some patients affected with pulmonary disease, who are now present.

(Several patients, cases of chronic pleurisy, phthisis pulmonalis in various stages, and emphysema, were tested, the height and age being first ascertained.)

They vary, you perceive, from 80 to 120 cubic inches expired. Not one of the above patients approaches to the normal vital capacity, in accordance with his height and age.

They are from 80 to 200 cubic inches below the standard according to the table.

I must confess, that I have some misgivings as to the accuracy of this rule, and cannot but suspect that another element than that of height regulates the extent of vital capacity, and that element is the muscular force of the respiratory muscles.

I express this only as a suspicion. The extent of Mr. Hutchinson's inquiries, the evident care, labor and conscientiousness with which he pursued his investigations, entitle them to the highest consideration, and they should not be lightly questioned.

But, in a considerable number of examinations I have made on healthy individuals, of the same height and age, with slight difference of weight, there is manifest such wide difference of vital capacity, that I cannot but hesitate in adopting the rule as universally applicable.

I have, for instance, examined, within 24 hours, three gentlemen in perfect health, one a member of our profession, who have all been and are engaged in active pursuits. They are, respectively, 5 feet 11 inches, 5 feet 11½ inches, and 6 feet in height; the vital capacity of the first two is only 170 cubic inches, and of the last 190 cubic inches. According to Mr.

Hutchinson's table they ought to have a vital capacity of 250 to 260 cubic inches.

Now, these gentlemen have a peculiar, and I may say, an American conformation. I am under the impression it is not common in England. They are tall, long limbed, thin, with very slender muscles.

The highest vital capacity I have met with, is in a young gentleman 5 feet 8 inches in height, in whom it is 280 cubic inches. He is of sanguine temperament, large, bony framed, and with well developed muscles. So far as about 100 observations have been made, I have not found that uniform relation, as stated in the rule, between height and vital capacity. The differences, from 20 to 100 cubic inches, are too great to be attributed to accidental circumstances. The individuals I speak of are all in high health.

More numerous and extended observations are, however, required before a positive conclusion on this subject can be justified.

It has occurred to me that the discrepancies between Mr. Hutchinson's statements and my own observations, should they be confirmed by more numerous experiments, may depend on differences of race. The English are far more homogeneous than the Americans. In this country races are mingled, and continue to be more blended every day. As a race the English are bony, muscular and sinewy. Experiments with the Dynamometer have shown they possess a superiority of muscular force.

In a homogeneous population the average height and weight would be in accordance with an average development of the muscular system. But in a mixed population the same rule would not apply.

I believe there can hardly be a question as to the very marked difference in the general aspect and structure of the native born Americans, who are generally a mixed race, and those of the English, German, Irish, and French.

In examining Mr. Hutchinson's Table A, exhibiting the total capacity of 15 different classes, there are very striking differences to be seen. Pugilists, seamen, fire and police-men, and grenadier guards, have the greatest vital capacity. This is shown in the column of the table for the height of 5 ft. 8 in. to 5 ft. 9 in., and from 5 ft. 9 in. to 5 ft. 10 in.

Table of the mean Vital Capacity of 15 different Classes.

	5 ft. 8 in. to 5 ft. 9 in.	5 ft. 9 in. to 5 ft. 10 in
Seamen, - - -	239	258
Fire Brigade, - - -	231	237
Police, Metrop., - - -	226	248
Ditto Thames, - - -	250	240
Paupers, - - -	199	262
Mixed Class, - - -	238	246
Grenadier Guards, - - -	233	240
Compositors, - - -	214	231
Pressmen, - - -	245	239
Draymen, - - -	223	245
Gentlemen, - - -	208	236
Pugilists, &c., - - -	243	273
Chatham Recruits, - - -	251	266
Woolwich Marines, - - -	240	246

In this table the vital capacity certainly does not correspond to height as it respects different classes. Those classes comprehending individuals whose occupations require athletic, robust, and picked men, exhibit a vital capacity varying from 20 to 40 cubic inches higher than paupers, composers, and gentlemen.

This table appears to sustain the conclusion which seems to follow from the observations I have made here with the Spirometer, that it is muscular power, and not height, that governs the "vital capacity."—*Med. Examiner.*

Clinical Lecture on Ligature of the External Iliac Artery. By PROF. MORR.

On Friday, Dec. 13th, Professor Mott, after some concluding remarks upon the subject of Thoracic Aneurism, proceeded to speak of Ligature of the External Iliac Artery. This operation, he remarked, had been first performed by Abernethy, in 1796, but unsuccessfully. He tied it in 1806, with a successful result. He was followed by Messrs. Tomlinson and Freer, in England; while in this country it was first ligatured by Dr. Dorsey, and next by Dr. Wright Post, of this city. Afterwards by Dr. Smith, of New Haven, Dr. A. H. Stevens, Dr. Jamieson, and Dr. D. L. Rodgers, and others. Dr. Mott described the different modes of operating, and then his own manner of performing it, which he exhibited at the same time upon the subject. He makes a curvilinear incision, commencing just above the external abdominal ring, and extending outwards, and parallel with Poupart's ligament, towards and a little above the anterior superior spinous process of the ilium.

The skin and superficial fascia are divided, and the tendon of the external oblique clearly exposed. This tendon is then cautiously divided to the extent of the external incision, and it is then separated from the internal oblique, and the flap turned up. The edge of the internal oblique and transversalis muscles is then carefully detached from Poupart's ligament, and turned upwards. A portion of the funnel-like, or tubular process of the fascia transversalis, which invests the cord, is then pinched up and raised by the forceps, and then divided transversely with the point of the knife. The finger is then passed into it, along the cord to the internal abdominal ring. The pulsation of the artery is then felt behind and below the ring. The cord thus serves as a guide to the artery, while, by the above method, we are sure of getting *below* the peritonæum, so as to raise it from and above the artery. In this mode of proceeding, there is less danger of tearing, or otherwise injuring the peritonæum, than in any other plan of performing the operation. Having raised up the bag of the peritonæum, the edges of the wound are to be separated by spatulas, and by the fingers of assistants, so as to enable the operator to get as good a view as possible of the artery, which is then to be carefully separated from the vein which is below and on the inside of the artery; and only to an extent sufficient to allow of the passage of the aneurism needle. In this, as indeed in all cases, the vessel should be disturbed and isolated from its sheath as little as possible. The needle should be passed *from*, not *towards*, the vein. The artery is tied generally about an inch above Poupart's ligament, and care should be taken before tying it to ascertain that no nervous

filaments are included in the ligature. The edges of the wound are brought together by a suture, and slight adhesive straps, but no bandage of any kind should be applied, nor any thing which may constrict the limb, or tend to interfere with its circulation. Loose cotton, or some equally good non-conductor of heat should be placed all around the limb, from the toes to the groin, so as to cherish the heat and vitality of the part.

Dr. Mott, in speaking of the Statistics of the Operation, stated that he had ligatured this artery seven times—four times with success. Of the three remaining cases one died from secondary hæmorrhage; one from peritonitis, caused by excess in the use of spirituous liquors, and the last from gangrene of the inferior extremity. This was a case of traumatic aneurism, in which the aneurismal sac communicated with the femoral vein.—*N. Y. Register of Med. and Pharmacy.*

On the Use of Chloroform. By J. D. Gross, M. D.

I have used chloroform extensively during the last two years, and am disposed to believe that, as an anæsthetic agent, it is, in every respect, preferable to sulphuric ether. The first case in which I exhibited it was that of a colored woman who had an enormous tumor of the thigh, involving the femoral artery. The operation was performed in February, 1848, and, although necessarily tedious, was unaccompanied by the slightest pain. The patient soon fell into a tranquil sleep, and so continued during the dissection. Since that period, I have employed chloroform constantly in all my most important operations, and have never, except in one solitary instance, witnessed any disagreeable effects from it. The case to which I refer was that of Benjamin Cunningham, of Western Virginia, sixteen years of age, who had a large tumor on the left side of the neck. He was tall and slender, and of a feeble, delicate constitution. The operation was performed before the medical class of the University of Louisville, in November, 1849, and lasted less than five minutes. The tumor weighed nearly six pounds. The quantity of chloroform administered was very small, not certainly exceeding one drachm and a half; it was inhaled from a small cup-shaped sponge. The patient became affected in a few seconds, falling into a profound sleep, and thus continuing for fifty-five minutes. The pulse during the whole of this time was small and feeble, but perfectly distinct; the countenance was pale; the eyes were turned up and the pupils dilated; and the extremities were cold. There was no vomiting, and apparently no nausea. As soon as the excision of the tumor was over, and several small vessels tied, the youth was removed from the amphitheatre into my private room, where, under the influence of cold air, of volatile alkali freely applied to the nose, of sinapisms to the præcordial region, and of warmth to the feet, he gradually awoke to consciousness. It may be observed here that the utmost care was taken, during the performance of the operation, to prevent the entrance of air into the veins of the neck. Had this precaution been neglected, it might have been difficult to determine whether the effect in question was produced by this cause alone, or partly by it and partly by the chloroform. Cunningham rapidly recovered from the operation without an untoward symptom.

This is the only instance, out of several hundred, in which the use of chloroform at all alarmed me. For a time it was uncertain how the case would terminate; and I must confess I felt greatly relieved when I saw the young gentleman regain his consciousness.

I have administered chloroform at almost every age, from that of sixteen months up to that of seventy years; in both sexes; in slight operations as well as the most severe; and under various circumstances of health and disease. In some of my cases, it has been found necessary to keep up a sustained action of the remedy for nearly half an hour.

I have remarked that there is much diversity in different individuals in relation to the susceptibility of the system to chloroform. In some, as in young Cunningham, the smallest quantity sufficed to produce the desired effect; while in others a large dose was necessary, as well as a longer exhibition of the remedy. Children usually require very little, much less, comparatively speaking, than grown persons, and in them the effects also seem to be less evanescent.

I have not noticed in any of my cases that the use of chloroform exerted any injurious effect upon the recovery of my patients; that it excited an unusual degree of fever or local irritation; or that it retarded, impeded, or prevented the adhesive process. I have repeatedly witnessed the occurrence of erysipelas after it, but not more frequently, I think, than would have taken place had the operation been performed without it. The excitement consequent upon the administration of the remedy has usually passed off in a short time, and in no instance have I witnessed any secondary effects that were justly ascribable to its influence.

In several of my cases, transient opisthotonos appeared to have been produced by the inhalation of this article. This phenomenon was strikingly displayed in a young gentleman of Covington, Ky., to whose hip I applied the actual cautery on account of coxalgia. While under the influence of chloroform, which he inhaled rather badly, his head was forcibly thrown backwards, and the body forwards, precisely as in opisthotonos. The effect did not last longer than fifteen or twenty seconds. No stiffness of the jaws was noticed.

On two occasions, while performing the operation of lithotomy, the patients were affected with the most violent priapism, which, however, subsided, in each case, in less than half a minute. The ages of the patients were, respectively, four and twenty-two years. I may remark here that I never witnessed a similar result in any of my stone cases in which chloroform was not administered.

Since I have commenced the use of chloroform, I have abandoned that of sulphuric ether altogether, being persuaded, from ample experience, that it is, in every respect preferable. It produces the desired effect in a much shorter time, causes less excitement of the brain, is more persistent in its action, and is more easily inhaled. In a word, I have every confidence in its efficacy and safety, and I could hardly imagine a more desirable anæsthetic agent.

Finally, my rule always is to bring the system gradually, not rapidly, under the influence of this remedy; an object which is usually attained in thirty or forty seconds, provided the patient gives us his thorough co-operation, and does not embarrass us by his obstinacy or timidity.—*Trans. Am. Med. Association.*

Refracture of a Leg, to Improve Defective Surgery. By R. D. MUSSEY, M. D., Prof. of Surgery, Medical College, Ohio.

On the 29th January, 1848, Miss J. E. Kingsley, a school teacher, in Jefferson Co., East Tennessee, in descending a hill, was thrown from a buggy, and had both bones of the left leg broken in two places; one three and a half inches below the knee, the other two and a half inches above the ankle.

It was six weeks before Miss K. began to sit up in bed, and four months before she was able to ride out. She came to Cincinnati in July of the same year. Ever since the injury, the leg had been considerably swollen, and there had not been a day without more or less pain, sometimes severe, extending from the upper fracture to the heel, back of the foot and toes, indicating lesion or compression of the fibular nerves.

Both fractures were firmly consolidated. The lower fracture was well enough, exhibiting no deformity—at the upper one, the leg was sadly bent, exhibiting a prominent external convexity, or angle, so great as to shorten the distance from the knee to the inside of the foot about an inch and a half; the planter surface of the foot looking inward, and its outer edge looking directly downward. Of course the limb was altogether useless in walking; any attempt to apply the foot to the ground aggravating the pain. It was impossible to place the sole of the foot down flat, or bring the heel within an inch of the ground. The limb was therefore left to swing, while Miss K. moved about upon the other leg, and a pair of crutches.

In September, 1848, aided by my son, Dr. Wm. H. Mussey, I operated in the following manner. A firm pad an inch and a half thick was laid upon the inside of the knee, another upon the inside of the ankle, extending five inches up the leg. A splint of hard wood, one inch thick, and three inches wide, was laid, and secured by a bandage upon these pads. A broad padded belt was placed over the angular projection of the fracture, and gradually tightened by a mechanical power, derived from Jarvis' adjuster, till the fracture was crushed, and the leg straightened.

Miss K. having been placed under the influence of chloroform, was wholly unconscious of pain during the operation, and occupied herself all the while, in singing sacred songs, and holding celestial conversation; and while a bandage and splint were being applied to maintain the new position of the limb, finding herself coming to earth again, she entreated most earnestly for more chloroform, to prolong the ecstatic illusion. After the operation, the pain in the leg and foot were diminished, and in two months the fracture was consolidated.

Dec. 12. There is now no pain at the heel, and comparatively little in the leg and foot. The limb has its natural direction, is as long, and apparently as strong as the other. She can now walk with a cane, and limpingly without one.

Feb. 1849. Miss K. now walks very well without crutch or cane, and only now and then feels slight pain in the leg, the nervous injury having been almost repaired. Some months after the above date we saw Miss K. walking well in the street as if nothing had happened.

CINCINNATI, Nov., 1850.—*Western Lancet.*

EDITORIAL DEPARTMENT.

On the History of Quinine; the Preparations of that Article, and their Use in the Treatment of Diseases. By THEODORE S. BELL, M. D., of Louisville, Ky.

Nearly ten years ago, we communicated for the American Journal of Medical Sciences a report on the treatment of Intermittent fever, in which the following positions were advanced: 1. That Quinia may be administered in single doses of from 15 to 30 grs., at any period during the apyrexia, without any unpleasant consequences, and with the effect of arresting promptly the paroxysms; and that in some instances in which doses of a single grain of this remedy repeated at short intervals are with difficulty retained, five grains, or more, give rise to little or no uneasiness. Facts derived from an analysis of a large number of cases were submitted in support of these conclusions. The employment of Quinia, in the doses just mentioned, was not claimed as original. Other observers had reported results leading to similar conclusions. We were not, however, aware of this while the cases which we reported were in progress. Since the publication of the article referred to, Quinia has been administered much more freely than we then deemed safe, or should now venture upon, save in some desperate cases. Owing to the tendency to extremes, this invaluable drug, of late years, has been employed in doses as excessively large, as they were formerly inadequately small. In discovering that most of the dangers which once restricted its employment to particular periods, as well as in minute doses, are imaginary, it seems to be overlooked, by some, that any evils can possibly arise from it—that, like other remedies, it is capable of exerting a potency for evil, as well as good.

2. A second position advanced was, that, in the management of Intermittents, the specific remedy, the Quinia, should be promptly resorted to, without waiting for what is known as the "preparatory treatment." We contended that no preparation was needed, and that not only was time lost by the delay required for the operation of cathartics, emetics, and mercurials, but the cure was rendered thereby more difficult, and less complete. The plan of preparing the system in the reception of the Quinia is still enjoined by most, if not all systematic writers on practical medicine, and pur-

sued, if we mistake not, by the majority of practitioners.

3. A third position was, that the sooner the paroxysms are arrested, the less is the liability to relapses. An opposite opinion has been advocated, and is still a popular belief, leading persons suffering from Intermittent fever, to permit the disease to go on unchecked, with the expectation that it will, in the end, "wear itself out."

4. A fourth position was, that by promptly resorting to the use of Quinia in the early stage of remitting fever, the progress of the fever, in many instances, is arrested, and the disease converted into an Intermittent which is readily curable.*

Ten years' experience has served to confirm, more and more, the correctness of the foregoing opinions.

Our attention has been called to the subject by the perusal of a highly interesting, and able memoir in the Louisville Medical Journal, the caption of which is prefixed to this article. Dr. T. S. Bell, one of the accomplished editors of the Louisville Journal, author of the memoir, in addition to various historical, and other facts relating to Quinia, enunciates, as the results of his own abundant experience, views similar to those set forth in the four positions which have just been stated. By premising these positions we do not mean to imply any connection between the article in which they were contained, and the opinions of Dr. Bell. Dr. Bell's views, indeed, as he states, were formed prior to the publication of that article, nor was the latter seen by him until his memoir was written. He mentions the latter fact, and takes occasion to refer to our communication in flattering terms of commendation.

A correspondence in views which are at variance with commonly received doctrines is always a source of gratification; and, in this instance, we assure Dr. B. that his own sentiments are reciprocated, and for the same reasons assigned by him, reasons which are much more pertinent in their application to himself. Believing that the practical views at which we have arrived, each by his own observations and reflections, independently of the other, will, ere long, be generally adopted by the medical profession, we confess that we have some personal interest in referring to the publication made by us on this subject, in the Am. Jour. of Med. Sciences, Oct. 1841, and this belief is, in part, our apology for so doing by way of introduction to some extracts from the memoir by Dr. Bell.

Passing by several pages of historical details which, however will well

* A fifth position might be added, viz., that if complications exist, the paroxysms should first be arrested, and the complications afterward attended to. The reverse has been, if it be not still the common practice.

repay perusal, we quote the following, simply adding that we trust the writer's caustic satire upon the *Hepatic pathology* will do something toward rendering it obsolete—a consummation devoutly to be wished:

The writer of these remarks was taught in the lecture room, that the use of quinine was altogether unnecessary in the management of intermittent and remittent fevers. The doctrine seemed so clearly demonstrated, that it was as implicitly relied upon as if it had been written by Moses on Mount Sinai. No one, after a long experience with quinine or Fowler's solution, ever gave those remedies with more entire confidence in a favorable result, than I gave pills of calomel, aloes, and rhubarb with a certainty of purging off the periodical return of the cold stage. It was a matter of amazement that agues could be so remarkably stubborn, as I found them, but then I had been taught that they were often very stubborn and intractable under any treatment. And even when necessity forced a surrender of the simple and beautiful theory to the stubborn requirements of experience and observation, the theory was comforted, and the patient was victimized, with an error scarcely less pernicious than that which was abandoned—that error was, that there was an absolute necessity of preparing the system for the use of quinine. I had been taught that there was a viscus in the human economy, called the liver, which played such an active, omnipresent part in all diseases, that there seemed to be no room for any thing else in the human body. Its boundaries were very imperfectly understood. In a very large class of diseases of the brain, the liver was there; in the most of thoracic diseases, the liver was in the thorax; in nearly all abdominal diseases, the ramifications of the liver were to be distinctly traced. The brain, the lungs, the heart, pancreas, stomach, intestines, kidneys, uterus, bladder, etc., were mere satellites moving around a central sun, and that sun was the liver. In examining Dr. Stokes' daguerreotype of the Hepatic school of medicine, "in which the existence of almost every organ, except the liver, seems to be forgotten, and in which the creed seems to be that there is but one viscus, the liver, one source of disease, biliary derangement, and one cure, mercury;" it struck me, that I knew a man, whether in the body I shall not tell, or whether out of the body I shall not tell, who was covered by the picture as perfectly as any other sleeper ever was covered by a blanket. There seemed to be a necessity for looking up the other viscera, and for ascertaining whether they had any actual uses, and what were their conditions in health and disease. This was a work of labor, but the rewards have been very great, and the improvement has been so useful, that the exercises that produced it may be reasonably commended to others as conducive to the making up of the practitioner of medicine. The discovery of one additional viscus, where the existence of only one had been recognised before, is something in the way of progress; the full recognition of the movements and relations of all the viscera, in health and disease, is worth all the labor and perseverance that are necessary to its attainment.

It is somewhat singular with what tenacity an error frequently holds its ground in the practice of medicine. The necessity of putting the stomach and bowels in a special condition for the use of quinine, in the treatment of intermittent disease, and of remittent fever, is one that had such a feeble footstalk and is so palpable, that it is wonderful it could have lived long

under the light of reason. The theory that gave the error much of its *animus*, was sufficient of itself to overthrow it. The first link in the chain of evils was said to be weakened action of the heart; this brought on congestion of the vena cava; this produced derangement of the liver, and that opened Pandora's box. And yet we were taught to permit this state of things to go on day after day, to remove congestion at night, and have it renewed the following day, or the day after, as the case might be, thus introducing into practical medicine the labors of Penelope! It is evident that each returning chill, each exacerbation of fever, must renew those derangements which physicians were endeavoring to remove, and that the proper mode of breaking up the concatenation of morbid phenomena would be found in that plan that promptly arrested the chill or the exacerbation of fever. And when the inquiry was made, what is there in the condition of the stomach and bowels, at the outset of periodical fever, that forbids the prompt use of quinine for its immediate arrest, it was impossible to give any satisfactory reason for delay. But custom was too strong, and the prejudices of early education are almost almighty. I broke through the meshes of early teaching with great difficulty, but got out with rapidity after the first attempt was made. A case of malignant chill occurred in this city, many years since, which created some noise at the time. I heard of the case with surprise, and upon inquiry, I was told that there was nothing unusual in the two chills that preceded the fatal one; that there was nothing whatever that induced a suspicion that the third chill would assume a malignant form. This aroused in me a determination never to permit a patient of mine to have a third chill, and this determination was strengthened soon after by the distressing case of a professional friend in the county of Jefferson. I had seen him but a few days before his illness, in full and vigorous health, apparently, and was surprised by a summons to visit him one evening, at his place in the country. I found him in the tenth hour of a malignant chill, presenting all the phenomena of a collapsed case of cholera. There was not a symptom of that stage of cholera absent from this case of malignant chill. Upon inquiry, I found that the victim in this case had paid little or no attention to the first and second chills, but had attended to his professional duties during the intermissions, and had trusted to an emetic, on the morning of the third chill, for arresting the ague fit. The emetic failed, and reaction never came on.

The two cases of malignant chill thus brought before my observation effected a thorough change in my views of the correct treatment of intermittent fever, and I have had no reason to regret the change. The rule adopted then, and persevered in ever since, was that the presence of intermittent fever was the exact preparation that required the use of quinine, and that the earliest administration that could be made of it, was the very best thing that could be done for the patient. Auxiliaries have sometimes been resorted to in this practice, but they were always subservient to the quinine—that is the chief and reliable remedy. In commencing this practice, which I was then persuaded was new, my intention was to amend the condition of the chylipoetic system after the intermittent malady was broken up, but in a long series of years, and in a great multitude of cases of all the various forms of intermittent and remittent fever known to this locality, I have rarely found it necessary to do anything after the ague was destroyed. The questions to be decided are: is it safe to administer quinine

in intermittent and remittent fever, without preceding its use with emetics or purgatives? Is the patient likely to do as well under the immediate administration of quinine, as under the use of it, after emetics or purgatives? Experience and widely extended observation answer in the affirmative, and go yet farther, for they testify that under the immediate exhibition of quinine patients are not only in as safe a condition as under the preparatory treatment, but that they are infinitely safer. They escape prolonged sufferings, avoid dangerous lesions, and are free from the risk of the malignant chill. These are abundant reasons for preferring the prompt use of quinine in periodical disease, but the advantages are much more numerous than those I have named.

The following extract relates to the same subject:

It is unnecessary to dwell at length upon the fatal lesions that may result from delay in breaking up the proximate cause of dangerous congestions. They are too familiar to all who have practiced in a malarious region. These dangers may not always result in perilous lesions, but even the morbid effects stand prominent enough, as beacons, to admonish a prompt removal of the periodical attack. Mere functional derangement, as a consequence of persistent intermittent fever, in many cases makes itself felt in all of the subsequent life of the patient. Truth, experience, observation, fidelity to the profession of medicine, and a faithful guardianship of the welfare of the sufferer, all imperatively require of the physician the utmost promptitude in arresting the chills. When I can cut the first paroxysms off in the middle of its efforts, I always do it, and never permit a second one to show itself, when I am called in time to interfere. As long as we are ignorant whether the attack of chills may terminate in the malignant variety; of what fatal lesions may occur; of what functional disorders may be left as heir-looms, by child's play with the disease, that length of time will physicians be called upon to use promptly the resources of their art in breaking up that state of things that is so frequently perilous, and the existence of which is always unnecessary. I have seen patients purged day after day for weeks, the physician apparently waiting on providence in the hope that something would turn up to justify the use of quinine. And when the state of the patient at length compelled the use of that medicine, the patient's system was not as well "prepared" for the use of the quinine, as it was before the scientific preparatory treatment was commenced, yet to their amazement the patient recovered without any evil effects from the quinine. I should think that such facts would have an effect upon the after-practice of the physician, but there are men who will walk through mud-holes in a well-beaten path, especially if the path has been beaten by themselves, rather than turn out of the road. Upon such the lights of experience shed their rays in vain; with them all the phenomena of disease, and recovery, are as accidental as the throws of the dice, and they forget that in practical medicine, fortunate numbers may turn up, if the dice are properly loaded. The man who does not improve under the teachings of experience is a pitiable object, but the physician who does not advance in such a school is baleful.

With respect to the use of Quinia in doses that formerly would have been deemed hazardous, the writer holds the following language:

Within the past few years a great change has taken place in the mode of administering quinine, especially, in intermittent fever. The practice formerly was to commence in the apyrexia, and give two grains of quinine every two hours until the return of the period for the recurrence of the paroxysm. This plan was supposed to be the only safe one; a great fear was felt of the stimulating effects of the medicine, and it was quite common "to watch the effects," as the phrase goes. Although many physicians stepped out of this well worn rut, the experience of the physicians of the United States army, in Florida, first gave prominence to the great superiority of administering quinine in a large and decided dose, at once. The paper, read on this subject before the National Institute, first taught the value of the practice, and it has steadily increased in the estimation of observant physicians, from that time to the present. Where an interval of from fourteen to eighteen hours can be found, a single dose of twenty grains of sulphate of quinine, administered to an adult, at the commencement of the apyrexia, and from four to six grains to a child, are the best means of managing the disease. This plan is quite as certain in its result, in arresting the paroxysm, as the divided portions of quinine were; the disturbance of the patient, the breaking of his sleep, and the consequent injury to the nervous system, are all avoided by the decided dose. A patient can take twenty grains of quinine, and rest secure against any further return of the chill.

The writer gives his experience in the use of Quinia in Remittent fever in the following paragraph:

In simple remittent fever I have generally pursued the course described in the treatment of intermittent fever. The great object is to make as perfect a remission as possible, and this can usually be done by cold ablutions of the body and spine; cloths with hot water in them, to the head, and a judicious use of the lancet. These means, combined with the administration of quinine, are all that I usually find necessary in the treatment of simple remittent fever, and the success has not only been gratifying, but I have been able greatly to facilitate the cure of such cases, in comparison with the former treatment. It is seldom that the lancet, purgatives or nauseants have to be used. The quinine may be used in the early periods of remittent fever, in the same way that it is given in the intermittent form. Of course, after serious inroads are made upon the healthy functions of the viscera, other means than the quinine must be resorted to, but even then, that medicine is an important adjunct.

The principle involved in the following extract we are satisfied is as true, as it is practically important in the treatment of intermittents. We have long entertained and acted upon the opinion that, by continuing the remedy for a long period after the paroxysms are arrested, the liability to relapse is lessened, and also that Quinia possesses prophylactic virtue. We are persuaded that we have repeatedly warded off a relapse by a timely resort to the remedy. Dr. B. cites his own case as follows:—

In 1835 I had an attack of intermittent fever, and became the subject of

frequent relapses. They became so annoying that I consulted a number of medical friends, whose age and experience, I hoped, might furnish me some means to avoid the troublesome returns of the disease. They assured me that they knew of no medical means that would answer the purpose. It seemed to me that I was condemned to a life of suffering and inaction, and the subject occupied my thoughts almost constantly. At length I struck out an idea that ought to have been palpable at first. It is, that the quinine that interrupts the action of the cause of the paroxysm, if continued long enough, will remove the cause altogether, and I acted upon the thought at once in my own case. From that time to the present I have had no relapse, and as fifteen years have elapsed, I think there is little danger of one now, and that the principle has fairly tried itself. In taking quinine for this object, it is necessary to use it but once a day, in doses varying from three to ten grains. Under this course of treatment I have seen but little difficulty with relapses. Some years after this use of quinine, I was gratified in finding that Dr. Graves, of Dublin, was advocating the principle, which had been successful in my own case.

We have confined our quotations from the memoir by Dr. Bell to portions relating to the points embraced in our introductory remarks. The writer treats of the applicability of the Quinia to other diseases than Intermitting and Remitting fevers, viz., to Congestive fever, Yellow fever, Rheumatism, Neuralgia, Cholera, etc. His remarks upon these relations of the subject are valuable; but to do justice to the memoir we should transfer it entire to our columns, which our limits forbid.

Demonstrative Midwifery.

In the Dec. No. of this Journal, we charged a review of the Report of the trial of Loomis for libel, signed C. M., in the American Journal of Medical Sciences, No. for Oct., 1850, with containing, first, a misrepresentation of the published facts relating to demonstrative instruction in midwifery at the Medical College in this city; and, second, an unjust imputation of unskillful management of the case of labor which was the subject of demonstration. We pledged ourselves to make good these charges, if occasion seemed to require it.

In the January No. of the American Journal of Medical Sciences, the Editor, premising that he has received a letter from Prof. White respecting the two grounds of complaint just mentioned, and expressing a desire that on every occasion the fullest justice should be done to all persons, says, with regard to the first point, i. e., the misrepresentation of facts, "that the reviewer has honestly expressed the impressions made upon his own mind from a careful perusal of the evidence, and that as he has been

misled, others may be equally so; and," he adds, "we are therefore happy to have the opportunity of allowing Professor White to place the facts in their true light."

That any reader, "after a careful perusal of the evidence," should have received the impressions expressed by C. M., is a matter of wonder, but we have no disposition to gainsay the assertion. An individual cannot be held responsible for a lack of apprehension, but it is certainly not an unreasonable ground of complaint that a writer should assume to exercise the functions of a critic, if he be so liable to be misled in the endeavor to comprehend plain facts set forth in a publication which he undertakes to review. In the libel suit growing out of the demonstration of labor at the Buffalo Medical College, inasmuch as the chief object of the prosecution was to have the facts fully and clearly developed, special pains were taken that there should be no occasion for honest misapprehension on this score.

With respect to the management of the case of labor, the position of the head of the child, as ascertained by stethoscopic examinations prior to the delivery, and confirmed by the ocular demonstration, was "occiput to the right posteriorly, face left anteriorly." The Reviewer, C. M., declared a "*great error*" to have been committed in not rectifying this faulty position in the early stage of labor, "by bringing the occiput into such a position as would permit it to emerge under the arch of the pubis, while the face followed the curvature of the sacrum." The Reviewer farther affirmed, that, by omitting this "first duty of an accoucheur," the professor "permitted the life of both mother and child to be put in jeopardy." Concerning this accusation the Editor of the Am. Journal, in the Jan. No., says, "upon consideration of the subject we cheerfully allow that the language of the reviewer was too strong. We consider the practice advocated by Dr. Denman, and the late Prof. Dewees, than whom we cannot admit any higher authority in midwifery, to correct this presentation in the early stage by bringing the occiput into such a position as will permit it to emerge under the arch of the pubis, to be the best; but it must be admitted that the observations of Naegele show that nature herself will often accomplish this, and even when she fails to do so we have several modern authorities for non-interference, and who maintain that though the labor is more protracted, still delivery may take place with safety to mother or child."

Were we to discuss the subject in its scientific bearings, we should respectfully dissent from the view, as thus expressed, of the weight of authority respecting the duty of the accoucheur in cases of the position referred to.

We are aware that Denman, Dewees, and other of the older writers on midwifery, recommended early manual interference in such cases, but we suppose, agreeably to views now generally entertained, these eminent writers to have been deceived with respect to the feasibility of the manœuvre recommended by them, attributing, incorrectly, to the efforts of traction exerted upon the head, spontaneous changes effected by the action of the uterus upon the body of the child; and that not only *several*, but the *majority* of the most approved teachers of the present day do not recommend that procedure, by omitting which, according to C. M., a "*great error*" was committed by Prof. White, jeopardising, thereby, the life of mother and child. Be this as it may, the falsity of the accusation of malpractice made by the reviewer with so much assurance, is repudiated by the able Editor of the American journal, in a manner creditable to his honor, and sense of Justice. We cannot, however, avoid expressing surprise that the amende was relinquished by the author of the review to the honor and sense of justice of the Editor. One might not unnaturally be led to presume that a degree of sensibility to be shocked by an ocular demonstration of labor for purposes of instruction, would be accompanied by a corresponding acuteness of the moral sense which would insist upon the privilege of retracting a false accusation into which the writer might, through prejudice or inadvertence, have been betrayed—more especially when the accusation affected a fellow member of the profession, and a public teacher, in a point in which he might well be expected to be most sensitive, viz: his professional knowledge and ability.

We cannot concur with the respected Editor of the American Journal of Med. Sciences in the remark that, inasmuch as these are matters of importance only that injustice be not done to individuals, they are therefore of secondary interest. The rights, the character, and the feelings of individuals, as it seems to us, should be not less sacred than the claims of science and education. We conceive that a writer is in no instance justified in proclaiming with his pen, what would be in violation of propriety and courtesy if pronounced with his tongue. The reviewer and the Editor, we submit, are bound to be guided by the same ethical rules, in the columns of a Journal, which should govern colloquial intercourse. Nay, the moral obligation is even stronger, inasmuch as more injury may be inflicted by written, than by spoken injustice. Who would not choose to abide a verbal slander, circulating slowly, and within circumscribed limits, from mouth to mouth, rather than an aspersion made indelible by print, and scattered in all directions, where the vindication might perhaps never follow it. It

will not surely be questioned that for a medical practitioner to stigmatize the practice of his professional neighbor as unskillful, and dangerous to lives, would, among honorable men, be deemed, to say the least, ungenerous, even if there were valid grounds for the charge; but if the imputation were wholly without foundation, it would imply something more than a want of generosity, and could hardly escape harsher terms of rebuke. Now, is it any the less censurable that an unfounded accusation of malpractice is made in the columns of a Journal supposed to circulate more extensively than any other in the United States? If we are mistaken in the belief that there does exist no real distinction in the two cases—that the position of a reviewer furnishes no exemption from those rules of honor, of courtesy, and morality, which should regulate verbal expressions affecting the character or feelings of members of the same profession, then we not only stand ready to be corrected, but to make all proper apologies for any injustice which may be implied in the views we have just expressed.

It would be unfair to close this article without stating that the Editor of the *Am. Jour. of Med. Sciences* declares his full concurrence with the reviewer in the opinions of the latter with regard to the propriety and advantage of demonstrative midwifery. He expresses also the opinion that these sentiments are not confined to a small minority of the profession. He says, "We have yet to meet with a single respectable physician in Philadelphia who does not concur in them," and they are the sentiments, he adds, "if we may judge from our sources of information, of a large majority of the profession in the United States." The opportunity of the Editor to become conversant with the opinions of the medical profession in Philadelphia, on a point of science or education, it would be presumed, of course, should be better than our own, yet we chance to know that more than one member of the profession of that city, occupying positions second to none, are far from being opposed to demonstrative midwifery. What the editor's sources of information are, with respect to the prevailing sentiment on the subject throughout the United States, we do not assume to know; but the impression derived from our sources of information is quite the reverse of that entertained by him. The discussion of this point, however, would be alike unprofitable and irrelevant. We believe, after considerable reflection, that the interests of science, education, and humanity would be promoted by rendering the study of midwifery demonstrative with the aid of the living subject. This opinion is formed irrespective of the opinions of others, upon a consideration of the merits of the question, uninfluenced, so far as we may judge, by any feeling, or bias. We claim, of

course, the prerogative to hold our own opinions on this, as on other topics, and to utter them as we deem proper, at the same time conceding freely to others the same privileges, and desirous, at all times, to treat the views of those who may honestly differ from us, with due consideration and respect. For ourselves, and, we will venture to add, on behalf of the school with which we are connected, it is far from being contended that the profession of the country shall unanimously consider demonstrative teaching in midwifery either expedient, or useful. All that has ever been claimed is, that the facts relating to the subject should not be misrepresented, and that motives should be construed with a proper regard to truth and justice.

OBITUARY. *To the Editor of the Buffalo Medical Journal:*—

DEAR SIR,—Hardly has there occurred to me a more earnest admonition of the uncertainty of life, than the recent demise of an intimate and noble friend and companion of my pupilage, Alfred Milton Rowe. A year ago, of the one hundred and fifteen disciples who frequented the amphitheatres of the Medical College at Buffalo, none with stricter fidelity fulfilled the obligations of a sincere courtier of useful knowledge, or more assiduously aspired at a career of discipline and distinction. You can not fail to remember the sincerity and dignity of his demeanor, and the air of severe devotion to the duty of preparation for professional labor, which he manifested in the lecture room. As a companion, he was cordial, constant, fraternal. A more congenial and cheerful associate, I have seldom met. His heart was a fountain of sympathy and disinterested sentiment. He had eminently the characteristics and impulses of a broad humanity.

The features of his mental constitution were by no means so ordinary as not to justify the most gratifying hopes of success as a practitioner and medical inquirer. He had a readiness of suggestion and originality of argument, which rendered him if not formidable, at least much to be respected in private debate; and he so ably mustered the expository lore of the amphitheatre, as to match and confound the theoretic elasticity of his antagonists.

In allusion to the means of improvement, and the pleasures of social intercourse, which he so well appreciated and enjoyed, he often, as if involuntarily, exclaimed—"These are golden days!" Truly, with what a feeling of exhilaration and hope did he glance over the field of prospective industry and usefulness; and how suddenly has the earthly vision fled!

The tide of life, the mystery of whose perpetuity, and the phenomena of whose derangements he was struggling to comprehend—ere his work was accomplished, was arrested in his own stature, and the spirit which animated forsook the tabernacle in which he sojourned. Touched by the remembrance of his fidelity and promise, his personal and peculiar excellence, his industry and his endowments, I can not withhold so brief a recapitulation of virtues not less worthy of imitation than commemoration.

Let the living profit by his ambition, of which the sum is thus expressed:

Si honorem, non laborem,
Quæris, frustra niteris,
Si præ-esse, non prodesse,
Studeas, nihil officia.

And as well ought it to be remembered, that the little island of existence allotted to mortals, but briefly overlies the infinite sea into whose unexplored profound we all must come, but in whose solemn solitudes are the portals of eternal life to the wise.

I append a short paragraph from the letter of a friend of the deceased:

"Mr. Rowe left Gowanda, where he had previously resided, in May, 1850, and went to Glade Mills, Butler Co., Pa., where he located himself in business, and designed to remain for some time. He had been there but a few weeks, when he was attacked with bilious fever, and died Sept. 13th, after an illness of four weeks. His friends were not acquainted with his sickness, until a week before his death, when he expressed a wish to see his father. A letter was despatched without delay, but too late. He was buried before his father's arrival. His remains were taken to Sunderland, Mass., where his family and friends reside. He was perfectly conscious to the last, and seemed prepared for the great change that awaited him. His age was 24."

Respectfully yours,

CLINTON COLEGROVE.

Buffalo Medical College.—(The annual commencement in this Institution will be on the last day of the term, Wednesday, the 26th inst. On the day preceding the commencement, the examination of candidates for graduation before the Board of Curators will take place. The address to the Graduates on commencement day will be delivered by Prof. Coventry. It is hoped that there will be a general attendance of the Curators, and that many other medical gentlemen interested in the prosperity of the College, will be present.

Buffalo Hospital of the Sisters of Charity.—By the “first general report of the Trustees and managers” of this Institution, we learn that up to the time of making the report, Nov. 27, relief had been extended to 1513 sick. The following financial statement is copied from the Report:—

The Hospital was begun in 1848. In July of that year, a lot and a house sadly wanting repairs, were purchased for \$3,700; other lots at an expense, interest, &c., computed, of \$3,375, were purchased soon after. The first repairs of the house, fencing, &c., cost \$1,300; a first addition with baths, &c., was erected at an expense of \$1,700; a second addition, large and commodious, with furnaces, a room for the surgical operations, &c., cisterns, &c., was put up at an expense of \$5,400; the dead house and stable cost \$200; beds, bedding, furniture, medicines, surgical instruments, provisions, &c., cost \$9,192; blinds, repairs, articles recently purchased, amount to \$350; servants' hire for two years and three months, cost about \$600; making a total of \$25,817. To meet this expense a collection was made in Buffalo, many of all religious denominations kindly contributing, it produced \$980; donations by visitors amount to about \$300; the Fair of 1848, yielded \$1,016; receipts from patients, or from those who charitably paid for them, amount to \$2,666; the State Legislature generously granted \$9,000; for the county poor, \$577 has been received; Bishop Timon collected from his friends out of this Diocese, \$3,100; a citizen of Buffalo has donated \$2,800; making the aggregate sum received, \$20,579. There is due to the Hospital by the Commissioners of Emigration, about \$340. The Hospital yet owes \$3,800 on the lots and house; \$1,000 has been borrowed to meet current expenses, and some small debts, not exceeding \$500, are due through the city.

The following extract exhibits the revenue derived from patients:—

Since the opening of the Hospital 605 patients have paid for themselves generally at the rate of \$1.50 per week; 823 have been charity patients; many were classed among pay patients, who, their funds giving out, really paid but for a part of their stay; thus 79 persons, classed as pay patients, stayed 1603 weeks and 3 days in the Hospital, yet they could only pay for 126 weeks and 1 day, leaving the greater part, viz., 1477 weeks, on charity.

A fair has been held since the publication of the Report, the net proceeds of which were fifteen hundred dollars.

The following are the medical officers of the Institution:

From April 1st to Oct. 1st.

Attending Physicians—Dr. Mackay, and Dr. Geo. N. Burwell.

Consulting Physicians—Dr. G. F. Pratt, and Dr. I Barnes.

Attending Surgeon—Dr. A. S. Sprague.

Consulting Surgeon—Dr. J. G. Camp.

From Oct. 1st to April 1st.

Attending Physician—Dr. Austin Flint.

Consulting Physician—Dr. E. Wallis.

Attending Surgeon—Dr. F. H. Hamilton.

Consulting Surgeon—Dr. James P. White.

House Students—Sandford Eastman, and E. H. Gibbs.

New Medical Journals.—Two new Medical Journals have recently made their appearance.

The *Philadelphia Lancet*, is to be issued semi-monthly, at one dollar per annum. It is a folio with double columns, and is intended to be a medical newspaper. The Editor, Thomas Dunn English, M. D., states, in his salutatory that he shall "oppose quackery in any and every place;" that he shall oppose all false reverence for men and cliques, adding that "we have no reverence for any opinions contrary to our own;" and that his primary aim is an "aggregation of useful facts." The Editor enters upon his undertaking with sanguine expectations. He says "that we shall succeed we feel confident, for we laid the proper foundation to our fabric before we raised the superstructure." Firm faith in success doubtless contributes to its attainment, and we hope that, if necessary, it may prove so in this instance.

The *Stethoscope; and Virginia Medical Gazette* is the title of a new journal, issued at Richmond, Va. This is a monthly of 64 pages, well gotten up, at \$3 per annum, or \$4 at the end of the year. By the cognomen selected we infer the objects of the Editor, Dr. P. Clairbone Gooch, to be to explore the internal, vital elements of science and the profession, and to study the *signs* of the times. We cordially wish him success in his diagnostic efforts, and trust he may succeed in curing the maladies which his investigations may discover.

Departure of Dr. Alfred Stille for Europe.—We are pained to learn that Dr. Alfred Stille, of Philadelphia, who has been very zealous and active in promoting the measures of reform in the medical profession, contemplated by the organization of the American Medical Association, of which he is a member, has, on account of a serious cerebral attack, been compelled to abandon, for a time, the practice of his profession, and to seek in Europe a restoration of his health. May he reap the greatest benefit from his travels, and return in full vigor of mind and body.—*Charleston Med. Journal.*

To Publishers.—We must ask the continued indulgence of those who have favored us with new publications. We have received several works which we shall notice so soon as we can find leisure to bestow upon them due examination.

Prof. White—Prof. W. sailed for Havre in the packet ship *New York*, on the 17th ult. He expects to remain abroad a year, chiefly with a view to objects connected with instruction from the Chair held by him in the University of Buffalo.

Evansville Medical College.—The Western Medical Chirurgical Journal states that the above named Institution has “generously proposed to admit Sons of Temperance at half their usual fees,” whereupon the Sons reciprocate the compliment by publishing that “they have no hesitation in recommending the school as in every way worthy of confidence.” The Editor of the Western Medico-Chirurgical, a professor in a neighboring school at Keokuk, Iowa, expresses his scorn of such methods of competition, and adds, “rather than lecture to a class obtained by such means, however large it might be, we would lay aside our professorial robes, and, if need be, take up the language of Francis 1st, “all is lost but our honor!”

Surgical Report for the American Medical Association.—The committee is invited to meet in the Charleston Hotel, South Carolina, the evening of the first Tuesday in May next. All professional brethren who have *surgical* facts connected with the improvement of this branch of the profession during the year, will please address them to the chairman of the committee by the first of April, at Augusta, Georgia. As all cannot be reached by a circular, it is hoped no one will wait for a more direct application than this general invitation.

PAUL F. EVE, M. D.

Prof. of Surgery in the Louisville University, and Chairman of the Committee on Surgery of the Am. Med. Association.
Louisville, Ky., Dec., 1850.

American Medical Association.—The Committee of Arrangements request all societies and other institutions authorized to send delegates, to forward a correct list of those selected to attend the next annual meeting, to the Secretary, Dr. H. W. DeSaussure, at Charleston, S. C., on or before the first day of April.

In consequence of the resignation of Dr. Stille, one of the Secretaries, from ill health, all communications intended for the next meeting of the Association must be addressed to the remaining Secretary, Dr. H. W. De Saussure, Charleston, S. C.

The Fourth Annual Meeting of the American Medical Association will be held at Charleston, S. C., on the 2d Tuesday of May next.

Editors of Medical Journals will please give the above notices an early insertion in their respective Journals.—*Charleston Med. Journal.*

To Correspondents.—Communications from Drs. Hamilton, Gardner, and Hubbard will appear in our next No.

BUFFALO MEDICAL JOURNAL

AND

MONTHLY REVIEW.

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NO. 10.

ORIGINAL COMMUNICATIONS.

ART. I.—*Lithotomy on a Female.* Reported by FRANK H. HAMILTON, M. D., Prof. of Surgery in the Medical Department of the University of Buffalo.

Caroline Friesz, of Buffalo, aged 23 years, has had symptoms of stone in the bladder since she was three years old. During the last few months she had suffered greatly from incontinence, and pain, and her health had become so much impaired that she was confined to her bed. Several days previous to the operation, she had been unable to sleep, and it was evident that without relief she must soon die.

On the 19th of March, I made an operation for the removal of the stone. Having placed the patient for a subpubic operation, and having committed the hands and feet to assistants, for she was not tied, I introduced a narrow, probe-pointed, straight bistoury, without a director, into the bladder, and cut outward and downward.

I adopted a precaution in this stage of the operation, similar to what I have before recommended and practised in making the same operation upon the male. In the male, I recommend keeping the forefinger of the left hand in the rectum, while the thumb of the same hand follow the wound, to avoid cutting the rectum. The danger of cutting the vagina

in the female is quite as great, and I therefore kept the forefinger of the left hand in the vagina, and the thumb of the same hand in the wound.

Still more effectually to avoid opening the vagina, the edge of the bistoury was at first directed *outward* and then downward. If the directions given to cut "outward and downward" are literally followed, especially in females who have borne children, where the urethra fairly sinks into the vagina, a wound of the latter will often be inevitable.

The stone was easily seized, but could not be disturbed from its bed at the fundus of the bladder. It was then broken, and after very protracted efforts, about two-thirds of the whole stone was removed. During a portion of this time I was assisted by Dr. Alden Sprague of this city, and by the counsel of other medical gentlemen who were present. Although the stone was large, it was quite evident that it was not for want of a sufficient opening that we failed to remove it, but because it was encysted. We at length desisted, leaving a portion of the stone yet within the bladder. During the whole time occupied in the operation, the patient was kept more or less completely under the influence of chloroform; one ounce was consumed! Before the operation was completed, she gave signs of sinking, and we commenced the free administration of brandy. She soon rallied, and remained quiet and comfortable for some hours, when a peritoneal inflammation, supervening, she died on the 3d day after the operation.

Autopsy.—Serum and lymph in the cavity of the peritoneum. The portion of stone which had not been removed found situated in the fundus of the bladder, and could not be dislodged by pressure upon it, or by seizing it with the forceps, the body of the bladder being contracted to the extent of two or three inches from the neck upward, so as to resemble the neck itself. This narrow portion was knobby and irregular upon its inner surface. The fundus, containing the unremoved stone, was more smooth, but had several indentations of three or four lines in depth, into which corresponding prolongations from the calculus were fixed.

To have removed this calculus entire it would have been necessary to have made an incision along the narrowed and thickened body of the bladder until the disturbed fundus was reached and laid open, a proceeding which must have rendered a fatal urinary infiltration inevitable.

I was also able to determine in the autopsy, that the fundus of the bladder lay too high to have been reached from the vagina. The entire stone, including the fragments which were removed in the operation, weighed four ounces.

Its composition is ammonio-phosphate of magnesia and lime with traces of lithates.

ART. II.—*Case of Paraplegia.* Reported by WM. H. GARDNER, M. D., of Whitesboro, N. Y.

On the 21st Sept., 1847, I was called to R. P., a strong, healthy and intelligent boy, aged 13 years. I learned on inquiry that eight or ten days before,* he fell backwards from a lumber wagon, striking his back across a stick of wood, somewhere in the upper part of the lumbar region. He complained some at the time, but continued at work the remainder of that, and for four or five days following, without complaining, and as he said with little lameness. On the fifth or sixth day after the fall, when the weather was cold and damp, he was out in the field with other boys, amusing himself by throwing apples, and lay on the ground for some time, which was damp. During the evening, he was attacked with pain and lameness in his back, at the seat of the former injury, which had increased up to the time when I saw him. He was now, when still, suffering but little, but on the slightest motion of the body the pains became excruciating.

On examination, no perceptible marks or swelling were visible at the seat of the injury, and there was but little if any tenderness there. The constitutional symptoms were inflammatory. Vs. was practiced, together with cupping over the seat of the injury, and Tinct. of Colchicum, with cathartics, prescribed.

The next day, Sept. 22d, I saw him again. He expressed himself much better—had been able to get off the bed without assistance, and could move, or be moved, with much less suffering, but was complaining of a sensation of prickling in all that portion of his body below the seat of the injury.

Sept. 23d.—The sensation of prickling had increased, and there was partial loss of motion, the lower extremities not readily obeying the dictates of the will.

24th.—The symptoms of the previous day were aggravated, except the pain, which had gradually diminished. Cups were again applied, and as his bowels had not moved for the last day, a cathartic, or aperient, was administered, with directions to repeat the cathartic if necessary.

25th.—Dr. T. by my request saw him with me. His bowels had not responded to the medicine, nor had he passed his urine since the day before. In addition to this there was now total paralysis of all that portion

* In conversing with the parents and friends, I have taken the time that elapsed after the injury, before I saw him, from them. My own impression was that it was not so long.

of the body below the injury. The prickling sensation was gone, and he was free from pain. On examination, the bladder was felt fully distended, but he said he had not suffered at all from it. A catheter was now introduced without difficulty, and about three pints of urine brought away. He said he did not feel any sensation on its introduction.

A blister was now applied to the seat of the injury, and cathartics freely administered without the slightest effect, for the space of five or six days. During this time, injections with a pump syringe were resorted to, but after the first no fecal matter was brought away, and the liquid flowed back, as from a leather hose. A motion of the bowels at last followed the administration of a common Seidlitz powder, and copious stools, attended with flatus were discharged. After this, his bowels became regular, moving at proper and stated intervals without the aid of medicine; but yet he was not conscious of it at the time. During all this time, and for some days after, he voided no urine. It was regularly drawn twice a day, with the catheter. He never complained in the least of its introduction, declaring that he had no sensation about it. Sometimes when left a little more than twelve hours, he complained of an indefinite sense of uneasiness, which he could not locate, but which was at once relieved by the use of the instrument. About the tenth day from the paralysis and suppression, his urine commenced dribbling away in small quantities, from one to two ounces at a time, as it was judged, and often repeated. The catheter was after this once or twice introduced, but the bladder was found empty. The quantity discharged at a time was gradually increased, and the interval between the discharges prolonged till it was nearly normal. But still both feces and urine were voided without his consciousness. From the commencement of the disease, not the slightest delirium had been manifested, his mind appearing ordinarily active. His appetite for food had not been craving, but yet he had daily called for, and taken some simple nourishment. In this respect he had gradually improved. Still the paralysis, below the injury, was perfect. On passing different substances down the course of the spine, sensation was perfect until it arrived to a certain point or line, where it ceased altogether, and this line seemed but a hair's breadth in width. On requesting him to try and move his feet, his uniform reply was "I can't try." Still, slight spasmodic twitchings were observable in the lower extremities. At the time of the injury he was fleshy, but had become somewhat emaciated, but *no disproportion was observable between the different portions of his body*; and the circulation was apparently as active in one part as the other.

About the first of October, a swelling was first observed upon the back, although it had been examined from time to time, situated upon both sides of the spinal column, but greater upon the left than the right. At this time the principal enlargement was between the lower portion of the left scapula and the adjoining vertebræ, but extended much above and below this point. This continued to increase in size, but more downwards than upwards, till it reached from the neck to the sacrum. Some two weeks after, it began to show symptoms of pointing, at or near the place of the original injury.

On the 16th of Oct., Dr. T. again visited the boy with me, and, on consultation, it was concluded to make an opening into the tumor, notwithstanding fluctuation could not be felt very distinctly. The point selected for the opening was on the left side against the upper part of the third lumbar vertebra, which happened to be immediately below the line of sensation. The skin was raised deliberately to make a valve, after which the knife was passed slowly inwards and upwards till matter was reached, at the depth of from one to one and a half inches. During all this time, the patient sat perfectly still, not even a muscle moved, nor was there a perceptible change in his breathing. He afterwards said, on being told that it was done, that he was not aware of it, and supposed that we were "only getting ready." The discharge was copious, and the pus healthy. After it had discharged, as was supposed, about a pint, the opening was closed with sticking plaster, and it was left till the next morning, when the plaster was removed, and the discharge permitted to go on. The swelling now disappeared, and the cavity soon filled up so that no marks of the abscess remained, except a small cicatrix at the external opening. The back now appeared natural; the spinous processes of the vertebræ could be distinctly traced both by the eye and the fingers, from one extremity to the other, and not the slightest curvature or displacement was discovered. But yet, in defiance of our predictions, neither motion or sensation had returned in the slightest degree. Still, when placed on a chair, he sat quite erect, using his hands to right himself, when by accident his body became bent in any direction. His appetite was now good, and he was allowed a generous quantity of nourishing food; he gained flesh rapidly, his countenance soon assumed the glow of health, and no perceptible difference could be seen between the growth of the different portions of his body. But the paralysis still remained. At this time he had a bed-sore near the promontory of the ischium, in which there was deep sloughing, and into which, from the position he assumed, foreign substances were

often crowded so as to occasion bleeding and all the signs of irritation; still he never complained, and it healed rapidly. Other remedies were now resorted to successively, as friction, galvanism, blisters, strychnine, issues and setons, but all without any effect upon the paralysis. The blisters were so placed as to extend over the sensitive and insensitive portions, and healed as rapidly upon one as the other. The issues were made with caustic potash, immediately below the line of sensation, and he never uttered the least complaint. Both issues and setons healed readily. On irritating these sores when dressing them, and at other times, *the lower extremities were observed to move with a start*, such as is ordinarily witnessed on applying the hand suddenly to one in a healthy subject. These experiments were repeated, and the same effect was almost uniformly produced. Still he declared that he had no consciousness of any irritation about the sore, and that he did not know why his feet moved. Indeed, he was not conscious of the motion unless it was so great as to be transmitted to the upper portion of his body. On dashing cold water upon the lower extremities, the same motions were produced; and they in no way differed from those in ordinary cases. This was done at times when he did not see the water, and the same effect produced. On slapping the foot or leg smartly with the hand, like motions were produced and repeated for a great number of times. Similar motions were sometimes noticed without any apparent cause, but when the experiments were tried, they were uniform, or nearly so.

I continued to treat the patient till the 10th of April, 1848, visiting him occasionally. At this time he had acquired more than his usual amount of flesh, was in good spirits, and to a casual observer he presented the appearance of perfect health. Yet all my efforts to restore to him the use of his limbs, were totally unavailing, and he was still voiding his excrements without consciousness.

Soon after this, I learned that a respectable physician from an adjoining town had been called, who saw fit to prescribe for the patient without seeing or consulting me. He placed the boy, as I have been informed since, upon a bed, and enjoined perfect rest, in a recumbent position; but what else he did I do not know, as I never have had any conversation or correspondence with him upon the subject. In June, 1850, I was again called to treat another member of the family, when, as they were doing nothing, having lost faith in medicine, I took occasion to examine my former patient.

Dec. 30th, 1850, I again visited the boy, for the express purpose of examining him more closely, and to compare my own recollection of the

case with that of his family and friends—which was found to agree. He has now grown to the stature of an ordinary sized man; and there is still little of any disproportion between the inferior and superior extremities. I thought the muscular portion of the legs was not quite as fully developed as the corresponding portion of the arms, but this was not as great as would be supposed to exist from the lack of exercise, and would not be noticed, without an examination and comparison. The legs are well shaped, and he has what would be called a good and healthy form. When I saw him before, I thought the legs were slightly flaccid to the feel, but now it was not perceived. Still the paralysis is as perfect as ever. He says that never, since the attack, has he experienced the least sensation in, or been able to move his lower extremities to the slightest extent, by his own volition—and he continues to pass his excrements without knowing it. On requesting him to try to move his foot, I was again met with the reply—“I can't try.” On passing the back of a pocket knife blade down the course of the spine, I found the point at which sensation ceased was against the body of the second lumbar vertebra, and from thence extended around the body in the course of the nerves. I now repeated some of my former experiments. On dashing cold water on his legs, the same starts as above described were again witnessed, and repeated as often as the application was renewed. I then struck the limbs smartly with the flat of the hand, and repeated the blow for full twenty times, and every blow was followed with a like muscular contraction. This was tried on both limbs with the same effect. The person who had dressed the setons and issues informed me that the same phenomena was always witnessed on cleaning the sores and applying the dressings upon his back, till they were allowed to heal; and still, he never showed any signs of suffering from those that were located below the line.

The above case has been thus briefly detailed. Much doubtless is left out from lack of recollection, or for the reason it was not deemed of importance. But so far as it goes, I believe it perfectly accurate, having revived my memory by all the means within my reach, and compared it with that of others, with which it agreed.

The case may not be altogether devoid of interest in a medical or surgical point of view. The treatment can not now be very fully or accurately described, nor would it perhaps possess interest if it could. In restoring to the unfortunate boy the use of his limbs, it has been entirely unsuccessful. Slight differences of opinion might exist, in regard to the diagnosis and treatment of the case. But yet there can be little doubt that all,

on an examination would agree, that nervous communication between the pain and lower portion of the body had been cut off or suspended in some manner. If this be so, and we know of no reason to doubt it, the case demonstrates several physiological facts of some importance. It shows that the brain is not important to the proper performance of the functions of assimilation and disassimilation—or those of organic life. For in this case, for the space of more than three years, they have been carried on, as healthily below the second lumbar vertebra when the communication is stopped, as above it.

And it shows also that there *may be motion independent of the pain, or of any nervous communication with it.* Thus if the nerves are the only medium by which impressions are transmitted from one part of the body to another, there must be a set or system of nerves, not universally acknowledged, which preside over, and induce involuntary motions—a set that in their action, or functions, correspond to Marshall Hall's excito-motor or true spinal system. For in this case, motion—an apparent start, may at any time be produced by outward impressions, whose impulse is not transmitted to the brain, of which the mind takes no cognizance, and for the production of which no volition can be transmitted.

Dec. 31, 1850.

Constitutions and Temperaments. By SILAS HUBBARD, M. D.

TO THE EDITOR OF THE BUFFALO MEDICAL JOURNAL:

SIR,—Having read a paper before the Erie County Medical Society at its last semi annual meeting, which received the favorable attention of the society, I am induced to hope that it may possess some interest to those of your readers who did not hear it, and that it may not appear entirely presumptuous in me to offer the same for publication. I therefore transmit to you the substance thereof, as follows:

GENTLEMEN:—On this occasion, I choose for the subjects of my discourse, the Constitutions and Temperaments. It will be perceived that in employing the words constitutions and temperaments, I make a distinction between them. Writers, both ancient and modern, have, in their imperfect description of the temperaments, confounded with them the constitutions. Without now stopping to show the particular defects of either ancient or modern writers, I shall proceed directly to give a very brief digest of the subjects of my discourse.

The constitutions, as I shall now define them, are those natural causes

of the differences of individuals, indicated by the developements of the head, thorax, abdomen, and the osseous and muscular systems.

The *temperaments* are those natural causes of the differences of individuals, produced by the qualities of the *constitutions*, and indicated by the complexion of the hair, eyes, and skin, texture of the muscular system, &c.

The constitution, as a whole, is divided into four principal classes, or heads, as already mentioned, which I shall name as follows: 1st, the Cerebral; 2d, the Thoracic; 3d, the Digestive; 4th, the Motive;—and shall treat of them in that order.

I. *Cerebral*. The size of this constitution is generally indicated by the developement or extent of the external periphery of the cranium. The brain, as a whole, is the organ of all those intellectual faculties and affective feelings, of which we can become conscious in this life. It is intimately connected with every portion of the economy, through the medium of the nervous system, and is related to every phenomenon of animal life. I shall not here give the anatomy, or discuss the physiology of the brain; but if we regard the principles of phrenology to be true, it will be seen that the peculiarities of developement, or structure of the brain, must constitute a great source of differences of individuals: but whether the details of phrenology are true, or not true, still the brain, by reason of its predominance or inferiority, has the power of modifying the whole economy—probably to a greater extent than any other portion of the general constitution.

On this occasion, I would scarcely be able to give more than a plan of the tendencies of the palpable external developements of this constitution. For this reason, I shall merely recommend to every physician the particular importance of studying the size and external conformations of the cranium as far as he can, in indicating the physical, moral, and intellectual peculiarities of individuals—both in children and adults.

II. *Thoracic*. The thorax is one of the splanchnic cavities; bounded, posteriorly, by the vertebræ; laterally, by the ribs and scapula; anteriorly, by the sternum; above, by the clavicle; and below, by the diaphragm. It is destined to lodge and protect the chief organs of respiration and circulation—the lungs and the heart. “The lungs and heart, as a whole, have for their principal functions the arterialization and circulation of the blood; and also largely participate with other vital organs in the production of calorification; and are thus the principal vivifying organs of the whole system. The capacity and power of these organs, and their room for action, can generally be indicated during life by the external

dimensions of the chest, and width of the shoulders. The capacity for forcible respiration, and power for exercise, are generally greatest at about thirty years of age. The general health and power of the individual, and the capacity for exercise, are dependent to a great degree on the development of this constitution. Its external development can, (as can indeed the development of each constitution,) be made an indication of predisposition to health and disease to a considerable extent; but which I do not now propose particularly to consider.

III. *Digestive.* By this term I particularly mean the abdominal viscera. The abdomen is the largest of the three splanchnic cavities; bounded, above, by the diaphragm; below, by the pelvis; behind, by the lumbar vertebræ; and at the sides and fore part, by muscular expansions. The chief viscera contained in the abdomen are the stomach, intestines, liver, spleen, pancreas, kidneys, &c.

There is considerable difference of capacity of the abdomen in different individuals, which will generally be developed in proportion to the size, or capacity of the contained organs. There are even national differences in this respect. The Germans and English are greater eaters than the French or Spaniards. Some people, especially after the middle age, have a tendency to corpulency, and possess a great development of this cavity, which is not supposed to indicate particular size or activity of the digestive organs. It is not important that I should now fully state the causes of this development; but suffice it to say, that such persons are generally good livers, or particularly fond of good living. A full development, and good degree of activity of the digestive organs are favorable; but there may be an excessive, or an insufficient development of these organs.

IV. *Motive.* I call this the motive constitution, because it comprises the osseous and muscular systems—the principal mechanism for all muscular movements. The osseous and muscular substances are generally duly proportioned to each other. They generally constitute the principal bulk of the system as a whole. The development of this constitution causes the principal difference of the size of individuals. Men have a larger endowment of this constitution than women. The peculiar and relative state of organization, or development of this, and the cerebral constitution—being the chief mechanism for all muscular movements and mental operations—principally decide the proper avocations of individuals.

I have thus briefly and imperfectly described the principal constitutions, more for the purpose of giving form to my plan of drawing a line of distinction between them and the temperaments, than the expectation of

throwing extensive light on either. The constitutions are intimately connected with, and related to each other. I shall not attempt even to name all these relations, but will briefly notice a few of those most palpable. The organs of circulation are intimately connected with, and dependent on the digestive organs, in the formation of blood; and every portion of the human system is dependent on the circulatory and respiratory organs for nutrition, calorification, vivification, &c. Also, every portion of the whole system is dependent on the cerebral constitution as the principal organ of innervation. Some of these relations existing between the encephalic organs and other portions of the constitution, I shall briefly allude to, on account of their supposed external palpable nature. Dr. Hoppe professes to have discovered the cerebral organ of Alimentiveness, appetite for sustenance, situated in the anterior inferior portion of the middle lobe of the cerebrum. Mr. J. S. Grimes professes to have discovered the organ of Pneumativeness, presiding over the respiration, located contiguous to Alimentiveness, and that there is a relation existing between the development of the head indicating the size of Alimentiveness, and the digestive organs; also, that there is a relation existing between the development of the head indicating the size of Pneumativeness, and the respiratory organs. Some learned and zealous opponents of phrenology, in their arguments against it, admit that there is a corresponding relation existing between the developements of the head, mapped by phrenologists as Alimentiveness, and the digestive organs; and of Pneumativeness, and the respiratory organs; and that the strength of the propensities in question are in proportion to the respective development of said organs: but they account for this apparent mutual relation of development of parts and power on very different principles from those of the phrenologists. I shall not here assume the province of deciding who are the nearest correct; both may be correct to a certain degree; but the fact that there is a correlation existing between the developements of the portions of the head in question, and the principal digestive and respiratory organs is worthy of record.

Some phrenologists, and also anti-phrenologists, are agreed that broad heads are generally associated with strength and vigor of constitution, or athletic powers. It seems to me that there is a greater coincidence between the development of the cranium indicating the size of the cerebellum, and the athletic powers or motive constitution, than any other portion of the cranium. So striking is this coincidence of development, that I am inclined to believe what several authors have hinted, namely, that the cerebellum is an animal organ which "has considerable reference to the

strength and perfection of the bodily frame." Indeed I would advance the opinion that its chief function is to preside over *nutrition*. In proof of this observation, I might adduce the following facts—1st, the cerebellum attains its fullest size at the time the bodily frame acquires its fullest perfection; 2d, not only is this difference of size of the cerebellum, and development of the athletic powers, observable in individuals of the same sex, but—3d, the cerebellum and osseous and muscular system are generally larger developed in the male than in the female. 4th, not only is this true of man, but of the animal creation generally, which possess the cerebellum, and—5th, the lower the animal in the scale of organization, the more deficient is the cerebellum. 6th, I have observed that those possessing the cerebellum small are more subject to perversions of nutrition than those with it large or full. 7th, I have observed that where the cerebellum is small or moderate, or the cerebrum much predominates in size over the cerebellum, the individual is possessed of a delicate constitution, and is likely to belong to that class described by authors as of the nervous temperament. I do not wish to be understood that it is my opinion, that it is the sole function of the cerebellum to preside over nutrition, or the development and maintenance of the bodily frame—it may have other functions; but I have not wished to speak of it only so far as it seems to relate to the subjects in question.

Having thus described, though imperfectly, the principal constitutions, I shall now proceed briefly to notice

The Temperaments.—The temperaments, as previously defined, are those natural causes of individual differences, produced by the qualities of the constitutions, and indicated by the complexion of the hair, eyes, and skin, texture of the muscular system, &c. In this discourse I shall only treat of them as they are exhibited in the Caucasian race. And before giving my own views upon them, I shall briefly state the manner in which they have been treated of by ancient and various modern authors; and shall then adopt such portions of their opinions, and make such criticisms and substitutions as I think most useful. The ancients generally admitted four, denominated from the respective fluids or humors, the superabundance of which in the economy was supposed to produce them; the *sanguineous*, caused by a surplus of blood; the *bilious* or *choleric*, produced by a surplus of yellow bile; the *phlegmatic*, caused by a surplus of phlegm, lymph, or fine watery fluid derived from the brain; and the *atra-biliary* or *melancholic*, produced by a surplus of black bile—the supposed secretion of the atra-biliary capsules and spleen.

This division was kept up for ages, without modification, and still prevails with one or more additional genera. The epithets have been retained in popular language without our being aware of their parentage. For example, we speak of a *sanguine*, *choleric*, *phlegmatic*, or *melancholic* individual or turn of mind, with nearly the acceptation given to them by the Hippocratic school—the possessors of these temperaments being presumed to be, respectively, full of high hope and buoyancy; naturally irascible, dull and sluggish; or gloomy and low-spirited. Metzger admits only two—the *irritable*, (*reizbare*,) and the dull or *phlegmatic*, (*trage*.) Urisberg, eight—the *sanguine*, *sanguineo-choleric*, *choleric*, *hypochondriac*, *melancholic*, *Bæotian*, *mæek*, (*sanftmuthige*,) and the *dull* or *phlegmatic*. Rudolphi, also, eight—the *strong* or *normal*; the *rude*; *athletic* or *Bæotian*; the *lively*; the *restless*; the *mæek*; the *phlegmatic* or *dull*; the *timorous*, and the *melancholic*; while Broussais enumerates the *gastric*, *bilious*, *sanguine*, *lymphatico-sanguineous*, *anæmic*, *nervous*, *bilioso-sanguine*, *nervoso-sanguine*, and *melancholic*. It is obvious, that if we were to apply an epithet to the possible modifications caused by every apparatus of organs, the number might be extended much beyond any of these. If we should regard phrenology as true, then just from the state of development of the cerebral constitution alone, we might multiply the temperaments to a greater extent, and with as much propriety as many of those above named. We might enumerate as many as there are different mental and moral qualities of individuals, or in other words as there are organs of the brain, or as many as we may choose from combinations of organs. “Perhaps the division most generally adopted is that embraced by Richerand, who has embodied considerable animation, with much that is fanciful, in his description.” These temperaments are the *sanguine*, the *bilious*, or *choleric*, the *melancholic*, the *phlegmatic*, and the *nervous*. In giving my description of the temperaments, I shall principally make requisitions on this last division.

The temperaments, as I shall divide them, are three:—1st, the *Nervo-sanguine* or *active temperament*; 2d, the *Bilious* or *enduring temperament*; 3d, the *phlegmatic* or *easy temperament*.

I.—*The Nervo-sanguine* or *Active Temperament*. This temperament is indicated by sandy, or chestnut, or light colored hair; florid complexion, or fair skin; light, or blue, or gray eyes, and firm flesh. If the general constitution is well developed, it is accompanied with strong, frequent, and regular pulse. The particular qualities of this temperament are, superior nervous susceptibility, and excitability and activity of mind and body.

Every thing else being equal, all the functions partake of more activity, than in the other temperaments. The peculiar complexions and characteristics of this temperament, I suppose are produced by a predominance of arterial blood, or by some inappreciable modification of the nervous and circulatory systems. This temperament has frequently been confounded with a predominant cerebral constitution, or an impaired or diseased constitution, and called the nervous temperament; or with a predominant thoracic constitution, with a moderate cerebral constitution, &c., and called the sanguine temperament. Every other circumstance being equal, the diseases of this temperament, are generally more violent, and oftener seated in the circulatory and nervous systems; as fevers, inflammations, hemorrhages, and nervous complaints, than in the other temperaments.

II.—*The Bilious or Enduring Temperament.* This temperament is indicated by dark hair, dark eyes, and dark or brown skin, or skin inclining to yellow, muscles firm, subcutaneous veins prominent. Every other condition being equal, the pulse is rather stronger and harder, though not quite so frequent as in the *nervo-sanguine* or *active temperament*. Individuals of this temperament, *cæteris paribus*, are stronger, and capable of enduring (protracted mental, or physical fatigue or exercise, better than those of the other temperaments; they are not so often precocious, or susceptible to the effects of medicine, or external influences, as those of the preceding temperament. The diseases are generally combined with more or less derangement of the hepatic system. We are not entirely certain what are the causes of the peculiar complexions, and qualities of this temperament; they may be produced by a peculiar structural organization of the liver, or biliary organs in general, or by a predominance of dark or venous blood, or it may be both causes combined. Richerard enumerates as examples of what he calls the bilious temperament, Alexander, Julius Cæsar, Brutus, Mahomet, Charles XII, Peter the Great, Cromwell, Sextus V, and the Cardinal Richelieu. To these, Good has added Attila, Charlemagne, Tamerlane, Richard III, Nadir shah and Napoleon. Richerard and Good do not mention the fact, that these great men derived their superior abilities, as much from a favorable endowment of the constitutions, as from the peculiar qualities imparted by this temperament. This temperament has been combined by various authors with a small, or moderate endowment of the thoracic constitution, or with derangement of some of the abdominal organs, or in the nervous system, and called the melancholic or atrabilious temperament.

III.—*The Phlegmatic or Easy Temperament.* This temperament, I

think, is better defined by authors, than any of the others. The following by Dunglison, is the usual description of this temperament; which he calls the *Phlegmatic, Lymphatic or Pituitous Temperament*.—"In this case, the proportion of the fluids is conceived to be too great for that of the solids; the secretory system appearing to be active, whilst the absorbent system does not act so energetically as to prevent the cellular texture from being filled with humors. The characteristics of this temperament are:—soft flesh; pale skin; fair hair; weak, slow, and soft pulse; figure rounded, but inexpressive; the vital actions more or less languid; the memory by no means tenacious, and the attention vacillating; with aversion to both mental and corporeal exertion. Pomponius Atticus, the friend of Cicero, is offered as an example of this temperament, in ancient times; Montaigne, in more recent history. The latter, however, possessed much of the nervous susceptibility that characterizes the more lively temperaments. Dr. Good suggests the Emperor Theodosius as an example in earlier times; and Charles IV of Spain, who resigned himself almost wholly into the hands of Godoy,—Augustus, King of Saxony, who equally resigned himself into the hands of Napoleon,—and Ferdinand of Sicily, who surrendered for a time the government of his people to the British,—as instances in our own day. It would not be difficult to find, amongst the crowned heads of Europe, others that are equally entitled to be placed amongst these worthies." This temperament, I think, has suffered too much disparagement in comparison with the others; so much so, that no individual would think himself at all honored to be called of the phlegmatic temperament. I call it the *easy temperament*, because the repletion of the cellular tissue, and other peculiarities of it, disposes the individual to quietude, and thus protects all of the organs from injury by too powerful exercise of their functions, and by the defence afforded by the secretions. Every person possesses more or less of the qualities which constitute this temperament; and which are essential, to a certain degree, to our very existence: but we rarely speak of this temperament, excepting when these qualifying causes exist so superabundantly, as to be quite palpable, and to almost constitute morbid conditions.

Such are the temperaments as they seem to me. There may be other qualities of all the constitutions, produced by fineness, or coarseness, or intimacy of structure, which cannot be detected during life, only by their effects; some of them may be seen by dissection, but which I shall not dwell upon in this discourse, which may have been already protracted too long for your patient hearing. I shall also avoid further protracting this

discourse, by not even naming trifling points of differences, and by omitting to speak particularly of the constitutions and temperaments, as modified by parentage, age, sex, national differences, habit, cultivation, climate, food, disease, &c.

I shall conclude this discourse by recommending to you the great importance of studying the constitutions and temperaments as they are developed by external indications. They are sufficient of themselves to constitute a large volume. It would be useful if physicians would note the constitution and temperament of all their patients, and ascertain all the diseases to which they have been subjected. We might thus form statistics of all the diseases most likely to be associated with the various constitutions and temperaments. Not only might we make a proper understanding of the constitutions and temperaments a source of immense advantage in understanding the causes and proper treatment of diseases: but it may also be made a source of immense advantage, in directing the moral, intellectual, and physical culture of mankind.

BUFFALO, January, 1851.

ART. IV.—*Case of Fallopian Pregnancy, with Rupture, ending fatally.*
Reported by W. H. Watkins, M. D., of Racine, Wisconsin.

MR. EDITOR:—The following unfortunate case having lately required my attention, I forward you the history.

I was called, at nine P. M., on the thirty-first of Dec., to attend Mrs. E—W—of this city, æt. thirty years.

Found her at her husband's jeweller's shop, some half mile from their residence. Mrs. W. was a strong, plethoric woman; health had been invariably good for years. To use her own expression, "had known what pain was only from what she had endured during confinement."

She was now suffering excruciating pain in the left Iliac fossa; pulse slow, weak, scarcely perceptible; hands and feet cold, and a deathlike pallor overspread the countenance; had vomited slightly; bowels regular: had eaten nothing more than usual; was nursing a child some eighteen months old; had menstruated regularly for the previous six months; discharge lessened in quantity, but slightly however, at the last two periods; time had returned for her being again unwell; had exerted herself considerably that afternoon, by lifting a stone out of a barrel, which weighed sixty to seventy pounds. In reply to my interrogatories concerning character of pain: she said it first resembled labor pain, but immediately became lancinating and changeable in its locality. Ordered Tinct. Opii, grs.

ji, and some gin being handy, gave one ounce in conjunction. This was repeated in twenty minutes; hot bricks were placed to her feet and warm cloths to abdomen. Being somewhat relieved, a sleigh was ordered, and I rode home with her. Pain had changed from left to right side—still severe. Gave Acet. Morphia gr. $\frac{1}{4}$. An unsuccessful search for hernia was then made—ordered mustard cataplasm, large, to cover whole abdomen, and patient to be surrounded with warm woolen cloths. Morphia and gin sling repeated. Patient began to revive, pain decreased, and finally she fell into a gentle sleep fifteen minutes after eleven. I then returned to my office, leaving Morphine powders to be given with stimulus. Mr. W— called for me at one in the morning, saying pain had returned with increased violence. I stepped to the house of my partner, Dr. Hanchett, and engaged him to accompany me. We found our patient writhing in agony; pain over region of the stomach; increased upon pressure; pulse hardly perceptible; skin cold, relaxed, but with only an appearance of moisture. She complained of thirst, dryness of fauces, &c.

Gave Acet. Morphia, gr. i.
Capsicum, gr. iii.
Camphor, gr. iii.

Fomentations, warm, to the abdomen. Warm drinks and warm gin sling to alleviate thirst. Anodynes and stimulants were frequently repeated. In searching for a cause for the above stated aggravating symptoms, poisoning, either accidentally or designedly, occurred to us. The tongue was pale, flabby and tremulous; the gums were pale around the edges. We learned that she had been painting her front room and also the window frames and sash throughout the house, and that the sabbath previous she had occasion to send some sugar of lead, which she had in the house, to a neighbor.

We came to the conclusion that she had been poisoned by lead, though such a supposition would not account for all the symptoms in attendance, and only an idiosyncrasy would explain the severity of any. We left her more quiet at four in the morning, with directions to attendants to continue above treatment. Saw her at nine o'clock and found her in perfect state of collapse; learned upon inquiry that the opiates and stimulants had not been given, vomiting having supervened soon after our departure. Motion of the bowels had taken place, and I was informed by the old ladies that a show was discoverable.

Ordered, Brandy f3iv.
Tinct. Capsici gr. xx.
Acet. Morphia gr. i.

and an enema of starch and laudanum.

- and an enema of starch and laudanum.

Dr. Hanchett was again called in and we gave the stimulus in conjunction with the Ext. Belladonna gr. i frequently repeated.

Pain continuing, and patient not being relieved, further counsel was called. Drs. Groves, Page and Wooster saw her. Same treatment continued but without relief. And finally my patient succumbed a little past noon, only fifteen hours from time of attack.

A request for post mortem being acceded to, it was performed at ten A. M. the following day. There were present Drs. Hoy, Thompson, Talcott, Wilson, Wooster, Hanchett, and McNeill of Peoria, Illinois. Abdomen much distended. Upon making a vertical incision through parietes of the abdomen, we found the whole peritoneal cavity filled with bloody serum and large clots of coagulated blood—some five lbs. in all. The fallopian tube of the left side was enlarged, and a rupture, nearly round and only two lines in diameter, was discovered, and through it was seen a pale, fibrinous body. Upon opening it we found a fetus, membranes, cord, &c., all perfect. The placenta presented at the rupture, being, in fact, a case of placental presentation.

The fetus, judging from perfectness of development, rather than size, must have been from three to four months old. It was an inch long, head large, lower extremities well developed, membrana pupillaris quite distinct, mouth large and open, sex very evident, being a male.

Uterus double common size, lined with deciduous membrane one third inch thick. No connection between uterus and cavity in fallopian tube. The mouth of uterus not closed by lymph.

The principal point of interest in this case was in the diagnosis. In this there was a great mistake. Had there been an examination made by physical signs, doubtless a more satisfactory conclusion might have been arrived at; but no such exploration was made after I saw her the first time, and then there was only a slight tympanites.

It is of some interest as furnishing a case of tubal pregnancy where it was not supposed that the subject was pregnant.

Conjecture is the only resource left us as to the cause for such abnormal deviations from the prescribed course of nature.

The suggestion may be worth consideration however—whether nursing longer than nature dictates to be necessary, might produce such an effect. This idea is supported by the history of nearly all cases on record so far as I have had the privilege of examining.

WM. H. WATKINS, M. D.

RACINE, WISCONSIN, Jan., 1851.

Remarks on the foregoing Case.—In a note accompanying the foregoing article, the reporter requests us to add some remarks upon the case, as respects particularly, the diagnosis. Several circumstances combined to render the character of the attack obscure. The existence of pregnancy had not been suspected, and the continuance of lactation, together with the regular recurrence of menstruation, was opposed to such a suspicion, even had it been entertained. It is somewhat singular that the patient had not been aware of the presence of a tumor in the abdomen, but it does not appear from the history that any such discovery had been made. The symptoms of the outset denoted peritonitis, which was occasioned by the sudden rupture of the fallopian tube at that time, but it may readily be conceived that circumstances did not at once appear to justify the diagnosis of that affection; still less that the attack proceeded from such an origin. The case, however, may serve to illustrate the importance of a close consideration of symptoms and signs with reference to the discrimination of peritoneal inflammation from a neuropathic affection. Subsequently, on examining for hernia, it might be supposed perhaps that either the fallopian tumor would have been apparent, or a degree and kind of tenderness over the abdomen, which would have led to the supposition that peritonitis was present, if not to a conjecture as to the cause. The corpulency of the patient doubtless stood in the way of a successful exploration as respects the tumor, and owing to the great loss of blood with the consequent collapse, probably, the sensibility was so much obtunded that the abdominal tenderness was not so prominent as it otherwise would have been. Moreover, what tenderness was present seems to have been more marked in the epigastric region than elsewhere. Had the peritonitis been diagnosed it might under the circumstances have been attributed to other causes than rupture of the impregnated fallopian tube. Perforation of the intestine would have occasioned a train of symptoms in many respects similar, but less speedily followed by collapse and death, because there would have been less hæmorrhage. The latter was doubtless the immediate cause of death in this case.

A correct diagnosis, however, had it been made early, would have, in such a case, but little, if any influence on the treatment. The accident must almost, of necessity, prove fatal, and the only therapeutical indications were those which the symptoms rendered obvious, and which were adopted.

The history of the case is highly instructive, and should serve to impress the importance of bearing in mind the occurrence of fallopian pregnancy and rupture, as one of the causes of peritonitis suddenly developed.

ART. V.—*Report of the Sanitary Commission of Massachusetts.* 1850. Boston. 8 vo. pp. 540.

There are few, if any subjects, affecting the temporal welfare of human society, which possess greater importance than the subject of *Public health*. To any intelligent person, after due reflection, this proposition must be almost self-evident. To increase the probabilities of the duration of life, and to diminish the liabilities to disease, are objects which it would be hardly necessary to prove are secondary only to those which concern, more directly, the intellectual, moral, and religious improvement of the race. With the latter, confessedly higher objects, the subject of public health has closer relations than is by many supposed.

The attention which this subject has received from physicians, philanthropists, and legislators, bears an inverse ratio to its intrinsic claims. Its almost utter neglect in times past, would seem quite incredible, did not the history of science exhibit the curious fact, that the tendency of the human mind is to objects remote from human existence. Astronomy was one of the sciences earliest studied. Men were desirous of becoming acquainted with phenomena and laws belonging to other worlds, before they had fairly commenced investigations restricted within terrestrial limits. And as astronomy was one of the earliest, so the science of human life and organization was one of the latest pursuits to be cultivated with assiduity and success. This latter fact affords, in part, an explanation of the remarkable inattention to matters relating to public health. The subject in many of its aspects could not be appreciated while the functions of the organism were very imperfectly understood. The importance of the subject, in its various relations, has, of late years, been progressively advancing in the estimation of intelligent, reflecting portions of society, until it has, at length, become, what it has just claims to be considered, one of the leading subjects of the many which concern the welfare of the human family. In England and France it is emphatically *the* important subject of the day. In this country interest in its behalf, commensurate, in some degree, to its importance, is beginning to be felt. Compared, however, with what has been done, and is now being done, in Europe, the medical profession, and the state, with us, are backward in the sanitary movements which may be said to be characteristic of the present time.

The subject is one which, although not belonging exclusively to physicians, and indeed is cultivated with distinguished success by others, yet commends itself in an especial manner to members of the medical profession. The medical profession, as well as other branches of society, are open to censure for its past and present neglect. We have been too much

engrossed in the investigation of diseases with reference to their pathology, discrimination, and treatment, bestowing far too little consideration upon the questions in how far are the sources of diseases within our control, and how can they be arrested by prophylactic measures rendered applicable to all the individuals composing communities.

With these desultory remarks, which open up various and extensive topics of inquiry, we proceed to invite the attention of our readers to the publication, the title page of which is prefixed. Although not relating to practical medicine, or surgery, we need not amplify on the relevancy of the subject of public health to those inquiries which are peculiarly appropriate to the medical reader; nor will an apology be required for devoting to the subject considerable space in a medical Journal.

In July, 1850, three Commissioners were appointed by the Legislature of Massachusetts, to "prepare, and report to the next General Court, a plan for a Sanitary Survey of the State, embracing a statement of such facts and suggestions as they may think proper to illustrate the subject."

The commissioners selected by the Governor of the State, were Lemuel Shattuck, of Boston, Nath. P. Banks, Jr., of Waltham, and Jehiel Abbott, of Westfield. The name of the chairman, Mr. Shattuck, a member of the legal profession,* has long been identified with the prosecution of statistical investigations relating to health and disease, and it is not doing injustice to the other gentlemen of the committee to state, that the labor of collecting the materials, and preparing the present report, devolved upon him. And we would say here, what we think our readers will infer from the imperfect analysis which will follow, that Mr. S. has produced a work not only highly interesting, but in an eminent degree useful, being well calculated to advance the objects for which it is designed, viz., to enforce the importance of the subject of public health, and to inculcate the means by which the sanitary movement in this country is to be advanced.

By a Sanitary *Survey* is meant, "an examination or Survey of the different parts of the commonwealth—its counties, its towns, and its localities—to ascertain the causes which, favorably or unfavorably, affect the health of its inhabitants."

The following paragraph presents a concise expression of the facts upon which are based the benefits to be expected from such a Survey:—

We believe that the conditions of perfect health, either public or personal, are seldom or never attained, though attainable;—that the average length of human life may be very much extended, and its physical power

* Since this was written we learn that we were incorrectly informed as respects the profession of Mr. S. He is, or has been a merchant.

greatly augmented;—that in every year, within this Commonwealth, thousands of lives are lost which might have been saved;—that tens of thousands of cases of sickness occur, which might have been prevented;—that a vast amount of unnecessarily impaired health, and physical debility, exists among those not actually confined by sickness;—that these preventable evils require an enormous expenditure and loss of money, and impose upon the people unnumbered and immeasurable calamities, pecuniary, social, physical, mental, and moral, which might be avoided;—that means exist, within our reach, for their mitigation or removal; and that measures for their prevention will effect infinitely more, than remedies for the cure of disease.

The relative value heretofore attached by Legislators to scientific investigations relating to public health is significantly shown in the facts contained in a passage given in a note, which we quote, with the remark that facts quite as significant, if not much more so, might be cited from the legislative proceedings of the State of New York within the few past years.

The valuable Reports of the Commissions heretofore existing in Massachusetts, are of considerable length. That on *Insects* contains 460 pages; that on *Invertebrata*, 374 pages; that on *Fishes, Reptiles, and Birds*, 416 pages; and that on *Trees and Shrubs*, 547 pages; besides illustrative plates in each. The first of these reports has been ordered to be reprinted this year. It would be reasonable to suppose that MAN was entitled to a consideration equal to either of these subjects.

The first part of the work is devoted to the past history, and the present condition of the Sanitary movement. We cannot condense all the valuable information contained in the one hundred pages occupied with these topics. The historical and other details pertaining to the Sanitary movement in Great Britain are chiefly dwelt upon. The prime actor in this movement in that country was Edwin Chadwick, Esq., barrister-at-law. It was chiefly through the able writings of this gentleman, in 1828, that the attention of Parliament was directed to the subject. The publications of Dr. T. Southwood Smith were also instrumental in bringing about this result. In 1832 was appointed a commission to "inquire into the practical operation of the laws for the relief of the poor in England and Wales;" and in 1833 another, "to consider and report on the general state of parochial registers, and on a general registration of births, baptisms, marriages, deaths and burials, in England and Wales." In consequence of the information contained in the latter report, "an act for the registration of births, marriages, and deaths, in England and Wales," was passed in 1836, and went into operation in 1837, a central office being established in London, presided over by an officer styled the *Registrar-General*. The quarterly reports which have since emanated from the office of the Registrar-General, are of very great value. The author of the Massachusetts Report gives a compi-

lation from the appendix of the ninth annual report of the Registrar-General, to show the rate of mortality among four different populations in England, to wit, 1, the whole of England; 2, one of the most healthy districts; 3, one of the most unhealthy districts; 4, London. Now, a single item of the several points of disparity between these populations, obtained by registration, suffices to illustrate the great importance of vital statistics. In the most healthy districts of England 4.123 per cent. of the males die under five years of age; while in the most unhealthy 14.372 per cent. die in the same age! This is but one point of comparison. Other differences not less striking appear from the tables. Such facts show a preponderance of morbid influences commensurate with the increased ratio of mortality in certain sections. Then the important questions arise, whence this preponderance? what are the causes of disease which occasion in one section a degree of mortality nearly four times greater than in another section? These questions are to be answered by facts developed in the prosecution of Sanitary Surveys; and upon the conclusions thus arrived at, are to be based those measures of sanitary improvement which will diminish the operation of morbid influences, reduce the rate of mortality, and increase all the desirable results flowing from augmented health and prolonged life.

During the last twelve years several valuable publications, in addition to the reports of the Registrar-General, have emanated from the British press. Among those deserving particular mention are the following: statistical reports of Maj. Tulloch on the sickness, mortality, and invaliding among the British troops stationed in different quarters of the globe; statistical reports on the health of the Navy, by Dr. John Wilson; report of a select committee appointed by the house of commons, on the "circumstances affecting the health of the inhabitants of large towns, and populous districts; reports upon the employment of the children of the poorer classes in mines and collieries, etc.; report of the poor law commissioners on the training of pauper children; report on the prevalence of certain physical causes of fever in the metropolis, by Drs. Arnott and Kay; several reports on the physical causes of sickness and mortality to which the poor are exposed, and on the prevalence of fever, by Dr. Southwood Smith; reports on interments in towns, by E. Chadwick, Esq. Several able papers have also appeared in leading periodical reviews. Mr. Shattuck sums up the conclusions to which he is led from an analysis of the documents and works just referred to, in the following propositions:

1. *It is proved* that there die annually, in each 100 of the population, of

the whole of England, 2.27; of the most healthy district, 1.53; and of the most unhealthy district, 3.58. And that the living to one death are, in these districts, respectively, 44, 65, and 27.

2. *It is proved* "that the various forms of epidemic, contagious, and other diseases, caused, or aggravated, or propagated, by atmospheric impurities, produced by decomposing animal or vegetable substances, by damp and filth, and close and over crowded dwellings, prevail amongst the population in every part of the kingdom, whether dwelling in separate houses, in rural villages, in small towns, or in the large towns, as they have been found to prevail in the lowest district of the metropolis."

3. *It is proved* that disease and mortality fall more heavily upon those who live in large towns and populous places, than in the country districts, and particularly upon those who live in narrow streets, confined courts, damp dwellings, close chambers, cellars, undrained, unventilated, and uncleaned; and affect most severely the infantile portion of the population, and the heads of families between twenty and thirty years of age.

4. *It is proved* that, in such situations, the average duration of life is five to twenty-five years less than it might otherwise be; and that, during this curtailed period of existence, the working power of those who live, and their capacity for enjoyment, are greatly diminished by a constant depression of health and spirits, and by the active attacks of fever, cholera, scrofula, and consumption.

5. *It is proved* "that such diseases, wherever their attacks are frequent, are always found in connection with the physical circumstances above specified; and that where these circumstances are removed by drainage, proper cleansing, better ventilation, and other means of diminishing atmospheric impurity, the frequency and intensity of such diseases are abated; and where the removal of the noxious agencies, and other causes of disease, appears to be complete, such diseases almost entirely disappear."

6. *It is proved* that the annual mortality might be reduced, in the whole kingdom, from 2.27 per cent., or 1 in 44, to less than two per cent., or 1 in 50; and in all large towns, as low as that general average.

7. *It is proved* that this unnecessary excess of mortality above 2 per cent., occasions an annual loss of more than 50,000 lives in the United Kingdom,—“greater than the loss from death or wounds in any wars in which the country has been engaged in modern times;” and that the causes of these unnecessary deaths occasion at least twenty cases of unnecessary sickness, on the average, to each death, or one million cases annually, which might have been prevented.

8. *It is proved* that of the 43,000 cases of widowhood, and 112,000 cases of destitute orphanage, relieved from the poor rates of England and Wales alone, the greater proportion of deaths of the heads of families occurred from specified removable causes; and that the average of their ages was under forty-five years, or thirteen years below the natural probability of life, as shown by experience.

9. *It is proved* that the preventable causes of disease, and the unnecessary mortality, impose upon the people immense pecuniary burdens which might be avoided.

10. *It is proved* that the younger population, bred up under noxious physical agencies, is inferior in physical organization and general health to a population preserved from such agencies; and that these adverse circum-

stances tend to produce an adult population, short-lived, improvident, reckless, intemperate, immoral, and with excessive desires for sensual gratifications.

Having given an historical sketch of what has been done in other countries toward the sanitary movement, Mr. Shattuck next enters upon the consideration of this movement at home. He has consulted the past history of the country in order to collect therefrom information respecting public health, in times gone by. Many interesting and curious items are presented under this head, but in number and character they are far from being satisfactory for purposes of scientific investigation. On the meagreness of historical annals with respect to this subject, even in modern days, he thus remarks:

For the last forty years, notwithstanding the mass of medical literature that has been published, less definite information has been obtained concerning epidemics than in the previous periods. The almost entire neglect of records, prior to the adoption of the registration system, renders it difficult to give any thing approximating to an accurate view of the subject. If a careful examination were made into the history of each town, many important facts might be gathered. But it is curious and lamentable to observe, in looking over our published local histories, how little attention has been paid to this matter. *The History of the Health of the People* should be regarded as the most important part of history, yet it has generally been considered unworthy of notice, or, if noticed at all, merely among the incidental matters of little consequence.

The sanitary movement can only be said to have as yet commenced in this country, although, during the short space of time that any attention has been directed to the subject, many valuable contributions have been made. The Transactions of the American Medical Association already contain some interesting and useful papers furnished by Mr. Shattuck and others. It is obvious that the movement cannot advance without a registration of births, marriages, and deaths. Legislation has lately provided for such a registration in Massachusetts, which is now in successful operation. We are not aware that this is the case in any other State, but on this point we do not profess to have accurate information. An act was passed a few years ago providing for such a registration in this State, but from some defects in details it did not succeed, and is now, so far as we know, to all intents inoperative. To secure this object in all the States is a great desideratum. This, indeed, is the initiative in the progress of the sanitary movement—it is the first step. Regarding it in this light the Am. Med. Association at one of its first meetings urged upon the medical societies of the different States to endeavor to enlist the interest of legislators in the subject, and procure effective legal enactments.

To illustrate the importance of the results to be deduced from an examination of the statistics obtained by means of registration reports, Mr. Shattuck gives a statement of the rate of mortality among the inhabitants of Boston for 1830, 1840, and 1845; and of an interior country town in Mass. for 1830. The rate of mortality of different ages from 5 years and under, up to 70 and over, is given in this statement in a tabular form. By a glance at the latter the reader is able to determine the liability to death at different ages, and of all ages collectively, in the different places specified. For all ages the average rate of mortality for the space of nine years was 2.53 per cent. In the towns in the country it was 1.49 per cent. In Boston the mortality under 5 years of age was 9 per cent.; in the country only 3.05 per cent. It is evident from these specimens of the facts thus elicited, that statistics from all parts of the United States would open a wide field for comparisons, possessing great value, as well as interest. Another table compiled by Mr. Shattuck from the registration reports for five consecutive years, shows the influence of the seasons, in Mass., on mortality. The deaths during each month in the five years, for different ages, are given in this statement. The results are that the summer quarter is attended with the most fatality; next the autumnal; next winter, and last the spring. August and September are the most unhealthy months in Boston, and October the most unhealthy month in the country. The fatal diseases during these months in the city affect the digestive organs, and in the country the occasion of the greatest fatality in October is the prevalence of fevers in this month.

Statistics from registration reports are adduced to exhibit the influences on health and longevity derived from *occupation*, and domestic condition. From these it appears that *farmers* are the longest lived; next to these, *hatters*; next, *coopers*; next, *clergymen*; next, *lawyers*; next, *physicians*, etc. *Stone cutters*, *shoemakers*, *painters*, and *laborers* are low in the scale, but mechanics and printers are the shortest lived. The average length of married life is found to be, of men, 28.90 years, and of women, 22.16. The average length of the period of widowhood is, of men, 19.33, and of women, 28.90 years. The average age at which men marry, for the first time, in Mass., is 25.71 years; and women, 26.61 years.

To illustrate the influence of disease, which is the most important test of the sanitary condition of a State, the author presents a table containing the number of deaths in Boston by each known cause from 1811 to 1849 inclusive, a period of 39 years. This table furnishes the requisite data for judging of the comparative prevalence of different diseases at Boston during

the time mentioned. The table also contains the number of deaths from known diseases in the remainder of the State, i. e. exclusive of Boston, during the last seven years, the period covered by the registration reports provided for by the laws of Massachusetts.

The fatality from diseases classed under the denomination *zymotic*, i. e. those which are due to epidemic, endemic or contagious causes, as appears from this table, has been constantly increasing during the last 36 years. As these diseases constitute the great index of public health, this fact shows that the sanitary condition has been progressively deteriorating. Of the individual zymotic diseases, dysentery, cholera infantum, scarlatina, and small pox are those which have especially increased. Deaths from small pox have increased from 0.8 to 1.34 per cent. In the country 9.09 per cent. of all the deaths are from Continued fever. Deaths from hydrocephalus have nearly doubled in Boston within the last 30 years. The results of an analysis of the latter with reference to the effects of season, age, and sex, upon pulmonary consumption, are stated, which we must omit. Finally, the author concludes this division of the subject by the enunciation of several conclusions which we copy:—

1. *It is proved* that there is a great difference, in this State, in the longevity of people living in different places and under different circumstances.

2. *It is proved* that causes exist in Massachusetts, as in England, to produce premature and preventable deaths, and hence unnecessary and preventable sickness; and that these causes are active in all the agricultural towns, but press most heavily upon cities and populous villages.

3. *It is proved* that measures—legislative, social and personal—do not at present exist, or are not so fully applied, as they might be, by the people, for the prevention, mitigation, or removal, of the causes of disease and death.

4. *It is proved* that the people of this State are constantly liable to typhus, cholera, dysentery, scarlatina, small-pox, and the other great epidemics; and to consumption, and the other fatal diseases, which destroy so many of the human race in other parts of the world.

5. *It is fully proved* that the active causes of disease and death are increasing among us, and that the average duration of life is not as great now as it was forty or fifty years ago.

We are fully aware that the general opinion does not coincide with this fact, and that a directly opposite one has been expressed. It has been frequently said, that, owing to the different modes of living, the increased medical skill, and other causes, diseases have been ameliorated, and the average length of human life has been extended; and particularly within the last fifty years. We have long thought differently, especially in regard to the more recent periods of our history. Those who make this assertion seem to rely upon imperfect or uncertain data to support their opinion. Statistical observations of the living and the dead, gathered in ancient times, should be taken with great caution as comparative tests. Ten years since,

it was said that "the average value of life is not as great as it was twenty years ago; that it was at its maximum in 1810 to 1820; and that it has since decreased."* Subsequent investigations have fully established the correctness of this statement.

It is undoubtedly true, that in many things society has improved; that medical skill in the cure of disease has greatly increased; and that some diseases are not as fatal as formerly, or are now better understood and controlled. But while all this may be true, it is no less true that the active causes of disease have increased faster than the appliances for their prevention and cure; that new diseases, or old ones in a new and modified form, equally fatal and uncontrollable, have appeared; and that sickness and death advance more rapidly than the improvements devised to arrest them.

These statements, concerning the decreasing vital energies of man, are confirmed by recent investigations in England.

In the third division of the Report Mr. Shattuck presents a *plan for a Sanitary Survey of the State*. The plan embraces measures to be regulated and controlled, *first* by the legislative authority of the State, and, *second*, by social organization and personal action. The several measures proposed are stated in a series of articles each of which is considered by Mr. S. at more or less length. This portion of the report covers an hundred and thirty-seven pages. Our limits will only allow us to present a list of the more important measures, omitting the considerations which the author offers respecting their applicability, importance, and the means by which they are to be enforced. As the reader may imagine, we are thus obliged to forego any account of much valuable information concerning the various topics involved in the subject of public health. We shall give the more important recommendations in succession, but without the division into separate articles which is adopted in the Report.

A revision of the laws of the State relating to Public health is advised, and a new act to be passed in their stead.

The general execution of the laws of the State relating to the enumeration, the vital statistics, and the public health of the inhabitants, should be entrusted to a **GENERAL BOARD OF HEALTH**.

This board should be composed of two physicians, one counsellor at law, one chemist or natural philosopher, one civil engineer, and two persons of other professions or occupations; all properly qualified for the office by their talents, their education, their experience, and their wisdom.

A secretary, to be appointed by the Board, should be required to devote

* American Journal of Medicine for 1841, Vol. 1, p. 382.

his whole time and energies to the discharge of the duties of his office, and secure a proper salary for his services. A LOCAL BOARD OF HEALTH, should be appointed in every city and town, charged with the particular execution of the laws of the State, and the municipal ordinances and regulations relating to public health, within their respective jurisdictions; and these local boards should appoint a secretary, and, if occasion require, a surveyor and health officer.

The local boards should endeavor to ascertain, with as much correctness as possible, the circumstances of the cities and towns, and of the inhabitants under their jurisdiction; and issue such local sanitary orders, and make such regulations as are best adapted to their circumstances, resorting to compulsory process only when the public good requires it.

Appropriations should be annually made by the State for the purchase of books for the use of the General board of health; and by each city and town for the purchase of books for the use of each local board of health.

Each local board of health should be required to make a written report annually to the town, concerning its sanitary condition, during the next preceding year, and to transmit a written or printed copy to the next general board of health.

Successive enumerations of the inhabitants of the State should be so made, abstracted, and published, that the most useful and desirable information concerning the population may be ascertained.

The laws relating to the public registration of births marriages and deaths, should be perfected, and carried into effect in every city and town of the State.

Provisions should be made for obtaining observations of the atmospheric phenomena, on a systematic and uniform plan, at different stations in the commonwealth.

An uniform nomenclature for the causes of death, and for the causes of disease, should be used in all sanitary investigations and regulations.

In laying out new towns and villages, and in extending those already laid out, ample provision should be made for a supply, in purity and abundance, of light, air, and water; for drainage and sewerage, for paving, and for cleanliness.

In erecting school houses, churches, and other public buildings, health should be regarded in their site, heating apparatus, and ventilation; and before erecting any new dwelling house, manufactory, or other building, for personal accommodation, either as lodging house, or place of business, the owner or builder should be required to give notice to the local board of

health of his intention, and of the sanitary arrangements he proposes to adopt. The local boards of health, also, should endeavor to prevent or mitigate the sanitary evils arising from overcrowded lodging houses and cellar dwellings.

Open spaces should be reserved in cities and villages for public walks; wide streets should be laid out, and both should be ornamented with trees. Special sanitary surveys of particular cities, towns, and localities, to be made, from time to time, under the direction of the General Board of Health.

Local boards of health, and other persons interested, should endeavor to ascertain, by exact observation, the effect of mill-ponds, and other collections and steams of water, and of their rise and fall, upon the health of the neighboring inhabitants.

The local boards should provide for periodical house-to-house visitation, for the prevention of epidemic diseases, and for other sanitary purposes. The importance of this measure in epidemic cholera is exemplified in a striking manner by the following results of a general order for its introduction into London in 1849, for the first 52 days after its adoption:—

Diarrhoea, cases discovered,	43,127.
Rice water purging discovered,	976.
Cholera discovered,	779.
Passed into cholera after treatment,	52.

The author adds, "had it not been for these visitations very many more of these cases would have terminated in cholera and death. What facts can more forcibly illustrate the utility of preventive measures."

Measures should be taken to ascertain the amount of sickness suffered in different localities; and among persons of different classes, professions, and occupations.

Measures should be taken to ascertain the amount of sickness suffered, among the scholars who attend the public schools and other seminaries of learning in the commonwealth.

Every city and town should be required to provide means for the periodical vaccination of the inhabitants.

The causes of consumption and the circumstances under which it occurs, should be made the subject of particular observation and investigation.

Measures should be taken to prevent or mitigate the sanitary evils arising from the use of intoxicating drinks, and from haunts of dissipation.

The General management of cemeteries and other places of burial, and of the interment of the dead, should be regulated by the local boards of health.

Measures should be adopted for preventing or mitigating the sanitary evils arising from foreign emigration.

The foregoing measures are those to be adopted under state and municipal authority. The following are to be carried into effect without any special legislative authority, and are therefore distinguished as social and personal measures:—

A sanitary association should be formed in every city and town for the purpose of collecting and diffusing information relating to public and personal health.

Tenements for the better accommodation of the poor should be erected in cities and villages.

Public bathing houses and work houses should be established in all cities and villages.

The refuse and sewerage of cities and towns should be collected and applied to purposes of agriculture; and the smoke nuisance should be prevented as far as practicable.

The sanitary effects of patent medicines and other nostrums and secret remedies should be observed; and medical compounds advertised for sale be avoided unless the material of which they are composed be known, or unless manufactured and sold by a person of known honesty and integrity.

The local boards of health and others interested should endeavor to prevent the sale and use of unwholesome, spurious, and adulterated articles, dangerous to public health, designed for food, drink, or medicine.

Institutions should be formed to educate and qualify females to be nurses of the sick.

Persons should be specially educated in sanitary science, as preventive advisers, as well as curative advisers.

Physicians should keep records of cases professionally attended.

Clergymen of all religious denominations should make public health the subject of one or more discourses annually before their congregations.

Having submitted the various measures involved in a complete sanitary reform, Mr. Shattuck next considers the reasons for approving of the plan recommended, and meets several objections to it which, it may be anticipated, some of his readers may offer. He then gives a bill for enactment by the legislature, embodying provisions for carrying the plan into execution.

This concludes the Report proper, embracing 321 pages. The publication, however, contains an appendix of about 225 pages. The latter contains all the acts relating to public health that have been passed by the state of Massachusetts; sanitary surveys of several towns in the State, and

several miscellaneous papers relating to the subject of health, all of which possess importance and interest.

We have devoted considerable space to an analytical notice of this work, but not more than is due to its merits, as well as to the subject of which it treats. In our view it is the most valuable publication which has, for some time past, emanated from the American press. The medical profession, and the public, are under no small degree of obligation to Mr. Shattuck for his labors in behalf of public health, and we sincerely hope that the time is not far distant when he will be repaid by the satisfaction of seeing his important recommendations adopted, not only in Massachusetts, but in every State of the Union.

To effect an end so desirable, medical practitioners must become more generally imbued with a sense of the practical benefits which are to flow from its attainment. It belongs to them to give direction and impetus to public opinion as the basis of intelligent and efficient legislation on the subject. There are two points of view in which the sanitary movement appeals most forcibly to the physician for support and encouragement. *First*, it proposes to increase the sum of life and health. We need not look farther than this, and inquire in how far national health, and individual happiness, are to be enhanced by such a result. The object is sufficient in itself. More health and a longer average of life are attainable—this is enough for the physician, for these are what he chiefly aims at in the discharge of his professional duties. He strives to increase the sum of life and health by curing diseases. Should he not then sympathize in efforts to secure the same humane purposes by measures calculated to develop the ability to resist and surround disease, and by warding off the causes of disease? As we have already remarked, the latter have, undoubtedly, received a share of attention from medical philosophers disproportionate to their relative consequence. The grand objects have been to mitigate diseases after their development: to study their pathological characters, the methods of discriminating them, and their therapeutical indications. These are subjects of vast importance, but yet of not greater importance to the welfare of society than the prevention of disease. And here we may cite a fact with respect to the latter which will serve to enforce its comparative claims: The progress of the scientific investigation of diseases is not attended by corresponding success in their therapeutical management. This fact must be admitted. We do not mean to assert that the therapeutical branch of medical science has not advanced, and is not constantly advancing. Far

from this. But it will not be denied that discoveries in this branch have been less brilliant than in morbid anatomy, pathology, and diagnosis. This fact should serve to turn our attention to sanitary inquiries, to ascertain if, in that way, we cannot realize more fully the ultimate aim of all medical science—to wit—the increase of life and health. Moreover, the progress of our knowledge of diseases teaches us that we cannot expect to cure them, in proportion as we succeed in their investigation. The lesson is, indeed, sometimes quite the reverse of this, viz., that if we expect to diminish the mortality from some diseases which are the most frequent in their occurrence, as well as the most fatal in their character, we must ascertain, and subject to our control their causes—in other words the reliance is on prevention rather than cure. Tuberculosis may be mentioned as a striking illustration of the truth of this remark.

This leads us to the *second* point of view in which the sanitary movement appeals to the physician. It proposes means of ascertaining and controlling the causes of disease—it aims at prevention. As respects etiology, the statistical facts developed by registration reports seem to us to be of very great importance. By determining the value of life as affected by differences of residence, occupation, etc., we are enabled to appreciate the different degrees of morbid agency due to these extrinsic, and, in a measure, controllable circumstances; and by establishing relations between these circumstances and the prevalence of particular diseases, the sources whence the latter originate are made apparent. Suppose, for an example, that we discover a particular occupation or locality to be more prejudicial to life and health than other localities and occupations, the inquiry at once arises, what are the morbid agencies incident to this occupation or locality which account for such a disparity, and how do they operate upon the organism? Investigation follows this inquiry, and is thus excited and directed by the conclusions deduced from statistics. Again, we discover from the registration tables that certain diseases prevail among a certain class, in certain sections, or in certain seasons; we are then led to seek for the etiological causes therein involved. Much information has already been evolved in this way, but much more remains to be developed as the accumulation of data increases, in consequence not only of a longer period for their collection, but of a more general adoption of the system. In thus attaching great value to sanitary statistics we do not overlook the fact that we are liable to error from this source, and that deductions are to be made with great care and circumspection. Figures are by no means infallible. They constitute but a means of disclosing truth, and, like all other potent

Abstracts are to be managed with caution and skill. In their application to the study of books, as well as disease, they are a lifeline in themselves adequate to compass the objects desired, but they go in the way, and point in the right direction. Statistical results do not constitute the final objects of our pursuit, but they furnish the *bee lines* which direct our steps in the way of successful investigation.

ECLECTIC DEPARTMENT.

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS

On the Treatment of Scarlatina by Inunction. By Dr. SENESEMAN.

[In Vol. IX, p 14, of our "Abstract," we briefly mentioned this treatment on the authority of Mauthner, a physician of high repute at Vienna. We now present our readers with a more detailed account, promising, that however painful this treatment may appear, *prima facie*, its harmlessness may entitle it to a trial where a more hazardous proposition might reasonably be rejected. The following account is abridged from the "Lancet," Sept. 15, 1849.]

Description of the Treatment.—As soon as we are certain as to the nature of the illness, the patient must be rubbed, every morning and evening, over the whole body, with a piece of bacon, in such a manner that, with the exception of the face (?) and hairy scalp, a covering of fat is everywhere applied. In order to make this rubbing in somewhat easier, it is best to take a piece of the size of the hand, choosing a part still armed with the tuck, that we may have a firmer grasp. On the soft side of this piece, cuts are to be made in various directions, in order to allow the seeping out of the fat; and this is still further promoted by placing the tuck for some time previously to using it, near the stove, in the oven, or in the bath. But the fat must be allowed to cool before being used.

The rubbing must be so performed, that the skin may be regularly, but not too quickly, saturated with fat. During the process, only that part being rubbed is to be uncovered, or the whole can be done under the bed-clothes; but this precaution is unnecessary. Although this is true from the mess it makes, is not calculated to find favor, as it dirties bed and linen, as well as the persons of the children, yet the first few days yield results which make all this forgotten, and inspire the mothers with enthusiasm. With rapidity the most painful symptoms of the disease are allayed; quiet, sleep, appetite, and good humor return. Other things, however, are necessary besides inunction with fat; but still the most important share of the merit may be imputed to this treatment. The truth of this will appear from a citation of the results which follow:

1. The improbability, I might say impossibility, of the patient getting cold. Were this the sole benefit of the inunction, it would be great.
2. The dry brittleness of the skin, and the tormenting itching, are not

only materially alleviated, but, for the most part, fully put a stop to. Hence children generally like the rubbing-in, and often ask for a repetition of it before the time is come.

3. The influence on the physiological functions of the skin is still more important. During the coming on of scarlet fever, the skin becomes diseased, in consequence of which it dies off; and, until a new covering is prepared for the surface, the functions of the skin are ill performed, or, during the desquamation, probably not performed at all. To appreciate the importance of the imperceptible functions of the skin, merely mechanically viewing the matter, I refer to the experiments of Seguin, which fix the quantity of matter thrown off from the outer skin at eleven grains per minute in a grown person, and therefore more than two pounds per day. What efforts it must cost the organism to lead so large a quantity into other paths, in order to throw it off when the skin is incapable of doing so! To give a striking proof of the bad influence which the interrupted functions of the skin produce on the healthy activity of relative, even of distant organs, I may cite the fact, that death is always the result where more than one-half of the skin has been destroyed by fire or boiling liquid. A similar destruction of the skin ensues in scarlet fever, with this difference, that it takes place gradually, and thereby the organism is better enabled, by employing all the activity of the body, to find aid against the mischief which, to the injury of the patient, must result from the cessation of the functions of the skin.

4. The oxidation of the blood is thus considerably promoted, the interrupting of which is the cause of such serious phenomena. As the disease of the throat is not improbably, in great part, due to the interruption of the function of the skin and lungs, it must naturally disappear, or not present itself, where these are continued in integrity: and such is found, in practice, to be the case.

5. Owing to the fatty covering, the skin is kept moist, and the cuticle cannot be driven about the room by currents of air; and thus *one fertile source of infection is kept closed up*, it being well known that infection is most easily communicated at the period of desquamation. The danger of infection, under any circumstances, is materially lessened with the disappearance of the eruption from the skin, inasmuch as the process of generating infectious material is interrupted by restoring the skin to its normal state.

6. By shortening the period of desquamation, and protecting against the sequela of the disease, the duration of the treatment can be shortened to a period of from six to ten days.

7. The remedy is (cheap,) harmless, practical, and is perhaps never counter-indicated. The linen must not be too frequently changed, as a clean shirt takes up more of the fatty matter than one already saturated, and hence the skin is sooner deprived of its fatty covering. The rubbing-in is to be kept up twice a day for three weeks, and once a day during the fourth. The patient is, after this, to be daily washed with cool water and soap, and then only is the warm-bath to be commenced. This process does not interfere with the natural course of the malady, and expose the individual to second and third attacks. In severe cases, the remedy may be repeated three or four times within the twenty-four hours. The main point is, to keep the skin *always* cool and moist; and here, even with all

possible precaution, the skin will sometimes come off in certain places. The practitioner will do well to fix the exact hours at which the rubbing-in is to take place; it will then most probably be better and more regularly performed.

Other points, which are also important enough, now remain to be noticed:

8. *Temperature.*—Experience proves that it requires no great daring to keep the patient *cold* instead of hot. Cold washing is *not* to be employed, as it promotes desquamation. Cool air seems to have a bracing influence on the system, and a soothing one on the respiration; and all danger is avoided by the fatty covering. The temperature of the bedroom should never be above 13 deg. R. The idea of throwing back the eruption by mere cold air is an error. Great heat is much more likely than cold to throw in the eruption. In fact, the fatal cases of this kind are principally those where, through keeping the patient too hot, the cerebral affection has been brought on; this has given rise to paralysis, which appears sooner in the skin than in other parts, and thus to the withdrawal of the eruption,—for the skin dies sooner than other parts, as shown in collapse, where mustard poultices do not act as in other states.

9. *Bed.*—The patient should not remain in bed any longer than is absolutely necessary. As soon as the fever, headache, and a desire to remain in bed are gone, he may quit it, for in bed the skin is between two fires,* and the functions of the body do not go on as well as in moving about. Hence, even when the patient must lie down an hour or two daily, he should still go about the rest of the time.

10. *Diet.*—The diet should be light, but there must be no starvation, and as rapid a return as possible to the usual food.

11. *Washing.*—Although it brings on desquamation, it will be as well to let the patient occasionally wash his hands and face with water and soap. It reconciles him to the dirt attendant on the rubbing-in.

12. If *constipation* ensue, it must only be combated with medicines; when, at the end of forty-eight hours, it makes no semblance of disappearance, then a clyster of poppy oil is the best remedy.

The author enters his protest against a partial employment of his remedies.

Complications require a modification of the system.—1. *Severe cerebral symptoms at the commencement.*—The above treatment can only be applied, where time is allowed for the development of the restorative process. Occasionally the case is accompanied at its very outset by severe cerebral affection, and convulsions. Here bleeding may be employed, and, if necessary, unhesitatingly repeated. To support this view, Dr. Schneeman instances the authority of Armstrong, Bernd, Stieglitz, Hammond, Hings-ton, &c. Venesection is, accordingly, the sheet anchor. The other remedies are:—The application of concentrated cold to the head; and the best form for this is ice. The cold dash is often more hurtful than useful, on account of the serious reaction which follows it, and exposes the patient more to the dangers of an apoplectic attack, although its results for the

* Here it will be as well to state, that in Germany a feather bed is frequently substituted for bed clothes.

minute are often very flattering. *At the end of every two hours the bladder of ice should be removed*, in order that the uninterrupted effect of the cold may not weaken too much the tone of the vessels of the brain. Warm mustard plasters to the shins are a most valuable remedy. Internal remedies are generally of little use where the above given remedies fail; the only one of any importance is the carbonate of ammonia. Mercury is of little value, except just to open the bowels; for its specific action never comes into full play till the system is throwing off the affection. There are, however, much better purgatives than calomel; the saline, for instance. Emetics ought not to be tried in cases complicated with cerebral affection: in others they may. The aconite failed in the author's hands, both in tincture and solution. With regard to the treatment by leeches and ammonia, so many writers have already pointed out its good results, that the author can safely recommend it, but with the proviso that in urgent cases bleeding be substituted for leeches.

2. *The Affection of the Throat.—Primary.* As this is but a link in the whole, so must the measures taken for its removal be such as will remedy the general affection. *Secondary.* For this, the rubbing-in, as it acts by prophylaxis, is the best possible remedy; but where this has not been brought into use, or where, from keeping the patient too warm, desquamation has come on, and the secondary sore-throat has set in, the best remedy is the use of emetics. They not only remove the tough glazy slime, but excite the secretions and excretions to more normal disposition, and this is especially the case if the disorder have a gastric character. Many, by confounding the employment of emetics in the early and latter stages, have brought them into discredit. For the swelling of the tonsils, an excellent remedy is a solution of nitrate of silver, (twenty grains to an ounce of water,) with which they and the soft palate are to be painted; but so many varieties present themselves in these secondary attacks following on scarlet fever, that no general rules can be laid down. Here everything depends on the discrimination and judgment of the practitioner.

Prophylaxis.—Warm clothing, separation from school, and, above all, light diet, are the best preventives. That the younger children, as more predisposed than the others, must be more carefully separated from the sphere of infection, is doubtful practice. Not only are many children never infected, who, however, by adopting this system, require to be as carefully secluded as the others, thus causing great inconvenience; *but the milder influence and actual advantage of a gradual accustoming to the infection are thereby lost, and consequently the attack, when it comes, is so much the more severe.* The free communication with the patient is good, in order that constant exposure may, as in the case of physicians and nurses, blunt and wear out the disposition to the disease; for as it now is, children are separated from the patient during the first period, which is the mildest, and exposed to contact with him during the process of desquamation, which is the source of the most dangerous infection. Many preservatives have been vaunted against this malady during its epidemic appearance; of these, however, the only one apparently deserving of much credit is the belladonna. Its action appears to be, "by altering the relations of the nervous system, to diminish the disposition to the disease." The author's prescription is—Take of extract of belladonna one to two grains; distilled water, one ounce. *Mix.* Give to each child, morning

and evening, as many drops as it has years, and continue to do so at least fourteen days. But not to confine himself to his own testimony, Dr. Schneemann gives that of Jordens, Ettmuller, Hedenus, Gumpert, Hufeland, Martius, Pormey, Behr, Benedix, Thaer, &c., who have testified to the value of belladonna; and though he admits that as many opponents might be found, he thinks it scarcely credible that all the former had deceived themselves. In certain sections of the circle of Bayeux, Dr. Feron preserved from scarlet fever all the children who had not been attacked before he commenced operations. In 400 cases near Valenciennes, treated with belladonna, not one person was attacked.—(*Vide* REPORT.)—*Half Yearly Abstract of the Medical Sciences.*

On the Treatment of Obesity. (Corpulence, or Excess of Fat in the Human Body, etc.) By Dr. T. K. CHAMBERS.

[The subject of corpulence has not met with such consideration as it deserves, we, therefore, have great satisfaction in being able to lay before our readers an extract from a series of papers in which the light of modern science has been most admirably concentrated upon it. Obesity, though too often the subject only of witticism, is in many cases a real evil; and the practitioner will, therefore, gladly avail himself of any rational mode of obviating its inconvenience. In regard to treatment, Dr. Chambers observes:]

That form of disease which commences at birth, and goes on increasing during infancy and childhood, is, I believe, so invariably fatal before the age of puberty, that I do not think we have reason for hoping that it is in any way amenable to medicine. At all events, I have not been able to discover any one whose experience has led him to pronounce it curable. It is a form of monstrosity; and as the subjects of it commonly display some other bodily malformation, and a deficiency of intellect, their death is a relief from a miserable prospect.

When it begins in childhood, or about the time of puberty, we must not be deterred by the circumstance of its being hereditary from attempting to remedy the inconveniences arising from it. We cannot truly reduce our patients entirely to the average size and weight, but we may enable them to pass life in comfort and usefulness.

The later the disease commences, the more controllable it is by management, until the middle period of life is passed, and then old age impedes in some degree the benefit which we may confer, not by rendering our measures inert, but by preventing our employing them quite so actively as we should have done earlier.

The first thing indicated, in all cases, is to cut off, as far as possible, the supply of material. Fat, oil, butter, should be rigorously interdicted in the diet table. But all eatables contain some portion of oleaginous matter, and especially those most convenient to advise the use of for a lengthened period. And almost all are capable of a transformation into fat, when a small quantity of this substance is previously present. It is desirable, therefore, that the mass of food should lie in the stomach as short a time as possible, in order that at least a fatty fermentation may not be set up in it. Very

light meals should be taken at times most favorable to rapid digestion, and should consist of substances easy of solution and assimilation. To this end, the time of the meals should be fixed for an early hour in the day, before exertion has rendered the powers of the entrails languid and weak. Breakfast should consist of dry toast, or what is still better, sea-biscuit; and if much active exercise is intended, a small piece of lean meat. Dinner at one, on meat with the fat cut off, stale bread or biscuit, and some plain-boiled maccaroni, or biscuit-pudding, by way of second course.

Liquids should be taken, not at the meal, but half an hour after, so as not to impede the action of the gastric juice upon the mass. Here should end the solid feeding for the day; no second dinner or supper should follow, nor, indeed, any more meals be taken sitting down. A piece of biscuit and a glass of water can be taken standing up, if faintness is experienced; a cup of gruel or roast apple before going to bed.

This is not a scale of diet by any means unattainable. A butcher and retired pugilist has adopted it for some years with the greatest comfort to himself. He is able, upon it, to work in the most violent manner in a small garden which he cultivates for himself in the suburbs. He has reduced himself from 20 to 17 stone; whereas his brother, who has not the same strength of mind, has increased to 23 stone in weight. Persons of more refined education ought, and often do, practise the same self-imposed restraint more easily. J. R.— has reduced himself from 22 to 18 stone, and sometimes brings himself down to 17, but finds that he derives no particular advantage from being of the lower weight.

The smallest amount of nutriment consistent with the health of the individual can be found by experiment only; but we need not fear that ten ounces of solid food a day is too little, for the last-mentioned gentleman confined himself for a long period to that quantity, and found his mental and bodily powers always equal to the strain which the pursuit of a laborious profession in London demands. It may be remarked, by the way, that it is often advisable to add a small allowance of malt liquor at dinner, as otherwise the craving of the appetite is less easily appeased. The beers to be avoided are of course the thick, sweet kinds, but that which is thoroughly fermented, at a low temperature, in the Bavarian way, seems to contain very little injurious matter.

I do not know that any advice concerning sleep is peculiarly applicable to obese persons, beyond what we should recommend to all classes of men. A draught of morning dew, "*nocturni roris auram ante solis ortum bibendam*," which Aurelian prescribes for the corpulent, is equally beneficial to every one. They are usually uneasy sleepers, and though lethargic, by no means averse to early rising.

In cases where the fat is largely accumulated in the omentum, it is very convenient for the patient to wear a band round the abdomen, which may be tightened gradually. The support thus given to the abdominal muscles relieves the dragging sensation in the loins, which many persons, whose viscera are heavy in proportion to their strength, experience. It enables exercise to be taken with more facility, and appears also by pressure, to afford some assistance to the absorption of fat.

The above remarks will apply equally to all forms of obesity; the abstinence recommended can be borne even by the aged, and only comfort be experienced.

As respects exercise, however, a distinction requires to be made. The young and vigorous, whose obesity does not prevent the use of their legs, cannot employ them more usefully than in walking as long as they are able. The greater number of hours per day that can be devoted to this exercise, the quicker will be the diminution of bulk. But as riding, by the gentle shaking of the abdomen, excites the secretions of the digestive organs more, it should, where practicable, be employed in addition. Where freedom of motion has once been gained, rowing, shooting, any or all of the forms of British gymnastics, should be adopted as regular habits.

But in the asthenic form of the disease, especially in elderly people, this is scarcely practicable. The defect in muscular power prevents the use of the limbs in walking for a long time enough to be advantageous. But where riding can be managed, it should on no account be omitted, and the suspensory belt before mentioned, is often a valuable auxiliary to the employment of this exercise.

The ancients were much more in the habit than we are of using various forms of friction to the skin in treating chronic complaints; and we find in Aurelian a recommendation to the corpulent to employ dry rubbing, either with cloths alone, or with the addition of various powders. Modern habits of cleanliness supersede, in some degree, these remedies. But the skin is not unfrequently greasy from a thick sebaceous secretion, and the circulation through it languid in asthenic obesity, and in these cases horsehair gloves may be used with great advantage. Dr. Flemyng strongly advises friction to be employed to the trunk of the body as promoting absorption and invigorating the surface. The Greek additions of cold bathing or sponging, especially with sea-water, the vapor or hot-air bath, followed by rubbing with salt or with sand, and many other modifications of the same principle enumerated by Aurelian, will naturally suggest themselves to every intelligent patient. The same author very sensibly advises these remedial measures to be employed fasting, and no food to be taken for some time afterwards, and modern habits render before breakfast a convenient time. To these rules of management, medicines, strictly so called, must be viewed as secondary and auxiliary. Unless these laws are obeyed, pharmacopœias are useless.

Purgatives I have generally found not needed in the plethoric form; the bowels usually act once or twice in the day. But in the asthenic obesity of *old people*, where the abdominal walls are weakened by long pressure of an unnatural weight, it is necessary to employ them.

But there is one class of medicines so universally applicable to *all* cases of obesity, that I think a trial of them should never be omitted. The chemical affinity of alkalies for fat, point them out as appropriate alteratives in this complaint, and experience proves that they are suitable to the state of the digestive organs. The most eligible one is liquor potassæ, and it may be administered in much larger quantities than any other. If given in milk-and-water, we may safely commence with half a drachm and raise the dose to a drachm and a drachm and a half three times a day. The milk covers the taste of the potash better than any other vehicle. It has truly the advantage of saponifying a portion of the remedy, but there is no evidence to prove that its efficacy is thereby endangered; indeed soap itself has been strongly recommended. A physician, whose case is recorded by Dr. Flemyng, reduced himself, by alicant soap alone, two stones in weight.

I have often given the above-mentioned doses of liquor potassæ (even to children in cases of scrofula and consumption) without any harm arising from its use, when taken, as desired, in milk. The fear of alkaline medicines has probably arisen from the injury observed by Huxham to follow the use of Mrs. Stephen's saponaceous mixture, at one time so popular, and therefore often misapplied. The injury appears to have originated from their having been employed in improper cases, such as debilitated gouty subjects, chronic stone in the bladder, and the like, to which, of course, much harm would be done.

A poor woman who sold eggs in Chelsea, was becoming quite unable to gain her livelihood by her ordinary occupation. I have not kept a note of her weight and height, and therefore she is not mentioned in the table of cases, but she was extremely obese, and the cause of a variety of symptoms she complained of seemed traceable entirely to the accumulation of fat. By taking liquor potassæ only, without change of diet, she was reduced so far as to carry on her trade with comfort.

Another case was communicated to me the other day, of a gentleman who weighed 19 stone and 7 lbs. By regimen, exercise, and liquor potassæ, he was reduced two stone and a half in six weeks.

I have mentioned bleeding, and perhaps that may cause some surprise, after the observations which have been made on the state of the circulation in fat people. But where distinct signs of plethora are present—such as pain over the eyebrows, beating of the temples, restless sleep by night, lethargy by day, with full lips and an elastic skin—it is capable of being employed with safety; and where it is employed, the advantage derived at the commencement of a course of treatment is very great, for it gives all the other remedies a fair start; and by affording immediate relief to many symptoms, gives the patient a favorable opinion of the plan he has undertaken.

On the other hand, it is scarcely necessary to say, that much risk attends the loss of blood; for if the heart has become atrophied and weak, it will not stand the shock. Venesection may cause either sudden death, from failure of the heart's action, or effusion of blood in the brain, from disturbance to the circulation.

Bitter tonics are often of great advantage in enabling the stomach to digest more easily and rapidly, and therefore to be contented with a smaller quantity of really nourishing food. The increase of appetite which they cause does no harm; for when patients are getting better, they are usually more obedient to their medical man, and can be taught to control it. Gratitude for the benefit they have received, makes them glad to follow advice, however hard.

Some medicines must now be mentioned, which have been recommended for the cure of obesity, but which analogy and experience do not approve.

Vinegar has been employed by those who are foolish enough to practise upon themselves; but as it produces thinness only by injuring the digestive organs, the benefit is not worth the price paid for it, and no medical man would ever advise the use of such a remedy.

Iodine has been spoken of as likely to do good, from the power it exhibits of stimulating the absorbents in cases of scrofula and tumors. But its moderate use certainly does not cause the disappearance of healthy fat. Indeed it has been noticed by Lugol, and is matter of daily observation at

our metropolitan hospitals, that patients frequently acquire a considerable degree of embonpoint during the time they are taking iodine. The cases of tumors and of fats are very distinct. As Dr. Pereira remarks, "the enlargements which these agents (mercury and iodine) remove, are not mere hypertrophies; their structure is morbid, and they must in consequence have been induced by a change in the quality of the vital activity; in other words, by morbid action. Medicines, therefore, which remove these abnormal conditions, can only do so by restoring healthy action." But the action which causes the deposition of fat in the adipose tissue is, though excessive, of a healthy nature, and harm, rather than benefit, is to be expected from the medicine under discussion; that harm which always accrues from a valuable remedy wrongly employed.—*Half Yearly Abstract of the Medical Sciences.*

Acetate of Lead for Granular Lids.—Dr. Cunier, of Brussels, has recently published the successful results of the practice of Dr. Buys in granular lid. His treatment consisted in the application of the acetate of lead to the palpebral conjunctiva, instead of the nitrate of silver commonly used. Dr. Buys takes neutral acetate of lead, reduced to an impalpable powder, dips a moistened pencil into it, and takes up about a grain, or a grain and a half of the salt, which is enough for one lid. When the lid is touched, it should be kept everted until the tears have dissolved the acetate, and those portions of the salt which escape solution should be taken off with the pencil. On the upper lid the tears will be insufficient to dissolve the salt, and moisture must be applied to it from the angles of the eye. As soon as the acetate is dissolved by the tears, and has penetrated the tissues, the latter are noticed to contract powerfully, and a very remarkable phenomenon takes place; for the granulations, and the grooves between them, disappear at once, if they be of a moderate size, and the conjunctiva looks smooth and uniform as long as it remains exposed to the air; but the shining, white color produced by the lead is perceived only when the lid comes again into contact with the globe of the eye. When the granulations and furrows between them are large, two or three applications are necessary to obtain smoothness; one, however, is sufficient when they are not of great size. These applications produce more burning and striction than pain, and when the lid is replaced, hot and whitish tears escape, the flow of which may last from a few seconds to two or three minutes; it rarely lasts longer, and the eye can then be kept open. When the granulations are of an indolent character, the slight injection of the ocular conjunctiva, which occurs on the application of the lead, goes off rapidly; if, on the other hand, the cornea be vascular, the vessels become smaller soon after the cauterization of the lids. Slight œdema of the palpebræ may follow, but in twenty-four or forty-eight hours it wholly disappears; a dry compress rubbed with camphor will hasten resolution, and likewise lessen the burning sensation, if the latter happen to persist. One lid only should be touched at a time, and not again be interfered with, unless all trace of the salt is removed. Dr. Buys has used the lead in a great many cases of contagious ophthalmia among the military, and Dr. Cunier in acute and chronic catarrhal ophthalmia; the results seem to

have been more satisfactory in the latter variety, as also in scrofulous affections of the eye, vascular cornea, and ulcers of that membrane. The mineral has likewise succeeded in cutting short incipient inflammation of the organ.—*Half Yearly Abstract of the Medical Sciences.*

Epidemic Dysentery of 1849-50: a Paper read before the Medico-Chirurgical Society of Cincinnati. By JOHN A. MURPHY, M. D. With Remarks by the Editor of the Buffalo Medical Journal.

I do not intend, in this paper, to weary the patience of the society, or to abuse its good sense and knowledge of dysentery as a disease, by extending this essay to an immoderate length. Every member of this society has had, in all probability, as much observation of the disease as myself; and possibly each and all may have had better success in its treatment.

I have been impressed with two or three points somewhat new to myself, in the last two years, in the management of this disease, which I have been desirous of presenting to the consideration for some time.

There is, as I conceive, too much eagerness manifested on the part of many in the profession, to make advances and progress (a word, by the way, pregnant with as much evil as good) in every department but that of sound, practical therapeutics. This should not be so, yet I suppose we all must give way, for the time, to the fashion. There is also too much of what may be called physiological practice, on the part of some; while with others, we have the routine practice pursued by those called *experienced*. The latter are much to be preferred. In epidemics, however, they fail; at least, if not failing, the success is comparatively small. In the treatment of dysentery in this city, for the last two years, this certainly has been, in some measure, true. The mercurial practice, in this disease has, I am sure, been inapplicable and unhappy. The hepatic pathology of dysentery is an old one, and, at the present day, an unsound one. The treatment of this disease on its received pathology has proved this true. And here, in passing, I will say, that the depletant and alterative mercurial treatment is receiving, in a great measure, the go-by in all inflammatory affections, at least by a respectable portion of the profession. It is, for one reason, if for no other, too perturbing and depressing. The medical constitution of this whole continent, if not of the world, is asthenic, and has received this impress by the advent of the cholera. That its manner and power of impinging the vital organization has been changed, is true; and that we are justified in attributing this change to the late great epidemic and the one which preceded it, is, I think, too evident to need much demonstration. I know very well that it is unphilosophical to assume a cause, and, in the present case, I hope no one will be inclined to attribute such an assumption to me. That the constitution of diseases was altered by the first epidemic is well attested by the character of those which followed it. It was only succeeding that great epidemic that the attention of the profession was called to the frequent and very general predominance of bowel inflammations in almost all diseases, especially of fevers. During and since the late epidemic, it is a very rare thing indeed to meet with a

case of simple or compound bilious remittent fever. But not, however, to indulge in further speculation, I can well and truly say, that we have encountered a form of dysentery, for the last two years, which was severe in its onset, wonderfully rebellious to treatment, accompanied by the entire and perfect depression of all the powers of life. The whole innervation was seriously and powerfully implicated. There can be no doubt, I believe, that the great proximate cause producing this disease, was a modified form of the epidemic virus of cholera. It was modified in its potency and action. As far as I can learn, it was not as severe in this city, and in towns of smaller size, as its predecessor, cholera. And what is somewhat more remarkable, it prevailed in the country with great fatality, and that, too, in many places where but few cases of cholera were observed. The opinion that it was produced by the choleraic poison, gains much plausibility and probability from the fact, that the ordinary causes of epidemic dysentery were not at all in existence; and if they were here and there, but few of those attacked were exposed to them. Cold and dampness have been the great causes originating this disease. Every one here will remember the very warm and dry weather of the past summer. More than all this, epidemic dysentery generally prevails in armies, or in places where large numbers of people are crowded together.

There can be little doubt, I think, held, as to the very prominent cause. The dysentery of the last two years has been so entirely dissimilar to the disease as it ordinarily prevails, that the treatment has been modified and changed in several of its strong and main points. This is the result of my experience. More than this, I am convinced that mercurial and depletant treatment, as ordinarily practised in hepatic dysentery, was wholly inefficient and inapplicable.

The disease, pathologically considered, did not depend, in the great number of cases, on hepatic derangement. Bilious missecretion was not present in the greater part of the cases. I mean by this term, bilious missecretion, a suspension, a change in the character of the bile, or an increased secretion. No pain in right hypochondriac region was complained of. Further, the majority, in the beginning, were what is called dysenteric diarrhœa, the stools being composed, at first, of feculent matter, blood, and some mucus. So much for the hepatic pathology.

The disease consisted essentially of a low type, with a strong tendency to terminate in rapid ulceration and gangrene. If a post mortem examination of the fatal case could have been had, I think that ulceration of the larger part of the colon, with gangrene, would have been found. The ordinary signs and symptoms of severe inflammation of a tonic type, were not present at the beginning of but few cases. I should be much astonished for any one to tell me, that he had treated a case successfully either by decided general or local bleeding. The disease was well marked by all that debility and utter prostration, which accompanies extensive disease of the glands and mucous lining of the bowels. Instead of the full, strong pulse, hot skin, tenderness on pressure along the track of the colon, there was the frequent, small, weak pulse, damp, clammy skin, with coldness of the extremities, facies hypocratica, constant tormina and tenesmus and dragging in the lower part of the abdomen, accompanied with urgent desire to go to stool almost constantly. These were a grave class of symp-

toms, and demanded, as every one well knows, the very opposite of the depletant treatment.

This class of symptoms ushered in the disease in the adult and infant. And here I may say it has dealt severely with children. I have no general data, but I am safe, from my own observation, in saying, that it has formed the largest number in the bill of mortality of children, for the last two years. From the history of many cases, I was not led to believe that the disease had been induced by improper food, or by a change of diet. Too often it succeeded ordinary cases of bilious diarrhœa.

I observed the disease in four different conditions:

1. That following other diseases.
2. That which began as mild dysenteric diarrhœa, and ran rapidly into well-defined.
3. That in which the patient was seized with the disease, in all its severity, at the very onset. In this class of cases, ulceration and gangrene made their appearance early.
4. That form of it when it had become chronic.

This disease was, when present in women, always more severe and resistant to treatment than in men. If in children, it was severe, and as I have before said, in the majority of cases, fatal.

It was of vast trouble when it ensued on other diseases—especially was this the case when it supervened on cholera infantum. Attacking patients convalescing from cholera, the prognosis was bad. I have seen it proceed to ulceration of a chronic character, when it had followed an attack of cholera. This ulceration was very slow and unmanageable. But of this I shall speak further along.

In the second class, or that of dysenteric diarrhœa, or when the stools contained healthy feculent matter, mixed with blood and mucus, and accompanied with tormina and tenesmus, the treatment, if early, was easy and efficient. The stools, in this class were frequent, though tenderness along the colon was not, as a general rule, well marked. Fever was present in very few cases. The dragging in the lower part of the abdomen was generally, and in some few cases was painfully, present. I noticed in all cases a strong disposition to ulceration at an early period. This ulceration was not confined to the rectum, but extended as high up as the head of the colon. The disease simulated, in this respect, the severe ulceration of the bowels of 1846, which prevailed along the Ohio and Mississippi rivers. It was this ulceration which carried off so many of the returning and discharged volunteers of the army in Mexico. It prevailed largely in this city at that time. In the Hospital, during that year, I treated a large number of this form of ulceration, and can say that, in many respects, the two diseases were much alike. That ulceration was the disease itself, while this was an early result.

I stated in passing, that there was, as a general rule, no evidence of biliary missecretion in this form. The stools were feculent, mixed with blood and mucus. The three principal features of dysentery, tormina, tenesmus and frequency of discharge, only being present. The stools, so far as the amount of blood and mucus were concerned, were of only secondary importance. In other words, the signs and symptoms seemed only to declare the length which the disease might go, if uncontrolled. The pulse was generally small, frequent and weak—not a pulse at all permitting bleed-

ing. The heat of the surface was not above the ordinary standard, in but few cases. The tongue was moist, covered generally with a whitish fur. There was nothing pathognomonic about it.

This form was much more amenable to treatment than either of the others. The third form of the disease was, by all odds, the severest, most malignant and fatal. The patient was attacked severely, and that with every bad symptom. The tenesmus, tormina and prostration, were great. The stools were of blood and mucus, containing shreds of what seemed to be flesh, mixed with purulent matter. The odor was very foetid. It was like the cadaveric smell which arises in dissecting rooms, when the weather becomes warm and moist. In one word, it was the odor of mortification. It was an unpromising characteristic. The depression in many of these cases was great, and increased so rapidly that the patients were unable to leave the bed to stool. The tenesmus very often was so great and so continuous, that, in a short time, the muscular contractility of the sphincter ani was paralyzed so as to permit the contents of the bowels to escape involuntarily. The stools, as I have said, were composed entirely of blood and mucus in the beginning, but after the first three or four days, in several cases, were accompanied with green matter, and in other cases, with large lumps of darkish green matter. There was also a large admixture of pus in all of these stools.

The physical sign of severe inflammation, as evidenced by tenderness on pressure along the colon, was not as well marked as could have been expected under such circumstances. There was a tympanitic distention; not, however, severe; owing, I presume, to the extension of the inflammation to the muscular coat. A constant sense of dragging was present, in the majority of cases. It was a very painful symptom.

In women, this dragging sensation was very painful and severe. The disease in women, especially in those of advanced years, or who had borne several children, was malignant, prostrating and unmanageable. This stubbornness and severity arose, I suppose, from the excited sensibility of the uterine system of nerves.

The discharges, as I have before stated, consisted of large quantities of greenish matter. I know of no theory to explain the presence of these discharges, but that they were composed of effused blood, changed in its color by the acid in the bowels. Golding Bird, of London, in an article on cholera infantum, demonstrated, by the microscope, the green discharges of cholera infantum to consist of effused blood, changed by the acids in the bowels.

The supposition that these stools were dependent, in this disease, on morbid secretion of the liver, is to me a very mistaken and fallacious one.

A correct pathology, and a sound therapeutical indication, is very generally, I may say always, proved beautifully and clearly, *ceteris paribus*, by the treatment.

The fourth form, or the chronic, deserves something more than a passing notice. I view the disease as being chronic, after the third week. The treatment was very difficult and protracted. Ulceration, more or less extensive, was present in all of the cases. The tormina, tenesmus, and blood and mucus, were soon removed, but there continued an irritable state of the bowels, with five or six evacuations in the twenty-four hours.

But let me proceed to pronounce what I have to say on the treatment.

The treatment, then, of this disease, as ordinarily pursued and relied on, failed, or was inapplicable. I mean by this treatment, general and local bleeding, astringents, injections and mercury. The most important of these agents opposed to and cutting short inflammation, blood-letting, was wholly useless, for the reasons I have given in passing. Local bleedings by cups amounted to but little, as the quantity of blood abstracted by the cups, was too small to lessen or influence the inflammation. The benefit resulting from their use was attributable to the counter-irritation. Astringents, as acet. plumbi. tannin, combined with opium, did not act well in the majority of cases. The controlling effect exerted by them was manifested very late, and that in very few cases. I have seen patients, while taking 3 and 4 grs. acet. plumbi. with ss. gr. opium every three hours, have just as many stools, and in every respect as painful. Calomel, or blue mass, in conjunction with astringents, acted no better. It was not the disease for calomel as an alterant. Of injections I can speak with no more commendation. Indeed, I am sure I have witnessed the sufferings of the patients increased by enemata; and that too by those composed of simple mucilage and laudanum. Very often the smallest and most soothing injection dexterously administered, would not be retained a minute. The tenesmus was so severe, the irritability of the anus and rectum in other cases, was so great, that a simple suppository of extract opii would be expelled the instant it was introduced.

My precept of practice in regard to the use of enemas, was founded on the severity of the tenesmus: if it was great, if almost constantly present, and if accompanied with small, bloody mucus and puerlent stools, with the odor of mortification, I did not order them. I did not order them for I soon found they would not be retained, and often aggravated the tenesmus. I have used a great many articles in enema, as acet. plumbi. tannin, tinc. opii, bals. copaibæ, mucilage, &c. In several cases, enemas of cold water were not retained for any length of time, and did not seem to have any good effect.

The idea of salivating patients under such circumstances, is a very bad one: the attempt to do so is very profitless and oftener impossible. The treatment is not as rational to me as that of salivating a patient with typhoid fever, and that I hope every one here believes is utterly bad, unsound and absolutely the signed warrant of death to the patient.

To ptyalize when we are ignorant of what else to do, is only worthy of a medical man of the mediæval ages, and as such should be banished and cast out from all sound practice. There are two conditions of the economy in which we can not obtain the constitutional effect of mercury; that of exalted, and that of depressed action. As an illustration of the latter, let each and all recur to few instances of salivation in those cases of cholera where such immense quantities of calomel was given. Drs. DeLamater and Ackley of Cleveland state that but few cases were salivated of all those who were treated after Ayres's plan, the most successful, by the way, of all others. The calomel, they say, was found in large quantities lining the mucus membrane of the stomach and bowels of those who died. It acted in some way on the secretions of the bowels, which were highly acid from the lack of a proper proportion of alkalies in the blood; and frequently I have no doubt, dysentery was induced by this cause.

In the great number of cases, the debility was so great, that salivation

could not be induced, and if it was brought about, it was just half past the "eleventh hour." Calomel was not needed but sparingly, much less its constitutional effect. It acted as an irritant; it added to the already great irritation of the constitution; it detracted from the already too small quantity of fibrine in the blood; it stimulated the liver to morbid action: in one word, it produced mercurial bile—a term which every one will understand.

Here let me stop to say, that I have no specific treatment of dysentery—no one or many articles combined, which cured all cases. But this much I can say, that the prescription which I shall give shortly effectuated the removal of the disease more safely, more speedily, and more pleasantly to the patient and doctor, than any thing else I tried.

It must be remembered always, when speaking of remedies, no matter how potent and how various their power in curing, that there are a certain number of cases of every disease, be it epidemic, ideopathic or sporadic, which are what Sir Gilbert Blane calls "determinedly fatal."

My favorite prescription was oil sweet almonds ℥ij. to ℥ss., pulv. accaciæ ℥j, laurel water ℥ss., sugar ℥j, sulph. morphine gr. j. to gr. jss., water ℥ij., of which a half to a full tablespoonful was to be taken every three hours.

Some care is to be had in making this emulsion. The oil of almonds and the morphine is to be rubbed up well with gum accaciæ, and then the laurel and common water is to be added. It is especially necessary to have the oil almonds fresh and free from rancidity, while it is well blended with the g. accaciæ. When the discharges were frequent, I have often, and that with good effect, added ℥j. acet. plumbi. Generally, however, I have given the original prescription.

This prescription was first used by my friend Dr. Unzicker. My friend, Dr. David Judkins, first reported to me its genial effect last year, since which time I have used it with the best results. The acet. plumbi did not belong to the original prescription.

It constituted, however, my main medical treatment in the disease. It very soon relieved the tormna, tenesmus and frequency of stools. It brought about a cure in from ten days to two weeks. During its administration, I occasionally gave x. or xv. grs. pill mass hydrg., with ext. hyoscyam, in divided doses to counteract the restraining action of the opium on the secretions generally. At other times I gave one or two grs. calomel, for the same purpose. In conjunction with this, I resorted early to stimulants.

If the disease was of the severe character, I ordered brandy, beef tea, chicken tea, and so on. If milder, I gave port wine. I prescribe stimulants irrespective of fever. Iced gum arabic water was given to allay the thirst.

Not in one case could I perceive any bad results from the stimulants. The local and constitutional irritation was so great that stimulants were essentially necessary. The doctrine of Travers, in his monograph on constitutional irritation is certainly correct, when he says, "debility is the basis of morbid irritation, and those causes of debility which operate with the greatest force and directness most invariably aggravate the state of irritation."

That *questio vexata*, the *modus operandi* of the prescription, may be raised upon me here.

The great efficacy is owing to the peculiar combination. In addition to

the sedative and anodyne power of the morphine, we have that also of the almond oil and laurel water.

The morphine did not control the disease by itself. Passing over this point, I can repeat what I have before said, that I observed the most admirable results from it.

In chronic dysentery I continued its use until the tormina and tenesmus were removed. To heal the ulcers, I alternated the syrup ferri iodide, in xv. grs. doses, three times a day, with a pill of nitrat. argent $\frac{1}{2}$ gr., sulph. morphine $\frac{1}{4}$ gr., four times a day. I have treated a large number of cases of this disease in the last two years, and have lost very few. Here I may say, that the treatment of dysentery, by the regular profession in this city, has been very successful.

I have to regret my inability to present the results of a post mortem examination. One word in conclusion and I am done. I have described the disease as I saw it, and have given the treatment which, after much anxious trial, I found the most successful.*

Remarks by the Editor of the Buffalo Medical Journal.—The considerations presented in the foregoing essay relate to a highly interesting and important practical subject, and are intrinsically valuable. It is gratifying to observe that the hepatic pathology of dysentery is receiving its just deserts in the estimation of the intelligent portion of the profession. What can be more gratuitous than to hold the liver responsible for the phenomena due to a phlogosis of the muccus tunic of the large intestine? All the data upon which this hypothesis is based are assumed, not only without proof, but in direct opposition to plain facts. The hepatic practitioner tells us that the philosophy of dysentery involves, first and chiefly, suppression of the secretion of bile; next, in the order of sequence, congestion of the portal circle, and from these morbid conditions combined, the intestinal symptoms pertaining to the disease derive their origin. Now, the point of departure for this imaginary train of events is wholly supposititious. It may be doubted if the secretion of bile be more affected in this disease than in other disorders into the pathology of which it is not supposed to enter. And if it were, why should such consequences be expected to follow? Is not the biliary secretion suppressed totally in some cases of icterus, without dysenteric, or any symptoms of intestinal disease being present. "Congestion of the portal circle," too, an expression so often quoted, so convenient and satisfying to the minds of some—what are the positive evidences that such a morbid condition plays an important part, or even that it is present at all in dysentery? But admitting the portal system to be congested, what right

* An interesting discussion occasioned by the reading of the foregoing essay is omitted for want of space.

have we to infer that any of the symptoms of dysentery are due to that source? We know that the portal system is congested in cirrhosis of the liver to a degree causing mechanical transudation of the serosity of the blood through the coats of the vessels into the peritoneal cavity; but we do not meet with dysentery in that connection.

Place against these gratuitous assumptions the positive facts so easily developed if practitioners will take the trouble to examine the mucous tunic of the large intestine in a few fatal cases of dysentery—the increased vascularity, thickening, softening, ulcerations extending, more or less, from the outlet of the rectum to the ileum, or even higher in the intestinal tube—and the hepatico-congestive theory will appear puerile enough to all except those in whose pathological notions the “liver (to quote the language of our friend Dr. T. S. Bell) is the central sun around which all the other organs of the body revolve as so many satellites.”

The factitious influence attributed to the Liver in the production of this, as well as of many other diseases, has led to therapeutical errors which must be abandoned, or materially modified, just in proportion as the vicious pathological ideas upon which they rest are supplanted by those more in accordance with the present condition of science. The practitioner who sees in the phenomena of dysentery suppression of bile and portal congestion as the prime and essential morbid conditions—the first links in the chain of causation—will be timid in the use of opiates for fear of increasing the congestion still more, and diminishing, if possible, in a greater degree the biliary secretion; and, with the notions which have been current respecting the specific action of mercurials, he will be likely to employ calomel in excessive doses to ‘act upon the liver, and promote free circulation through the portal circle.’ The effect, thus, of the hepatic pathology, here as in other instances, is positively and negatively bad. It stands in the way of remedies calculated to relieve painful symptoms, and abate the severity of the disease, and it encourages the injudicious use of a valuable remedy, the abuse of which has done not a little to bring the practice of medicine into discredit.

These remarks have been suggested by reading the article which precedes this, copied from the *Western Lancet*. The subject of dysentery opens a wide field for remark. We have noticed only one of the many topics which a full discussion of the subject would embrace; but we have no design of engaging in such a discussion at this time. We will, however, add a few observations.

There are few diseases which present such wide extremes as respects

degree, as well as such variations in important symptoms, as dysentery. In its sporadic and epidemic forms it is hardly the same disease, the points of difference preponderating so much over those of agreement. In ordinary sporadic dysentery the affection seems to be purely a local affection, not extending even over the whole tract of the large intestine, and sometimes confined to the rectum. Such cases almost uniformly recover, and efficient treatment is not called for. In epidemic dysentery, on the other hand, the local inflammation is more extensive, affecting the whole of the large, and perhaps more or less of the small intestine. It is easy to understand that the gravity of the symptoms due to an inflammation of the external surface must be commensurate with the extent of surface inflamed; and it is obvious that the same rule should hold good with respect to an internal surface. An area of the cutaneous tegument equal to that of the mucous tunic of the large intestine expanded into a plane, if affected, for example, with erysipelas, would by no means be a trivial affection. Here then, is one cause of disparity.

But another and probably not less important cause involves the immediate action of a morbid principle upon the system. Epidemic dysentery is a zymotic affection, of which the inflammation of the intestine is but a local expression—an effect, or a complication. Dr. Ramsay's views on this point we doubt not to be correct. Hence, the gravity of the symptoms will be affected by the degree of those inappreciable changes appertaining to the system which are the proximate effects of the action of the epidemic poison. Hence the variations in different seasons, and places, as well as in different individuals due to the special cause, and the circumstances appertaining to its action upon the system, irrespective of the intestinal affection.

The intestinal inflammation in many cases of epidemic dysentery, bears to the whole disease a relation somewhat similar to that existing between the pneumonitis which occurs as a complication of Typhus, and the febrile disease. We might, with as much propriety, say that the pneumonitis, in the latter case, constituted the disease, as in the former case, to overlook a morbid state ulterior to the local affection.

If these views be correct, a routine uniformity of treatment for this disease is, in the abstract, an absurdity, and, in its consequences, can hardly fail to be, in a certain proportion of cases, productive of a degree of harm proportionate to its potency for evil as well as good. That pathology which recognises "but one organ, the liver,—one disorder, the biliary,—and one remedy, calomel," however it may commend itself for its simplicity and

convenience, will prove, in its practical results, to be far from an innocent vagary.

It is at once obvious that a rational treatment comprehends measures appropriate to the local and general morbid conditions, and adapted to meet the variations incident to time, place, and individual cases. This mode of expression refers to generalities only, but we cannot here amplify upon this point sufficiently to consider particulars. Suffice it to say, that in fulfilling the various and varying indications of treatment which belong to this disease, opiates, astringents, tonics, stimulants, mercurials, etc., may all be brought into requisition; and, in the management of this disease, as in other diseases, that practitioner will be the most successful who is best qualified to appreciate in each case the particular objects which it is desirable to fulfil by remedies, and who, in connection with an abundant personal experience, and an extended acquaintance with the experience of others, can bring the best judgment and tact in applying means to effect these objects.

EDITORIAL DEPARTMENT.

Discovery of the Source of the Rochester Knockings.

All our readers have heard of the *Rochester knockings* that have occasioned not a little stir in different parts of the country during the past two or three years. The *knockings* were first manifested in a family of the name of Fox, then residing in a small town in the western part of this State, and the removal of this family, shortly afterward, to Rochester, whence have emanated many of the marvelous stories connected with the subject, has secured for that city the honor of forming the adjective in the title by which they are commonly mentioned. The *knockings*, however, have not been confined to Rochester, but have been heard in some other places. They accompany members of the Fox family in their peregrinations, of course, but we understand that other persons than those belonging to this family have assumed to be *media* for similar supernatural manifestations.

Being regarded by the credulous and superstitious as phenomena produced by the agency of departed spirits, indicating their presence, and furnishing a means of communication with them, it is not singular that, however ridiculous the subject may seem to persons of well balanced

minds, to those of a different mental cast, it assumes a different aspect, and becomes invested with great interest and importance. In every community persons are to be found who are fond of indulging and cultivating a love for what is marvelous, and who are ready to believe that a supernatural agency is involved in whatever transcends their comprehension. Such tendencies are by no means found in connection exclusively with low intellectual powers, and small attainments. On the contrary, it is not infrequently the case that persons of education, of reflection, and even of superior mental endowments in some respects, are led astray by what appeals strongly to the mental qualities underlying an unfortunate excess of credulity. The chicaneries of mesmerism, the faith inspired by revelations like those of Davis, etc., sufficiently attest the truth of the remark just made. We might also quote, as illustrations, the transient success of homœopathy, and other kindred medical delusions. The annals of every age furnish abundance of examples showing the absurd extravagances into which men may be led who allow unrestrained scope to the imaginative and superstitious elements of the mental constitution; showing, also, the astonishing extent to which cunning impostors are able to take advantage of these elements of human character. Based, as are the various delusions, impositions, and humbugs that prove successful, upon qualities of mind which it is not to be expected will soon cease to be predominant in certain individuals, albeit science and knowledge are progressively advancing, and despite the accumulated lessons of experience, we are not to suppose that the future, more than the past and present, will be devoid of instances exemplifying human weakness and folly like that to which reference has been made. But to return to the *Rochester knockings*. We have not taken pains to ascertain how extensively belief in their supernatural character has prevailed. Many of our readers are probably better informed on this point than ourselves, as our pursuits do not permit us to keep up with the times in matters of this kind. That many well meaning persons have been beguiled and carried away with this subject, we know, and that not a little time, money, thought, and feeling, have been expended in the efforts to hold communion, by *rappings*, with the inhabitants of the spirit world, is a fact but too apparent to any one who looks into newspapers. The imposition, unfortunately, is not to be considered merely a successful but harmless experiment on the exhaustless fund of human credulity. Among other serious consequences, we have been told that several cases of insanity have originated in the mental excitement occasioned by fancied intercourse with the spirits of departed friends.

The imposition, which had already escaped detection for several years, would still find plenty of dupes, if the *mysterious sounds* were to continue unaccounted for. The absurdity of the professed spirituality of the knockings can undoubtedly be fully proved in a variety of modes, but the only effectual preventive of the farther progress of the humbug is to determine satisfactorily their nature and source. To do this is to strike at the root of the delusion by rendering it as ridiculous as the explanation is simple. We are not aware that the curious and (in other than a literal sense) striking phenomena have been, as yet, accounted for. To what extent they have been made the subject of investigation by physicians we cannot say. As we are prepared to unravel the mystery, we trust our readers will not think the subject unworthy the space which we propose to devote to it, more especially as the sounds possess interest in a physiological point of view, apart from the remarkable imposition to which they have been made subservient.

Two members of the celebrated family of *Rochester knockers* recently made their debut in this city, accompanied by the noisy spirits, and commenced operations, drawing crowds of visitors at a dollar a head, many of whom were impressed with the wonderful revelations interpreted from the *raps*, and several intelligent persons became converts to the doctrine of the spiritual origin of the sounds. From motives of curiosity we were led, with our colleagues, to pay them a visit, and, we must confess, we were surprised and puzzled by the loudness of the sounds, the apparent evidences of non-instrumentality on the part of the females, and the different directions from which they seemed to emanate. Close observation, however, of the countenances and deportment of the two females, led to the conviction that the production of the sounds involved a voluntary effort by the younger sister of the two—a girl about seventeen years of age, the elder sister (who is said to be a widow) being about thirty-five. The latter was apparently the *managing partner*, conducting the spiritual communications, while the former, it was clear, was the performer, i. e. the one that produced the knockings. Assuming the above as a point of departure, by the process of reasoning given below, the diagnosis was, that the sounds must necessarily be articular. This conclusion and the process by which it was arrived at, were stated to a number of persons directly after the visit. The question, then, was how such sounds could come from joints. The snapping of the phalangeal joints of one hand by lateral motions made with the other hand, is familiar to every one. Some persons have the power to produce the same snapping by means of the muscles inserted into

the phalangeal bones, without any aid from the other hand. Dislocated bones return to their place with an audible snap, as all surgeons know. A patient once consulted us for a loud noise in his joint produced by walking. Almost every one has occasionally, by an accidental oblique movement of the lower extremities, caused a loud report in the knee joint. These facts suggested themselves, but works on physiology, anatomy, and dislocations, were consulted, in vain, for any account of loud noises like the *Rochester knockings* originating in the articulations. While pursuing these inquiries, which had been unexpectedly provoked, we chanced to meet with a person who said that his wife could produce similar sounds. He did not then know in what way they were produced; his wife had, in jest, kept him in ignorance on this point. At our request he immediately went home to ascertain, and returned with the information that the noise came from the knee joint, and that we were at liberty to satisfy ourselves with respect to this fact, and also of the mode in which they were produced. Accordingly, at first alone, and afterward accompanied by Drs. Lee and Coventry, (in concert with whom the prior investigations were conducted,) we visited the lady referred to, and on the following day the subjoined exposition was communicated for one of the daily papers of the city.*

To the Editor of the Commercial Advertiser:

Curiosity having led us to visit the room at the Phelps House in which two females from Rochester, (Mrs. Fish and Miss Fox,) profess to exhibit *striking* manifestations of the spiritual world, by means of which communion may be held with deceased friends, &c., and having arrived at a physiological explanation of the phenomena, the correctness of which has been demonstrated in an instance that has since fallen under observation, we have felt that a public statement is called for, which may perhaps serve to prevent further waste of time, money, and credulity, (to say nothing of sentiment and philosophy,) in connection with this so long successful impositions

The explanation is reached almost by a logical necessity, on the application of a method of reasoning much resorted to in the diagnosis of diseases

* In transferring that communication to our columns, we have corrected an error in the account of the displacement of the joint which produces the sounds. The exposition was drawn up hastily, and published at once, in order to check as promptly as practicable the farther progress of the imposition, and the mechanism was not so fully ascertained, as it has been by subsequent examinations. We will thank editors of medical Journals who may notice this matter to copy the anatomical explanation from this article, and not from the newspaper, and to make the correction referred to, should they have already quoted the first statement.

viz: reasoning by way of exclusion. It was reached by this method prior to the demonstration which has subsequently occurred.

It is to be assumed, first, that the manifestations are not to be regarded as spiritual, provided they can be physically, or physiologically accounted for. Immaterial agencies are not to be invoked until material agencies fail. We are thus to *exclude* spiritual causation in this stage of the investigation.

Next, it is taken for granted that the *rappings* are not produced by artificial contrivances about the persons of the females, which may be concealed by the dress. This hypothesis is excluded, because it is understood that the females have been repeatedly and carefully examined by lady committees.

It is obvious that the rappings are not caused by machinery attached to tables, doors, etc., for they are heard in different rooms, and different parts of the same room, in which the females are present, but always *near* the spot where the females are stationed. This mechanical hypothesis is then to be *excluded*.

So much for *negative* evidence, and now for what *positively* relates to the subject.

On carefully observing the countenances of the two females, it was evident that the sounds were due to the agency of the younger sister, and that they involved an effort of the will. She evidently attempted to conceal any indications of voluntary effort, but in this she did not succeed:— a voluntary effort was manifest, and it was plain that it could not be continued very long without fatigue.

Assuming, then, this positive fact, the inquiry arises, how can the will be exerted to produce sounds (rappings) without obvious movements of the body? The voluntary muscles are the only organs (save those which belong to the mind itself) over which volition can exert any direct control. But the contractions of the muscles do not, in the muscles themselves, occasion obvious sounds. The muscles, therefore, to develop audible vibrations, must act upon parts with which they are connected. Now, it was sufficiently clear that the rappings were not *vocal* sounds: these could not be produced without movements of the respiratory muscles, which would at once lead to detection. Hence, *excluding* vocal sounds, the only *possible* source of the noises in question, produced, as we have seen they must be, by voluntary muscular contractions, is in one or more of the movable articulations of the skeleton. From the anatomical connections of the voluntary muscles, this explanation remains as the only alternative.

By an analysis presented in this manner we arrive at the conviction

that the *rappings*, assuming that they are not spiritual, are produced, by the action of the will, through voluntary muscles, upon the joints.

Various facts may be cited to show that the motion of joints, under certain circumstances, is adequate to produce the phenomena of the *rappings*; but we need not now refer to these. By a curious coincidence, after arriving at the above conclusion respecting the source of the sounds, an instance has fallen under our observation which demonstrates the fact that noises precisely identical with the *spiritual rappings* may be produced in the *knee joint*.

A highly respectable lady, of this city, possesses the ability to develop sounds similar both in character and degree to those professedly elicited by the Rochester imposters from the spiritual world. We have witnessed the production of the sounds by the lady referred to, and have been permitted to examine the mechanism by which they are produced. Without entering, at this time, into a minute anatomical and physiological explanation, it is sufficient to state that, owing to relaxation of the ligaments of the knee joint, by means of muscular action, and pressure of the lower extremity against a point of resistance, the large bone of the leg (the tibia) is moved laterally upon the lower surface of the thigh bone (the femur) giving rise, in fact, to partial lateral dislocation. This is effected by an act of the will, without any obvious movement of the limb, occasioning a loud noise, and the return of the bone to its place is attended by a second sound. Most of the Rochester rappings are also double. It is practicable, however, to produce a single sound, by moving the bone out of place with the requisite quickness and force and allowing it to slide slowly back, in which case it is noiseless.

The visible vibrations of articles in the room situated near the operator, occur if the limb, or any portion of the body, is in contact with them at the time the sounds are produced. The force of the semi-dislocation of the bone is sufficient to occasion distinct jarring of doors, tables, etc., if in contact. The intensity of the sound may be varied in proportion to the force of the muscular contractions, and this will render the apparent source of the rappings more or less distinct.

We have witnessed repetitions of experiments in the case just referred to, sufficient to exhibit to us all the phenomena of sounds belonging to the Rochester rappings, and without further explanations at this time, we append our names in testimony of the facts contained in the foregoing hastily penned exposition.

Feb. 17, 1851.

University } AUSTIN FLINT, M. D.,
of } CHARLES A. LEE, M. D.,
Buffalo. } C. B. COVENTRY, M. D.

The disclosure announced in the foregoing communication occasioned not a little excitement among those who had become interested in the *knockings*. The correctness of the explanation was not only called in question by these, but was doubted by many who had not hesitated to look upon the matter as a gross deception. The Rochester *Ladies*, of course, stoutly denied the imputation that the sounds proceeded from the joints, or were produced by any agency of theirs, and, the next day, they inserted in the daily papers the following card:—

ROCHESTER KNOCKINGS.

To Docts. Flint, Coventry and Lee:

GENTS: We observe by a communication in the *Commercial Advertiser*, that you have recently made an examination of a highly respectable lady of this city, by which you have discovered the secret of the "Rochester Imposters." As we do not feel willing to rest under the imputation of being imposters, we are very willing to undergo a proper and decent examination, provided we can select three male and three female friends who shall be present on the occasion.

We can assure the public that there is no one more anxious than ourselves to discover the origin of these mysterious manifestations. If they can be explained on "anatomical" and "Physiological" principles, it is due to the world that the investigation be made, and that the "Humbug" be exposed. As there seems to be much interest manifested by the public on this subject, we would suggest that as early an investigation as is convenient would be acceptable to the undersigned.

ANN L. FISH.
MARGARETTA FOX.

The invitation thus proffered was accepted by those to whom it was addressed, and on the following evening, by appointment, the examination took place. After a short delay, the two Rochester females being seated on a sofa, the knockings commenced, and were continued for some time in loud tones and rapid succession. The "spirits" were then asked "whether they would manifest themselves during the sitting and respond to interrogatories." A series of *raps* followed, which were interpreted into a reply in the affirmative. The two females were then seated upon two chairs placed near together, their heels resting on cushions, their lower limbs extended, with the toes elevated and the feet separated from each other. The object in this experiment was to secure a position in which the movements of the knee joint should be made tense, and no opportunity offered to make pressure with the foot. We were pretty well satisfied that the displacement of the bones requisite for the sounds could not be effected unless a fulcrum were obtained by resting one foot upon the other, or on some resisting body.

The company, seated in a semi-circle, quietly waited for the "*manifestations*" for more than half an hour, but the "spirits," generally so noisy, were now dumb. The position of the younger sister was then changed to a sitting posture, with the lower limbs extended on the sofa, the elder sister sitting, in the customary way, at the other extremity of the sofa. The "spirits" did not choose to signify their presence under these circumstances, although repeatedly requested so to do. The latter experiment went to confirm the belief that the younger sister alone produces the *rappings*. These experiments were continued until the females themselves admitted that it was useless to continue them longer at that time, with any expectation of *manifestations* being made.

In resuming the usual position on the sofa, *knockings* very soon began to be heard. It was then suggested that some other experiment be made. This was assented to, notwithstanding the first was, in our minds, amply conclusive. The experiment selected was, that the knees of the two females should be firmly grasped with the hands so applied that any lateral movement of the bones would be perceptible to the touch. The pressure was made through the dress. It was not expected to prevent the sounds, but to ascertain if they proceeded from the knee joint. It is obvious that this experiment was necessarily far less demonstrative, to an observer, than the first, because if the bones were distinctly felt to move, the only evidence of this fact would be the testimony of those whose hands were in contact with them. The hands were kept in apposition for several minutes at a time, and the experiment repeated frequently, for the course of an hour, or more, with negative results: that is to say, there were plenty of *raps* when the knees were not held, and none when the hands were applied save once, as the pressure was intentionally somewhat relaxed, (Dr. Lee being the holder,) two or three faint, single *raps* were heard, and Dr. Lee immediately averred that the motion of the bone was plainly perceptible to him. The experiment of seizing the knees as quickly as possible when the knockings first commenced, was tried several times, but always with the effect of putting an immediate *quietus* upon the *manifestations*.

The proposition to bandage the knees was then discussed. This experiment was objected to, on the part of the friends of the females, unless we would concede that it should be an exclusive test experiment. We were not prepared with appliances to render the limb immovable, and therefore declined to have it considered such a test. This was the experiment anticipated, and one which, we presume, the females thought would end in their triumph. A bandage applied above and below the patella, admitting



of flexion of the limb, will probably not prevent the displacement, as we have but little doubt had been ascertained by the Rochester females before an examination was invited. Should it become necessary to repeat experiments in other places, in furtherance of the explosion of the imposition, we would suggest that the bandage be not relied upon. Plenty of roller, with lateral splints, firmly applied, so as to keep the limbs extended, and render the joints immovable, would doubtless succeed in arresting sounds so far as they involve the knee joint. It will be observed that, in our exposition, we do not claim that this joint is exclusively the source of sounds, and had our experiments, which were first directed to this joint, failed, we should have proceeded to interrogate, experimentally, other articulations. This, however, as the reader will note, seemed quite unnecessary. The conclusion seemed clear that the *Rochester knockings* emanate from the knee joint.

Since the exposition was published, we have heard of several cases in which movements of the bones entering into other articulations are produced by muscular effort, giving rise to sounds. We have heard of a person who can develop knockings from the ankle, of several who can produce noises with the joints of the toes and fingers, of one who can render loudly audible the shoulder, and another the hip joint. We have also heard of two additional cases in which sounds are produced by the knee joint. We have not, as yet, had an opportunity to make a personal examination in any of these cases, or to hear the sounds. The exposure of the imposition opens a new and curious field of physiological inquiry, and we would commend the subject to those who have leisure and facilities for prosecuting it. *Articular* as well as *articulated* sounds seem to claim an investigation which they have not heretofore received. Had the facts which the detection of this trick has developed, been contained in anatomical or physiological treatises, the progress of the deception would have been arrested long ere this. Doubtless these facts are not entirely new—they must have been observed in other cases the histories of which have escaped record. That sounds so loud should originate in the way we have ascertained that they are produced, would surprise even the medical listener, and perhaps seem almost incredible. It is readily conceivable how to other than medical listeners, the phenomena should appear, not only inexplicable, but in a high degree mysterious. The remark was made by many after the explanation was published that it required almost as much stretch of the imagination to believe that such sounds could be produced in joints, as that they involved a supernatural agency. The anatomical conformation of the knee joint is

evidently most favorable for the production of loud sounds by displacement. The broad articular surfaces offer considerable space for lateral motion, provided the ligaments are sufficiently relaxed, and the requisite motor force is properly applied. The relative shortness of the outer condyle of the femur favors the outward displacement, and true dislocation in this direction would be likely to occur, were it not for the numerous strong ligaments which render this the strongest articulation in the body. Owing to the great protection afforded by the ligaments against injuries to which, from the position and relations of this joint, it is particularly exposed, dislocations are, in fact, very rare in their occurrence. The displacement occasioning the *knockings* is sufficient to remove the ridge of bone which divides the two articular surfaces of the upper extremity of the tibia, from its situation in the sulcus between the condyles of the femur, and to carry it, more or less, upon the surface of the outer condyle. This movement gives rise to the first sound, and the return of the bone to its place causes the second sound, which, in the *Rochester knockings*, generally follows quickly upon the first. We are unable to explain fully the precise mechanism by which the displacement is effected. In the case of the lady of this city who reproduces the *spiritual rappings*, the bone slips outward with very slight voluntary effort, and it is not easy, from her own account, or by manual exploration, to determine the particular muscles that are brought to bear upon the joint. In this case the displacement daily occurs, in bending the limb, when no effort is made to produce it, but, under these circumstances, it is not generally attended with much noise. The bone returns to its place directly the muscular effort which has produced the displacement ceases. To develop sound the displacement must take place with a certain quickness and force, and the latter may be graduated, in some measure, at will. A fulcrum of the foot appears also to be requisite as already stated. The lady just referred to is now able to produce the sounds in one knee only. In early life she had this power in both knees. From the number and volume of sounds produced, it is evident that both the knees of the Rochester rappers now in this city are endowed with sonorous powers. It might be supposed that the frequent repetitions of these displacements would produce after a time irritation and disease within the joint. In the case of the lady of this city they are followed by some soreness, but in early life, when she was in the habit of practising them daily more or less, she experienced no pain, nor any unpleasant consequences, and she was then able to develop louder sounds than she can at present. How rare are instances of that peculiarity in the

condition of the joint which admits of the audible phenomena that have given origin to the new science of *spiritual rappings*, we are unable to say. That they are not common is evident from the fact that the Rochester imposture has eluded detection so long; and that instances of a similar idiosyncrasy do occur, is shown by the fact that several *rappers* have appeared in different parts of the country. It is a sad commentary on human nature that the latter should prefer to have adopted and carried on the imposition when they discovered their peculiar power, rather than disclose the secret, and thus put a stop to the progress of the deception. Mrs. P., the lady of this city, to whom we are much indebted for the means of establishing the exposure to the satisfaction of the public, thus, deserves honorable mention, and the thanks of the community. A difficulty with some persons who have visited the *Rochester rappers*, in believing the sounds to be articular arises from the idea that the *raps* come from different quarters of the room, at a distance from the place at which the females are stationed. This difficulty involves several explanatory circumstances. In the first place, the sounds do not really come from a distance. It may seem that this is so, but it is a delusion, arising from not appreciating correctly some of the laws of acoustics. We do not ordinarily determine the direction from which aural impressions are received, save by the conjoined exercise of other senses. Variations in the supposed distance of the source of sound may be imitated, simply by variations in intensity of the sound, provided the source be not obvious to other senses than hearing. Upon these principles the deceptions of the ventriloquist are based. The ventriloquist does not transmit his voice in different directions, and at various distances, as is vulgarly supposed, but he graduates its intensity so as to make it appear more or less remote, concealing, at the same time, all the external evidences that he makes the sounds, and he relies upon directing, by his conversation, the attention of the audience to particular places, for the success of his effort to make it appear that the sounds proceed from these places. The knee knockings are muffled by the dress, and the slight movements are also thus concealed; hence, females make the best impostors in this line. The *raps* are then conducted by whatever solid substances are in contact with the limb, or body. The *Rochester knockers* prefer that their visitors should be seated around a long table, they sitting at one extremity of the table. Placing the limb, then, in contact with any part of the table, the knockings seem to be upon the latter. But if the limb is in contact only with the floor, the sounds will appear to come from below. The Rochester females, when they wish to give exhibitions of the

sounds, sometimes stand near a door. If they touch the door with a limb, or rest against it, the sounds seem to come from the door, and the door may be felt to vibrate. If they stand at a little distance from the door, the sounds appear to come from below. The *raps* do not, in reality, ever appear to come from much distance, unless the delusion is aided by a vivid imagination, or a degree of credulousness very easily operated on. The loudness of the sounds will, aside from the degree of motive power and quickness by which the displacement is effected, depend on the conducting properties of different bodies in contact.

That part of this scheme of imposition, which relates to the communications made by means of the *knockings*, opens a field of curious inquiry, not devoid of interest and importance. Admitting that the sounds are shown to be physically produced, and dependent on the volition of those engaged in conducting the deception, some, who have been impressed by the degree of penetration manifested in the accuracy of certain of the responses, and the striking character of the fancied revelations, will ask, 'How are these phenomena to be accounted for?' In accounts that have been published by many—we doubt not well-meaning and, on most subjects, sensible persons—there are statements which, to the reader who does not see fit to deny in *toto* the veracity and intelligence of the narrators, certainly must appear extraordinary. We do not propose to discuss at length this view of the subject. To do this does not belong to us, and would be inappropriate in the pages of a medical journal. We will offer but a few remarks.

Having traced the *knockings* to their source, explained the mechanism of their production, and thus divested them of their supernatural character and of all mystery, the field of inquiry just referred to presents an aspect different from that which it had prior to the exposition. While the origin of the sounds was unknown, the belief in their spiritual derivation would be entertained by those whose mental constitution and habits favored credulity in such matters, and the communications would be received with a corresponding degree of faith; and even some, not over credulous persons, might reason themselves into the conviction that the sounds must be due to intelligent, invisible spirits, from the apparent utter impossibility of accounting, on any other hypothesis, for the information thereby obtained. But assuming that the deception is unmasked, and the mode in which it is conducted satisfactorily explained, it follows, of course, that the communications are part and parcel of the humbug, and it only remains to show how it is that they are of a character to occasion surprise and

astonishment. This question might be disposed of, so far as the present subject is concerned, by saying that phenomena of the same character, and equally extraordinary, occur in connection with fortune-telling, into which it is not professed that spiritual agencies enter, and which no one supposes to involve aught beyond human sagacity. The question covers all the various modes of imparting pretended supernatural revelations.

Much is due to the laws of probabilities alone—in other words, many of the wonders are coincidences, which always occur in a series of random guesses. This plain fact is not always recollected, viz : That whenever a response involves either an affirmative or negative, the chances that it will be right or wrong are exactly equal. Guesses under such circumstances, in the long run, will be as often true as false. It may be admitted, however, that the whole philosophy of the matter is not resolvable into the laws of probabilities: other reasons must therefore be given.—Several reasons suggest themselves, some of which we will mention, without attempting to assign to them, respectively, their precise force.

A person of close observation and great shrewdness can acquire a degree of skill in furnishing communications purporting to be spiritual, which can hardly be appreciated by one who has not given much thought to the subject. This is a kind of acquirement not sought for, except by those who mean to make it subservient to deception ; and, therefore, by most persons is but little understood. Let an individual of proper capacity, make it a business to study the significance of every slight movement, intonation of voice, and expression of countenance, as criteria of concealed thoughts, and let this pursuit be prosecuted for years, under the incentives afforded by the love of gain or applause, and the fear of detection, and the tact thus acquired will be likely to develop results that appear almost incredible, and by the superstitious are regarded as divinations. This is one consideration to which not a little weight belongs.

Another explanatory consideration is as follows : Persons resorting to oracular communications, in proportion as their minds became excited, and full credence secured, can hardly fail to exhibit in various ways indications which are so many clues by which a practiced observer is led to apprehend facts supposed to be competely hidden. A person, who has been much interested in the *knockings*, and who believes that there exists a kind of mesmeric relation between the females and the questioners, by means of which the knowledge of the latter is perceived by the former, informed us that he observed those persons who had full faith that they should obtain true responses, generally got them, while those who were incredulous

were unsuccessful. We do not doubt the correctness of this observation, and it is fully explained by reference to the consideration just stated.

They, too, who become converts, are anxious to explain any errors and incongruities in the Sibylline responses, and are ready to accept explanations which are only pertinent by a large latitude of construction. They have an eager desire that what they seek to have communicated shall be communicated, and are ready to adopt any kind of interpretation which will secure the credit of the spirit which condescends to hold intercourse with them. It is sufficiently obvious to those who have made the *art of discovering truth by observation* a subject of study, that a pre-conceived notion often gives a bias even to the exercise of the senses. Not a few of the *false facts* of science are thus derived. Persons are apt to see precisely what they have pre-determined they shall see. How much more is it to be expected that this self-deception will be operative, when, instead of the sober realities of scientific research, the credulous mind is in pursuit of information to be imparted by miraculous means !

Again, the impression produced by successful hits in any of the arts of soothsaying or conjuration, is naturally greater than is consistent with a due regard to the failures. The number of the latter is forgotten, while the former are remembered, and thus acquire an undue preponderance.— More especially this consideration will apply to the prodigies related in written narratives, taking cognizance of those things which are only wonderful when isolated. The principle is the same as that upon which certificates of secret nostrums appeal to the confidence of the public. Admitting the certificates to be authentic, and even true in point of fact, we have only the extraordinary cures, without any of the host of cases in which the effect of the remedy was either nugatory or pernicious. These cases may predominate immensely over those in which benefit was attributed, while the latter, if considered exclusively, seem to furnish an overwhelming mass of evidence. We might add to these considerations, others ; but we have already said more upon this branch of the subject than we had intended, and perhaps more than the indulgence of our readers will lead them to excuse. We must offer as an apology for according to the subject so much space, in addition to the reasons before assigned, the personal interest in it growing out of the part we have taken in the detection and disclosure of the source of the Rochester Knockings. In engaging in this investigation, we literally followed the scriptural injunction, to “believe not every spirit, but try the spirits.” The result is an exposition, the correctness of which rests, in the first place, upon a

train of reasoning which we claim to be in itself conclusive ; and, in the second place, upon demonstrative evidence, tested by experiments which may be readily repeated and extended in all places where the *knockings* may be re-produced. It remains to see whether this result will succeed in bringing the career of this singular species of imposture to a close, and thus to say the least, diverting the current of credulity into some new channel.

Certain physical phenomena in addition to the knockings, are said to be occasionally produced in connection with the latter; such as moving of tables and chairs; opening and shutting bureau drawers; pulling the hair, etc., of persons assembled to witness the exhibition, and various other palpable demonstrations of what is claimed to be an unknown and mysterious agency. With regard to these phenomena we have only to say that none of them have fallen under our observation, nor are we aware that any have, as yet, been exhibited in this city, although we understand it has been intimated that they will appear by and by. Assuming that such phenomena do take place, we leave for others the task of explaining the mechanism by which they are produced.

The length of the article on the *Rochester knockings* excludes other editorial matter prepared for this No. The reader will observe that the publishers have added two pages to the present No. Several communications received too late for the present No. will appear in our next.

Buffalo Medical College. Commencement.—The annual commencement in this institution was on the 26th ult. The examination of candidates before the Curators took place in the forenoon, and the degrees were conferred in the afternoon at *Townsend Hall*. The public exercises were opened with prayer by the Rev. Dr. Shelton. In the absence of the Chancellor of the University, His excellency Millard Fillmore, the degrees were conferred by Dr. T. M. Foote, late minister to Bogota. The address to the graduates was delivered by the Dean of the Faculty, Prof. C. B. Coventry. It was a sterling address, replete with sound judicious counsel to those about to enter on the duties and responsibilities of the practice of medicine.

The number of graduates was thirty. We defer the names, for want of space, till the next No.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Shoulder Accidents.* Reported by Dr. FRANK H. HAMILTON, one of the Attending Surgeons of the Buffalo Hospital of the Sisters of Charity.

Case 1. Error of Diagnosis.—Michael Crichton, aged 30, a laborer; admitted Nov. 22d, 1849. Crichton was caught in the machinery of an elevator, on the day before admission, by which his right shoulder was dislocated, also the thumb and middle finger of left hand, and various other slighter injuries were received. These several dislocations were reduced, and the wounds dressed by Dr. Alden Sprague, before the patient was sent to the hospital. The *left* shoulder was injured also, but not particularly examined, as the patient had already suffered very much and needed rest. The Doctor, however, sent a message calling my attention to the left shoulder. On the 23d I found a fracture of the left humerus through its surgical neck. Of this no doubt existed, for the ends could be made to crepitate, and also to bend freely upon each other. I discovered also the head of the bone in the axilla, and supposed it was dislocated in that direction. The swelling was considerable, and temporary dressings were applied.

On the 28th, seven days after the accident, I consulted with Dr. White, and we concurred in the above diagnosis.

On the 29th, I counseled with Dr. Sprague, and we also being agreed in opinion, I proceeded with Jarvis' adjuster to attempt a reduction of the dislocation. The patient was advised that we should probably not succeed. The extension was continued nearly thirty minutes, and no alteration in the position of the bone was effected. It must be remembered that the fracture of the humerus was at a point so near the joint that it was impossible to seize upon the upper fragment, and we only hoped to succeed in the reduction through the aid of such soft attachments as might continue to exist between the upper and lower fragments, and by pushing upon the head of the bone as it lay in the axilla.

Renouncing all expectation of ever reducing the dislocation, we adjusted the fracture of the humerus, and applied the usual lateral splints and roller, placing a moderate pad in the axilla and thus securing the arm tightly to the body. The patient was too ill to leave his bed, and lay on his back with his left forearm across his chest.

The same dressings were continued with only occasional removal for the purpose of examining the fracture, until the 1st of January, 1850, nearly six weeks, when the bandage and splints were finally laid off. The fracture was united.

I now discovered also that the head of the humerus was exactly in place, and so it has remained ever since. The motions of this shoulder are as extensive and free as before the injury, and much more so than the right shoulder. There is no depression under the acromion, or fullness in the axilla. In short, the shoulder is perfect.

The conclusion to which I am forced is, that what we supposed to be a dislocation was a fracture of the neck of the scapula, allowing the head of the humerus with the glenoid cavity to fall into the axilla, and that by means of the axillary pad and the band securing the arm to the body, the reduction was effected without our knowledge or intention. This explanation is the only one admissible.

Case 2. Dislocation of Shoulder, reduced at the end of eight weeks.
John Bowles, aged about 45, admitted Jan. 8, 1851. Dislocated his left humerus into axilla just eight weeks before admission into the hospital. No attempt had been made at reduction. John is a moderately stout-built laborer. The shoulder was examined by Profs. Lee, Coventry, Webster, Ford, with several other medical gentlemen, who were also present during the reduction. The arm was one inch and a half longer than the other!

Having administered chloroform, I applied Jarvis' adjuster; but as soon as the bands were tense the patient became wild and unmanageable, and

as we persisted in the use of chloroform he became more violent, and finally actually convulsed throughout his whole body. I therefore discontinued the chloroform. Extension was made through the medium of the adjuster, and by every other means, and in every possible direction, during an hour, with only occasional intermissions. At this time the attachments about the joint were evidently broken, and seating my patient on a chair, with my knee in his axilla, I reduced the bone with great ease. It could also be displaced with like facility and again reduced. The arm still remained an inch and a half longer than the other, but could be brought to its natural length by pushing the elbow upward.

A pad was placed in the axilla, and a sling under the elbow, and the arm secured to the body. In two weeks he was dismissed, the arm still in its place, and only very slight inflammation having resulted from the operation of reduction. The arm was now only three-quarters of an inch longer than the opposite, and could be made of the same length by lifting the elbow.

Case 3. Amputation at Shoulder Joint.—William Lynn, aged 28; admitted Dec. 30th, 1850. William was in the employ of the Buffalo and Rochester Railroad Company, and was managing the plough while the train was attempting to penetrate a heavy bank of snow, when he was thrown under a laden car in the rear of the plough. Four wheels traversed his right upper extremity, one passing over the hand, one over the forearm, one over the elbow, and the fourth over the arm near the shoulder joint. At the last point every thing was severed but a portion of the skin and the axillary artery—the muscles were torn entire from the shoulder and thrust through a rent in his flannel shirt and three heavy woolen coats.

I made the amputation in a shanty beside the railroad, on the night of the 29th of Dec., assisted by one of my pupils, Mr. John Boardman, with no light except what was furnished by two or three tallow candles.

We first tied the brachial artery, as it lay exposed under the skin but not ruptured. The skin was then divided sufficiently low to cover a portion of the humerus if we should conclude to save its upper fragment; but the muscles were so torn out and lacerated quite to the joint that it was deemed expedient to disarticulate; in accomplishing which an artery, probably the posterior circumflex, bled so freely as to excite some alarm. Our tallow candles shedding an imperfect light into a deep and irregular cavity, rendered it difficult to secure the vessel. Pressure, however, with the finger upon the subclavian artery just *below* the clavicle, restrained the

bleeding while Mr. Boardman sought out the vessel and applied the ligature. The patient having lost much blood before we arrived, became extremely blanched almost immediately after this bleeding commenced, and I am quite certain he must have been lost had it not been for the prompt and efficient aid rendered by my student.

The flaps were closed with sutures, straps, &c., and the wound has healed kindly.

ART. II.—*Letter from a Correspondent at Paris.*

Paris, Feb. 6th, 1851.

MR. EDITOR: *Dear Sir*,—In accordance with my promise, I send you a short article, containing a few items relating to our profession, which may be of interest to your numerous readers. Though no great excitement prevails in the medical world in Europe at present, yet we have occasionally an invention or discovery here which may not as yet have reached America in print.

The course of lectures this winter at the *Ecole de Medicine* is attended by about 1,600 students of all ages, and all nations from Ethiopia to Lapland. At Orfila's chemical lectures may always be seen mingled together, a mottled group, comprising the tall, intelligent Englishman, American and Scotchman,—the robust, studious German,—the gay, effeminate Frenchman,—the jaundiced, taciturn Italian and Spaniard,—the yellow-skinned Greek,—the small-eyed, dwarfish Turk,—the copper-colored Malay, and the woolly-haired, sable African. And though there has been little sympathy in common between the descendants of Shem, Ham and Japheth since the flood, yet here there appears to have taken place a re-union of the alienated members of the great family of Noah—here they meet on a common "platform" for the study of those sciences which constitute the noble profession, which every true disciple of *Æsculapius* must admire.

The winter course is given by Messrs. Andral, Berard, Malgaigne, Becquerel, Orfila, Denonvillias, Adelon, Gavarret and Jerdy. Many operations are to be seen daily at the hospitals. Private courses by the older graduates of the school are constantly going on, in all the various branches of medicine and surgery. The prices of these courses are exceedingly low: the lecturers though at present mostly men of second and third rate reputations, are still, some of them destined to higher places.

Becquerel states in his lectures that cholera has re-appeared in the Hospital La Pitié; but we have heard of none elsewhere.

A chemist here, whose name I have not yet learned, claims to have discovered a new compound resembling chloroform, one or two drops of which will produce anesthetic effects sufficiently powerful for any surgical operation.

M. Andral has lately promulgated in his lectures a theory which he claims to be new, and in which he attempts, by reasoning and statistics, to prove that pulmonary consumption is more prevalent and fatal in warm, than in cold climates. Against this idea there appear to be two objections—first, it is by no means new—second, the statistical tables do not appear to embrace all the various conditions necessary to make them conclusive.

M. Jobert has lately been successful in several cases of recto and vesicovaginal fistula, by a new mode, which consists in making incisions in the mucous membrane at a little distance from the edges of the wound to be united, so as to allow the parts to glide towards this point, and thereby avoid the tearing of sutures by the tension which takes place after the parts begin to swell and maturate.

M. Piorry has written out in detail a new nomenclature of pathology, which may have some advantages, but it is not yet adopted. He has also advanced some new ideas in relation to the pathology and treatment of hypertrophy of the spleen. He finds, or appears to find, in the Hospital Charity, many cases of this disease, with which he connects a numerous train of symptoms. He claims common salt, (muriate of soda,) to be a specific for this form of disease. He defines the borders of the organ by the pleximeter, and maps it out with a pencil; he then gives the patient about two drachms of salt dissolved in water, and waits for the effect, which he says, in some cases is almost instantaneous. In a few months, according to *his percussio*n, the spleen has been reduced to one-fourth or one-third its former size, and in a few days a perfect cure is obtained. A circumstance corroborating this theory is the fact that in France most kinds of food are eaten with little or no salt; if then hypertrophy of the spleen may follow the withholding from the system the necessary amount of this element, a supply of it to the system may possibly cure the disease thus produced. But further experience alone can establish the fact and remove the jealousy with which the profession very justly receive all new theories.

The old practice is still prevalent in Parisian hospitals, of stuffing wounds and sores full of charpie, so as to prevent union by first intention, or healing without profuse suppuration. This practice seems to be partly the result of a thorough prejudice against every thing like English surgery, and partly

from the erroneous idea that it is always necessary to allow the formation of matter.

Now, although the hospitals of Paris afford the widest field for the study of disease of any on the globe, and although French instruments and manual dexterity stand unrivaled, still, without any prejudice or disrespect to the profession here, I seriously doubt whether surgery is among the curative means of the profession. The hospitals must always be full, because all the poor, and "their name is legion," are prescribed for as in or out patients of these institutions; and when a patient has the misfortune to enter one of them, he is prepared by the treatment to be an inmate, more or less, to the end of life. The coolness with which fearful experiments are carried on for the purpose of establishing personal reputations and private theories, is astonishing to any one who is in the habit of placing any value upon human life, or feeling sympathy for suffering humanity. It is true, this practice sometimes develops a great and useful truth; but the end in this case does not sanctify the means used. Common benevolence ought in all cases to be guaranteed to the poor and sick of our unfortunate fellow men, between whom and ourselves there is but a single step.

A young man in Paris will need be constantly on his guard, and be well fortified by sound general principles, to avoid the danger of *false science*; he must resist the allurements of show, and the eclat of "*cutting*," or the community to whom he ministers the healing balm will have reason to regret his "tour to Europe."*

Yours respectfully,

M. M. RODGERS, M. D.

Report of Medical Cases. Communicated by S. C. ROGERS, M. D.

Addison, Steuben Co., Feb. 15, 1851.

DR. FLINT: *Dear Sir*,—I take the liberty to send you the history of a few cases that have come under my treatment and observation. I give them to you as I find them in my note-book.

Case 1. William Beach, M. D., aged 40, plethoric, large head and short neck. Saw the patient at 4, P. M., on the second day after an attack of neurosis, located in the tibial nerves, seat of the pain midway betwixt the ankle and knee. The pain was of the most excruciating character. The patient took, of his own accord, some five or six ounces of paregoric, which produced profound sleep. An emetic was administered, which evacuated the contents of the stomach; was bled in the foot, and a blister over the seat of pain. At 7, P. M., on the second day, he again took two and a half

* We hope to hear frequently from our correspondent.

ounces of paregoric, which produced a deep sleep—stertorous breathing, no effort being made to counteract its effects until after twelve hours from the time it was taken. At this time there was rigidity of the muscles of the face; copious perspiration. It was at this time that the patient came under my treatment. I then made use of friction, strong coffee, calomel, cold to the head, mustard to the feet and stomach; pulse at this time very full and slow, head hot, face flushed, sweat profusely. This state of things lasted for forty-eight hours, when with difficulty he could be aroused to answer some questions. As consciousness returned, the pain in the leg returned, but not so acute, and gradually disappeared, by the use of morphine and aconite. There was a partial paralysis of the extensors of the leg. I will here say that this paregoric was prepared by an inexperienced apothecary, and was nearly if not quite the strength of laudanum.

Case 2. J. A. P., aged ten months. Feb. 13. This child was of good proportion, head of a medium size; and from its birth till within the last six weeks had been remarkably healthy. At that time (six weeks ago) the upper incisors made their appearance, and since that time has been "worrisome," its head and surface generally of natural temperature; pulse did not appear to be accelerated, bowels constipated. Prescribed magnesia to move its bowels.

14th. General appearance of the child unchanged; no movement of the bowels. Gave injection; after it passed, repeated the magnesia in laxative doses.

17th. More worrisome; no febrile action; desired the breast continually. Some vomiting had followed the use of tea and some other domestic drinks. Prescribed mustard over the stomach, and to the soles of the feet; six grains calomel in two grain doses, once in three hours, followed by castor oil and spirits of turpentine.

18th. A free evacuation of the bowels; continued to "worry," restless and uneasy, no improvement.

19th. Saw the patient with a neighboring physician. No movement of the bowels since the previous day. No increased heat in the head, but continued to worry; would nurse the most of the time. Its parents had been in the habit of giving it paregoric ever since it commenced teething; they gave it last night 18 gtt. which produced much excitement. A few of the first times we gave paregoric, it would sleep soundly all night; afterward vomiting occurred after it had swallowed it. This led to an examination of the paregoric, and it was found to be of the same quality or strength as that used in the above case.

From these facts we came to the conclusion that the irritation caused by teething was less than the injury received by the "paregoric," and that its use, which had been daily, should be entirely suspended, and, so far as was possible, to counteract its effects by acidulous drinks, and magnesia to obtain and keep up a laxative condition of the bowels. Solution of Hydriodate of Potassa as an alterative and diuretic.

24th. Since the last date there has been little improvement, if any. No increased heat or tenderness of any part; bowels constipated—quiet only when nursing.

25th. Child worried incessantly, throwing its arms, countenance indicating distress. Slight strabismus. Eye-brows knit, head cool, pulse feeble. Blisters behind the ears and between the shoulders; wine and quinine.

26th. Blisters well filled, tongue slightly furred at the back portion; slight strabismus, vomiting occasionally. Continued the tonics.

27th. Saw the patient in consultation with two neighboring physicians. We diagnosed at this time a state of congestion of the brain. Prescribed calomel in three grs. doses, once in three hours, the third one followed with oil and turpentine.

28th. Calomel had worked off well, pulse small and feeble, skin pale and cool. Prescribed calomel, quinine, ammonia, as one grain, once in four hours. Hydriodate of Potash had been used from the commencement sufficient to keep up free diuresis.

29th. Strabismus; pupils dilated, but would contract by bringing it to the light. Continued the tonics and alteratives with as much nourishment as it would take.

March 2. Convulsions, followed by hurried and difficult breathing. Expired on the 3d in convulsions extremely painful to witness.

Autopsy revealed no congestion of the vessels—the membranes presenting not the slightest trace of inflammation. On dissecting into the brain—opening into the ventricles, the water gushed out from the right not less than two ounces, and one and a half from the left ventricle.

Case 3. S. C., aged 63, called upon me, Nov. 30th, to consult me in regard to his health. He had some weeks previous contracted a hard cold, leaving him with a cough of which he was nearly well; he has, slightly, bronchitis; countenance of a yellowish cast, tongue clean, pulse 70, small and feeble, skin dry but not feverish, appetite good; eats hearty food, and had daily evacuations of the bowels; has no pain any where about him, but has observed for the last three months that his strength was failing him.

Upon making sudden exertion he became very much exhausted. I was unable to decide what the true cause of this debility was, for it seemed to be general—concluded that the liver was most in fault, and prescribed 12 grs. calomel, in three doses, comb. with Dovers Powders, to be taken once in four hours and followed with oil.

Dec. 2. The calomel had thoroughly evacuated the bowels. Prescribed carbonate of iron and quinine aa two grs. three times per day.

4th. Gums slightly affected; said he had the ague in the face. The right cheek was swollen, and at the mouth of the salivary duct there was a patch of dead membrane of the size of a dime; this was daily penciled with nitrate of silver, and with the use of astringent washes became healthy. Examined the chest by auscultation and percussion, but could detect no disease of the lungs other than in the bronchial tubes. No local disease could be detected but that, and that but slightly. Put the patient upon small powders of ipecac in expectorant doses. Tonic bitters of wine, iron and quinine three times daily, with nutritious food. My views of the case and treatment were entirely unsatisfactory to all except the patient himself. The friends desired counsel, to which I consented. My associate diagnosing tubercular deposits in the lungs and hepatization, the patient remained under my care, and the tonic treatment followed.

Jan. 10. Additional counsel called, who sustained my early diagnosis, viz., general debility from an unknown cause. Continued the tonic and expectorant treatment, with laxatives and nutritious diet. Under this treatment he gradually improved; the patient was discharged with the advice that he continue the tonic bitters, with bathing and friction. By the advice of a neighboring physician he took ox gall and cod liver oil with no marked effect. So long as the above treatment was followed his health and strength permitted him to walk forty or fifty rods and back, but it was apparent that the solids were being converted to fluids. He was very pale, suffered none from pain, rested well nights, ate very hearty food, bowels regular.

Feb. 14. Patient failing in strength.

March 16. Expired without a struggle.

Autopsy revealed the following appearances:—The bronchi presented the usual appearances following a slight degree of bronchitis. Lungs perfectly healthy; kidneys and liver normal. On separating the vena cava a substance having something the appearance of a tape worm, 6 inches long, 1-4 broad, and 1-16 thick, tough, and of a light color, was found; tracing the vena cava along, the same substance was found in the auricle; and in the right ventricle a tough and well organized membrane was found adher-

ing firmly to all the carneæ columnæ and the walls of the ventricle; it was from 1-16 to 1-8 of an inch thick.

Case 4. March 24. Mrs. W., aged 45, the mother of several children. I was called in consultation with Dr. Wagoner, and Dr. Brown the attendant physician. This patient, until the last thirteen days, had been treated for gastritis. Within this time she has taken neither food nor medicine, nor a drop of any thing except cold water; and has suffered less foöm pain than while under treatment. Patient not much emaciated; eyes bright and expressive; skin dry and cool, pulse 76, bowels constipated. Much tenderness over the cervical vertebræ, over which a blister was drawn. The only thing she could be prevailed upon to take was a spoonful of wine whey, which was immediately ejected; repeated the second time, and with a like result—it being the last she ever swallowed except cold water. On the thirty-first day of abstinence there was an evacuation of the bowels. She expired after abstaining from medicine or nutriment for *fifty-three days*.

Autopsy revealed that the thoracic viscera were healthy. Abdomen—liver normal; gall bladder somewhat distended with a black fluid; stomach of its natural size, containing not more than one ounce of a cream-like substance—its mucous coats were thickened, more particularly at its cardiac and pyloric orifices, but no stricture. The mucous membranes of the duodenum were much thickened and of dark color. No examination of either the spine or brain.

Case 5. D. E., aged 38, a very healthy and hard-laboring man, received an accidental blow from the point of a chain-hook while engaged in hauling square timber, on Monday. This blow stunned him, but he soon recovered himself, and worked the remainder of the day and every day during that and the subsequent week. Sunday and Monday he had much pain and heat in the head, and was insane. Tuesday, became stupid, a physician was called, but made no examination of the injury. Wednesday night, saw the patient, found him in a heavy sleep, pulse full and slow, not easily aroused, eyes suffused, tongue thickly coated, much heat in the head, but the friends said it was not as hot as it had been; paralysis of the left extremities, incontinency of urine, answering questions slowly but rationally, had a dull heavy pain in the front and top part of head. Upon examination a fracture was detected at the point of injury, a little to the right of the sagittal suture and anterior of the coronal. The fracture would only receive the end of the index finger, but there was a depression at least one-eighth of an inch. I bled him largely from a large orifice in the upright posture, producing fainting, which much relieved the pain in the head; active

purge, cold to the head, sinapisms to the feet, solution of antimony hourly. Left the patient somewhat more comfortable, but expressed the opinion that the bone should be elevated—the patient and friends wishing to avoid the operation if possible. This was the sixteenth day since the injury was received.

17th. Saw the patient with an associate who agreed with me as to the fracture, but thought we were not warranted in operating. There was less pain in the head than on the day previous—otherwise no improvement. Blister to the nape of the neck.

19th. Saw the patient in consultation with three other physicians—patient's symptoms unaltered. Two of us were in favor of delaying the operation; the operation was consequently deferred, but he was kept strictly upon the antiphlogistic treatment. During all of this time patient was disposed to be stupid and taciturn.

24th. Patient had become comatose. The operation of trephining was then made. Dr. Beach and Dr. Foot were present and rendered their assistance; after making the flap over the point of injury, the trephine was planted upon the uninjured bone, taking in about one-half of the fractured portion, and carefully sawing through the bone, that portion encircled by the saw was removed by a strong pair of forceps. Several spiculæ of bone were removed, from two to four lines in diameter, besides many smaller, some twelve in the whole. The dura mater presented a lacerated appearance, and seemed to press into the aperture made, with considerable force. An opening was then carefully made through it, but very little pus, if any, was discovered. After all hemorrhage, which was but little, had ceased, the flap was turned back; one stitch was taken at the point of the incision, the remainder of the dressing was adhesive straps. Patient was conscious after the operation, and said he was not sorry it was done.

First day after the operation, an increased heat in the head, but rational; kept the head wet with ice water, bowels moved. Blister applied to the nape of the neck, and antimonial solution hourly.

Second day, could not be aroused, nor could he be made to swallow. Died in the evening of the second day.

Autopsy. After removing the calvarium, the vessels were observed to be much congested. Making an incision in course of the longitudinal sinus and removing the dura mater from the right lobe, the surface of the brain presented a normal appearance. Opening into the right ventricle it was largely distended with well organized pus, containing not less than two ounces. For want of time the dissection was arrested.

ART. IV.—*Letter from Prof. J. P. White.*

The following letter from Prof. White, will be read with interest. It is the first of a series with which we hope to be favored during his sojourn in Europe.—EDITOR.

Rouen, Feb. 5, 1851.

After visiting the various objects of interest in this city, I was much gratified with an interview with the celebrated physiologist and naturalist, M. Pouchet. He is slight in form, rather below the medium height, high forehead, and a piercing black eye, fine countenance, and about 45 years old. It is to M. Pouchet, more than to any other physiologist, indeed, in my opinion, more than to all others, that the profession is indebted for experiments which establish the existence of spontaneous ovulation in all the mammalia. But a few years since it was the established doctrine that the influence of the male was necessary for the formation of the germ or ovule. That after a fruitful connection the ovary was excited to action, that then, and never until then, an ovule or germ was secreted or formed, which was subsequently transmitted to the uterus, where it was matured and prepared for external life. It was of course, therefore, supposed that the eschar formed by the escape of the ovum from the ovary, or *corpus luteum*, was never to be found except as the result of fruitful coition. It was farther supposed that the number of the *corpora lutea* corresponded to the number of pregnancies of which they were sufficient evidence.

It was held, also, that menstruation fitted and prepared the uterus for conception, but it was not known that there was a simultaneous congestion or engorgement existing in the ovaries.

This whole theory has been, by the valuable experiments of M. Bischoff, Raceborski, and especially by the labors of M. Pouchet, of Rouen, entirely overturned. It is now ascertained beyond all doubt, that at each menstrual period in the human female, an ovum is secreted or formed, that the ovisac is ruptured, and an ovum or ovæ escape irrespective of the influence of the male. During the child bearing, or menstruating period then, or from about the 15th to the 45th year, females, once in about 28 days, discharges an ovum, or egg, which may be fecundated, absorbed, or discharged, and it leaves an eschar, which cannot be distinguished from those which have been fecundated and retained to the full period of uterogestation. The same observers establish the fact, also, that in all the mammalia at the rutting season, or season of heat as it is termed, the same thing occurs whether connection be permitted with the male or not. All who desire may, by consulting the atlas of beautiful plates of M. Pouchet,

find there shown the ovaria of the sow, the sheep, and the hare, containing ova fully matured in the young animal which had been carefully secured against all intercourse with the male. Indeed M. Bischoff has examined the ovarian tube of many of the inferior animals which had been secluded, and in several instances been so fortunate as to find the ova in the fallopian tube, with the cavity which it had just left filled with blood, and the corpus luteum forming. Other experiments show the graafian vesicle in varying degrees of development upon the same ovary, some fully grown and the sac ruptured, others still less advanced, whilst in another instance where the animal was killed, or the ovary removed at a later period, the sac was found shriveled and approaching the condition when it is termed the yellow body, or corpus luteum. In the few instances which have been afforded of examining the human ovary where death has occurred during, or immediately subsequent to menstruation, they serve to establish the same theory. It seems certain that at each menstrual period the ovaries become engorged, a higher state of vital action is induced, one or more ova increase in size, its capsule is ruptured and it is discharged. If it come into contact with the *liquor seminalis* of the male, it may be fecundated, but this is entirely unnecessary to its maturation in the ovary. That ova exist in the ovary of the young girl, is also well ascertained, and that *corpora lutea* are to be found in the virgin, is conceded by most physiologists of the present day. It would appear that the ovary is the organ primarily excited to action, and the critical sanguineous discharge from the uterus is the result of this exalted action in the ovaria. This doctrine assigns to the ovaria a more important position in the female generative apparatus, than has heretofore been given them, and makes their pathological condition doubly important to the practitioner who would treat understandingly the various derangements of the menstrual functions.

Not only does this doctrine of spontaneous ovulation extend to man and the mammalia generally, but to the entire animal kingdom. The Batracian or frog species, is a familiar example of ovulation without the influence of the male. If when the ova have been deposited the male semen is brought in contact with them, then they are fecundated, but it is accomplished externally and after the eggs are deposited.

It may be added, in support of this theory, that the anatomical characters of the human ovary, at the various periods of female life, are precisely what would be expected. In the very young girl they are small and firm, in the young menstruating female they are much larger, or more developed, smooth, of a shining whiteness much more embossed. In the female who

has menstrated many years, they become roughened, or seamed with cicatrices, and finally, in the old woman they are diminished or greatly shriveled, returning to their original proportions.

I might dwell at much greater length upon this subject, but you are doubtless already familiar with these matters, and I will subscribe myself

Your friend,

JAMES P. WHITE.

ART. V.—*Case of Internal Otitis, extending to Brain. Fatal.*

Janesville, Jan. 25, 1851.

DR. A. FLINT, Editor of the Buffalo Med. Journal.

I have just lost a patient—a girl fourteen years old, my own niece, and daughter of Gen. Crabb, one of the Editors of the "Badger State"—which has created much excitement in this place, from the fact of her having been a general favorite, by reason of her beauty and amiability. She was indeed the May-flower of this place. In a medical point of view, the case, to us of the profession, was of deep interest, and a report of it may not prove altogether useless. Another cause of interest to all parties was the association of a brother's death of about the same age, some five years since, and from a similar disease.

During a severe sickness of a younger sister, whom I was attending, and at its crisis, this one was attacked with pain in the ear, and my attention being confined (too much, perhaps) to the patient spoken of, and the ear-ache being a malady generally left with mothers, my attention was not directed to the case till several days thereafter. Upon examination I found pain, not exactly within the meatus, but behind it, and in the region of the mastoid portion of the temporal bone of the left side, with tenderness of the integuments and swelling extending down the neck. Mumps were at first suspected, but the mother being quite certain that the patient had had that disease, and upon further examination the parotid gland not appearing to be involved, the disease was located in the mastoid cells and other parts of the internal ear together with the integuments in that region.

The patient had taken previously one or two cathartics, and fomentations had been applied to the part.

The pulse now was 120, and somewhat hard. Continued fever, but not high. Tongue white. Skin husky and dry. Great nervous excitement, but mind perfectly sane. The pulse did not fully warrant blood-letting, and other antiphlogistic means were substituted; but the symptoms not

being abated the next day, venesection was resorted to; but few ounces were taken, however, before syncope was produced. The patient remained faint and sick at the stomach a considerable time. As she rallied, a gentle perspiration ensued, and the pain considerably abated. The blood exhibited the buffy coat, but was not cupped. A full dose of calomel was then administered, followed with oil; free catharsis was effected, stools dark and bilious. The patient was now put upon antimony, which was at times pushed to vomiting and prostration. When steadily kept under its influence, moisture of the surface, abatement of pain and of all other symptoms were manifest. Blisters were applied to the sternum and shoulder. None was applied to the back of the neck, for two reasons. I feared the consequence of the too close proximity of a blister to the swelling, and, more than all, the difficulty of getting at this part, in consequence of pain in the neck, occasioned by change of position, which had become intolerable when such change was attempted. Drafts and heat to the feet were constantly applied, with an occasional foot-bath. Volatile liniment was applied to the swelling, and the bowels kept open. Notwithstanding these rigid means, the pulse increased in frequency although not hard. After the lapse of four or five days she complained of some pain, generally over the head, and particularly in the occipital region, as well as in the back of the neck. A slight preternatural heat of the head was observed, and cold applications were accordingly directed. The mind continued clear. Some ten days after the attack, and about 9 o'clock in the evening, collapse suddenly ensued, from which she rallied by the administration of brandy and quinine, and had a comfortable night. In the morning the pulse had gone down from 150, the day previous, to 120, and tongue cleaned.

Doct. A. Clark, of Beloit,—who was then at my house for the purpose of accompanying me to the Medical Convention and meeting of the State Medical Society, at Madison, and who had seen the patient the evening before, both previous to, and after the collapse—agreed with me that the crisis was past, and that the case was terminating favorably. The external swelling having greatly subsided, all other symptoms materially improved, and the patient expressed herself as being quite comfortable. We accordingly concluded that we might leave her with safety in the hands of our friend Doct. Lewis, of this place, who was called in consultation for that purpose. We were absent from Tuesday till Saturday, when I found my patient very low. Doct. L. having been necessarily called away, a homœopathist was called in, matter having formed behind the ear. As

strange as it may seem, this dupe to the infinitesimal humbug very judiciously opened the abscess. This practitioner, however, be it remembered, had been taught how to use the lancet in a regular school, being a renegade from the profession.

All hopes of recovery were now abandoned. As stated, the patient was very low—the pulse varying from 140 to 150—low grade of fever—muttering delirium and sordes of teeth and lips, with total deafness of both ears—the deafness having accrued on the preceding Wednesday. Two erysipelatous blotches made their appearance on the cheek. Still, remedial means were continued, even against hope—as blisters to the thighs, drafts to feet, head shaved and cold applied thereto, and when inclined to sink, the latter withdrawn and brandy and quinine were employed as required. The patient, at times, was so sensible as to read questions put to her upon the slate and answer them rationally. Much of the time during these periods, she spent in prayer, using language that transfixed the bystanders in astonishment, by reason of its beauty and elegance of diction. She often spoke with tranquillity of dying, lectured her parents and such of her school mates as were permitted to see her, in the most feeling manner, concerning a preparation for death, took an affectionate leave of them, sent for her minister and gave him a text from which to preach her funeral sermon, &c. Thus she remained some forty-eight hours, when death closed the affecting scene.

I have mentioned these and other details, that you, and others, may explain and reconcile them with the autopsy yet to be given, which, I must confess, in all respects, I, at present at least, am unable to do.

Post Mortem three hours after Death.

Dura mater, Pia mater, and Arachnoid injected. Small depositions of pus were found at different points upon all these membranes. Sinuses and large veins engorged with semi-coagulated blood. The ventricles were filled with serum, and other cavities contained both serum and pus. Portions of the brain were soft, and the remainder dotted with red points. Upon removing the cerebrum, matter oozed from the internal ear through the meatus auditorius internus, where it, to the amount of a teaspoonful or more, had already collected. This matter was traced to the base of the cerebellum, which, upon being detached from the medulla oblongata, at least a table spoonful of clear pus was observed to lie in the foramen magnum. How far this matter had penetrated the spine was not ascertained but a large portion of the spinal cord appeared to have suppurated. The semi-circular canals, &c., were found softened and carious, and the whole

internal ear filled with black, fetid matter. The superior part of the mastoid portion of the temporal bone was carious and perforated to an extent of two lines by five, communicating with the external abscess, which, upon being laid open, contained a thin layer of black, fetid matter.

Since the death of this child, I am informed by the parents that a discharge of matter from this ear was, from an early period of her childhood, kept up, till within about six months of her death, about which time menstruation commenced. From which time, also, there was a manifest improvement of health that previously had been delicate, but now soon became apparently perfect, cheeks rosy, features rounded, and puberty fast developing itself.

I should be very happy to find some remarks in your Journal upon this case and practice, by you or some of your distinguished faculty; for to the less favored and enlightened practitioners of this western village, it is somewhat novel. And although God forbid that it should ever be my lot to treat another case of the kind, still, whether or not this be the case, others of the profession may be benefited, and such doom to other gentle and cherished flowers of the far-west be averted.

Yours respectfully,
J. MITCHELL.

ART. VI.—*Assimilated Rank in the Navy.*

The efforts of the Medical officers of the Navy to obtain their just and reasonable rights as respects an assimilated rank, are, as yet, ineffectual. The subject is one in which the whole profession of the country should feel an interest, and such aid as different members of the profession are able to render in the undertaking, should be cheerfully bestowed. We concur with the Editor of the Philadelphia Med. Examiner in the opinion that the attention of medical readers should be invited to the subject by means of communications in the different Journals, and, if consistent, we shall reproduce hereafter the extended and able editorial article contained in the March number of our contemporary just referred to. In the mean time, we take the liberty of inserting a letter written by one who is not only conversant with the merits of the subject, but who is so situated as to be able to appreciate fully its importance to our brethren in the naval service. We feel compelled reluctantly to withhold the name of our much respected correspondent, inasmuch as we assume the responsibility of publishing the letter.

EDITOR.

MY DEAR DOCTOR,—You are aware that for some years, in truth, almost from the foundation of our service, an effort has been making by the medical officers and other staff corps of the Navy, to be put upon a footing of rank and respectability proportioned to the importance of their duties, and which would relieve them from relations of uncalled for humiliation with the officers of the Line.

A small body of professional men, but few of whom are in the United States at the same time, cut off, by their duties, from the great body of their profession, have maintained this contest against the vanity, the prejudices, the numbers, power and influence of the whole line of the Navy. It has been a long and vexatious struggle, such only as truth and justice could sustain against such odds. So manifestly right were the claims of the staff officers, and so vexatious were their grievances, that the Executive came finally to their relief, and issued a regulation which put the medical corps upon a tolerably respectable footing. That is, it declared that the older surgeons should be assimilated in rank with the lowest grade of commanding officers of the line, other surgeons with lieutenants, passed assistant, and assistant surgeons after a class of officers called "masters," but who are in reality passed midshipmen. This was not much of a lift for the *profession*, but it was an amelioration of the condition of that portion of it doing duty in the Navy, and with it the medical officers were satisfied. But the Line chose to consider itself so fixed in its privileges, so unapproachable in its aristocracy, that it entirely disregarded the Executive regulation, and drove the staff corps to seek the protection of an act of Congress. In this stage of the matter the medical profession, throughout the length and breadth of the land, came nobly and generously to the aid of its nautical branch, and through individuals, societies and Associations memorialized Congress in its behalf. Had the measure been brought up in Congress at this time, it is believed that the position of the staff corps would have been fairly and honorably settled; but unfortunately such a reference was made of it as called for the appointment of a Board of officers to report upon the matter. Some years ago a similar Board of officers of the Line was appointed by the Executive, and it made a report so prejudicial and partial that no attention was paid to it. When the present board was seen to be composed exclusively of officers of the line, some of whom had been active in their opposition to the assimilated rank of the staff, of course no hope was entertained of any thing favorable to the interests of these corps. The Board has made its report, and good may result from the proof it gives the public that the complaints of the staff officers

were neither imaginary nor exaggerated. The Board seeks, not only to roll back the medical corps from the position it has attained, but to legitimate every evil of which the staff corps have complained. I will briefly sum up its recommendations.

It recommends that the use of the word "rank" in relation to staff corps be abolished, and confined only to the line. In this the Board violates all usage, civil, ecclesiastical and military, and aims at legitimating castes, marked by specific terms. It recommends an order of precedence, which places the most senior staff officer behind the most juvenile of the grade with which he is assimilated, although it assimilates him below his present position.

It provides that at mess tables, on boards and surveys, the senior sea-officer, or officer of the line shall command and preside over all staff officers associated with him, no matter how senior may be those staff officers. It has heretofore been held a principle of military service, it is such in the British army, that there is no rank at the mess table, and the only presiding officer is the "caterer," elected by the mess. The only boards and surveys, upon which a medical officer would be likely to be associated with officers of the line, are surveys of medicines or sick men. The line officer would, of course, derive his opinions from his medical associate, and yet the line officer, though only a midshipman, by this regulation, presides over medical officers, it may be from twenty to fifty years in the service; for there are surgeons who have been in service over half a century. I will copy one paragraph of the report of the Navy Board, as this will sufficiently show the spirit of the whole report. At the time of the sitting of the Navy Board, a board of army officers was convened upon the same subject; but the medical and navy departments were represented upon the army board, and the gentlemen representing these corps felt it to be their duty to make a protest against the action of the army board. Yet it will be seen that the liberality of the army was so far superior to that of the navy board, that the former administered what was virtually a severe rebuke to the illiberality of the latter in regard to the naval staff corps. These two boards were to arrange the position of army and navy officers in respect to each other, and it would naturally be supposed that the medical staff of each service would be on an equal footing in respect to each other. But, no; the Navy Board depressed its medical staff so that the oldest naval surgeon—and it has been said there are some of more than half a century's service, can only rank with an assistant surgeon of four years' standing in the army; and why? Because a more equal

position with their army brethren, would give them a more fair and just position relatively to the naval line. But hear the Navy Board speak for itself.

“The board of army officers is of opinion that the relative importance of the duties devolving on the pay masters, surgeons and first class of chaplains in the army, when compared with those of the officers of the line, entitle them to precedence with majors, whose corresponding grade in the navy, as proposed, is lieutenant’s commandant.”

“The board of navy officers, after a very careful examination and reconsideration, and with a desire to give due importance to the respective duties of the sea and civil officers, considers surgeons, pursers and chaplains in the navy as only fairly entitled to precedence with lieutenants in the navy—a grade corresponding with captains in the army, with which rank and precedence is granted to *assistant surgeons of four years standing in the army.*”—*Congress. Doc., No. 30, page 10.*

How this depression of staff officers in the navy was regarded by the army board, may be learned by the following extract from a paper addressed by the army to the navy board.

“If the board of navy officers, in their scale of precedence, place what they call their civil staff below corresponding classes in the army, the board of military officers have no right to object to it; but they cannot consent, nor do they think it would be just, to change the scale which they have proposed for the precedence of those officers’ in the army.”—*Cong. Doc., No. 30, page 41.*

These short extracts show how little hope the medical corps can have of obtaining even military justice from the navy. The medical profession, if it exerts itself with the legislators, can procure a just position for their nautical brethren. The army and navy medical boards have, during the medical reform movements, been spoken of as giving the highest professional standard; and if those who have reached this standard, are legislated into an inferior and humble caste, the insult and the degradation is to the whole profession.

Why are not medical officers fit to preside over courts, boards, and surveys of which they are fit to be members, when their years and length of service would entitle them to the position of presiding officer? It is a contemptuous mark of inferiority to exclude them. Why are not medical officers put upon courts martial when a member of the medical corps is tried? It cannot be said that they are ignorant of the military law under which they live, and for the violation of which they are tried. Marine officers

taken directly from civil life constitute entire courts. Moreover, medical officers, from their studies in legal medicine, are perhaps better acquainted with the general principles of law than any other class of naval officers. Whilst the present usage exists, and the present prejudices of the line against the staff, medical officers are not tried by their "peers" when tried by a court of line officers. They are tried by those hostile to their rights.

Yours very truly.

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ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

On the Present Condition and Treatment of Venereal Diseases in Paris.

From Notes taken during a recent visit to the French metropolis. By WILLIAM AOTON, Esq., late Surgeon to the Islington Dispensary, and formerly Externe to the Venereal Hospitals of Paris.

In preparing a second edition of my work on Venereal Diseases, I thought it advisable to pay a visit to the French capital, to observe for myself what improvements my late master, M. Ricord, had been making, during the last two years and a half, in the treatment of syphilis. Although the medical journals every now and then describe any new plan of treatment he may prescribe, still they give but an imperfect idea of the progress medical science is making; and the following notes may not prove uninteresting to the profession, founded, as they are, on the results of my own observation.

The civil authorities of Paris levy a duty on provisions entering the city. The tax goes towards defraying the expense of the hospitals, which are not supported by voluntary contributions, as in this country. The administration has founded large syphilitic institutions, both for men and women. The one I am about to describe is entirely given up to the treatment of men suffering under venereal diseases. The Hospital du Midi contains nearly 400 beds, and there are at present three surgeons attached to it—M. Ricord, Vidal de Cassis, and Puche. The in-patients are treated gratuitously; the out-patients have only advice given—they are obliged to pay for the medicines, which are not dispensed at the institution. In most other French hospitals sisters of charity attend upon the sick; in this institution the duties are performed by men. The wards are very lofty and long, containing about twenty-five beds each. The committee require the patients to be seen daily; and it is the custom with French surgeons to go round the wards early in the morning, from eight to eleven. On Monday, in the wards of M. Ricord, every patient is seen; on Tuesday, only such as require any alteration in the treatment; on Wednesday, those that were admitted the previous day, when he himself, assisted by his house-surgeon, prescribes for all out-patients that apply. It struck me as singular, that since the revolution the students no longer take off their hats while in the wards, thus showing neither respect to the surgeon nor to the

institution; yet that common mark of politeness of lifting the hat when entering a cafe, is kept up. Of course, in the wards of a Venereal Hospital, many are to be found who have taken, and will take, a part in a street row, barricade, or revolution; yet the greatest order reigns in the institution; any disturbance is met with immediate dismissal by the resident civil officer; and the patients well know that soldiers in the adjoining corps de garde would be instantly called in to expel any refractory patient, notwithstanding the words *Liberte, Egalite, Fraternite*, written on the public buildings. The brotherly feeling, almost amounting to familiarity, observed by Ricord to his patients, is seldom abused; he will laugh at, or with a patient, ridicule another's imaginary fears, and the students will participate in his mirth, and the patient will smile; and all goes on smoothly. References are oft n made by all parties to the political events of the day, and censure is bestowed on the administration, the house-surgeon, dresser, or on the treatment followed in other hospitals; then comes a clinical lecture at the bedside of the patient, if the case is deserving of it; and the patient is cognizant of all that is said and done. Students are rather encouraged to put questions; and the wit and talent of the professor dreading no *contretemps*, everything is open to canvass; but impertinent observations are never indulged in, for were they employed, a laugh raised at the poor coxcomb's expense, would prevent a repetition of it—*mauvaise honte* and impertinence are equally absent. Let a patient, however, be refractory to treatment, or object to follow the prescribed routine, the light raillery gives place to just indignation, and he receives such a public admonition as serves for a warning to the inmates of the whole ward, who equally respect the kindness and the talent of the professor. The in-patients are recruited from the out-patients who are seen daily at the hospital, by one or other of the surgeons; they amount to enormous numbers, and the most severe cases are those admitted.

A surgeon attached to a large hospital, with a considerable private practice, is a very hard-worked man in Paris. Let me give you some idea of Ricord's way of spending his day. He leaves his home in the morning, between seven and eight; he first has to visit his *Maison de Sante*, which I need not observe, is a private establishment for single persons who wish to have comforts and attendance they cannot procure when sick at their own homes; at eight he enters the wards of his hospital, and often remains there till eleven. His carriage is in waiting; and then he visits his patients in Paris until three o'clock; he now returns to a hasty breakfast, and is occupied at home until seven, eight, or ten o'clock in the evening; and he assured me that he was rarely able to dine out, in consequence of his patients occupying his time to so late an hour. He has now to answer his correspondents, and prepare matter for the press, and here for the first time he can obtain assistance. Now, although Ricord is nearly fifty, and has led this sort of life, to my knowledge, for twelve years, he is as young-looking as ever; his brown hair remains unchanged; he has no care-worn appearance, but is as cheerful as ever; the same sparkling wit as formerly; and never has a surgeon been more adored by his patients, who feel convinced that he does nothing all day but consider their own particular complaints.

A French surgeon's consultation room is well adapted to the purpose: placed in the centre of several others, with doors opening into each, pa-

tients are admitted and divided into little parties. *Egalite* is not strictly observed here, particularly if the servant be feed, a practice as common in Paris as it is in London consultation rooms. The rich man need not wait so long as he who has only just enough to fee the surgeon. Late events in Paris have caused a great diminution in the practice of the French surgeon; he is called on to give a great deal of gratuitous advice. M. Ricord tells me he has always made it a rule to prescribe for patients who apply; and he never demands a fee, but on his table are seen sums varying from ten francs to Napoleons and sovereigns, which patients place there on leaving.

Asking pardon for this digression, I must return to the more proper subject of this paper. The 2d of April, 1850, the day I visited M. Ricord's wards, was Easter Monday, and the French, like the English, make it a holiday; there were a few beds vacant, but the following is the *resume* of the cases.

Indurated chancre	-	-	-	33
Secondary symptoms	-	-	-	21
Bubo	-	-	-	8
Vesicle catarrh	-	-	-	4
Phagedænic chancre	-	-	-	2
Epididymitis	-	-	-	7
Urinary Fistula	-	-	-	1
Vegetations	-	-	-	2
Tertiary symptoms	-	-	-	8
Iritis	-	-	-	1
Simple chancre, non-indurated	-	-	-	11
Gonorrhœa præputialis	-	-	-	3
Scrofulous affection of the testis	-	-	-	1
Blenorrhagia	-	-	-	3
Stricture	-	-	-	1
Chancre of the anus	-	-	-	3
Gonorrhœal rheumatism	-	-	-	1
Albuminuria	-	-	-	1
Hæmorrhoids	-	-	-	1

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The first thing which strikes the most casual observer, on looking over the cases, is the large proportion of indurated chancres—thirty-three cases in the wards at one time. Here the student may study induration in all its forms. The frequency of this symptom in the wards depends upon M. Ricord admitting by preference, as patients, those who suffer under this form of chancre. Like all writers on syphilis, he lays the greatest stress on this symptom; but differing from all his predecessors, he does not consider all hardness attending a chancre to be induration. To diagnose this true symptom of syphilis, he looks for, and points out to the pupils, the enlargement of the glands in the groins, occurring a few days after the induration on the penis. These buboes rarely suppurate; when matter forms it is the result of inflammation superadded to the disease, and is an accident, whereas the enlargement in the groin is a constant attendant upon indurated chancre. When this symptom is not to be detected, he

often keeps the patient in the wards, to show that the false ulceration will subside of itself, and secondary symptoms not occur.

I said above, that you may study, in these thirty-three cases of indurated chancre, the disease in all its varied consequences. In by far the majority of cases, secondary symptoms had already appeared; and on reference to the table, twenty-one cases are noted down in which secondary symptoms were the most prominent feature; here indurated chancre had previously existed. In these cases of constitutional syphilis, M. Ricord dwells particularly on a character not noticed by previous writers—namely, enlargement of the glands at the back of the neck, a very constant occurrence in secondary symptoms, and one that comes on early—say five weeks—after general contamination of the system. The treatment of all these cases consists in ordering the mineral, in the form of proto-ioduret of mercury, a preparation he almost exclusively gives, but one which in England I have been unable to prescribe, as the same pills which patients bring from Paris act too violently on the bowels in this country, inducing colic and diarrhoea,—consequences rarely seen in Paris, and which we must attribute to climate and difference in diet in the two capitals. I need not dwell on the long period mercury, he thinks, must be given in cases of indurated chancre or secondary symptoms, and continued even after their disappearance; three or six months' persistence will scarcely be long enough to eradicate the syphilitic diathesis, and prevent relapses when the constitution has been once infected. All his further experience has only convinced the Professor of the Hopital du Midi of the correctness of his ideas long since given to the public, but he now further assists their action by prescribing more frequently.

Fumigations.—This mode of giving mercury is so rarely followed in London, at the present day, that perhaps few of my readers are aware of the means employed.

The patient is placed naked in a box, his head being the only part exposed to the air. This box is heated by a furnace below, on which the bisulphuret of mercury is placed, in proportions of three drachms. The heat soon volatilizes the mineral, and the fumes come in contact with the skin. The patient is allowed to remain twenty minutes in a box, and a tumblerful of water is given him. In a few minutes perspiration comes on, and the patient is then taken out and well rubbed down; and the fumigation may be repeated twice a week for a month or six weeks, according to circumstances. In old chronic affections which do not yield to mercury, given internally, the most marvelous effects are often produced, and I have seen cases (resisting all other treatment) get rapidly well under this plan; and I would recommend a trial to my professional brethren in the numerous instances that come before them of obstinate complaints. I am, however, by no means convinced of its general application, or that it is likely to supersede the mineral, given internally. It has been said that patients may employ fumigations, and yet pursue their ordinary occupations. One case that has lately come under my notice induces me to recommend that patients, after employing the cinnabar, should not expose themselves to the open air. I fear, likewise, that in country practice the use of it must be circumscribed. A fixed apparatus will be necessary, as the volatilization of the mercury is difficult, in consequence of the great quantity of heat required. The ordinary spirit-lamp appears insufficient

I lately placed a patient in a vapor-bath, and after weighing three drachms of the cinnabar, placed it on the plate, over a strong spirit-lamp. In twenty minutes the substance had only lost one scruple in weight, independently of waste, thus showing that this is a very imperfect way of volatilizing the mercury, the vapor of which is so heavy that it will not rise more than one foot, unless carried up by a strong current of air. These peculiarities explain why fumigations have fallen into disuse, as the proper means of application are not always at hand.

Tertiary symptoms may be seen in the wards in all their variety; and I noticed one which will form the subject of a separate communication, viz: of amaurosis, depending upon disease of the sphenoid bone. Suffice it, at present, to say that iodide of potash is our sheet anchor, given *in large doses*. It is a very general belief in London, among our leading surgeons, that all the good that iodide of potash can do, will accrue if three or five grains be given, three times a day; more than that, I am daily told, will do harm, or if more be given with impunity, the article is spurious. I do not wish to underrate the good effects of the salt often resulting from small doses, but I am as equally convinced that large doses may be given with impunity. I lately had a gentleman from Scotland under my care, who took the remedy in such quantities that he purchased it by the half pound, and yet it was a genuine article; but I will go further than this, and assert, from a pretty large experience of its effects, that small doses will not do any or the slightest good in many instances. I have now under my care a patient with affection of the nose, and destruction of the *ossi nassi*, who had been taking these small doses of iodide of potash for many months, under a late lamented surgeon, and the disease remained pretty nearly in *statu quo*. She came under my care, and the affection is now on the point of complete recovery under increased doses.

I met Mr. Wallace in consultation a short time since, in reference to a case of tertiary symptoms of the nose and brow, which iodine had relieved, but not cured, although taken in these small doses by direction of another surgeon for nine months; here the salt had been obliged to be left off, because iodic intoxication was said to have been produced, together with symptoms of affection of the brain. Now in these instances, time, rather than the dose, was in fault, and the surgeon who entertains these ideas scruples not to prescribe iodide of potash for years, rather than give it in larger doses.

I saw, with Mr. Vickers, a gentleman who has now entirely recovered, to whom we gave large doses of iodide of potash with bitters, yet small doses had failed in curing him, although given for long periods under the advice of other practitioners. I mention these cases because prejudices exist against the employment of the remedy, whereas, in France, Ricord gives it in anything but homœopathic doses, and with the most signal success.

Phagedænic Chancres.—At the time I visited the wards, only two cases existed of this disease, but generally it is much more frequently met with. Under this head may be included all those anomalous sores met with in London, or elsewhere, which are classed as irritable chancres, occurring in bad constitutions, indisposed to heal, spreading, instead of healing kindly, and causing the greatest annoyance to both surgeon and patient. In consultation practice in London, these are the cases I am most frequently con-

sulted about to sanction the use of mercury. All sorts of washes have been used, caustic has been applied, but without avail, and the case is getting worse. In all such instances, iron is the remedy, and its effects are perfectly magical. I need not say that mercury aggravates such cases. Of all preparations, the tartrate of iron is best given in the following manner:—potassio tartrate of iron, 1 ounce; water, 6 ounces. Mix. Two table-spoonsful three times a day.

M. Ricord largely employs it, and prescribes it now, even in the treatment of simple cases of chancre; he uses it, likewise, as a local application, as well as an internal remedy.

Chronic Catarrh of the Bladder.—Ricord now uses caustic injections into the viscus itself. Having emptied the organ of urine, he passes a gum-elastic catheter, and then, with a glass syringe, throws the injection into the bladder, and repeats it, according to circumstances, every three or four or six days. There is some pain and irritation set up; the urine becomes a little bloody, but the ropy secretion soon ceases. The different preparations of turpentine are soon given, and I saw four cases in their different stages. He has not met with any ill consequences of the treatment, although the injections used are composed of nitrate of silver, 2 drachms; distilled water, 4 ounces. Mix for an injection.

Strictures.—On the day I took notes of the cases, only three instances of stricture of the urethra presented themselves in M. Ricord's wards, and one of urinary fistula. The whole of these were serious examples of their respective diseases, and formed the subject of a clinical lecture. I am happy to say that the eminent Professor of the Hopital du Midi corroborated many of the opinions I expressed during the last winter, on the subject of treatment. In the ordinary cases, he employed simple dilatation by means of conical gum-elastic bougies, with a little olive-shaped point, such as I now produce; when retraction takes place, the case is treated by incision with instruments passed down the urethra, and he has lately introduced several modifications in the shape of the instruments with which the stricture is to be incised. Nitrate of silver is rarely employed in stricture; and on the subject of potassa fusa, M. Ricord agrees with modern surgeons, that in small quantities the kali becomes saponified, and is insufficient; that in larger masses, when pressed against the obstruction, it is attended with the most serious consequences, and is not a remedy at all adapted to overcome the impassable strictures which every now and then come before the surgeon.

I mentioned to M. Ricord the fatal results which had in London attended the operation of incising the perineum as recommended by Professor Syme. He thought that one or two unsuccessful cases should not deter us from prosecuting the plan, provided all others had failed. He acknowledged that very puzzling cases every now and then came before us, and he cited an instance in his own practice, where death followed from incising the stricture by instruments introduced through the urethra, the fatal result (just as in the London cases) following from typhoid symptoms.

But the numerous late instances of repeated deaths pointed out the caution that should be exercised, and he was by no means convinced that the stricture would not return after a certain period. Still, (added he, in his peculiar laughing way, which often winds up a learned disquisition on a plan of treatment,) I should much rather find my patient complaining of

the return of the stricture, than the surgeon regretting the non-return of his patient, as you state is the case in London.

I regret being obliged to omit many important observations which I made during my stay, as well as other remarks on the out-patients, but I hope I have said enough to show how much society and the medical profession are indebted to M. Ricord for the manner in which he has successfully carried out the treatment of venereal diseases, and following in the footsteps of the immortal John Hunter, has recalled the attention of the profession to a class of diseases that of late years had become the especial domain of quacks.—*London Lancet*, December, 1850.

The Nature of Evidence, and the Requisites for the Credibility of Testimony. By ASA D. LORD, M. D., Columbus, Ohio.

All the knowledge we possess, or are capable of acquiring, may be referred to one of the three following heads: First, those primary truths given us by reason, the various operations of our own minds as revealed by consciousness, and the thoughts and opinions formed by the combined action of the several faculties of the mind in the processes of reflection; second, the information acquired through the medium of the senses in the processes of observation, experiment, or investigation; third, that which is derived from the testimony of others as the result of their experience, observation, or research. Hence, it is proper to name the exercise of the senses, the operations of our own minds, and the evidence of testimony, as the sources of knowledge.

To the last source we are indebted for the greater portion of the information we possess, and for nearly all that which we value most highly. In history, geography, and astronomy, in chemistry, and nearly all the branches of natural science, we must depend, mainly, upon the testimony of others for our information. Few have the opportunity to travel, and view for themselves the localities described by the geographer and the historian, the instruments for seeing the wonders revealed by the telescope and the microscope; and few have either the time or the conveniences for performing the experiments which substantiate the great truths on which the whole superstructure of many of these sciences rests.

From these considerations, it is obvious that some acquaintance with the nature and laws of testimony, is important to every individual, but perhaps no class of persons, except those connected with the Bench or the Bar, are more frequently placed where such an acquaintance is of the utmost importance, than those engaged in the practice of Medicine. Not only is the Physician often called to the stand under circumstances of the deepest interest to all concerned, and when the nature of his testimony is to decide questions of the highest importance, and the manner in which it is given may do much for the credit or reproach of his profession, but the new and conflicting views and theories in regard to the nature and causes of disease and the mode of treatment, which are constantly claiming his attention, demand, on his part, a familiar acquaintance with the laws of belief, and the ability to apply them to the facts and arguments by which these theories and opinions are sustained, with clearness of perception and correct judgment, if he would not be a mere empiric in his practice, and receive his

opinions and theories ready-made, from those whose lead he prefers to follow.

NATURE OF TESTIMONY.

The office of testimony is to give to us the same information we should obtain by the use of our own powers of observation and investigation, in regard to facts and phenomena, which have not come under our notice; it is employed to establish the existence or validity of facts, not to sustain opinions or theories. Opinions must be substantiated by arguments: the validity of arguments must be tested by the rules of logic. Theories must be sustained by facts. In order that a theory may be regarded as the *law* by which the facts for which it professes to account are governed, it must be consistent with all the facts and phenomena pertaining to that subject. The existence of one single, well-attested fact at variance with a theory, is sufficient to destroy our confidence in it altogether: not only must it be thus accordant with the facts, but it, or an identical one, must be absolutely demanded by them. Thus, the theory of gravitation is not only consistent with all the facts for which it professes to account, but it, or a similar theory, is imperatively demanded by them; hence, this is without hesitation denominated the Law of Universal Gravitation; but the same is not true of either of the two theories which have been proposed in regard to the nature of light. Harvey's theory of the circulation, is also exactly in point; it was demanded by the facts known, or easily ascertained, in regard to the structure of the heart, the arterial and the venous systems, the pulmonic and the systemic circulation: the same is true of the modern theory of the nervous system, the facts demand the existence of the distinct classes, denominated nerves of sensation, of voluntary motion, and of organic life.

The existence, or the validity of facts which have not come under our own observation, must be established by testimony. Alledged facts may be of two kinds, impossible or possible; no amount of evidence can justify us in believing an impossible thing; only the latter, therefore, can be substantiated by testimony. We may be called to believe strange things, those which are marvelous, and, with our present knowledge, unaccountable; but they must, at least, be *possible*, they must not involve any palpable contradiction, or manifest absurdity. We are accustomed to speak of impossibilities of two kinds, *physical* and *moral*: in the former are included things which are contrary to the known laws of the natural world; as, that a body should exist in two places at the same time; that a circle and a square, or a cube and a globe, should be the same thing; by *moral* impossibility is meant, a very high degree of improbability. The term *physical* impossibility, or the phrase *physically impossible*, is also employed for describing mathematical and other absurdities; as, that two and three should equal seven; that an idiot should demonstrate an intricate problem; while the statement that one who had been an idiot up to the age of twenty, had from that time manifested the possession of an intellect of the highest order, would be so improbable as to be considered a *moral* impossibility. Possible facts may be classified, first, as *probable* or *improbable*, when they do or do not accord with our experience, or the results of our observation: second, as *credible* or *incredible*, when they do or do not correspond with the known powers of the agent to which they are ascribed, with the laws in accordance with which they are supposed to occur, or by which the producing causes are governed.

To classify accurately, with reference to these distinctions, the statements made in the reports of remarkable cases or wonderful cures, and the certificates of the inventors of specifics, and the venders of nostrums, and the graver assertions of those who are supposed to have a claim to credence, is often a work requiring no little care, and no small degree of critical acumen; for, while an indiscriminating credulousness is indicative of a weak mind, incapable of thinking or judging for itself, a tendency to indiscriminate skepticism is equally characteristic of a bigoted mind, which reasons incorrectly if at all, or upon imperfect data, or from unsound premises, and makes its own pre-conceived opinions the test of all new theories, and its limited observation the standard of probability, by which all alleged facts are to be tried. This is the disposition which, when told that facts do not accord with his favorite theory, leads the holder to exclaim impatiently, "So much the worse for the facts then;" or like the King of Siam, who, when told by a Dutch traveler that in Holland, water sometimes became so solid that an elephant might walk on it, replied, "I have believed many extraordinary things which you have told me, because I supposed you a man of veracity; but now I know that you lie." With a cultivated mind and the ability to discriminate clearly, we can readily see how the king just named, or any other person who had ever seen the metals liquefied by the application of heat, and again become solid when it was abstracted, might have regarded the traveler's statement, not only as possible, but as credible, and even highly probable, without reference to the credibility of the traveler. In the same manner, we can easily conceive how Archimedes, or any other similarly intelligent philosopher of his time, might have given credence to a description of the feats of a steam engine, or any other piece of modern machinery, while his ignorant countrymen would have regarded it as entirely fabulous.

In order to prove the existence of facts or the occurrence of events of which we have not personally taken cognizance, it is desirable that they should be substantiated by the concurrent testimony of two or more individuals; but cases frequently occur, in which, from the circumstances, this is impossible, and our opinions must be formed from the testimony of a single witness. In such cases, it is necessary that the witness possess most or all the characteristics needed to render his testimony valid; and hence the importance, to every person, of a knowledge of these characteristics.

REQUISITES IN A SINGLE WITNESS.

The most important requisites for the credibility of testimony from a single individual, are the following:

1. Unexceptionable moral character; or, at least, a character for veracity unimpeached and unimpeachable: the importance of this is too obvious and too well understood, to need comment.
2. That he be disinterested in the present case; that he have no private or personal ends to accomplish.
3. That he possess a capacity for understanding the facts in regard to which he testifies: the savage would be but poorly qualified to give information in the processes required in the mechanic arts; the man unacquainted with human anatomy, physiology, and pathology, would be equally incompetent to testify to the facts revealed by a post-mortem examination.
4. He must have had full opportunity to observe, for himself, the facts

to which he testifies: this will, of course, exclude every thing like "hearsay," or second-hand testimony.

5. There must be satisfactory evidence that adequate attention was given to the facts at the time of their occurrence, and that the memory of the witness does not deceive him. This is a requisite of the utmost importance; the testimony of many a witness is invalidated, and his character for veracity rendered questionable, by the manifest want of attention to the facts, and the confused remembrance of them indicated in his statements.

6. His testimony must be given in plain, unequivocal terms. Nothing is more directly calculated to discredit the testimony, or bring suspicion upon the character of the witness, than the use of ambiguous expressions; and it adds no weight to testimony, to have it expressed in technical language, when common and perfectly intelligible terms may be employed with equal accuracy and propriety.

7. There must be consistency in his statements, and between them and his conduct. Had Columbus manifested an unwillingness to return to this continent, or had he even seemed indifferent to the undertaking of another voyage in this direction, who would have believed his statements in regard to the discoveries he had made?

CORROBORATING CIRCUMSTANCES.

Aside from these characteristics of the witness himself, there are often circumstances entirely unconnected with his character, his capacity, or his conduct, which have an important bearing upon the credibility of his testimony; among those which tend to confirm his statements are the following:

1. The entire absence of all motives to misrepresent the facts, or to give false testimony, and especially the presence of strong motives of an opposite character; under such circumstances, we often feel impelled to receive testimony without reference to the moral character of the witness.

2. That the witness be aware that he is open to conviction if his testimony is false.

3. When the facts stated, though unknown before, accord with others of a similar nature, already known.

4. When, though without parallel or analogy in our experience or observation, they accord with the known powers of the cause alleged to have produced them.

5. When the facts stated develop laws not previously discovered, or reveal properties or powers in the alleged causes, or the existence of relations, before unknown; here, the farther removed from experience, the greater the probability of the truth of the facts: because, if not true, they would have been unknown to the witness. One of the earliest Greek historians mentions that a ship's crew professed to have sailed from the Red Sea, around the Southern extremity of Africa, and returned by way of the Straits of Gibraltar; but adds, "We know they do not tell the truth, because they affirm that, during a great part of their voyage, the sun appeared north of them at noon!"

6. The entire absence of all conflicting or contradictory testimony, and the knowledge of facts which cannot be accounted for, except by supposing the statements true.

TESTIMONY FROM TWO OR MORE WITNESSES.

When two or more witnesses appear to testify to the same facts, it is important,

1. That each possess all the requisites previously named.
2. That they concur in their statements in all essential particulars.

The following circumstances tend to increase our confidence in concurrent testimony thus given.

1. When one witness mentions facts or particulars omitted by others.
2. When coincidences occur in regard to which the nature of the case and their circumstances, or relations to each other, preclude the idea of collusion.
3. When statements apparently contradictory, are reconciled by a more full acquaintance with the subject.
4. When the characters of the witnesses, and their relations to the subject of investigation, are materially different, and hence their testimony, if influenced at all by personal feelings, would vary.
5. To these may be added, in regard to recorded testimony of every kind, one of the strongest corroborating circumstances is, the general assent to its truth of all those to whom the statements were originally made, and during whose life-time they were recorded.

It will be obvious, without remark, that the views here presented may be applied in every department of investigation or research. To those already familiar with the subject, it is unnecessary to say that the writer claims very little on the score of originality: his object has been to arrange, in a form as condensed as possible, and convenient for reference, an outline of the subject, for the benefit of those who may not have time for consulting the voluminous legal treatises upon evidence, and who may wish to be able to form for themselves opinions which they can defend, as well as state and illustrate, in regard to every subject to which their attention is directed.—*Ohio Med. and Sur. Journal.*

Lead-poisoning in the City of New Orleans. Proceedings of the Physico-Medical Society.

Those of our readers who have obtained the first volume of Dr. Fenner's *Southern Medical Reports*, cannot have overlooked his report on the *epidemic colic* that prevailed in this city during the summer and autumn of 1849; nor could they have failed to attribute this colic to *lead-poisoning*, derived chiefly from the use of *soda water*. The same disease having appeared again in the summer just past, gave occasion to Dr. Fenner to renew his investigations into its cause. On the 14th of September, he read before the Physico-Medical Society, an elaborate paper on the subject, of which the following is a very brief *resume* or synopsis, together with the report of a committee of able physicians appointed by the Society to examine the subject. The original paper is reserved for the second volume of the *Southern Medical Reports*, which we learn will appear about the 1st of June next. The subject of this paper is of vital importance to the citizens of New Orleans, and we trust it will command the attention it deserves.

*Abstract of the Proceedings of the Physico-Medical Society of New Orleans.
Regular Meeting, Saturday evening, Sept. 14th, 1850.*

Dr. Fenner read a paper on the subject of *lead-poisoning* in the city of New Orleans, to which he believed the prevailing bilious colic of this and the last summer, might be justly attributed. He pointed out the striking resemblance existing between the colic lately seen and that which undoubtedly arises from lead, as well in its symptoms as its consequences, and mentioned standard authorities which maintain that there is no material difference between the bilious colic or dry belly-ache of the West Indies and true lead colic.

He quoted authorities to show that in all the places where colic has prevailed extensively, under careful investigation the disease has been fairly traced to *lead*; and added, that the skepticism which has ever existed on the subject, arose from the imperfect and unsuccessful efforts to discover the lead.

He called attention to the brief experiments he made with *soda water* last year, which indicated the presence of a great amount of lead in that beverage, and stated that recent experiments under the eye of Prof. Riddell, had removed all doubt in regard to it.

He then proceeded to detail the results of the recent examinations of the water in common use from our *city hydrants*, which likewise appeared to indicate the presence of lead to a great and dangerous extent. In connection with this point, he related some cases of colic that had recently fallen under his observation, and which, if attributed to lead, must have derived it chiefly if not solely from the use of hydrant water.

He said he had examined the dark deposit thrown down by the introduction of sulphuretted hydrogen gas, and thought that by means of the blow-pipe he had succeeded in developing a few small globules of lead, but was not fully satisfied of the fact.

He had examined hydrant water which had been previously clarified by alum, and found a very slight trace of lead, which he thought would appear to indicate that in this cheap and convenient article we should find both a *corrective and antidote* to the existing poison.

He had tasted water taken direct from the river, and found that it likewise threw down a black deposit after the introduction of sulph. hydrogen gas, but he thought it more likely that this proceeded from the presence of *iron* than lead.

He had tasted various specimens of wines and malt liquors from bottles, without detecting any appreciable presence of lead.

All these experiments had been made with sulphuretted hydrogen gas, in the laboratory of Prof. Riddell, who was present and confirmed what was stated by Dr. Fenner. Dr. F. made some remarks upon the *importance* of the subject, and hoped it would claim from the society the attention it deserved.

Dr. Jones moved that a committee of three be appointed to investigate the subject as set forth by Dr. Fenner, and report at their earliest convenience. Whereupon the following gentlemen were appointed, viz: Drs. Jones, Riddell and Axon.

REPORT OF THE COMMITTEE.

The committee appointed on the 14th of September, to examine the

facts set forth in the paper of Dr. Fenner, read to the Society, on the subject of *poisoning from Lead*, beg leave to report, that they have repeated the experiments on the soda, hydrant and river water, in company with Dr. Fenner; and without repeating the processes they have followed, have arrived at the following conclusions:

1st. That they have carefully tested the various specimens of soda water procured from different fountains in the city, in all of which they readily found evident traces of lead. These investigations have been in their opinion, abundantly sufficient to confirm the opinion of Dr. Fenner that lead exists in all the soda water transmitted through leaden pipes, and in adequate quantity to produce the deleterious effects by him thereto attributed.

2d. They have also frequently examined the hydrant water in the Chemical Lecture Room, which is transmitted through about seventy feet of leaden pipe. During the greater part of these investigations, and while the water was freely drawn and in constant use, they have not been able to detect the slightest traces of lead by the most delicate tests. Traces of lead have been detected, however, by Drs. Riddell and Axon, in company with Dr. Fenner. That under certain circumstances, traces of lead are to be found in hydrant water; though, whether in sufficient quantity to produce the imputed effects, is, in their opinion, not satisfactorily established.

3d. The Committee having repeatedly examined the water taken immediately from the river, for indications of the presence of lead, unanimously concur in asserting that on no occasion has its existence been demonstrated. The dark precipitate produced by sulphuretted hydrogen, both in this and in the hydrant water, having been effected by its action on the suspended sesqui-oxyde of iron. The Committee do not therefore concur in the suggestion of Dr. Fenner that lead exists in an appreciable quantity in ordinary river water.

While compelled to disagree with the author of the paper read to the Society in the greater number of his conclusions, the Committee cannot refrain from expressing their approval of these investigations, which it is to be hoped will be followed up not only by the gentleman alluded to but by the members of the Society. Dr. Fenner has certainly established the deleterious character of soda water, for which he is entitled to the thanks of the Society and of the community at large, for whom it would be demanding a just protection by having proper municipal ordinances adopted, prohibiting henceforward, under heavy penalties, the transmission of carbonated water through leaden pipes.

Signed, JAMES JONES,
A. FOSTER AXON,
J. L. RIDDELL.

New Orleans, Nov. 2d, 1850.—[N. O. Med. and Surg. Journal.]

Treatment of Dysentery. By J. WATSON TULLIS, M. D., Troy, Ohio.

Dysentery prevailed very extensively in Troy and its vicinity, during the latter part of the summer of 1850. It was marked by the usual symptoms of Epidemic Dysentery. A number of cases proved fatal, under the

ordinary treatment. I mean, by ordinary, the mercurial and anodyne treatment, with the occasional use of cathartics.

During previous years, I had treated Dysentery according to the plan generally recommended in the books, but never with the uniformly happy result which attended an almost entirely different course of treatment, adopted by me during the last summer.

I had lost confidence in the mercurial treatment, not only from my own experience, but from the numerous concessions as to its uncertainty, on the part of those who had trusted in it.

The cathartic treatment, resorted to by almost all, so far as I know, it seems to me, is still more objectionable. Although it induces fecal evacuations, yet it does so at the expense of the delicate and inflamed mucous membrane of the bowels. The natural evacuations, thus induced, do not depend on a subsidence of the *disease*, but upon the excited contractions of the muscular coat of the bowels, upon the already inflamed and painful mucous membrane, which must inevitably aggravate the pain and inflammation, and, consequently, increase the danger of the case. If the mucous secreted is lessened, or suppressed by such treatment, it is because there is such an exaltation of organic action, as is incompatible with the process of secretion; and the mucous, or muco-bloody discharges will return, probably, with increased profusion and frequency; unless, indeed, from the mildness of the disease, or the recuperative power of the system, recovery is effected in spite of the treatment.

But I will briefly present the simple treatment which I have adopted, and which I can recommend, with the utmost confidence, to my medical brethren.

I direct Sulphate of Morphine, in doses varying from 1-6 to 1-3 of a grain, one of which is to be taken every time the bowels are moved, provided the interval be not less than one-half hour: thus giving time for the action of the medicine, lest the patient be narcotized. I uniformly give 10 or 12, or more, powders, of quantity suited to the patient. If I know nothing of the susceptibility of the patient to the action of morphine, I sometimes direct that he should begin with half powders, repeating as above; but if not very soon relieved, to take full doses.

I never used morphine with such profusion as in our late epidemic, and yet, in an extensive practice, did not narcotize a single patient. It is astonishing what an amount of morphine, in Dysentery as well as in many other diseases, is tolerated by those who, under ordinary circumstances, would be readily narcotized.

Having adopted the morphine treatment during the entire period of this epidemic, and in a variety of constitutions, and with, certainly, not less complications than are usually met with in dysentery, during the summer and fall months, I am inclined to prefer it very much, above any other treatment. It is safe, salutary, and free from the horrible torture incident to the mercurial and cathartic treatment.

Of course, regimen is of the utmost importance in this affection. Total abstinence from food, mucilaginous drinks and a suitable dose of morphine after each evacuation of the bowels, will frequently effect a permanent cure in less than 24 hours.

It was only in a few cases necessary to use enemata of laudanum and

starch. This, however, should never be neglected, if tenesmus and tormina resist the morphine.

In all cases which resisted treatment by internal means, or were marked by unusual pain, or tenderness of the abdomen, or great prostration, or any other indication of danger, I cupped *freely* and *repeatedly*, and with the *very happiest effect*. In an acute case of dysentery, not controllable by ordinary means, the neglect of local depletion is unpardonable. Perhaps almost the only cases which would not recover without medical aid, are left to perish for the want of *local bleeding*, under the delusion that a *blister* is as salutary as *scarificator* and *cups*. This delusion, or opinion, is not sanctioned by any respectable modern work extant; and yet, perhaps, more than nine-tenths of our western country practitioners, practice upon it, to the torture and sacrifice of hundreds of patients.

Warm emollient poultices, constantly applied, are of unquestionable utility, and should never be neglected in serious cases.

Blisters should never be employed in acute dysentery, because they are a thousand times more painful than cupping, and a thousand times less useful, and because when they fail, you can neither repeat the blister, nor apply cups, until the surface be healed.

In conclusion, permit me to say that, in all ordinary cases of dysentery, suitable doses of morphine, taken immediately after each dejection, with rest and abstinence from food, will effect a speedy cure. As to the severer cases of dysentery, I propose to make further observations, in a subsequent article.—*Ohio Med. and Surg. Journal*.

Axillary Dislocation of the Humerus after five weeks' duration. Reduction by the Twisted Ropes under the influence of Chloroform. By DAVID PRINCE, M. D.

(Read before the St. Louis Medical Society.)

Michael Carmoly, aged fifty, muscular, never having been sick except a fever twenty years ago, a recent immigrant from Ireland, presented himself to Dr. Marshall, November 25th, 1850, saying that five weeks ago, while coming up from New Orleans, he fell through the hatchway of the steam-boat, and stunned himself so that he knew nothing for several hours. With a cut upon his head, and a sprain of one ankle, and a bruise of the other knee, the shoulder escaped especial attention; and as those got well without a doctor, he thought his shoulder might get well, also. "But," said the patient, "upon stripping myself this morning, before the women, they told me the shoulder stuck out more than the other, and I must go and see a doctor." Upon examination, the injury was found to be an axillary dislocation of the humerus, with the usual signs. As Dr. Marshall invited me to superintend the procedures in this case, I suggested the employment of the *twisted rope*, instead of the pullies, an apparatus always available, having all the advantages of the pullies; cheap, as a bed cord, and easily mended when broken, and to be replaced in any by-place. Every adjustment was made, as if for the use of the pullies, except that a rope quadrupled, with a stick introduced for a *twister*, replaced the pullies. The patient was brought under the influence of Chloroform; the rope twisted, and thus

shortened, by turning the stick which had been introduced, until, after sufficient pulling and waiting, the head of the humerus was found to occupy the glenoid cavity of the scapula. Here it was retained by suitably adjusted bandages. The effect of the chloroform was, in this case, most admirable—removing all voluntary contraction of the muscles, and leaving only the tonic shortening to be overcome, and the new formed adhesions to be broken up.

But the particular object in reporting this case, is to call attention to the employment of ropes, twisted by a stick, as affording all the power and convenience of pulleys, and few or none of their disadvantages, except that the operator cannot “pull the rope” and manipulate the displaced bone at the same time. The assistant, who turns the *twister*, can be directed to *twist*, to *hold fast*, or to *untwist*, at the pleasure of the operator; and if a rope should break, a new one could be easily procured—or in default of a new one, the old one could be tied. A cut, showing the mode of applying the *twisted rope*,* may be seen in the April number (page 355) of the American Journal of the Medical Sciences, for 1845, in connexion with a communication upon the *twisted rope*, from Dr. D. Gilbert, Professor of Surgery in the Medical Department of Pennsylvania College, Philadelphia.

Though a contrivance so simple can hardly be claimed as an invention, for any one, Dr. Gilbert ascribes the credit of first using it to Dr. P. Fahnestock, of Pittsburgh, Pa.

“The power furnished by the twisted rope,” says Dr. Gilbert, “answers every indication, furnishing great power as well as the pulleys, and is, indeed, preferable, on account of its simplicity and availability in every possible situation, enabling the surgeon even to dispense with assistance should that be necessary.”—*St. Louis Medical and Surgical Journal*.

Treatment of Internal Hæmorrhoids.—Dr. I. P. GARVIN has recently published, in the *Southern Medical and Surgical Journal*, a very interesting paper, in which he states that he has treated a considerable number of cases of internal hæmorrhoids, some of them very severe and of long standing, by the use of cold water, in the following manner:—He directs about a gill of cold water to be thrown into the rectum immediately before every attempt to evacuate the bowels, and that this enema be retained several minutes, if possible. This usually produces an evacuation of the fæces, which have been so far softened on their surface as to permit their escape without the least straining or irritation. After every evacuation, it will be proper to use ablutions of the parts, more especially in such cases as are here attended by some protrusion of the bowels. The treatment is to be continued until some days after all uneasiness is removed. In old or very severe cases, to effect such amendment generally requires several weeks. It is highly important to impress upon the patient the absolute necessity of perseverance in the use of cold water, even though he should be so far relieved as to feel almost well, for if it be suspended too soon, a very slight cause will bring on a relapse. So decided is the relief afforded by this treatment, that few persons would be disposed hastily to abandon it, but for the inconvenience of apply-

* This is copied into Sargent's Minor Surgery.

ing it daily. The ordinary apparatus for enemata are so unwieldy, that they cannot be carried about conveniently. All difficulty from this source may be obviated by the employment of a small pewter syringe with a ring handle to the piston. One which will hold two ounces is very convenient, and may be carried in the pocket when necessary. When such enemata of cold water fail to procure sufficient alvine evacuations, the quantity of fluid may be increased to half a pint, or it may be necessary to resort to mild laxatives. Active purgation must be carefully avoided. The patient should be advised never to aid the natural expulsive action of the bowels by straining.—*Am. Jour. Med. Sciences.*

Novel Method of Treating Chronic Abscess with Iodine Injections. By R. L. Howard, M. D., Prof. of Surgery in Starling Med. College.

On the 25th of January, 1850, Mrs. L. G., aged 21 years, was attacked by local inflammation on the anterior and upper part of the right thigh. The acute symptoms soon subsided, but a chronic painless swelling remained unreduced. During the following spring and summer, the swelling gradually increased, but produced no decided inconvenience. Her general health variable, and always delicate, was not materially impaired by the local disease or its cause.

During the prevalence of the cholera in our city, some time in the latter part of August, she for the first time applied to me for advice. I found on examination, a large fluctuating tumor, extending from Poupert's ligament to within four inches of the knee, and from the triceps adductor, internally, to the outer border of the vastus externus muscle. As it was entirely devoid of pain and other evidences of inflammation, and fluctuated at every part, I supposed it to be a chronic abscess.

On the 3d of September, after having corrected so far as practicable her general health, I introduced a tenotomy knife in an oblique direction, into the most dependent part of the tumor, and drew off about one pint of serous pus. I then sealed the orifice hermetically by applying collodion and adhesive plaster. I then procured a laced band, wide enough to pass nearly around the thigh, and of sufficient length to cover in the entire swelling. This was applied and drawn by cords as tightly as the patient would bear. The possibility of wrinkling or slipping down was prevented by the insertion of several sticks of whalebone, parallel with the axis of the thigh. This band was re-applied daily. At the end of one week I again tapped the abscess—removed about the same quantity as before, and treated the incision in the same manner, which was followed by adhesion by the first intention. Subsequently, I repeated these incisions once in from seven to twelve days. My object was obviously to induce a gradual contraction of the walls of the abscess, and by compression, to induce adhesion between them, so as to finally effect an obliteration of its cavity.

Within the period of three months after the first incision, I tapped this abscess twelve times—at each operation I found the fluid to lose more and more its purulent character, so that it finally became almost entirely serous, holding in suspension flakes of lymph and ill concocted pus. The object of insuring immediate adhesion between the lips of the wound, was, of

course, to prevent the admission of atmospheric air, an accident, connected with so large a cavity, fraught with the utmost danger.

After three or four of the first operations, the size of the abscess seemed gradually to diminish; but it finally became stationary and seemed to contract. Here I reflected upon the propriety of laying it open by a free incision, so as to invite the admission of atmospheric air, and an unobstructed discharge of all its contents. As my patient was constitutionally feeble, and had suffered much from former sickness, I feared the consequences. I then adopted the following plan:

On the 11th of November, I punctured the abscess and removed as usual, nearly a pint of glairy fluid, and injected an ounce of water, holding in solution gr. iii. of Iodine, with gr. x. of Iodide Potass.—closed the orifice and induced adhesion. A burning sensation followed this injection, which continued for twenty-four hours. This was accompanied by an elevation of temperature, and increased swelling. These symptoms, however, subsided in two or three days, and the swelling remained as before.

November 18, I again tapped and injected into the cavity, after having drawn off most of its contents, gr. v. Iodine, gr. xv. of Iodide Potass. in an ounce of water, and treated the puncture as before. More decided marks of inflammation followed this operation, but at the end of about one week, all excitement, pain, and acute swelling, subsided, and left the tumor in its original condition.

November 27, I injected, after removing all the contents, Iodine gr. x., Iod. Potass. dr. ss., water oz. i., and closed the orifice. This operation was followed by quite a sufficient amount of inflammation. The patient complained of excessive heat and pain in the part—the walls of the abscess, and the surrounding soft parts, became thickened and indurated by excessive deposit; and the general system was involved in systematic fever. I made use of but moderate remedies to subdue the general or local inflammatory excitement, as it was upon this excitement, within certain bounds, I relied for obliteration of the cavity by granulation and plastic exudation. In the course of a week, the acute symptoms began to abate, and a few days later my patient was again comfortable—and yet there was around the cavity a vast amount of solid swelling, the result of the preceding inflammation. I found on subsequent visits, that the tumor was not increasing in its dimensions, but on the contrary, sensibly diminishing.

February 7th, I visited my patient after a long absence, and found the swelling had entirely disappeared, and the general health much improved. I might say here, that during the autumn I had treated Mrs. G. constitutionally, as well as locally. Quinine and other tonics were administered a part of the time, with occasional evacnants, as circumstances required, but my principal reliance was upon Iodide Potass. While general remedies served to improve somewhat her health, they exerted no influence upon the local affection whatever.

I look upon this case, in connection with the treatment, as a most interesting one. Within the limits of my own observation, many patients have lost their lives from free and reckless incisions into such extensive chronic abscesses. Inflammation of the pyogenic membrane, induced by the admission of atmospheric air, together with hectic fever and a high grade of constitutional irritation, are almost certain to follow this rash, but simple operation.—*Ohio Med. and Surg. Journal, March 1.*

External Use of Diuretics. By PROF. CHRISTISON. N. Y. Journ., Jan., 1851, from Monthly Journ. Med. Science.

Some years ago a short paper appeared in a French periodical, on the treatment of ascites, by means of digitalis and squill applied outwardly in the form of liniment; and three cases were given, illustrating the diuretic and curative effects of this treatment, after failure by means of diuretics administered in the usual way, as well as by powerful purgatives. At the time, I had under my charge a boy, ten years of age, who had labored under simple ascites—that is, without any cedema, even in the limbs—for a period of five or six months, during which the effusion had slowly and steadily increased, notwithstanding the employment of brisk purgatives, various powerful diuretics, and mercurial action. There was great difficulty in referring this affection to any particular organic disease; but, on the whole, a previous chronic peritonitis was suspected to be the cause. When he came under my care, purgatives and diuretics internally were again tried, and especially digitalis and squill, but with no better success than before. On the contrary, the enlargement and tension of the belly became gradually very great, so that the boy was confined in a great measure to bed. The French suggestion came, therefore, most opportunely. A mixture of equal parts of tincture of digitalis, tincture of squill, and tincture of soap, was rubbed freely and diligently into the skin of the belly morning and evening. At an early period—so early, if I do not mistake, as the beginning of the third day—the urine began for the first time to increase; by and by a copious flow was established; the ascites quickly subsided, and in about fourteen days entirely disappeared; the boy at the same time gained flesh and strength under the use of simple bitters and chalybeates, and ere long his health was completely restored. When I last heard of him, four or five years afterwards, he continued well and strong.

The same treatment was tried afterwards in several cases more or less similar to this, but for a time with invariable disappointment. In all, however, the more ordinary measures had previously been exhausted to no purpose; and in most of them decided organic disease of some abdominal organ was ascertained to be the cause of the effusion. At length the following remarkable case revived the hopes excited by the first experiment. A boy, about nine years of age, had been affected with enlargement of the liver and ascites, for nearly a twelvemonth. Dr. James Duncan, his medical attendant, had employed all the most serviceable remedies, but to very little purpose. Mercury, iodine, purgatives, diuretics, both vegetable and mineral, had been faithfully given, occasionally with partial effect, but not with any satisfactory amendment. At length the ascitical distension became enormous, the integuments acquired a glistening translucent appearance, the respiration was impeded, and the tightness of the skin prevented the nature and extent of the disease of the liver from being ascertained. But there was no cedema, even in the limbs. In this state I saw him for the first time. Before resorting to the temporary expedient of tapping, it was proposed to make the trial of the diuretic liniment; but the integuments were so irritable, that the method by friction could not be persevered with. Dr. Duncan, however, proposed to substitute a strong infusion of digitalis, four times the pharmacæutic strength, and to apply it continuously by linen cloths covered with oil silk to impede evaporation. In a few days diuresis

sat in, and a profuse flow of urine quickly removed the whole dropsical effusion. The liver was soon felt to be greatly enlarged, lobulated, and rugose; which disease, it need scarcely be added, proved fatal a few months afterwards. But it is remarkable that the ascites never returned.

I have often used the same method since, both in simple ascites, and likewise where that affection formed a prominent part of a more general dropsy. The issue has of course been variable, and more frequently unfavorable than successful. But on the whole the results of my own observation, and the experience of others in Edinburgh, who have also tried the practice, lead to the conclusion, that digitalis not unfrequently succeeds in this way, when not only it, but likewise all other diuretics, taken internally, as well as the purgative method of cure, prove of no avail. It has likewise been tried with success in a few cases of obstinate excessive œdema of the limbs, in connection with general anasarca and Bright's disease of the kidneys, after diaphoretics, purgatives, and diuretics internally, had failed to give relief.

The late invention of the texture, called Spongio-piline, has facilitated the application of this treatment. A large sheet of spongio-piline, soaked moderately with a strong infusion of digitalis, made with one ounce of powdered leaves to twenty fluid ounces of boiling water, may be applied to the whole abdomen, or to each limb, and worn constantly without any material inconvenience to the patient. I lately met with a case of Bright's disease, in the University wards of the Royal Infirmary, in which considerable general anasarca, developed to a great extent in the limbs, and concurring, as sometimes happens, with a natural diuresis, was for a time reduced almost entirely by the soaked spongio-piline applied to both legs, although every other customary remedy had been administered unsuccessfully. In another case, that of a dissipated middle-aged woman, who presented all the characters of the variety of Bright's disease which is supposed to depend on chronic inflammation of the kidneys, diuretics given internally removed almost entirely the general anasarca, after diaphoretics had completely failed; the patient thereupon was speedily relieved also of an obstinate chronic bronchitis, liability to vomiting, and tendency to diarrhœa. But the ascitical effusion, which had been predominant from the first, was not reduced at the same time. On the contrary it went on increasing till the integuments became tense. The foxglove infusion was then applied by means of the spongio-piline, with the effect of augmenting the flow of urine in a few days; and then the ascites decreased, at first quickly, afterwards more slowly, till at length all dropsical effusion had disappeared, so that the woman, after seven months of treatment, left the hospital in a fair state of health to outward appearance, and with the urine much less albuminous, and much less loaded with the debris of tube-casts and epithelial cells.

On Nitric Acid in the Treatment of Asthma. By T. S. HOPKINS, M. D.,
Waynesville, Geo.

Since the spring of 1847, I have prescribed nitric acid in seven cases of asthma, with the most satisfactory and gratifying results. I was led to prescribe the acid in consequence of its accidental administration to a child, suffering from asthma, under the treatment of an esteemed medical friend, in an adjoining county. The child rapidly recovered. He informed me of

the fact, and I determined to test its virtues when an opportunity offered. In a disease of such protean form, assisting, as it often does, the best directed efforts of those who have grown gray in the pursuit of our time-honored profession, I deem experiment justifiable, particularly when it could result in no harm to the patient. As to its *modus operandi*, in the cure of asthma, I have nothing to say at present. I submit the question to those who have a better knowledge of the pathological condition existing in that disease than I profess to have. My knowledge of the efficacy of the remedy is based upon facts which I feel assured will convey to the mind of any one who may be disposed to give it a trial, the most incontestible proofs of its *decided* utility. I send you the following cases, with their history, as collected from the patient himself, the parent, or the master. Cases 1st and 7th were attacked with asthma, on being removed to the "lowlands," the one being free of the disease during a period of two years, the other twelve months. Could this be considered a *relapse*, or was it not *asthma*, produced, in these cases, by those causes which would have caused the disease in those who had never labored under it before; such as a humid atmosphere, with exposure to heavy fogs, walking bare-footed upon wet, low ground during the day, and sleeping with cold, wet feet at night.

CASE I.—Emma, negro girl, aged seven years, belonging to Mr. T. G. had been asthmatic for the last three years. At night she suffered much from dyspnoea and cough. Slept but little. During the day there was an amelioration of her sufferings, though she was never entirely clear of *dyspnoea*.

I ordered five drops of nitric acid, three times daily, in a wine-glass of sweetened water. I did not see her again until half an ounce had been taken. Every symptom of the disease had disappeared. I saw this patient in January, 1848. She continued well until last winter, when she had a return of asthma, which lasted a few days. Since that time she has shown no symptoms of the disease. A short time previous to the last attack, she had removed to a *low swamp* plantation.

CASE II.—Doctor, negro girl, aged six years, belonging to Mr. W. D. T., had been asthmatic from birth. Her father had suffered long and severely from the same disease. Prescription as in case 1. The disease rapidly disappeared; and although two years have elapsed, there has been no return.

CASE III.—Myra, girl, aged — years, property of Mr. W. S. T., had had asthma twelve months. The acid was ordered in doses of four drops, three times daily. A speedy recovery followed. I saw this case with case 2. She has continued well ever since.

CASE IV.—I never saw this patient. Twelve months ago, Mr. C., of Camden county, called upon me for a prescription for a negro girl, aged eight years, who had been suffering from asthma for a long time. I recommended the use of the acid, as in case 1; stating that I should charge him nothing for the advice. This gentleman's brother informed me, a few weeks since, that the child was rapidly *cured* by my prescription, and had continued well since.

CASE V.—W——, aged seven years, a son of an estimable friend and planter. This was the most severe case I had ever seen. The nightly paroxysms of dyspnoea and cough were most alarming and distressing, frequently threatening immediate suffocation. He had been treated by the most skilful physicians, in vain. I freely and unhesitatingly expressed my

doubts as to my ability in relieving this case. The acid, in doses of five drops, was prescribed, three times daily. In a week there was a decided improvement. In a month he was *cured*. Two years have elapsed, and he continues *well*. The father of this boy died during the summer of '49, of phthisis pulmonalis. His mother presents the symptoms of incipient tubercle, and his brothers and sisters, without exception, have been the subjects of ulcerated sore throat.

CASE VI.—Mrs. C——, aged fifty, a resident of Middle Georgia. She spent the summer in this place, when I was called to see her. Fifteen years ago, she had hydrothorax, from which she suffered long, and came near dying. She, however, slowly recovered, and after a few years interval of apparent good health, she was attacked with asthma. The attacks were periodical, lasting about a week, and were distressingly severe. She had tried a little of everything. I recommended the acid, in doses of 10 drops, three times daily. Immediately after, she left for her home, and I heard nothing of her until a month since, when I received, through a friend and relative of her's, the gratifying intelligence that the remedy had put a speedy stop to her sufferings, and she was then well.

CASE VII.—Was a negro woman, some fifty or sixty years of age, belonging to Mr. R. H. She had been asthmatic for many years. The usual remedies had been administered in vain. I prescribed the acid as in case 6. I prescribed for this case twelve months ago, and heard nothing of her until the past week, when Mr. H. informed me that she was speedily, and as he thought, permanently relieved, under my prescription; but upon removing her to the low lands, a few weeks since, she was exposed to a shower of rain, and the disease returned.—*Charleston (S. C.) Med. Journal and Review.*

Sense and Genius in the Study of Medicine.

"Pray, Mr. Opie, may I ask you what you mix your colors with?" said a brisk dilettante student to the great painter. "With *brains* Sir," was the gruff reply—and the right one. It did not give much of what we call information; it did not expound the principles and rules of the art; but, if the inquirer had the commodity referred to, it would awaken him—it would set him a-going, a-thinking, and a-painting to good purpose. If he had not, as was likely enough, the less he had to do with colors and their mixture the better. Many other artists, when asked such a question, would either have set about detailing the mechanical composition of such and such colors, in such and such proportions, rubbed up so and so; or perhaps they would (and so much the better, though not the best) have shown him how they laid them on; but even this would leave him at the critical point. Opie preferred going to the quick and the heart of the matter—" *Brains, Sir.*"

Sir Joshua Reynolds was taken by a friend to see a picture. He was anxious to admire it, and he looked it over with a keen and careful eye. "Capital composition—correct drawing—the color, tone, chiaroscuro excellent; but—but—it wants, hang it, it wants—*that*," snapping his fingers—and, wanting "that" though it had everything else, it was worth nothing.

Again, Etty was appointed teacher of the students of the Royal Academy, having been preceded by a clever, talkative, scientific expounder of æsthetics, who delighted to tell the young men *how* everything was done, how to copy this, and how to express that. One came up to the new master. "How shall I do this, Sir?" "Suppose you try." Another, "What does this mean, Mr. Etty?" "Suppose you look." "But I have looked." "Suppose you look again." And they did try, and they did look; and they saw and achieved what they never could have done, had the *how* or the *what* (supposing this possible, which is not likely in its full and highest meaning) been told them or done for them; in the one case sight and action were immediate, exact, intense, and secure; in the other, mediate, feeble, and lost as soon as gained. But what are "*brains?*" what did Opie mean? and what is Sir Joshua's "*that?*" What is included in it? and what is the use or the need of trying and trying, of missing often before you hit, when you can be told at once, and be done with it; or of looking when you may be shown? Everything in medicine and in painting—practical arts—as means to ends, let their scientific enlargement be ever so rapid and immense, depends upon the right answers to these questions.

First of all, "*brains,*" in the painter, are not diligence, knowledge, sensibility, a strong will, or a high aim—he may have all these, and never paint anything so truly good or effective as the rugged woodcut we must all remember, of Apollyon bestriding the whole breadth of the way, and Christian girding at him like a man, in the old sixpenny "*Pilgrim's Progress;*" and a young medical student may have zeal, knowledge, ingenuity, attention, a good eye and a steady hand—he may be an accomplished anatomist, stethoscopist, histologist, and analyst; and yet, with all this, and all the lectures, and all the books, and all the sayings, and all the preparations, drawings, tables, and other helps of his teachers, crowded into his memory or his note-books, he may be beaten in treating a whitlow or a colic, by the nurse in the wards where he was clerk, or by the old country doctor who brought him into the world, and who listens with such humble wonder to his young friend's account, on his coming home after each session, of all he had seen and done, and of all the last astonishing discoveries and revolutions of the day. What the painter wants, in addition to, and as the complement of, all the other elements, is *genius and sense*; what the doctor needs to crown, and give worth and safety to his accomplishments, is *sense and genius*: in the first case, more of this, than of that; in the second, more of that, than of this. These are the "*brains*" and the "*that.*"—*Edinburgh Monthly Journal of Medical Science.*

On Cod Liver Oil. By Dr. GERHARD.

1st. *Of the Oil.*—That the light colored oil can be taken without difficulty by patients whose stomachs have steadily rejected the brown oil.

2d. *Of the mode of administration.*—That a few of the patients have taken the oil without any adjunct to disguise its taste. That its nauseating properties are corrected by its administration with milk; but that its taste is most effectually disguised by the froth of porter.

3d. *Of the time of its administration.*—That, as a general rule, it has been taken *before* meals, but that in four instances where it was not tolerated before meals it was taken readily after meals.

4th. *Of diarrhœa as a contra indication to its use.*—That the existence of diarrhœa is not a positive contra-indication to its use. In three instances in which patients were thus affected, no increase of the symptoms was produced by its use, and no diminution by its abandonment. In a fourth instance, when the diarrhœa had previously existed, the discharge appeared to be increased by the exhibition of the oil, and abated with its withdrawal.

5th. *Of its effects in cases of phthisis pulmonalis.*—That patients using the oil have increased in flesh, in weight, and strength. That, while using the oil, their cough and expectoration have diminished; that with some, hectic and rigors have entirely disappeared.

That six of them have been so much benefitted as to leave the hospital and resume their former occupations. That in one instance, a patient who entered the hospital with cough, copious purulent expectoration, extreme emaciation, inability to leave his bed, and with the physical signs of a cavity under left clavicle, after six month's use of the oil, left the hospital weighing 140 lbs., with little or no cough, no hectic or rigors, and with an almost entire absence of expectoration: the physical signs having greatly diminished.

6th. *Of the physical signs.*—That the improvement of the physical signs is not coincident with that of the general symptoms.

7th. *Of its use in general scrofula.*—That in scrofulous diseases where there was no reason to suspect the existence of pulmonary tubercles, the improvement of the patient's health has been very decided.

8th. *Of congestion of lungs as produced by cod liver oil.*—That there has been no decided evidence of such a result following the use of the oil in the preceding cases. [Two patients of the twenty, while using the oil, had severe attacks of hæmoptysis, but there was no reason to refer them to the use of the remedy.]

9th. That in those cases which have terminated fatally, the appetite, the nutrition, and the strength of the patient appeared for a time to be decidedly increased; that the life of the patient appeared to be in this manner temporarily protracted; but that for a few weeks immediately preceding death, the remedy seemed to have entirely lost its value.

10th. *Length of Time, &c.*—That to be of any decided permanent benefit its use must be steadily persisted in. It should be continued even after the most striking symptoms of the disease have in a great measure entirely disappeared.—*New York Journal of Medicine.*

Treatment of Nævi by Compression, or Strangulation Knot. By Mr. FERGUSSON.

When Mr. Fergusson wishes to destroy the tumor by strangulation, he does not follow the usual method of transfixing the nævus with two pins at right angles with each other, and twisting strong silk around them, but we have seen him proceed in several cases, and amongst others, in a very recent one, in the following manner:—

He armed a common curved surgical needle with a strong thread, about eighteen inches long, and passed the needle, (the thread being double,) through the eye of the tumor. One of the threads, about two inches from the eye of the needle, was then cut across, and Mr. Fergusson introduced one of the loose ends, lying on the other side of the tumor and corresponding with the cut thread, into the eye of the needle, which latter then carried two loops. (The end to be chosen is the one continuous with the thread first cut.) The needle was now carried at right angles to the direction it took at first, and when it was disengaged from the threads, the latter presented around the tumor *two loops and four ends*. Mr. Fergusson now pulled the threads gently, so as to ascertain their relations, and he then made two tight knots with the four loose ends. Thus the tumor was effectually strangulated, being surrounded, not only with two knots, but with four firm *loops*.

The treatment of *nævi* would, however, appear to require various modifications, according to the size of the growth, and the locality where it has sprung up. We saw, for instance, a few weeks ago, a child under Mr. Fergusson's care, in whom the vascular tumor occupied the right cheek. Here it was desirable to obtain obliteration without much loss of blood, or an unsightly cicatrix; and Mr. Fergusson determined on trying the effects of firm and permanent compression. He proceeded in the following manner:

Two strong pins were first thrust through the base of the tumor, (which was about the size of a crown piece,) from below upwards, at about one inch distant from one another; and two others were passed transversely, being separated by the same space as the first. Mr. Fergusson then rolled a narrow strip of lint into the shape of a thick string, and applied it firmly round the circumference of the swelling, and under the pins. The constriction having thus steadied the tumor, a circular pledget of lint, about a quarter of an inch thick, was then placed upon the *nævus*, and secured in that situation by strong silk threads, which Mr. Fergusson wound round the pins, and carried across the pledget. The latter, by these means, exercised a powerful pressure upon the growth, which was expected to be sufficient to cause the complete atrophy of the latter. Nor was Mr. Fergusson deceived in his expectations. Some weeks afterwards, when the needles had been withdrawn, and some ulceration which had taken place at the orifices had cicatrized, the tumor seemed all but obliterated.—*London Lancet*, Oct. 1850.

Report of a Committee of the Massachusetts Medical Society on Homœopathy. Adopted by the Counsellors, Oct. 2, 1850, and ordered to be printed.

At a meeting of the Counsellors of the Massachusetts Medical Society, held May 30th, 1850, an application was made by Dr. Isaac Colby, of Salem, to resign his Fellowship, assigning as a reason for so doing, that he had become a convert to the doctrines of homœopathy. It was therefore "resolved that the documents (Dr. Colby's papers) be referred to a committee of three, whose duty it shall be to devise some course of action to be pursued by the Society in regard to all homœopaths, and report at the next meeting."

The undersigned regret that they were selected to serve on this committee, because they feel that the subject is a delicate one, and that any course which may suggest itself will be attended with some degree of difficulty and embarrassment. At the same time, however, they do not intend to shrink from the duty that has been imposed on them. They are satisfied that the period has arrived when the Society should decide and make known what position it intends to take in relation to homœopathic practitioners. Whether it means to regard them as fellow-laborers in the great cause of science and humanity, in which its own members profess to be engaged; or whether it will hold no communion with them, but place them on the same footing with the various classes of irregular and empirical practitioners with whom it acknowledges no fellowship.

It is obvious that one of these courses must be adopted, and yet the Society will bring upon itself some degree of odium whichever it may choose to pursue. It does not seem right that individuals, who hold in contempt the commonly received doctrines of the medical profession, as received by this Society, and who freely denounce them as unsound, if not unsafe, should at the same time be enrolled as members of our deeply-cherished institution, and thus gain a position, which they perhaps would not otherwise have so readily attained.

On the other hand, it may probably be urged, that some of the practitioners of homœopathy are men of talent and education, who have been well instructed in the commonly-received doctrines of medicine, but who, from some new light with which they have been favored, feel bound to abandon them and adopt a very different course of practice from that pursued by the great mass of medical men—that they have done nothing to forfeit their good name; that they are acting conscientiously, being fully convinced that the world was in the dark in relation to the healing art, till it was enlightened by the revelation of Hahnemann.

The Committee are not disposed to deny the truth of this statement; at the same time they cannot perceive that it should have any influence on the course which the Society may think proper to adopt.

A great majority, probably, of the Fellows of our institution believe, that the homœopathic physicians are mistaken in their views of the nature of disease and the mode of treatment. They believe that the system is radically wrong; based on false views of pathology; resting rather on gratuitous assumptions than well-founded facts. They do not believe that there is anything to support their doctrine of the nature of disease, or their principles of its treatment, or their mode of administering remedial agents. It is of no consequence to us whether this system is adopted by individuals in good faith, from a conviction of its truth, or whether it is taken up from mercenary or other sinister motives. If the system be a bad one, mischievous and unsafe when carried into practice, it is of very little consequence to the community whether its disciples are honest or not; whether they are trying to dupe others, or are merely duped themselves.

The Committee, however, have neither time nor inclination to discuss at large the doctrines of homœopathy; nor do they think it necessary to do so, as they have no doubt that the subject is well understood by most of the Fellows of this Society. They will merely remark, that in their opinion, the boasted cures that have been effected by this mode of practice are attributable to the unaided efforts of nature; to the fact, that the medical

attendant did not interfere with the natural restorative processes, by means of which a vast many of the maladies to which human nature is subjected are brought to a favorable issue. It is enough for us to believe that the doctrine of the origin of disease, of the principles by which it should be managed, and the method of administering remedial agents, as proposed by Hahnemann and his disciples, are untrue and unsafe, and that therefore we cannot give to those who adopt this mode of practice the sanction of our Society by receiving them as members of our fraternity. We cannot say to them in good faith, as we do in our diploma to all who become our associates, that they are well skilled in the healing art, and most worthy of the honors and privileges of the Society.

It does not seem to your Committee that either the homœopathic practitioners or the community at large would have any right to complain of this course. If these practitioners honestly believe the system they have adopted, they can surely have no wish for any professional connection with individuals whose doctrines they repudiate; and the public certainly could derive no advantage from an association whose members entertained such discordant opinions.

It would, perhaps, be doing injustice to homœopathy, if it were not admitted that the promulgation of its doctrines had, at least indirectly, been of some service to the cause of medical science. It may have taught us to place more confidence in the curative powers of nature, and less in medicinal agents, in the management of disease, than we have hitherto done; and it may well be doubted, if it has exerted any salutary influence beyond this.

It cannot be denied by any one, who has given any considerable degree of attention to the subject, that the present period is an inauspicious one for the success of any medical theory, that does not rest on a substantial basis. At no time in the history of the world, has the science of medicine been cultivated on principles so philosophic as at the present. All theories have been abandoned; we hear nothing of the humoral pathology or the doctrine of solidism. There are none at the present day who acknowledge themselves to be the disciples of Boerhaave or Hoffman; of Cullen or Brown; and there is not probably an individual who now supports the once popular doctrines of Rush or Broussais. All the true votaries of our profession are earnestly seeking for truth alone; accumulating facts by patient and toilsome observation of disease and its effects, and drawing conclusions from them by the cautious process of inductive reasoning. At no period have the various changes which disease produces in the different tissues, organs and secretions of the body been subjected to so rigid a scrutiny. The microscope and animal chemistry have been enlisted in the service of the pathologist, and we are already reaping rich fruits from his labors. If we have not yet gained more control of disease than our predecessors had, we better understand the power of remedies; we know more than was formerly known, when it is best to withhold them altogether, relying on the powers of nature, and when they can be used advantageously in aid of those powers.

At such a period as this, your Committee cannot persuade themselves that the doctrines of homœopathy can have any very extensive or permanent influence; at the same time they think that it is the duty of this Society to avoid giving them its sanction in the slightest degree. They

therefore beg leave respectfully to offer for the consideration of the Counsellors the following resolutions:—

1st. *Resolved*, That any Fellow of this Society who makes application to resign his Fellowship in consequence of having adopted the principles and practice of homœopathy, may be permitted to do so, on paying his arrearages; but he shall not be entitled to any of the privileges of Fellowship, nor shall his name be retained in the list of Fellows.

2d. *Resolved*, That a diploma from a homœopathic institution shall not be received as any evidence of a medical education; nor shall the Censors of this Society regard the attendance on the lectures of such institutions, nor the time passed at them, as qualifications which shall entitle candidates to an examination for a license from this Society.

Which is respectfully submitted, by

GEO. HAYWARD,
J. B. S. JACKSON, } *Committee.*
O. W. HOLMES.

Boston, Oct. 2, 1850.

Inunction in Scarlatina.—Dr. Ebert, of Berlin, reports 28 cases of severe scarlatina, in which this mode of treatment was very encouraging. He says no desquamation occurred in any case after thorough inunction, nor were any of the frequent sequelæ observed in such cases as dropsy, rheumatism, abscesses, etc. He also regards the anointing as preventing infection.—*N. Y. Medical Gazette.*

[I have employed it recently in three cases of scarlatina—two uncomplicated, the third accompanied by strongly marked typhoid symptoms—with apparently happy results. I say *apparently*, because it would be premature to draw any deduction from its use in so small a number of cases. My object is merely to call attention to this mode of treatment, which, according to the experiments of Dr. Schneeman, published in our January issue for 1850, corroborated by those of Dr. Ebert, of Berlin, above mentioned, promises, in general, to conduct to a favorable issue a very intractable malady.

In these brief remarks, I cannot give the details of the symptoms and phases of the cases, but will simply mention the general effects which seemed to me to result from the anointing process.

In the three cases, there was a more rapid reduction in the force and frequency of the circulation, and, *pari passu* with it, a modification of the burning heat of the skin, than in cases treated in the ordinary way; the abatement in the febrile phenomena being attended by a sensation of comfort. Indeed, in one case, so great was the relief from the parched heat afforded by the inunction, that the patient, a black boy, begged his father to rub him once or twice every night, that is to say between the time of the last rubbing prescribed at night, (9 o'clock,) and the first rubbing in the morning. All expressed themselves as not being in the slightest degree incommoded by the heat of the skin, if the intervals between the rubbing were not too long. A happy effect was produced upon the skin; its temperature, after two day's use of the lard or sweet oil, was very pleasant to the hand of the observer. In all it was rendered soft and velvety,

(even in that of the negro;) and in one of the cases, a lad about 17 years old, at the Marine Hospital, a very abundant crop of miliary vesicles (sudamina, made its appearance on the second day of the rubbing, covering him from head to foot, which symptom I considered as denoting the free action going on in the skin, as proving that its pores were completely opened.

In none of the three patients was there discharge from the ear, abscess, diarrhœa, dysentery, or dropsical swelling. In two, the urine only moderately red, and soon became clear; in the third, it retained its ordinary pale color. Dr. Ebert, above quoted, states that in his twenty-eight cases no desquamation occurred; it was not so in those treated by me; but it was so slight, that it did not amount to more than a slight furfuraceous exfoliation.

The fat or oil (for I have used both animal and vegetable,) seems to me to exert a powerfully sedative influence primarily on the cutaneous surface, relaxing its textures, opening its pores, and subduing phlogosis; thus restoring the function to this important organ, the interruption to which is, in my humble opinion, the source of the formidable train of sequelæ, so often noticed, such as congestion of the kidneys, accompanied by albuminous urine, and giving rise to dropsical collections; that of the bowels causing diarrhœa, dysentery, etc.; the susceptibility to atmospheric vicissitudes, the formation of abscesses, etc. The secondary effect is on the nervous system, allaying the high excitement induced in it by the cutaneous inflammation, etc.

It is understood that I am only speaking of inunction as, at best, the base of the treatment; other means must not be omitted; especially in Scarlatina Maligna, as every physician is aware, must the powers of the system be kept up by tonics, stimulants, etc.

I have used both animal and vegetable oils, and do not know that a preference should be given to either class. The general impression is that olive oil is more sedative in its operation than the animal fat. My experience does not enable me to decide upon their comparative properties.

Charleston Medical Journal.

D. J. CAIN.

New Method of relieving Retention of Urine without the use of the Catheter. By M. J. J. CAZENAVE.

When called to a patient having retention, complete or incomplete, M. Cazenave in the first place directs the large intestines to be cleared out by an enema. When this has returned, a second is administered, but consisting solely of a quart of cold water. Absolute rest on the bed is enjoined; while cloths dipped in cold water, or, better still, bladders of ice-cold water, or pounded ice, are applied to the anus, perineum, thighs, and hypogastrium. If the patient do not void his urine in the course of half an hour, or void it only very scantily, he is placed on the edge of the bed, which is properly guarded, and a stream of cold water is poured on the region of the bladder during from twenty to twenty-five minutes. After the lapse of this time another enema of cold water, and small, smooth fragments of ice, are introduced into the rectum, the cold application to the external parts being at the same time continued. The cases in which this mode of treatment is found applicable are those in which the retention proceeds from acute inflammation or spasm.—*L'Union Medicale. New York Jour. of Med.*

Puerperal Mania and its connection with Ovaritis. By PROF. C. R. GILMAN, M. D.—Dr. Gilman stated that he had recently witnessed a post mortem examination of a patient, who died six weeks after her confinement, and fourteen days after the occurrence of puerperal mania. The symptoms of the mental disease commenced with the derangement of the genital functions. On making the autopsy, no disease of any organ was discovered, except that the right ovary was very much enlarged and intensely congested; the broad ligament of the same side was in the like condition. The left ovary and broad ligament was but very little congested. The uterus had resumed its normal size and appearance. He asked if any member of the Society had noticed the coincidence of this condition of the ovary with puerperal mania? Dr. Barker, late of Norwich, Conn., had told him that he had seen one case, in which a similar condition of the ovary was found. Vascular excitement does not usually attend this disease, (puerperal mania.) Dr. McDonald, in the *New York Journal of Medicine*, and in the *American Journal of Insanity*, has called attention to a form of the disease, attended with great vascular excitement, which proves fatal. Usually when the pulse is much excited, danger may be apprehended. Dr. G. further stated, that he had seen two such cases, both of which proved fatal.—*N. Y. Journal of Medicine.*

EDITORIAL DEPARTMENT.

Registration of Births, Marriages and Deaths.

Some years ago a registration law was passed by our State Legislature which, from some defects in its details, has only been partially operative. The subject has been under discussion during the present legislative session, and efforts have been made to obtain a repeal of the law, in the place of improving it so as to render it effective. What disposition has been, or is likely to be made of the matter, we are unable to say; but it is to be hoped that a measure of so much importance to medical science, and the public welfare, if it have the go-by at the present session, will be revived hereafter, until it is perfected, and its practical operation secured. Our attention has been called to some remarks on the subject by Dr. Tuthill, member of assembly, which are reported for the *Albany Evening Journal*. We copy such portions as will more especially interest the Medical reader. Medical practitioners, if they are disposed, can do much to further the passage of a proper law, by, in the first place, informing themselves of the ad-

vantages to be secured by it, and, in the second place, taking pains to enlighten those who are directly or indirectly instrumental in the making of laws. With a view to these ends, we make the following extracts from the able speech of Dr. Tuthill, who is entitled to the thanks of the profession for his efforts in behalf of the bill.

The House having resolved itself into committee of the whole, on the bill to repeal "An act providing for the registry of births, marriages and deaths,"

Mr. Varnum moved to amend by striking out all after the enacting clause, and substituting the amendment proposed to the Senate.

Mr. Tuthill said: Mr. Chairman, I hope the amendment of the gentleman from New York (Mr. Varnum,) will prevail, and that all the papers on this subject may be referred to a select committee for their criticism and report. The defects of the present Registry Law are obvious, and its friends are willing to adopt any amendment which will facilitate its operation; but against its unconditional repeal we earnestly protest. While we were prepared to hear from all quarters of this House, that the law is unpopular and inefficient, we confess our surprise to hear a doubt of its utility if successfully carried out. But the gentleman from Onondaga (Mr. Le Roy,) has discovered that the whole system is a vain and useless incumbrance, an unprofitable tax upon our time and property; and the legislature is asked to pronounce, by a repeal of our law, that to be folly, which the philosophers of all time have regarded as the highest evidence of a thoughtful attention to the public good. Surely here is another evidence that in our day wisdom has come to its culmination. Yet, although it may not *instruct* it may *amuse* us to look back occasionally and note how seriously they of the olden time regarded these matters, which to our clearer vision, are so trifling.

Registration of births, at least, has been practiced in almost every civilized country, and for nearly two thousand years. Marcus Aurelius ordered that parents should report the name and birth of each child before it has passed the thirtieth day of its existence; and the learned Ulpianus required records of all who arrived at any of the great eras of life, during the year. Similar records were made at Athens, and the Athenians regarded them as the only safe basis for calculating their future strength; and before any enterprise was undertaken, which might last through a series of years, they were consulted as the census of the future. The first tables in which the ages of the dead were recorded, were kept at Breslau, in Silesia. From these Dr. Halley deduced his "table of the probabilities of human life at every age," the first of the kind ever prepared; and a little later, De Moivre, reasoning from the same facts produced his "Treatise on Annuities," and "The Doctrine of Chances." Registries were established in London, 1728, and at Northampton, 1785, and on these were based the calculations of the British Life Insurance companies, until quite lately. Bills of mortality on an improved plan, and much more valuable, and for many more purposes, were established in 1772 at Chester, and in 1778 at Warrington. The Prussian government has always exhibited great care and attention to these vital statistics, and still more re-

markably has Sweden, which has faithfully kept them in their present details, since their establishment in 1775.

I am well aware that our wise demagogues disclaim all the maxims of ancient or foreign wisdom; and the habit of these countries is not quoted as our example. The "Spirit of the Age" is in custody of men too much attached to our home, to be warned or benefited by foreign experience. The spirit's cry, dictated by men reckless of every thing but their immediate popularity, is, *down with every institution and repeal every act that does not yield its six per cent. for the current year.* Were this the time and would the form of the question under debate allow, we would plead that this law, unprofitable as gentlemen deem it, will very soon repay us all its costs. I suppose the tables of longevity most used in the United States for estimating the value of life estates and annuities are those of Prof. Wigglesworth, and that all of them are founded on observations taken in other lands, of dissimilar climate, unlike habits of diet and regimen, differing customs, and a multitude of other disparities, all which tend to make any foreign registry an incorrect basis for determining the probable length of American life, and to vitiate all depending conclusions. What propriety would there be in the operation on a New York community of an annuity table, calculated on the high expectation of life, in Geneva? In that city, the probability of life is forty-five years. How long would a Life Insurance Company, with its premium regulated on that probability, be able to survive the drafts upon its treasury, if its policies are held in New York, where the average age of death is supposed to be somewhat less than twenty years? The relief we seek is universally needed if not universally felt. For no widow exchanges her legal thirds of a deceased husband's estate for an annuity or its present worth, but gains or suffers by any lack of adaptation to her situation of the fundamental tables of the computation. In the language of the Edinburg Encyclopædia, "there are few subjects capable of being submitted to calculation, in which so many errors have been committed, or errors in theory led to so much individual distress, as in the subject of annuities. The Life Insurance companies among us, generally take the Carlisle tables as the basis of their operations. That these, though they more nearly approach the truth than any other, are not true for our locality, is suspected by all conversant with the history of these institutions. Many of those on the mutual plan are making annual dividends of fifty per cent. on the premiums received. There are other well sustained reasons to explain these enormous profits, but doubtless *one great* cause is the fact, that the Carlisle tables put the expectation of life too low. To acquire data for determining accurately what that is, is an incidental but vastly important result, derivable from a system of registration. Indeed, the gentleman from Onondaga hinted, that it was only of use to the Life Insurance companies. Barring the exclusiveness of the objection, the gentleman has given here a worthy and valid reason for sustaining this or some better system. For so universally extended are the dealings of these societies, and the people have to such a degree availed themselves of their privileges, that whatever benefits the company with little or no loss to the State, must prove a general good. And whatever tends to increase the light thrown on the subject of life annuities—whatever tends to lessen the amount of the premium demanded, without endangering the value of the policy, directly benefits that large and increasing minority of the people who are insured.

Again, bills of mortality of the fullness contemplated in the present law will do much to increase our medical and physiological knowledge. When taken in connection with the meteorological observations now made so nearly perfect, they will soon give us data sufficient to settle the question, of much interest to the political economist, of the relative mortality of the sexes. They will enable the sanitary philosopher to learn "the law that governs the waste of human life," perhaps arm him to drag to light the hidden causes of the mortality of our cities, and to some extent to stay the fearful progress of our epidemics. The registry will give him a map of the fatal diseases of the State, and on the same sheet a map of the employment, age and habits of the deceased—while the meteorological record gives him a map of the atmospheric phenomena of the season; and by applying the one to the other, he can demonstrate the healthfulness of any required profession in any one spot, as compared with the same occupation in any other place. The causes which make the prosecution of any branch of business, at one point, detrimental to health can be removed, and thus infinite gain accrue to property and life, by predicting with certainty the result of a contemplated enterprise.

These are not fancies but sober facts, as a reference to the experienced of any city where such records have been kept, will show.

In Geneva, records of mortality have been kept for 260 years, and they show that a child born there now, has five times greater expectation of life than one born in 1590. The probable duration of life there, at the close of the seventh century was less than 20 years; a century later it was 32 years, and in 1833, it had reached 45 years. It is not pretended that this great improvement is due solely to the registration there practised, but, that it has done much we are not at liberty to doubt. Instances might be quoted, showing how places enjoying a reputation for salubrity have been startled on referring to their newly established registres, at a surprising mortality in their midst. The tables annually published sometimes exhibit striking diversities in the mortality of different parts of the same city, which arrest the attention of the medical philosopher. His deductions and conclusions are kept before the people till they enter into general calculation and modify popular practice. Local partialities which had blinded them to their peril are removed. Nuisances are abated, and deleterious practices omitted. Employments injurious to health are abandoned, or so followed as to lessen their danger. Thus life is lengthened, comparative immunity from disease is secured, and human happiness is increased. We do not look for such results in one year or four; they are the offspring of time and study. The State of New York, with an enlightened forethought and liberality worthy of the high commendation accorded her, has expended thousands of dollars on a Geological Survey. The facts accumulated by that survey are, for a time, of limited use; yet no thinking man doubts that hereafter they will become practically useful to the people. Geological details which seem to possess no possible interest except for the scientific man who has made geology his study, when subjected to the chemistry of thought, will surrender some unsuspected law of nature. And that law becoming incorporated with the maxims of the agriculturist will modify and improve his practice, and his farm will yield the more abundantly. So these statistics, sneered at now as valueless, will afford a rule of regimen, of diet, or of physical habit, that shall give us an increase of days.

and render them more free from the ills to which flesh is heir. Considerations of this sort influenced the National Convention of Physicians assembled at New York, in 1846, to urge upon this, and the Legislatures of other States, the adoption of a registry system. The arguments were deemed valid then, and I know not what has occurred in the five years past to invalidate them.

* * * But it has not, nor will it, prove a failure. With all its faults about it, when once it is understood that we mean to settle down to it as a law of the land, I have no doubt the registries will come in full and accurate from almost every district. I do not believe our constituents wish this act repealed. They have not asked for it. True, four several petitions—all from the same county—have been received, praying for an unconditional repeal; and as an offset, four Life Insurance companies have sent up their prayers for an act providing for a more thorough registry. Possibly there may be found some men in the State who wish to get rid of it—knowing well its advantages; and more, regarding it as one item in an obnoxious School Law, inasmuch as School District Clerks are charged with its duties, extend their enmity to that law over this. But the thinking part of community I believe approve of the law, and when it learns that the orders of the Legislature will not be countermanded before they can be obeyed, will cheerfully submit to all its requirements. And here I venture a prophecy, that when this or a similar law has stood twenty years upon the Statute Books, a proposition to strike it out would be received with about as much surprise, and about as much favor as a serious proposal to refuse to take the census.

New York State Medical Society.—The annual meeting of the New York State Medical Society was held in Albany on the 4th and 5th of January last. We learn that the attendance was larger than usual, and that the members present were highly gratified. The Society are to hold a *semi-annual* meeting in Buffalo on the first Tuesday in June next. This is a new movement on the part of the Society, which will, as we think, be very acceptable to those residing at a distance from Albany. We do not doubt that we express the general sentiment of the Profession in this city when we say that we feel gratified and honored in the selection of Buffalo for the first *semi-annual* meeting. Dr. Frary, of Hudson, Columbia Co., was elected *President* of the Society; Dr. I. S. Sprage, of Otsego, *Vice President*; Dr. Hun, of Albany, *Secretary*; Dr. Van Olinda, *Treasurer*. Dr. B. Burwell, of this city, was elected one of the Censors for the Western District.

Buffalo Hospital of the Sisters of Charity.—Dr. Charles Winne has been appointed one of the attending surgeons at the above institution.

Annual Meeting of the American Medical Association.—The official notice of the annual meeting of the *American Medical Association*, published in this, as in most other medical journals, contained an important error. The meeting is called for the *second Tuesday* in May. It should have been the *first Tuesday*, which will be May 6th. Our readers will please take notice of this correction.

The meeting will probably be well attended. It will afford members of the profession at the north, a good occasion for visiting one of the most attractive cities in the Union, and forming acquaintances with our southern brethren.

The Anatomy, Physiology and Pathology of the Eye. By HENRY HOWARD, M. R. C. S. L., Surgeon to the Montreal Eye and Ear Institution. Montreal: Armour & Ramsay. 1850. 12 mo., pp. 517.

This work has been on our table some weeks, and we must confess even now that we are not prepared to speak of its merits from personal examination. The commendatory notices which it has received in other periodicals lead us to think it a valuable contribution to ophthalmic literature. For sale by Derby & Co.

Legal Restrictions on Entering the Professions.—A medical friend has handed us a number of the Newark, N. J., *Sentinel of Freedom*, containing the subjoined editorial remarks on the policy of allowing the largest liberty to all desirous of practising the professions of law and medicine. Our readers will agree with us that the remarks are not without significance and force; and it is refreshing occasionally to meet with an expression of such views in the newspaper press. New Jersey is one of the few States in which some legal restrictions upon the practice of the professions are retained. In most states of the union, such restrictions, if they ever existed, have been actually or virtually abrogated. In this state the clamor against the paltry provisions that formerly existed, was so great that the majority of the profession were in favor of giving the free trade system a trial, in order that the public might, in the end, discover the fact that proper legal restrictions are for its own protection, much more than for the protection of the profession. The public are now enjoying the fruits of a liberal legislation on the subject; and if the people are satisfied, the profession, we pre-

sume, will not be the first to find fault. We venture the prediction, however, that the time will come, although we or our readers may not see it, when the public will learn that its own true interests and safety are concerned in deciding who shall, and who shall not, practice the profession of medicine. The public, by that time, too, will have discovered that to make liberal appropriations for medical education, and the encouragement of medical science, is a matter affecting the general weal full as much as to pay, from the state treasury, large sums for the publication of works on entomology, ornithology, etc., or for engravings of the different varieties of Indian moccasins or Irish potatoes! The latter should, doubtless, be done, but the former should not be left undone. But if all these things were rightly estimated at once, there would be too limited a field in the future for progress!

We do not design at present to discuss the subject expressed in the caption to this article. The time has not come for such discussion, and it is quite uncertain when it will come. The profession, as well as the public, must undergo a change ere the discussion of the subject can be expected to tend to any effect. The profession has the power to accomplish much, by enlightening the public mind, and influencing the public will. Nay, the profession might effect anything that is truly to be desired. But for this, a different *status* is requisite than at present exists. When the whole profession of a state, and of the union, are imbued with a zealous regard for the honor and usefulness of the profession, and are willing heartily to cooperate in efforts to elevate its character, and promote its interests; then will open a new era in the annals of medicine; and if such an era do not open till then, the profession should be the last to complain. But we had no design of saying half so much by way of preface to the following article:—

“They are going, at Trenton, to *reform the professions of LAW and MEDICINE*. Well, not only the legislative humanity, but its intellect also, will be puzzled. The most remarkable feature of all is that they intend to make the medical profession in New Jersey, and legal too, for aught we know, almost as perfect as they are in New York. This will be a wonderful achievement. And nobody will be more surprised at New-York's being taken as a model to be copied, than the gentlemen of the faculty and the long robe in the great city themselves. According to the reiterated assertions of the most distinguished among them, there never was, before or since the time of Paracelsus, such a lamentable amount of quackery and pettifoggery, as now is growing under the cultivation of the reformatory enactments of the state of New York.

The supposition, then, must be, that they who are urging on improvement in the practice of those learned professions in this state, are not for following

in the footsteps of the erratic New York doctors and lawyers; but only for adopting the theory of their legislators, and copying their statutes. Does any one expect that the legislation of another state can be transplanted to the soil of Jersey, and not produce the same vile crop of incompetency and charlatanism that it has stimulated in the land whence it has been borrowed? The consequences of this "free trade" in law and medicine, so far as they have been developed by our sister across the Hudson, do not certainly recommend the theory.

There was a time, when wholesome restraints upon licence, which some persons call liberty, were thought to be useful checks against fraud and deception. The general enthusiasm for civil and political freedom has changed all this, and made the least restraint upon personal action in the highest degree unpopular. It is a wonder that any traces of it at all are left. There are, however, some. One, for example, is the requisition, that the owner of a steamboat shall procure and publish a certificate of the soundness of his apparatus at stated periods. People are even yet very much dissatisfied that other and more stringent restrictions are not imposed on men who deal in steamboats. Does not the public run a greater hazard every day, sometimes in respect to life, sometimes in relation to their property or reputation, in coming into connection with practitioners at the bar, or members of the medical profession? Certainly, the risk incurred, by being put in the power of these, in life, estate, and fame, is quite as great as travelling in steamboats; and the loss and suffering quite as hard to be foreseen, or be avoided. It requires as much sagacity, every one must allow, to distinguish between a good and bad physician, as to tell a sound steamboat from a rotten one.

It cannot be denied that the less favored portion of mankind need some help to save them from being made the prey of the unprincipled and designing. The qualifications required of men for the privilege of exercising certain callings, ought to be viewed, as they are evidently intended, as restrictions, not privileges; and designed as safeguards for the rest of the world. Somehow or other, instead of this obvious view, the reverse aspect has been so adroitly presented, and somebody has had the ingenuity to persuade the public that they have, as a body, a greater interest in extending the facilities of getting into a profession, than they have in increasing the securities that the professions at large shall discharge their responsible offices with integrity and talent. The very statement of the matter, proves, at a glance, that the principal concern of the mass of the public is, that there should be some evidence presented somewhere, and to somebody who is qualified to judge of such abstruse and difficult matters, as the qualifications of the practitioners of law and medicine, so that they who are not able to form any opinion, of themselves, shall not be imposed on by their inability to tell a man of real knowledge from a dishonest pretender. There is a great embarrassment now in the minds of the majority of the community on those points, where they have no particular means of information; and thousands of strangers, especially in cities, are just as likely to place their property, or their lives, in the hands of miserable charlatans, as of honorable men.

It is perfectly ridiculous to suppose that there is, when compared with the mass of the community, any number of men worth mentioning, who are anxious to embrace either of the professions. It is not their wish or advan-

tage, therefore, which is to be consulted, but that of the great balance of the people, which deserves the care and parental consideration of the Legislature. It is they, who are every moment in imminent danger of imposition, and know not how to avoid it, for whose protection barriers should be erected. Especially should none now existing be abolished, under the hollow pretence that such an act will be the destruction of the privileges of a few; when, in reality, those very requisitions, falsely called privileges, are the invaluable securities of the public against ignorant pretension, empiricism, and fraud.

Rochester Knockings.

We are not going to inflict on our readers a long article on the *Rochester knockings*. The subject of *sounds produced by voluntary muscles upon movable parts within the organism*, (the principle of the exposition,) is one which, in a physiological point of view, is deserving attention irrespective of the remarkable imposture to which it has been made subservient. It is a curious field of inquiry, in how many different methods sounds may be produced in conformity with the principle just stated. Since our last issue some farther facts bearing on this inquiry have fallen under observation. We propose to state these, as matters of scientific interest, and, at the same time, important to be known in order to accomplish the complete explosion of the *spirituality of knockings*: for it is probable that different "*mediums*," or *rappers*, employ different means of developing the sounds.

We have met with a person who raps with the tendon of the *peroneus longus* at the ankle joint. This tendon, the reader will recollect, passes behind the *malleolus externus*, and arches forward, terminating at the middle of the sole. In the case of the person just referred to, by a voluntary contraction of this muscle, the tendon is moved forward and backward over the malleolar protuberance, giving rise to a pretty loud sound in its movement in either direction. The *rapping* is not so loud as that done by the *knees* of the *Rochester rappers*, but it has the same character, and is much louder than would be supposed by one who had not heard it.

The young man who *raps* in this way has been able to do it from his earliest recollection. The ability evidently involves, in the first place, an anomalous defect in the ligament confining the tendon in the groove in which it passes, and, in the second place, a habit of producing forcible, quick contractions in the peroneus muscle isolated from the other muscles of the leg.

The motion of the tendon is plainly felt with the hand placed on the

ankle joint, and may also be seen if the foot be denuded. The contraction of the muscle is also perceptible if the leg be grasped by the hand.

We have heard of two other persons who are able to *rap* in this way, so that it is by no means improbable that some "*mediums*" will be found to make their mysterious noises in a similar mode.

As we have designated the sounds produced by joints *articular*, we propose to designate those occasioned in the manner just described *tendonical*! As this is a new province of science we must, of course, have a new nomenclature!

The sounds produced with the toes, by Mr. Burr, and others, in imitation of the *Rochester knockings*, do not emanate from the joints, but are caused by a motion of the toes on the sole of the boot, or on the solid floor. A pretty loud sound can be developed in that way, and almost any one, with practice, can, in so far as this accomplishment is available, become a "*medium*," or *rapper*. We have heard a person rap with his toes who has always been in the habit of doing it as a trick. The sounds are not so loud as the *Rochester knockings*, as this person has tested, having visited the Rappers, lately in this city, and occasionally throwing in his own *raps* in order to make the comparison. In distinction from the *articular* and *tendonical* sounds, the latter may be called *toeic*!

We have met with a person, a highly respectable lady of this city, who can rap with the knees, but not by a lateral motion of the two bones. The movement in this case is backward and forward. It requires a more sensible muscular effort than in the case referred to in our former article on this subject. The *knocking* is loud, but it is *single*, not a double sound. One of the "*mediums*," visited by the writer who has exposed the inconsistencies and absurdities of this imposition in a series of papers in the N. Y. Express, signed *Shadrach Barnes*, produced single raps. She may have made the sounds by the movement of the knee in the mode just mentioned. We are led to infer this from the fact of the sounds being *single*, although it is not impossible that double sounds may by some persons be produced by movements in the antero-posterior direction. This lady was accustomed to produce the sound in her youth daily, more or less, as a habit. It was not attended, or followed by any immediate irritation in the joint, but she was led to discontinue the practice lest the joint might become weakened. She can produce the sound only when standing.

A medical practitioner of this city is able to develop sounds in several modes, viz., 1st, by a semi-dislocation of the clavicle at the sternal extremity on moving the arms backward and forward. 2d, in the shoulder joint

by the same movements. 3d, in the neck, posteriorly, by movements of the head laterally. In the latter case the sounds are apparently occasioned by movements of the tendinous structures upon the spinous processes of the vertebræ. These sounds are not nearly so loud as the *Rochester knockings*, but have the same character. They require considerable visible motions, and are therefore not well adapted for purposes of spiritual communications.

We shall be glad to receive communications from any of our readers who are in the way of meeting with interesting illustrations of this newly developed branch of physiological inquiry.

Some of our readers may desire to be informed of the *finale* of the *Rochester Rappers* in this city. They continued to hold daily and nightly *soirees*, which were well patronized, until the 25th ult., when they rather suddenly took their departure for parts unknown. Shortly after the secret of the *knockings* was announced, there was a change in the performances. The *knockings* now became a secondary matter, and the "*spirits*" were pleased to ring bells, gongs, play on the banjo, kick tables, chairs, etc., to select and admiring audiences.

The *rappers* were, of course, well enough satisfied of the truth of the exposition, although their disciples were in no wise convinced, and, hence, the addition of fresh novelties to keep up the attraction. The *physical manifestations*, as currently related, with the exaggerations usual in such cases, seemed to be very clever juggleries superadded to the knockings. But their marvelous character is lessened in no small measure by a knowledge of the conditions under which they took place. The bells, gongs, and banjos, were placed under a table, the "*mediums*" and the company around the table, the arms of the latter resting upon the table, and their feet beneath the chairs, on which they were seated. Their position was such as to prevent any one from looking beneath the table, or making explorations with the feet. A violation of these rules led to expulsion from the room, as was the case with a young gentleman who, not satisfied that by carrying his feet under the table he should come into contact with *spirits in visible shapes*, made the experiment, and avows that he caught between his knees the feet of one of the rappers! We have not personally witnessed these fooleries, and therefore we will not assume to say dogmatically how they were done. But we fancy they are susceptible of as simple an explanation as the knockings. With the latter it is evident they have no necessary connection. If the "*spirits*" will ring the bells, etc.,

provided the *mediums* are separated from the space beneath the table where the bells, etc. are placed, by a wooden partition reaching from the floor high enough to prevent the possibility of their nether extremities taking any inelegant positions, we will confess that we have *guessed* wrongly respecting the instruments displayed in these *feats of leger-de-pieds*.

Renal Affections: their Diagnosis and Pathology. By CHARLES FRICK, M. D. Philadelphia: LEA & BLANCHARD. 1850. 12mo. Pp. 189.

The author of this *brochure* has devoted, for several years, particular attention to the branch of animal chemistry pertaining to the urinary secretion in health and disease; and, in the meantime, has contributed several valuable papers for the *American Journal of Medical Sciences*. The object of the present publication is to aid those who wish to pursue the study of urinary pathology, but who have been deterred from the pursuit by the "formidable array of chemical changes, and the innumerable varieties of crystalline formations." The aim, in other words, is to simplify the subject as much as practicable, and to confine himself to matters only that are of practical application. The arrangement of topics is original, and possesses advantages. In most works on this subject, each constituent of the urinary secretion is treated of separately, so that, as the author remarks, "the reader is required to go through the whole book before he is able to make an examination of the most simple specimen." The course pursued, with a view to remedy this defect, is to lay down, in the first chapter, directions to be pursued when only a superficial examination is necessary. In the next chapter, rules for a more exact analysis are given; and so on, progressively advancing to those operations requiring greater accuracy of observation. We think this plan will be found to render the work more interesting, as well as more convenient for the practitioner.

The illustrations accompanying the text, are, for the most part, copied by himself from the field of the microscope, not transferred from European publications.

The pathological relations of urinary disorders have been rendered, by late researches, eminently important, as well as interesting. At the same time, there is, perhaps, no department of practical medicine which is so apt to be overlooked and neglected by practitioners. It would be an improvement if some of the excessive attention to some comparatively minor points, could be transferred to this source of symptomatic phenomena. For example, it might be commended to those who are so elaborately careful to

examine, by means of nearly all the senses, the alvine evacuations, to bestow a share of their zeal to scientific inquiries respecting the functions of the renal secretion, inasmuch as, in a large class of diseases, the latter present much more significant indications.

We greet Dr. Frick's little treatise with pleasure, and hope it will have a place in every physician's library.

Obituary.

Died, on the 12th ult., Dr. JOHN G. CAMP, of this city, aged 54 years.

The late Dr. Camp removed to this city from Pittsford, Monroe co., about two years ago. He was for many years an active practitioner in the place of his former residence, where he had a large circle of friends, and enjoyed the confidence and respect of the community, both as a physician and citizen. The period of his residence in this city was too short for him to have acquired an extensive practice. His success, however, was sufficient to induce him to fix upon this as a permanent location; and, being in the prime of life, with excellent health up to the attack which proved fatal, he had every encouragement to anticipate a long and prosperous career. Death, however, so often unexpected, and which shows so little deference to the hopes and prospects of this life, determined otherwise. He died, after a short illness, of typhoid pneumonia.

The following resolutions were adopted by the medical staff of the Buffalo Hospital of the Sisters of Charity, of which the deceased was a member, being one of the consulting surgeons:—

“A meeting of the Medical Board of the ‘Buffalo Hospital of the Sisters of charity,’ was held pursuant to a special call, March 13, 1851.

“Dr. Alden Sprague stated the object of the meeting to be, to make some testimonial of respect for our lately deceased fellow citizen, Dr. J. E. CAMP.

“On motion of Dr. Sprague, seconded by Dr. George N. Burwell, the following resolutions were unanimously adopted:

“*Resolved*, That in the decease of Dr. J. E. Camp, late one of the counseling surgeons of this Board, we have suffered the loss of a much respected companion, and the Hospital has been deprived of one of its most valuable officers.

“*Resolved*, That, while we recognise the hand of God, we, at the same time, deeply lament his death, and sincerely sympathise with his afflicted family.

“FRANK H. HAMILTON, *President*.

“AUSTIN FLINT, *Sec'y.*”

An Address delivered before the Erie Co. Medical Society, Jan. 7, 1851.
By REV. WILLIAM FLINT, M. D.

This is a highly interesting address on the subject of *medical delusions*. The talented author, for several years a practitioner of medicine, relinquished the profession for that of divinity, and is now rector of an episcopal church at Erie, Pa. Not forgetful of his first love, we find him, on all proper occasions, engaged in promoting the interests of medical science.

We quote the last paragraph of the above address :—" As one who has retired, I trust, forever, from the profession, permit me to close with one word of advice. Respect yourselves and your profession, and the world will respect you, and it. Whatever be the temptation, use no illegitimate means to extend your practice or your fame."

This advice is short, but comprehensive. It covers not a small share of the evils which make the character of the medical profession what it is, rather than what it should be.

Graduates of the University of Buffalo.

The following are the names of the young gentlemen upon whom was conferred the degree of *Doctor in Medicine*, at the annual commencement, February 26, 1851 :

W. H. Fish.	John Mitchell, Jr.
James P. Squires.	Sanford W. Huff.
W. S. Hicks.	Daniel Todd.
A. N. Braman.	Sanford W. Chapel.
Alfred J. Bigelow.	I. F. Reischenbach.
A. Hartley Jones.	John B. Lundy.
William Porter.	Sanford Eastman.
George T. Campbell.	Thomas D. Strong.
John Foote.	John J. Covert.
Lawrence McKay.	J. J. Edmunds.
Frank B. Seelye.	Justin C. Elliot.
J. O. Allen.	Horace Babcock.
E. M. Bemus.	Harvey Parkhurst.
Horace Halbert.	Lewis Barnes.
Daniel E. Foote.	Albert J. Myer.

Introductory Addresses.—We have been favored with several introductory addresses during the past winter, which we have filed away for notice and review in a future issue, provided the delay does not extend to the next harvest season. Lest our leisure may not permit us to devote to them proper editorial attention, we avail ourselves of this opportunity to return our thanks to their respective authors for having remembered us.

Physician and Patient: or a practical view of the mutual duties, relations, and interests of the Medical Profession and the community. By WORTHINGTON HOOKER, M. D., New York. Baker & Scribner, 145 Nassau Street and 36 Park Row. 12 mo., pp. 453. 1849.

Lessons from the history of medical delusions. By WORTHINGTON HOOKER, M. D., New York. Baker & Scribner. 12 mo., pp. 105. 1850.

The first of these works has been two years before the profession and public. It is but lately, however, that we have had the pleasure of perusing it. And should any of our readers have been still more neglectful of personal enjoyment and improvement than ourselves in this matter, we trust that, even at this late day, we may do them a service by urging them to procure the book and read it forthwith. As respects its merits we cannot do better than to quote the commendatory remarks from the report of the chairman of the committee on medical literature made at the last meeting of the Association. He says, "no where else have the subjects of this treatise been so fully and satisfactorily discussed. The author pleads the cause of medicine with the most cogent and persuasive arguments, and in a style at once simple, correct, and attractive. He evidently loves his profession, and reflects in his language the lineaments of a cultivated mind, and an upright character. Physicians in becoming acquainted with this interesting work, will find that it contains every needful protection against the seductions of false doctrine, and abundant encouragement to bear with serenity and fortitude the inevitable ills which beset the medical practitioner's path."

The work on medical delusions, by the same author, is a publication of the dissertation which received the Fiske fund prize awarded by the Rhode Island Medical Society. It gives an interesting and instructive account of some of the more prominent of the medical delusions that have prevailed in past time. Such a recital contains lessons which would benefit a portion of every community at the present time, if they were to be candidly and thoughtfully studied. This, however, is hardly to be expected save to a limited extent. Nothing but the bitter fruits of experience will suffice to open the eyes of a large proportion of those who have yielded their minds to a popular medical delusion. We should be glad to know that in this judgment we are uncharitable, and that Dr. Hooker's excellent treatise will do more good than we anticipate. It is not the fault of the work if it do not accomplish much in the way of promoting sound views with respect to illegitimate systems of medicine. We hope that its capacity for good results will be tested by a wide circulation.

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ORIGINAL COMMUNICATIONS.

ON THE USE OF WATER IN SURGERY. *By Alphonse Auguste Amussat, of Paris ; published at Paris, Jan, 1851, and translated from the French, by Frank H. Hamilton, Prof. of Surgery in the University of Buffalo.*

INTRODUCTION.

In choosing as the subject of my inaugural dissertation the "use of water in Surgery," I have intended, after having made historical researches, to compile most of the observations which have been published, as well as those unpublished, which pertain to the practice of my father, with the view of demonstrating the incontestible success which has attended the use of water in the treatment of surgical affections.

Next I have endeavored to determine in what surgical diseases we may, and ought to, have recourse to it.

It was also my original purpose to divide my work into four distinct parts.

1. History of the employment of water in surgery.
2. General considerations and various modes of application.
3. Classes of maladies in which it ought to be applied.
4. Special observations and modes, with reference to regions.

But the multitude of facts which I found in my possession, and the space occupied in the consideration of the diseases which demanded its use, compelled me, reluctantly, to limit the plan I had laid out for myself, by consi-

dering at present but the two first, viz: the "History," with "General considerations and various modes of application."

One point of paramount importance to which myself and my father have directed especial attention, is the proper *temperature* of water. After a careful analysis of the inconveniences which have been attributed to this agent, we have ascertained that they were due mainly to the cold. We are convinced that water at 18° to 25° centigrade (64° to 77° F.) possesses in general nearly all the advantages of cold water without any of its inconveniences. It appears to me, therefore, that the use of water at this temperature, ought to be the rule and the use of cold water the exception.

I regret that I cannot give to my subject at this moment, all the latitude which its importance demands; yet I hope to be able to demonstrate that water, skillfully applied, is a most potent topical application — that it is preferable to all others, and that we may make it the basis of a rational *therapia* in a majority of surgical affections.

However, I propose at some future time to resume this work, and to render it as complete as possible, especially in its practical portions. I will then publish the numerous cases which I cannot now make known, and I will examine with care what classes of surgical maladies can be advantageously treated by water.

CHAP. I.

HISTORY OF THE EMPLOYMENT OF WATER IN SURGERY.

I was curious to know how an agent so simple, and at the same time so powerful, as water, has been appreciated by the surgeons of antiquity, of the middle ages and of modern times. It is this, therefore, which I have undertaken to ascertain in the present historical review, and with a full understanding that I am yet far from having exhausted a subject so extensive. I think, nevertheless, that I have been able to prove that this therapeutic agent, whose value has not always been appreciated, has been gradually occupying a place of more and more importance in surgery.

Hippocrates, whose writings furnish an excellent summary of practice up to his time, has left us some precious documents upon the subject before us. In cases of ecchymosis, contusions, stretching and rupture of muscular fibres, in luxations, sprains, diastasis, fractures extending into articulations, etc., etc., he prescribes, after the application of suitable bandages, copious affusions of water.

In luxations of the astragalus, calcaneum, and in all articular lesions he recommends warm affusions.

In comminuted fracures with protruding splinters, and laceration of the integuments by the bone, he gives the following advice: "If in the summer, the compresses should be frequently moistened with wine; if in the winter, undressed sheep's wool, saturated with wine and oil, should be applied. Under the whole there should be placed a sheep's skin, to enable the liquids to drain off, and to keep the parts open to view, remembering that parts which remain a long time in the same position, become touched with excoriations difficult to cure."

We see, then, that physicians of antiquity employed, in the treatment of wounds, not only water, but also wine, oil, and decoctions of aromatic plants.

In the book of Aphorisms, we find rules on the temperature of therapeutical agents, the judiciousness of which every day's experience sanctions, and which, perhaps, have been too much neglected.

Celsus, who extolls more than any one else the use of water in internal, as well as external, affections, expresses himself thus upon the subject of the dressing of wounds: "a sponge immersed in cold water, alone answers in slight cases; but whatever may be the liquid with which it is charged, it allays pain so long as it is moist; *therefore we must not permit it to become dry.* In this way we may heal wounds without having recourse to *foreign, scarce and compound* medicaments."

Farther on we find the following passage: "if adhesion has commenced, and if there is but slight tumefaction, we must adhere to the first kind of dressing; but if inflammation is active, and there is no hope of agglutination, we ought to employ suppuratives. The use of warm water is equally necessary to resolve engorgements, to diminish hardness, and to render suppuration more active. *The warmth of the water must be such, that the hand, when plunged into the liquid, shall experience an agreeable sensation;* and it is well to continue this application until the wound appears less swollen, and has a more natural temperature."

Celsus further recommends the use of water in hemorrhages, fractures, diseases of the eye, &c.

Galen, whose writings had so much influence upon physicians and surgeons, in all the middle ages, employed water in a great number of affections. In his "Treatise on the nature and properties of simple remedies" he investigates the action of snow and of cold water upon our tissues.

In wounds he employed successively warm water, wine, vinegar, and he recommended especially affusions of warm oils for those cases in which

nerves and tendons were implicated. He cites many cases of cure by this last method; and having had his own clavicle luxated upon the scapula, he treated himself in this way.

In Aetius we find for the first time the word *irrigatio*; but at paragraph 172, we read the following passage: *Irrigationibus utimur uti ob obliquam circumstantiam adhibere prohibemur*; which proves that he made little use of irrigations, and that he especially confined them to medical diseases.

Rhazes advises warm water in fractures, and cold water, or rose water cooled by ice in burns.

Avicenna reproduces in his works the great principles laid down by Hippocrates and Galen, upon the use of water and oil in the treatment of certain surgical affections.

According to M. Malgaigne, Marianus Sanctus had projected a commentary upon the entire surgical works of Avicenna. He intended to teach a new mode of curing wounds, even those of the gravest character, by means of pure water alone; only adding thereto certain words; for all the art of medicine consists in words, herbs and stones! "This work," continues M. Malgaigne, "has not been published; there remains nothing of it but the preceding quotation, which I have recalled for a double purpose. It enables us to see how Italian surgery had declined after Jean de Vigo, since his best pupil had come to have faith in such superstitions; and it discloses to us the true origin, in modern times, of a mode of treatment generally ascribed to Michel-Ange Blondus."

In 1542, Biondo or Blondus published a work entitled *de Partibus ictu sectis citissime sanandis et medicamento aquae myres invento*; in which he recommends water as a new and efficacious remedy in the treatment of most wounds. But we have just seen that Marinus Sanctus had preceded him in this discovery.

Ambrose Pare is the first French surgeon who uses the word *irrigation*, which he defines as follows: *embrocation, according to the Greeks, or irrigation, according to the Latins, is a sprinkling, similar to rain, when from a height, we allow a decoction to fall, drop by drop, upon some part.*

Water at this epoch was especially employed by empirics, who, after the example of Marianus, Sanctus, and Blondus, only used it with the aid of sorcery and conjuring words. This induces our own great surgeon to write; "I will not omit to say that some cure wounds with pure water, after having pronounced over them certain words, and having applied linen cloths cut in the form of crosses and saturated with water, often renewed.

firm that it is neither the words nor the crosses, but it is the water which

cleanses the wound, and by its coldness repels the inflammation and the fluxion which might attack the injured part, in consequence of the pain. This healing can be accomplished when the wound is in a fleshy part, and in a body young and of good habits, and where the wound is simple."

This passage shows that A. Pare appreciated water at its just value, and sought to disembarass its use from the superstitious practices of charlatinism.

1560. Gabriel Fallopius, imbued with the doctrines of the early physicians, recommends pure water and tepid in the dressings of ulcers; but in luxations he insists that it should be mixed with oil. This distinguished surgeon closes his chapter on wounds with the following passage: *Imo ego attestor vulnera vidisse sanata ex aqua etiam non benedicta.*

1570. Philippus Palatius, whose work *de vero methodo quibus cumque vulneribus medendi, cum eo medicamento quod aqua simplici et frustulis de cannabe vel lino constat*, is very rare, (and which I have not been able to procure,) has been variously estimated by those who have read it. Percy and M. Scoutetten eulogise it; M. Malgaigne differs in opinion with the surgeons just mentioned, and gives an analysis of the mode employed by Palatius.

We extract the following passage: "Palatius," says he, "teaches to cover the wound with three *frustula*, cut into a nearly circular form, preferring well dressed male flax or hemp rather than female. I thought at first," continues M. Malgaigne, "that these must be pledgets of tow;" but he speaks clearly in another place of tow, of which a separate pledget is to be made, covered with the white of an egg and applied immediately to the wound; and the *frustula* of hemp was placed over this. *The whole must be moistened night and day with simple water, at such a temperature as that its refreshing effects may be obtained without discomfort to the patient.*" This exposition is remarkable for its paucity of ideas and prolixity of phrases.

1577. In a learned discussion which took place between Dangaron, E. Joubert and Martel, the latter sustained this proposition: *gunshot wounds may be cured with simple cold water*, and with such soundness of argument that he finally convinced M. Joubert. This surgeon closes, however, by saying: "cold water possesses all the qualities requisite to accomplish a cure, and will answer the indications in every instance, provided the system be otherwise strong, the heat vigorous and the body in good condition.

Martel, in his *Apologie pour les chirurgiens*, resuming the same subject, declares in his fifth paradox: "most wounds may be healed by a simple remedy, which is common water or oil."

In the explanation which he gives, he expresses himself thus: "I repeat then, that I have treated many wounds with water alone, being in the army deprived of other means, and I have seen very happy results from it..... But I think one of the principal means to expedite the cure of wounds, is to keep them very clean; now water thus cleanses and deterges. Water, by its coldness, tempers the heat of the humors, and drives them elsewhere, and serves as a repellent."

More than a century now passes without our finding any thing worthy of note upon the use of simple water. Notwithstanding the efforts of Fallopius and of Martel, ointments, powders, plasters, miraculous waters of all kinds, are preferred to a medicinal agent which doubtless in the view of men of that age had the great fault of being too simple.

At the beginning of the 18th century, some writers endeavored again to attract attention to the use of water in surgery.

Smith, in England, in his "Treatise on the medicinal virtues of common water," devotes several pages to the treatment of contusions, ulcers, sprains, hæmorrhages and wounds, with simple water.

1713. Sancassini published at Venice a small work in which he makes mention of pure water in the treatment of wounds. In the 36th and 37th "*centuries*," he expresses himself thus:

"Pure spring water is employed by many surgeons in the treatment of even very grave wounds. There is no superstition in believing in the cures which it effects; for it prevents any thing from interfering with nature, which is the true physician, especially in wounds.

"Dogs," says he, "cure their wounds provided they can lick them. Their saliva is a balm which nothing equals; it prevents the air from harming the wounds."

And farther on, "the remedies generally recommended for wounds, are more hurtful than useful, they act as foreign substances."

In 1732, Lamorier read before the public assembly of the "Royal Society of Sciences of Montpelier," a memoir upon the use of common water for wounds; we regret that we have not been able to find this work, and that we are obliged to limit ourselves to the following extract made by Paul:

"It is surprising that common water is not more in use for wounds; perhaps the remedy is too common. The public value lightly what nature gives us in profusion; but places a high estimate upon that which is scarce, or comes from afar off, or is perchance dearly purchased, or is involved in mystery. Many also think that a remedy so simple as water can have no efficacy. To remove these prejudices Lamorier has made many experiments: among

others three several experiments were made in the month of January last, upon three men, of whom one had an old ulcer upon the outer side of the ankle, of the size of the palm of the hand. The second, a soldier of the regiment of Medoc, had received a blow from a sabre upon the back of the hand, which had cut the extensor tendons of the thumb and fingers, and had separated the two bones of the metacarpus which sustained the little and ring finger. This wound was followed with fluxions and abscesses, which involved almost all of the forearm. The fever and the drying of all his body, caused serious apprehensions that he would die. The third, another soldier of the same regiment, had received a sword cut across the forearm, which had opened the artery which lies between the bones. Much blood was effused among the muscles and extensive suppuration occurred. A copper boot was constructed, in which was placed common warm water for the purpose of immersing therein the ulcerated leg; in this bath the patient rested the limb an hour each day. A few days after the hard borders melted away, the cicatrix advanced insensibly, day by day, and he was completely cured.

“Two machines of sheet iron were also constructed, in which the two soldiers could comfortably immerse the arm from the hand to above the elbow. By bathing their wounds in water, suppuration became much more healthy; they were able to move the fingers with greater ease, the pain and the fever diminished daily; in a word, they were entirely cured.”

Thus we see to Lamorier is due the credit of having first thought of prolonged local baths in the treatment of surgical affections. This new mode, it appears also, attracted public attention, for some years later, Chirac reports that the Duke of Orleans having been severely wounded in the left hand, during the siege of Turin, he was cured by prolonged local baths in the waters of Balaruc. I might have reported this case complete and have examined what may be the influence of mineral waters also in the treatment of wounds: but such researches would have led me too far, and beyond the limits which I have traced for myself.

Gradually this method of treating wounds with prolonged baths of simple water was forgotten, and, as we shall see, more than a century afterwards, this ingenious idea was brought forward anew and extolled by a Swiss surgeon.

Water was also employed in surgery in Germany nearly at the same time that we see Lamorier advocating its use in France.

The period was moreover very favorable; the great Frederic was then astonishing Europe by his victories, and furnished to the surgeons of his

army frequent occasions for the employment of a mode of treatment so efficacious and so simple. It is indeed upon battle fields, where every thing is wanting, that we are obliged to depart from ordinary rules, and give the preference to a dressing the most simple, sometimes the only possible dressing, for water can always be obtained.

Theden, a very distinguished surgeon in the armies of the king of Prussia, although very partial to his *eau d'arquebusade*, has left us the very interesting case of a subordinate officer of the regiment of Budembrok, whom he cured of a violent inflammation of a lower limb, by enveloping it in cloths wet with cold water, and kept constantly moist.

He relates also, that having been pricked in the end of his finger by his bistoury in opening a fistulous depot in the anus, the pain, slight at first, became soon intolerable. The disease propagated itself along the forearm, attacked the elbow joint, which became very painful; the limb swelled considerably, and fever was lighted up. Finally, in a short time the progress of the malady was such that he determined to have his arm amputated. But being reminded of the good effects of cold water, he wished, before submitting to this operation, to make a trial of it: and the success was so remarkable, that, contrary to his expectation, he was promptly and completely cured.

Schmucker, Dauter, Boenerken, Ritcher and Platner, also frequently resorted to water in the treatment of surgical affections, but often combined with it saline substances.

The learned surgeons whose works have illustrated the Academy of Surgery, have contributed not a little by the reforms which they have sought to introduce into dressings, and especially in purging surgical therapeutics of the mass of balsams and salves whose value was more than problematical, to prepare the way into which others have since entered, and which tends to widen itself day by day. Although no special memoir has been devoted to the subject of the employment of water in surgery, the numerous passages which follow, quotations from the "Memoirs" and "Prize Essays of the Academy of Surgery," enable us to appreciate how the ancient edifice of Arabian polypharmacy was sapped at its foundations.

Recollin, in a work upon "the utility of injections of warm water into the womb, when portions of the afterbirth remain, after an abortion," cites three conclusive cases, and argues his title to the priority in the use of the means which he recommends; he continues in these words; "My observations do not leave a doubt — I have myself made the injections into the actual cavity of the womb. I have observed also the good effects they are

capable of producing where putrefaction and suppuration are present : which is sufficiently shown by my first case. I use nothing but pure warm water, to which I shall always give the preference over decoctions or preparations of any kind, because simple water must naturally be more dissolving than what is charged with foreign particles, which give it always more consistency or viscosity. It is preferable, therefore, when it is only designed to expel purulent and fetid humors, or to dissolve blood-clots, and especially to remove by the impulsion of the fluid portions of the afterbirth retained in the womb."

The use of water in the cases cited by Recollin, has been improved upon latterly by M. Wuillamoz-Blanc.

Pibrac, in reporting a case of crushed finger, says: "My dressings were plain. The upper part was enveloped with compresses dipped in simple cold water of marshmallows. The cure has been very rapid.

"Greasy remedies relax improperly, resins and balsams united with grease, to form digestive salves, possess stimulating properties by which the fleshy parts are necessarily irritated; it is not surprising then, that the suppression of these remedies should have such favorable effects.

"I have used cold dressings, because I am persuaded that warm fomentations, by rendering the humors thin, contribute not a little to the primitive swellings which supervene to wounds."

De Lamartiniere, in a "Memoir upon the treatment of wounds from fire-arms," expresses himself thus: "In regard to topical applications, there is in my opinion, nothing preferable, especially during the first days, to *sea water*; it resolves coagulated blood, an accident of severe contusions which sometimes ends in gangrene. This remedy so simple, and which is every where found, is a grand resource in the most extensive contusions."

Tollin reports the history of a lady, about sixty years old, who received upon her left foot, a stone which had fallen from the third story. Several toes were broken and separated from the foot.

"I proceeded," says he, "to separate the great toe entirely, I dressed the wound at first simply with coarse charpie, I then enveloped the foot and the leg with several compresses which I adjusted with a few turns of a roller. I ordered these compresses to be wet with an *emollient decoction*, and to renew the fomentations every two hours without disturbing the dressings.

"When I did open it, I found the wound in a good condition; I then dressed it with a digestive salve, and directed two poppy heads to be put into two pints of emollient decoction, with which the compresses were to be moistened every hour. . . . This treatment had all the success which I

had hoped from it; the patient was completely well on the forty-eighth day."

Among the Memoirs which have been presented to the Academy of Surgery, and which have obtained prizes, we find several in which mention is made of the advantages to be derived from the use of water in surgery.

"There are," says Mannoni, "some ulcerations accompanied by severe contusions which dry up under the use of cold water; this adjuvant employed skillfully, arouses the diseased part to action, as we shall see by the following history:

"In the winter of 1746, the son of an advocate fell in descending a ladder; he lifted himself up with his face sadly bruised, having a large contusion upon his forehead, eyelids and nose, beside a long wound on the inner surface of his upper lip, but which was, in fact, not very deep. Having been called to see him, I used no other remedies *but fine sponges saturated with fresh water*. The servants were diligent in renewing the applications as well upon the bruises as upon the wound, and five days after the fall, the patient was perfectly well."

"Water," says Guyot, "*is the first, the most powerful and the most universal emollient; I speak of fresh water in its most simple form.*"

"But that water may prove emollient, it must be neither cold, nor too warm; we know that cold water, far from relaxing the fibres, produces a constriction and considerable retraction; this is proved by the disappearance of veins, before apparent, from the surface of the body, under the influence of cold, and by the experience of many practitioners who have reduced hernia's by pouring or applying cold water or snow upon the tumors, and who have arrested considerable hemorrhages with the same remedy.

"It is necessary, then, that water should have a moderate warmth, nearly the temperature of the sound body, in order to produce an emollient effect.

"Experience demonstrates its usefulness and its good effects when applied externally in the form of vapor baths, fomentations and *douche*.

"Most topical emollients derive their chief value from the water, such as fomentations and emollient cataplasms."

"Lukewarm water," according to the illustrious Louis, "is of all medicines the most simple. Yet we derive from it benefits without number; *lukewarm water* relaxes parts which are over stretched, opens the pores; the particles of water insinuate themselves into the vessels, dilute the fluids, and increase the diameter of the small invisible vessels, they facilitate the flow of humors, and open passages to substances which need to be expelled. It is for all these reasons that Pare recommends fomentations of lukewarm

water in several places, and especially in the thirtieth chapter of the fifteenth book upon "fractures."

"Common water, which contributes," says Poutier, "to the preparation of most remedies, and which supplies them with a convenient vehicle is itself, *when lukewarm, the emollient par excellence.*"

"Water, as a sedative and antiphlogistic," says Mopilier the younger, "diminishes the irritation of the vessels, relaxing them by its liquidity."

Champeaux washes wounds with tepid water and with wine. Sometimes he bathes the wound every three or four hours with vegeto-mineral water.

In ulcers he practices bathing with an emollient and resolvent decoction every four hours.

Finally Chambon says:

"Simple water, harmless, homogenous, analagous to our own juices, is capable, according to Palatins, of healing the largest wounds. What renders this liquid so worthy of recommendation, is not so much its actual qualities as its exemption from the evils which attach to other fluids."

We see by the preceding passages, that it is in the "Memoirs" and "Prize Essays of the Academy of Surgery," we find the germ of the simplification of dressings; and if we must in justice say that surgeons of that period have rather sought to destroy old prejudices and traditions, than to trace new rules for our guidance, we must nevertheless acknowledge that some among them had discovered the advantages of water as an antiphlogistic and *topique* in the treatment of wounds.

Evidently we may say that the Academy of Surgery has given the impulse to the reform which has since taken place in the mode of dressing, and that necessity also has compelled a resort to water, when our military surgeons found themselves deprived of every other means.

Indeed, to Lombard, one of our most celebrated military surgeons, was reserved the honor of establishing a mode of dressing so simple and so advantageous.

In 1786, Lombard published in his *Opuscules de Chirurgie*, a very remarkable article, in which he established the value of a therapeutic agent until then too much neglected, and opened a new road which soon had to be followed by the surgeons of the French armies during the great wars of the revolution, and of the empire. This work, entitled "Summary of the properties of simple water, employed as a topical application in the cure of surgical maladies," is divided into two parts: in the first, he treats of the properties and advantages of cold water; while he devotes the second to the consideration of tepid water and to an examination of the affections and

temperaments which demand the use of tepid rather than cold water, and the reverse.

I will give in my second division, many extracts from this highly practical work, which will enable us to appreciate its value.

Percy being at Strasbourg in 1785, as surgeon-major of the regiment of Berry, had an opportunity of seeing the results obtained by Lombard from simple water dressings upon some soldiers wounded in a trial of ordnance. A quick observer, he comprehended immediately all the advantages of this mode of treatment upon the field of battle. We may judge of his opinions by the following passages, copied from his article entitled "Water," in the "Dictionary of Medical Sciences;" an article which contains in germ nearly all the applications of water which have been made since in the treatment of surgical affections, even irrigations.

"Among the kind of miracles," says he, "which I have seen wrought by water in wounds from fire-arms, I will instance the cure of nearly sixty young volunteers of a battalion called "Louvre," which having left Paris on the first day of December, 1792, was ordered on Christmas day to the assault upon Montagne-Verte, near Treves. The enemy placed upon a height, made upon the battallion a well sustained fire, and most of these young men were wounded in their feet. Many were taken to the military hospital of Sarrelouis, of whom only a few could be saved without amputation. The others remained in the convent of Consarrebruck, with two German surgeons, who were charged with their care. By my advice, and *perhaps in default of other remedies*, the attendants bathed their feet incessantly, and showered them with water moderately cool, covering their wounds with compresses constantly moistened with the same. No other dressings were used, and I attest that only four died, of whom two died of adynamic fever, which disturbed and interrupted the treatment of the wounds with water; one of colliquative diarrhea, and the fourth of trismus. All the others recovered rapidly; several had not even ankylosis, although their feet had been traversed in every direction, complicated with tearing of tendons, aponeuroses and ligaments, and with splintering of the bones, sometimes of the tarsus, sometimes of the metatarsus."

Larrey relates in the following manner the advantages which he derived from the use of water in the campaign in Egypt.

"One would be astonished without doubt, to learn that with a few sea biscuit, a little good water which was carried with each wounded man, and *by the use of brackish water only for their dressings*, a very great number of these individuals, suffering under severe wounds of the head, of the

breast, of the abdomen, or deprived of some of their limbs, crossed the deserts, which separate Syria from Egypt, a distance of about sixty leagues, without any accident, and with so much benefit, that most of them found themselves cured when they reached this latter country."

Briot, a very distinguished military surgeon, is much pleased with the employment of water, which he regards as *le vulnérable par excellence*, in the dressing of wounds received upon the field of battle.

"The general method of dressing which we employed," says he, "consisted in doing nothing without a motive. We used with success, and almost always to the accomplishment of perfect cures, *cold water* in wounds made by small arms; also in cases of stupor, in wounds of tendons, of aponeuroses, of capsules and of membranes; and *tepid water* in those made by fire arms and which were suppurating."

The most notable cases of success resulting from the use of water in gunshot wounds are unquestionably those reported by Dr. Treille, after the battle of Baylen.

"I obtained, seven years ago, the most happy effects from the indiscriminate application of pure water upon every variety of gunshot wounds.

"A very extraordinary circumstance compelled me to employ this means alone. I confess that at first I was not without some solicitude as to the results, but I was quickly reassured by my success. The facts were thus: After the battle of Baylen (Andalusia,) I remained upon the field the only surgeon to take care of five hundred wounded. Deprived of all medicines, I had all the wounds washed with pure water. I continued my dressings in this way during twenty-one days that we remained upon the field of battle, receiving nothing from without but some linen and provisions. As it would have been impossible for me alone to dress five hundred wounded, I arranged them in three sections and dressed one section each day, and they dressed themselves the two other days.

"Only seven or eight wounds became gangrenous, and I had but two cases of tetanus.

"When attention is given to the circumstances in which I was placed, it will be apparent what we ought to think of simple water in the treatment of recent wounds. Here were five hundred wounded lying upon the ground from the nineteenth of June to the tenth of July, (1808,) under the broiling sun of Andalusia, having nothing whatever for shade but the thin branches of olive trees, deprived of the consolatory hope of ever again seeing their own country, and given up to the mercy of the inhabitants of Sierra-Morena, who were all in arms and highly exasperated.

"In a word, the moral as well as the physical was but little favorable to the treatment of wounds; I have shown you, nevertheless, what was my success."

I have had the more pleasure in citing this remarkable passage from the thesis of doctor Treille, because it has been generally overlooked by those who have given attention to the use of water.

In the Dictionary of Samuel Cooper, we find the following passage, which gives the opinion of Guthrie upon the employment of water in gunshot wounds:

"By cold water," says Guthrie, "the inflammation is in certain cases entirely prevented, in many greatly controlled, and in almost all much subdued, whilst the suppurative process is not sufficiently impeded to prevent the subsequent action of granulation. In all simple cases of gunshot wounds, that is to say wounds which implicate only the soft parts, in persons of a healthy constitution, a little lint dipped in oil, or on which some ointment has been spread, is the best application in the first instance to prevent irritation: supporting the whole with two strips of adhesive plaster laid across. A compress or folds of linen wetted with water are then to be applied over it, and to be kept constantly wet and cold, even by the use of ice, if it can be obtained, and it be found comfortable to the patient."

And a little farther on,

"Cold water is not an infallible remedy; its use is not even always advantageous: there are many persons with whom cold applications do not agree; there are more with whom they disagree after a certain period, and in these two cases they should not be persisted in. Cold does no good at any stage of inflammation when the sensation produced by the first application is disagreeable to the patient, and when it does not sooth; for if it produces a sensation of shivering, or any other uncomfortable feeling; if it causes a stiffness in the part, it is doing harm, and if we change the treatment and cause a genial sensation of warmth in the limb, it will not only prove more comfortable, but be found actually advantageous. This happens in general at the period of the commencement of suppuration; and in such cases cold prevents the complete effect of the suppurative action, which heat favors. Fomentations are then excellent."

Profr. V. Kern, of Vienna, published in 1809 a work in which he recommends tepid water in the treatment of wounds.

Among the authors who have written upon this subject we will mention, in France, M. M. Rouboud, Mauricheau-Beaupre, Laurent, Tanchou, Cloquet, Serre (of Uzès), Marjolin, Blandin, etc.; in England, Samuel Coo-

per; in Germany, Dzondi, professor at Halle, Rust, etc.; Mayor, of Lausanne.

We should also mention in a particular manner, Sanson, who writes thus in his article upon "Water" in the "Dictionary of Practical Medicine and Surgery," published in 1831:

"With water," says he, "I have seen cured by first intention, contused wounds, accompanied with more or less laceration and stretching of the parts; I have been able to save most persons upon whom I have practised amputation, or other grave operations, from the fever called traumatic; indeed I have been able to cure, without amputation, and even without active inflammation or copious suppuration, many persons having fractured limbs complicated with wounds and projection of the fragments."

The eulogium which Sanson bestows upon water is based upon extensive experience, and it acquires more value when it is known with what cautious reserve this expert surgeon adopts all surgical innovations.

From this historical glance we see that the use of water in surgery has made great advances since the publication of the work of Lombard. During the wars of the Revolution and of the Empire, necessity often compelled surgeons to resort to it, and it is surprising that, seeing the success which attended their practice, it has not come to be generally adopted. Fomentations and affusions, the only methods then employed, although very simple in their application, really efficacious and possessed of all the advantages of cataplasms without their inconveniences, still left much to be desired.

To regulate the application of water upon the surface of the tissues, so that its action may be always precisely the same, by substituting for the hand of man a very simple apparatus, and which requires the least care to ensure a uniformity in the flow and temperature of the liquid, is an improvement for which we are indebted to M. Josse, sen.

The method of continued irrigations which he employed before 1830, at the Hotel Dieu, of Amiens, is one of the most remarkable and useful surgical innovations. In 1834, M. Breschet, at the suggestion of M. Josse, the younger, tried this method at the Hotel Dieu, in Paris, and obtained such success as to attract the attention of surgeons. It is indeed very probable that had the experiment not been made in so large a hospital, and under the auspices of so distinguished a surgeon, its use would have been yet for a long time confined to the patients in the Hotel Dieu, at Amiens.

At this period Doctor Rognetta made known in an able article, the two first successful experiments of Breschet, and also the apparatus employed.

In 1835, M. Josse, the younger, published in his "Miscellanies of Surgical Practice," the method employed by his father for seven years. The practical illustrations contained in this work and the physiological suggestions which the author has added, have enabled practitioners to estimate at their true value the advantages which may be obtained from this new mode of treatment.

The same year Augustus Berard presented, in the "General Archives of Medicine," a "Memoir upon the employment of cold water as an antiphlogistic in the treatment of surgical maladies." This work contains an explanation of the process employed by this distinguished surgeon for irrigations, and some very interesting observations upon his success in the use of this means, to which he had more or less resorted since 1833.

Since then many practitioners have adopted its use; among many others, the theses of M. M. Boudrie, Omouton, Roger, Martineau, Ichon, Roberty, Delamotte, Laden, Gravis, Gueury, presented to the Faculty of Medicine, of Paris, the works of M. M. Christophe, Godin, Nivet, Phillips, of Liege, inserted in the periodical reviews, the able chapter which M. Denouvilliers has devoted to this subject in the "Compendium of Practical Surgery," have enriched science with new facts, and important practical suggestions.

M. Baudens, ever partial to the traditions of military surgery, and to the principles of Lombard, of Percy, of Larrey, of Treille, and of Guthrie, thus speaks in his "Treatise upon gunshot wounds," in relation to his own practice since the commencement of the African campaign:

"The dressings of wounds should be as mild as possible; such as perforated linen smeared with cerate, lint, compresses and rollers. These dressings should be wet with cold water constantly for several days."

Since then M. Baudens has always professed the same doctrines that he taught in his book; he has even gone farther, for he employs now ice and refrigerant mixtures with success. But we must not forget that beside great experience in the use of this therapeutic means, the surgeon of Valde-Grace had generally *only strong and robust young men to treat*, and we shall understand how he could be so successful with an agent at once so powerful, and, as we think, so dangerous.

In his "Operative Surgery," (2d ed., 1839,) M. Velpeau, after having enumerated the objections alleged against irrigations, closes as follows: "I think I can predict, after what I have seen, that irrigations will not continue to be used except as an occasional modification of the dressings and in a small number of cases."

It is to be regretted that M. Velpeau has shown himself so little favor-

able to irrigations. Indeed, his clinical teachings, sustained by his high position, have without doubt contributed more than any thing else to arrest the progress of this new therapeutic means.

In 1839, our worthy *confrere*, M. Lacorbiere, issued his learned "Treatise upon cold," employed both externally and internally "as a hygienic, medical and surgical agent." In this work, the most complete which our science possesses upon this subject, the author sustains with great justness of argument, supported by numerous facts, all the advantages which had been claimed for cold water in surgical affections. In a letter addressed to M. Lacorbiere, and published in his work, M. Alquié declares in these words his opinion upon the use of water:—

"I have under a multitude of circumstances, says he, derived advantages, *almost marvelous*, from the action of cold water, and from ice in cases of severe traumatic lesions. In 1823, when I directed the medico-chirurgical service of the hospital of Perthus, I attributed to this means the cure of several gun-shot wounds situated in the feet and hands. In the case of a drummer of the eighth regiment of voltigeurs, whose right foot had been traversed by a ball, breaking the first cuneiforme and producing great disturbance of the parts, I could only avoid serious accidents by the diligent application of cold water to the wounded member.

"Especially in large contused wounds has this means been useful. When I was surgeon major of the sixth regiment of dragoons, a captain of this regiment, M. David, received at Pontivy a kick from a horse upon the middle of his right leg. A large wound, four inches in length, resulted from the tearing of the inner half of the gemellus and the integuments. The periosteum along a portion of the tibia had been scratched by the iron, which had made an indentation in the inner side of the bone; it was a horrible wound! Ice water applied continuously during sixty hours, prevented completely all immoderate inflammatory action, and this extensive solution of continuity united almost without suppuration. It appeared that we had the power to regulate the inflammation precisely to the condition necessary for re-union."

I could cite similar cases which occurred among the dragoons of the sixth regiment, and several others at the hospital of Toulon and of Alger.

In 1841, Mathias Mayor, in his "Surgery simplified," writes thus:—
"Water has been invoked and proclaimed in every age and by the greatest surgeons, as the most important agent in the treatment of most surgical affections, and as the means, *par excellence*, to favor the efforts of nature. It is to be regretted that this opinion so prevalent among surgeons and phy-

icians has had so little influence with the people, and especially with military men.

"Water applied under the form of baths, and baths much prolonged, so that a part seriously injured is kept constantly cool, produces most generally effects which we shall in vain seek to obtain from any other therapeutic agent."

In this book M. Mayor discloses the new method of local and prolonged baths suggested by his son, and the *appareil* which he invented for their application. The idea of prolonged baths in the treatment of surgical affections can be traced back to Lamorier; nevertheless we ought to give credit to M. Charles Mayor for having introduced this mode, and for having conceived ingenious apparatus for its application.

In 1842, M. Malgaigne presented to the concours of the surgical clinique of the Faculty of Medicine of Paris, a thesis upon "irrigation in surgical affections." This truly excellent work winds up with the following conclusions:

"First, cold irrigations are an excellent antiphlogistic when employed in superficial wounds or inflammations; but even then they are not infallible, whilst in deeper wounds and inflammations they only mask the symptoms, and ought to be rejected.

"Second, continued irrigations are only suitable for the hands and feet, and perhaps also the forearm, but my facts are not sufficient upon this last point to decide; and even in these regions recourse should not be had to them except in the most severe cases. I give them an almost absolute preference in gun-shot wounds, but for other wounds I prefer intermittent irrigations.

"Third, in all cases the parts undergoing irrigation should be carefully covered with compresses, so as to exclude the air.

"Fourth, simple water appears preferable for continued irrigations, but for intermittent irrigations I choose *eau blanche* (plumbi diacetat, &c.) in cases of wounds, and a solution of sulphate of copper or wine in cases where no wound exists.

"Fifth, the temperature must *vary* according to the sensations experienced by the patient when the water is first applied: and in all cases we must abridge the duration of the irrigations as much as possible."

I believe with M. Malgaigne, that irrigations are not absolutely infallible any more than other means employed in surgery: but, with the numerous facts which I possess of lesions located in almost every part of the body, forearm, leg and knee, I cannot agree with him in the limits which he has

placed upon its use. As to the duration of the irrigations, I think, where a moderate temperature is suitable, they may be continued a long time with advantage, and I am reminded among others, of a case, which I will soon publish, where a cure was effected of a severe wound of the leg, by irrigations of water at a temperature of 20° C. (68° F.) continued more than two months.

In 1844, M. Nelaton, speaking of the treatment of contused wounds, in his "Elements of Surgical Pathology," thus expresses himself: "We have before said that cold irrigations are useful in the treatment of contused wounds; they are especially useful in wounds of the extremities, complicated with crushing of the bones and laceration of the soft parts.

"The treatment of wounds by the continued action of cold water claims the highest antiquity; Hippocrates, Galen, and Celsus have all spoken favorably of it. In our day several eminent practitioners have made similar trials, and with similar success. The eulogiums which M.M. A. Berard, Sanson, Breschet, Velpeau and Josse bestowed upon it, have contributed to its extension, yet it is still far from being generally adopted. Cold water has not always been employed in the same manner in the treatment of wounds; sometimes there is applied upon the wounds one or several compresses dipped in this liquid; and when they begin to become warm they are renewed, or dipped again in cold water. This method exposes the part to alternations of heat and cold, either by a complete omission to renew the cloths, or by their not being renewed sufficiently often: this method has therefore been abandoned, and continued irrigations with cold water have been substituted.

"I have seen, in the service of M.M. Breschet and Sanson, irrigations continued ten, fifteen, twenty days, and even a month, for the gravest articular wounds, and for the most complicated fractures. In many cases I have seen complete restoration, and never have I seen serious accidents supervene which could be charged to the irrigations.

"Nevertheless, in spite of its advantages, permanent irrigation with cold water cannot be employed as a general method of treatment. All who have employed it regard it as an exceptional method, especially applicable to contused wounds, and more especially to wounds complicated with fracture, where, in the case of the upper extremity, the injury is not above the elbow, and in the case of the lower extremity, not above the knee.

"M.M. Breschet, A. Berard, and Pinel Grandchamp have substituted tepid water for cold in irrigation, and the results have been generally satisfactory."

In 1847, M. Richet presented to the concours *pour l'aggregation*, the surgical section, a thesis upon the employment of cold and heat in the treatment of surgical affections. In this excellent treatise, which answers perfectly to its title, the author discusses carefully the advantages and the inconveniences of cold and of heat; we have derived from it some very valuable information, both historical and practical.

Having completed this historical sketch upon the surgical employment of water, I will dwell a moment upon hydrotherapia. Two distinguished practitioners, MM. Scoutteten and Schedel, have published two important works upon this subject, which will enable us to judge of its merits.

This system, founded, as they say by a simple peasant of Silesia—Priesnitz—already reckons so great a number of partizans, that there is not a country in Europe where we do not find hydropathic establishments. Priesnitz began by applying fomentations and baths to surgical affections, such as sprains, fractures, slight wounds, and the results seemed to him so marvellous, that he proceeded to extend its use, by applying it in most affections, internal as well as external. One of his first cures was made upon himself. In the season of hay-making, says M. Scoutteten, he was kicked by a horse, which throwing him down, and the wagon passing over him, broke two of his ribs. The accident was so serious that the surgeons thought he would be maimed for life. Priesnitz disputed their judgment, and determined to treat himself; the consequence was, he perfectly recovered.

Since then, hyriatic means have been applied with success in a great number of analogous cases, and very recently Dr. Fleury has published a statement of the uterine diseases successfully treated by this method.

If now we examine the different modes of dressing which are used in the hospitals of Paris, we shall find that cataplasms of flour, linseed, linen spread with cerate, etc., constitute almost the only means in cases of wounds and contusions of every kind. If cold water irrigations are sometimes resorted to, it is only as an exception, yet I have myself seen cases of comminuted fractures, wounds, simple and contused, felons, &c., in which the application of water would have proved serviceable in the reduction of inflammation, to say nothing of its effect upon the pain, which is certainly abated, and often destroyed by simple water dressings; a result which cannot in the same degree be obtained from cataplasms and the other modes of dressing.

Yet I acknowledge that M. Baudens, of Val de-Grace makes continued applications of refrigerant mixtures, or of ice and simple cold water in most traumatic lesions, and after a great number of operations. M. H. Larrey,

surgeon-in-chief of the hospital of Gros-Caillou, also uses irrigations frequently.

In private practice, in the city of Paris, this means is proportionably more employed than in the hospitals. My father resorts to it constantly in the wounds and complicated fractures which he has to treat; during the last few years, especially, he has generally used water in most surgical affections, and after operations.

As to the practice of foreign surgeons, my information is not sufficient to enable me to speak positively: I know, however, that in the hospital of the University of London, nearly all the dressings are made with simple water.

I believe the use of simple water in irrigation is quite common in the provincial towns of France. It is used with success by M. le Clerc of Saint-Germain, M. Guyot of Rennes, M. Fontan of Niort, M. Maher of Lorient, MM. Chaumette and Erigohen of Bordeaux, M. Rigal of Gaillac, M. Villepin of Compiègne, M. Patry, etc., etc.

HISTORICAL SUMMARY.

It will not perhaps be uninteresting to review, in the briefest possible manner, the successive phases through which the use of water in surgery has passed.

In the first periods of the world water must have been almost universally employed, because of its simplicity.

Hippocrates, Celsus, and Galen have all assigned to water a most important place in hygiene and medicine, and have recommended it also in a great number of surgical affections.

In the middle ages, powders, ointments, plasters, miraculous and charmed waters, were too much in favor, and their use was too much in accordance with the general spirit of the age, for a remedy so simple to possess any credit with either physicians or their patients. At a period somewhat later, Fallopius, Ambrose Pare, Martel, &c., endeavored to introduce the use of water, and to separate it from superstitious practices.

In modern times, Theden, and especially Lamorier, sought to attract the attention of their cotemporaries to an agent which had been so successful in their own hands, but without being able to secure for it fairly a place in surgical therapeutics.

The Academy of Surgery, which has furnished so many valuable works upon most surgical diseases, has especially endeavored to simplify dressings, and it would seem that to the accomplishment of this end they would naturally have urged the use of water, inasmuch as it would effectually and

advantageously have replaced all those agents whose abuse and use even, it desired to abolish.

It is true, as the passages which we have quoted from the memoirs and prize essays of this learned Society abundantly prove, that some of its surgeons appreciated the value of water, but they did not sufficiently insist upon its advantages to command attention. So evident is this, that not one author who has since written upon this subject has referred to the writers of that period, a period so glorious for French surgery.

Lombard, by indicating precise rules for the employment of water in surgery, in a work published in 1786, may be regarded as the founder of this new practice. The period was moreover very favorable for its introduction; the necessities of war compelled surgeons to employ this liquid under circumstances where nothing else could be procured, and we are astonished after seeing the results that were obtained by Percy, Larrey, Treille, &c., that the use of water did not from this time become general. Still its reputation has been progressive, and we can see that water was in much more universal use at the beginning of the present century, than in the preceding centuries.

When the attention of surgeons was attracted to irrigations by the works of M. Josse and A. Berard, published in 1835, as also by the success of MM. Breschet, J. Cloquet, &c., a great many other surgeons resorted to it, and it attained a temporary popularity. But gradually, through the influence of old habits, and perhaps also on account of the care and attention which the use of water demands, and especially on account of the occasional accidents resulting from the *cold*, this powerful agent has been neglected, until, as we have remarked, its employment in the Parisian hospitals is only exceptional.

Very recently, also, I have visited the hospitals of Lyons, Bordeaux and Geneva, and I have found but few exceptions to the common mode of dressings, such as is practised here, and generally elsewhere. The same is true even at the hospital of Lausanne, where resided the elder and the younger Mayor, who next to Josse, father and son, have more eulogized the topical application of water, than any other person.

Finally, the great number of water establishments which actually exist in Europe, and in which water is systematized through all its forms in the treatment of surgical diseases, the hospital of the University of London where all the dressings are moistened with this liquid, encourage a hope, that at no distant day this therapeutic agent will occupy the first place in surgery as a topical antiphlogistic.

(To be Continued.)

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Iron. From the French of Trousseau and Pidoux. Translated for the N. H. Journal of Medicine.

The preparations of iron, almost excluded from the *materia medica* during the reign of the school of Val de Grace, have, within a few years, received a new impulse, to which we are not strangers, and now they have resumed the important place which they held in the last century. In our day, there are few physicians who do not frequently use iron, and who do not rank it, as to its utility, by the side of quinine, mercury, opium, &c., &c.

Physiological action of iron upon man in health. The martial preparations given internally produce inconsiderable effects upon man and woman in a state of health, and yet they ought to be noted.

Under their influence, no apparent effect is immediately produced, but, after eight or fifteen days, a feeling of fulness and of plethora sometimes manifests itself, which throws one into an indescribable distress. The head becomes heavy and painful, the understanding less clear, and in a word, the symptoms of sanguineous plethora follow; the face, the chest and the back of women are frequently covered with pimples of acne, (*varus sebaceus*;) which only yield when the iron has been discontinued for some time. There is no fever, no excitement properly speaking, no modification of the secretions. Its effects on the stomach are hardly appreciable. It does not increase the appetite, it even more frequently diminishes it, and causes heaviness of the stomach, fetid eructations, diarrhœa, and more frequently constipation. The stools almost always take a black color, like that of ink, and this appearance has frequently deceived physicians for melœnic dejections. This black color, according to Barruel, is due to the action of gallic or tannic acid, which is found mixed with our food. Bonnet of Lyons attributes it to the combination of sulphur with the iron, and in this case he thinks to the formation of a sulphuret of iron.

At first sight it seems that the opinion of Barruel should be admitted without dispute. But we have many times seen the black stools from patients who had been put upon an exclusively milk diet, and who consequently were not taking any aliment which contained tannin. It is necessary then to have recourse to another explanation; the reaction pointed out by Bonnet, like the other, solely chemical, cannot satisfy the practitioner who sees the stools of certain persons retain the yellow color, although iron is taken in considerable quantity, and though the stercoraecous matters are very fetid.

May we not consider this coloring of the stools as due to a modification

in the secretion of the liver, analogous to that which takes place when we administer calomel to a patient? In this latter case the excrements are green, and chemistry is till now as unable to explain this fact, as many others occurring in our economy.

We ought not to forget to note the action which iron has upon the teeth. All persons who take the ferruginous mineral waters know that after a few days the teeth grow black, chiefly at the line of union with the gums. This trouble arises, moreover, only when one takes soluble preparations, and when, consequently, the salt of iron is in direct contact with the teeth. With some persons this stain resists the action of a brush, and renders the aid of a dentist necessary.

Some practitioners have affirmed that the ferruginous preparations may cause a somewhat energetic venereal orgasm. We can ourselves be witnesses to this effect.

Occasionally, with women, the use of the martial preparations in a somewhat large dose, causes in the bladder an active irritation, manifested by frequent attempts to micturate, and smarting in the meatus urinarius, unimportant accidents which readily yield to the use of hip baths, or emollient lotions.

The influence of iron upon menstruation is entirely different from that which is commonly attributed to it. According to all therapeutists the ferruginous preparations excite the menses, but some investigations made with care have shown us that the menses are frequently retarded, and sometimes rendered less abundant, by these preparations. We shall see below what has been the cause of the general adoption of the contrary opinion.

Topically, the preparations of iron have an astringent action upon the tissues; they moderate the suppuration of ulcers, hasten the cicatrization of wounds, and check hæmorrhage. The soluble preparations are clearly the most energetic; the insoluble have nevertheless the same styptic properties as the others, but in a less degree.

Therapeutic action of the ferruginous preparations. In order to understand the mode of action of the chalybeates in the diseases to which these remedies are adapted, it is necessary to enter into some consideration of the disorders which the modifications in the crisis of the blood produce in the economy.

After a large bleeding, doubtless because the organs no longer receive the normal supply necessary to the accomplishment of their peculiar functions, numerous disorders arise in the economy. These disorders, at first very noticeable, disappear by degrees, in proportion to the renewal of the blood. But if the bleedings are repeated, so that the blood cannot renew itself; if the alimentation is not sufficiently nourishing to furnish materials for this repair; or yet more, if a disease, unknown in its essence, and very common among women, blanches the blood yet more completely than is that of those who have suffered great losses of blood; there appears in women that which is known under the name of chlorosis, in men that which has received the name of anæmia. Chlorosis is almost always spontaneous. Anæmia is usually the result of losses of blood.

It is difficult to say why chlorosis is so almost exclusively peculiar to women that we can scarcely find a young boy chlorotic. This is explained, at least in part, by the difference of blood in the two sexes. In fact, the

analysis of Fœdich has shown that the blood of a young man in health contained in a hundred parts in two different trials—

Cruor.	Serosity.	Fibrine.	Iron.	Water.
13.611	8.801	2.460	0.880	74.248
15.000	9.320	3.111	1.001	71.568

The blood of a healthy woman contained in two different analyses:

12.400	8.601	2.511	0.801	75.687
14.400	8.920	2.507	0.901	73.278

From which it follows that in the physiological state the cruor and iron are less abundant in woman than in man. Now these are precisely the two elements which are deficient in the chlorotic woman, as is proved by the two following analyses made by Fœdich upon the blood of two women affected with chlorosis:

Cruor.	Serosity.	Fibrine.	Iron.	Water.
9.141	9.261	0.640	0.330	80.628
8.590	8.221	0.631	0.501	83.075

The analyses of Messrs. Andral and Gavarret, recently published, appear to confirm those of Fœdich.

In the normal state, the blood according to them contains in a thousand grammes:

Fibrine,	3 grammes.
Hæmatosin,	2 "
Solid albumen, constituting with the hæmatosin the red globules, }	125 "
Liquid albumen,	68 "
Soluble salts,	12 "
Water,	790 "

In chlorotics, the proportion of the globules may go down from 127, (the normal type,) to 38, as these gentlemen have observed in one case.

On one point only do their results differ from those of Fœdich. He states that in chlorotics the fibrine, which in the healthy woman does not exceed 25 ten-thousandths, may go down to 6 ten-thousandths in chlorotics, while Messrs. Andral and Gavarret assert that the fibrine remains in the same proportion; a thing which we confess appears to us hardly probable.

These curious analyses of the blood suggest at once a reason for the paleness and fluidity of the blood of chlorotic patients, and beside, of the most of the singular symptoms which they present. One can conceive in fact how the blood, deprived in part of its exciting principles, the cruor and the iron, and of its reparative principle, the fibrine, no longer suffices for the material nourishment of the organs, and that numerous functional difficulties result from it.

The voluntary muscles become blanched, atrophied and relaxed—hence the difficulty and slowness of the movements; the muscles of organic life participate in the same troubles, hence the flaccidity of the heart, the difficulty of the circulation, the sluggishness of the stomach, the constipation, the flatulence. In fine, the blood not arriving at the nervous centres, or the glands, or the membranes with its natural qualities, the nervous centres, the glands and the membranes cannot perform their functions as in

the normal state. If then we restore to the blood the principal elements which it wants, we make it anew fit to influence the economy naturally. Now iron answers this indication.

It is asked how iron thus accomplishes the coloring of the blood. Some attribute to this remedy a tonic action only, in virtue of which the digestive and nervous functions are so influenced as to render innervation and nutrition more perfect, and thus the organic reconstitution is rapidly facilitated. Others, and these formerly less numerous, are now the majority, suppose that the iron being absorbed may pass directly into the blood, may restore to it immediately the principles which it wants, and may make at once of this fluid a reparative element.

The existence of iron in the blood was admitted, and had already been demonstrated a long time ago, and it was on this metal that the color of the clot was made to depend by Jos. Badia, Galeacius, Menghinus, Rhades, Widmer, Haller, and Fourcroy. But this presence of iron, explicitly denied by Wright, was demonstrated in a positive manner by Forcke. Even after chemistry had made immense progress, the question remained in dispute, and many persons considered the facts as invented on which the authors rested who affirmed that they had proved the existence of iron in the blood. At the present time no doubt can remain upon the subject, since Barruel, the conductor of the chemical investigations of the faculty of medicine at Paris, has shown that the blood contains an enormous proportion of iron; that in the blood the coagulable portion alone contains it; and has, in the presence of a great number of physicians, obtained iron from all the blood which was brought to him. We can here add our own testimony. One of us, being at the medical school in 1832, met with accidents which made a copious bleeding necessary. He was bled and had taken from the vein a kilogramme (2 lbs. 8 oz. 1 dr. 14 grs.) of blood. This operation was performed in presence of Barruel, who proposed to extract before us the iron contained in the blood. We agreed. Barruel calcined the blood, then having put it in a crucible prepared in a peculiar manner, as for the reduction of metals, he submitted it to the action of a very hot furnace fire, and we found at the bottom of the crucible a globule of iron, weighing one gramme, (15.43 grains Troy.) The same Barruel treated in like manner 350 grammes of blood drawn from Orfila, dean of the medical faculty of Paris, during an attack of cholera which brought him to the gates of death, and he obtained a globule of 35 centigrammes, (5.4 grs. Troy.) which Madam Orfila had mounted in a ring. Again, in 1835, a young man in our employ fell from a horse, on account of which we were obliged to draw 500 grammes. He had heard from us of the experiments of Barruel, and he also desired to have the iron which his blood contained. When he had recovered, we went together to find Barruel, who obtained in his presence a globule of iron, weighing 45 centigrammes, (6.94 grs.) which, set in a ring, was offered as a gift to a celebrated actress of Paris. Moreover, the analyses of Fœdish, quoted at the commencement of this article, show the same thing.

The point was now to prove that iron was actually absorbed. And in the first place, as we have said, they have been able to show the presence of this metal in the urine. Tiedemann and Gmelin have found iron in the bladder and especially in the blood of the mesenteric veins, and of the portal vein of a horse which six hours before they had made to swallow a

solution of 28 grains of protosulphate of iron. There are besides many observations which show that nut galls can blacken the urine of persons who have made much use of ferruginous waters and preparations.

Very recent and perfectly satisfactory experiments have been made by Brueck, at Dribourg. "We do not know," says this author, "that iron is really the coloring principle of the blood, but new experiments on rabbits have proved that iron administered enters actually into the mass of blood; it has been found that the phosphate, the muriate, and the carbonate of iron, and less rapidly iron filings, are digested and assimilated in the dose of five centigrammes a day for the former preparations, and in that of two and a half centigrammes for the latter. In the whole, the mass of the blood of a rabbit cannot be impregnated with more than forty or fifty centigrammes; the assimilation afterwards appears to stop for some time, and the masses of iron afterward given were evacuated by the rabbits for fifteen days."

"In comparing, adds Brueck, "these experiments which show the introduction of iron into the mass of the blood, one sees that with chlorotic women the blood takes, under the influence of this medicine, a redness more and more intense. It appears to us allowable to draw the conclusion that the iron, although indeed it may not be the immediate cause of the color of the blood, yet increases the parts of this fluid capable of receiving color by the aid of respiration, to wit, the globules or their envelope."

To rest then upon the true and the undeniable, we may say: 1st, that the blood of chlorotics contains less of clot and of iron than the blood of healthy women: 2d, that by the use of ferruginous preparations the blood quickly recovers the clot and the iron it had lost: 3d, that the iron is evidently absorbed, circulates in the vessels and is separated by certain secretions. As to the rest, we will not here recall it: this is theory, and our readers have been able to perceive that we do not hold much to theories.

The blood thus modified by the iron, again recovers the properties which it had lost, a proof that the reparative fluid has been completely recomposed.—*New Hampshire Journal of Medicine.*

Inoculation in Rubeola. By JOHN E. MCGIRR, A. M., M. D., L. L. D., Professor of Chemistry, Physiology, &c., in the University of St. Mary's, Physician to the Catholic Male and Female Orphan Asylums, Chicago.

Inoculation in Rubeola is no new experiment. As to the advantage of the process, diversity of opinion exists. Drs. Home, in Edinburg, Dewees, and Chapman, at the Dispensary in Philadelphia in 1801, practiced inoculation without any satisfactory results, while the experiments of Prof. Speranza of Mantua, and others, were varied, decisive and successful. Having no opinion of my own to confirm, wishing only to arrive at the truth, if possible, I determined when the very favorable opportunity presented, by the breaking out of Rubeola in these Asylums, to test this point. The Asylums are situated, (the female in north, and the male in south Chicago,) without the thickly settled portion of the city, having the advantage of healthy locations. The houses are large, well ventilated, and are under the charge of the Sisters of Mercy; thus the best nursing could be secured, and the best opportunity which might ever again occur to me of watching

every stage of the progress of the disease. Early in December the first case of measles was brought into the Female Asylum. I proceeded to inoculate from this case, when the eruption was at its height. Blood was drawn from a vivid exanthematous patch on the diseased child's arm, and inserted into the arms of the three children first mentioned in the list below. On the fourth, sixth, and seventh days, after the inoculation, the measles appeared, pursuing a regular and mild course. The result of these cases determined me to carry the experiment farther, and that the trial might be a fair one, I selected for comparison those whose physical conformation and constitutional idiosyncrasy seemed most nearly alike, giving the disadvantage of age to the inoculation. The following table contains the names, ages, and results of all the cases whether inoculated or not:

NOT INOCULATED.		INOCULATED.	
Died.	Age.	Recovered.	Age.
Ellen Brown,	3 years.	Ellen Kehoe,	11 years.
Katy Russell,	2	Ellen Grant,	4
Philomena Kehoe,	3	Mary McCarty,	8
Elizabeth Patton,	2	Rose Mack,	5
Ellen Crowley,	5	Mary Grant,	9
Recovered.		Eliza Hurley,	4
Mary Carroll,	9	Ann Cahill,	8
Ann Brennan,	6	Ella Welsh,	5
Mary Patton,	7	Ann Mulhall,	9
Johanna Cabill,	5	Ann Hagan,	3
Emeline Hurley,	4	Mary Mulhall,	4
Mary Nugent,	5	Ellen McCarty,	10
Mary Brain,	10	Anna O'Brien,	13
Elvira Gilmartin,	5	Cath. Power,	9
Fanny Mooney,	12		
Mary Ann Tell,	10		
Total Inoculated,	15	Total not Inoculated,	14

This table gives us 29 names, 24 recoveries, and 5 deaths, all occurring among those not inoculated. The cases of all those inoculated, commencing from the fourth to the ninth day after inoculation, proceeded regularly, with the ordinary symptoms of simple measles, to convalescence, which was speedy and complete, with one exception, viz., the first case. This child entered the asylum about a year ago, suffering with violent ophthalmia. She had been cured. On the disappearance of the measles, the ophthalmia returned, and though the sight was much endangered, yet there now remains only a little weakness which is disappearing. All these cases occurred consecutively from the first week of December to the second week of January.

Four children who were known to have had measles in the spring of 1840, were inoculated; nothing else was observed than the inflammation which would follow any ordinary lancet puncture.

Of those not inoculated, with four exceptions, the antecedent symptoms were very severe. The fever was violent; distressing vomiting occurred in three cases. The catarrhal symptoms were violent; throat sore, hoarseness, rigors, cough almost continuous, dry, the whole chest sore, difficult respiration, delirium at night in some of the cases.

Four had the "congestive modification," the eruption appeared slowly and imperfectly; one of these died. Two others presented the Typhoid variety; one died of diarrhœa, the other recovered, but afterwards four dangerous ulcerations appeared on the limbs, and gangrenous stomatitis, in the left lower jaw. All of the teeth of that part of the jaw fell out, the left side of the tongue and the cheek were involved in the disease. This case ultimately recovered. Bronchitis supervened in six cases. Three had partial aphonia, one complete; this one died.

When these last mentioned cases attempted to swallow any liquid, it was thrown back through the mouth and nose with violent expulsive effort.

In the Male Asylum, there were 23 cases and 6 deaths. None were inoculated, but 3 of the whole number had the disease mildly, and these were the three first attacked. The others had violent antecedent symptoms, and tedious convalescence. Five of those who died had the aphonia and difficult deglutition before spoken of, the other died of phthisis.

In review of these facts much might be said. I have chosen, however, to give them as they occurred, without comments, leaving to the readers of the Journal to estimate them at what they are worth; merely adding, *that if there is no advantage in inoculation, the result which the second column furnishes would be a strange anomaly.*—*North Western Med. and Surg. Journal.*

On Cupping with Tumblers to arrest Vomiting.—The usefulness of the tumbler, applied as described by Dr. Lyle, for controlling irritability of the stomach, cannot be too highly estimated. I have never seen it fail to prevent vomiting, and have heard patients plead for its removal, in order to give them a chance to puke. A very respectable practitioner of medicine in this city, who devoted himself most assiduously in attendance upon the victims of cholera, in what is known here as the plague spot, in the lower end of the city, was himself violently attacked with cholera. When I saw him his efforts at vomiting were almost incessant, and they were among the most painful I have ever seen. From the moment of the application of the tumblers, as cups, over the epigastric region, all vomiting ceased, and the patient declared that from the moment of the application of the tumblers, until they were removed, he felt as if he had no stomach, and said that he did not believe that any emetic could have acted upon him while the tumblers remained on him. I have used them in the way recommended, a multitude of times, and their success has been unvarying. Dr. A. P. Elston used them extensively, for the same objects mentioned here, and he bears testimony to their uniform efficiency. He mentioned to me a case, in which he used the tumblers and arrested the vomiting; upon their removal, it returned, and it was again arrested by the re-application, and these alterations continued until the patient was relieved of the disease.

The value of this remedy for vomiting is the greater from its eligibility. It may generally be applied in a few minutes. It is only necessary to pour into a tumbler a small quantity of brandy or alcohol, and shake the liquid so as to smear the inside surface of the tumbler with the alcohol. Then throw into it a lighted piece of paper, and while the alcohol is burning, invert the tumbler promptly over the epigastric region. The vacuum

formed by the burning alcohol, gives the external atmosphere an opportunity of making a considerable pressure, and the tumbler remains fixed, frequently, for hours. If the vomiting were one of the dangerous symptoms of cholera, we should have a great control over the disease, by this simple remedy. But unfortunately, the gastric movements are more distressing and annoying, than dangerous, and patients often rapidly sink into death after the complete arrest of the vomiting and purging, and many fatal cases occur without any vomiting.—*Dr. T. S. Bell in Western Jour. of Med. and Surg.*

On the Treatment of Rheumatism by Lemon Juice. By G. OWEN REES, M. D., Assistant Physician to Guy's Hospital.

Although the treatment of rheumatic diseases by lemon-juice has received a considerable amount of favorable notice from the profession, I am still inclined to believe that it has scarcely gained the credit it deserves. This would appear to depend in some degree on its use in cases which experience has shown me are but ill-adapted for its exhibition, and which are in all probability improperly placed by our pathology in the same category with those forms of the disease in which early benefit is derived from the administration of the remedy. There is much indeed in the history of those conditions designated rheumatic to lead us to hope, that as medical science advances, important distinctions will be made where none are as yet recognized.

From the experience I have had in the use of the remedy, I feel I have sufficient evidence before me to justify the opinion, that there are certainly two forms of rheumatic diseases which cannot be benefitted by the administration of lemon-juice.

The first of these is generally observed in cachectic subjects, and for want of a better name I shall call it cachectic rheumatism. It occurs in all classes of life, but more commonly, I believe, among the lower orders. It is more frequent in females than in males. The swelling and redness of the parts affected are less marked than is generally the case in acutely painful rheumatism. The pain is, however, very severe, and occasionally partakes of the neuralgic character. The patient is sometimes anæmic, and on inquiry we may perhaps discover a history of long mental or physical suffering. The skin is perspirable; the pulse weak and rapid; the tongue varies, being sometimes moist and white, and sometimes clean and less moist than natural. In cases of this description I have not succeeded in relieving the patient by the exhibition of the juice; and if occasional relief has been obtained, the disease has shown a tendency to relapse, and become unmanageable under a continuance of the remedy. It may perhaps be well to mention, that in these cases I have derived the greatest benefit from the exhibition of opium in full doses at frequent intervals. The other form of rheumatism in which I have failed to obtain relief by the administration of the juice is that attendant on syphilis. In all the cases in which I have made trial of it among the female out-patients at Guy's Hospital, it has failed to exert any beneficial influence. The nature of the disease, so distinct from that of ordinary rheumatism, never indeed gave me any great hope of success. If we except the disease above described,

and cases simulating rheumatism, but really connected with ordinary dyspepsia, or, as is sometimes the case, with the existence of Bright's disease, my continued experience has but the more persuaded me of the great value of lemon-juice as a remedy for rheumatism. Its action is most remarkable, causing cessation of pain and decrease of swelling and redness, such as we can rarely obtain with colchicum, even when administered in large and hazardous doses. That lemon-juice sometimes fails to effect this rapidly is certainly true, and that too with respect to cases apparently identical with those in which early benefit has been observed; but the history even of these less favorable instances will generally bear comparison with the results obtained by the ordinary plans of treatment; and it is my full conviction, that since lemon-juice has been introduced at Guy's Hospital as a remedy for acute rheumatism, the period during which it has become necessary to confine patients so affected to bed has been very materially lessened.

I am anxious now to direct attention to a class of chronic rheumatic cases in which I have used lemon-juice with very great advantage. I allude to such as are connected with deposit of lithate of soda in and about the smaller joints, and which partake more or less of the gouty character. I have met with great success in this form of disease by the continued use of lemon-juice in combination with small doses of the tincture of the sesquichloride of iron, and in several instances have effected absorption of deposits which have resisted all other plans of treatment. A case of the above description was lately reported to me, in which like success attended the administration of the lemon-juice alone. The patient, a lady, had been a cripple for several years, and was eventually restored by persevering in the use of the remedy for six or eight weeks.

Lastly, with respect to the dose with which the juice ought to be administered. Experience has shown me that it should be larger than I was at first inclined to consider necessary. In acute rheumatism, from one or two ounces should be given every four or six hours; and should pain be felt in the bowels, or diarrhœa occur, which is very rarely the case, four or five minims of tincture of opium may be added to each dose of the remedy.—*London Lancet.*

The Mania for Operation.—When anæsthesia was first introduced into the practice of surgery, it was justly hailed as a most valuable boon to suffering humanity. The knife of the surgeon lost, as it were by magic, all its terrors. When the sufferer for the first time was presented to the eye of the spectator, lying passive under the influence of chloroform, how strongly was marked out the difference between the sensible and insensible object of operative procedure. There was no longer witnessed the cry of agony issuing from the frail body of some poor nervous, emaciated woman, whose breast was about to be submitted to the knife; nor the scarcely less painful effect of subdued emotion, in the strong frame, while it quivered under the strokes of the scalpel. The surgeon now has not to contend against these calls upon his humanity, and his responsibility is not increased, by knowing, that whilst he is performing a painful duty, he is inflicting great, though

necessary pain. There lies the patient, under the influence of the Lethean vapor, revelling perhaps in dreams of happiness, while the operator is employed in removing a limb, or dragging away some portion of necrosed bone—the patient not being the least sensible of either the pain or the danger of the operation.

Such are some of the more prominent benefits which chloroform has conferred upon sufferers from disease. Like all such blessings, however, it has its drawbacks and evils, amongst the more conspicuous of which may be mentioned the facility with which patients are now persuaded to submit to the knife, and the encouragement which it holds out to what are called "promising young men" "to carve their way into practice." But for chloroform, it is scarcely to be believed, that some of the formidable operations for the removal of ovarian tumors would have been resorted to, when other and more justifiable means have been devised for their relief, or that the head of the thigh would have been submitted to the knife and the saw with as much nonchalance as though it were being removed from the dead body in the dissecting-room. The reports of discussions in the medical societies during the last session are frightfully illustrative of this operating mania. The "exploration," as it is called, of an ovarian tumor, by an incision through the walls of the abdomen, is regarded in no more formidable light than the application of the stethoscope; and an incision from the ensiform cartilage to the pubis is declared to be an innocent proceeding. How are we to show this? Not in the public records of the operations, for in these only the successful cases are brought under notice, whilst the instances in which the patient has succumbed are buried in oblivion. Can it be true, as is currently reported, that the statistics of what is called ovariectomy are mere attempts to blink the question, and that the results have been so unfortunate that the practitioners who have performed the operation dare not tell the truth on the subject? However this may be, rumor asserts this as the motive which influences certain operators in withholding from their professional brethren the result of their experience. With this, indeed, they have been openly charged in the first medical society of the kingdom. Can it be true? The silence that has followed the charge, makes it evident that in some particular instances there must be strong motives for concealment. The publisher of unsuccessful cases, be these what they may, is, at all events, to be lauded for his candor. He may have erred in judgement, but his honesty must be held to be unimpeachable.

But what are we to say to those operators, honest as they may be, who have performed operations for which there is no justification, and which cannot be mentioned without exciting our strongest feelings of reprobation? An operation which has been denounced by Brodie, Syme, and Coulson, may justly be suspected; but what shall we say to such an operation when performed under the painful and pitiable circumstances as are alluded to in the Report of the Medical Society of London last week. We do not wish to enlarge upon this really distressing subject; but public duty demands that such operations should be held out as beacon-lights to the young surgeons of this empire, to warn them from pursuing a course which must eventually terminate in their discomfiture. With the grave thinkers, and the great operators of our time, the resort to the knife has been always regarded as an opprobrium upon the skill of the surgeon; not so with some of the young gentlemen who would vainly aspire to walk in the footsteps of a Cooper or

a Liston. Let them remember, however, that these eminent members of our profession owed less of their just fame to their successful use of the knife than to the exercise of those acquirements and that sagacity which enabled them to select the proper cases for operation. Such men as these did not operate for the sake of cutting; they resorted to the knife only as a substitute, and that to them, a lamentable one, for other less successful, but always employed resources of surgery.—*London Lancet*.

The above, though calculated for another meridian, is equally applicable to our own; and we fear may be extended so as to include some beside the "promising young men" of the profession.—*New Hamp. Jour. of Med.*

Importance of Latin and Greek to the Physician.—We quote the following effusion by the editor of the Transylvania Journal, not because we concur fully in his views, but that our readers may enjoy a smile at the facetious manner in which the writer treats the subject. We think that the importance of classical studies is placed too low, rather than too high, by the medical profession generally, and we should regret to see the position taken by the American Medical Association that they might be dispensed with, as a preliminary to medical studies. Nevertheless, it must be admitted, if the test proposed by the writer of the following were to be insisted on, the Association would be likely to waive the discussion of the subject:—

EDITOR BUFFALO MEDICAL JOURNAL.

"It has always appeared to us as a laughable absurdity for the Association to be so gravely emphatic on the subject of 'the classics,' while not a tithe of them can master a chapter of *Viri Romæ*, or stumble through a forced version of the first paragraph in the 'pons asinorum.' That the amount of Latin and Greek which is usually cudged into a boy's brains, through some mysterious route beginning at the posteriors, may be of no vital disadvantage to him when he has tossed his *Virgil* to the jakes, shaken his fist under the the pedagogue's nose, and turned medical student, we cheerfully admit, with a proud reference to the small stock of Latin maxims which we have preserved from the ruins of a 'classical education,' and which we refresh whenever our pedantic vein approaches, by reference to dictionary of quotations.

"We commenced the study of the profession with a full faith in the extent and profundity of our classical acquirements, and, possibly, might have passed muster before a committee of the National Association; but we found to our consternation that Tityrus and Melibeus were asses in the matter of pukés and purges, and, alas! not a word of squirts and clysters, or of medical reform, was to be found from one end of *Tooke's Pantheon* to the other, and the naked gods and goddesses were utterly worthless even for the purpose of the dissecting room.

"We shall insist, whenever the Greek and Latin clause is brought before the Association again, upon the members being put through their moods, tenses, and a parsing spell.

"Gentlemen may therefore anticipate an opportunity of displaying the tattered remains of a schoolboy erudition; and we would advise every one to stuff the old, and much sworn-at grammar in his portmanteau, and to go through a course of sprouts with some strong armed disciplinarian, in order to revive those early and fresh 'classic' memories which are so closely allied to the penitential visitations of the much abused seat of honor."

On some of the Therapeutical uses of Manganese. Read before the Burlington Medical Association, February 5th, 1851. By S. W. BUTLER, M. D.

The ultimate object of all scientific research and investigation is, or ought to be, to arrive at truth and apply it to the benefit of mankind. In this field of boundless limits, there remains much, very much, both in nature and in Providence, which the unmaturing but ever-growing mind of man has yet to search out. How pleasing, how ennobling the thought, that there is a principle in man, which is ever expansive; and which, unsatisfied with present attainment, is ever on the alert to acquire new thoughts and ideas, and to establish new principles which he may appreciate to his own advantage, and that of his fellow-man.

Death, with disease and all our woes, is the penalty of sin, yet, for death, naturally the most revolting of all these, infinite mercy has provided a remedy, an infallible remedy, so that if misguided man will but appreciate it—will but "look on the brazen-serpent"—he shall live again, not for a day or a year, but for eternity.

To such as appreciate this remedy, the death of the body is but the beginning of life, like the seed, which though it rots in the ground, yet has a vital principle which wakes to life and vigor a plant calculated to adorn and beautify the earth, while it contributes to its well-being, and the happiness of mankind.

If then Infinite wisdom has provided a remedy for death itself, may He not provide, and *has He not provided* in nature, remedies, by which most of the pains and diseases which afflict the body may be relieved or overcome?

This is a proposition which the observation and experience of every one places beyond the need of proof.

But man is ever learning, and, profiting by the observation and experience of the past, is ever adding link to link of that chain of knowledge which connects all past with all future time, and with eternity itself. Indeed, the result of the observations of the past fifty years is proof positive that the mind of man is yet in its infancy in matters even of worldly concern.

It is not therefore to be supposed that we have exhausted nature, or ever will exhaust her in our attempts to draw from her vast resources materials for the relief of human suffering. How unreasonable, therefore, are those who are "afraid of new remedies," and who, content to turn their backs on the present and the future, are satisfied with the observations of the past.

True, the Pharmacopœia is burdened with a list of remedies, to learn the names and applications of which would alone be the work of a life-time.

But why should this preclude further observation? Is there no room for improvement? Are there no more materials to draw from? May not remedies yet be discovered which shall be so far superior to many now in use as to render their retention unnecessary?

Is it not likely that the *Materia Medica* list of 1851 will be as little esteemed two hundred years hence as that of 1651 is now?

We think that there are many important observations yet to be made, and that while other things around us are progressing, medical science will not be a whit behind hand, but will advance with equal strides with its sister sciences.

From these desultory remarks we will proceed to the subject of our essay, and briefly call the attention of the Association to manganese, in some of its forms, as a therapeutical agent.

In doing this I but embody the results of other men's observations, having had no personal experience with it.

A few experiments were made with manganese soon after its discovery, by Kapp, Vogt, Coupar, Gmelin, and others, but their observations did not seem to attract much attention to the subject, and some of their experience with manganese seems to entitle it to the favorable regard of the profession.

This mineral has been found in inconsiderable quantity in the human organization. M. Millon found it in the blood, and his observations have been confirmed by those of M. Hannon.

There are conditions of the system, when it is thought that iron is indicated as a remedy, and it is used with marked and acknowledged success. But if manganese as well as iron forms a part of the organism, why may not its use be indicated as well as the former, and why may it not alone, or in combination with iron, fulfill indications which the latter is incapable of fulfilling by itself?

Manganese, in the form of a binoxyd, commonly called the black or peroxyd of manganese, is found abundantly in nature. Its affinity for oxygen is so strong that the metal is isolated with difficulty, and must, when pure, be preserved in naphtha, or it soon becomes oxydized.

In the metallic state it very much resembles some forms of cast iron, and that metal is always or almost always found associated with it.

So little has it been employed as a remedial agent, that it is barely considered as worthy of a place in some of the best works on *Materia Medica*, while recent observations seem to entitle it to a place of no inconsiderable importance. Professor Jackson, of Philadelphia, has for several years been in the habit of calling the attention of his classes to manganese, thinking it a remedy worthy of more consideration than it has hitherto received. In prescribing iron, Dr. J. usually gives manganese in combination with it.

M. Hannon recommends various forms in which it may be used, such as the oxyd, carbonate, neutral malate, tartrate, phosphate, and iodide.

We are now prepared to notice briefly, some of the indications which manganese has been found to fulfill.

1st. *As a Cholagogue.* Gmelin in some experiments with the sulphate of the protoxyd on animals, found it capable of producing an extraordinary secretion of bile, which was so considerable, that "nearly all the intestines were colored yellow, by it, and the large intestines had a wax-yellow color communicated to them." These observations have been confirmed by other writers. Dr. T. H. Jewett, of South Berwick, Maine, found it a very prompt

and efficient evacuant of the bile, giving relief in cases of jaundice, where other remedies had entirely failed.

He thinks that "beyond any question the sulphate of manganese stands at the head of all cholagogues, doing its work with the promptness and efficacy of no other medicine." (*Boston Journal*, Dec. 18th, 1850, vol. 43, p. 393.)

From half a drachm to two drachms, dissolved in half a pint of water, and taken on an empty stomach has been found sufficient to produce bilious stools. There has been some discrepancy with regard to the quantity of the sulphate which may be taken at a dose, for while in the experience of some it has been found in large doses to have a powerful effect on the cerebro-spinal system, inducing apoplexy and palsy, others think it may safely be given in the dose of an ounce like Epsom salts. Dr. Thompson, of Glasgow, has seen it swallowed in the dose of an ounce without any other effect than that of a brisk cathartic. Is it not likely that in small doses it proves cathartic on account of the extraordinary secretion of bile it produces; in fact, that it is the bile itself that is the real cathartic? A quack remedy to evacuate bile, the *sel desopilant* of Rouviere, was found to contain the salt in question.

If these observations should prove true with regard to the sulphate of manganese as a cholagogue, (and we think there is strong evidence in its favor,) it is well calculated to fill a vacancy in the *Materia Medica* which has long existed, and been painfully felt to exist.

How the sulphate of manganese acts as a cholagogue is a very interesting question, but we will not now stop to notice it.*

2d. As a tonic, manganese has been found highly useful in cases where other remedies, even iron, have failed to exert a beneficial influence. Kapp regarded it as a "permanent stimulant," promoting the appetite and digestion. Vogt places it among the tonics. M. Hannon tried its effects on himself, first taking the carbonate in the dose of one grain a day, increasing it to four grains at the end of the first week, and eight grains at the end of the second. At this time he experienced symptoms of plethora, the appetite was increased, the pulse became stronger, and the color of the palpebral conjunctiva was heightened.

He reports several cases of anæmia from various causes, in which he employed the carbonate, sulphate, syrup of the phosphate, and syrup of the iodide, with marked success. He found that it frequently at first produced nausea, but the medicine was soon tolerated and its beneficial effects rapidly followed. It seemed in M. Hannon's experience to be particularly applicable to some cases of phthisis, in which complaint iron is often inapplicable on account of its increasing the cough and causing constipation.

When it is desired to obtain the general tonic effect of manganese, we would theoretically prefer an oxyd, carbonate, chloride, iodide, or lactate, if such a preparation exists.

It may be well in passing, to notice some of the toxicological effects of this mineral.

*Since this address was read, several of the physicians of Burlington have used the sulphate of manganese as a cholagogue very much to their satisfaction. One of them found from six to ten grains sufficient to excite slight bilious discharges. It has been used by them in doses varying from a few grains, to a drachm or more.

Dr. Coupar noticed that when gradually introduced into the system, by those who are employed in grinding it, a paralysis of the motor nerves is induced, commencing with symptoms of paraplegia. This poisonous effect differs from that of lead in not producing colina pictonum or constipation; and from mercury, in first affecting the lower extremities, and in not exciting tremors of the affected part.

The above observations we think may prove worthy the attention of the members of the Association, and it is to be hoped that a fair trial of the remedy may result in a confirmation of what has been said.—*New Jersey Medical Reporter*.

Observations on the use of Veratrum Viride in Fevers, Convulsions, &c.

By WESLEY C. NORWOOD, M. D., of Cokesbury, N. C.

We are always glad to give place in our Journal, so far as we have room, to whatever promises to be of practical value to the profession. Being favored with a large list of exchanges, we have it in our power to select many articles which we would be glad to transpose entire to our columns, but as our space for selected matter is limited, we must be content to give the substance of most of the articles, in doing which, it shall be our endeavor to do justice to the authors.

We insert below a portion of an article by W. C. Norwood, M. D., which we find among other excellent original papers, in the *Southern Medical and Surgical Journal*.

If Dr. Norwood's experience with the veratrum viride is confirmed, no one can deny that it is, in the language of a committee of the American Medical Association on Indigenous Medical Botany, "eminently deserving the attention of the profession." Indeed, the evidence of its power to control the action of the heart does not rest solely on Dr. Norwood's experience, as Drs. Tully and Osgood give their decided testimony in its favor. See an article by Dr. Osgood, in the *Amer. Journ. of the Medical Sciences*, vol. xvi, p. 296.

The remarks of Dr. Norwood, quoted below, are followed in the original article, by several cases which certainly seem to warrant their decided tone.

If any of our readers are induced to try the remedy, we hope they will make known their experience with it.

It may be well to remind the reader, that whether a tincture be employed, or in whatever form the root is used, it should be fresh, as it deteriorates by time.—EDITOR OF NEW JERSEY MEDICAL REPORTER.

"In a former communication, we stated some of the leading and prominent powers and properties of American hellebore and its peculiar adaptation to the treatment of pneumonitis. We then stated that all the powers and properties it was alledged to possess were true, and free from all exaggeration. We now go farther, and state, from the fact that some are disposed to doubt the validity of the assertions, that the half was not told. Its powers were not fully unfolded nor revealed, lest by giving too glowing and brilliant a statement of its capacity to arrest and subdue disease, it might meet the fate of many articles which flourish no longer than the

short time occupied in breathing their praise. We challenge trial, and pledge ourselves to demonstrate at the bedside, before any body of physicians, every power and property we have heretofore claimed for it and asserted it to possess. We go farther, and state that all of its leading effects are so striking that we are compelled to attribute them to the article, and to nothing else. We are sorry that we dwelt so long on the ill effects, if it can be said to possess any. It is not a drastic emetic in the sense that tartar emetic is said to be. Its nauseant powers are the most disagreeable of any effect belonging to it. From the description given, we learn that it deterred many from using it, and led others to question its usefulness. We deemed it far more prudent to enter fully into any unpleasant effects, than to keep them concealed, and present nothing but its prominent beneficial effects. We now boldly hazard the assertion, that it is the only article or agent known that will control certainly, and without disappointment, the action of the heart and arteries—that it is the only therapeutic agent known through which we can say to the heart and arteries, so fast shalt thou beat or pulsate, and no faster. We unhesitatingly assert, that it has not failed us in a single case to reduce the action of the heart and arteries, down to any point we wished, as to the number and frequency of pulsations. We believe that it will be eminently useful in every disease in which increased frequency of the action of the heart and arteries take place to any extent. We also believe, that when its powers are fully ascertained, tested and developed, it will produce an era in the treatment of disease.

We have no doubt that veratrum viride will prove a valuable agent in the treatment of scarlet fever. We stated in our former article in this journal, that veratrum viride certainly reduced the frequency of the pulse—that it produced the most intense paleness of the surface, and unusual coolness or coldness of the surface, accompanied with more or less moisture. In scarlet fever, we know there is usual frequency in the pulsations of the heart and arteries; also, that there is great heat and dryness of the skin, and congestion, if not inflammation in the capillary system. Veratrum viride eminently subdues, overcomes and removes every condition we find to exist in scarlet fever. In scarlet fever there is often more or less mucus in the fauces and trachea, and ulceration and inflammation often exists. Veratrum viride, by its emetic effect and acrid properties, will remove the mucus and change the action in the fauces. In scarlet fever, the heat, redness and dryness of the skin are extreme. Veratrum viride produces coolness, paleness and moisture of the surface; thus certainly relieving all these annoying conditions, and affording to the patient every opportunity of comfort and relief.”—*Southern Medical and Surgical Journal*, Jan. 1851.

On Morbid Poisons. By JOHN SIMON, Esq., F. R. S., Surgeon to St. Thomas's Hospital, &c.

By *morbid poison* we understand a product, which is the supposed specific cause of certain specific diseases; of syphilis, for instance; of scarlatina, of typhus, of glanders, of small-pox, of hydrophobia, and the like; a product, which has many striking differences from all other poisons, but chiefly these: first, that while other poisons (oxalic, or hydrocyanic acid,

or sulphuretted hydrogen gas) act directly in proportion to their dose, becoming more or less deadly in proportion as more or less of them is brought to bear on the organism; you may observe, contrariwise, that the *morbid* poison (the poison of contagion) produces in its characteristic results, when given in the minutest conceivable doses, just as surely, and just as deadly, as when the system is saturated with it;—and secondly, that while common poisons diminish from the body, or at the most remain stationary, during the production of their effects, morbid poisons apparently undergo, within the body on which they act, a striking and singular increase.

The phenomena which follow infection with a morbid poison consist of certain local changes, attended by a peculiar constitutional state. The *local* changes may be generalised as sub-acute inflammatory processes, attended (perhaps preceded) by the deposition of a *specific material*, which material in most cases contains an agent capable, by inoculation, of producing in another person the same symptoms as have attended its own generation in the original sufferer. The peculiar *constitutional* state is one essentially of depression; modified no doubt, and intermixed with those phenomena of reaction which the living body (like a spring) always opposes to the direct pressure of exterior influences.

Of the local changes, partaking of an inflammatory character, our memory can give many illustrations: such as are seen in the pustules of small-pox; in the cynanche and erythema, and kidney affection of scarlatina; in the intestinal ulcers of typhus; in the catarrh and eruption of measles; in the rupia or periositis of syphilis; in the swollen parotid of mumps; in the dysentery of malarious fever: in suppurating tumors of glands; and in various other symptoms that might be quoted.

And it is because of these local differences in *effect* that we are impelled to distinguish the *causes*, and to speak of them as *specific*: syphilis never produces ulcers in the ileum, scarlatina never causes iritis; the causative poison of the one disease differs from the causative poison of the other, for on the self-same subject it produces different effects.

[But there is probably a *material* in the system, or in the blood, upon which the morbid poison may act.]

What this material—the principle of infective disorders in the human subject—may originally have been, we are totally unable to say, but whatever may have been its first method of generation—we can now confidently speak of it as a possible product of the human body; we know that it is liable to develop itself out of some constituent of the human blood.

What are these constituents? Observation and argument sufficiently show, that the blood corpuscles and albumen can hardly be the constituents in question: first, because, after death by zymotic disease, they are found without evident alteration, and no considerable change in them could escape notice; secondly, because they are indispensable to life, and their even temporary transformation (if complete) would of necessity be fatal; thirdly, because immunity could never be attained by one attack of any particular disease, if it were requisite to exhaust these products: re-exposure to infection would insure a return of the disease, and a re-appearance of its phenomena.

For somewhat similar reasons, we may conclude, that the salts are not the elements concerned. Fibrine and the so-called extractive matters are what remain—can these be the ingredients in question? Substituting for

the chemical phrase "extractive matters," the physiological one "waste of the tissues." I am strongly disposed to think an affirmative answer to that question; or, at all events, unhesitatingly to point here as the direction in which accurate pathological investigation may be made with most prospect of success.

For, in the first place, they are matters already in progress of decay, and therefore eminently susceptible of new modification; in the second place, they are unessential to the nutritive processes, and that removal of them from the system, which would give immunity from re-infection, might be accomplished without withdrawing a vital ingredient from the blood; in the third place, only of such matters as these can it be said, that some of them occur but once in life. In infancy, in early age, and till puberty, there are certain waste materials which never afterward occur; the temporary cartilages have to waste away, the thymus gland has to decay, peculiar changes referable to the sexual system have to be accomplished, and the effete products of these changes have to be eliminated from the system. And fourthly, notice that the surfaces and organs most prone to affection in the diseases in consideration are those which are eliminative and defecating; those whose normal products can hardly be retained for any time within the body, much less out of it, without undergoing a foetid decomposition, which sufficiently stamps them with an excrementitious character. Bowels, skin, kidney, tonsils, are the favorite resorts of the several fever poisons, just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated. And it may not be amiss to notice that, whereas the normal and healthy discharge of these substances commonly tends to occur in the highest attainable form of oxidation; and whereas, under a variety of atmospheric circumstances interfering with their efficient oxidation, they must tend to accumulate in forms more susceptible of foetid decomposition; so it is peculiarly under such circumstances—where ventilation is defective—where human beings are unduly crowded—where the air is loaded with de-oxidizing influences—that zymotic diseases tend to affect the system either through a new generation of their poison, or through some vast increase of susceptibility thus engendered.

On inquiry, it might appear that the relations of infective material to these natural products are definite and constant; that one—let us, for instance, say syphilis—would stand in the particular relation to fibrin; it would be obvious that such an one would be of almost universal inoculativity, and could only for a very short time, if at all, exhaust the patient's susceptibility to re-infection; and that a drug having certain relations to fibrin (mercury, for example) would interfere with the affinities established by the disease. It might appear that another material, having its origin in the organic waste of nervous substance, would constitute the liability—say, to typhus; such an origin would almost fix the circumstances increasing our proneness to that disease, as well as prefigure the symptoms attending it. Of another material it might appear that it originates in the infantile decay of temporary cartilage, or of thymus, a decay occurring only once in life; that such material would constitute the susceptibility to measles or hooping-cough, a single attack of which commonly exhausts the patient's susceptibility for ever. Of a fourth material it might appear, that it arises in those changes of blood which attend the inflammatory and

reparative processes, under direct atmospheric influence (as in open wounds, cutaneous or mucous,) and that in such a product would consist the humoral liability to erysipelatous infection, and to puerperal fever. I need not multiply these hypothetical cases, but, before leaving them, let me beg you to understand, that I employ them only as *illustrations*; that I do not adduce them as pictures of what occurs, only as diagrams explanatory of my meaning.

That the specific materials of the several morbid poisons, as they now pass daily under our notice, constituting the principles of zymotic infection, are either actually derived from the blood, or might have been thus derived is quite a certainty. Whether each of them, in its first and original derivation was a native ingredient of the blood, identical with that on which we now see its influence exerted; and whether its first conversion into a specific *materies morbi* occurred without exterior infection, are points which cannot be decided with confidence. In respect to many infected diseases, however, this view of their having first of all arisen spontaneously, would seem consistent with analogy. Experience confirms this theoretical view; for we not unfrequently hear of an outbreak of small-pox or scarlatina, where no communication can be traced with a person previously infected; and we constantly have cases of typhus arising sporadically, where we may fairly consider the patient to have originated the disease within the limits of his own organization.

It is too much the custom to speak of the personal predisposition to infective disorders as though it consisted in a condition of mere debility. There is no foundation for this view. If we examine cases of pure debility—such as occur under the influence of extreme inanition, or after severe injuries with loss of blood, or toward the close of chronic exhaustive disorders, we do not find in them any marked liability to the infection of morbid poisons generally. In hospital practice, for instance, we do not find that typhus or erysipelas propagates itself among the patients of a ward in proportion to their weakness. It cannot be too distinctly understood that the predispositions to these various disorders are themselves as various as the disorders, and consist in specific conditions of blood, hitherto very imperfectly explored. Mere debility, as such, has nothing directly to do with the matter: a person is liable to the infection of small-pox, because he has one matter in his blood; to that of measles, because he has another; to that of typhus, because he has a third; to that of scarlatina, because he has a fourth, and so on. A predisposition to one of these disorders is by no means necessarily, and possibly not at all, a predisposition to any other of the class. As regards the exterior circumstances which are considered predisponent to infection, I apprehend there can be little question that their mode of operation must for the most part be indirect; that over-crowding and defective ventilation will increase the liability to small-pox or scarlatina only so far as they hinder a natural elimination from the blood of those materials in a state of imperfect oxidation favorable to the zymotic change. In examining the habitations of the poorest classes in our large cities, we find the atmosphere highly animalised—often fetid with organic matters: the air is so little changed, that it stinks with the volatile excretions of the many human beings crowded together; and to these contaminations are very generally added products of decomposition arising from their other secretions, which lie in cesspools, or has soaked into the

soil beneath and around their dwellings. Such an atmosphere can do little toward purifying the blood of matters wherewith itself is already so loaded; the effete matters of the organism, which naturally seek their elimination in an oxidised state, and which for the healthiest elimination ought probably to be in a high degree of oxidation, are here debarred from completing their discharge in its most normal form: and the inhabitants of such localities are consequently maintained artificially, replete with those humoral products which constitute the predisposition to zymotic blood diseases. It is in these localities, if at all, that such diseases originate *de novo*. I entertain no doubt that some of them do thus originate; though I am unable to state what it is which gives the requisite impetus, or why it is given with one disease oftener than with another. Typhus appears so frequently to arise in this manner, and the predisposition to it is so intimately associated with local circumstances, that some writers have been disposed to overlook the unquestionable evidence that exists of the reproduction and multiplication of the poison in the person of the sufferer, and have inclined to consider it an enchorial disease, incommunicable by personal intercourse.

With respect to other alledged predisponent causes, I incline still more strongly to the view already expressed in regard of atmospheric influences, that they can operate only indirectly, only by means of those specific blood products which constitutes the true predisposition in each case. If fatigue predisposes to a particular infection, it can hardly be for any other reason than that the fatigued organ furnishes the material permissive of infection. If errors or insufficiencies of diet, or certain courses of medicine should be found to form a predisposition to certain infectible disorders, their mode of operation could scarcely be otherwise, than by increasing the formation, or diminishing the discharge of that blood product, which is the immediate object of attack to the zymotic poison.

Therefore, as respects those instances of human morbid infection with which we are best acquainted, we may recapitulate our facts, and state our theory, in the following terms: that certain materials of the blood—materials not essential to the performance of its nutritive functions—are, by certain circumstances, rendered liable to undergo definite and specific changes; under the influence of which they become determined, with increased rapidity, to the outlets of the body, and irritate those outlets in their passage; that these changes continue, until the materials affected by them are completely exhausted from the blood; and that the severity and duration of those changes is in proportion to the quantity of material seeking elimination: that the new matters engendered are capable in various ways, and with more or less certainty, of producing a precisely similar succession of changes in the blood of another individual, or of any number of individuals: operating always on the same ingredients of the blood as that whence themselves arose, and determining it to the same outlets as that whither themselves were determined; so that the choice of material in the blood, and the choice of outlet in the body, constitute specific characters for the several morbid poisons distinctively, and so that the final products act always as special catalytics for that original material of the blood, wheresoever they may encounter it. But, finally, that under certain possible conditions of accumulation, or tension, in that original material, other circumstances may serve to start in its progress of specific decomposition, without any

demonstrable influence from that exterior catalytic, which is the ordinary occasion of its change.

From our foregone analysis of the pathology of morbid poisons, it is not difficult to deduce philosophical principles of treatment, or to devise a rational explanation of such success and such failure as medicine has hitherto encountered in this department of its ministrations. To check the further conversion of the material in the blood; to destroy the poison, or to turn it into harmless combinations; to aid or to anticipate the eliminative efforts of the disease; these would be the indications which pathology would suggest, and these have already, in great part, attained the sanction of experience. But both pathology and practice would concur in adding to these principles another, which, in our present age of palliative medicine, admits of almost infinite application; to remember (namely) that in each zymotic disease, nature is proceeding in her own way toward a curative termination, and that where (as too commonly happens) we are incompetent to conquer the disease by direct neutralizing antidotes, it behooves us chiefly to devote ourselves to the humbler task of moderating local phenomena, and sustaining constitutional power. Thus it is, that, in a vast number of perilous infections, we are able to assist nature through her difficult process of cure, by no other treatment than the judicious administration of natural dietetic tonics—food and wine. Thus it is, that while we recognise the absolute efficacy of mercury against the poison of primary syphilis, we constantly find ourselves without an antidote against its later combinations, and confidently rely on measures adopted, without reference to the specific nature of the disease, solely on the ground of their general invigorating power. Till you can neutralize the poison of typhus, of erysipelas, of scarlatina within the blood of your patients, as you would neutralise an acid or an alkali in a test-tube, never lose sight of this important principle; never forget that these morbid poisons are eminently depressive to life; that they tend to kill by shock and debilitation.

And finally, see what vaccination has done for one of them, perhaps, formerly the most malignant and unsparing. I have taken pains to explain to you the pathology of its preventive power, and very little reflection on the argument of this lecture will convince you that there is, in the nature of things, no reason why small-pox alone should be frustrated in its tendency—no reason why each zymotic disease should not have its own preventive catalytic—no reason why, in connection with these other pestilences, other men's names should not hereafter be remembered as gratefully and as gloriously as Jenner's in relation to small-pox. Our resources for this great purpose of preventive medicine are not restricted to the teats of cattle. We have the pharmacopœia before us, many of its articles acting catalytically on the blood, and determining products of decomposition, in a characteristic way, to a specific plurality of organs. Not only is there no reason against the possibility that many of these medicinal catalyses may be preventive of the zymotic catalyses; but there is every reason for such a possibility. To give you an illustration, why should not belladonna (determining the products of its operation to the throat, the kidneys, and the skin) act as a medicinal catalytic of that material which constitutes the susceptibility to scarlatina, and thus, in recognised reality (as heretofore in vague tradition) be preventive of that disease? Again, why should not the direct counteractive influence of drugs be extended in respect of these

diseases, when they already are in attack? Why should we not be enabled by one drug to arrest the blood-change of typhus, and by another that of plague or glanders, just as with quinine we render the blood insusceptible of further detriment from the malarious poison.—*Lancet*, August 24, 1850.

Respiration and Air.

The following remarks on the function of respiration in its relations to ventilation, etc., we find in an able article on *Cholera in its relations to sanitary measures*, contained in the *Medico-Chirurgical Review* (London, Eng.) No. for Jan., 1851. The importance of this subject is not always sufficiently appreciated by the Physician, and it is generally overlooked by the Public. We would commend the facts and considerations presented in the extract to the attention of medical readers, who must consider themselves in some measure responsible for the violation of hygienic conditions by that portion of the community who cannot be expected to investigate the laws of health, and who therefore are dependent on the influence to be exerted by those whose pursuits embrace prophylaxis, as well as therapeutics.—EDITOR BUFFALO MEDICAL JOURNAL

Of all the circumstances affecting health, none is so important as the condition of the air that we breathe. On it, more than upon the food we eat, depends the purity of the blood and the right exercise of every function of the body. Hence the especial care manifested for the maintenance of the respiratory process in all animals, alike by the admirable structure of the organs which subserve this purpose, and the beautiful arrangements and adaptations of the media in which they live, and the circumstances in which they are placed. In a state of nature, all goes on healthily and well. The same air is not breathed a second time until it has been rendered pure as before; for whenever the air has been brought into close proximity with the blood of a living animal, it immediately experiences a change which disqualifies it for healthy respiration. One-fifth of the oxygen which it contained—we speak of the function in the human being—disappears, and its place is occupied with a nearly equal volume of carbonic acid. How, then, can the same portion of air be inhaled again without detriment? A determinate amount of oxygen is required, at each act of breathing, to enable the system to get rid of the carbonic acid, which requires to be discharged every moment, and which, if not eliminated as nature intended, acts as a slow poison to the blood. If we take the ordinary computation of 20 cubic inches of air being drawn into the lungs at each inspiration, and of there being about 20 respirations in a minute, then 333 cubic feet, or 33 hogsheads, are made use of by each person per diem. Not less than between 10 and 12 cubic feet of carbonic acid are therefore evolved in the course of the twenty-four hours, a quantity which will be found to contain at least six ounces of solid carbon! The consideration of this fact alone must suffice to show the extreme importance of there being no impediment

to the due exercise of the respiratory function, and prepares us at once for the pernicious results that inevitably attend upon the breathing of an atmosphere which does not enable the system to remove its self-generated poison. The retention or the inadequate discharge of the carbonic acid,—besides the more direct effects of oppression, loss of appetite, and greatly impaired muscular energy,—renders the body more prone to every form of diseased action, and far less capable of resisting the influence of baneful agencies from without.

But the chemical change in its composition is not the only alteration which respired air has undergone. Besides the large amount of carbonic acid, it has become charged with a watery vapor, which of course renders the atmosphere of unventilated chambers unduly moist—a condition to which we shall afterward have occasion to allude, as one of the causes favoring the development of epidemic disease. The quantity of fluid exhaled from the lungs, under circumstances of health, is believed to be at least from 16 to 20 ounces in the course of the four-and-twenty hours. Of course, if the process be imperfectly performed, as it must be liable to be in an atmosphere already loaded with moisture, nature must seek an outlet for the excretion at some other channel. And let it not be supposed that it is mere aqueous vapor that is thus discharged: there is effluvial animal matter mixed with or dissolved in it,—a matter which, upon being condensed, is found to be of a highly putrescent and rapidly decomposable nature. It is in fact one form of the waste, and therefore excrementitious, products of the system, which are being continually eliminated by the various emunctories of the lungs, skin, bowels, and kidneys; each of these organs having its special task to perform in the great process of excretion and purification, so indispensable to the maintenance of healthy life. That the atmosphere of ill-ventilated rooms, where a number of persons have been congregated for some time, is tainted with this animal effluvia, is obvious from the peculiar sickening smell perceived on entering a crowded dormitory in the morning before the doors and windows have been opened, or even a large school room after children have been in it for several hours; unless, indeed, there has been—as ought always to be—a thorough and continual circulation of pure air. Medical men and others, after visiting the wretched abodes of the poor, often retain for several days in their clothes, the offensive smell of such localities:—how, then, must the bedding and furniture of such apartments, as well as the garments of the inmates be impregnated with it! It clings to the very floors and walls of the rooms; and these, moreover, from the vicious construction of the windows—being seldom or ever made to open from the top—can scarcely be kept quite sweet, even under the favorable circumstances of non-crowding and cleanliness. The pollution of the atmosphere from the cause now mentioned, viz., the existence of a noxious organic matter in it, is probably a more influential cause in generating certain forms of disease, and in powerfully predisposing to the invasion of epidemic disorders, than even the vitiation of it from an excess of carbonic acid. It is necessary, however, to keep both in view, as well as the increased amount of aqueous vapor, in estimating the baneful effects of deficient ventilation in the dwellings of the poor. But the lungs are absorbing as well as excreting organs; they are as ready to take in, as they are active in giving out. The blood is permeating every part of these with such rapidity, that the entire mass of it is believed to

circulate through them, and to be consequently exposed to the air contained in their minutely divided cells, in little more than three minutes. How favorable, then, is such an arrangement, coupled too with the exquisitely delicate fabric of the pulmonic tissue, to the imbibition or absorption of all volatile matters in the respired air, and their immediate transmission to every part of the body! There can not surely be a reasonable doubt, that not only are the special morbid poisons generally received into the system in this way, but that also the gaseous products of animal and vegetable decomposition, and the effluvia given off from living bodies, are apt to be drawn into the circulation by the lungs, and serve to vitiate the blood—more or less rapidly and powerfully, according to age, vigor of constitution, and other accessory causes—and to deteriorate the health. The general effect of the absorption of such matters is to lower, in a very marked degree, the physical energies of the system, rendering it at the same time much more irritable and less enduring of fatigue; while the more special result is to induce a tendency to diarrhoea and other disorders of the bowels, a proneness to fever and inflammations of a typhoid type, and, as might be expected, a peculiar liability to all epidemic disorders.

It is, of course, an important point to determine what amount of space is required, under ordinary circumstances, for the healthy respiration of one person. Now, it has been the result of experience, the conclusion to which the most competent observers have come,—that unless extraordinary means are taken for the constant removal of the air by some special apparatus for ventilation, so that the impure air is carried off as quickly as it is exhaled from the lungs, health and strength cannot be maintained, in a space of less than from 700 to 800 cubic feet; and that to live and sleep in a space of less than from 400 to 500 cubic feet for each individual, is not compatible with safety to life, even when there is no extrinsic or superadded cause of atmospheric impurity. And let it not be supposed that even the first-named spaces would be sufficient in a hermetically-closed box or chamber; for life would become extinct long before all the oxygen had been consumed. Air containing only ten per cent. of carbonic acid is incapable of supporting life.—*British and Foreign Medico-Chirurgical Review.*

Application of cold as an anæsthetic agent in operations for removing warty excrescences. By Tomas W. Nunn, Esq., Surgeon to the Western Dispensary.—[Mr. Nunn says, that having taken advantage of the hint given by Dr. Arnott, he has been so satisfied with the result that he considers it due to that gentleman to publish the following case. It occurred in a young married woman, who applied to the Dispensary for the purpose of being relieved of a large accumulation of warty growths about the pudenda.]

The excrescences depended from the whole of the labia minora, and surrounded the clitoris so completely, that it was difficult to distinguish the meatus urinarius. Some of them were of considerable size—as large as a common fig—others were oblong, and were attached by a narrow pedicle. A great many small ones surrounded the orifice of the vagina. She also suffered from leucorrhœal discharge. No other symptom of importance

appeared. The excrescences were the cause of a great deal of suffering and inconvenience in a variety of ways.

Assisted by my friend Mr. Weston, I applied little wedge-shaped pieces of ice to the necks of the larger growths, till they became perfectly *blanched* and cold, and with a single stroke of a curved probe-pointed bistoury, removed several of the larger ones successively, without causing the patient any but slight pain.

I afterward removed a very small growth without first applying ice. The result was, as might have been anticipated, the patient found the pain insupportable.

I found it necessary to introduce a bougie into the urethra, in order to indicate the position of the external orifice of that canal, so buried was it amongst the vegetations.

A great advantage obtained by the use of the ice, was the absence of hemorrhage; it being hardly requisite to apply a sponge during the operation. I was thereby enabled to proceed without hindrance, and a clear view was obtained of the exact extent of each sweep of the bistoury.

No reaction in the least degree excessive followed the proceeding, the progress of the case being satisfactory.

It appears to me, that by the above simple plan we may often save our patients considerable suffering, without exposing them to the least extra risk, which can not be said of chloroform. It has been proved, on the one hand, over and over again, that if anæsthesia be not *complete*, there is the contingency of undesirable phenomena; while, on the other hand, if the anæsthesia be perfect, a greater chance of accident is incurred.—*Lancet*, August 31, 1850, p. 262. *From Braithwaite's Retrospect.*

Pruitus of the Vulva in Children. By M. Vallez.—It is not very uncommon to find young children complaining of distressing itching of the vulva and anus. This forces them to rub these parts, which leads sometimes to violent irritation in them. The affection is serious in consequence of its occasionally inducing bad habits which may continue to be practiced after the original cause of the itching has disappeared; namely the presence in the genital regions of small ascarides, designated by Rudolphi by the name of *oxyures*.

Of this affection M. Vallez has observed two cases. In one, after having unsuccessfully used a local treatment for some days, he carefully examined the parts, and was astonished to find in the fossa navicularis, and around the fourchette, a quantity of small worms which, by their motions, produced the irritation. He immediately prescribed hip baths, each containing in solution half a pound of saltpetre. After the third bath, the child was quite cured.

In the other case, the patient, a young girl, had for two years endured great suffering from the irritation of the vulva. A great variety of means had been resorted to in order to relieve her, but with no effect. On examination, M. Vallez detected the presence of the *oxyures*. Two of the saltpetre hip baths effected a cure.—*Bulletin General de Therapeutique.*

On the use of Phosphorus in Diseases of the Skin.—There is a remedy which exercises a powerful influence on the cutaneous exhalents, and which I can specially recommend in the treatment of the more rebellious forms of diseases of the skin—namely, phosphorus. My attention was first directed to this remedial agent during the period of the cholera in 1832, and I found it successful, when every other remedy had failed, in several cases of that disease, where the vital powers seemed exhausted, and the patient in the lowest stage of collapse. In these cases, it appeared to act as a violent stimulant, principally through the nervous system, accelerating the circulation, and exalting the muscular irritability in the highest degree. I can now recommend it as one of the most valuable medicinal agents we possess in those inveterate cutaneous diseases—leprosy, psoriasis, lupus—in which the skin seems to adapt itself to the morbid condition, which it retains with singular tenacity against all the usual methods of treatment.

The phosphorus treatment of these maladies may be internal or external. The best method of administering the remedy internally is, dissolved into oil or ether, and the phosphorated oil or ether then mixed up with powdered gum arabic and mint water. Camphorated lard is the most appropriate vehicle for applying phosphorus externally. Its energetic revulsive properties may likewise be turned to account in certain diseased conditions of the skin. Phosphorus, the iodide of arsenic, cantharides, and the binioidide of mercury, are the most powerful internal remedies for skin diseases we possess.—*Dr. Burgess in monthly Journal.*

New method of arresting Uterine Hemorrhage. By M. Diday, of Lyons. By a similar dilatable caoutchouc tube as that recommended by M. Gariel for epistaxis, M. Diday succeeded in arresting the uterine discharge when all other means had failed.

The apparatus used consisted of a long tube with a little bladder at the extremity, which was introduced into the vagina, conducted by the index finger, and thus maintained in place whilst insufflation was practised at the other extremity of the tube. The bladder was thus increased in dimensions until it acquired a diameter of about twelve inches, the air being retained by means of a ligature on the free portion of the tube. The hemorrhage was instantaneously arrested; the patient regained strength; and after two days a small quantity of the air was allowed to escape, when no return of the hemorrhage occurring, the entire tube was withdrawn on the day following.—*Ibid, Aug., 1850, p. 129.*

Common Salt in Intermittents.—Prof. Piorry, in reporting to the “Academie de Medicine” upon the proposed use of table salt (chloride of sodium) in intermittent fevers, states that if administered in doses of two table-spoonfuls, it will not only arrest the disease, but also exert upon the spleen as marked an effect as quinine does. In twelve cases of intermittent fever, the salt uniformly arrested the paroxysms and lessened very materially the size of the spleen. The spleen was also found to diminish when the remedy was given in cases of typhoid fever.—*Southern Med. and Surg. Journal.*

EDITORIAL DEPARTMENT.

Sanitary Institutions.

The progress of modern Medicine is perhaps nowhere better shown than in what may be termed the Sanitary feature of the science. By this expression we mean to refer, not alone to public and private hygiene, but to the class of measures involved in the management of diseases which may be called Sanitary, in distinction from those which are medicinal in their character. A nicer discrimination in the cases which call for the active interference of medical aid; a greater reliance on the conservative and restorative energies of the organism; and, in proportion to an increased circumspection in the use of medicinal agents, a juster appreciation of sanitary treatment, may be said to characterize modern therapeutics, as exemplified in the practical views of the best practitioners of the present time.

As one of the results of the present tone and tendency of practical medicine, we hope to see institutions established, under the auspices of the Profession, in which the sanitary, as well as medicinal treatment of chronic affections may be more systematically and faithfully conducted than is, in many cases practicable, in private practice. We have, for some time, entertained the belief, that the absence of sanitary retreats, or rather their relinquishment into the hands of empirics, is a misfortune which ought not to exist. Such institutions, placed under the supervision of well educated, experienced, judicious Physicians, would do a great deal of good, and by providing for an obvious necessity for them, would protect many persons from the delusions of Quackery. There is a certain class of patients requiring the hygienic discipline of Institutions of the description referred to, where the benefits of withdrawal from the cares and excitements of business; the moral influences of novelty, recreation, and change of scene; together with properly regulated habits of diet and regimen, and such special sanitary measures as may be indicated, are conjointly brought to bear on the recovery of health. We need not stop to describe the class of patients to whom we allude, for there is not a medical practitioner actively engaged in the duties of his profession, who cannot, at once, recall instances to be met with in his daily walks. This class of patients, as every physician's

experience tells him, cannot be cured by drugs alone. Different remedies are prescribed in succession, until the physician and patient become tired, and often separate in mutual disgust. It is by this class that patented remedies are consumed, and empirics of all kinds are, in a great measure, supported. As a general remark, the difficulty, in such cases, in the way of cure, consists in certain physical, mental, or moral obstacles that cannot be removed, except the patient be placed under circumstances in which the influences acting in these three directions, severally and collectively, can be effectually controlled; and this can only be done at well regulated sanitary retreats.

Hitherto such institutions have been mostly in the hands of irregular practitioners, and are conducted with reference to some exclusive dogma. Hydropathy is the prevailing basis at this time, and nearly all the public institutions for the restoration of health are *water-cures*, generally under the management of Homœopaths, Thompsonians and Eclectics, or uneducated persons who do not belong to any medical sect, but are a 'law to themselves.' The fact that these establishments are crowded with inmates shows the existence of a want which it is the duty, not less than the true policy of the profession to supply. If there were proper institutions placed under the supervision of Physicians, and conducted in a manner to meet the approval and encouragement of the Profession, it is probable that they would be preferred by a large majority of the patients who now flock to those which cannot, and should not be recognized as within the pale of legitimate practice. Hence, a much greater amount of good would be effected, in the first place, by giving better advantages to the individuals for whose benefit sanitary institutions are designed, and, in the second place, by providing against the encroachments of Quackery. So long as the Profession continue to relinquish this field of practical medicine, it will be occupied by those who now have possession of it, and who are shrewd enough to perceive and profit by its fruits. The Profession in this, as in some other matters, has been neglectful of its own interests, and allowed *squatters* to remain unmolested, until they are emboldened to prefer a pre-emption claim to valuable territory belonging rightfully to legitimate medicine.

That sanitary institutions would give efficiency to the management of a large number of chronic cases which are now the opprobria of the profession, may be deduced from the fact that beneficial results are attained at the establishments, professedly of that character, which now exist. We freely admit this to be a fact, while it is not less certain, that, owing to errors and defects arising from ignorance, want of judgment and discrimination, and the adoption of exclusive and erroneous doctrines of therapeutics, much harm is done. The failures, and instances of positive injury, probably preponderate greatly over the aggregate of cases in which cures are effected, or real benefit is experienced. But let sanitary retreats be instituted in which, as in our insane asylums, and hospitals, patients can enjoy the advantages of medical knowledge, experience and skill, and while the amount of good would be immeasurably enhanced, there would be no counterbalancing evils.

We have penned these few remarks in order to invite the attention of our readers to a subject which seems to us to possess considerable importance, and to which we may recur at some future time.

Memoir of Amos Twitchell, M. D., with an appendix, containing his addresses, etc. By H. J. BOWDITCH, M. D. Boston: Printed by John Wilson & Son, 21 School street. 1851, 12 mo., pp. 212.

The subject of this memoir, the late Dr. Twitchell, of Keene, N. Hampshire, as the majority of our readers need not be informed, was one of the most distinguished surgeons in New England, and, indeed, in the United States. Within the circuit of his practice, he was, as Dr. Bowditch styles him, the "autocrat of surgery," and this position he held for nearly half a century. More than this, he was a good citizen, a benevolent, estimable, and honest man. It is fitting that the memory of such men should not be impaired for the want of a worthy biographer, yet, how often is this the case! Perhaps it is oftener true in the medical profession than in any other! We could mention many names of eminent physicians and surgeons, whose lives would afford examples that should not be lost, and the record of whose virtues should be preserved in honor of the profession which, when living, they adorned, of whom there remain nothing save the recollection of their talents and worth, the grateful sentiments of surviving friends, and the abiding influences which they exerted in the communities in which they lived. Valuable as are these testimonials of an honorable and useful career, justice, alike to the pre-eminent excellence of the departed, and to the aspirations of those who would follow in their footsteps, claims more durable memorials. Faithful biographies of medical men distinguished for their abilities, and higher traits of character, would do much to foster among members of the profession an elevated spirit, and inspire persons of other pursuits with respect for a calling which, in the number of those by whose lives it has been ennobled, may challenge comparison with any other.

The memoir of Dr. Twitchell by Dr. Bowditch, leaves upon the mind of the reader impressions of respect for the biographer, as well as for the memory of the deceased. Next to the wish that men so justly distinguished as the late eminent surgeon of New Hampshire, might abound, we would hope that to record their virtues might be entrusted to hands as competent to do them justice as in this instance. Dr. Bowditch remarks, "to appreciate justly, and portray clearly the lights and shadows of any character, one must, to a greater or less extent, live over in his individual consciousness, the life of him whom he would delineate; and, the more the writer loses himself in the subject, the more perfectly he will succeed." In executing the duty assigned to him, the author has exemplified the correctness of this remark. Profoundly imbued with admiration of the talents and character of Dr. Twitchell, whose intimacy he had enjoyed, he has given us a narrative which in simplicity, fervor, and candor, may serve as a model for similar productions.

Marshall Hall, and the Spinal Nervous System.

Marshall Hall, having been appointed to deliver the Croonian Lectures, for 1850, at the Royal College of Physicians, London, embraced that occasion to present a synopsis of his views of the spinal nervous system. The Lectures, four in number, are contained in the London Lancet, and have been issued in England as a distinct treatise. The reader will find a

concise summary in the No. of Braithwaite's Retrospect which has recently appeared—part the twenty-second.

Dr. Hall, in the lectures and treatise just referred to, introduces a new nomenclature to express the different portions of that department of the nervous system, the elucidation of which is due, in a great measure, to his persevering researches. He proposes that what has hitherto been known as the "true spinal system," or the "reflex system," shall hereafter be designated the *Diastaltic nervous system*. He uses the term *diastaltic* as the correlative of *peristaltic*, the latter being applied to movements of internal viscera, which are supposed to be independent of the spinal marrow. We are unable, we confess, to appreciate any marked pertinency in this term as thus applied. The movements which are called *peristaltic* are those of the intestines. The contraction of the heart is not known as such; and the term *diastole* has long been applied to express a movement of the heart, which, agreeably to Dr. Hall's views, is *peristaltic*. Moreover, it is not certain that future investigations will not show closer relations of peristaltic movements with the spinal cord, than are now thought to exist.

The nerves conveying impressions to the true spinal, or diastaltic centres, he proposes to call *esodic*. Hitherto they have been usually termed the *incident* nerves. The nerves conveying a motor influence from the spinal centres, commonly called the reflex motor nerves, he designates *exodic*. *Anodic* is used to express an ascending, and *cathodic* a descending course of action. *Pallodic*, and *Panthodic*, are to denote the "action of the *vis nervosa* from each *one point* of the diastaltic system, in *many*, or even *all* directions to *every other*."

In our humble opinion this effort of Dr. Hall to substitute new terms for those formerly originated by himself, and which are now in general use, is unfortunate. He could hardly have suggested any thing better calculated to retard the progress of the views which he claims as discoveries, and with which, even now, a pretty large proportion of the profession are but indifferently acquainted. It is annoying, after having associated certain ideas with names, to be asked to throw the latter aside, and learn a new vocabulary. Some will make the change, but many will not, and thence will come not a little confusion. The nomenclature of medicine is susceptible of very great improvement, but advancement must be gradually made, or the evils produced will exceed those arising from the use of ill-selected, inappropriate terms. Dr. Latham has done not a little harm to the study of auscultation by choosing to use some of the terms proper to that subject in a sense quite different from other writers. It is conceded that when a person employs a word of loose signification, or that has different meanings, he may define it in the sense he pleases, and that a discoverer of new scientific truths is authorized to christen his offsprings as he sees fit; but to undertake to alter a whole series of technical phrases which have become settled by conventional usage, is quite a different matter.

While in a mood for criticism, we will add, that it is painful to see one who has really accomplished so much for medical science, as has Marshall Hall, display such an overweening sense of the importance of his own researches, and such excessive anxiety lest some other distinguished co-laborer should receive a fractional part of the credit to which he thinks himself exclusively entitled. We must confess we are unable to see the precise force belonging to the fact that he has devoted to the subject of

the nervous system 25,000 hours, save that it may serve to explain an apparent unconsciousness of aught in medical science save the true spinal, we beg pardon, the *diastaltic* nervous system. While we shall be glad to see the physiological discoveries of Marshall Hall better and more generally known, inasmuch as they are highly important in their pathological relations, we hope the new terms he has coined will never gain admission into our medical dictionaries.

New York Register of Medicine and Pharmacy, Edited by C. D. Griswold, M. D.

This is a semi-monthly publication, and an active competitor of the Gazette, with which it does not appear to sustain the most affectionate of relations. By this remark we are reminded of a fact, which is perhaps worthy of note, viz., that since we have been a member of the *corps editoriale*, a period of nearly six years, during which we have received all the medical periodicals in this country, scarcely in a single instance that we recollect have there been any evidences of other than a courteous and kindly bearing on the part of any Editor toward his *confreres*. Discussions have uniformly been conducted temperately, and without violation not only of the rules that should govern intercourse with gentlemen, but with friends. We take it that, whatever may be thought of the imputation of quarrelsomeness upon the medical profession, which has long been a proverb, the fact just cited suffices to prove that medical editors, at all events, are not very pugnacious as respects each other. The reader may think there is some egotism in this train of remark, inasmuch as we are entitled to a fractional part of whatever credit may belong to the editorial fraternity on this score. We are willing to incur this criticism so long as we are unconscious of any occasion for the reproach that what we have stated is not entirely true in its application to ourselves. But to return from this episode, to the N. Y. Register. A new and attractive feature has recently been added to this periodical, viz., the introduction of biographical sketches of distinguished members of the medical profession, accompanied by their portraits. Dr. John W. Francis heads the list, and is the only one whose life and likeness have as yet appeared. A series, however, is promised, which will doubtless be very acceptable to the patrons of the Journal.

New York Medical Gazette.—We have omitted to state, that the N. Y. Medical Gazette, which was established as a weekly Journal, has been, for some time, issued bi-weekly. The efforts of Dr. Reese, the Editor and founder, to adapt it both to the professional and popular reader, appear to be successful. He wields a strong, as well as a ready pen, of which the abettors of Quackery and Humbugs must be aware, if they ever look into his pages, which they cannot do without discovering, as in a glass, what manner of men they are of. We hope his valuable services in behalf of the profession are adequately appreciated by a large and increasing list of subscribers. Words of compliment and encouragement are very well, but the demand for a larger edition tells much more forcibly on editors and proprietors. We are happy to say that we speak from experience on this point.

St. Louis Probe.—The *St. Louis Probe* has been discontinued, for reasons which the reader will discover in the following valedictory. We publish it for the benefit of a few of our readers:

"*The St. Louis Probe.*—The present number closes the first volume, and ends the publication of, the *Probe*. During a year's experience in journalism, we have been convinced that neither fame nor funds can be acquired by conducting a medical monthly, and that many members of the medical profession are miserably poor in pocket, and more are deficient in moral principle, however well they may be imbued with the principles of their profession. We are inclined to believe that a large number, who have received our journal without paying for it, have devoted themselves to the study of scurbitus, with some success; for we must say they have treated us most scurvily, and not a few have shown a thorough acquaintance—not with abstract principles—but with the principles of abstraction, which would entitle them to the consideration of the judiciary. For the kind favors, and warm support we have received, however, from the better portion of our brethren, we return our hearty thanks, and thus take leave of them. Our hearts are so very full, and our pockets so very empty, that we are unable to say more."

Ohio Medical and Surgical Journal.—Dr. S. Hanbury Smith has retired from the Editorship of this Journal, and is succeeded by Prof. Richard L. Howard, Prof. of Surgery in the Sterling Medical College. The new Editor announces in his last issue his intention of passing a few months in Europe during the coming summer.

We extend our cordial congratulations both to the late, and present Editor.

The foregoing notice was excluded from our last issue. In the mean time we have had the pleasure of greeting Prof. Howard in Buffalo, on his route to Europe, where he proposes to pass several months. During his absence the Ohio Journal will be under the management of its former Editor, Dr. S. Hanbury Smith.

New Jersey Reporter.—This Journal has been changed from a bi-monthly to a monthly publication, and presents a new and improved typographical appearance. We tender to the able Editor our congratulations on the success of his undertaking.

Semi-Annual Meeting of the N. Y. State Medical Society at Buffalo.—In our previous notice of the semi-annual meeting of the State Society we inadvertently made an error as respects the date of the meeting. It will be on the *second* Tuesday in June, not on the *first* Tuesday as was erroneously announced in our last number. Our readers will please bear in mind this correction.

It is to be hoped that the County Societies in Western New York will take measures to elect delegates, if they are not already elected, and that delegates will be sure to attend. This new movement on the part of the Society should be responded to by a full meeting.

Rochester Knockings.—Mr. C. Chauncey Burr, of Philadelphia, who has been lecturing lately in New York, and several places, on the *rapping*, and other tricks of the various spiritual "*mediums*" throughout the country, has just concluded a course of four lectures in Buffalo, in which the whole imposition was not only satisfactorily explained, but successfully illustrated. As respects the *Rochester knockings*, Mr. B. has a disclosure just made by a sister-in-law of the *Foxes*, in which she states that the rappings are produced by the *toes, knees, and ankles*. Margaretta Fox is the only one of the family who can rap with the knees. This is the younger of the two sisters who visited Buffalo. By reference to the exposition in the March number of this Journal, it will be perceived that this one was said by us to be the rapper, and that the sounds were supposed by us to come from the knees.

The lady who makes the disclosure has been induced to do so under religious convictions. She has herself acted as a *medium*, and an accomplice of the Fox girls in carrying on the imposition. This document, which is to be made public, will, of course, settle the matter as to the correctness of the physiological explanation made in the exposition signed by Professors Lee and Coventry, and the Editor of this Journal. Whether it will satisfy the dupes, and put a stop to the progress of *spiritual rappings* remains to be seen.

The other tricks, such as ringing bells, moving tables, etc., are so simple and shallow, as feats of *legerdemain*, we beg pardon, *slight of feet*, that it is almost inconceivable they could deceive any sensible persons. The whole imposition affords an instructive, but mortifying exemplification of the extent of human credulity, in an age which claims to be so far in the advance of the days of witchcraft and superstition.

Abortive Treatment of Small Pox by the Application of Collodion.—In the number of this Journal for August, 1850, Dr. S. B. Brinkerhoff, of Linesville, Pa., communicated the results of the application of collodion as an Ectrotic method of treatment in Small Pox. The idea was original with him, and the facts, as detailed in the two cases in which he had employed the measure, seemed to show that the suggestion would prove one of considerable value.

A French practitioner, M. Aran, in the *Bulletin General de Therapeutique*, for October, 1850, communicates a case in which the same practice was successfully pursued. The plan was probably original with M. Aran, but it will be noticed that the priority is due to our correspondent. Dr. Brinkerhoff's brief article was copied into some of the medical Journals of this country, but the communication of M. Aran, bids fair to have a more general circulation. We have already noticed the latter in some of our contemporaries in which the former has not yet appeared.

We are at this time much interested in watching the effect of the application of collodion in two cases of small pox. The influence upon the development of the pustules is decided and considerable, but we are not prepared to say to what extent the advantages desired will be secured. We notice the subject at this time, to claim whatever credit may belong to the suggestion, for Dr. Brinkerhoff, who is a promising young physician, a graduate of the University of Buffalo.



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