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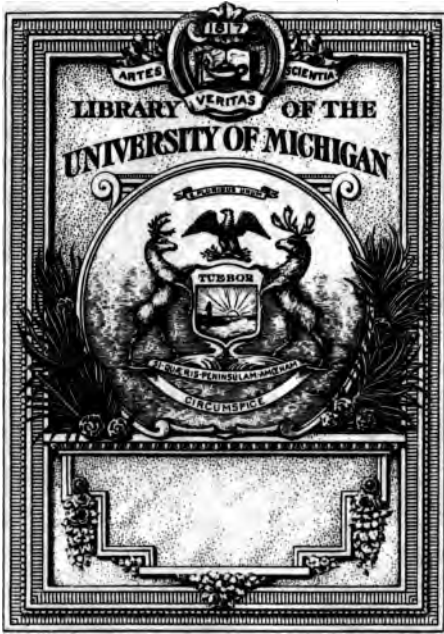
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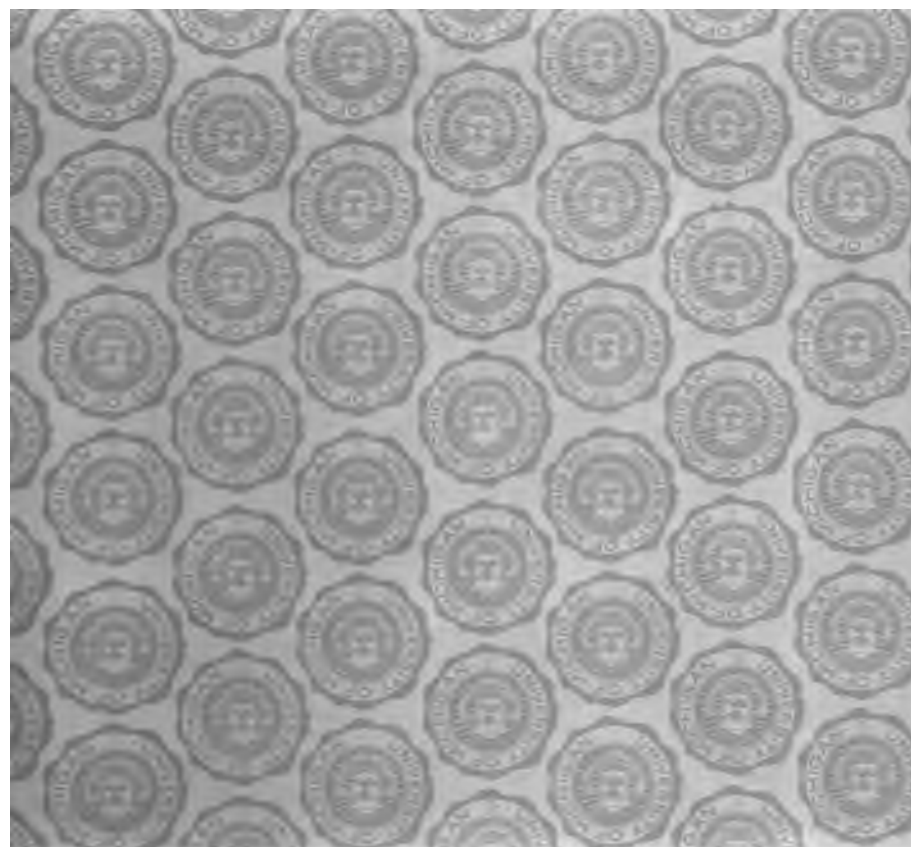
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Nitric acid communicates to it a topaz color, which does not immediately change by the action of heat. Tannic acid throws down a white precipitate.

It behaves with many of the reagents, like ammonia; thus the bichloride of mercury, and chloride of zinc cause a white precipitate; the gelatinous oxide of zinc is soluble in an excess of conicine; the chloride of platinum furnishes a yellow precipitate, soluble in water; the acetate of copper gives a blue precipitate, gelatinous, less soluble in an excess of conicine. The acetate and sub-acetate of lead do not precipitate it. It furnishes with the protochloride of palladium a chocolate-colored precipitate soluble in an excess of conicine.

Conicine, composed of carbon, hydrogen and nitrogen, may be represented by a compound of one equivalent of ammonia, three hydrogen, one nitrogen, and one of carbonated hydrogen, containing twelve equivalents of hydrogen, and nitrogen of carbon, ($H^{12}. C^1.$)

Conicine may be obtained by the following process:—Heat in a distilling apparatus, eighteen ounces (500 grammes) of the seed of hemlock, bruised and well mixed with about $\frac{3}{4}$ is. caustic potash, and three pints of water. The distilled liquid, containing conicine, ammonia, etc., is saturated with sulphuric acid, and evaporated to the consistence of a soft extract. It is then agitated in a mixture of alcohol and ether, which dissolves the sulphate of conicine, leaving the sulphate of ammonia, etc. The sulphate of conicine is decomposed by potash; the conicine floats upon the surface; it is then decanted and left to rest for a while on chloride of lime, to take up the water when it is distilled off.

ACTION OF CONICINE ON THE ANIMAL ECONOMY.

Experiments.—1. I administered to a dog of moderate size, twelve drops of conicine, recently prepared by the process above described. The animal immediately ran about the laboratory, and did not seem incommoded; at the end of a minute, it manifested slight vertigo and feebleness in the hind legs, although he continued to walk; *three minutes* after swallowing the conicine, he fell upon his right side, as if dying; immediately after, he had some slight convulsive movements in the extremities, without opisthotonos; then, the convulsions having ceased, the animal fell down immovable, and very languid, and died *five minutes* after the poisoning.

On opening the body, the digestive canal, the liver, the spleen, the kidneys, the lungs and the heart, offered no alteration, worthy of notice. The blood was partially coagulated. The tongue was pale over its whole extent *the epithelium* was easily detached on the parts, which had been touched '

the alkali. The fauces, the nasal fossæ, and the trachea, contained a notable quantity of sanguinolent mucus.

2. I gave to another dog a double quantity of the same conicine. The animal died at the end of *two minutes*, after having exhibited the same symptoms as in the preceding case, with this difference, that the vertigo did not last over half a minute, and the slight convulsive movements, were not immediately manifested after the cessation of the vertigo, and the dog fell upon his *left* side. He had neither vomitings nor stools, and the animal made no cries. On opening the body, the organs and the blood were found in the same state, as in the preceding case.

It is difficult to reconcile these facts with those that have been published by Dr. Christison regarding conicine. According to this author, this alkali is a poison of most extraordinary activity, scarcely less so than hydrocyanic acid. In fact, two drops, applied to a wound or to the eye of a dog, a rabbit, or a cat, caused death in less than ninety seconds; and the same quantity, in the form of the hydrochloric, injected into the femoral vein of a cat, often produced death in three seconds, or a little over. Its activity is much increased when combined with acids, especially the hydrochloric. It does not cause coma, whether administered in a free state, or in the form of a salt. It does not in any manner affect the heart. It possesses a local irritant, and its subsequent effects consist entirely in producing paralysis, which is promptly manifested in the muscular system, and which must always destroy life, by paralyzing the muscles of respiration.

Evidently, the conicine employed by Dr. Christison, was not in the same state of purity and concentration, as that with which I experimented.

I desired to ascertain, whether I could obtain, by a different process, a more active conicine; I therefore digested 18℥ of the seeds of the same hemlock in weak sulphuric acid, and after several hours of contact, I filtered it. The blackish liquor was made alkaline by an excess of potash, when it immediately gave off a strong smell of conicine; I then distilled it in a large retort in a sand-bath; the alkaline liquor in the receiver, strongly alkaline, was placed in contact with chloride of lime for twelve hours, when it was distilled upon the chloride, separating the products, so as to first volatilize the ammonia, lest it might change the conicine.

The alkali obtained by this process, was much more active than the former; in fact, a dose of ten drops killed in two minutes, a very large sized and strong dog. The symptoms exhibited by the animal were the same as I have already described in the former cases.

Every thing goes to justify the belief, that still more active conicine may

be obtained by saturating with pure sulphuric acid, the liquid condensed in the receiver, evaporating the liquor to one tenth of its volume, decomposing it by potash, dissolving the conicine which is separated, in ether, decanting, evaporating in the open air, in a current of hydrogen gas, and distilling the conicine obtained, on chloride of lime.

MEDICO-LEGAL RESEARCHES.

Experiments. — I made artificial mixtures of broth, albumen, and wine, or of meat, well hashed, currant jelly, tea and coffee. I added several drops of conicine to each of these mixtures. On the other hand I examined separately, the tongue, the fauces, the stomach and matters it contained, the liver, the spleen, the kidneys, the lungs, and the blood of the dogs I had destroyed with conicine; and *I easily detected this alkali*, both in the artificial mixtures, of which I have spoken, or, the tongue, stomach and its contents, the *spleen, kidneys and lungs*. The liver gave but a trace, and it was impossible to extract from it a single trace of blood, as M. Stas had previously observed with regard to nicotine. I noticed, also, that the lungs furnished a much larger quantity of conicine than the liver. However that may be, it results from these researches, 1st. That conicine is absorbed. 2d. That it may be detected in organs, to which it is carried after absorption. Every thing goes to show, also, that it may be easily detected after prolonged inhumation, as is the case with *nicotine*.

By the following processes, conicine may be extracted from the different organs and alimentary mixtures, whatever they may be; and they are precisely the same as those which I have recommended in searching for nicotine.

FIRST PROCESS.

Alimentary mixtures or animal tissues, cut into small pieces, are to be placed in eight and a half ounces of distilled water, acidulated with from four to six drops of pure concentrated sulphuric acid, afterward to remain for five or six hours on a filter. The liquid is then evaporated by a very gentle heat until it is reduced to one-sixth of its volume, in order to separate a certain quantity of organic matter. I would remark, that during this evaporation, the liquid undergoes a slight change of color, but does not seem to undergo the least decomposition. When the liquid is cool, it is to be agitated with twice its volume of very concentrated alcohol; this menstruum often throws down an additional quantity of organic matter, which, however, occasions no inconvenience, and requires no interference. It is then again *filtered*, and evaporated, until the alcohol is entirely volatilized; after leaving

the liquor to cool, it is saturated and made alkaline by an excess of soda; at the same time a distinct odor of conicine is perceived. The whole is then to be agitated with sulphuric ether for four or five minutes, in a closed tube, the layer of ether is to be separated by the aid of the finger and a funnel, and the ethereal solution left by itself in a small porcelain capsule. The ether is volatilized, and *leaves the conicine behind*. Nothing further is needed, except to distill it in chloride of lime. It is necessary, in order to extract as large a quantity of conicine as possible, to heat a second time by warm alcohol, (concentrated) the solid matter, resulting from the evaporation of the sulphuric liquor, and the first alcoholic treatment; experience has taught me, in fine, that this solid matter very often contains a small proportion of conicine.

SECOND PROCESS.

Instead of submitting to the action of ether, the liquor saturated by an excess of soda, and obtained in the manner described, it may be distilled over a rapid fire, in a retort, to which a receiver is adapted, which is to be placed in cold water; the conicine, as it comes over, is condensed in the receiver; and nothing more is necessary, except to concentrate it, by evaporation over a gentle fire, or what is better still, distill it on chloride of lime.

ART. II. — *Case of Rupture of the Tendon Achilles by Muscular Effort.*

By S. BARRETT, M. D., Le Roy, Genesee co., N. Y.

I send you the following, to me, unique case of Rupture of the Tendon Achilles, by muscular effort:

J. H., laborer, aged 48, of intemperate habits, while walking, in Nov. last, across a bridge, stepped upon the edge of a hole with his right foot, letting the heel drop into it; and making a sudden effort to recover himself, he felt a sharp pain extending from the heel up the leg, accompanied by an audible snap. In attempting to go a few steps farther he felt the same again, which rendered him unable to walk. He was brought home, and on examination the Tendon Achilles was found entirely separated about two inches above its insertion. The divided extremities were nearly an inch apart, as plainly to be felt as after a sub-cutaneous section for club-foot. He has been unable to walk without a crutch or cane until within the last month, but at the date of this the union seems to be pretty firm, and he walks pretty well by favoring it a little.

LE ROY, Genesee co., N. Y., April 24, 1852.

†

ART. III. — *Dislocation of Upper End of Femur, left Unreduced.*

DR. FLINT, — It is probably no very uncommon event for a dislocated hip to be left unreduced, even where the case has been under the hands of a clever surgeon; but it is certainly not often that such cases obtain a public record. We are not over-zealous, generally, to publish our own failures, and it is hardly generous to advertise the failures of others; so that between our selfishness and our unselfishness many of the shortcomings of our art are hidden.

Fortunately Mr. Chelius, the author of a most excellent "System of Surgery," has sufficient reputation the world over to enable him to bear a portion of these failures, without injury to himself or the profession which he so eminently illustrates. I shall therefore make no apology for requesting you to record this unsuccessful attempt to reduce a dislocated hip, in which he was himself the operator.

June 11, 1851. John Maurer, a German, aged nineteen years, called upon me at my office, and related as follows:

"When ten years old I fell from a tree, a height of six feet, and dislocated my left hip. I was then living twelve miles from Heidelberg; and I was immediately taken there, but I did not see Mr. Chelius until the next day. He took me to the university, and, before the medical students, attempted to reduce it, but he could not. During several weeks following he tried six times, using pulleys, &c., but he never could succeed."

I find the limb shortened two inches; the knee is turned in, and the toes out. The dislocation is upward and backward upon the dorsum ilii. He walks rapidly and without pain or discomfort, but with a manifest halt.

Yours truly,

FRANK H. HAMILTON.

ART. IV. — *Third Clinical Report on Continued Fever, based on an Analyses of Sixty-four Cases.* By the EDITOR.

Since the preparation of my Second Report on Continued Fever, in the summer of 1851, which is contained in the seventh volume of the Buffalo Medical Journal, another six months' service as attending physician to the Buffalo Hospital of the Sisters of Charity, has transpired. The hospital records during this period, viz., from October, 1851, to April, 1852, embrace a larger number of fever cases than have heretofore been treated, in the

same length of time, since the institution was established. This, probably, was partly in consequence of the hospital accommodations having been increased in 1851, so that it is now capable of receiving a considerably larger number of patients than hitherto; and, in part, because the sick supported by the emigrant fund have been more uniformly sent to this institution than formerly. The number of cases, the histories of which were recorded during my last term of service, is *sixty-four*, making with the number previously analysed, *one hundred and sixty-four*. The labor of the two analyses, so far as my own mind is concerned, instead of rendering the subject tiresome or disagreeable, has served to enhance an interest therein, so that is difficult to forego the gratification of subjecting this new collection of recorded data to similar methods of interrogation, in order to compare the results with those contained in the former Reports. The majority of the readers of the Buffalo Medical Journal, however, may not sympathize with this feeling, and I could hardly expect that their patience would endure another series of articles on this subject so extended as those which have preceded. In fact, conscious that the announcement of a *Third Clinical Report on Continued Fever* may be likely to give rise to impatience, if not disgust, at the prospect of a repetition of all the dry, tedious details of another investigation, I am desirous to make haste in giving an assurance that such is not my present intention. I design only to examine the cases with reference to points possessing peculiar interest or importance. In this Report I shall not aim to enumerate and compare all the events, or symptoms, that have been recorded, with a view to their relations, respectively, to each other, and to the natural history of the disease; but my object will be to study the facts only in so far as they relate to questions which are more or less unsettled, and in which every medical reader is supposed to feel an interest. The identity or non-identity of the two types of Continued Fever, *Typhus* and *Typhoid*; the circumstances involved in the distinctive diagnosis, and the treatment of the disease, will form the most prominent of the points to which attention will be directed; and I shall content myself with stating the results relating to these points as briefly as possible, indulging in very few comments. By pursuing this course, I shall hope to make the following Report not altogether uninteresting to those who have honored the previous Reports by a careful perusal, and, at the same time, render it acceptable to those who have become subscribers to the Journal with the commencement of the present volume, as well as to some, (if not many) who, from a want of fondness for the minutiae of statistical researches, may have been in the habit of leaving unread the articles that I have heretofore contributed on the subject. Should

I fail in attaining these ends, at all events the space occupied by the present Report will be comparatively small, so that, in consideration of good intentions, even the captious reader will perhaps vouchsafe his indulgence.

Of the *sixty-four* cases upon which the present Report is based, *fourteen* were of the *Typhoid* type; *forty-two* were cases of *Typhus*, and in *eight* the diagnosis is not positive. The latter, therefore, are to be included under the head of cases of *Doubtful type*. In this collection the cases of *Typhus* preponderate considerably over those of *Typhoid*. The reverse of this was true in both previous collections. This variation does not necessarily indicate that the *Typhus* form of Continued Fever was more prevalent during the past season, but it is in part owing to a greater number of emigrants having been sent to the hospital, and, as will be seen, it is partly due to the fact that more cases were developed within the hospital limits than heretofore. Adding the *Typhus* and *Typhoid* cases respectfully in the three Reports, and the result is as follows:

Number of <i>Typhoid</i> cases,	-	-	-	-	61
Number of <i>Typhus</i> cases,	-	-	-	-	65

The preponderance of the *Typhus* cases in this collection thus renders the aggregate of cases of each type nearly equal. Of the *fourteen* cases of *Typhoid* Fever *five* were fatal. *Five* of the *forty-three* cases of the *Typhus* type ended fatally. *All* of the *eight* cases of *Doubtful type* recovered. As respects mortality, this collection compares favorably with the cases treated in previous years. The consideration of this fact will come up in an appropriate place hereafter.

It will be most convenient to distribute the facts to be presented in this Report in the same sectional divisions that have previously been adopted. Reference can then be readily made to any particular topic in the three Reports. This plan will be pursued, but the limited scope of the present Report will involve the omission of more or less of the topics enumerated in the headings of the several sections.

I shall make no reference to the cases of *Doubtful type*, in the twelve succeeding sections, but devote some attention to them afterward.

SECTION FIRST.

Age, Sex, Nativity, Season. Constitution, and previous health. Period of Residence in this Country. Duration of Disease before coming under Observation.

Age. Typhus. The average age in *thirty-five* cases is a fraction under

26, or, to be precise, 25, 32-35 years. In this group of cases are included *five* in which the ages respectively were but 10, 9, 10, 15, 13. Excluding these cases, and the mean age is 28 1-3 years. This group embraces among the oldest patients the following ages:— 58, 46, 45, 43, 40, 37.

The average age in the *five* fatal cases is 28 2-5.

Typhoid. In the *fourteen* cases of this type the average age is a fraction over 25 years. The youngest patient was 20 years, and this was the age precisely in six of the cases. The oldest patient was 50 years; the next oldest, 34; the next, 27; and the next, 24.

The average age in the *five* fatal cases is 28 3-5.

In these enumerations we find, as in the previous Reports, that the *Typhus* type exhibits the maximum of age, and the highest mean. We find, also, that the average age in the fatal cases of both types is higher than in those ending in recovery.

Sex. Typhus. Males, 26; Females, 16.

Typhoid. Males, 10; Females, 4.

Nativity. Typhus. *Thirty-six* were from Ireland; *two* were Germans; *two* were English, and *one*, French. In *one* case the nativity is not noted. Of the *five* patients not from Ireland all, save *one*, were either immigrants lately arrived in this country, or the disease was developed in the hospital. The excepted case was that of the Frenchman, who had been in America three months, and in Buffalo six days. Whether he had been brought in contact with the disease prior to being attacked, or not, is not stated.

Typhoid. *Six* were Irish, and *eight* were Germans.

Season. Typhus. The cases were distributed among the several months as follows:— October, *none*; November, *four*; December, *nine*; January, *eleven*; February, *ten*; March, *eight*.

Typhoid. The cases occurred as follows:— October, *two*; November, *one*; December, *four*; January, *three*; February, *two*; March, *two*.

Constitution and previous health. Typhus. In *twenty-one* cases it is noted that the patients, up to the time of being attacked with fever, were in good health. In *fifteen* cases the facts with respect to this point are not noted. In *six* cases the histories show the previous health not to have been good. In *one* of these cases the patient had been poorly, without any definite ailment, during the passage across the Atlantic, being attacked with fever shortly after his arrival. In the *five* remaining cases the patients were attacked in hospital, having been admitted for other affections, as follows:— *acne, one*; *varioid, one*; *chronic conjunctivitis, two*, and in *one* case the patient had shortly before passed through the career of *Typhoid* fever.

Typhoid. In *six* cases the facts with respect to this point were not noted. Of the remaining *eight* cases the previous health was good in *six*. In *one* case the disease, as stated by his attending physician, was preceded by Inter-mittent fever, and in the other case, the patient had had cough and more or less diarrhoea for seven weeks before being attacked with fever; these symptoms being probably due to tuberculosis of the lungs.

Period of residence in this country. Typhus. In *nine* cases the patients were attacked within the hospital. In the remaining *thirty-three* cases, the patients had resided in this country as follows: — For a period not exceeding *one* week, (six days,) *one* case; not exceeding *two* weeks, *seven* cases; do. *three* weeks, *six* cases; do. *four* weeks, *two* cases; do. *five* weeks, *five* cases; do. *six* weeks, *one* case; do. *seven* weeks, *one* case; do. *eight* weeks, *one* case. For periods longer than eight weeks, *nine* cases, viz., five months, eight months, three months, seven months, ten months, eight years, three years, four months, two and a half years.

Five of this group of patients were members of the same family, and came over in the same ship. *Three* other patients were also related, and crossed the Atlantic together.

Typhoid. The shortest period of time any of the patients had been in this country was seventeen days. This was the period in *one* case. In *one* case the period was *nineteen* days. In *one* case the period did not exceed *three* weeks, and in *one* case it did not exceed *four* weeks. In *one* case the patient was attacked with the disease in the hospital. The periods longer than four weeks were respectively as follows: — fourteen weeks, five months, six weeks, six months, eight months, one and a half years.

Duration of disease before coming under observation. From the enumerations under this head in the last Report, it appeared that patients are more apt to enter the hospital so soon as they are obliged to take to the bed when the type of the disease is *Typhus*; and also that the average duration of the disease, dating from the time of taking to the bed, before entering hospital, is shorter in cases of *Typhus*, than of *Typhoid*. An examination of the cases in this collection with reference to these points develops the same results. Excluding cases in which the patients were attacked in the hospital, the duration prior to admission is noted in *twenty-four* cases of *Typhus*, and in *ten* cases of *Typhoid*. Of the twenty-four cases of *Typhus*, in *six* the patients did not take to the bed until they entered the hospital. This was true of only *one* of the ten cases of *Typhoid*.

The mean duration of the disease, from the time of taking to the bed to the time of coming under observation, in the cases of *Typhus* exclusive of

those who did not take to the bed prior to entering the hospital, in the *Typhus* cases is 5 10-17 days; in the *Typhoid* cases 7 7-9 days.

SECTION SECOND.

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

Access. In cases of fever received at different stages of the febrile career, it is frequently difficult to obtain precise, reliable information respecting the previous history. Owing to this difficulty, the duration of the access, (the only point that I shall consider under this head, in the present Report) was determined in a portion only of the cases, although more pains were taken to obtain this information than heretofore. It will be borne in mind that the data determining the duration of the access, as stated in the previous Reports, are the first prodromic symptoms, and, the time of taking to the bed. The latter circumstance is selected, as on the whole, the best criterion (although by no means perfect) for marking the end of the access, and the beginning of the febrile career. These data are noted in the histories of *eighteen* cases of *Typhus*, and *ten* cases of *Typhoid*. A comparison of the two types develops the following results:— Of the *eighteen* cases of *Typhus*, the attack was sudden in *seven*; that is to say, in these seven cases the patients took to their beds on the first day on which any symptoms of disease were present. The longest duration of the access in the *Typhus* group, was *seven* days. The mean duration in the cases of this type is 4 4-11 days.

Of the *ten* cases of *Typhoid*, the attack was sudden in but *one* case. The longest duration was *ten* days. The mean duration is 5 3-8 days.

These results, thus, exhibit a larger proportion of cases of the *Typhus* type in which the disease occurs abruptly, without any access; a higher maximum of duration in cases of *Typhoid*, and a shorter average duration* in cases of *Typhus*.

* In the Second Report, after giving the duration of the access in *ten* cases of *Typhoid*, and *six* cases of *Typhus*, I have remarked, "The foregoing results do not afford any confirmation of the law, deduced from extensive observations, that the *Typhus* type of Continued Fever oftener commences with a sudden attack, or is ushered in by an access of shorter duration than *Typhoid*." An examination of the figures shows that this statement is not altogether correct, and I regret to be obliged to conclude that it must have been written without due attention. If the reader will turn to the second Section of the Second Report, and take the trouble to compare the cases of the two types with

Circumstances supposed to have been concerned in the production of the disease. In a large proportion of cases the patients, as has been seen, were immigrants but lately arrived in this country. In these cases, and, indeed, as a general remark, in all excepting the patients who were already inmates of the hospital when attacked, it would be difficult to obtain reliable information concerning circumstances which, directly or indirectly, might be more or less involved in the causation of the disease. The histories, therefore, contain very few facts of importance except in the cases which were under observation before fever became developed. I shall direct attention, with reference to the subjects belonging under this head, to the latter cases only, and the chief point of interest will be the enquiry in how far do they exemplify the operation of *Contagion*.

Eleven of the patients affected with the *Typhus* type, were attacked with the disease in the hospital. I will give briefly the facts, so far as they relate to the present subject, which are contained in the histories of these cases severally.

CASE I. — This patient had been employed as a domestic in the hospital for twelve months. She had been accustomed to be frequently in the ward in which fever patients were received, making the beds of these, as well as other patients, etc. The case was the fifteenth that occurred in the order of time. She was attacked Jan. 3d. She was well up to the date of the attack. The case was the first that proved fatal.

CASE II. — This patient was admitted in December with acne. He was in the general ward with fever patients until the 18th of December, when he left the hospital. Jan. 26th he entered with fever. Strictly speaking, he was thus not attacked in the hospital, but had been in hospital shortly before being attacked. He was a German, and had been in this country a year. This case ended fatally.

CASE III. — This patient entered with varioloid, the disease not being declared prior to his admission. He remained in the general ward a couple of days, until the character of the disease was manifested, and was then transferred to a separate apartment. The varioloid was very mild, the eruption being quite small. He was an Englishman, and had arrived in this country but a few days before his admission. Twenty-two days after the date of his

respect to this point, he will find that the duration in the *Typhoid* cases is a fraction longer than in the cases of *Typhus*, the ratio being as 3 6-7 days is to 3. In the proportion of the cases of sudden attack, however, the law of the disease was reversed, i. e. the instances were more numerous in the *Typhoid* group, the ratio being as 3-10 is to 1-6.

admission, having, in the mean time convalesced from the varioloid, he took to the bed presenting the eruption and other symptoms of *Typhus*.

CASE IV. — This patient had been in the hospital three months in the capacity of porter. Had been accustomed, occasionally, to sit up with fever patients at night. He was attacked February 1st, having been, up to that time, in good health. This was the twenty-ninth case in chronological order.

CASE V. — This patient had been in the hospital three months for chronic conjunctivitis. She was in the surgical ward, and it is not stated whether she had been in the habit of visiting the female ward in which fever cases were received, or not. She had been in this country sixteen months.

CASE VI. — This patient had been in hospital a year with chronic conjunctivitis. It is not stated how long he had been in this country, nor how frequently he had been in the general ward in which fever cases were received. Surgical patients, however, frequently passed through, and visited this ward. This was the thirty-seventh case.

CASE VII. — This patient had been in hospital between three and four months, as a boarder, being in good health. It is stated that he had not slept in the general ward, but he was probably frequently in this ward. It is not stated how long he had been in this country. This was the thirty-fourth case.

CASE VIII. — This patient entered hospital about three weeks before being attacked. He was in good health when admitted. He was a German, and had been in this country about six months. He was attacked in March, making the thirty-seventh case.

CASE IX. — This patient had been employed as a cook for ten months. She had been in the ward in which were fever patients a few times, remaining each time but a few minutes. This was the thirty-eighth case.

CASE X. — This patient had been a surgical patient with chronic ophthalmia for ten months. It is stated that he had not been in contact with fever patients. Attacked in March.

CASE XI. — This patient entered with *Typhoid* fever presenting the characteristic eruption, etc., and was convalescent on the twenty-third day after taking to bed, and the sixteenth after the date of admission. He was attacked with *Typhus* on the twenty-sixth day after the date of convalescence from *Typhoid*, presenting the characteristic eruption, etc., of the former type. He was convalescent from *Typhus* on the seventeenth day after taking to the bed. He died about three weeks after my time of service expired, with tuberculosis of lungs. Some account of the autopsical appearances will be given in the section devoted to that subject.

In addition to these cases, one of the Sisters of Charity connected with the institution was attacked with *Typhus* in January, and died on the tenth day of the disease. Notes of this case not having been taken, it is not included in the collection for analysis. This Sister was one of two who were added during the past year, to the institution; so that, up to the date of this Report, *five of twelve* Sisters have had fever. A fact especially bearing on the question of contagion is, that to the Sister who died with the disease was assigned the care of fever patients, and she was the only Sister brought into close proximity to fever cases, except those who had already had the disease. *Up to the present date, in fact, every Sister to whom has been assigned, during the terms of my service the duty of nursing fever cases at this institution has suffered an attack.**

The accommodations at this hospital have, as yet, not enabled the entire separation of fever cases from patients affected with other diseases. Heretofore, no instance (with a single exception mentioned in the second Report) has occurred in which fever has apparently originated in the institution, save in those who have been brought into close and continued contact with the disease by nursing the sick. Assuming that in most, if not all the cases which have just been enumerated, the disease was due to contagion, there are several circumstances accounting for its diffusion in this way during the past winter. The number of *Typhus* cases admitted was considerably greater than in any previous year. The number of patients with various diseases was also greater, and hence the general wards appropriated to fever and other diseases, were more crowded. The winter, moreover, was unusually severe, and, on this account, ventilation was less practicable.

The two latter circumstances, it may be said, are adequate to account for the generation of the special cause of fever irrespective of contagion. It is probably true that from concentrated animal exhalations may be evolved a miasm capable of developing *Typhus*. Had these cases occurred in immigrants lately arrived, the supposition might be entertained that the special cause of the disease was imbibed prior to entering the hospital; but it will be observed that, with a single exception, all the patients had been in this country for a considerable period of time. The latter explanation is consequently hardly admissible. The former, however, it must be considered, has sufficient force to affect somewhat the conclusiveness of the evidence which the cases afford of contagion; and the argument of the non-contagionist is in some degree favored by the fact, that in some of the cases originating within the hospital,

* In some of the cases of *Doubtful type*, the disease was developed in the hospital. See section fourteenth.

the patients were not brought into close or frequent contact with the disease. Notwithstanding these considerations it seems most rational to attribute the origin of the disease, in these instances, to contagion.

In none of the cases of the *Typhoid* type did the disease originate in the hospital. The only instance admitting the supposition of a contagious influence was in the case of a female, the wife of a patient who died with this type of fever. She was not an inmate of the hospital during the illness of her husband, but was with him daily more or less. Three days after his death she was admitted with fever which proved to be *Typhoid*. In this case it may be suspected that the disease was communicated, but it is perhaps quite as reasonable to suppose that the special cause was received by both persons from the same source, and that the circumstances connected with attendance of the wife on her husband, were only indirectly involved in the development of the disease.*

Assuming the instances in which fever originated in the hospital to be explained on the supposition of contagion, it is to be observed that they were cases of the *Typhus*, not the *Typhoid* type. This shows, not only the greater contagiousness of *Typhus*, but that its offspring is the same form of fever — a fact of importance in its bearing on the question of the identity or non-identity of these two types. But the facts given in No. 11 of these cases is especially interesting with reference to this question. In that instance the patient entered with *Typhoid* fever, and in about three weeks after convalescence, was attacked with *Typhus*, presenting, successively, the distinctive characters of both forms of fever, including those pertaining to the eruption. This is a striking case, which cannot but be considered to sustain the doctrine of the non-identity of *Typhus* and *Typhoid*.

SECTION THIRD.

Symptoms referable to the general aspect and expression of countenances.

I do not propose to enumerate the symptoms belonging to this section. The results would show the existence of more or less capillary congestion of the face as a constant element of both types, and a marked degree of this congestion as a distinguishing feature of *Typhus*. They would also show that a circumscribed flush of the cheeks pretty uniformly attends cases of either type complicated with pneumonitis, and the absence of this symptom in cases in which pneumonitis did not exist.

* A son was also admitted with *Typhoid* about the time my term of service ended, making three cases of the same type in this family.

I have given sufficient examination to the histories with reference to these points to be warranted in the general statements just made.

SECTION FOURTH.

Symptoms referable to the nervous system. Mind. Coma.

Mind. More or less manifestations of delirium, consisting in incoherent talking, or efforts to get out of bed, were noted in *thirty* of the *forty-two* cases of *Typhus*; and in *nine* of the *fourteen* cases of *Typhoid*.

It is by no means probable that the patients who exhibited no manifestations of delirium were entirely free from mental aberration. It is very rarely the case that the faculties of the mind remain wholly unaffected during the career of Continued Fever. It is possible that had these cases been more constantly and completely under observation than is practicable in a hospital, even with the greatest attention, some evidences of delirium might have been discovered. The presence of this symptom was noted in all instances in which any manifestations were apparent at the daily visits, or had attracted the attention of the attendants in the intervals between the times of making the daily records. Inquiries were always carefully made with respect to this point, and it is uniformly noted either than delirious manifestations were, or were not apparent.

It will be observed that the proportion of cases in which this symptom existed, is larger in the *Typhus* group; the exemption from delirium in this group being in the ratio of about twenty-eight per cent., while it is about thirty-five per cent. in the *Typhoid* cases. The disparity between the two types, in this particular, is less striking than in the former collections.

The difference was greater as respects the stage of the disease at which the manifestations of delirium occurred. In no case of *Typhoid* did they occur until the febrile career had continued for several days, while they were frequently observed in the cases of *Typhus* early in the disease, and sometimes almost at the very commencement.

In nearly all the cases in which there were no manifestations of delirium, the disease was mild, as shown by the symptoms and the shortness of the career. This was true of every instance in the *Typhoid* group, and in all but three instances of the cases of *Typhus*. In every fatal case, save one, delirium occurred. In the excepted case, which was of the *Typhus* type, the patient died with apoplectic coma.

The degree of delirium was variable in the different cases, and also at different periods in the same cases. It was always more prominent at night, and frequently was only manifested at that time.

None of the cases in this collection were characterized by very active, persistent delirium, requiring restraint, of which a few instances are given in the former Reports.

Coma. The comatose state became suddenly developed, proving fatal, in four cases, two of *Typhus*, and two of *Typhoid*. In two other (*Typhus*) cases there occurred a close approach to coma, and in several instances a lesser degree of approximation thereto. I shall have occasion to refer to these cases in the section devoted to the Respiratory System, and I will therefore defer the details until I take them up in that connection.

SECTION FIFTH.

Symptoms referable to the digestive system. Alvine dejections, Tympanites, Tenderness of abdomen.

Of the *forty-two* cases of *Typhus* more or less constipation was present in *twenty-seven*; and diarrhoea was absent in all but *ten* cases. In each of these ten cases the diarrhoea was mild, and of short duration, in several instances lasting but a single day.

Of the *fourteen* cases of *Typhoid*, diarrhoea, more or less in duration and degree, existed in *thirteen*; the single remaining case was characterized by constipation. The diarrhoea was generally mild, and easily restrained by an opiate administered by the mouth, or by injection. In some instances it continued more or less throughout the febrile career, and in other cases it was of short duration.

The proportion of *Typhus* cases characterized by diarrhoea is not far from that developed by the previous analyses; but the proportion of cases of *Typhoid* in which this symptom was present is considerably larger than in the former collections. I can assign no reason for the latter fact. It renders the contrast between the two types, as respects this symptom, more striking than appears in the former Reports.

Tympanites. The following enumerations, relate, *first*, to the absence of this symptom *during all the time the cases were under observation*: that is, by the statement of its absence is meant that it was never at any time present. *Second*, they relate to the *degree* of tympanites as expressed by the terms *slight*, *moderate*, and *considerable*. Here, also, it is meant that these respective degrees were present once, or oftener, while the cases were under observation. Frequently in the cases in which moderate or considerable tympanites existed, it continued for a short period, perhaps for a single day only; and in some instances in which it existed in a slight degree, it was

absent the greater part of the time. The results then, it is to be borne in mind, have no relation to the habitual condition of the abdomen as respects this symptom while the disease was in progress; but only the proportion of cases in which tympanites, in different degrees, was present at any time, and for ever so short a period during the febrile career. It is obvious that, with this explanation, the enumerations do not cover an important point of comparison, viz., the proportionate duration of the symptom. Thus, two cases may be on equal ground in the fact that moderate meteorism existed in both; but in one case the moderate meteorism may have continued during the whole course of the disease, and in the other case it may have existed but a very short period, perhaps being noted only on one day. It would be difficult to institute a comparison of the two types as respects the duration, as well as presence or absence and the degree of tympanites, without occupying considerable space. It will perhaps suffice to say, in general terms, that the individual cases in the *Typhoid* group presented this symptom, in its different degrees, much more frequently during the progress of the disease than the cases of *Typhus*. In a large proportion of the latter group of cases, in fact, this symptom was of very short duration, often being observed only once or twice. In this point of view the two groups of cases exhibited a contrast more striking than is apparent in the following enumerations:

Of the *forty-two* cases of *Typhus*, tympanites was absent in *twelve*, and present in *thirty* cases.

It was slight in *ten* cases; moderate in *fourteen* cases, and considerable in *six* cases.

Of the *fourteen* cases of *Typhoid*, tympanites was absent in *one* case, and present in *thirteen* cases.

It was slight in *two* cases; moderate in *five* cases, and considerable in *six* cases.

These results differ from those developed by the former analyses, in the larger proportion of instances in which tympanites was present in the *Typhoid* cases. It is curious to observe how nearly as respects the proportion of cases in which the symptom was present in the *Typhus* cases, the results of the former analyses are repeated. Adding together the cases of *Typhus* in the two previous collections and tympanites was present in the ratio of 16 - 22. In the present collection it was present in 15 - 21! This similarity is striking. But in the *Typhoid* cases the same conformity does not exist, for while in the cases which enter into the two previous collections the symptom was present in but *thirty-four* of *forty-seven*, the present analysis gives it in *thirteen* of *fourteen* cases. I can assign no reason for this disparity.

It will be remarked that the proportion of instances in which the tympanites was considerable and moderate, is much larger in the *Typhoid* cases. This accords with the results of the former analyses.

Tenderness of abdomen. The remarks made with reference to the duration of tympanites will measurably apply to the present symptom. The enumeration of the cases in which tenderness was present, embraces all in which it was noted at any time, even on a single record, during the career of the disease; and, on the other hand, in the cases in which it was not noted, it was never present at the times of the daily examinations. So in those cases in which it was considerable, it is not to be understood that it was uniformly present in that degree, but that it was so on one day, or more. With respect to the continuance of this symptom, the same is true as with tympanites, viz., it was more frequently or uniformly present during the progress of the disease in the cases of *Typhoid*, than in those of *Typhus*, its greater duration constituting quite as distinguishing a trait of the former type, as that of tympanites.

Of the *forty-two* cases of *Typhus*, abdominal tenderness was absent in *twenty-seven*, and present in *fifteen*.

It was *slight* in degree in *nine* cases; *moderate* in *five* cases, and *considerable* in a single case.

Of the *fourteen* cases of *Typhoid*, abdominal tenderness was absent in not a single case.

It was *slight* in *three*; *moderate* in *five*, and *considerable* in *six* cases.

These results accord with those developed by the previous analyses in showing a greater frequency of tenderness in cases of *Typhoid*, and its presence in a more marked degree in cases of that type. They differ, however, from the results contained in the former Reports in exemplifying the distinctive traits of the *Typhoid* type in a more striking manner. In the present collection of cases, tenderness was in no instance absent in the *Typhoid* group, while in the former collections it existed in a large proportion of cases, but was not uniformly present. In this collection the tenderness was oftener considerable in degree in the *Typhoid* group. And in the *Typhus* group, in this collection, tenderness was absent in a larger proportion of cases than in the two former collections. Thus, a comparison of the results of different analyses with respect to this, as well as other points, is interesting as showing sometimes considerable numerical fluctuations, but nevertheless concurring to establish by their uniformity certain laws of the disease, and principles of diagnosis. The results of the former analyses authorized us to consider the more frequent occurrence of abdominal tenderness, and the greater prominence

of this symptom, as characterizing the *Typhoid* type of Continued Fever. The foregoing results differ from those previously developed, in furnishing more striking proof of the correctness of this distinction!

In this collection of cases, as heretofore, the abdominal tenderness was frequently confined to the right or left iliac regions, more especially to the former, and when more extensive it was especially marked in these situations. This was true of the cases of either type, but particularly those of the *Typhoid* group.

Hæmorrhage from the bowels did not occur in any instance, save that in one fatal case of *Typhoid*, shortly before death, the dejections were somewhat bloody.

Slight bleeding from the gums occurred in one case, which was of the *Typhus* type. This case did not prove fatal.

Parotitis occurred in but a single case, which was of the *Typhoid* type. It became developed about the time of convalescence. The right gland was alone affected. The affection proceeded to suppuration, and the patient recovered. The case presenting this complication, or, rather, in this instance, sequel, was admitted in March, being one of the latest of the recorded cases.

Perforation of intestine did not occur in any of the cases in this collection, nor *peritonitis* irrespective of this accident.

SECTION SIXTH.

Cutaneous eruptions.

Typhus. Of the *forty-two* cases of this type, the characteristic maculated eruption was present in *thirty-six*, and absent in the remaining *six*.

The eruption was quite abundant in *twenty-seven* cases.

It was more or less copious on the limbs, as well as body, in *twenty-seven* cases.

A few *maculæ* were observed on the face in one instance.

In *seven* cases, intermingled with the *maculæ*, were more or less of the rose-colored *papulæ* characteristic of *Typhoid*: in other words, the eruption exhibited mixed characters. Inasmuch as considerable interest and importance belong to the eruption in its relations to diagnosis, and the question of the identity or non-identity of the two types, I will give a brief statement of the facts pertaining to this symptom in the cases just referred to.

CASE I.—The patient presented on her admission, the sixth day after taking to the bed, an eruption, extending over the body and limbs, consisting in *part* of spots, rose-colored, somewhat elevated, the redness disappearing on

pressure; and in part of others which were small, dusky, the redness not disappearing on pressure. It is subsequently noted that the greater part of the eruption was of the latter description. The eruption continued, gradually fading, for fifteen days after the date of admission.

CASE II. — The patient presented, on admission, the fifth day after taking to the bed, an eruption, the greater part of which consisted of rose spots, large, elevated, and disappearing on pressure, the remainder small and dusky. The eruption extended over the body and limbs, and continued until the day of death, this being a fatal case.

CASE III. — At the time of taking to bed, this patient presented an eruption with mixed characters. It was copious, extending over the body and limbs. It continued to present mixed characters so long as observed, remaining visible up to the day before convalescence.

CASE IV. — On the fourth day after taking to the bed, a few rose spots were visible, elevated, and disappearing on pressure. The day following, a copious eruption appeared over the body and limbs, being in part rose colored, etc., but the greater part having the typhus characters. The eruption gradually faded, but was visible up to convalescence.

CASE V. — On the second day after taking to the bed, an eruption appeared over the body and limbs, dusky, not disappearing on pressure. On the fourth day it was noted that the eruption presented mixed characters, and this was observed for the three following days.

CASE VI. — An eruption appeared in this case on the day on which the patient took to the bed, over the abdomen and chest, copious, and presenting the characters of *Typhus* and *Typhoid* intermingled. It continued to be thus mixed, and disappeared two days before the date of convalescence.

CASE VII. — On the second day after taking to the bed an eruption made its appearance, copious, dusky, not elevated, and the redness not readily disappearing on pressure. Subsequently a few rose spots with the *Typhoid* characters became developed, and were intermingled. The eruption continued visible for ten days, gradually fading.

I have no doubt respecting the correctness of regarding the foregoing cases as belonging to the *Typhus* group. In all instances in which there seemed any room for doubt as to the diagnosis in this collection, as heretofore, the cases were rejected, and placed under the head of *Doubtful type*. The distribution was made after carefully surveying all the symptoms and events belonging to the history. These cases, thus, presented the *Typhus* eruption with more or less of the *Typhoid* eruption superadded. The history of the eruption, aside from the sensible characters, points to *Typhus*. It appeared

earlier in the disease than the *Typhoid* eruption, it continued longer, it was more copious, and extended over the limbs as well as the chest and abdomen. I shall revert to this subject presently.

In *nineteen* cases, the time of the first appearance of the eruption, reckoned from the date of taking to the bed, was accurately determined. This was not practicable in a larger number of cases, in consequence of patients frequently not entering the hospital until after the eruption had appeared. Of these *thirteen* cases, in *four*, the eruption appeared on the very day of taking to the bed; in *ten*, it appeared on the *second* day; in *two*, on the *third* day; in *one* case on the *fourth* day; in *one*, on the *sixth*, and in *one*, on the *eighth* day. In the two cases last mentioned, the patients took to the bed as soon as they were attacked.

The average period elapsing from the time of taking to the bed to the first appearance of the eruption, according to the foregoing data, is about *two and a half* days.

The duration of the eruption was noted in *fourteen* cases. The number of days in the cases respectively are as follows: — 17, 16, 10, 7, 9, 8, 12, 11, 14, 15, 16, 7, 7, 10.

The average duration will, thus, be between *eleven* and *twelve* days.

The eruption uniformly, after a few days, commenced to fade, and gradually became less distinct from day to day until its disappearance.

Typhoid. Of the *fourteen* cases of this type, the characteristic eruption was present, in a greater or less degree, in every case.

The eruption was abundant in but a single case, the rose spots in all the cases, with this exception, being not numerous, and in some instances quite few. The smallest number of rose spots counted in any case was *three*. So small a number as this occurred in but a single instance. The next smallest number was *four*. In only two cases, were so few as this observed. In the remainder of the cases, exclusive of the case in which the quantity was copious, the number ranged from a dozen to fifty. The number varied at different periods of the febrile career.

In not a single instance is it noted that the eruption extended to the extremities, being confined, in all, to the chest and abdomen.

In no instance among the cases in this group did the eruption present mixed characters. The traits distinguishing the *Typhoid* type were uniformly preserved.

The time of the first appearance of the eruption, reckoning from the commencement of the febrile career, or the date of taking to the bed, was ascertained in *six* cases. The number of days in these cases successively, are as

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follows:— 2, 6, 7, 3, 12, 11. This gives an average of a fraction less than seven days.

The duration was noted in seven cases, and was, in the cases, individually, as follows:— 10, 4, 7, 13, 3, 6, 11.

The average is found to be seven days. The foregoing results differ from those developed by the previous analyses chiefly in the uniform presence of the eruption in the cases of the *Typhoid* group. In the two collections of cases before analyzed, it was present not invariably, but in a large proportion of the cases of this type. On reference to the first and second Reports it will be found that in the *Typhus* group the eruption existed in the larger number of instances. The reverse is true of the present collection, but this is not owing to the proportionate number of cases of *Typhus* in which the eruption was absent being greater, but to the constancy of the eruption in the cases of *Typhoid*. Why such a contrast should exist it is impossible to say.

On comparing the time of appearance, and the duration of the eruption in the two types, we find very striking points of distinction in the earlier period at which it is observed in *Typhus*, its abundance in a larger proportion of cases, extending very frequently over the extremities, its mixed character in a few instances, and its longer duration. There are important points to be considered, in discriminating the two types, in addition to the striking differences in the sensible characters of the eruption, viz., that in *Typhus* being maculated, small, orbicular, dusky, and the redness not readily disappearing on pressure; in *Typhoid*, papular, larger, oval, rose red, and the redness disappearing momentarily on pressure.

The mixed character of the eruption in a small proportion of the cases of *Typhus*, is a point of interest and importance. In discriminating between the two types considerable stress is properly to be laid on the sensible appearances of the eruption, and, hence, when the two eruptions are intermingled, difficulty as to the diagnosis might be expected to arise from this circumstance. My observations thus far lead to the conclusion that this intermingling occurs in cases of *Typhus* only. It is desirable to settle by more numerous observations, whether cases of the *Typhus* type are alone liable to this variation from the general rule of each type, preserving with distinctness its peculiar symptom. Should this law be established, such variations would, of course, occasion no practical difficulty whatever. The presence of a well marked *Typhus* eruption would denote the type of the disease whether more or less "rose spots" are intermingled or not. Were the eruption frequently to exhibit mixed characters, the fact might be considered justly to militate against the doctrine of the non-identity of the two types. Instances in which

this peculiarity is observed, are, however, few in number, as shown by their small proportion in this, and the preceding collections.

One of the distinguishing features of the *Typhus* eruption is not always very strongly marked. I allude to the non-disappearance of the redness on pressure. In several of the cases in the present collection it is noted that the redness did not *readily* disappear on pressure. Frequently the redness can be made to disappear on firm pressure, and in this particular different spots present at the same time, in the same case, will be found to differ. Reference was made to this fact in a single case in the second Report. It will also occasionally be observed that some rose spots in the *Typhoid* eruption disappear but partially on pressure. This was noted in one case mentioned in the first Report.

A petechial eruption occurred in but a single case in this collection. The petechiæ were few in number, confined to the abdomen and chest, appearing late in the febrile career. The case ended in recovery, and was not one of unusual severity.

Miliary vesicles, or sudamina, were observed in *four* cases, all of the *Typhus* type. They were not carefully sought for in every case, and may therefore have existed in some instances, in small numbers, and escaped notice. In the cases in the histories of which their presence is noted, they were abundant.

SECTION SEVENTH.

Symptoms referable to the Respiratory Apparatus. Pneumonitis. Epistaxis. Aberrations of respiratory movements.

Pneumonitis. The data are insufficient for determining the precise number of cases in which pneumonitis existed. This arises from the histories not uniformly embracing records of physical signs. The presence of this complication is rendered positive by the evidence of physical explorations in *ten* cases, *seven* of which were of the *Typhus*, and *three* of the *Typhoid* type. It undoubtedly existed in a greater number of instances. In all the cases in which it is certain that pneumonitis existed, the respirations were increased in frequency, the cheeks generally presented a circumscribed flush, and there was some dilation of the *alæ nasi*. These symptoms are more distinctive than cough and expectoration. It is superfluous to say that physical exploration furnishes demonstrative evidence either for, or against the existence of this complication, and that this cannot be claimed for any, or all the symptoms referable to the respiratory system.

I regret that my observations in the cases forming this collection only authorize these few remarks. My apology is, that my daily duties rendered it extremely difficult, and almost impossible to determine and record the physical signs daily while the cases were under observation.

Epistaxis. Of the *forty-four* cases of *Typhus*, epistaxis occurred in *five*, and its non-occurrence is noted in all the remainder. In the five instance in which this symptom was present, the periods of its occurrence were as follows:— 1. On the *eighth* and *ninth* days of the febrile career, and in this case slight hæmorrhage from the gums occurred. 2. *Fourth* day. 3. Day before taking to bed. 4. Several times before admission. 5. *Fifth* day. It was slight in all but one instance, in that case being considerable, but not profuse.

Excluding one case of *Typhoid* in which nothing is said respecting this symptom, of the remaining thirteen cases its presence is noted in the histories of *four*, and its absence in the remaining *nine*. The periods of its occurrence were as follows:— 1. Twice during the access. 2. *Eighth* and *ninth* days. 3. *Second* day. 4. *Ninth* day. In none of the cases was it profuse.

These results correspond very nearly with those developed by the first analysis. They show the presence of the symptom in a larger ratio of cases in *Typhoid* than in *Typhus*, sustaining the correctness of regarding this fact as one of the points distinguishing the two types from each other.

Aberrations of respiration. Under this head I shall direct attention only to the occurrence of *spasmodic inspiration*. In the second Report I gave a brief account of the circumstances connected with this aberration in several cases, which appeared to invest it with considerable importance as a symptom accompanying and forerunning the development of sudden coma, or an approach to the comatose state. Special attention was directed to this symptom in observing the cases entering into the present collection. It occurred in several instances, and I will proceed to give briefly an account of the circumstances in the cases respectively.

Typhus. CASE I.— The symptom occurred on the *seventh* day after taking to the bed. The record of symptoms on the *sixth* day is as follows:— “Reports better. Aspect the same. Passed a quiet night. No pain. Mind dull. No subsultus. Pulse 128. Capillary congestion considerable. Respiration 32. Slight cough. Surface warm and dry. Tongue coated and dry. Slight sordes about teeth. Anorexia. Thirst. One dejection. Eruption continues. Slight meteorism. No abdominal tenderness.”

On the *seventh* day, at A. M., the record is as follows:— “Reports the same. Passed the night quietly, manifesting no delirium. No pain. No

subsultus. Mind quite dull. Eyes suffused. Tongue moderately coated and dry. No nausea. Anorexia. Thirst. Three or four dejections during the past twenty-four hours. Pulse 128. Capillary congestion considerable. Respirations 28. Eruption continues. Considerable meteorism. No abdominal tenderness."

"P. M. Pulse 128. *Spasmodic inspiration*. Treatment—T. opii, grs. xx. Blister to nucha. Brandy ζ i. hourly."

Eighth day. "The difficulty of respiration increased during the evening until the inspiration became loud and highly spasmodic. During the night she gradually improved. *She is easily aroused this morning, but in the evening with difficulty*. She reports feeling well. No pain. Eyes less suffused. Speaks more distinctly. Surface perspiring. Alæ nasi dilate moderately. No subsultus. Tongue dry and coated. Sordes about the teeth. Two or three dejections during the last twenty-four hours. Pulse 120. Moderate capillary congestion. Respirations 36. Occasional cough without expectoration. Eruption continues. Some meteorism. No abdominal tenderness. Treatment—Brandy ζ i. hourly."

After this date she continued to improve daily, and six days afterward was distinctly convalescent.

CASE II.—This was one of the gravest of the cases ending in recovery. The aberration under consideration occurred at night on the twelfth day of the febrile career. The symptoms on the morning of this day were noted as follows:—"Reports no better. Delirious during the first part of the night, but slept quietly during the latter part. No pain. Mind quite dull. No deafness. Slight subsultus. No tinnitus. No dilation of alæ nasi. Eyes slightly suffused. Remains on her back in a somnolent state, but is easily aroused. Anorexia. Thirst. No nausea, nor vomiting. No epistaxis. Takes brandy and nutriment readily. Pulse 128. Moderate capillary congestion. Eruption continues over body and limbs. Considerable meteorism. Abdomen tender on pressure. Skin warm and dry. Treatment—Brandy ζ ss. every half hour, with carb. ammonia grs. iii."

Thirteenth day. "At nine last evening the respiration became disturbed, the inspiration being short and quick. A blister 4×4, was applied to the nucha. This morning the respirations are frequent, but the rhythm is normal. Somewhat delirious during the first part of the night, but slept occasionally during the latter part. Moderate capillary congestion. Eruption continues. Meteorism slight. Abdominal tenderness continues. Frequent cough, without expectoration. Mind dull. Slight deafness. Marked subsultus. Quite restless, constantly moving her hands and throwing off the

bed clothes. Tongue moist and coated. No sordes. Respirations 24. No epistaxis. Pulse 144. No dejection for several days."

From this date she slowly improved, and was convalescent on the twenty-fifth day.

CASE III. — The inspiration became spasmodic on the *seventh* day. Up to that time there were no symptoms denoting unusual gravity of disease. On the *fifth* day the record is as follows: — "Reports the same. Passed the night quietly. No pain. No subsultus, nor tinnitus. Eyes suffused. Slightly deaf. Mind somewhat dull. Tongue coated and dry. Sordes about the teeth. No nausea. No dejection. Pulse 114. Moderate capillary congestion. Respirations 34. No cough. Eruption slightly faded. Meteorism moderate. No abdominal tenderness."

Seventh day. "Passed a very restless night, talking incoherently, and frequently attempting to get out of bed. He did not, however, appear to be in an alarming situation until about 5 o'clock, A. M., when he became dull, and the inspiration spasmodic. Stimulants were given freely, sinapisms applied to different parts of the body, and a blister over the occiput. At 7, his appearance denoted some improvement. The respiration was still distended, and the inspiration, except when he was aroused, slightly spasmodic. The respirations were also irregular, an interval occurring after about every sixth respiratory act. He reports better. Says he has no pain. Mutters. Considerably deaf. Eyes suffused. No subsultus. Tongue dry and coated. Sordes about the teeth. Tongue readily protruded. No nausea. One dejection during last twenty-four hours. Pulse 130. Respirations 6 or 7. Surface warm and moist."

Died at evening.

At the autopsy, in this case, the larynx was carefully examined to ascertain if œdema, or any mechanical obstruction existed. None whatever was found, and the antero-posterior diameter of the glottis appeared to be unusually large.

CASE IV. — Spasmodic inspiration and coma occurred in this case, on the *sixth* day of the febrile career, and death on the following morning. Up to the evening of the day on which the above symptoms became developed, there were no circumstances in the history of the case denoting immediate danger. The disease, in fact, appeared to be unusually mild. The following is the morning record on the *sixth* day: — "Reports better. Slept considerably. Aspect improved. Somewhat deaf. Mind not dull. No subsultus. Nausea and vomiting frequently. Tongue covered with a thick yellow coating. Some sordes. Herpetic eruption on lips. One dejection

Respirations 16, and rhythm normal. Slight cough. Moderate capillary congestion. No suffusion of eyes. No epistaxis. Skin mellow. No perspiration. No meteorism. Slight abdominal tenderness. Somnolent. Pulse 96 and well developed."

Evening. "Has spasmodic inspiration and cannot be aroused. Pulse feeble and frequent."

Seventh day. "Died at 4, A. M."

Typhoid. CASE I.—Disorder of the rhythm of respiration was first noticed in this case on the thirteenth day of the febrile career. Up to that time the symptoms denoted a mild grade of disease. The following is the record on the morning of the *thirteenth* day:—"Aspect the same. Passed the night quietly, without delirium. Says that he has pain in the umbilical region, but it is not sufficient to keep him awake. No subsultus. Tongue dry and thickly coated. Sordes on teeth. Pulse 108. Slight capillary congestion. Respirations 26, rhythm normal. Slight cough, without expectoration. Nineteen rose spots. Moderate meteorism and abdominal tenderness."

On the *fourteenth* day the record is as follows:—"Yesterday morning this patient was observed by the Sister nursing him, to catch his inspirations in taking drink. In the afternoon Mr. Smith, the resident student, observed that the inspirations were slightly spasmodic. A blister was immediately applied to the nucha. This morning the respirations are 16, the inspirations slightly spasmodic. He can with great difficulty be roused. Up to the present time he has been able to swallow without much difficulty, and he can now be made to swallow but with considerable difficulty. He began to grow dull last evening but was easily aroused. The pulse was then 114 and well developed. He was found this morning apparently moribund. Pulse 132, feeble."

Died at 1, P. M.

CASE II.—In this case a sudden change for the worse, eventuating speedily in coma, with spasmodic inspiration, took place on the seventh day of the febrile career. There were evidences of pneumonitis on both sides in this case, but, aside from this, the symptoms did not denote great gravity of disease. On the *sixteenth* day the record was as follows:—"Not dull. deafness much diminished. Eyes not suffused. Tongue dry, and moderately coated. No sordes. One dejection in past twenty-four hours. Pulse 110. Respirations 42, rhythm natural. Frequent cough, with slight expectoration. Crepitant rale on both sides posteriorly."

Seventeenth day. "Last evening she became suddenly worse; restless, groaning frequently; perfectly rational, not somnolent, declared she should

not live till morning. About an hour afterward the pulse was 122, full; moderate capillary congestion; appeared quite comfortable; no change of rhythm of respiration; talked rationally. This was at 8½ o'clock, P. M. At 10 o'clock she was comatose; respirations 23, and the *inspiration spasmodic*. Surface warm and perspiring. *Deglutition difficult*. The nueha was scarified, and strong aqua ammonia applied, after which she became considerably aroused. In about half an hour afterward, she relapsed into the comatose state, and died at 3, A. M., of the following morning.

In the autopsy of this case the larynx was carefully examined, but no appearances of cedema were present.

These were all the instances in which this aberration of respiration was present in a marked degree. In several other cases there was some approach to a spasmodic inspiration. Having been led by the observations of the previous year to regard this symptom as a precursor of coma, the cases were closely watched with reference to its occurrence, and whenever it was observed in ever so slight a degree, a blister was directly applied over the nape of the neck. This measure appeared in several instances to determine the return of normal rhythm, and prevent the development of the comatose state. That it exerted any such influence, however, is of course only a matter of conjecture.

The circumstances connected with this symptom in the cases in which it was observed in the present collection, and in the cases upon which the second Report was based, certainly seem to invest it with considerable importance. In the last Report I ventured to indulge a hypothesis respecting the pathological condition upon which it depends. I am not, however, prepared at this time, more than when that Report was written, to offer any positive proof of the correctness of the hypothesis.

SECTION EIGHTH.

Symptoms referable to the circulation.

In stating facts referable to the circulation, I shall give simply results, without the details presented in the two previous Reports.

The pulse was enumerated once, and sometimes twice daily in all the cases, during the progress of the febrile career. From these data I have calculated the mean frequency in the cases individually; and from these, in round numbers, i. e. disregarding fractions, the following averages are obtained:

Typhus. The average mean frequency of the pulse in the *Typhus* cases is a fraction over 107.

Excluding the *fatal* cases, and the mean frequency in the remainder is a fraction over 104.

The mean frequency in the *five* fatal cases is a fraction over 126.

Typhoid. The average mean frequency in the *Typhoid* cases, collectively, is 99.

Excluding the *fatal* cases, and the mean frequency in the remainder is 93.

The mean frequency in the *five* fatal cases is 110.

These results concur with those developed by the previous analyses, showing in the *Typhus* group a higher average of frequency of the pulse, in the cases collectively, in the fatal cases, and in those ending in recovery. It will be perceived, also, on reference to the corresponding sections in the two former Reports, that the results, irrespective of this comparison of the two types, are not very discordant.

The greatest degree of frequency of the pulse observed during the febrile career, and the highest mean in any individual case, in this, as well as in the preceding collections, are found in the *Typhus* group. The greatest degree of frequency observed in any case not proving fatal, was 156. So great frequency was noted in but one case. The next highest in frequency, was 148. The next, 144. In several cases the pulse reached 140.

The greatest frequency noted in any of the fatal cases, was 160.

The maximum of the mean frequency in any of the *Typhus* cases ending in recovery, was 124, and in the cases ending fatally, 148. The lowest mean in any of the cases ending in recovery was 86, and in the fatal cases 106.

In the *Typhoid* group, on the other hand, the greatest degree of frequency observed in any of the non-fatal cases was 132. So great frequency as this occurred in but a single case, and it diminished at the evening of the same day to 100. The next highest was 128, and the next 116. In the fatal cases the highest point reached was 138. The maximum of the mean frequency in any of the *Typhoid* cases not proving fatal, was 103, in the fatal cases 117. The lowest mean in the non-fatal cases was 80, in the fatal cases 100.

All these results show a marked difference between the two types in the amount of influence exerted by the disease on the circulation.

In this collection, as heretofore, the cases of either type exhibited striking diversities in the frequency of the pulse, and striking fluctuations in this respect were often observed in different periods of the febrile career in the same case.

A fact which is mentioned in the former Reports was in a few instances noted in this collection of cases, viz., a diminution of the frequency of the

pulse below the average of health, occurring about the time of convalescence. In one case it fell to 54; in another to 56, and in two cases to 68. All these cases were of the *Typhus* type.

Capillary congestion, evidenced by redness of the surface, disappearing on pressure, and returning more or less quickly after the pressure was removed, is noted in the histories of all the cases save two (one of *Typhus* and one *Typhoid*) and in these excepted cases nothing is stated relative to this point. This congestive redness was always more marked on the face than in any other part, and sometimes confined to that situation.

A point of disparity between the two types relates to the degree in which this symptom was present. The congestion, as a general rule, was greater in *Typhus*, frequently giving a dusky or dingy hue to the countenance.

Arranging the variations in three grades distinguished, respectively, as *considerable*, *moderate* and *slight*, and comparing the cases of *Typhus* and *Typhoid*, as regards the number of instances exemplifying each grade, and the results are as follows:—

Of *forty-one* cases of *Typhus*, in *fifteen* the capillary congestion was *considerable*; in *twenty-five* it was *moderate*, and in *one* case only was it *slight*.

Of *thirteen* cases of *Typhoid*, in but *one* case was it *considerable*; in *nine* cases it was *moderate*, and in *three* *slight*.

[The Report will be concluded in the number for July.]

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Discussion on Anæsthesia. By Members of The Philadelphia County Medical Society.

We copy the following article entire, notwithstanding its length, from the *Phil. Med. Examiner*; in the first place, because the subject is one of great interest and importance; and, in the second place, because it contains a pretty full expression of views held by some of our brethren in Philadelphia, which

are somewhat antagonistical to the enthusiasm exhibited in other places. We confess we were taken somewhat by surprise by the statement that anæsthetic agents have never been administered in the Pennsylvania Hospital. Granting that it is by no means settled that the use of these agents is unattended by danger, how are the limits of their proper application to be determined? Plainly, by accumulated observations. The policy of the Penn. Hospital would seem to be to wait until observations made elsewhere have fully settled this question. This kind of conservatism of course prevents the institution from contributing facts toward the settlement of the merits of this or any other proposed improvement in medical and surgical practice. The remarks by Dr. Patterson on this subject are plain and to the point. — *Ed.* BUFFALO MEDICAL JOURNAL.

Dr. Parrish requested that Dr. Patterson would give to the Society the result of his observation on the use of anæsthetics in Europe, he having lately returned from a visit to that country.

Dr. H. S. Patterson said that he should have much preferred to hear a continuation of the discussion of the previous meeting by the gentlemen who so ably participated in it. He was unfortunately not present at that meeting, but he had read the report of its proceedings with much interest and instruction. But, being thus publicly challenged by Dr. Parrish to speak on the subject, he felt it his duty to contribute his mite to the discussion. He had until recently felt, as the great body of the profession in Philadelphia feel, in regard to the anæsthetics. His position had been that of doubt, of distrust, and avoidance of them as a dangerous innovation. These agents came to us in a very questionable shape originally. When the first report of anæsthesia in surgery reached us from Boston, it came, not only startling us by its novelty and the magnitude of the change in practice it contemplated, but also shocking us by its violation of our ethical notions and the savor of empiricism that hung about it. The new agent had a new, high-sounding and unscientific name, and there were rumors of a patent-right to be secured to its discoverers. But when it was shown to be plain sulphuric ether, which any chemist could manufacture and any practitioner administer—when the name of Letheon disappeared, and the letters-patent were no longer heard of—especially when indisputable evidence of its power and utility reached us, we should have given the subject a more favorable reception and examination than we have done. But the mass of resistance to the use of anæsthetics has been more obstinate and impregnable here than any where else in Christendom; and our hospital—alone among great hospitals—has never permitted their employment even in the most painful operations. Participating in these feelings, and never having administered either chloroform or ether in his practice, Dr. P. visited Europe during the past summer, and one of the objects he proposed to himself was to obtain, if possible, more light upon this much mooted question.

In Edinburgh he had the opportunity of seeing and hearing a great deal of the use of chloroform, and it was in regard to the experience of that city that he desired mainly to speak. The undeniable and most important fact is,

that in Edinburgh, where chloroform has been used with a degree of freedom, and with a frequency and extent elsewhere unknown, not a single unpleasant circumstance has attended its administration. Every obstetrician, surgeon and dentist in that capital uses it, without exception. Every teacher of obstetrics advises its employment, and gives it to his patients in labor, some in all cases and others only in cases of unusual duration and severity. No surgeon thinks of performing a serious or painful operation, one likely to be attended with great suffering or severe shock, without bringing his patient under its influence. Thousands of patients have inhaled it to full anæsthesia; the chemists manufacture and sell it daily in large quantities; and no one has been injured or even made ill by it. There does not remain a practitioner in Edinburgh opposed to its use. An infinite amount of pain has been relieved, great suffering has been prevented, the dread of operation has been in a great measure removed, and no harm has been done to a single individual. There is no denying these facts. There can be no supposition of concealment or unfair dealing. The character of the gentlemen in whose hands its employment is, of itself precludes the idea as one not to be entertained for a moment. In a large proportion of the cases, if not in all, concealment would be impossible were it attempted. Dr. Simpson introduced the chloroform to our notice as a therapeutic agent, and he bears the unqualified testimony that he has never seen it do any mischief whatever to his patients. Dr. S. is a man whose word, in a matter of medical experience, must be taken without qualification. He has not only the candor of an honest man, but also the discriminating judgment which is not readily deceived, and the calm deliberateness which is not easily led away by the enthusiasm of a hobby. By his kindness Dr. P. was enabled to witness the administration of chloroform to several patients, and to converse with others who had used it in childbirth, some for a considerable length of time. In some cases Dr. P. has maintained its effects for several hours. This powerful agent was always administered with due caution, but with a fearless hand. The patient was thrown into a condition of perfect insensibility, and underwent operations otherwise painful, without any apparent sensation, and certainly with no subsequent recollection of suffering. The manner of Dr. S. indicates the most entire confidence in both the safety and utility of his practice. The lying-in women whom Dr. S. saw were all in excellent health and spirits, and Dr. P. thought that their average "getting up" was shorter and better after chloroform than without it. All asserted that they awoke from the slumber without the slightest after-sensation or recollection of pain, without head-ache or vertigo or nausea, and precisely as after a refreshing sleep. One spirited and intelligent lady, with whom Dr. P. had the honor to converse, roundly asserted that the ladies of Edinburgh *would* have chloroform whether their doctors gave it to them or not; that she would not approach her confinement without it, and that the knowledge that this invaluable boon was within her reach, had taken away all the trembling apprehension which had previously made her pregnancy a period of dread and suffering.

In other parts of Great Britain and on the continent, the anæsthetics are not used so freely as in Edinburgh, but still they are resorted to in all severe surgical operations, and very frequently in obstetric practice. There are very few who use them in every case of midwifery to which they are called, but there are still fewer who do not use them in some cases. No surgeon of any note refuses their employment altogether. They are used in every hospital

of London and Paris, more or less. Dr. P. was amused by the ardor of a gentleman connected with St. Bartholomew's, who, in a conversation on the subject, enthusiastically declared that all the other benefits conferred on mankind by the discovery of America, (bark and our democratic liberty included,) were eclipsed by the inestimable addition of anæsthetics to the treasury of the healing art.

The substance used exclusively in Edinburgh, and most generally elsewhere, is chloroform. The accounts we have of unpleasant or fatal effects from it, do not deter them from administering it. Dr. P. saw no sulphuric ether given, and met with no physician who employed it. The objections to its use are its highly stimulating qualities, (increasing the risk of fever and hæmorrhage,) its unpleasant and exceedingly persistent odor, and its liability to leave a disturbed condition of stomach with gastric uneasiness and acid eructations. The question of danger from chloroform he would not discuss at present, except to express his conviction that it is ascribable, in all cases, to some error in the method of administration or in the management of the patient or of the inhaling apparatus. Dr. Simpson gives it on a napkin or handkerchief, lightly folded in a funnel shape and held near the mouth. In obstetrical cases he gives it on the approach of each pain, removing the napkin the moment the relaxation of the patient's muscles shows that the pain is passing off, and not reapplying it until her uneasy motions indicate the approach of another pain and that it is beginning to be felt.

In view of all these circumstances, Dr. P. inquired whether there is not reason to believe that we in Philadelphia have been a little unreasonable in our opposition to these agents. Certain it is, that they alleviate much suffering for which we have hitherto had no remedy, and they have in a measure taken away the curse of child-birth, and deprived surgical operations of much of their horror.

Dr. Candie was inclined to believe, that a very considerable change, in the opinion of European practitioners as to the employment of anæsthetics, had recently taken place. That a much greater degree of caution in their administration was now inculcated, and that the circumstances under which a resort to them solely to prevent pain, is proper, are admitted to be much more restricted than was the case upon their first introduction. This, he inferred, from the very different tone assumed, of late, by the medical journals of Europe when treating of anæsthetics and of the cases in which their administration is justifiable. Much of the wild enthusiasm by which the propriety of a resort to them, in almost every case attended with pain or suffering, was insisted upon by their early advocates, has disappeared. It is now admitted, on all hands, that the leading anæsthetic (chloroform) is an active poison, and that to prevent disastrous consequences following its administration, the utmost caution must be observed; that we are not always able to determine the effects that may be produced, in different individuals, by the inhalation of the same quantity of the article; that the induction of anæsthesia is not justifiable in trifling operations, or in ordinary cases of perfectly natural labor; that, even in those cases in which anæsthesia is considered proper during parturition, it is unnecessary to push it to the extent of inducing an entire abolition of consciousness; and finally, that there are a number of circumstances the presence of which forbids absolutely, in all cases, a resort to it.

Anæsthetics have been in use for too short a period to enable us to arrive at any very certain conclusions in regard to their real value as therapeutic

agents—to determine accurately all the cases and circumstances to which they are adapted, or to lay down the proper rules for their safe administration. It unfortunately happens that, whenever any important remedy is announced, instead of a dispassionate examination of its properties, and a cautious testing of its curative powers, it is at once seized upon by a set of enthusiasts, who laud it far beyond its deserts, and ascribe to it remedial virtues under circumstances where it is afterward ascertained to be powerless for good, if its effects be not even positively injurious; and if the agent be one of great activity, it is only after the infliction of much injury from its injudicious employment, that it attains its proper place upon the lists of the *materia medica*. When iodine was introduced as a remedy—to believe those who first described its properties and therapeutic powers—it was to remove, at once, consumption from the list of incurable diseases, and to arrest all scrofulous affections, with as much certainty as the bark or quinine is known to arrest the paroxysms of intermittent fever.

But, in reference to no remedial agent, has this wild enthusiasm been displayed to as great an extent perhaps, as it has in regard to anæsthetics. By their agency pain was to be expunged from the catalogue of human evils—child-bearing was to be divested of its agony, and the knife of the surgeon was to remove the diseased member from the body without the patient suffering a single pang, and this without the slightest danger or inconvenience either immediate or remote.

No wonder need be experienced that anæsthesia should have secured from the very first so many ardent advocates, that it should have been recklessly resorted to in almost every operation, in every case of labor, and in almost every disease attended with pain or acute suffering of any kind. Nor has, as yet, the occurrence of death after death, directly attributable to the influence of the agents employed in its production, been a sufficient warning to deter all from a resort to these, merely for the prevention or relief of pain.

By the physician, pain is to be studied with very different feeling from those prompted by a morbid sensibility. Its removal may not, under all circumstances, be advisable. Though invariably an evil, still, cases may certainly occur, in which it would be better to allow the patient to endure, for a short period, even severe suffering, than to induce in him a state of complete insensibility. It was the opinion of Dr. Physick, that, generally speaking, important operations are more successful in those patients who give full vent to their feelings, than in those, who, by a powerful effort of the will, suppress all indications of suffering. And there is some foundation for the inquiry whether, even admitting that, by the employment of anæsthetics, surgical operations may be divested of pain without any immediate danger to the life of the patient, the ultimate result of such operations is as favorable, upon the whole, as of those in which anæsthesia has not been induced.

A comparison that has lately been made by an acute and accurate observer, in our midst—based on the statistics of several large hospitals—of the results of operations in the same institution, before and after the introduction of anæsthetics, as well as the results of operations performed in those hospitals where anæsthetics are now invariably employed, with that of the same class of operations performed in the Pennsylvania Hospital, without the induction of anæsthesia, would lead us to suspect that the abolition of pain by anæsthetic agents does actually exert an unfavorable influence upon the result of operations; an

opinion at which Dr. Porter of the U. S. army had arrived from his experience of the use of these agents, during the war with Mexico.

In obstetrics, Dr. Ramsbotham, in the last edition of his *Obstetric Medicine and Surgery*, has denounced the employment of anæsthetics, excepting in some few exceptional cases, while Dr. Chowne, in a late discussion before the London Medical Society, remarked, that he had seen most disastrous consequences follow the administration of chloroform—not only simple after-consequences, but serious mental disorders. The fact of the occurrence of insanity from the use of chloroform is insisted upon by Dr. Webster, who has adduced several cases in which such a result occurred; his statements are of a character that forbid our treating them with entire indifference.

But even if we could, with positive and invariable safety, prevent the pains of parturition by the employment of anæsthetics—would it always be proper to do so? There may be some practitioners of such consummate skill in the application of obstetrical instruments, who, directed solely by their knowledge of the anatomy of the pelvis, and the position of the head, can apply them, without any risk of injury to the organs of the female, as well while she is in a state of entire unconsciousness as when she is in a condition to advise him, promptly, of any pain he may inflict upon her. Dr. Condie, however, very freely confessed that he preferred always, in the application of instruments during labor, that the susceptibility of the female to painful impressions should be unimpaired, as an additional safeguard to prevent her suffering injury from maladroitness on his part.

Much surprise has been expressed by Dr. Parrish, that the comparatively few cases in which fatal effects have resulted from the employment of anæsthetics should be used as an argument against a resort to them in surgical operations and during labor, when the same objection will equally lie against the administration of any of the more potent and useful articles of the *materia medica*. But is it not far more surprising that the gentleman should not see the very material difference between the employment of a dangerous remedy to *save life*, and a resort to a similar remedy *merely to destroy pain*, in a case that we have every reason to believe, so far as the safety of the patient is concerned, will do very well without it? It will not do to point to the 9000 operations safely performed at St. Bartholomew's Hospital under the influence of anæsthetics, in evidence of the propriety of resorting to it, as a mere preventive of pain—for the one single case of death acknowledged to have occurred in that institution from the use of anæsthesia, independently of others of which we have been informed, is sufficient to counterbalance all the good supposed to have resulted from it, by the mere abolition of pain, in the 9000 cases. The life of one patient in 10,000, ought not to be sacrificed to obtain for the remaining 9999, relief from what is, in fact, in most cases, a very short period of suffering.

Dr. C. remarked, that he did not wish to be considered as altogether opposed to the employment of anæsthetics as therapeutic agents. He was satisfied that their introduction had armed us with a most potent and valuable means of relief in a numerous class of cases. From their internal use, by inhalation, as well as from their application externally, he has derived the most beneficial results in many instances: he has seen them arrest, immediately, the most violent paroxysms of puerperal convulsions, allay promptly the intense suffering induced by cramp of the stomach and intestines, and that

attendant upon neuralgic affections of the internal organs, and of various portions of the surface. He could even conceive of operations in which the risk of danger from their employment, should not deter us from availing ourselves of their pain-abolishing effects. An operation may be necessary to save life, but one, at the same time, attended with so much and so long continued suffering, as to cause the patient to hesitate between its endurance and the *certainty of death* should it be omitted or delayed. In such a case Dr. C. would not hesitate to recommend the induction of anæsthesia—believing that the risk the patient will incur from it will be fully compensated by its enabling him to undergo an operation, the pain of which it would, otherwise, be scarcely worth enduring to purchase the few additional years of existence it may possibly bestow. Cases too, occur, not necessarily fatal, of violent paroxysms of pain, occurring periodically, at shorter or longer intervals, and during a very long period of time, where relief from suffering, by which life is rendered a burden, is cheaply purchased by any remedy, even one, the exhibition of which may be attended with considerable danger. Such cases are, unquestionably, proper ones for the employment of anæsthetics; under many other circumstances also, they will no doubt be found useful and judicious remedies—even although no other good effect should attend their exhibition than the relief of pain.

Dr. Parrish remarked that he was surprised to find the statistics of hospitals introduced here as opposed to anæsthesia in large surgical operations. The basis of the calculation, as I understand it, is a comparison between the results of amputations in the hospitals of New York and Boston, since the introduction of anæsthetic agents into these institutions, and those of the Pennsylvania Hospital, where they are not used. Dr. Condie, who generally speaks with such accuracy, on questions when facts are concerned, has not brought forward the figures by which he claims to sustain his position, but has made a general statement that a comparison between these institutions is altogether unfavorable to the use of anæsthetics.

Now, will any one undertake to say that a slight difference in the percentage of mortality, after operations of any given class, running through a period of only a few years, and without taking into account all the attending circumstances, the comparison being made between three institutions, and those not of the largest class, is to be taken as evidence against the use of an article, which has been employed thousands of times in the largest hospital establishments in the world?

Why, it is well known that the statistics of a hospital bearing upon any given point, may vary very much from year to year, and that a series of unfortunate results in one or two years will influence very materially the general results for a series of years. Thus an epidemic erysipelas may prevail, and carry off a large number of patients who have been subjected to capital operations; and if the institution should be visited with such an epidemic for several seasons in succession, the mortality may become so heavy, as to keep it in the rear of the more favored institutions for a long series of years. There are, too, various other circumstances which may influence the mortality, entirely independent of the use of anæsthetics, and which in my judgment would render it very unsafe to draw conclusions, based upon the experience of two or three institutions during a period of a few years. I apprehend, also, that a difference will be found in the percentage of mortality after any given capital operation, during a series of years, amongst the best hospitals of this

and of other countries, both before and since the introduction of anæsthetic agents, showing that other causes are constantly varying the results.

But if a difference in the mortality of hospitals prior to, and since the introduction of anæsthetic agents, is to be taken as an argument for or against them, we have a much more extended field of comparison than that offered by the three chief hospitals of this country.

The large hospitals of Europe furnish much more reliable data; and it so happens that the advocates of anæsthesia point triumphantly to the statistics of these institutions as affording ample evidence of a diminished mortality after the great capital operations, since the introduction of these pain-destroying agents.

In the elaborate work of Professor Simpson, on anæsthesia in Surgery and Midwifery, the reader will find two chapters devoted to the question, whether anæsthesia increases or decreases the mortality attendant on surgical operations. The author has here collected the statistics of mortality after the large amputations, in different countries, as derived from official sources, including Phillips' tables, which comprise not only the results in hospital practice, but in the private practice of physicians in Great Britain and in other countries, so far as recorded in the periodical literature of these countries. He has also availed himself of Malgaigne's tables of results from the Parisian hospitals, forming altogether one of the most complete and extensive collections of cases yet published. The percentage of mortality after large amputations, calculated from these data, which were formed prior to the introduction of anæsthetic agents, and then compared with the results after the same operations with anæsthetics, as derived from the British and Parisian hospitals, forming a series of upward of 300 cases, up to the time when Professor Simpson wrote.

The conclusion arrived at is, that the mortality since the introduction of anæsthesia is sensibly diminished, and the argument in favor of anæsthetics thus derived is considered by Dr. Simpson as most cogent and convincing. I can only refer the members to this array of figures on the ether side of the question, and ask them to compare them with those now brought forward on the other side, and see which is the stronger.

I believe myself that there is some degree of uncertainty in this method of calculation, but if it is relied on, etherization has evidently the advantage.

In regard to the recent unfortunate death from chloroform at St. Bartholomew's Hospital—it is certainly against it—though it is only one death in over nine thousand cases, in which the article has been administered there; still, this with the others which have been reported, shows that chloroform is a most potent agent, and that it should not be given on trivial occasions, and without great caution, while at the same time one death in ten thousand should not cause us to abandon it. I myself have always preferred the ether, believing it to be safe and reliable, and should not use the more powerful agent, where this answered the purpose.

Dr. Hays stated that he was not disposed to discuss, on the present occasion, the question of the propriety of using anæsthetic agents; but the statements made this evening by the gentleman who opened the debate, relative to the general and indiscriminate use of chloroform in Edinburgh, without a single instance of injurious effects resulting therefrom, were calculated, he conceived, to convey the very erroneous impression, that no risk attended the employment of that potent agent. He felt impelled, therefore, by a sense of duty, to put in a word of caution. Nearly thirty cases in which death has

been produced by the administration of chloroform have been recorded with their full details; and perhaps double that number have been casually referred to in debates before different learned bodies, but the particulars of which have never been published. Not a few cases also of death after the administration of chloroform have occurred, in which, he believed, the result was chargeable to that agent, though ascribed to other causes. Be this, however, as it may, he thought that undoubted cases enough might be brought forward to demonstrate that the employment of chloroform was not devoid of danger.

Some of the recorded cases of death from chloroform have been ascribed to the article employed being impure; others to carelessness in its administration; and others again to some constitutional peculiarity or organic disease which should have forbid its employment; and thus it has been attempted to prove that in all these cases the fatal result might have been obviated by proper caution.

A case of death from chloroform, however, has very recently occurred, which does not allow of any such loophole to escape, from the fact that danger always attends the use of chloroform. This case occurred—where of all places it should have occurred in order to teach a salutary lesson of caution—in St. Bartholomew's Hospital, London—where it had been boasted that chloroform had been used in between nine and ten thousand cases, in not one of which "has its employment left a stain on its character as an innocuous agent of good."

The subject of this case, was a young man only twenty-three years of age, affected with aneurism by anastomosis implicating the ear and a portion of the scalp behind it, and which had commenced when the patient was four years of age. He was admitted into St. Bartholomew's Hospital, and Mr. Lloyd determined to attempt its cure by ligating the vessels supplying it. Chloroform was given to the patient, and he was kept under its influence for half an hour, the operation having proved a tedious one. He recovered from its influence without any ill effects. The tumor was not, however, entirely obliterated. A little over a month subsequent to this operation, Mr. Lloyd found, on examination, an artery between the angle of the jaw and mastoid process, which pulsated strongly, and on compressing it so as to arrest the circulation in it, the pulsation in the tumor entirely ceased. Mr. Lloyd determined to apply a ligature to it. Chloroform was again administered by the careful and experienced assistants of the hospital, and the article used was from the same bottle which had been administered in the previous operation. In from five to ten minutes he came under the influence of the article, and Mr. Lloyd commenced the operation. Scarcely had he incised the skin when his assistant informed him, that the patient's pulse was sinking, and before any restoratives could be applied, he ceased to breathe. Every means that could be devised were resorted to, in order to re-animate him, but without success. On post-mortem examination, no morbid organic change in any organ was found to explain the fatal occurrence; but the blood appeared to have been poisoned. "It was fluid, and it remained without coagulation after its escape from the heart and vessels. It had also a brownish purple hue, much like that which is commonly observed in the spleen; none of it, when thinly spread out, presented the ordinary dark, black or crimson hue of venous blood." But for this untoward administration of chloroform, the patient might have lived to the usual term of human existence. Here, then, is a case in which no human foresight could have provided against the fatal

result. The chloroform used was pure, and had been employed before without any ill effects; there was no idiosyncrasy in the patient forbidding its employment, for he had previously been under its influence with apparent impunity; that there was no want of caution and skill in its administration, we have the assurance in the fact that it was given by the experienced assistants of the hospital; and that there was no organic disease forbidding its employment, was demonstrated by the autopsy. If this case is not calculated to prove that danger attended the administration of chloroform, Dr. Hays said he would be at a loss to know what kind of evidence gentlemen required for that purpose.

In reply to an inquiry made by Dr. Parrish, whether he (Dr. Hays) considered the case he had alluded to forbidding the use of chloroform in every case, Dr. H. stated, that were he compelled to undergo a very painful and protracted operation, he would himself take an anæsthetic, preferring to run the risk rather than to endure long and severe suffering, and he would allow his patient the same option, if, after fully explaining to him the state of the case, he should desire to do likewise. But to resort to chloroform for every trifling operation, and in all cases where the slightest and but temporary pain was to be endured, he conceived not only to be injudicious, but actually criminal.

P. S. Since these remarks were made, two more cases of death from chloroform have occurred, one in Boston, the other in New Haven.

Dr. H. S. Patterson remarked that the meeting had got pretty well into the discussion of the question on its merits, when Dr. Hays brought down the St. Bartholomew's case upon us as a sudden extinguisher, and it appears we cannot get beyond it. Around that all the discussion seemed now to centre, and it must be got rid of before any further progress could be made. He did not know that he could get rid of it, but at all events, it could be looked fairly in the face, and its real importance determined. The idea that arose in his mind while Dr. H. was speaking, was that this, like other cases of alleged death from chloroform, came with an air of mystery about it. A mischief has been done, somehow or another; life had been extinguished in some way; but the only fact now apparent is that the chloroform has done it. Let this be admitted, and it does not necessarily follow that the agent in question cannot be used at all therapeutically. The conclusion is greater than the premises will bear. The superstructure is much too large for the foundation. The fact seems to be that chloroform may be—indeed has been—inhaled in such manner as to produce death. Every article of the materia medica of any value, is toxic or at least injurious in some method of exhibition or some dose. Generally, the toxic is in the direct ratio of the therapeutic power. The potency which is curative in its judicious employment, may be deadly otherwise applied. The mere fact that a medicinal agent is capable of destroying life is no argument against its use. Is there any substance of acknowledged power in our pharmacopœia that may not do mischief? Are there not many that have been fatal to human life, not only when used criminally, but also injudiciously employed, although with the best intentions? He (Dr. P.) is not one of those who would willingly uncover the nakedness of the profession or expose its shame, but he would ask whether chloroform has destroyed more human lives than opium, even in its medical use? He would only refer to the old treatment of *delirium tremens* by heroic doses of that drug. He had seen patients die in that disease, in the Pennsylvania

Hospital and in the Alms-House, and who would say whether the condition which preceded death was coma or fatal narcotism? He had his own convictions on the subject, and they were such as to lead him to seek a mode of treatment for that affection without opium. But, admitting all this, does it prove that opium should not be used? On the contrary, opium remains an indispensable portion of our means of cure in innumerable cases. If a substance is poisonous, it is so in certain doses, in a particular mode of exhibition, and according to fixed and ascertainable laws. We can determine by observation what functions it affects, in what quantities, and in what way. We do not hesitate to use poisons much more deadly than chloroform is alleged to be by its most vehement opponents, because we know their action and can regulate it to good therapeutic ends. If a patient should die of a dose of hydrocyanic acid or strychnine medicinally given, there would be no hesitation in concluding that there had been a gross error in the dose or in the manner of exhibition. Why refuse the application of the same rule to chloroform? It may be asserted that its poisonous influence is so subtle and so fatal that it cannot be regulated. But this is disproved by thousands upon thousands of cases of its successful administration. If it is poisonous at all, it is so only in a certain quantity or by a certain rapidity of introduction, which can be clearly ascertained, scientifically regulated, and securely guarded against. Let us now look, with a more thorough scrutiny, into the St. Bartholomew's case, which has been thrown in our way as an impediment not to be got over. The first fact to be noticed is, that this same patient, not a month previous, was kept under the full influence of chloroform for twelve minutes, during a painful operation, and without the slightest inconvenience or interruption to his recovery. There was plainly no "idiosyncrasy" here. It is proved that chloroform could be administered to that very patient with safety and with the most beneficial results. On the last and fatal occasion, he inhaled the vapor of chloroform for a much shorter time, and died before the first incision was completed by the knife of the surgeon. Now, can any man believe that the chloroform was administered in precisely the same way, in the same quantity, and to the same extent, as on the former occasion? Like causes produce like effects. The man who takes a grain of opium to day with beneficial effects, will not be fatally poisoned by a grain of opium a month hence. The probability is that in the first instance the chloroform was properly administered with a due admixture of atmospheric air, while in the latter it was hastily presented, of full intensity and undiluted. The quantity to be estimated is not altogether that poured upon the napkin or introduced into the inhaling apparatus, but that actually received into the lungs of the patient, and absorbed from their mucous membrane. A better case for the illustration of the principle just laid down could not be desired. The blame rests with the erroneous mode of the use, and not with the substance used. As for the mere allegation of toxical power, Dr. P. would give very little for a medicine that could not produce such effects in any case. He suspected the efficacy of every agent whose powers were so feeble as not to render it noxious in its inappropriate or immoderate employment.

As to the remarks made by Dr. Condie in reference to the enthusiasm of the advocates of new measures, Dr. P. thought that such a charge in reference to anæsthetics was singularly out of place in Philadelphia. The error here seemed to be all on the other side. He did not wish to complain of the conservatism of the profession here generally. Their cautious skepticism

in regard to medical novelties had its great and lasting benefits. But these anæsthetics are no longer a novelty. Their precise value has not been definitely determined, nor have we settled all the laws that should regulate their use; but that they have great and important uses can no longer be doubted. It appears certain that they will become a fixed portion of the armament of the surgeon and the obstetrician. Why should we in Philadelphia alone occupy this position of dogged resistance, and refuse to receive them? Dr. Condie has warned us against the enthusiasm of novelty. Dr. P. acknowledged the truth and value of his remarks. There are *quidnuncs* in the profession as well as out of it, and they will run wild after new hobbies. But he would remind Dr. C. that this is not the only dangerous enthusiasm. There is another that, in its relation to other matters, has been recognized and pretty well comprehended in our country, where it has received the generic title of *old-hunkerism*. It consists in an obstinate conservatism, which brands every new thing as an innovation, in the bad sense of the term, without waiting to see whether it may not turn out an improvement. It rests content with old things, it wants no progress, and it resists all new things as essentially evil or mistaken. He was afraid that there was a leaven of the enthusiasm of *hunkerism* in this resolute opposition to the anæsthetics. At all events, the truth must soon make itself manifest, and the fact of the matter be established. There can be no doubt that anæsthesia will become, when better understood, a well regulated and well established portion of our practice, and the most the Philadelphia profession can claim will be the merit of having been the drag on the wheel that prevented a too rapid attainment of the goal.

Dr. Bell, in reply to a question from the President, (Dr. Jackson,) said that Dr. Mussey, his former colleague in the *Ohio Medical College*, and the *Commercial Hospital of Cincinnati*, uniformly made use of a mixture of ether and chloroform for the subjects on whom he operated; and without, it is believed, in any case, sinister results.

Dr. Atlee said: There is one point in the case mentioned by Dr. Hays that, perhaps, may have been overlooked. If we take up any book on surgery, and refer to the subject of operations on the neck, we will find that instantaneous death occasionally results during the operation, and that this is attributed to the entrance of air into the veins of the neck. The question, then, would naturally arise, Could such an occurrence have taken place in this unfortunate case, and death have resulted from this cause, and not from chloroform? Always in dread of such an event, I have never administered anæsthetics in important operations on the neck, lest death accruing under such circumstances, might be erroneously attributed to chloroform, and thus impair the character of a very valuable agent.

Dr. Page remarked that, notwithstanding the current of the argument, the opinion of all present on the subject of the use of anæsthetics, seems, "like a handle of a jug, all on one side." All of the gentlemen, including those who spoke on the negative side of the question, admit that under certain circumstances these agents may, and even ought to be administered, while under other circumstances they should be repudiated as likely to be productive of injury. What then are the circumstances requiring their employment? and the answer is, who can tell? The opponents of their indiscriminate use employ them under very opposite conditions. We are told by some that they should not be applied in cerebral excitements, in disease of the heart, and in

pulmonary affections, &c.; but Dr. Condie has used the anæsthetic in convulsions with marked benefit; and in asthma and difficulty of breathing, ether has been highly extolled; and it is very far from probable, that some of the many thousands who have been placed in a state of anæsthesia for surgical operations, the extraction of teeth, &c., and who have done well, should not have been at the time laboring under severe cardiac affection. Now, for my part, I consider that the chief use of the agent, and the principal cause requiring its administration, is for the relief of pain; but to its indiscriminate use I am as much opposed as any member of the profession. I regret that so few of the members here speak from the book or card. They give us the experience and observations of others and not of themselves, and we know that there are few subjects on which there has been so much written and to so little purpose, as on anæsthetics, within the last few years, except perhaps, cholera. With me, after a principle is once adopted, it matters not where it comes from, nor does it require a great accumulation of evidence for its elucidation; and I look upon it as a fixed fact that the anæsthetic may be administered whenever severe and prolonged pain would be otherwise suffered, unless strong indications exist to the contrary. None of us would recommend a patient to be placed under the influence of sulphuric or chloric ether or chloroform, for a trivial surgical operation, or for the extraction of a tooth, nor would we use them on ourselves, because the pain is soon over, and it is not worth while to run any risk; and in this sentiment I heartily concur, as risk there is; as we know that unfavorable results have followed their administration; and strange as it may seem, many of the fatal cases have occurred after the performance of what are commonly called slight operations. If I recollect aright, two fatal cases occurred in quick succession in New York; one when the operation was for fistula in ano, and the other for hemorrhoids, both very simple and every day operations in surgery; and we were thrown into the utmost consternation that human life should have been thus endangered, and could scarcely believe that the result was due to any other cause than the chloroform. But, sir, some of the most simple operations occasionally terminate fatally, without any discernible cause. I once saw a patient lose her life from the passage of a thread through a small vascular tumor in the bend of the arm; and I also saw M. Velpeau remove a small tumor from the shoulder of a woman, and she died in a few hours without any assignable cause. Now, sir, patients will die after operations, and sometimes suddenly, and the surgeon cannot, with all his ingenuity, account for the death, unless it be ascribed to the luckless anæsthetic.

But it is for the relief of pain during the performance of the more important operations only, as has been before remarked, that it should be administered, and here it not only relieves great suffering, but may have done away with past anxiety, and may remove or diminish future danger. The dread of pain is one of the most depressing and distressing influences to which the suffering patient, or one who is about to submit to a severe operation, is liable. Now if this can be removed, much is gained. The patient who has once made up his mind to the necessity of an operation, does not fear its ultimate results or consequences. If it is one which, of itself, compromises life, the mind becomes settled as to its necessity, and the great fear is of the pain which must be endured during its performance, and the great objection made is, that it will *hurt*. Now all of this feeling yields to the confidence which the anæsthetic inspires, and the common question which the patient puts and

has always put to the surgeon is, "can you not give me something to relieve the pain?" We all know how intimate the relations are between the mind and body; and if we keep in view the old adage *sana mens in sano corpore*, the whole matter will be fully understood. The mind and nervous system being placed at ease under the belief that no pain will be experienced during an operation, no matter how severe, will diminish much the dangers of what all surgeons have too well known as the nervous shock, or that condition in which patients die from the immediate effect of operations. It was formerly a common thing to hear of persons dying on the table during an operation; but who has known this to occur in Philadelphia when ether has been administered, and we are all aware that it has been given extensively and almost indiscriminately?

Another argument against its use is, that the plasticity of the blood is altered, and that the healing process will not take place so readily as when the agent has not been employed. Facts seem to prove the contrary. I have never seen the cure more tardy after the use of ether than without it, and indeed in some instances which have fallen under my observation, recovery has taken place with remarkable rapidity. I assisted Dr. Goddard in an amputation of the thigh after a gunshot wound of the knee-joint, when the injury was tremendous, and when all the circumstances were so unfavorable as to induce me to oppose the administration of ether, not because I thought that it would do harm, but because I thought that the patient would most likely die, and that the fatality of the case would be ascribed to the agent. It was however given under the advice of Drs. Jewell, McClellan, and Wilson, of Bustleton, and the only unpleasant consequences were a slight vomiting and faintness. The wound healed, and the old gentleman is now well, and an approver of ether from experience. I have performed capital operations when the patients have been under its influence, and I have not been satisfied that it has ever proved injurious. I amputated a man's arm, and on the fourteenth day he was well enough to elope from the almshouse. I have removed thighs, legs, the penis and the testicle, with the patients in an unconscious state, and they have all done well. I operated for strangulated inguinal hernia on a patient of Dr. Naudain, to whom chloroform had been previously given freely during the trial of the taxis, both by Dr. N. and myself, and at my visit on the thirteenth day, I received a message that the man had waited all the day before for me, and that he had gone out to walk. These cases and others in my own practice, besides a vast many which have fallen under my observation in the hands of others, make me believe that the agent may be often used without injury, if not with advantage. I lost a patient eighteen months since, after the operation for hernia, when chloroform was employed, but I could not ascribe the death to the agent. I have lost others both before and since, when no anæsthetic was given.

It has been said that the color of the blood is changed, and that black blood flows from the wound after the use of anæsthetics. This I have not observed. After amputations when the arteries have been secured, which is immediately done, the tourniquet is partially loosened, and then it is that the black blood flows, but it comes from the veins and not from the arteries, and the observer, who is perhaps anxious to view it in this light, mistakes the one for the other; but when the constriction above is entirely removed, all bleeding usually ceases.

In medicine anæsthetics have been extensively employed, and even by those

who repudiate them in surgery, without disadvantage, and many have been rash enough to use them freely in obstetrics.

In midwifery I have used ether with the view of promoting relaxation of the os uteri, vagina, and neighboring parts, and to induce a free secretion from *the parts*, which is the harbinger of the coming good time to the patient and accoucheur, and so far I have had abundant cause to be satisfied with its use. It promotes secretion and induces relaxation, while it does not interfere with uterine contraction or the bearing down effort. This is, I know, contrary to theory, but I believe it to be the fact. It seems, as has been well established, to act especially on and to control the voluntary muscles. Now the patient should not be placed in a state of full insensibility, but should be so far affected as not to suffer pain; and while thus influenced the sympathetic or synergic action of the abdominal muscles and diaphragm will be readily called into play by that of the uterus, which acts involuntarily. We may, indeed, say that a patient suffering no pain will readily yield to this sympathetic action, and will bear down forcibly, when without a freedom from suffering, she would be almost unable to follow the assiduous directions of her accoucheur to assist herself. A remarkable case of this kind fell under my care a short time since, in the person of a young lady in her first confinement, who had been suffering active pain from 7, A. M., to 4½, P. M., without making further progress than the dilatation of the os uteri to the size of a quarter dollar. She suffered much and cried loudly for something to relieve her. She inhaled ether; the dilatation took place speedily, *the parts* became relaxed and moist, she bore down *like a woman*, and before 6 o'clock the child was dressed and the mother bandaged up. She expressed herself as comfortable, and so she continued. I have never yet witnessed the occurrence of any unpleasant accident from the administration of ether during labor, and I have seen much suffering avoided.

We are told that it should not be given when instruments are to be applied, and for the reason that the practitioner should be advised by the patient when he is giving her pain and when he is doing injury. This is to a certain extent true, but obstetrics should teach the practitioner how to apply his instruments, and how to avoid mischief without depending entirely on the assistance given by the woman's silence or moans. Who ever yet pulled on the forceps without giving pain, and who has desisted because his patient cried out? Other indications do and should guide him; he should know when and how to operate, and should not trust entirely to his patient for the success of his practice. Nervous irritability may be mitigated in obstetrics as well as in surgery, by the timely and proper use of different remedies, and if we resort to ether instead of opium and its preparations, and other narcotics, it is because it is more efficacious and not more injurious. Every surgeon and every accoucheur will sometimes give the different narcotics. As before remarked, the full anæsthetic effect should not be induced; enough of the agent should be given only to relieve the pain, as it may have to be used for a considerable time.

One strong expression is that ether is intoxicating, but this must depend on the meaning of the term intoxication. If the soothing effect of opium, hyoscyamus, lactucarium, &c., or perchance their exciting or poisonous effects, or the phenomena attending convulsions, coma, concussion, &c., can be called intoxication, then the anæsthetic is intoxicating. We know that the effect of ether is transient and soon passes off, so that after its administration sensibility

soon returns; but how soon would a young lady be able to leave a dentist's chair and go into the street after an inebriation from brandy or wine. The effects of drink do not pass off thus suddenly, and etherization is not intoxication.

I do not wish to advocate the use of anæsthetics under all circumstances, nor their indiscriminate application. One circumstance contra-indicating their employment, although other things may call for them, is when it is absolutely necessary to keep a patient perfectly still, as during the operations for hernia, cataract, and others of equal delicacy, when a slight involuntary movement might cause a cut to be made where it should not be made. It should not be used in operations in the mouth, because the patient might not be able to get rid of the blood, and might suffer from its passing downward. Other contra-indications must constantly arise in particular cases, which should be duly considered by the practitioner. Of this I am convinced, that it is not very easy to kill a person with ether, else death would have occurred in very many instances, for no agent has been so widely and indiscriminately used, and yet with so few bad consequences. Ether should be preferred to chloroform. It does not act so suddenly nor so powerfully, and experience teaches that it is the safer agent. A mixture of the two is used, but I think it objectionable, as ether is the more volatile, and if its effect is not speedily induced it will soon evaporate and leave the chloroform behind to have its full effect. The better plan would be to give ether alone, and then, should the patient be insusceptible to its action or become excited, a few drops of chloroform may be administered. Under all circumstances, a full supply of air should be allowed, and the more simple the apparatus employed, the better.

EDITORIAL DEPARTMENT.

Presuming that our readers will be better satisfied with a full account of the proceedings of the National Association, than with contributions from our pen, we yield the entire Editorial, and a portion of the Eclectic department of the present No. for that purpose. We are indebted for the following authentic report to the courtesy of Dr. Gooch, editor of the *Stethoscope*, and one of the secretaries of the Am. Med. Association:

We find that our limits oblige us to omit portions of the proceedings. The parts omitted will appear in our next number.

Proceedings of the Fifth Meeting of the American Medical Association.

TUESDAY, May 4, 1852.

The association met in the Second Presbyterian Church at 11 o'clock—the president, Dr. Moultrie, in the chair.

Dr. James Beale, president of the Medical Society of Virginia, and chairman of its committee of reception, welcomed the delegates to the city of Richmond.

Dr. Haxall, chairman of the committee of arrangements, read a list of the delegates who were present, and who answered to their names as follows:—From Maine 2; N. Hampshire 1; Massachusetts 17; Rhode Island 6; Connecticut 9; New York 23; New Jersey 8; Pennsylvania 33; Delaware 3;

Maryland 10; Virginia 90; North Carolina 5; South Carolina 13; Georgia 4; Alabama 4; Louisiana 2; Tennessee 2; Kentucky 8; Ohio 10; Michigan 1; Illinois 3; Missouri 6; Iowa 1; District of Columbia 6; U. S. Navy 1; Foreign 2—275.

Dr. Hays, of Pa., offered the following resolution:

Resolved, That a committee of one from each state, to be selected by its own delegation, be appointed to nominate suitable officers for the association.

The resolution having been adopted, the association took a recess of ten minutes, to allow the delegations to appoint the nominating committee.

At the expiration of the recess, the president announced the nominating committee as follows:

Maine—Isaac Lincoln; New Hampshire—Jeremiah Blake; Massachusetts—Jacob Bigelow; Rhode Island—H. W. Rivers; Connecticut—Charles Hooker; New York—Joseph M. Smith; New Jersey—G. R. Chitwood; Pennsylvania—G. W. Norris; Delaware—H. F. Askew; Maryland—G. S. Gibson; District of Columbia—C. Boyle; Virginia—James Beale; North Carolina—James H. Dickson; South Carolina—H. R. Frost; Georgia—C. B. Nottingham; Alabama—A. Lopez; Kentucky—W. L. Sutton; Missouri—C. A. Pope; Ohio—D. Tildon; Illinois—D. Brainard; Michigan—Z. Pitcher; Iowa—J. H. Ranch; Tennessee—Paul F. Eve.

The president requested the secretary to call the roll.

Dr. Cox, of Md., offered the following resolution:

Resolved, That when the roll be called, each member shall *rise* in his place and answer to his name.

The resolution was not adopted.

The secretary then proceeded to call the roll, and the members present having answered to their names, the president delivered a lengthy and able address.

The nominating committee reported the following as officers of the association:

For President—Beverley R. Wellford, of Va.

For Vice-Presidents—Jonathan Knight, of Conn., James W. Thomson, of Delaware, Thos. Y. Simons, of South Carolina, and Chas. A. Pope, of Miss.

For Treasurer—Dr. Francis Condie, Pa.

On motion of Dr. Atlee, of Pa., it was

Resolved, That the officers thus nominated be and are hereby elected the officers of the association for the ensuing year, and that the nominating committee be requested to nominate secretaries, and to decide upon the next place of meeting at as early a period as possible, the present secretaries to retain their offices until other nominations are made.

This resolution having been adopted, the gentlemen nominated were declared the officers of the association for the ensuing year; and on motion of Dr. Atlee, of Pa., a committee of three, consisting of Drs. Atlee, of Pa., Haxall, of Va., and Eve, of Tenn., were appointed a committee to announce his election to Dr. Wellford, and conduct him to the chair.

Dr. Wellford having taken the chair, returned his thanks for the honor conferred upon him.

Dr. F. C. Stewart, of N. Y., offered an invitation to the association to make the city of New York the next place of meeting.

On motion of Dr. Boyle, this and all similar invitations were referred to the committee of nominations.

Dr. Hays, of Pa., offered the following resolution:

Resolved, That the report of the committee on the constitution be made the special order for to-morrow morning.

It was moved by Dr. Stille, that the resolution be so amended as to make it the special order for Thursday. This amendment was lost, and the question being taken on the original resolution, it was adopted.

Dr. Hays also offered the following resolution:

Resolved, That the report of the committee of publication and on prize essays be made the special order for the afternoon session.

Dr. Phelps, of N. Y., moved that when the association adjourn, it will adjourn to meet at 4½ o'clock this afternoon.

This resolution was adopted.

Dr. Haxall, chairman of the committee of arrangements, offered the following preamble and resolution, which were unanimously adopted:

The American medical society in Paris being so constituted that it would be entitled to representation if it existed in this country, and as it is recognized abroad as an American institution—

Resolved, That the delegates accredited to the association by the American medical society in Paris be and are hereby invited to take seats in this body.

Dr. Drake read the following resolutions, which were laid on the table; and on motion, the association adjourned till 4½ o'clock, P. M.

1. *Resolved*, That every report on a medical or other scientific subject, shall be referred to a select committee, to be read, analyzed and reported on to the association; said select committee indicating its general character and worthiness of publication, provided the authors of every report shall have the right of appealing to the association.

2. *Resolved*, That no report shall be read before the association until it has been examined and reported on by the committee to which it may be referred; nor then but under an order of the association.

3. *Resolved*, That no report shall be published in the Transactions of the association but in virtue of its order.

4. *Resolved*, That all professional and other scientific communications made to the association, shall be referred and treated like the reports of committees.

5. *Resolved*, That the president, vice presidents and secretaries of the association shall be charged with the appointment of the aforesaid committees, being themselves eligible for such appointments.

6. *Resolved*, That the authors of all reports and papers aforesaid, shall have the privilege of reading and explaining the same before the committees.

AFTERNOON SESSION.

Dr. B. R. Wellford called the association to order at 4½ o'clock, P. M.

Dr. D. Paul Lajus offered the following resolution, which was unanimously adopted:

Resolved, That Dr. Brown Sequard, of Paris, be invited to occupy a seat among the delegates at the present meetings of the association.

Dr. Paul F. Eve, from the committee on nominations, then reported that the committee had *resolved*—

1. That St. Louis be designated as the place for the meeting of the association in 1853.

2. That Drs. P. C. Gooch, of Virginia, and John S. Moore, of Missouri, be nominated for secretaries.

On motion, the report was laid on the table.

Dr. Gooch offered the following resolution, which was rejected:

Resolved, That the members of the press be admitted to seats on the floor, and that a committee of three be appointed to raise by voluntary subscription a sum sufficient to defray the expenses of reporting and publishing the proceedings of the meetings, and to make an arrangement for such.

Dr. Isaac Hays read the report of the committee on publication and the reports of the treasurer.

The reports were received, and the following resolutions, appended to the report of the committee of publication, were put and unanimously adopted:

1. *Resolved*, That the assessment for the present year shall be *three dollars*.

2. *Resolved*, That the committee of publication be authorized to fix the price at which the Transactions for the present year will be furnished to such of the members of the association as shall remit the amount decided upon by the committee, within a specified time, (to be fixed also by them.) And that it shall be the duty of the said committee to issue a circular informing the members of the terms upon which the Transactions will be furnished to them.

3. *Resolved*, That the committee be authorized to take such measures in relation to the disposal of the copies of the Transactions remaining after all such members are supplied as shall comply with the terms set forth in the circular of the committee, as they may deem expedient.

On motion of Dr. Ives, the vice presidents were requested to take seats allotted to them in front of the president's chair.

Dr. Hayward presented the report from the committee on prize essays, and broke the seal of the packet containing the name of the author of the essay, entitled "*On Variations of Pitch in Percussion and Respiratory Sounds, and their Application to Physical Diagnosis*," and which was deemed worthy of the prize. The author proved to be Dr. Austin Flint, of Buffalo, N. Y., to whom the prize was awarded, and the report was referred to the committee of publication.

The report of the committee on the Medical Botany of the U. States for 1850-'51, from Dr. A. Clapp, chairman, was presented and referred to the committee of publication.

Dr. Drake called up his resolutions offered at the morning session, which were read and discussed. On motion of Dr. Lopez, of Alabama, they were indefinitely postponed.

The reports from the regular standing committees were then called for in order, and were severally laid over or continued. Letters were read from Dr. J. B. Johnson, of Missouri, asking to be excused from further duty as chairman of the committee on epidemic erysipelas, which was granted; and Dr. Thomas Reyburn, of Missouri, asking that the committee on the epidemics of Missouri, Illinois, Iowa, and Wisconsin, be continued, which was also granted.

Dr. Ro. W. Haxall, of Virginia, read a short report of the progress of the committee on the epidemics of Virginia and North Carolina, and asked to be continued, which request was granted.

Dr. Wm. A. Patterson extended to the association an invitation from W

P. Tunstall, president of the Richmond and Danville railroad company, to an excursion on their road on Friday, 7th May, which was accepted; and, on motion, the thanks of the association were voted to the company.

Dr. Askew moved that when the association adjourn, it adjourn till 9 o'clock on Wednesday, and that it sit from 9 A. M. till 2 P. M. Carried.

On motion of Dr. Gooch, the editorial corps were invited to take seats on the floor.

On motion, the association then adjourned.

WEDNESDAY, May 5, 1852.

The association met at 9 o'clock—the president, Dr. Wellford, in the chair.

The minutes were read and approved.

The secretary informed the association that he had enclosed copies of the preambles and resolutions adopted by the association at their sessions of 1850-51, relative to assimilated rank of the medical staff of the army and navy, to the several departments ordered by the resolution. From Dr. Harris, chief of the bureau of medicine and surgery, he had received a letter approving of the course of the association, which letter was read.

Dr. Pinkney, of the navy, asked leave to read a memorial which he had prepared to present to congress, on the subject of assimilated rank. Leave being granted, the memorial was read and explained by its author.

Dr. Cox, of Maryland, offered the following resolutions:

Resolved unanimously, That this association approves the memorial emanating from Surgeon Ninian Pinkney of the United States navy, and respectfully asks of congress a calm and dispassionate consideration of its contents; and we, the representatives of the medical profession in the United States, will anxiously await a decision, confidently believing that the relief asked for in the memorial on behalf of the medical corps of the navy, will be granted.

That it is a matter of great interest to the medical profession at large that an act of congress be formally incorporated into the national legislation, and at the present session, which shall define clearly and definitely the relative rank of the medical officers of the navy.

That the bill proposed by Surgeon Ninian Pinkney is approved by this convention, and earnestly recommended as forming a proper and equitable basis for an adjudication of the relative rank, and that this convention will regard any scale less satisfactory to the medical officers of the navy, as unjust to them, and degrading to the profession at large.

That the secretary of this convention be directed to address a copy of these resolutions, together with the memorial of Dr. Pinkney, to the secretary of the navy and the presiding officers of both houses of congress.

On motion of Dr. Yandell of Kentucky, these resolutions were referred to a committee of three, to be appointed by the president.

Dr. Atkinson, of Virginia, offered the following resolution:

Resolved, That we have listened with great pleasure to the able and eloquent remarks of Dr. Ninian Pinkney, in vindication of the honor and interests of the profession, and that we will second his efforts to obtain justice at the hands of congress by every means in our power; which was referred to the same committee.

Dr. Hayward, of Boston, offered the following resolution:

Resolved, That no member of the association be allowed to speak longer than ten minutes at a time, nor more than twice on the same subject.

Which was unanimously adopted.

Dr. Simmons, of S. C., offered the following preamble and resolutions:

The accumulation of passengers who are emigrants, crowded in ships coming to our shores from foreign ports, having in a great many instances numerous cases of aggravated fever, many of which prove fatal, and likewise producing similar results at the lazarettoes, and even cities; the number, likewise, of sick arriving from California, and some of the South American ports, and the fact that none of these vessels are required by law to have physicians or surgeons on board, seem deserving of our attention as conservators of health, and as an act of humanity and duty on the part of the American medical association, to bring these facts respectfully to the consideration of congress, and to request its legislation thereon.

Be it therefore resolved, That the American medical association do memorialize congress to require all vessels carrying steerage passengers on the sea, to have a surgeon on board.

Resolved further, That a committee of this association be appointed to draw up a memorial to congress, making such suggestions as it may deem fit as regards the importance of this measure.

On motion, of Dr. Wood, of Pa., the resolutions were laid on the table for the present.

Dr. Storer asked a suspension of the regular order, to enable him to bring to the notice of the association a scurrilous attack upon him as the chairman of the committee on obstetrics, which he pronounced to be malignant, vindictive and false, and which he would not have noticed had it been directed against him personally.

Dr. J. B. Flint, of Kentucky, proposed the following as an alteration of the constitution, which, according to rule, was laid over till the next meeting:

It is proposed to alter the constitution, in the fifth article of it, so as to provide, that instead of the annual volume of Transactions, the association may establish and maintain a quarterly journal, to be a medium for the publication of its proceedings, and of the most valuable contributions of its members—an organ of resolute and impartial criticism, and an official exponent and advocate of the views of the association on medical science, education and ethics.

The report of the committee on the constitution being the special order, Dr. Hays, chairman of the committee, made a report. Dr. J. H. Yardly, a member of the committee, made a counter report. Much discussion ensued, and many resolutions and amendments were proposed and withdrawn in favor of the following resolution offered by Dr. Thomas, of Maryland, and amended by Dr. Stewart, of New York:

Resolved, That the two reports on proposed alterations of the constitution be referred to a committee of three, to be appointed by the chair, with instructions to report to-morrow morning, in definite and proper form, such amendments as will embrace the views set forth in the reports, and such other views as may appear to them advisable.

This resolution was adopted.

Dr. Watson, of New York, offered the following resolutions:

Resolved, That the report of the nominating committee, now on the table, be referred back to the said committee, with instructions to report complete

on the standing committees, and such other committees as may be requisite for providing business for the association at its next annual meeting.

Resolved, That the invitation from the New York delegation for the meeting of the association in the city of New York in May 1853 be accepted, and that the nominating committee be instructed to that effect, and as usual to provide for the appointment of one of the secretaries from among the members residing at the place to be selected for the next annual meeting.

Dr. Stewart, of New York, moved to amend the resolutions, by referring the report of the nominating committee back to the committee without instructions.

This amendment was lost.

After some discussion and the proposal of several amendments, the question was taken on the adoption of the original resolutions, and they were unanimously adopted.

The secretary read the following communication from the New York academy of medicine, which, on motion, was referred to the publication committee and ordered to be printed:

NEW YORK ACADEMY OF MEDICINE, }
New York, April 22d, 1852. }

SIR—I have the honor herewith to transmit to you a copy of the preamble and resolutions adopted at a regular meeting of the New York academy of medicine, held April 21st, 1852.

Whereas the clinics now held at the medical colleges, as at present conducted, are or may be made tributary to the private interests of the professors at the expense of other and younger members of the profession, depriving them, by an odious monopoly, of *practice* and *operations*, and often of *fees*, to which they are justly entitled: Therefore,

Resolved as the sense of this academy, That to prescribe or operate upon the legitimate patients of any other physician, knowing them to be such, although done gratuitously at a clinique, is equally unwarrantable and unprofessional, with similar interference with the patients of another in private practice; and in either case, is a violation of the code of medical ethics adopted by this body.

Resolved, That the possible perversion of these clinics to the private emolument of those conducting them, by transferring patients to their private offices, and thus exacting fees from those found able to pay, divests the clinics of all pretext for professing to be public charities, and should be scrupulously guarded against in all our colleges by stringent rules.

Resolved, That a copy of these resolutions be sent to the authorities of the several medical colleges in this city.

The secretary was also instructed to forward a copy of the resolutions to the American Medical Association.

Respectfully yours,

JACKSON BOLTON, M. D.,
Recording Secretary.

P. CLAIBORNE GOOCH, M. D.,
Sec. Am. Med. Asso., Richmond, Va.

Dr. Haxall, chairman of the committee of arrangements, offered the following resolution, which was adopted:

Resolved, That after to-day the association hold a morning session from 9 o'clock, A. M. to 3 or 3½ o'clock, P. M., and have no afternoon session.

Dr. Hayward, of Boston, read a letter from Dr. Horatio Adams, of Waltham, Massachusetts, regretting his inability to be present at the meeting, owing to a serious accident, and presenting the report of the committee on the "*action of water on lead pipes, and the diseases resulting from it*," asking the reference of the report to the committee on publication. The report was accepted and referred.

Drs. Drake, of O., and Rogers, of Va., offered several suggestions in regard to the constitution, which were referred to the committee on that subject.

The chairman of the nominating committee requested that the delegates from states not represented when the committee was organized, should appoint their committee-men forthwith.

Drs. Gwathmey and Watson, of Virginia, Smith, of California, and Beck, of New York, were, on motion, admitted to the floor of the association during its sittings.

Dr. Corbin, of Va., read the following resolution, which he desired to lay on the table for the present:

Resolved, That one member from each state represented in this association be appointed a delegate to represent it in the medical associations in Europe, and that they be requested to visit the foreign hospitals, and to report to the next meeting of the association the various improvements in the several branches of science connected with medical education, and in the treatment of diseases in general in foreign countries.

On motion, the association then adjourned.

AFTERNOON SESSION.

The president, Dr. B. R. Wellford, took the chair at half-past 4 o'clock, P. M.

Dr. Drake, of Ky., offered the following:

Resolved, That all papers and reports on scientific subjects shall be read to the association before the question of their publication shall be decided.

Dr. Wood, of Pa., opposed the resolution.

Dr. Phelps, of N. Y., offered an amendment, which, together with the resolution, was, on motion of Dr. Thomas of Md., laid on the table.

Dr. Condie, of Pa., presented a paper on chemistry, from a gentleman not a member of the association, and Dr. Drake presented a similar one by Dr. Wright, of Ohio, on the influence upon the health of daguerreotypists of their occupation. On motion of Dr. Condie, they were both referred to a select committee, consisting of Drs. Ro. E. Rogers, A. T. B. Merritt, and J. R. W. Dunbar, with instructions to report on them to-morrow.

On motion of Dr. G. F. Terrill, of Va., Drs. T. L. Scott and W. H. Fox, of Va., were admitted to seats on the floor.

Dr. Eve, from the committee on nominations, recommended the following officers for the ensuing year:

For Secretaries—Dr. P. Claiborne Gooch, of Va., and Dr. Edward Bead, of N. Y.

Committee on Publication—I. Hays, of Pa., P. Cl. Gooch, of Va., E. L. Beadle, of N. Y., Isaac Parrish, of Pa., G. Emerson, of Pa., D. F. Condie, of Pa. and G. W. Norris, of Pa.

Committee of Arrangements—F. Campbell Stewart, John Watson, Wm. Rockwell, James R. Wood, Robert Watts, Jr., Alfred C. Post, John G. Adams and H. D. Bulkley, of New York.

On motion, the report was received, and the gentlemen named were unanimously elected officers of the association for the ensuing year.

The chair then announced the following appointments in compliance with resolutions adopted at the morning session.

Committee on Amendments to the Constitution—Dr. F. C. Stewart, of N. Y., Dr. Worthington Hooker, of Conn., and Dr. Robert H. Thomas, of Md.

Committee on Dr. Cox's Resolutions in regard to the Rank of Medical Officers in the Navy—Dr. Samuel Jackson, of Pa., Dr. Jonathan Knight, of Ct., and C. C. Cox, of Md.

The report of the committee on "The Blending and Conversion of the Types of Fever" was then read by Dr. A. B. Williman of S. C., (in place of Dr. Dickson, not present.)

On motion, the report was ordered to be printed, and referred to the committee of publication.

Dr. Hayward, of Mass., presented and read the report of the committee on "The permanent Cure of reducible Hernia;" which was ordered to be printed and referred to the committee on publication.

On motion of Dr. Dunbar, of Md., seconded by Dr. Drake, a report of the case of Dr. Jameson, of Baltimore, was requested to be furnished for publication in an appendix to the report.

An application was presented from J. Wells, representative of the interests of the late Dr. Horace Wells, of Hartford, Conn., asking that a committee be appointed to enquire into and to report on the claims of the contestants for the honor of priority in the discovery of the principle of anæsthesia in surgical operations.

The application was laid upon the table.

On motion, the association then adjourned.

THURSDAY, May 6, 1852.

The association was called to order at 9½ o'clock—Dr. Wellford, president, in the chair.

The minutes were read, amended and approved.

On motion of Dr. W. E. Horner, Dr. Beylard, of Paris, was admitted to the floor of the association; and on motion of Dr. Wilson, of Virginia, Dr. W. T. Howard, of N. C., was also admitted.

Dr. Jno. Watson, of N. Y., offered the following resolution:

Resolved, That members of the association having questions for scientific enquiry to propose as part of the business for the ensuing year, be requested to submit the same in writing to the chairman of the committee on nominations, and that said committee be requested to report on the nominations of the special scientific committees, with the subjects to be referred to said committees, at its earliest convenience:

Dr. Wood, of Pennsylvania, offered the following amendment:

"And that the nominating committee nominate a committee of five, who shall select special subjects of investigation, and nominate chairmen of the of the committees on these subjects, and also to nominate the members of the committee on voluntary communications." Which was lost.

Dr. Watson's resolution was then adopted.

Dr. Atkinson, of Virginia, moved the following:

Resolved, That the thanks of this association are due, and are hereby tendered to Dr. Isaac Hays, for the very efficient and satisfactory manner in which he has discharged the duties of its treasurer, and to Dr. H. W. De Saussure, for the able manner in which he has discharged the laborious duties of secretary.

Dr. Green, of N. Y., offered the following resolutions, which were adopted:

1. *Resolved*, That at all future meetings of this association, all reports of committees, and all contributions on scientific subjects, occupying more than ten pages of quarto post manuscript, be accompanied each by an abstract or synopsis embracing the principle points of such report or paper, which abstract or synopsis may be read before the association.

2. *Resolved*, That the above resolution be transmitted by the secretary to the chairman of each scientific committee.

Dr. Stille, of Pa., moved the following resolutions, which were seconded by Dr. Blatchford, of N. Y., and unanimously adopted:

1. *Resolved*, That the elegant, varied and generous hospitality which the association has enjoyed during its present session, calls for its hearty and unanimous thanks, with the assurance that it can never forget an entertainment, unrivaled even among the festivities of the "Old Dominion."

2. *Resolved*, That the thanks of the association are hereby presented to the Medical Society of Virginia, to the medical profession and citizens of Richmond, to the trustees of the "United Presbyterian Church," to the managers of the Danville railroad, and to the several public institutions of this city, for the hospitable care of these bodies to promote the comfort and amusement of the association.

3. *Resolved*, That the association returns its thanks in an especial manner to the committee of arrangements, for the zeal, intelligence, and good taste displayed in performing its numerous and important duties.

Dr. Simons called up his resolutions in regard to the necessity of surgeons being employed on board of emigrant ships; which were advocated by him and adopted.

Dr. W. Hooker, of Ct., offered the following resolution, which was adopted:

Resolved, That special committees on medical education and medical literature be appointed, consisting each of five members, and that the nominating committee be instructed to nominate such committee to this association.

Dr. Sutton, of Ky., moved that a committee of three be appointed, whose duty it shall be to enquire whether any, and if any, what action this association shall take in reference to requesting the congress of the United States to have a large edition of the medical statistics, furnished by the census lately taken, published in a separate form for distribution among the medical profession of the United States, and to report to-day.

The chair announced the committee, to consist of Drs. Simons of S. C., Boyle of D. C., and Sumner of Conn.

On motion of Dr. Condie, of Pennsylvania, it was

Resolved, That a committee of five be appointed to examine and report on the communication of Dr. Drake, on the relation between climate and pulmonary consumption.

The committee was announced, to consist of Drs. Condie, R. E. Rogers, J. M. Smith, Moultrie and McGuire.

On motion of Dr. Rockwell, it was

Resolved, That the committee appointed to memorialize congress on the subject of compelling passenger vessels to carry surgeons, be directed also to call their attention to the importance of giving to each steerage passenger a certain amount of space between decks.

[We omit the communication from American Medical Society of Paris—shall refer to this society in our next number.]

Dr. Blatchford, of N. Y., offered the following:

Resolved, That a committee of three be appointed, to report at the next meeting of the association, on the best means of making pressure in the treatment of reducible hernia, and that Dr. Hayward, of Mass., be the chairman. Carried.

Dr. Usher Parsons, of R. I., offered the following preamble and resolution, which, on motion of Dr. Hays, of Pa., were laid on the table:

Whereas, it is required by law that a chest of medicines shall be furnished to every merchant ship, with suitable directions for their administration; and whereas the pamphlets now in use are written by apothecaries, instead of physicians, and are full of errors: Therefore,

Resolved, That a committee of three be appointed to prepare suitable directions to accompany medicine chests, that shall meet the wants of the officers and seamen in merchant vessels, under the sanction of ~~the~~ association, and report at the next annual meeting.

The report of the committee appointed on yesterday to consider the various propositions which had been made, suggesting amendments to the constitution, being called for, the chairman, Dr. F. Campbell Stewart, of N. Y., read a report and resolutions, which Dr. Hays, of Pa., moved to refer to the committee of publication, with instructions to print.

Drs. J. K. Mitchell, of Pa., and Wm. Hooker, of Conn., discussed the merits of the report, when Dr. Lopez, of Alabama, raised a question of order as to the propriety of a discussion of the merits of the proposition.

The chair decided that the discussion was in order at this stage of the proceeding.

Dr. Lopez, of Ala., appealed from that decision, which appeal was not sustained.

The discussion was then continued at great length by many members.

During the discussion, the following replies were elicited from several gentlemen, by questions propounded by Dr. Watson, of New York:

From Dr. Horner, University of Pa.—The shortest term of medical study in the University required for the doctorate was three years, but that under some few and rare circumstances, a deviation had been permitted as an exception.

Drs. Davis and Rogers, of Virginia University, stated that their laws required no specified time: nine months, and eighteen years of age even, were sufficient, but that two years were generally devoted to the study of medicine by their graduates. They explained the course of instruction at the University at length.

Dr. Huston, of Jefferson Medical College, Philadelphia, said that three full years were required, but that occasions demanded sometimes a departure from the stringent rule.

Dr. Frost, from South Carolina, offered some interesting observations upon the much abused subject of medical education, and insisted that the profession had not retrograded. That there had been a steady and gradual improvement in our medical colleges generally, and brought to the notice of the association the attention which was observed in preparatory education in the medical college of South Carolina, which was highly creditable to the same. His remarks were listened to with attention, and brought forth observations of a like character from other members present.

The report and proposed amendments were received, after having been amended so as to read as follows: [We omit the report.]

ARTICLE I.—*Title of the Association.*

This institution shall be known and distinguished by the name and title of "The American Medical association." It shall be composed of all the members of the medical profession of the United States of good standing, who acknowledge fealty to, and adopt the code of ethics adopted by the association; and its business shall be conducted by their delegates or representatives, who shall be appointed annually in the manner prescribed in this constitution.

Strike out the whole of Article II, referring to "Members," and insert the following:

ARTICLE II.—*Of Delegates.*

§ 1. The delegates to the meeting of the association shall collectively represent and have cognizance of the common interests of the medical profession in every part of the United States, and shall hold their appointment from county, state, and regularly chartered medical societies; from chartered medical colleges, hospitals, and permanent voluntary medical associations in good standing with the profession. Delegates may also be received from the medical staffs of the United States army and navy.

§ 2. Each delegate shall hold his appointment for one year, and until another is appointed to succeed him, and he shall be entitled to participate in all the business affairs of the association.

§ 3. The county, district, chartered and voluntary medical societies shall have the privilege of sending to the association one delegate for every ten of its resident members, and one more for every additional fraction of more than one-half of this number.

§ 4. Every state society shall have the privilege of sending four delegates; and in those states in which county and district societies are not generally organized, in lieu of the privilege of sending four delegates, it shall be entitled to send one delegate for every ten of its regular members, and one more for every additional fraction of more than one-half of this number.

§ 5. No medical society shall have the privilege of representation, which does not require of its members an observance of the code of ethics of this association.

§ 6. The faculty of every chartered medical college acknowledging its fealty to the code of ethics of this association, shall have the privilege of sending one delegate to represent it in the association: *Provided*, That the said faculty shall comprise six professors, and give one course of instruction annually of not less than sixteen weeks, on Anatomy, Materia Medica, Theory ar

Practice of Medicine, Theory and Practice of Surgery, Midwifery and Chemistry: *And provided also*, That the said faculty requires of its candidates for graduation—1st. That they shall be twenty-one years of age; 2d. That they shall have studied three entire years, two of which must have been with some respectable practitioner; 3d. That they shall have attended two full courses lectures, (not however to be embraced in the same year,) and one of which must have been in the institution granting the diploma, and also where students are required to continue their attendance on the lectures to the close of the session; and 4th. That they shall show, by examination, that they are qualified to practice medicine.

§ 7. The medical faculty of the University of Virginia shall be entitled to representation in the association, notwithstanding that it has not six professors, and that it does not require three years of study from its pupils, but only so long as the present peculiar system of instruction and examination practiced by the institution shall continue in force.

§ 8. All hospitals, the medical officers of which are in good standing with the profession, and which have accommodation for one hundred patients, shall be entitled to send one delegate to the association.

§ 9. Delegates representing the medical staffs of the United States army and navy shall be appointed by the chiefs of the army and navy medical bureaux. The number of delegates so appointed shall be four from the army medical officers, and an equal number of the navy medical officers.

§ 10. No delegate shall be registered on the books of the association as representing more than one constituency.

§ 11. Every delegate elect, prior to the permanent organization of the annual meeting, and before voting on any question after the meeting has been organized, shall sign the constitution and inscribe his name and address in full, with the title of the institution which he represents.

Dr. Wadsworth, of Pa., offered the following resolution:

Resolved, That when the association adjourns, it will be to meet again this afternoon at 4½ o'clock P. M., and that the resolution adopted on yesterday be rescinded so far as it conflicts with this action.

Carried.

Dr. Smith, of N. Y., chairman of committee on nominations, made a report, which was recommitted, on motion of Dr. Patteson, of Va., for correction.

The chair then announced the following committee on Dr. Simons' resolution, in regard to the propriety of memorializing congress to pass some law requiring emigrant vessels to carry surgeons, viz: Dr. T. Y. Simons of S. C., chairman, Pope of Mo., Thompson of Del., Flint of Ky. and Mauran of R. I.

Dr. Knight, of Conn., moved to lay the report on amendments to the constitution on the table, to be taken up and voted on, section by section; which was carried.

The association then adjourned till 4½ o'clock P. M.

AFTERNOON SESSION.

Dr. Wellford, president, called the association to order at half past 4 o'clock.

Dr. McIntyre, of N. Y. moved to refer the report on the amendments to the constitution to the publication committee; which was lost.

Dr. Smith, N. Y., chairman of the nominating committee, reported and offered the following resolution, which was received and adopted unanimously:

Resolved, that the following gentlemen be appointed:

1. *Committee on Medical Literature*—Rene La Roche, M. D., of Pa., chairman; H. W. De Saussure, M. D., of S. C.; N. S. Davis, M. D. of Ill.; Jacob Bigelow, M. D., of Mass.; Ed. H. Barton, M. D., of La.

2. *Committee on Medical Education*—Zina Pitcher, M. D., of Mich., chairman; Austin Flint, M. D., of N. Y.; J. R. W. Dunbar, M. D., of Md.; James McKeen, M. D., of Maine; D. W. Yandell, M. D., of Ky.

The amendments to the constitution, as embodied in the amended report of the committee at the morning session, were then read, section by section, and after some debate, laid on the table as proposed amendments to the constitution.

During the discussion, Dr. Wilson, of Va., offered the following amendment, which was laid on the table, on motion of Dr. Thomas of Md.:

The faculty of every chartered medical college acknowledging its fealty to the code of ethics, and conforming to the requisitions of this association on the subject of medical education as adopted by this association in 1846, and reiterated at its subsequent meetings, shall have the privilege of sending one delegate to represent it in the association: provided that the medical faculty of the University of Virginia shall be entitled to representation in this association in consequence of its peculiar organization, but only so long as its peculiar system of instruction and examination shall continue in force.

Dr. Wilson gave notice that the above would be called up at the next meeting of the association as an amendment to the constitution.

Dr. Atlee, of Pennsylvania, moved the following, which was adopted:

Resolved, That this association still recommends to the medical colleges the propriety of lengthening their terms of instruction.

On motion, the following resolution was called up for consideration, and adopted:

Resolved, That the colleges exclusively of dentistry and pharmacy are not recognized by this association as among the bodies authorized to send delegates to its meetings.

On motion of Dr. Gooch, of Virginia, the two reports from the committee appointed last year to suggest alterations of the constitution, together with that of the committee to which they were referred on yesterday, were referred to the committee of publication, with instructions to print. * * *

[We omit report on rank of surgeons in the navy. We shall publish the report hereafter.]

Dr. Simons of S. C., chairman of the committee raised on Dr. Sutton's resolution, adopted on Wednesday, made the following report:

"The committee appointed, on motion of Dr. Sutton, to enquire what action should be taken to get Congress to publish the medical statistics of the census of the United States separately, to be presented to the medical profession under the auspices of the medical association, recommend that this or some other committee be empowered to memorialize Congress on the same."

On motion of Dr. Gooch, of Va., the report was received, and the same committee was instructed to carry out the recommendation in it.

Dr. W. Hooker, of Ct., read the report of the committee on the epidemics of New England, together with the following recommendation from the chairman of the several committees on epidemics, which was adopted:

In behalf of the committees on epidemics who are present at this meeting of the association, we present the following communication.

In view of the statement made in the report just presented, and of those which will be presented to you in some of the other reports on epidemics, the undersigned, members of a part of the committees on this subject appointed by this association, would recommend to the meeting the following resolutions:

Resolved, That the committees on epidemics be constituted in relation to the division into districts as they were the last year, and that they be continued in service during a period of five years.

Resolved, That the chairman appointed for each district shall have power to select associates, not exceeding four in number, to assist him in his labors.

Resolved, That the several state medical societies be requested to use their influence to procure the appointment by the legislatures of sanitary commissions.

(Signed)

W. L. SUTTON,
JNO. L. ATLEE.
W. HOOKER,
JOSEPH PARRISH,
Z. PITCHER,
RO. W. HAXALL.

Dr. Storer, of Mass., sent to the secretary's table a correspondence between the president of the Epidemiological society of London and the Hon. Abbott Lawrence, ambassador to England, together with some documents relating to the organization and usefulness of the society.

On motion of Dr. Córdie, of Pa., they were laid on the table.

A letter was received and read from Dr. Robley Dunglison, foreign secretary, and one of the vice presidents of the Sydenham society of London, presenting copies of the constitution and laws of the society; which, on Dr. Hayward's motion, were laid on the table.

Dr. Pope, of Mo., then read a report from the committee on the uses of water in surgery; which, on motion of Dr. Drake, was referred to the committee on publication.

On motion, the association then adjourned.

FRIDAY, May 7, 1852.

The association was called to order by the president at 4½ o'clock P. M.

The minutes of yesterday's session were read, amended and approved.

On motion of Dr. Stille of Pennsylvania, the paper read by Dr. Drake of Kentucky, on the "Influence of Climatic Changes on Consumption," was referred to the committee of publication.

Dr. Atlee of Pennsylvania, offered the following preamble and resolution, which were unanimously adopted:

Whereas, it is the duty of patriotism to do homage to those who have been benefactors to their country; and whereas the medical profession in the United States, heretofore not wanting in patriotic feeling or action, desire to

co-operate with the other public bodies and institutions of the country in rendering their profound reverence to the memory of him who was "first in peace, first in war, and first in the hearts of his countrymen;"

Be it therefore resolved, That a committee of five be appointed, whose duty it shall be to solicit subscriptions from members of the American Medical Association, for the purpose of procuring a suitable stone with an appropriate inscription, for insertion, in the name of this association, into the national monument to the memory of Washington, now in progress of erection at Washington city.

The chair announced the committee, to consist of Drs. Jos. L. Atlee, W. P. Johnston, Ro. W. Haxall, Alfred Stille and Gouverneur Emerson.

Dr. C. C. Cox, of Maryland, offered the following resolution, which was lost:

Resolved, That the committee on publication be and are hereby directed to distribute copies of the Transactions of this association, when printed, to the several booksellers in the principal cities of the Union, for the more convenient access of members entitled to the same, and also for the purpose of disposing of such copies as may remain on hand after the members shall have been supplied.

Dr. Corbin, of Virginia, called up his resolution offered on Wednesday, in regard to accrediting one member from each state represented in the association, to travel in Europe and to report upon foreign medical affairs to the association.

The resolution was adopted.

Dr. Phelps, of New York, then called up his amendment to the constitution proposed last year, to insert in article vii, p. 60, after the word "endeavors" the words "in reliance of divine guidance and support."

The motion so to amend was lost.

Drs. Flint, of Kentucky, and Hooker, of Connecticut, made motions in regard to the constitution, but they were withdrawn.

Dr. J. M. Smith, of New York, chairman of the committee on nominations, presented the following report, which, on motion of Dr. Corbin, of Virginia, was adopted:

The committee of nominations, in fulfilling the duty of their appointment, propose to continue most of the special committees appointed by the association in May, 1851, and to appoint several new special committees; they therefore submit the following list of chairmen of special committees, with the subjects to them committed: * * * *

[The names of persons composing the committees are deferred until the next No.]

The secretary then read a letter from Dr. S. D. Gross, of Kentucky, chairman of the committee "on the results of surgical operations for the relief of malignant diseases," regretting his inability, after strenuous exertions, to present a satisfactory report to the present meeting, and asking to be continued at the head of the same committee.

Dr. Paul Lajus, of Pa., offered the following resolution, which, after some debate, was lost:

Resolved, That a prize of \$250 be awarded hereafter to the best prize essay, and that honorable mention be awarded to the four next best essays, provided they be worthy of that honor.

Dr. Wood, of Pa., then moved that instead of awarding five prizes of \$50 each, annually, that the association hereafter grant two prizes of \$100 each, for the two best essays. Carried.

On motion of Dr. Stille, of Pa., the report on proposed amendments to the constitution was recommitted to the committee, with instructions "so to amend it as to admit a representation of the army and navy, and to make other alterations. * * * * *

The reports of the committees on scientific subjects being called for, Dr. Horner, of Pa., moved that they be read by their titles and referred to the committee of publication; which motion was adopted.

The following reports were then presented, read by their titles, and referred to the committee of publication:

"On the Toxicological and Medicinal properties of our Cryptogamic Plants," by F. Peyre Porcher, of S. C.

"On the Epidemics of New Jersey, Pennsylvania, Delaware and Maryland," by J. L. Atlee, of Pa.

"On the Epidemics of South Carolina, Georgia, Florida and Alabama," by Dr. W. M. Boling, of Ala.

Together with this report, which was handed in by Dr. Drake, of Ky., there was also presented a paper by Dr. D. J. Cain, of S. C.; which was ordered to be appended to the report when published.

"On the Epidemics of Mississippi, Louisiana, Texas and Arkansas," by Dr. Ed. H. Barton, of La.

"On the Epidemics of Ohio, Indiana and Michigan," by Dr. Geo. Mendenhall, of Ohio.

Dr. Stewart, of N. Y., then presented the report of the committee on the amendments to the constitution, and read the following additions which the committee had made since its recommitment:

To section 1, article 2, add "Delegates may also be received from the United States army and navy."

In section 6, article 2, add the words "Comprise six professors and" after "provided said faculty shall."

In section 6, add to 3d requisition on faculties, the words "and also where students are required to continue their attendance on the lectures until the close of the session."

Add section 7: "The medical faculty of the University of Virginia shall be entitled to representation in the association, notwithstanding that it is not composed of six professors, and that it does not require three years of study for its pupils, but only so long as the present peculiar system of instruction and examination practiced by that institution shall continue in force."

Add section 9: "Delegates representing the medical staff of the United States army or navy shall be appointed by the chiefs of the army and navy medical bureaux. The number of delegates so appointed shall be four from the army medical officers and an equal number from the navy medical officers."

After some discussion, and the failure of several motions to alter, lay on the table, etc., the report from the committee was accepted as amended by an unanimous vote, and the propositions (as seen on page 303 of third day's proceedings) were recommended to the next association as amendments to the constitution.

Dr. Bolton, of Va., then gave notice of the following amendment, which he

should call up at the next meeting: Add to section 6, article 2, "Provided that such college require of its matriculates an adequate preliminary examination."

Dr. F. C. Stewart, of N. Y., moved the following preamble and resolutions, which were seconded by Dr. Pope, of Mo., and unanimously adopted:

Whereas the building in which this association has held its present session was gratuitously furnished by the proprietors: Therefore,

Resolved, That the cordial thanks of the "American Medical Association" be and the same are hereby tendered to the pastor and trustees of the United Presbyterian congregation of the city of Richmond, for the kindness and hospitality manifested by them in tendering to the association the free use of their church and lecture room.

Resolved, That a copy of these resolutions be signed by the president and secretaries of the association, and transmitted to the pastor and trustees of the United Presbyterian congregation.

Dr. Dunbar, of Md., offered the following resolution, which was unanimously adopted by a rising vote:

Resolved, That the thanks of this association are hereby voted to the president for the able and satisfactory manner in which he has presided over its meetings, and also to the secretaries for the faithful manner in which they have discharged their laborious duties.

On motion of Dr. Thompson, of Delaware, and seconded by Dr. Rogers, of Virginia, the following resolution was unanimously adopted, and a copy of it was directed to be transmitted to Dr. Moultrie:

Resolved, That the thanks of the association are unanimously voted to Dr. James Moultrie, of South Carolina, its late president, for the able, impartial and faithful manner in which he has discharged the duties of president of this association during the past year.

On motion of Dr. Gooch, of Va., the president was empowered to make the appointments under Dr. Corbin's resolution offered on the second day and passed, at any time during the year.

On motion of Dr. Pope, of Missouri, the association then adjourned to meet in May next, in the city of New York.

The vice president in the chair, Dr. T. Y. Simons, of S. C., then, in a few appropriate remarks congratulated the members on the happy termination of their meeting, and declared it adjourned *sine die*.

Diluted Pyroligneous Acid as a Gargle. By JOHN EVANS, M. D.

I have for several years been using diluted pyroligneous acid as a gargle in cases of inflammation of the fauces and tonsils with better success than any other article that I have prescribed.

I put a teaspoonful of the acid obtained from the shops into a wine glass of water, and direct the patient to gargle the throat frequently with it.

In the sore throat caused by exposure, so common throughout the country, it generally relieves the soreness and stiffness felt in swallowing very promptly.

In chronic inflammation, with or without ulceration of the throat, I have found it a very valuable remedy.

In the sore throat of scarlatina, it has generally afforded a very prompt amelioration of this symptom of the disease.

In several cases of habitual tonsilitis, by using this gargle freely at the commencement of the disease, I have been able to arrest the progress of the inflammation and secure a resolution.

Its use is not unpleasant; it is safe, even if used for hours continuously, and has an additional advantage in removing the fœtor of the breath.—*North Western Med. and Surg. Journal.*

Operative Surgery, based on Normal and Pathological Anatomy. By J. F. MALGAIGNE, Professor Agregé de la Faculté de Médecine de Paris, Chirurgien de l'Hôpital de Dourcine, Chevalier de la Légion d'Honneur, et du Mérite Militaire de Pologne, etc. etc. Translated from the French by FREDERICK BRITTON, M. D. Illustrated by Wood Engravings, from Designs by WESTMACOTT. Philadelphia: Blanchard & Lea. 1851. For sale by Derby, 164 Main-st., Buffalo.

The original work "*Manuel de Médecine Opératoire*," was published in 1843, and we have long been accustomed to refer to it as one of the most valuable text books in our library. It has been translated into at least five continental languages, and in Paris, where it has already reached a fourth edition, it is a very general text book among the medical students of all nations.

We have, therefore, often expressed our surprise that it was not, before this, translated into the English.

To those who have ever seen "Malgaigne's Operative Surgery," it is superfluous to say that it is a valuable book. Although somewhat less elaborate, it occupies a place with Velspeau, Chelius, Lisfranc, &c.

The writings of Lisfranc, Nelaton and others, still wait for a translator. We wish they might fall into the hands of Dr. Brittain. F. H. H.

Surgical Anatomy. By JOSEPH MACLISE, Surgeon. With sixty-eight colored Plates. Philadelphia: Blanchard & Lea. 1851. Complete in five parts; price nine dollars. For sale by Derby, 164 Main-st., Buffalo.

We have received the concluding numbers of this excellent work, and we can assure our readers there is no falling off in the value of the matter, or in the style of the execution, and so much cannot always be said of a serial.

We are confirmed, therefore, in the opinion which we expressed when the first No. was laid before us, that "it is one of the best and cheapest anatomico-surgical works ever published by the American press." F. H. H.

BUFFALO MEDICAL JOURNAL

AND

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JULY, 1852.

NO. 2.

ORIGINAL COMMUNICATIONS.

ART. I. — *Report of Fractures treated at the "Buffalo Hospital of the Sisters of Charity," during the six months ending April 1, 1852, with Practical Remarks.* By FRANK H. HAMILTON, M. D., one of the Surgeons.

Simple Fracture of the Surgical Neck of the Humerus. Alexander Balentine, aged 62 years; admitted Dec. 19, 1851. He had fallen upon the sidewalk, striking upon his right arm. Dr. Johnson, of Buffalo, had reduced the fracture and applied appropriate dressings. No union of the fragments had yet occurred, but as the surfaces were in apposition it was only after considerable manipulation that the crepitus became distinct and gave unequivocal evidence of the fact of a fracture and of its situation.

I have found this a rare fracture, and usually difficult of diagnosis, owing to the nearness of the fracture to the head of the bone, where it is buried beneath the acromion and deltoid. Generally it is not much or at all displaced, and it can then only be recognized by the crepitus. The crepitus is, I think, most readily produced by bending the forearm at a right angle with the arm and making use of the forearm as a lever, rotating the arm on its own axis.

The treatment, after admission, consisted of one gutta percha splint accurately moulded and extending from above the shoulder to below the elbow,

and encircling one half of the circumference of the arm. The splint being secured with the usual bandages, &c. The advantage of carrying the splint over the shoulder and below the elbow (the elbow being bent at right angles) and moulding it to the surfaces is, that it is not liable to become displaced upward or downward. One broad splint thus applied is as good, if not better, than two or three narrow splints.

The result is a perfect limb. This has been the result in nearly every case which has come under my notice, whether treated by myself or others. I conclude, therefore, that it is one of the least difficult fractures to treat. Indeed I think the fragments would generally remain in apposition if no apparatus was applied, provided the limb was kept quiet.

Compound Comminuted Fracture of the Femur. John Street, aged 18 years. Admitted Jan. 4, 1852. The fractures occurred near the middle of the right femur; one fragment had pushed through the flesh and skin on the back of the thigh.

Assisted by Dr. Hill, of Buffalo, I dressed the fracture with a long straight splint — Potter's splint, a modification of Desault's; also with small side splints and rollers.

On the sixth day, I made an **extending** gaitre, with adhesive straps and a roller, as recommended lately by **Crosby**, of Dartmouth, and others.

The wound made by the bone **healed** promptly, and the case was thus reduced to a simple comminuted fracture. The soreness of the muscles through which the bone was thrust, probably increased their contraction and contributed to the eventual shortening of the limb.

The extension and counter-extension were continued six weeks, and the result has been a union with a shortening of one inch and a quarter. The limb is strong and the halt very slight.

I am scarcely satisfied with the adhesive plaster — for although no excoriations have been produced, yet it has been constantly yielding and requiring readjustment or retightening. The cotton and paste-bandage gaitre, as heretofore used by me and described at page 140 of the 5th vol. of this Journal, neither produces excoriations nor requires readjustment.

Transverse Fracture of the Patella. Edmund Laffy, aged 20 years, was admitted Oct. 30, 1851. The fracture occurred six days before; Dr. Shaw, of Silver Creek, had dressed it with Sir Astley Cooper's straps, rollers, &c. The same apparatus was continued with but slight modification until the 26th of Nov., when I removed the Cooper dressing and applied a straight splint, with adhesive straps, thus:

First, the splint — A single inclined plane, extending from the ischium to beyond the foot, the lower extremity of which was six inches higher than the upper; the splint was ten inches wide at the ischium and gradually narrowing to six inches at the foot, and furnished with an upright foot board. The splint thus constructed, being cushioned with a well made and carefully adjusted junk, the limb was laid upon it. Beneath the ham the padding was sufficient to raise the knee slightly — a position which I have always found less painful to the patient than complete extension.

Second. The limb was slightly secured to the splint by several strips of bandage.

Third. The upper fragment of the patella was now brought to its place while the center of a broad and long strip of adhesive plaster was made to embrace it from above, and to pass around the sides of the splint until they met and crossed behind and at a point below the knee. A similar strip was now applied below and upon the lower fragment, crossing behind and above. After which several other pieces were made to cover the patella more completely to prevent the fragments from becoming displaced forward, and to prevent also cedema of the knee.

The apparatus remained undisturbed seventeen days, during which it did not become loosened or disarranged — nor was it in any degree painful.

When removed, the patella had formed a very close and satisfactory ligamentous union.

I have always applied my bandages for securing the fragments in apposition, over the leg and *splint*, so as to obviate ligation of the limb; and for the same purpose I have made the splint broad, but I have never before in this fracture used the adhesive straps. I was charmed, as were also my pupils, with their convenience and efficiency.

Simple Oblique Fracture of Tibia and Fibula. Walter Redmond, aged 38 years; healthy; broke left leg below the middle, Dec. 13, 1850, by a misstep while walking in the street. On the 15th he was admitted to the hospital, and on the 17th I applied the paste bandage in the usual manner, and then laid the leg and thigh over a double-inclined plane.

Dec. 19. Opened the dressing at one point to examine the limb.

Dec. 20. Opened the dressing from end to end, and as, in consequence of the subsidence of swelling and drying of the bandages, they had become loose, I cut off a strip through the whole length, so as to reduce the circumference of the splint, and then closed it again with ten or twelve pieces of

tape. From this time I examined the limb frequently until the 19th of Jan., when the dressings were finally removed.

The limb was never but slightly painful; no excoriations occurred; he walked on crutches some days before the splints were taken off, and every thing was removed thirty-three days after the fracture. The limb is neither shortened or bent. The callus was very slight.

Simple Fracture of Tibia and Fibula. John Maloney, aged 16 years, broke his left leg in the lower third, while wrestling, and was admitted to the hospital on the next day, Jan. 8, 1852. The limb had been dressed by Dr. Lockwood, of Buffalo, with side splints roller, &c. As every thing appeared snug and in order, the dressings were not removed for the purpose of examining the limb, until the 28th, or three weeks from the time of their application. The fragments were then found in exact apposition.

The dressings were finally removed on the 7th of February, four weeks and a half from the time of fracture. The union was then complete, but he was not allowed to leave his bed for several days.

This case illustrates the advantage of so neatly adjusting the bandages, &c., at the first dressing that a renewal will not be necessary for a length of time; perhaps not until the union is nearly perfected. The union has been more rapid, and probably more perfect, in consequence.

The following cases having been treated as fractures only temporarily, will not be reported in detail:

Fracture of Os Innominatum and Neck of Femur. John O'Keife, admitted Oct. 23, 1851. Terminated fatally in a few hours.

Fracture of Inferior Maxilla and Superior Ossa Nasi, etc., etc. John Gallahan, admitted Oct. 3, 1851. Terminated fatally in twelve days.

Compound Fracture of Tibia and Fibula. Admitted March 25th, 1852. The leg was amputated soon after.

ART. II. — *Case of Poisoning from Arsenic.* By A. M. LEONARD, M. D.,
Lockport, N. Y.

I send you the following report for insertion in your Journal, should it in your judgment possess importance enough to give it a place there:

Tuesday, April 13th, 1852, I was called upon to examine the body of

Alexander Brown, who had died twenty hours before, under suspicious circumstances. In company with Drs. Worcester and May, I proceeded to the examination. The surface of the body presented no unnatural appearance, except a marked depression of the right side of the chest. On opening into the cavity of the chest, found evidences of extensive previous pleuro-pneumonitis of the right side, with very extensive adhesions of the lung to the walls of the chest. The stomach presented no unnatural appearance on its peritoneal surface, neither did the bowels, except the colon, which seemed very much contracted. I removed the stomach with the duodenum and the lower two-thirds of the cesophagus, and placed it in an open mouthed jar. Found several ulcers on the mucous surface of the duodenum. On making an opening into the ilium there issued nearly a pint of muco-purulent matter. Liver much enlarged, with its surface granulated, but no signs of recent inflammation in any of the viscera. As it was suspected that he had taken poison, I proceeded, the next day, to examine the stomach for arsenic. On opening into it I found from twelve to sixteen ounces of undigested food, with about the same quantity of a slightly sanguineous fluid. Appearances of inflammation in the mucous surface very slight, but the whole inner wall of the stomach was studded with small clots of blood underneath the villous coat. I also discovered, floating in mucus, and attached to the coats of the stomach, a white powder.

I poured the contents of the stomach into a vessel, and boiled them for fifteen minutes, when, in company with Dr. Worcester, I proceeded to test the fluid by the arseniated hydrogen test. Our apparatus was simply a Florence flask, with a cork nicely adjusted, through which passed a glass tube, furnished at its upper end with a jet of fine bore.

We first tested our reagents, when we got nothing but the characteristic flame of hydrogen, producing no stain whatever upon glass or porcelain. We then introduced $\frac{3}{4}$ ss. of the fluid from the stomach, applied a lighted taper to the gas as it came over, when we at once got the dull white flame of arsenic. A plate of porcelain was brought over the flame, when it was immediately coated with a thick, metallic crust of arsenic, and so abundant was it that we have no hesitation in saying that, from one-half ounce of fluid we might have coated a surface of eighteen inches square with metallic arsenic. Not wishing to inhale the black suffocating smoke which it so abundantly produced, we were obliged to throw up the windows and let it pass off.

This experiment, of itself, was perfectly satisfactory to us, though hastily performed, and not with that precision which would be desirable inasmuch as we were to testify in a court of justice. The wife of the diseased, with

several domestics were under arrest, and the court was waiting for us; yet, in a hurried manner, we instituted several other tests.

We removed from the coat of the stomach from six to eight ounces of the white powder, made an aqueous solution of it which saved us the necessity of filtering. Upon a portion of this fluid we instituted the ammoniacal nitrate of silver test, and readily produced the characteristic lemon yellow precipitate, which soon passed to a dark brown, on exposure to the light.

We next proceeded with the ammoniacal sulphate of copper test in the same manner, and readily produced the grass green precipitate. We burned a portion of the powder as obtained from the stomach, upon charcoal, and obtained a distinct garlic odor. We readily obtained the metallic stain and the arsenious acid by holding a glass tube over the flame in the hydrogen test. And so of every test we instituted, they all gave the evidence of arsenic in a large quantity.

The following facts were elicited on the examination:

He had been a very intemperate man, had lived very unhappily with his wife, and for the last two years had been much out of health. On the morning of his death he went down into the town, a distance of three quarters of a mile, and immediately on his return he was seen to drink a fluid from a saucer, upon the surface of which floated a white powder. He soon commenced vomiting, complained of pain in his stomach and bowels, and of feeling faint. There was more or less blood thrown up with the contents of his stomach. At the end of two hours a homeopathic physician was called in and there was therefore nothing done for the patient.* He soon sunk into a collapsed state, and died in five hours after the taking the poison. Some three hours after taking the poison he had several dejections, which were described as being of a strange appearance, and from this description we should think they partook very much of the character of the muco-purulent matter we found in the bowels.

It is a query in our minds whether the poisoning could have had any thing to do with producing that condition of the bowels.

We have no means by which to estimate the quantity taken. But judging from that which remained in the stomach undissolved, he must have taken a drachm at least. It was the large quantity which destroyed life so speedily, we have no doubt, and his speedy death accounts for the want of inflammatory symptoms.

* Would the Homeopathist in such a case give, as a remedy, an infinitesimal quantity of arsenic, in lieu of the hydrated per oxide of iron? — ERROR.

ART. III. — *Case of Dislocation of the Femur, reduced by Dr. Reid's method.* By J. H. BEECH, M. D., Cold Water, Michigan.

I am induced to send you the following report of a case of dislocation of the head of the femur downward and backward: first, because of the rarity of this accident; and secondly, on account of the perfect adaptation of Dr. Reid's femoral lever to the reduction of this luxation:

March 28th, 1852. Was called into the house of Mr. Samuel Otis, about noon, to see his son, aged two years and one month. Learned that, the day before, he was on his hands and knees, when his brother, two and a quarter years older, jumped suddenly upon his back, by which he was instantly brought to the floor upon his face and left shoulder, with the left leg under him. His screams were violent, and continued to be so whenever the limb was moved upon the body. The mother was obliged to keep him in her arms in a partially flexed position most of the time, and even then he seemed to suffer considerably.

I found the thigh slightly swollen; the toes inverted; and the limb half an inch longer than the sound one, when both were made straight, which gave great pain.

When laid upon the back with the thighs at right angles with the body, the knee was more than half an inch lower than the sound one. Laying upon the face, the body upon a pillow, the trochanter major was found farther back than the tuber ischii, and farther from the crest of the ilium, and also, the flatness between the crest and the trochanter, contrasted strongly with the roundness of the other side.

There was no crepitation, nor any difference in the length of the femurs that I could discern. My diagnosis was, of course, dislocation of the head of femur upon the spine of the ischium, or into the lesser ischiatic notch. From the violence of his cries during the examination, I thought an anæsthetic advisable, but as he resisted the inhalation with great energy, it was not persevered in, thinking better to try, first, "Dr. Reid's method."

Accordingly, the little patient was laid on his back upon a hard bed, with the shoulders confined by the mother, and the pelvis and right leg held firm by the father, while I proceeded to flex the thigh upon the body, and the leg upon the thigh, (allowing the toes to take their own direction, that of inversion,) carrying the knee over the right thigh in its passage upward. When a little higher than at right angle, I was confident that I felt the head of the femur come in contact with the edge of the acetabulum, upon which,

ORIGINAL COMMUNICATIONS AND REVIEWS.

I increased the adduction, and continued the flexion, bringing the knee pretty firmly upon the body, and then allowed it to move outward, upon which a sensation of gentle crushing, or sliding, was felt, and the patient altered the tone of his cries very perceptibly. The limb was now brought flat upon the bed, and all restraint removed. The position of the toes, and length of the limb, were found to correspond precisely with the sound one, and after remaining quiet a moment he asked to be taken up, but made no complaint as before, on being moved. He now sat erect on the lap with both hip and knee joints at right angles, and said cheerfully, "I aint sick now."

The reduction did not take half as long, and seemed no more painful than my examination had been. My own feelings were so much like "extacy" in the conclusion, that I was "very much obliged" to *Dr. Reid*, notwithstanding all his competitors for the honor of priority in the use of the shaft of the femur as a lever to reduce luxations of the head upward, &c.

If my letter has not become too lengthy, it may possess enough practical interest to be laid before the professional readers. It is most respectfully submitted to your judgment, and I shall not object to seeing it condensed or clipped with severity provided verity prevails. Perhaps I should add that,

March 30. Patient was comfortable; leg some swollen; has tried to step on it but cried from pain.

May 17. The father says the boy has been as well as ever for some time.

ART. IV. — *On the Bite of the Rattlesnake.* By S. W. WOODHOUSE, M. D.

ACADEMY OF NATURAL SCIENCES, }
Philadelphia, May 22d, 1852. }

I received a letter from my friend Lieut. J. C. Woodruff, in which he said that you would like to receive an account of the bite of the rattlesnake, and its treatment. The only case that has fallen under my observation was unfortunately that of myself; this occurred whilst encamped at the Indian Pueblo of Zani, N. Mexico.

The following is the extract from my Journal:

Wednesday, Sept. 17, 1851. This morning Lieut. J. F. Parke, Top'l Engineers, U. S. Army, and I, were walking out to procure some specimens of birds, and when about two miles from the Pueblo, I came within a few inches of treading upon a rattlesnake who immediately coiled himself up and got ready to strike; jumping back, I drew out my ramrod and struck him over the back with sufficient force to break it. Being a fine specimen I wished

to preserve it without further injury, when placing my gun upon its head, seizing it, as I thought, immediately back of the head, I picked him up, but unfortunately I had too long a hold, when he threw round his head and buried his fang in the side of the index finger of my left hand, about the middle of the first phalanx. The pain was intense, momentarily producing, as it were, a severe shock, and accompanied with much nausea. I immediately commenced sucking the wound, at the same time got Lieut. Parke to apply a ligature round the finger to prevent the too rapid absorption of the poison. I then scarified it freely and continued sucking until I returned to camp.

A man that was with us at the time I sent immediately back to get some aqua ammonia fort, and meet us on the road, which he did when we were about three-fourths of a mile from the town. I applied it immediately to the wound. Mr. Kern hearing what had happened, returned with him, and he wished me to try, as he said, the *Western Remedy*, that is to say, get drunk. This I had often heard of, and I was determined to try its efficacy. He was supplied with a bottle of whiskey, which I immediately commenced drinking; by the time I arrived at the Pueblo, I had drunk half a pint. Already the glands in my axilla were getting sore and painful. Took some ammonia internally, scarified my finger freely and held it in a basin of warm water, which caused it to bleed freely. Then commenced drinking brandy, at the same time held my finger in a cup of ammonia. It took one quart of fourth proof brandy and half a pint of whiskey (enough to have killed a man under ordinary circumstances) to produce intoxication, which only lasted about four hours. During my intoxication I vomited freely; soon after my recovery from this state I removed the ligature and applied a large poultice of Pulv. Sem. Lini. That afternoon I took ammonia internally and some pills composed of Mass Hydrarg. et Collocynth Comp., to act as a cathartic. In the evening the pain in the axilla and finger was very severe; took Pulv. Doveri, grs. x.

Thursday 18th. I passed a restless night without sleep, although during the night I took at least Pulv. Opii, grs. iv. This morning the pain in my finger is intense, and a well-marked line of inflammation extends along the arm to the axilla. I had the entire arm and hand painted with Tinct. Iodine, and the flaxseed poultice renewed, commenced taking a solution of Potassii Iodidi as an alterative. The pills not having operated I took Pulv. Seidlitz, which had the desired effect. Diet, boiled rice. Several times to-day I tried to walk across the room, but each time would be seized with nausea and commenced vomiting. Took at bed time Pulv. Doveri, grs. x.

Friday 19th. I rested pretty well last night, but this morning my hand, arm, and the glands in the axilla, are much swollen and very painful.

Repeated Tinct. Iodine. Diet, boiled farina. Took on retiring, Pulv. Doveri, grs. x.

Saturday 20th. Passed a tolerable night, but my back is getting very sore, as the blankets on the stone floor make rather a hard bed. This morning the pain is very great, and the swelling down my left side as far as my hip. Renewed Tinct. Iodine. I am still attacked with nausea and vomiting on my attempting to walk.

I removed the skin from off my finger, and it discharged freely a watery sanguinous fluid without smell. The nail is becoming loose. The broad-red line following the course of the lymphatic, is now filled with a yellowish serum. The point where the fang entered, for three-eighths of an inch in diameter, is of a dark brown color. Renewed the poultice. At bedtime took Mass Hydrarg. grs. v, Pulv. Doveri, grs. x. Continued Potassii Iodidi. Diet the same.

Sunday 21st. Passed a restless night, being much troubled with colic; took Magnesia Calc. et Spts. Menth Pip., which relieved me, and not having my bowels open took Pulv. Seidlitz, which had the desired effect. Hand much swollen and filled with serum. Diet as usual.

Monday 22d. Passed a comfortable night. The swelling has left my side and arm, but little remains in the hand. I can now walk a few yards without being seized with nausea; have been sitting up the most of the day. Continued Potassii Iodidi. Diet, mutton broth and farina.

Tuesday 23d. I awoke this morning much improved, the swelling and pain having left, with the exception of the finger, the first and second joint of which does not present a healthy appearance, the palmar surface having the appearance of gangrene, but the discharge is thin and watery, without smell. The granulations do not present a healthy appearance, they are rough, and many of them look as if they were sprinkled with yellow ochre. The nail is quite loose. Continued Potassii Iodide. Diet, mutton broth, with a little of the meat.

Wednesday 24th. This day we commenced our march. I placed my hand in a sling and mounted my mule; found myself rather weak, and the mule hard to manage with but one hand; the sun was rather hot, this, with the jolting of the animal caused me to suffer considerable pain; fortunately for me, after doing six miles, we encamped. I removed the nail. From this time on the finger gradually improved. I continued renewing the poultice daily until the last of October. In the meantime there was a large slough

which gradually came away and left the last phalanx exposed in two places. The granulations required occasionally the application of nitrate of silver. After this I made use of dressings of Cer. Simplex. Continued carrying my hand in a sling until the middle of November. A new nail commence growing and a sinus remained open in the end of the finger; upon the introduction of the probe into the latter, the bone could be felt quite rough. A discharge from this kept up until about the 7th of February, when I removed the exfoliation of the end of the phalanx, showing evidently that the fang had entered the periosteum. Soon after this the sinus closed, leaving the finger in a deformed state, ankyloses having taken place in the first joint. The circulation is very imperfect, one of the arteries being destroyed, which renders it very susceptible of cold. The insertion of the flexor muscle has also been destroyed.

I have heard of a number of instances of rattlesnake bites, in all of which the patient recovered if they succeeded in producing intoxication.

Dr. Fischer C. Smith, of this city, accompanied Capt. French, A. Q. M. U. S. Army, to El Paso last year, and on their return one of the teamsters was bitten by a rattlesnake, he gave him nothing but whiskey, and in three days after he was driving his team. In this case it took three pints of whiskey to produce intoxication.

Should this brief extract be of any service to you it is at your disposal.

ART. V.—*On the Use of Muriate of Ammonia in Cynanche Tonsillaris.*
By A. B. MCKAY, Green Bush, Illinois.

Although this disease is rarely fatal, yet it is one of the most distressing the human family is subject to. It is this which has induced me to lay before the profession a few remarks on a remedy which I have used with complete success in many cases of this disease; in fact, in every case it has succeeded, in which I have used it. The remedy is *muriate of ammonia*, or *sal ammoniac*.

My usual method of administering it is as follows:

℞ Ammon. Mur. ℥iii.
Tinc. Opii, f℥i.
Aceti, f℥iv.
Misce.

As soon as the vinegar is well saturated with the ammonia, I let the

patient use it freely and often, as a gargle. In the absence of vinegar and laudanum, I have powdered the ammonia and blown it on the affected parts. This remedy has succeeded so perfectly in my hands, that I am almost inclined to think it a specific in this disease. I use it with equal success in all stages.

May 14, 1852.

ART. VI.—*Third Clinical Report on Continued Fever, based on an Analysis of Sixty-four Cases.* By the EDITOR.

(Continued from page 31 and concluded.)

SECTION NINTH.

Symptoms (exclusive of eruptions) referable to the Skin.

Having given in the Second Report the symptoms relating to temperature, dryness, moisture and sweating, as noted in a considerable number of cases, individually, on successive days during the progress of the disease, I shall dispense, in the present Report, with these details. Were I to prepare similar tables (the data for which are before me) so far as I can judge from a general survey of the facts, the results would be materially the same. There would be nothing gained by such a repetition. I propose in this section to study the cases with reference to a single subject, which is one of interest and importance. I refer to the occurrence of perspiration in the course of Continued Fever, its influence on the progress and issue of the disease, and its significance as a prognostic. The results disclosed by the former analyses led to conclusions at variance with ideas which are frequently, if not generally entertained by medical practitioners. These conclusions were, "that we are not warranted in predicating expectations of speedy convalescence, or of recovery, upon either of these symptoms (moisture and free perspiration) disconnected from other circumstances, nor do these results afford any grounds for supposing that to induce moisture or sweating by therapeutical means, will be likely to prove beneficial." Now, do the facts contained in the present collection of cases sustain these conclusions? In the former analyses it was found that moisture and sweating "were observed at different periods of the febrile career, in a large proportion of instances not preceding by a short space of time the date of convalescence. Moreover, these appearance were observed in nearly one-half of the cases which ended fatally." These were the considerations upon which were based the conclusions just stated. Let

us see if, on examination of the cases in this collection, facts are found to correspond with those heretofore developed.

It will suffice to interrogate the histories respecting the presence of sensible perspiration without taking pains to state whether it consisted in moisture, or in free sweating.

Excluding fatal cases, of the *thirty-seven* remaining in the *Typhus* group, in *twenty-eight*, moisture of the surface or sweating more or less profuse, occurred either once, or several times, during the course of the disease, while the patients were under observation.

Of the *nine* cases of *Typhoid*, excluding the fatal cases, the same was true of *five*.

Of the *twenty-eight* cases of *Typhus* characterized by perspiration, it took place at, or near the date of convalescence in *eleven*; and at other periods of the disease, not being speedily followed by a favorable termination of the febrile career, in *seventeen*.

Of the *five* cases of *Typhoid* characterized by perspiration, it took place at, or near the date of convalescence in not a single instance, in all the cases not being speedily followed by a favorable termination of the febrile career.

According to these results, then, the occurrence of moisture or sweating in the course of Continued Fever, considered abstractedly, cannot be considered an indication that convalescence is near at hand. The balance of probabilities, in fact, is against such an anticipation.

Directing attention now to the fatal cases, of the *five* cases of *Typhus* ending fatally, perspiration occurred toward the close of life in *two*, and at periods in the disease more or less remote from the close, in *three*.

Of the *five* fatal cases of *Typhoid*, perspiration occurred toward the close of life in *three*, and at other periods in *two*. Thus, in one half the cases of either type ending fatally, perspiration took place irrespective of its occurrence at, or near the time of death.

The foregoing results, it will be perceived on reference to the former Reports, accord, in a striking manner, with those developed by the previous analyses. The views therefore, as set forth in the quotation from the Second Report, respecting the importance to be attached to the occurrence of perspiration in the progress of Continued Fever, and to the efforts to produce this symptom by therapeutical means, are sustained, provided the reasoning therein involved be legitimate.

Another mode of interrogating the facts contained in the cases respecting the influence of perspiration on the progress of the disease, is to examine the symptoms immediately preceding, and following its occurrence. It may be

supposed that perspiration generally accompanies an abatement of the severity of the disease, although it does not in the majority of instances herald convalescence. We will direct a little attention to this point. And in order to determine the condition of the patients prior, and subsequent to the occurrence of perspiration, it will probably answer to ascertain the relative frequency of the pulse at these times. The pulse may be considered a thermometer of the grade of severity of the disease sufficiently for the present object. I will give the enumerations of the pulse, in several cases, on the day previous to the occurrence of perspiration, the day on which the perspiration was noted, and the subsequent day. In the following table the instances in which perspiration occurred at periods of the disease more or less remote from the date of convalescence, will of course only be selected. In some cases perspiration continued for several days in succession, or was frequently repeated. These cases will be rejected. Fatal cases are also excluded:

Pulse day before Perspiration.	Day of Perspiration.	Subsequent day.
No. 1. <i>Typhus</i> , 120.	120 A. M. 116 P. M.	100.
No. 2. " 108.	104.	104.
No. 3. " 132.	124.	118.
No. 4. " 108.	100.	88.
No. 5. " 128.	128.	120.
No. 6. " 156.	134.	128.
No. 7. " 110.	120.	104.
No. 8. " 106.	124.	136.
No. 9. " 90.	100.	108.
No. 10. " 111.	108.	98.
No. 11. " not noted.	120.	120.
No. 12. " 120.	112.	120.
No. 13. " 112.	138.	112.
No. 14. " not noted, day of admission.	100.	96.
No. 15. " 132.	*126 and 116.	118.
No. 16. <i>Typhoid</i> , 132.	100.	100.

* Perspired two consecutive days.

The results displayed in the foregoing table, obtained after the preceding portion of this section was written, are interesting, and, I confess, to me unexpected. In this point of view, perspiration appears either to occasion, or signify, an improvement, in so far as the pulse is a criterion of the latter.

Finding no grounds, numerically, for the opinion that this symptom is generally the precursor of convalescence, it was natural to presume that it would be found to possess small importance as a means, or a sign of improvement. It would appear, however, that in a very large proportion of instances, it is followed by more or less diminution in the grade of severity of the disease. This being the case, the views which have been expressed respecting the probable inutility of diaphoretics may require to be modified, since it is manifestly a legitimate object of therapeutics to endeavor to abate the intensity of disease when we are unable either to arrest it, or abridge its duration.*

In several of the cases ending in recovery in this collection, moisture, or sweating more or less copious, occurred several times during the febrile career. It may be worth while to inquire respecting the gravity of the disease in the cases thus characterized. With reference to this inquiry, I will throw into a tabular form the facts contained in the histories of these cases, relating to—*first*, the number of days on which either moisture or free sweating occurred; *second*, the mean frequency of the pulse during the febrile career; *third*, the duration of the disease from the time of taking to bed to convalescence, and *fourth*, the number of days the patients were under observation.

Cases.	No. of days on which moisture or sweating was noted.	Mean frequency of Pulse.	Duration of Disease.	Number of days under observation.
1. <i>Typhus.</i>	5.	112.	13 days.	9.
2. “	5.	107.	12 “	12.
3. “	7.	108.	13 “	13.
4. “	4.	100.	12 “	12.
5. “	5.	94.	14 “	11.
6. <i>Typhoid.</i>	8.	89.	16 “	16.
7. “	5.	80.	16 “	16.
8. “	3.	80.	23 “	16.

The results exhibited in the foregoing table show a grade of severity, as exhibited by the pulse and duration, somewhat less than the average in all the cases characterized by the more or less frequent recurrence of perspiration, with a single exception, viz., the first case in which the mean frequency of the pulse was 112. The inference, therefore, from the cases now analyzed,

* The reader will observe the table did not embrace cases in which perspiration occurred for several days in succession, or was frequently repeated.

would be that, other things being equal, the repeated occurrence of perspiration is generally associated with mildness of the disease. This inference, however, is by no means to be adopted as a law of Continued Fever, for, in the first place, the data are too few for such a deduction, and, in the second place, it is to be considered that perspiration was repeated in several of the small number of cases in this collection which proved fatal. The facts with respect to the latter point are as follows:—Of the *ten fatal* cases of both types, perspiration occurred more than once in *four*. In one case it was noted on *five* days; in one, *three* days; and in *two* cases, *two* days.

SECTION TENTH.

Symptoms referable to the Genito-urinary System.

Notwithstanding the views expressed in the Second Report, of the importance of recording examinations of the urine in cases of Continued Fever, the histories that, in the mean time, I have collected, contain very little relating to this subject. The few observations that were made, I will give as noted in the cases respectively.

Typhus.

No. 1. On the *third* day the urine was acid, with a slight phosphatic deposit, and the sp. gr., 1.016.

No. 2. On the *first* day the sp. gr. was 1.023, and there was a slight trace of albumen.

No. 3. On the *second* day, the urine contained a small quantity of albumen. It deposited on cooling the urate of ammonia. On the *twelfth* day, sp. gr. 1.023.

No. 4. On the *second* day, sp. gr. 1.026. There was a considerable deposit of the urate of ammonia, and a trace of albumen. On the *fourth* day, sp. gr. 1.014, and a small quantity of albumen.

No. 5. On the *tenth* day the sp. gr. was 1.026; it deposited urate of ammonia, and exhibited a trace of albumen.

No. 6. On the *fourteenth* day, sp. gr. 1.0–5, and a trace of albumen.

No. 7. On the *sixth* day, deposited urate of ammonia. No other change.

No. 8. On the *sixth* day, sp. gr. 1.022, a small deposit of urate of ammonia, and a trace of albumen. On the *twenty-first* day (three days before convalescence) sp. gr. 1.023, and normal.

No. 9. On the *sixth* day, sp. gr. 1.014, and a small quantity of albumen.

Typhoid.

No. 10. On the *ninth* day, sp. gr. 1.026, deposit of urate of ammonia.

Twelfth day (four days before convalescence) deposited urate of ammonia. Day after convalescence, sp. gr. 1.019.

These isolated observations are of course insufficient for analytical examination. They show that urate of ammonia is apt to be deposited at different periods of the febrile career, and that the urine frequently contains a small quantity of albumen. If a conjecture were to be indulged respecting the latter, it might be attributed to a certain degree of congestion, which it is probable exists internally, as well as upon the surface of the body, and from which the kidneys would not be likely to be exempt.

The facts relating to the specific gravity have little or no value, inasmuch as the quantity of the secretion was not ascertained.

SECTION ELEVENTH.

Duration of the disease. Sequelæ. Mode of dying. Fatality.

Duration. In order better to compare the two types with respect to duration, I will examine the facts pertaining to each type under a separate head, giving, in this Report, results without details. The duration from two points of view only will be considered, viz., from the date of taking to the bed, and from the time of admission into hospital, to the day of convalescence, or to the day of death.

The duration of the access has already been considered in another section. It would be useless to determine the duration in the cases not fatal to the time of discharge from hospital, as this was too often affected by circumstances entirely foreign to the disease.

Typhus. The data for determining the duration from the date of taking to the bed, to the day on which convalescence was pronounced, are contained in the histories of *thirty-four* cases. The mean duration is *fifteen* 8-17 days. The maximum of duration, in this point of view, was *twenty-six* days. The minimum duration was *nine* days.

The duration from the time of admission into hospital to the day of convalescence is ascertained in *twenty-five* cases.* The mean duration is *eleven*. 9-25 days. The maximum duration, in this point of view, was *twenty* days, and the minimum duration, *six*.

Typhoid. The data for determining the duration from the time of taking to the bed to the day of convalescence, are contained in the histories of *six* cases. The mean duration is *eighteen* 1-3 days. The maximum duration was *twenty-three* days. The minimum duration, *eleven* days.

* The remainder of the patients were inmates of the hospital when attacked.

The duration from the time of admission into hospital to the day of convalescence, is determinable in *seven* cases. The mean duration is *thirteen* days. The maximum duration was *sixteen* days. The minimum duration, *nine* days.

These results approximate closely to those developed by the first analysis, differing, as did the latter, from those afforded by the second.

They show a shorter mean duration in *Typhus*, dating both from the time of taking to the bed, and admission into hospital.

The cases of *Typhoid* approximated, individually, nearer the mean duration than those of *Typhus*, the latter type affording the greater maximum, as well as minimum duration.

Directing attention, now, to the duration of the *fatal* cases, the following are the results:

Typhus. Mean duration from date of taking to bed, to fatal termination, *twelve* 3-5 days. From time of admission into hospital to fatal termination, *seven* days.

Maximum duration, from taking to bed, *eighteen* days; minimum, *nine* days.

Maximum duration, from admission into hospital, *nine* days; minimum, *five* days.

Typhoid. Mean duration, from date of taking to bed to fatal termination, *twenty-one* 1-5 days. From time of admission into hospital, to fatal termination, *fourteen* 4-5 days.

Maximum duration, from taking to bed, *twenty-seven* days; minimum, *thirteen* days.

Maximum duration, from admission into hospital, *twenty* days; minimum, *twelve* days.

Sequelæ. A large proportion of the patients remained in hospital but a short period after convalescence. My observations relative to *sequelæ*, therefore, not extending over much space of time, are few. I will give all the facts which appear in the histories.

Typhus.

Case 1. Mild diarrhœa occurred during convalescence.

Case 2. Sloughing, on the nates, superficial, and to a limited extent, occurred about the time of convalescence.

Case 3. Several furunculi appeared shortly after convalescence.

Case 4. Ecthyma occurred shortly after convalescence.

Case 5. In this case the patient became convalescent, and sat up on the

twenty-third day after the date of the attack. On that afternoon he was attacked with a severe chill which lasted an hour, during which the pulse could not be felt at the wrist. Five or six months prior to being attacked with *Typhus*, he had had Intermittent fever. The pulse became very frequent when reaction took place, numbering 148. Several dejections occurred, and muttering delirium. The next day he had another chill in the afternoon, and on the following morning perspired freely. Death occurred on the succeeding day. The record of the symptoms after the relapse occurred is rather imperfect.

Case 6. This case was followed by tuberculous deposit in the lungs and spleen, and death in about two months after convalescence. The patient had had *Typhoid* fever a short time before being attacked with *Typhus*, the latter disease being developed while he remained in hospital after convalescing from *Typhoid*.

Typhoid.

Case 1. Twenty-two days after the date of convalescence, the patient was attacked with Dysentery, which proved fatal.

Case 2. Parotitis occurred at about the time of convalescence. Reference has already been made to this case in the section relating to the digestive system.

Relapses. Case No. 5, (*Typhus*), in the foregoing list, might perhaps with propriety be considered a case of *relapse of fever*. Aside from this case, relapse occurred but in a single instance. In this respect the present collection as well as the first, presents a striking contrast to the second. In the single instance referred to, the patient was seized with headache, pain in the limbs, and febrile movement, *seven* days after the date of convalescence, having, in the mean time, been able to sit up a little. On the fourth day after the second attack, free perspiration occurred, which was renewed for several successive nights, and the patient was again shortly convalescent.

Mode of dying. Typhus cases.

1. Ninth day after taking to bed, by asthenia.
2. Eleventh do., and fifth after admission, by asthenia, preceded by apnœa.
3. Twelfth do., and seventh after admission, by apnœa, apoplectic coma.
4. Eighteenth do., and ninth after admission, by asthenia.
5. Thirteenth do., and seventh do., by apnœa, apoplectic coma.

Typhoid cases.

1. Fifteenth day after taking to bed, and twelfth after admission, by apnœa, apoplectic coma.

2. Twenty-seventh do., and twentieth do., by asthenia.
3. Thirteenth do., and twelfth do., by apnoea.
4. Twenty-fifth day do., and twelfth do., by asthenia.
5. Twenty-sixth day do., by apnoea.

Fatality. The number of cases that proved fatal has already been stated, viz., *five* of the cases of each type, i. e. *ten* in the whole number of cases analyzed, viz. *sixty-four*. In this enumeration the instances of death occurring subsequent to the career of fever are not included. Death took place in one case from dysentery supervening on *Typhus*; another from relapse of fever; and a third from tuberculosis. Since in each of these instances the patients had convalesced, and the issue was not due to the fever except so far as it may have predisposed to the consecutive affections, or diminished the power of resistance inherent in the organism, it would hardly be proper to consider these as cases of fever ending fatally.

As already stated, one case of *Typhus* which, the history not having been recorded, is ~~not~~ included in the collection, ended fatally. I refer to the death of the Sister of Charity mentioned in the Second Section. In determining the rate of mortality this should be included, which makes *eleven* of the ~~sixty-five~~ *sixty-five* cases, proving fatal. This gives nearly a ratio of *one* death to every *six* cases, or *17 per centum*.

This rate of mortality is less than in the two collections of cases previously analyzed. For reasons assigned in the last Report, it would be unfair to infer from this difference the superiority of the management of the cases in this collection. The treatment, as will be seen in the section devoted to that subject, was not altogether the same as heretofore, but the fluctuations in fatality at different times and places are so great as almost to preclude any positive deductions as to the success of any particular plan of management in any collection of cases. Negative conclusions, however, may be adopted with less reserve, in view of a large proportion of recoveries. In other words, the treatment under these circumstances, it may fairly be claimed, could not have been highly pernicious.

A striking point pertains to the dates of the fatal cases. Up to Jan. 3d not a single death from Continued Fever had occurred. More than half of my six months' service had then expired, and I had then treated *fourteen* cases of *Typhus*, *seven* cases of *Typhoid*, and *three* cases of *doubtful type*, in all *twenty-four* cases. The dates of admission of the fatal cases, respectively, were as follows: *Typhus*.—Jan. 3; Jan. 15; Jan. 15; Jan. 26; March 5. *Typhoid*.—Jan. 19; Jan. 26; Jan. 29; Feb. 18; March 11. Thus, it is perceived, *four* of the *five* recorded *Typhus* cases, and *three* of the *five*

Typhoid cases proving fatal, were admitted in the month of January. If, now, the reader will turn to *Section First*, he will find that during the month of January *eleven* cases of *Typhus*, and *three* of *Typhoid* were admitted, making *fourteen* cases; so that one-half of the cases received during this month proved fatal—an enormous proportion compared with the ratio of deaths to recoveries in any other month. It would hardly seem probable that this was owing merely to coincidence, and, if not, there must have existed during that month, for reasons utterly inexplicable, an unusual tendency, in cases of fever, to a fatal result. Striking differences in this respect, in different years, and at different places, are abundantly established. It is only circumscribing within narrower limits a similar fact, to show that a greater liability of death obtains during one of a series of months, in the same situation, and under the same kind of management.

The two previous analyses developed a larger ratio of fatality in the *Typhus*, than in the *Typhoid* cases. The reverse of this is true of the present collection. The proportion of deaths was considerably greater in the *Typhoid* group, being as 5—14 is to 6—42. This disparity shows that, for reasons unknown, there existed a stronger tendency to a fatal result in the *Typhoid* cases. Compared with the previous collections, the ratio of fatality, in the cases of the latter type, is much greater than heretofore, being nearly 36 per cent.; while, on the other hand, the ratio of fatality in the *Typhus* cases is as strikingly less, being a fraction below 14 per cent.

SECTION TWELFTH.

Examinations after death.

I shall restrict myself, in this Section, to the appearances presented in the intestinal canal, more especially the small intestine. The appearances in the latter situation are interesting in consequence of their bearing on the question of the identity or non-identity of *Typhus* and *Typhoid* fever. My reasons for taking this limited view of the post mortem examinations are, *first*, it is with reference to the question just stated that the contribution of facts is, in a special manner, desirable; and, *second*, for the reasons given in the previous Reports, the dissections did not generally embrace an inspection of all the important parts of the body. Intestinal lesions were always sought for with care, and the appearances recorded, but in most of the cases the condition of some organs was not ascertained, and, except with reference to some special point of interest like that relating to the lesions stated to be characteristic of the *Typhoid* type, an autopsy is unsatisfactory which does

not embrace all the facts, negative as well as positive, which the scalpel can disclose. I have already had occasion to refer to two instances of death by sudden coma, with spasmodic Inspiration, (Section Seventh,) in which the larynx was inspected in order to ascertain whether oedema, or other morbid conditions existed in that part. Any other facts noted, aside from the intestinal appearances, which shall appear to have any particular interest, will be mentioned; otherwise, for the sake of brevity, they will be omitted.

Of the *five* fatal cases of *Typhus*, examinations were made in *four* instances; and of the *five* fatal cases of *Typhoid*, examinations were made also in *four*. In addition to these I have had an opportunity of inspecting the intestinal canal and other parts of a patient who has died since the expiration of my term of service, the appearances taken in connection with the history of the case being peculiarly interesting. In another case, not embraced among the cases of death from fever, in which the patient, after sitting up, suffered a relapse which proved fatal, a post mortem examination was made. The appearances in this case, which was of the *Typhus* type, may also be included.

I will give a succinct statement of the results of these examinations of the cases respectively.

Typhus.

Case 1. Death by asthenia, on the 11th day. Several of the Peyerian patches developed so as to be visible, with black points, presenting the *shaven beard* appearance. No enlargement of the mesenteric glands. A patch of arborescent redness in the ileum just above the coecum. Large intestine free from disease.

Case 2. Death by apnœa, on the 12th day. A few Peyerian patches developed so as to be visible. No enlargement of mesenteric glands.

Case 3. Death on the 18th day, by asthenia. Deep capilliform redness of mucous membrane lining the lower part of the ileum. A single Peyerian patch near the coecum, and solitary glands developed so as to be visible. No enlargement of mesenteric glands.

Case 4. Death, by apnœa, on the 13th day. Several Peyerian patches developed so as to be visible. Mesenteric glands slightly enlarged, the largest being of the size of a small pea. Two small *Tricocephali* found in the coecum.

Case 5. Death on third day after relapse of fever, the patient having convalesced from Typhus. An account of this case is given in *Section Eleventh*. So far as the post mortem appearances are concerned, it has as much value as any of the *Typhus* cases.

The Peyerian patches presented the *shaven beard* appearance, being developed so as to be visible. Some of the solitary glands also developed.

Mesenteric glands somewhat enlarged.

Several small ulcerations in coecum.

Typhoid.

Case 1. Death, by apoplectic Coma, on the 15th day. The Peyerian patches greatly developed, projecting two or three lines above the level of the surrounding mucous membrane. This enlargement was most marked in the patches nearest the coecum, and it progressively diminished on ascending the tube. The patches did not present any sloughing, except, to a limited extent, in the patch nearest the coecum. The mesenteric glands much enlarged, some as large as filberts.

Case 2. Death, by asthenia, on the 27th day. Peyerian patches much diseased, those nearest the coecum presenting deep excavations exposing the fibres of the muscular tunic. On ascending the tube, in some of the patches partial sloughing of the glands and morbid deposit had occurred, while in those still higher in the tube the morbid deposit remained, i. e. the patches were salient, and not ulcerated.

Mesenteric glands moderately enlarged.

Case 3. Death, by asthenia, on the 25th day. { Peyerian patches extensively ulcerated, a large proportion presenting excavations, the morbid deposit and the glandular follicles having sloughed away.

Mesenteric glands much diseased.

Three tricocephali in the coecum.

Case 4. Death on about the 26th day, by apnœa. Peyerian patches developed, and those nearest the coecum presenting excavations. The solitary glands enlarged. Mesenteric glands enlarged.

Case 5. Death fifty-three days after convalescence from Typhus, the patient having previously had Typhoid, being attacked while at the hospital with the former type shortly after convalescence from the latter. To this very interesting case I have already had occasion to advert, (Section Sixth.) The patient entered the hospital with *Typhoid* fever, Dec. 27th. He had then been confined to the bed seven days. He was convalescent Jan. 11th, (sixteen days.) On February 7th he took to the bed with *Typhus*, and was convalescent from the latter on the 23d of February, (seventeen days.) Death occurred on the 15th of April, fifty-three days after the date of convalescence from *Typhus*, and ninety-six days from the date of convalescence from *Typhoid*. He presented successively the characteristic symptoms of each type

of Continued Fever, including the eruptions. The following post mortem appearances were observed: The *lungs* contained numerous disseminated tubercles, and some masses of about the size of filberts. The tuberculous deposit was most abundant in the right lung. There were no excavations.

The surface of the *spleen* was studded with tubercles of pretty uniform size, about as large as small shot. Being reserved, in order that its external appearance might be copied, it was not examined internally.

The peritoneal surface over the liver presented a space of about the size of the palm of the hand, which was thickly covered with a granular deposit, apparently of tubercle.

The small intestine presented near the coecum an oval space presumed to be the site of a Peyerian patch, of a slate color, without any black points, not depressed, nor salient. The dark color caused the patch to be well defined, contrasting with the pale color of the surrounding mucous membrane. The mucous membrane over this patch was somewhat softened. Upward the same discoloration was observed in several patches, but less in degree. Numerous small oval excavations existed in these patches, extending below the mucous coat, not presenting a granulated or ulcerated aspect, but apparently covered with a delicate, serous like membrane. Several similar depressions, also, existed in the mucous membrane in the neighborhood of the Peyerian patches. Some twenty or more of these excavations were counted, being less numerous and more superficial in proportion to the distance from the coecum.

The mesenteric glands were enlarged, some being as large as a small sized bean.

The appearances in the foregoing case (No. 5) are interesting from the fact that they were the remnants of the characteristic lesions of *Typhoid ninety-six days* after convalescence from that type of fever, the patient having, in the mean time, passed through *Typhus*.

In the two former Reports the appearances found after death were submitted without comment. The few remarks which I shall offer on the subject now, will be based on the results of the examinations in fatal cases contained in all the three collections. My remarks, as already stated, will be limited to the appearances found in the small intestine. What are the conclusions to be drawn from these data as respects the appearances which distinguish the two types from each other? It is chiefly with reference to this inquiry that I propose to review the facts which I have recorded.

The three Reports contain an account of the appearances of the small intestine in *twenty-two* cases. Of these twenty-two cases, *thirteen* were of

the *Typhoid* type, *eight* were of the *Typhus* type, and in one case the patient had lately passed through both types. In each of these cases the diagnosis was made during life, and based on evidence deemed unequivocal. In nearly all the cases the distinctive symptoms embraced the peculiarities of the eruption. There can be no room for doubt that in every instance the *ante mortem* history, exclusive of the *post mortem* appearances, fully warranted the determination of type. I am particular to make this statement, for it is obvious, when we are prosecuting investigations in order to ascertain what lesions distinguish either type from the other, it is necessary to settle positively on the diagnosis in individual cases, irrespective of the lesions of which we are in search. In other words, with reference to the question which has just been propounded, the *post mortem* appearances should not enter into the diagnosis. On this point the reader is referred to some remarks contained in the supplement to the two first Reports, under the heading of the "*Identity of Typhus and Typhoid fever.*"

What are the morbid appearances that have been found in the *Typhus* cases? In each of the *seven* examinations in cases of this type, the Peyerian patches have been somewhat changed, the alteration consisting apparently of a *slight* degree of hypertrophy, causing their development so as to render them visible. In some instances the patches have been studded with black points, giving rise to what has been called the *shaven beard* appearance. Occasionally the solitary glands have exhibited the same kind of development, and the glandular bodies of the mesentery have been *slightly* enlarged. In no case of the *Typhus* type have the appearances exceeded those just described, but more or less of these appearances have been uniformly observed. Both these facts are important to be borne in mind. These results prove sufficiently the incorrectness of the statement that the Peyerian patches and mesenteric glands are entirely unaffected in *Typhus*. So far as these results go, they show, on the contrary, that the Peyerian follicles are uniformly affected, but they contain proof only of a certain amount of morbid change, consisting, judged by the gross appearances, simply of a slight degree of abnormal development or hypertrophy.

On the other hand, what are the morbid appearances in the *Typhoid* cases? In each of the instances of this type the Peyerian patches exhibited a *notable degree* of alteration, the change not consisting in merely a slight development, but in great enlargement, causing the patches to project several lines above the level of the surrounding mucous membrane, or in excavations involving more or less destruction of the mucous coat and follicles, the surface of these excavations sometimes having a granular, ulcerated aspect, and

in other instances covered by a thin, transparent serous-like tissue. Generally, the solitary glands were also the seat of similar excavations, or of great enlargement. The mesenteric glands, also, were invariably increased in size to a much greater extent than in the *Typhoid* cases, in some instances being quite large. To be more precise with respect to the *Typhoid* type, in *three* of the *thirteen* cases the Peyerian patches were notably enlarged, without excavations, and in the remaining *ten* cases the latter, with more or less enlargement, existed.

These appearances show, in the *Typhoid* cases, a morbid deposit in the Peyerian patches taking place early in the disease in considerable abundance, and in the *Typhus* cases simply a tumefaction. They show, at a more advanced stage of the *Typhoid* type, a sloughing of the enlarged glands with the morbid deposit and the mucous covering, leaving ulcerated excavations extending to the muscular tunic, sometimes leading to rupture and perforation; and, at a later stage, more or less advance toward cicatrization and filling up of these excavations; while, in the *Typhus* cases, in no instance were these processes of ulceration and sloughing apparent.

Although, therefore, the Peyerian patches are not wholly unaffected in *Typhus*, the changes, when compared with those that obtain in *Typhoid*, are quite insignificant. The contrast, as remarked in another place, is scarcely less striking than if the follicles in *Typhus* had remained invisible. The difference in the appearances is as great as that, for example, which exists, on the external surface of the body, between the efflorescence of measles, and the pustular eruption of small pox. The force of the difference in its bearing on the question of the identity or non-identity of the two types, cannot be considered to be much impaired by the fact that the Peyerian patches are frequently, if not generally changed in a slight degree in *Typhus*.

It seems to me more important to dwell on these points with some emphasis, because an idea appears to be very generally entertained that if the Peyerian patches are found to be affected in ever so slight degree in *Typhus*, its non-identity with *Typhoid* is disproved. But this conclusion assuredly does not follow, if it be true that the affection of these parts in *Typhoid* is quite different in character, as well as degree, from that in *Typhus*. Moreover, observers are likely to be confused, and distrustful in verifying the differential diagnosis by the intestinal appearances, in consequence of the erroneous idea just mentioned.

As respects the intestinal lesions of *Typhoid* fever, several points of interest remain to be studied. The nature of the *typhous* material, or deposit, does not appear to have been ascertained. It has been compared to the

tuberculous deposit, and it appears to pass through changes somewhat analogous to the evacuation of tuberculous material into the bronchial tubes.

The Peyerian patches are frequently destroyed by the sloughing process. Are they afterward reproduced? What is the condition of these parts at a period considerably remote from the time of convalescence, after the processes of restoration are completed? Do the Peyerian patches *invariably* present the notable degree of alteration found in my examinations in *Typhoid*, and, in what proportion of cases, are they wholly unaffected in *Typhus*? These, and other inquiries which suggest themselves in this connection, afford interesting fields of research.

A point of interest, in conclusion, relates to the history of the eruption in the cases in which examinations were made after death. Directing attention to the cases entering into the present collection, an eruption existed in *two* of the four cases of *Typhus*, and in *all* the four cases of *Typhoid*. In one of the *Typhus* cases the eruption had mixed characters, i. e., a combination of the *Typhus* and *Typhoid* eruption. In one of the *Typhoid* cases, only *four* "rose spots" were counted, and in another case only *five*. The two last mentioned cases show that the intestinal lesions bear no relation of proportion to the abundance of the eruption.

SECTION THIRTEENTH.

Treatment.

The cases in this collection were treated with greater simplicity, but, in some respects, more efficiently than those heretofore analyzed. The first object in the management, of course, was to direct those measures which, in the opinion of the reporter, would most conduce to the welfare of the patients, and afford the best chance of recovery. It is quite superfluous to add, that this object must ever be paramount to every other in the minds of conscientious practitioners. Objects, however, of a scientific character, may properly, and in fact should, whenever practicable, be superadded — objects relating to self improvement, and the collection of facts by which, it is to be hoped, science may be enriched, and the resources of our art enlarged. In the latter point of view, two ideas were prominent in my mind during the time the cases were in progress. One of these ideas was to prescribe medicinal remedies only when clear and definite therapeutical indications were recognized, adopting, as heretofore, the expectant method. Another idea was to meet the indications which appeared to present themselves, directly

and efficiently, employing for this end the fewest and simplest means adequate thereto. These ideas, it may be said, so far from possessing any novelty, are those upon which a rational practice is usually based. Without stopping to engage in any discussion on this topic, I will merely remark, that reflection and observation, as it seems to me, must convince us that every one is open to more or less self-deception on this score. So difficult is it to repress the tendency to resort to therapeutical interference, that there are few, if any, acute affections concerning which it is not still a desideratum to know what would be the consequence of permitting them to run their course uninfluenced by medication. All must admit the importance of this knowledge as a point of departure for rational principles of management. I have no reluctance to confess a desire to observe the progress of Continued Fever divested of all extrinsic influences save those which, considering my views of what was due to the interests of the patient, I should not be justified in withholding. The same tendency which stands in the way of refraining from all medication, even when it might be better for the patient, as well as more conducive to scientific knowledge, also leads to the multiplication of different remedies for the attainment of a common end. The unfavorable effect of the latter upon medical observations is two fold. It is obviously difficult to assign to each remedy its proportionate degree of efficacy when several are employed conjunctively. This is one evil. Another is, that in distributing our expectations of benefit among a variety of means, we are apt to attach undue importance to some, or to their collective operation, and thus employ too sparingly those which are the most efficient, and upon which it were better that the main reliance should be placed. Having aimed to have these considerations exert, practically, their full effect, the treatment of these cases, as already remarked, was even more simple than that pursued during the two previous years; and I shall be better able to submit a summary of the facts pertaining to this, certainly not the least interesting or important part of the history. In endeavoring to give the reader an account of the kind and amount of medication practiced, the most convenient method will be to consider the different remedies employed, under separate heads. I will, therefore, examine the records of the cases with reference to the extent to which different articles of the materia medica were prescribed.

In two cases, both of the *Typhus* type, no medicinal remedies whatever, were prescribed. The disease passed through its career favorably, sanitary measures being alone directed. In all other instances some remedies were employed.

Laxatives. Laxatives were prescribed in several cases in which no

dejection had taken place for several successive days. In no instance was an active cathartic administered. The laxative measures consisted either of castor oil taken by the mouth in the dose of half an ounce, or in simple enemata. The oil was prescribed, I find, in *ten* cases. *Nine* of these ten cases were of the *Typhus* type. In every case but one, oil was given once only during the course of the disease. In the case furnishing the single exception to this statement, the oil was repeated on the following day. It was given in nearly every instance early in the disease, usually at the time the patients entered the hospital.

A single enema was prescribed in *three* cases, all of the *Typhus* type.

In one case in which castor oil was given, and in one of the cases in which an enema was resorted to, the dejections which followed were so frequent as to call for remedies to restrain them, which, in both instances, at once succeeded. The same was true of another case in which the patient had taken some cathartic just before entering the hospital.

In *two* instances (*Typhus*) no medicinal remedy was prescribed in addition to a single dose of oil; and in *one* case a simple enema constituted the sole treatment.

Anodynes. Anodynes were employed in a few cases to restrain diarrhoea, and the undue operation of laxative remedies. The tincture of opium was used for this end, taken either by the mouth, or by enema. It was prescribed in *five* cases of *Typhus*, and in *eight* cases of *Typhoid*. The proportion of the instances in the latter type, it will be perceived, is very considerably largest. In all these cases it was employed to a very limited extent, and frequently only once during the course of the disease.

In treating cases of fever, heretofore, I had been accustomed, very generally, to prescribe opium, or morphia, with a view to relieve mental aberration, and promote sleep. I have supposed that an anodyne influence was salutary even when vigilance or delirium were not prominent symptoms. In this collection of cases, however, I resorted to remedies of this class, except to restrain too frequent dejections, in but a very few instances. Morphia was prescribed in only *two* cases of *Typhus*, and in one of these it was given to allay cough. In *one* case of *Typhoid*, Dover's powder entered into the treatment.

My sense of the necessity of Anodynes, save for the object first stated, was lessened by the observation of cases in which they were omitted. Had it been otherwise, they would have been given more frequently.

Astringents. I gave the *tannic acid* in *one* case of *Typhoid* in which

diarrhoea called for restraining measures. This was the only instance in which any remedies of the astringent class were employed.

Sedatives. The only articles employed which will fall under this class are *tartarized antimony and potassa*, and *camphor*. The former was prescribed in *three* cases of *Typhus*, and in *one* case of *Typhoid*. In the *Typhus* cases it was given to relieve delirium, in doses of gr. 1-16. In the *Typhoid* case it was given with reference to pneumonitis which existed as a complication. In one of the *Typhus* cases the antimony constituted the sole medicinal treatment.

Camphor was given in *three* cases of *Typhus* for ataxic symptoms, and in no case of *Typhoid*.

Ammonia. The carbonate of ammonia was given whenever there was a marked failure in the forces carrying the circulation. It was prescribed in all the cases ending fatally by asthenia. It entered into the treatment of *six* cases of *Typhus*, and *four* cases of *Typhoid*.

Quinia. Quinia was prescribed in doses of *three* grains, three times daily, and continued for several days in *four* cases of *Typhus*. In each of these cases it was given from the commencement of the febrile career. It was given in *one* case of *Typhoid*.

Huzam's tincture was prescribed in *one* case.

Spiritus ether nitrosus. This remedy was given in *two* instances, once for some difficulty in urination, and in the other case the indication does not appear in the history.

Vesication. Blisters to the nape of the neck were resorted to whenever spasmodic inspiration was observed, or a tendency to coma. It entered into the treatment of all cases which ended fatally by apnoea. The employment of blisters was limited to the above indication, save that in one case they were applied behind the ears for external otitis. They were applied to the neck in *nine* cases of *Typhus*, and in *four* cases of *Typhoid*.

Sinapisms and stimulating liniments. These were occasionally employed in cases complicated with pneumonitis, and when coma existed, or was threatened.

Diffusible stimulus. The only form of diffusible stimulus employed, save in one or two instances, was *brandy*. This remedy entered into the management of the great majority of the cases of both types. It was omitted in but *five* cases of *Typhus*, and *two* of *Typhoid*. In these cases the disease was mild. Two of them were those which received no medicinal treatment. In one the treatment consisted only of a single enema, and in another of antimony in doses of a sixteenth of a grain.

In several of the *Typhus* cases (in all *nine*) brandy constituted the sole remedial agent employed.

The mode of administration, in all save two cases in which milk punch and egg nog were given, was with cold or warm water, from two to three parts of the latter, and, occasionally, the addition of sugar. The quantity given at a dose was generally half an ounce, or a tablespoonful. In some instances this quantity was increased to an ounce. The dose was repeated at intervals varying from half an hour to four hours. The shortness of the intervals corresponded with the apparent urgency of the indications, and the apparent effect. The frequency with which the doses were repeated varied very considerably in individual cases, and at different periods of the febrile career in the same case. I commenced its use, in a large proportion of the cases, early in the disease, frequently at the time the patients first came under observation, and it was usually continued pretty steadily throughout the disease.

Prostration, coolness of the surface, feebleness, and, more especially, notable frequency of the pulse were regarded as indicating its free administration. But it was given in many cases in which these symptoms were not particularly prominent, the object being to forestall their occurrence by sustaining the vital forces.

I have thus given a brief statement of the medicinal treatment. The dietetical and sanitary management was deemed not less important than that pertaining to the exhibition of remedies. In every instance nutriment was systematically administered; the forms being the same as heretofore, viz., essence of beef and milk porridge. These articles of nourishment were given at short intervals during the course of the disease, without waiting for an expression of, or consulting the wishes of the patients, and sometimes against their inclinations. Attention to cleanliness, ablutions, and ventilation, were attended to so far as circumstances permitted. Owing to the hospital being more crowded than hitherto, the sanitary conditions were perhaps somewhat less favorable than during the previous years. The larger number of patients also rendered it more difficult to bestow always the same vigilant attention.

I have not mentioned any efforts to cut short the disease. The only measures resorted to for this end was the *wet sheet*, which was applied in *two* cases of the *Typhoid* type. In one of these cases it was applied on the *third* day after admission, and the *fifth* of the febrile career. The fever continued, convalescence being pronounced on the *nineteenth* day.

In the other case it was resorted to on the second day after admission, and the fifth of the febrile career. The progress of the disease was unaffected,

the patient being convalescent on the *sixteenth* day. In this case a relapse of fever occurred a few days after the date of convalescence.

It is at once obvious that in presenting the foregoing summary of the treatment pursued in this collection of cases, the object is not to furnish any proof of the effects, either immediate or remote, apparently due to each of the different measures in the cases individually. In order to form any estimate of these results it would be necessary to consider the details of the treatment in each case, together with the symptoms from day to day during the course of the disease. In other words, the histories of all the cases should be given at length. The enumerations that have been given only afford a *coup d'oeil* of the treatment which the cases received collectively, the remedies employed being so few, that, without occupying much space, it has been practicable to present a statement of facts which it is presumed will be rather more satisfactory than mere general statements.

The reader will perceive that the medicinal remedy upon which most dependence was placed was brandy. In this, with the administration of nutriment, consisted most of whatever efficiency belongs to the management. The other remedies, for the most part, were secondary in importance; and it is more than probable that the treatment might have been more uniformly restricted to the use of diffusible stimulus without affecting materially the results. That is to say, some of the remedies which were prescribed in a small proportion of cases, did not possess enough potency for much importance to be attached to them.

What conclusions are to be drawn from the management of these cases? I am half disposed to leave this question wholly with the reader. I shall devote to it but a very few remarks. We are to regard this collection of cases as exemplifying, therapeutically, the treatment of *Typhus* and *Typhoid* fever mainly by stimulants and nourishment. The rate of mortality certainly was not large. It is probably below the average of cases of the same description under analogous circumstances, irrespective of remedies, or under different plans of management. I am not disposed, however, to deduce from this fact any inferences as to the positive merits of the mode of treatment. It is well known that these forms of fever exhibit at different times and places widely different tendencies to a fatal result. Statistics may be referred to, showing a small rate of mortality, in which stimulants and a nutritious diet did not form a prominent feature of management. All that can be claimed in behalf of the treatment on the ground of the small fatality is, that it did not exert an unfavorable effect. In making this admission I would not be understood to mean that I attach only a negative value to the measures

pursued. I cannot but entertain the conviction that these measures did exert more or less real efficacy. If I had thought otherwise, I should assuredly have withheld them to a greater or less extent. Attaching to them considerable importance, I could not conscientiously deprive the patients of the advantage which I supposed was to be therefrom derived. This is, however, an *opinion*, which is quite another thing than a truth demonstratively or logically established.

The probable influence on the rate of mortality in a collection of cases is, however, not the only, nor the most reliable source of information respecting the efficacy of remedies. We estimate their influence, in the formation of our opinions by experience, by studying their immediate effects in individual cases. This kind of observation has led me to attach considerable importance to the employment of diffusible stimulants, in conjunction with alimentation, in cases of fever. I cannot, as it seems to me, be deceived in the fact that the symptoms are frequently improved thereby, and that the condition of patients frequently deteriorates when these measures are withheld. I have in several instances in this, as in the collection of cases before recorded, observed the pulse to fall in frequency, the mind become more clear, etc., after stimulants, with nutriment, have been administered, or the quantity been increased, when it was hardly conceivable that this connection of events was one of coincidence merely. I have sometimes withheld these measures, or diminished their use, for a short time, in order to watch the result, and found their usefulness apparently verified in that way. In one of the *Typhus* cases in this collection, presenting the greatest degree of gravity, the pulse being nearly or quite 140 per minute for several successive days, I was led to try this experiment under the suspicion that the great acceleration of the pulse might possibly be due, in a measure, to over stimulation. In that instance, after the discontinuance of an ounce of brandy every half hour, for a few hours, the pulse rose from 140 to 150 and again fell to 140 on resuming the stimulus in the doses previously given. The patient finally recovered. It was frequently remarked that the pulse was more frequent, and other symptoms more unfavorable in the morning, when, owing to the greater difficulty of administering stimulants and nutriment with the same regularity during the night as during the day time, these measures had been less efficiently carried out; and I cannot but think that the happy issue of some cases was due to the vigilance with which the patients were watched at night as well as day, the quantity of stimulus being graduated carefully by the immediate effect on the pulse and other symptoms.

With respect to the uniformity with which these sustaining measures

should be pursued in cases of fever, and, in this point of view, the degree of importance which belongs to them, my impressions are not so definite. I am by no means prepared to assert that they could not have been dispensed with in some cases, or employed less freely, without compromising the safety and welfare of the patients. Continued observations relative to these, and other points in therapeutics, are desirable. A combination of *negative* deductions from the analyses of different collections of cases, may, in the end, settle the principles which should govern the treatment of Continued Fever more *positively* than comports with our present knowledge of the subject.

SECTION FOURTEENTH.

Cases of Doubtful Type.

In the foregoing Sections I have made no reference to the facts contained in the histories of the cases in which the type of the disease was not determined. I have thought it preferable in concluding this Report to devote to the cases of *doubtful type* a distinct Section. They will require but a brief consideration. I shall examine them only with reference to the subject of *diagnosis*. It is in this point of view chiefly that they possess interest. The question naturally arises—‘Is it true that the type of a certain portion of the cases in each collection is indeterminate?’ If it be difficult or impossible to assign to 8-64 or $\frac{1}{3}$ of the cases falling under our observation their proper situation in either the *Typhus* or *Typhoid* groups, does not, it may be asked, this fact militate against the doctrine that Continued Fever is resolvable into these two forms? I propose to analyze the cases of *doubtful type*, in this collection, with reference to these inquiries. Let us see what are the facts pertaining to the distinctive diagnosis in the cases severally. Confining the examination to the points which distinguish the two types from each other, it will not occupy much space to present a summary of the history of each case. This I will proceed to do, appending to each case a few remarks, and afterward offering some general conclusions.

CASE I. Thomas Daily, aged 11 years, Irish, recently arrived in this country. Entered hospital in the month of November. The attack was abrupt, having no period of access.

There were no manifestations of delirium.

The bowels were at first constipated, no dejection occurring for three successive days; afterward the bowels were loose, but diarrhœa was not a prominent symptom.

There was moderate meteorism, and considerable abdominal tenderness.

No eruption.

No epistaxis.

The maximum of the frequency of the pulse was 144; the mean frequency a fraction over 115.

Capillary congestion was moderate. The duration of the febrile career was about twelve days.

Remarks. From the foregoing assemblage of symptoms the type of the disease is presumed to have been *Typhoid*. This opinion is mainly predicated on the abdominal symptoms, which, although not present from the first, nor prominent at any time, were yet sufficiently marked. The abruptness of attack is unusual, but with respect to this, as well as other points, it is not improbable that a collection of cases of the disease occurring in young subjects would show some variation from the laws obtaining in cases of adults.

CASE II. — Mary Daily, sister of No. 1, aged about eight years. Admitted in November. Attack abrupt, took at once to the bed. No manifestations of delirium. Deafness was marked.

Mild diarrhœa from the first, or, rather, looseness of the bowels.

Meteorism and tenderness is not mentioned.

Maximum of frequency of pulse, 152; mean frequency, 107.

Moderate capillary congestion during the early part of the career.

Epistaxis occurred on the third day.

Duration, fourteen days.

Remarks. There can scarcely be a question respecting the type in this case. It was doubtless *Typhoid*. The evidence is stronger than in case No. 1, and this being so, the fact of the relationship of the patients adds to the evidence in the first case. The history is defective in not containing statements relative to tympanites and tenderness. This was the reason, chiefly, of the case being excluded from the *Typhoid* group.

CASE III. — Henry Nigelab, German, aged 40. Admitted in December. Been in America two years, and in Buffalo five weeks.

Attack was abrupt.

Delirium occurred on the third day, consisting in incoherent talking, and efforts to get out of bed. Eyes suffused. Bowels constipated, two dejections only occurring in the space of eight days. No abdominal tenderness, nor tympanites.

An indistinct eruption noted on one day.

Maximum of pulse, 128; mean frequency, 107.

Moderate capillary congestion. Epistaxis (moderate) on the fourth day.

Duration, eight days.

Remarks. The type of the disease was unquestionably *Typhus*. The absence of abdominal tenderness and meteorism, the constipation, the sudden attack, the age of the patient, the early delirium, the mean frequency of the pulse, constitute conclusive proof of the correctness of this opinion; and had the details of the histories been studied a fully at the preliminary examination, when the distribution of the cases were made, as now, the case would have been included in the *Typhus* group.

CASE IV.—Aaron Young, English, aged 23. Admitted January. Been in this country but five days, and stated that there was fever on board the vessel in which he came over. He entered January 11th, with cough, debility, and febrile movement. He kept the bed several days, when he began to sit up, and was supposed to be convalescing from an attack of ephemeral fever. The chest was not explored at that time, but subsequently the existence of tuberculosis was determined. He was probably laboring under incipient phthisis when he entered.

On the 16th of February, (thirty-six days after date of admission) he was attacked with symptoms of fever.

He took to the bed after an access of *two* days duration.

Delirium was manifested on the third day, consisting of incoherency and efforts to get out of bed.

The bowels were rather loose, i. e. on some days two dejections occurred during the twenty-four hours.

There was moderate tympanites, but no abdominal tenderness.

On the second day after taking to the bed a copious eruption appeared, consisting of *maculae*, dusky, not disappearing on pressure, intermingled with which were *rose spots*.

Capillary congestion moderate.

Maximum of pulse, 108; mean frequency, 95.

Slight epistaxis on the fourth day.

Duration, thirteen days.

Remarks. The looseness of bowels, together with the mixed eruption led to the rejection of this case, but that it was a case of *Typhus* hardly admits of doubt. This is shown by the abrupt attack, the short duration, the absence of abdominal tenderness, the early manifestations of delirium, the early appearance of the eruption, its copiousness extending to extremities as well as over the body, and the probability of the disease having been contracted by contagion in the hospital ward.

CASE V.—John Jany, Irish, aged 22. Admitted in February.

Been in this country ten days.

Duration of access not determined.

No manifestations of delirium.

One dejection daily during the career of the disease, save that on one day there were two.

No tympanites, nor abdominal tenderness.

On the fourth day there was no eruption. On the fifth, sixth, and seventh days the daily record of symptoms was not made. On the ninth day there were three *rose spots* which remained for several days.

The maximum of pulse was 90; mean frequency, a fraction over 75.

Moderate capillary congestion.

Duration of febrile career fourteen days.

Remarks. The interruption in the daily records, and consequent imperfection of the history, was the chief reason for excluding this case. It was a case of *Typhoid* without doubt, of a very mild grade.

CASE VI. — John Welch, aged 25, admitted in March.

Been in this country five years.

Entered on the *sixth* day after taking to the bed.

Access of eight days duration.

Slight manifestations of delirium, consisting only of incoherency at night.

Mild diarrhoea during the whole of the febrile career.

Moderate meteorism and slight abdominal tenderness.

Presented at the time of his admission an eruption which is described as dusky, not elevated, and the redness not disappearing on pressure. The same characters are recorded the following day. On the next day it is simply noted that the eruption remains. On the next day several *rose spots* were observed, disappearing on pressure.

Maximum of pulse, 120; mean frequency, a fraction over 104.

Moderate capillary congestion.

Epistaxis occurred early in the disease.

Duration, eighteen days.

Remarks. The variation in the characters of the eruption in this case renders the diagnosis doubtful. The patient, it will be perceived, did not enter until the *sixth* day, so that the time of the development of the eruption, and the character which it at first exhibited are not ascertained. Aside from the eruption, the symptoms clearly point to *Typhoid*. The long duration of the access and the duration of the febrile career; the abdominal symptoms; absence of delirium, and the occurrence of epistaxis, would render the discrimination sufficiently easy, were it not for the mixed characters of the eruption.

This, it will be observed, is the first of the cases of *Doubtful type* in which the facts pertaining to the history really occasion much hesitation as to the diagnosis.

CASE VII. — Charles Madden, Irish, aged 27, admitted in March. He entered hospital three weeks before being attacked with fever, with some trifling ailment which was not closely investigated. He was able to be up and about until attacked with fever.

It is not stated whether he had recently arrived in this country.

Access of four days duration.

Slight manifestations of delirium on the second night after taking to bed.

Diarrhœa, the dejections being frequent, occurred on the second and third days, but did not afterward continue. Previously there was constipation. No meteorism, nor abdominal tenderness.

An eruption appeared on the second day consisting of three or four *rose spots*. The next day there was a copious eruption, dusky, not disappearing on pressure, and intermingled a few *rose spots*.

Maximum of pulse, 114; mean frequency, 96.

Moderate capillary congestion.

Duration of disease, thirteen days.

Remarks. The *rose spots*, and the diarrhœa, give rise to some doubt as to the diagnosis in this case. Aside from these, the evidence of *Typhus* predominates. The absence of tympanites and tenderness over the abdomen; the early development of delirium; the appearance of the eruption early in the disease and its copiousness; the short duration of the disease and the probability of its having been contracted in the hospital, are in favor of the *Typhus* type.

CASE VIII. — George Gaylord, aged 25, English, admitted in January.

Been in America ten months, and in Buffalo eight.

A brother of this patient entered December 2th, with *Typhus*, and his case is included in the *Typhus* group.

Admitted on the *fifth* day after taking to the bed.

Duration of access, *three* days.

Slight incoherency from the time of admission.

Bowels constipated until the eighteenth day, when they were slightly loose.

Slight tympanites noted on one day only.

No abdominal tenderness.

Eruption abundant, presenting mixed characters. It is thus described: "Eruption copious over the body and extremities, mostly consisting of *maculæ*, not disappearing on firm pressure, rather dusky. Intermingled with

these a sparse eruption of *rose spots*, larger in size, elevated, and the redness readily disappearing on pressure."

The eruption was present when admitted, and had been already observed for two days, making its first appearance as early as the third day.

Maximum of pulse, 124; mean frequency, a fraction over 111.

Moderate capillary congestion.

Slight epistaxis on the fourth day.

Duration of the disease twenty-three days.

Convalescence was retarded by pneumonitis.

Remarks. There is but little room for doubt in this case as to the *type*. What doubt exists is occasioned by the mixed characters of the eruption. Admitting that the eruption does present occasionally characters more or less mixed, without necessarily compromising the claims of such cases to be considered either *Typhus* or *Typhoid*, I should have been warranted, as it appears to me, in including this case in the former group. The early development of the eruption, its copiousness and extent; the short duration of of the access; the mean frequency of the pulse, and the fact that a brother, shortly before, with whom he had been associated, was admitted with clearly marked *Typhus*, render the diagnosis almost, if not quite positive.

It is sufficiently apparent from the circumstances relating to the histories of these cases, that the type of the disease, generally, was not very doubtful. In fact there were but two cases (Nos. 6 and 7,) in which much uncertainty could be considered to rest upon the diagnosis. That these few cases were included in the class of *Doubtful type*, does not, therefore, show that difficulty in practically discriminating between the two types, which the reader, without these details, might naturally have been led to infer. Their exclusion from the *Typhus* and *Typhoid* groups, was owing to the care not to include in either group a single case in which the type was in the least degree doubtful, it being deemed far better to be hypercautious in this respect, than to run any risk of error. So far as my observations go, the principles of diagnosis briefly presented in the Supplement to the Second Report, admit of a ready and positive application in all but a very small proportion of instances. The fact must be considered to have considerable significance in connection with the question as to the identity of *Typhus*, and *Typhoid* fever. At the same time, the fact is not to be overlooked that we do find, in a certain proportion of cases, a combination, to a greater or less extent, of the symptoms distinctive of either type. The latter fact applies to the eruption, as well as other events belonging to the natural history of the two forms of fever. In view

of this fact, it may seem, in the minds of some, to be a rational conclusion that the two forms of Continued Fever are not so distinct but that they may be blended, developing a kind of hybrid disease, which cannot properly be considered to be either *Typhus* or *Typhoid*. This certainly opens a very interesting point of inquiry. It cannot, however, be prosecuted, with any expectation of coming to a satisfactory conclusion, by means of speculative reasoning, and my observations relative to the subject do not afford data adequate to any logical deductions.

Note. It is proper to state that the histories of the cases upon which the foregoing Report is based were, for the most part, not recorded by my own hand, but by Mr. J. LEWIS SMITH, resident medical student at the hospital, for whose faithful services I desire to express my obligations. The records were made under my daily direction and supervision.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

California—Her Hospitals—Prevalent Diseases—Mortality, etc.

Through the kindness of a friend, we have recently received the Report of the Trustees and Physicians of the *Sacramento State Hospital*, from which we glean the subjoined interesting facts and statistics:

The hospital was opened for the reception of patients on the 28th of May, 1851, and consequently had been in operation up to the 10th January, 1852, when this report was made out, seven months and thirteen days. During these seven months and thirteen days

The whole number admitted was	- - - - -	592
“ “ discharged	- - - - -	415
“ “ died	- - - - -	72
Remaining in Hospital	- - - - -	104
For the same time there were admitted in Insane Department	- - - - -	38
Discharged cured	- - - - -	16
Remaining in hospital	- - - - -	22
Received from San Francisco	- - - - -	20

We append some of the principal diseases for which the 592 patients were admitted, in order to indicate the influence of climate upon emigrants:

Admitted of bilious remittent fever, 123; of rheumatism, 49; of intermittent fever, 46; of typhoid fever, 33; do. mental derangement, 38; do. diarrhoea, 38; do. wounds of various kinds, 30; do. Panama fever, 23; do. erysipelas, 11; and scorbutis, 11.

The foregoing seems to be the prevalent diseases for which patients were admitted into the Sacramento State Hospital. The other diseases which go to complete the list, are such as are met with in our hospitals in this portion of the United States, and therefore deserve no special notice.

Of the 592 admissions 72 died, and of the following diseases:

Dysentery, 8; abscess, 1; consumption, 5; diarrhoea, 14; bronchitis, 4; enteritis, 2; cerebritis, 4; scorbutis, 1; hemiplegia, 1; erysipelas, 2; anasarca, 1; coxalgia, 2; fever congestive, 2; fever bilious remittent, 5; scarlatina, 1; bowels, ulceration of, 1; fever typhoid, 13; fever Panama, 4; delirium tremens, 2—making a total of 72 deaths out of 592 admissions.

Of the admissions, 342 were natives of the United States; and 250 adopted citizens, representing 20 different foreign countries.

The report concludes in the following words—as drawn up by Doctors Bryarly and Williams, the former the visiting, and the latter the resident physician, of the hospital:

“By referring to our report of the different diseases, it is easy to observe what may be considered as the prevailing diseases in this, the northern district of California.

“The largest number of any one class have been those of fever, particularly those of bilious remittent fever. This is not surprising, when it is considered that every feature of the country, and the general habits of the people are most conducive to this disease. During the dry season, the miners are compelled to resort to the rivers and water courses for work; here they are exposed to all the miasma originating from the decomposition of the vegetable matter from the overflowed lands during the wet season. This produces intermittent, or common chills and fever.

“The general living of these people is decidedly bad; not only in reference to their food, but more particularly in reference to their sleeping apartments. They either sleep in the open air, exposed to the sudden changes peculiar to our climate, or they are huddled into tents and cabins, where they cannot but suffer from the effects of contaminated atmosphere. These things, connected with the fact that many of them work in the water six or eight hours each day, bring about such a state of the system, and such a habit of body, as renders it peculiarly susceptible to take on the most malignant forms of every disease with which they are attacked. It is thus that our worst fevers are produced, and these are the reasons of their frequency in our district.

“Although bilious remittent fever has far doubled the number of any other fever, it is to be observed that the mortality has been much less in comparison.

“From our mortality report, the greatest number of deaths have been from chronic diarrhoea. This is the most formidable disease in our whole country. We find it mostly attacking those recent in the country, and almost always following the extreme debility of the acclimating fever, Panama, typhus or ship fever, to which it seems to be the most regular sequel. The neglect of this in its acute stage, is followed by its passing into the chronic. The fact that there are so many existing causes, such as strong mental emotion, of a depressing or anxious kind, exposure to dampness and cold, indigestible food,

intoxicating drinks, bad water, and general debility, that oftener the organic disease, before coming under the treatment of a physician, is so great, as to be out of the reach of human aid.

"In some of the northern portions of this district, during the past summer, the erysipelas has raged as an epidemic, with great mortality; and in some few places, even now, continues its ravages.

"The counties of Shasta, Nevada, and El Dorado, have been the worst sufferers. In many instances every inhabitant of small mining camps has been attacked, often assuming the most malignant form and proving fatal in a very few hours.

"The most apparent cause seems to be, the peculiar constitution of the atmosphere, exposure to all weathers, bad and unwholesome living, general tendency to scorbutis, which, combined with a natural predisposition, and the contaminated air of crowded and ill-ventilated apartments, are all calculated to render the subject peculiarly susceptible to erysipelatous inflammation.

"It will be seen, that although situated in the interior, we have not been exempt from the reception of 'Panama fever' in our wards.

"From the last steamer that arrived, (the Northerner) fourteen of her passengers have been admitted here.

"The appellation of 'Panama fever,' is very common at the present day; but the cases from the Northerner can be much more easily recognized under the head of 'ship fever.'"—*New Orleans Med. and Surg. Journal.*

Diagnosis between Infantile Remittent Fever and Hydrocephalus. By Dr. C. TAYLOR.*

It is often an anxious point in infantile pathology to determine whether the cerebral symptoms which may be present in a given case, are indicative simply of sympathetic disturbance of the sensorium, or of the series of anatomical changes which characterize the disease known as hydrocephalus. In deciding the question, some assistance will be derived from the following summary of the distinctive symptoms in each:

<i>Remittent Fever.</i>	<i>Hydrocephalus.</i>
Head, slight pain in.	Head, violent pain in; tossing of; drawn backward, and bored in pillow.
Delirium at night frequent; convulsions rare—sometimes at onset.	Delirium seldom; convulsions not early—more toward end of disease; aversion to light and noise.
Easily aroused.	Roused with difficulty; stertorous breathing; paralysis in late stage.
Cry fretful, if any.	Cry peculiar, sharp and shrill; frequent sighing.
Hands usually thrown about bed (Coley.)	Hands tossed toward head.
Countenance heavy and dull; vacant expression, as of fever in adult.	Countenance sometimes anxious, sometimes dull.

* *Infantile Remittent fever* is to be considered as embracing *continued fever* affecting children.—ED. BUFFALO MED. JOUR.

Neither knitting of brows nor pupil of eye affected.

Senses of sight and hearing often dull.

Pulse quick throughout the disease.

Bowels occasionally constipated at first; frequently relaxed.

Motions various; often clayey and deficient in bile; very offensive.

Vomiting occasionally at first, but never continuous.

Pain often in the iliac regions, particularly the right.

Abdomen in advanced stage sometimes tumid.

Appetite mostly destroyed; will not take any thing.

Thirst often great from commencement.

Tongue often loaded with yellowish white fur, in gastric form, and elongated and projected papillæ, giving it a "strawberry appearance;" red, dry, and occasionally brown, in malarial form.

Skin very hot; abdomen hotter than the head; picking of the nostrils, corners of the mouth, etc.

Paroxysms regular; exacerbations toward night, remissions in the morning.

Seldom occurs under three years.

Ranking's Abstract.

Knitting of brows; wakefulness; pupil of eye contracted in early stage — sometimes oscillatory, afterward dilated.

Senses of sight and hearing often acute in early stage.

Pulse quick, but irregular in its action and force in early stage; often beating of carotids, and pulsation and prominence of fontanelle; pulse afterward becomes slow, but, on raising the child, again quickened.

Bowels constipated, and very difficult to move.

Motions peculiar and characteristic — dark green and slimy, like chopped spinach.

Vomiting early in first stage; often very constant, especially on assuming the erect posture of sitting up.

Pain occasionally at hypochondrium.

Abdomen drawn in in advanced stage.

Appetite sometimes good; will take food.

Thirst not great in first stage; often in latter stage great avidity for constant drink.

Tongue white; nothing indicative.

Skin not so hot, afterward cold; head the hottest part.

Varies in intensity without regularity.

Frequent under the third year; less so after the fifth. More frequent in boys of a scrofulous habit.

On the Varieties of Alvine Discharges in Children. By Dr. MEREL.

The intestinal discharges mentioned by the author are :

1. The *yellow* discharge. This is the regular kind of stool in infants. It is a mixture of intestinal secretions with bile. As children advance in age, and begin to take substantial food, the color of their regular discharge becomes more and more of a light brown color.

2. The *mucus* discharge. White mucus matter, more or less thick or liquid, and mixed with serum, sometimes with a proportion of bile. This discharge is preceded by but moderate pains, and frequently by no pains at all. It denotes a catarrhus, sub-inflammatory, or irritable state of the intestines, and is almost always of local, and not of sympathetic, origin; in general, it is not dangerous, and at its commencement is easily manageable by opiates, warm poultices, and convenient hygiene. If neglected, it becomes pertinacious and severe, and not seldom connected with swelling, softening, or granules of the mucous membrane, or ulceration of the follicles. If stripes of blood are mixed with the mucus, and pain be present, it denotes a higher degree of inflammation, in particular of the follicles. The highest development in this direction constitutes enteritis or colitis (dysentery.)

Sometimes we find among the mucus, consistent *plastic concretions* of a more or less tubular shape, similar to those of laryngeal croup, but larger in proportion to the volume of the intestines. This is the strongest degree of the catarrhus process, which I might term the *croup of the intestines*. Among the whole number of my little patients, which may be about 30,000, I met with this discharge perhaps only twenty or thirty times. The discharge is effected with very painful efforts at a stool.

3. The *serous*. In general, after more or less severe pains, the discharge takes place with a certain rigidity and noise, after which the pains lessen or subside. It consists of an abundant quantity of serous liquid, dirty whitish, yellowish, or greenish, as besides mucus, bile is the most common mixture with the serum. The serous diarrhoea is commonly the effect of rheumatism in the peritoneum, in the serous or fibrous membranes, or in the nerves of the intestines. I found in these cases the abdomen very hot. If a great deal of mucus and some blood are mixed with the serum, we may suspect parenchymatous enteritis; if the serous membrane alone enters into the state of acute inflammation, frequently transudation takes place on its free surface.

I have seen cases of profuse serous discharge, in a very short time, even in less than twenty-four hours, produce collapse and death, and in some of these instances necropsy could not discover an adequate alteration either in the mucous or in the serous membrane.

The serous species of discharge is frequently merely a product of sympathetic secretion. I observed it sometimes connected with large transudations in the chest, and with chronic hydrocephalus.

Speaking in general, serous diarrhoea, if even arising from rheumatism, is more difficult to manage than the mucus. Very minute doses of calomel, with Dover's powder and mustard poultices, are frequently beneficial.

Pure serum, like rice-water, is a less favorable quality than the dirty white or yellowish. Dark brown serum frequently denotes a disorder in the portal system, present in some severe gastric or typhoid fevers, but I have seen a similar quality also in chronic affections of the brain, and very frequently in

scrofulo-impetiginous children. This is worthy our attention, in particular if eczema or impetigo has disappeared from the head and face. This brown and foetid discharge accompanies sometimes the commencement of chronic hydrocephalus. I treated it successfully, in this last case, with high but very diluted doses of iodide of potash.

4. *The green bilious discharge.* If pure bile, then the voided matter is, in general, not abundant. In young children, it is of a more yellowish than green color. The essential character of bile is, to be of a *greenish color* (in infants it is voided green) *at the very moment of its evacuation.* This kind of discharge is very frequently present in acute inflammatory and febrile affections; if dependent upon an affection of the brain, then we may find the color to be rather brown, and the abdomen retracted. If a similar source produces abundant serous-bilious discharges, then we find the abdomen much collapsed. But I must observe, acute affections of the brain are almost always connected with constipation; only in some cases of chronic hydrocephalus I met with the mentioned diarrhoea. Bilious discharge as arising from bilious fever, or from derangement of the liver, is rare in young children. In this case, the right hypochondrium will be more or less bloated up. We must be careful not to confound the green bilious discharge with the following.

5. *The discharge, like chopped eggs,* mixed with mucus, some clots of bile, and caseous coagula of indigested milk, or other kind of food, accompanied almost always by gripes and flatulence; its smell is disagreeably acid, and the whole matter, some minutes after being discharged and *exposed to the atmosphere, becomes green.* We know not exactly the chemical change which produces this coloration; it seems to be an oxydation of some of the elements. Then the essential character of this discharge is, that it is yellow at first, and becomes green by exposure to the atmosphere, while bile is green at the moment it comes out. I shall call this *the acid suburral discharge,* which is the most obvious before the sixth month of age, in particular if the sucking child takes, besides the milk, some farinaceous food. Practitioners commonly prescribe in this case rhubarb, with magnesia. For my part, I prefer, in tender infants, to rely more upon a convenient change in the diet, and as a remedy, aromatic frictions of the epigastrium, and internally bicarbonate of soda, dissolved in mint water.

6. *The bloody discharge.* Pure red blood is seldom discharged by children; in some rare cases I have seen half or one table-spoonfull come out, as the product of active congestion and hemorrhage. Very frequently, on the contrary, blood is combined with the mucus discharge, and in this case, if it is preceded by pain, without tenderness, it denotes an inflammation in the upper parts of the intestinal tube, at least not near the rectum. Tenesmus signifies that the seat of the inflammation is in the lower parts of the colon, or in the rectum. This form is commonly called *dysentery,* not dangerous, if it is without bilious complication and fever, and if treated in its early stage with Dover's power, some doses of castor oil, and warm poultices; in a stronger degree, leeches at the anus; but if neglected in the commencement, it becomes dangerous to the life of the child. Professor Rokitansky, of Vienna, describes most exactly what he calls the "dysenteric process," in three gradual degrees of anatomical change. The highest degree, presenting a dirty red and gray marbled surface, with considerable thickening, granulation, and ulceration, I never saw in the tender age. Young children die before this stage is developed.

Passive hemorrhage of the intestines very seldom occurs in children. I have seen, however, some cases where, without adequate pain, a considerable quantity of dark thin blood was discharged. Lastly, we have seen in this town, with Mr. Wilson, a case in a child six years old, where, during the course of a gastro-typhoid fever, more than one pint of carbonized blood was discharged in two days. The case recovered. The boy is affected with an enlarged spleen.

Moderate quantities of red blood, discharged without pain, frequently occur, mixed with mucus, and are, without signification, sometimes even connected with the advance of recovery from gastric affections. This is the same case as with epistaxis.

Golding Bird and Simon state, as the result of chemical analysis, that some dark green stools of children owe this color to blood which has suffered a certain chemical change; but those chemical inquiries are not yet arrived at a satisfactory exactness; we do not even know exactly what kind of green discharges were the subject of these inquiries.

7. *Calomel stools.* Green, more or less thick, or mixed with serum, and in this case more abundant, produced by full doses of calomel. Calomel stools resemble bile, and contain much bile, but they contain also some particular chemical elements, which we do not exactly know. In many instances it happens that the calomel diarrhoea commences some days or weeks after the use of mercury, and we must be aware of this, and not confound it with the primary bilious discharge. In the former case, the region of the liver is, in general, softer than in the latter. A clever practitioner will never try to stop directly, and with astringents, a green discharge, whatever be its origin and nature.

Calomel stools sometimes contain blood. After what I have seen in dissection, I incline to attribute this circumstance to a sub-inflammatory state, with superficial erosions of mucus membrane, which sometimes takes place in children after the continued use of calomel.

[The author states that he considers all these qualitative and physical distinctions of the discharges of children as very imperfect outlines of a sketch, which, by farther physical and chemical inquiry, can become corrected and perfected.]—*Rankin's Abstract.*

Note on Sulphate or Bebeerine. By HENRY S. PATTERSON, M. D., Professor of Materia Medica in Pennsylvania Medical College.

At a time when the discovery of a substitute for Sulphate of Quinia is a topic of general discussion, it may not be inappropriate to call the attention of the profession to a substance heretofore noticed, but too generally neglected. The Sulphate of Bebeerine has been shown, by Dr. MacLagan, of Edinburgh, to be a medicine of very considerable anti-periodic power, closely resembling the corresponding salt of Quinia, and in many respects equal to it, possibly superior. It is obtained from Bebeeru or Green-heart (*Nectandra Rodia*) of British Guiana, a tree of considerable size and extremely abundant. The bark yields the alkaloid largely, but it is particularly abundant in the nut. A decoction of the latter is the ordinary popular remedy for intermittent fever in Demerara; and, as I am informed by an intelligent gentleman

of that place, seldom, if ever, fails to arrest the disease. The nut may be collected in almost indefinite quantities, and could be obtained here, if a demand were created, for little more than the expense of collection and transportation. The process for separating the alkaloid is almost identical with that for quinia, and not more expensive. If, therefore, it proves on trial equal in efficacy to that alkaloid, we will have a cheap and effective substitute within the reach of all. The subject certainly deserves a more extended investigation than it has hitherto received. The object of the present communication is to invite attention to it, and induce the profession, in miasmatic districts, to give the remedy a fair trial.

Sulphate of Beberine occurs in shining brown plates, (sometimes with a with a greenish tinge,) is inodorous, and has a bitter, harsh, somewhat astringent taste. Like the Sulphate of Quinia, it requires an excess of acid for its perfect solution. It may be given in pill, solution, or powder. That it is a good general tonic, in small doses, is very evident. In the full anti-periodic dose it is more apt to disturb the stomach than the same quantity of Sulphate of Quinia, and occasionally vomits; but it possesses the advantage of being much less stimulating, and does not affect the head as that salt does. Dr. MacLagan asserts that it is "not so liable to excite the circulation or affect the nervous system," and Dr. Meligan adds, that "this conclusion is fully borne out by his experience." The patients who have used it under my care expressly state that it did not occasion in them the same headache and vertigo as the quinia had previously done. Its dose is stated at gr. i.—v., three or four times a day. Neligan directs it made into pill with conserve of roses, or in solution, with the addition of a few drops of Acid. Sulph. Arom. The anti-periodic dose may be stated at gr. xv.—xx.

A letter from my friend and former pupil, Dr. H. J. Richards, of Grey Town, Nicaragua, of the date of March 25th, 1852, contains the following: "I have used the Beberine, as you suggested, with uniform success in quotidian intermittents. I have since had no opportunity to prescribe it in remittents. All the intermittents of this coast, however, are comparatively easily treated at this season, and yield readily to both quinine and arsenic. The remittents and even intermittents of the fall months, are more virulent and often speedily fatal." Those months will certainly furnish a fairer test of Beberine; but it is something to know that, under existing circumstances, it produces the same effect as the Quinia.

Dr. Watt, of Demarara, thinks that it is tardier in its effects than the Quinia, not interrupting the paroxysms so immediately, but he also thinks that its effects are more permanent. The cases in which I have had an opportunity of using it, seems to confirm the latter opinion.

1. A gentleman residing in Blockley township consulted me in September last concerning an obstinate and constantly recurring tertian intermittent, under which he had labored for a length of time. He stated that the Quinia always interrupted the disease, but that it inevitably recurred in two or four weeks. I gave him Sulph. Beber. ʒ ss. dissolved in ʒ viij. water, a tablespoonful to be taken every four hours during the apyrexia. The next paroxysm was prevented, and he has had no return of the disease up to the present time (April).

2d. A. J. applied to me in October last, with a very similar statement. While residing in New Jersey, about six years since, he had a violent and protracted "bilious fever," since which time he has had, every month or two,

an attack of "intermittent fever," which has been generally speedily arrested by quinine. Such was his account of the case. I found his tongue furred, his eyes icterode, his breath offensive, his urine scanty and high colored. The anoréxia was complete and thirst considerable. He had a daily slight chilliness, followed by considerable fever and a slight sweat. I gave him a mercurial purge, and on the next day fifteen grains of the Sulphate of Bebeerine. He complained of some nausea, but no disturbance of the head. The same quantity of Bebeerine was given on the two succeeding days, when, the paroxysms no longer recurring, it was discontinued. He remains free up to this period (April), and says that he enjoys better health than he has done for years.

If the permanent character of effect, which these cases seem to indicate, should be established by a more extended experience, we will have in the Bebeerine an agent of very great value, adapted to cases which have hitherto seemed uncontrollable, except by arsenic, to which there are so many objections. It is also more speedy in its effects than the arsenic. Bouchardat (*Ann. de Therap.*) expresses his surprise that the Bebeerine has been so entirely neglected in France, where trial is daily made in agues with substances of inferior efficacy. I trust that the same remark may not long be made with regard to the American profession, but that the precise value of the medicine may soon be established by an adequate extent of observation. —*Medical Examiner.*

On Bandaging the Abdomen after Delivery. By W. B. KESTEVEN, Surgeon.

[Mr. Kesteven, although sensible that the weight of opinion is against him, records his conviction that too much stress has been laid upon the importance of the bandage after delivery, and that the rationale of its usefulness has been misunderstood. In order to arrive at a correct conclusion on the subject, he examines it under the following points of view:—1st. The alleged object to be gained by the bandage. 2d. Its real effects. 3d. Its proper object, and the right period for its application. With this intent, he thus proceeds:]

1st. The objects alleged to be gained by the application of the roller directly after the completion of labor, are:—*a*, to promote the contraction of the uterus; *b*, to lessen the severity of the after-pains; *c*, to prevent hemorrhage; *d*, to prevent syncope; *e*, to protect the patient against the consequences of sudden alteration of the balance of the circulation, by which syncope, inactivity of the uterus, hemorrhage, and subsequent diseases, have been produced.

On examining, at the bedside, the validity of these several objects, it may be observed, in the first place, that all or any, of these supposed ends may be gained without the use of the bandage.

a. In the vast majority of cases the uterus contracts rapidly, firmly, and permanently, directly upon delivery, without the aid of bandaging. That such is the case a very short experience among the *laboring poor* will soon convince the clinical student. The poor women who are delivered by midwives, and the hundreds, ay thousands, who are yearly delivered without any aid, would, were it not so, have all the dangers of uncontracted uterus to contend with. That such is rarely the case admits of no doubt.

b. That measure which shall promote the contraction of the uterus can hardly be seriously recommended as a means of lessening the severity of the after-pains; the contradiction is too manifest to require further comment.

c. For the prevention of hemorrhage the application of a roller certainly possesses no claim. Every practitioner who has diligently applied the bandage has had to remove it, in order to apply that efficient pressure to the uterus which is most important in promoting its contractions, hemorrhage having taken place in spite of the compression that had been made by the bandage. In fact the tightly bandaging the hypogastric region with the addition of pads, compresses, basins, &c. &c., has probably frequently given rise to hemorrhage by interfering with the gradual tonic contraction of the uterus. The early application of a binder and compress is a complete obstacle to that vigilant attention to the state of the uterus after labor, which it is the wisdom as well as the duty of the medical attendant to pay for some little time after delivery. Where pressure is properly made, hemorrhage is not frequently met with. The very officious accoucheur, who loads his patient's abdomen with divers pads, and other similar contrivances, must frequently have had occasion to remove them. Without these, the earliest signs of hemorrhage may be recognized; with them, they are often concealed; without these hindrances, therefore, the occurrence may be arrested at its outset. It is not the purpose of the present communication to dwell upon the treatment of uterine hemorrhage, but the above hints may serve to show that the bandage has few claims for adoption on that score.

Reproduction of Lactation. The *Am. Jour. of Med. Sci.* for Jan. contains a report of some cases read before the Rhode Island Medical Society, by Ariel Ballou, M. D., in which lactation was reproduced after an absence of from three to four months. Before seeing Dr. B.'s cases, the writer had occasion recently to recommend a similar course to a patient, in whom, in consequence of severe illness following confinement, the secretion of milk was suspended for several weeks. The result was entirely satisfactory.

The following is one of Dr. Ballou's cases:—

“The following case I report as having an important practical bearing on the treatment and disposal of a class of cases which occur in our community at the present day, to cure which, or otherwise dispose of satisfactorily to the physician, is often found difficult.

“CASE III.—Mrs. O. H. H., aged about twenty-one years, of feeble constitution, and nervo-lymphatic temperament, was confined in July, 1847. Previous to her accouchement she was troubled with chronic aphtha, red canker, or with that condition of the system which is well known as “sore mouth attendant on pregnancy and lactation.” Nothing unusual occurred at the time of delivery. No considerable loss of blood was sustained. As in similar cases, there was a remission of diarrhoea and sore mouth for a few days after accouchement, giving rise to a hope that, being relieved from the condition of pregnancy, she would recover the powers of digestion and the assimilation of nutriment, so as to enable the system to sustain the calls upon it consequent to lactation. But in the course of ten or twelve days after accouchement the sore mouth and diarrhoea returned with increased violence, producing great debility. The secretion of milk was copious; her pulse 120;

the tongue flabby; there were frequent copious dejections of yellowish water, the face and extremities bloated, &c. Fearing the worst results from my patient, I advised the immediate removal of the child from the breasts of the mother to those of a wet-nurse, at the same time informing the parents that on the recovery of the mother, she could at pleasure reapply the child to the breast and have a full supply of milk, and be enabled to perform all the duties and functions of a mother for an indefinite period of time. The child was given in charge of a wet-nurse, the milk gradually disappeared, and the patient recovered under the use of tonic remedies and a generous diet. Between two and three months after this the mother called on me, having the appearance of restored health, and inquired if she might now take her child home with a hope of realizing my former assurances that she would be able to reproduce her milk. I assured her there was no doubt in relation to such a result, and her ability for the future to nurse her child. She took the child, applied it to the breasts, and in the course of two weeks had a good supply of milk.

"I met her some nine months after, when she informed me she was happy in the enjoyment of good health, and, to use her words, she 'had a good breast of milk as if she had never dried it up.'"—*New Jersey Med. Reporter.*

On the Treatment of Neuralgia—Extracted from an article in the *Western Lancet*, by LANDON RIVES, M. D., of Cincinnati.—"Most practitioners use opiates to produce an anodyne effect; and in this, I think, the fault usually lies in the treatment of this affection. When opiates are used with persons of good constitution, they may effect their anodyne influence, but if administered to persons of debilitated constitution and nervous temperament, laboring under neuralgia, the excitant effect will more than counterbalance all the good which can be expected from the subsequent sedative operation of the medicine. The functional derangement in this disease is an exalted sensation—hence it is wrong to administer a medicine which excites, even in its primary action,—for, although the secondary action may be the one desired, the primary excitation will irritate the diseased tissue, and render the subsequent paroxysms much more violent. A more appropriate, and in my hands a much more efficient remedy to meet this indication, is small and frequently repeated doses of extract of hyoscyamus. This medicine, unfortunately, is not always kept of a good quality in the shops; hence, care should be taken to procure a good article. With a view to prevent the recurrence of the paroxysms, there can be nothing used more efficacious than quinine. It has been my good fortune to cure a number of cases of neuralgia, with sulphate of quinine and extract of hyoscyamus, given in doses of one and a half grains each, at periods of from two to four hours during the intervals of the paroxysms. It is often necessary, and I may say, generally well to premise this course, by some gentle cathartic. I have sometimes relieved the pain and cut short the paroxysms by a pill of two grains of extract of hyoscyamus alone.

"If the distinction is properly drawn between neuralgia and those affections only involving the neurilemma, and a sedative anodyne, instead of an excitant anodyne used in connection with quinine, this disease will cease to be an opprobrium to medical science, and its treatment will become much more satisfactory to the practitioner as well as to the patient."—*Ibid.*

Dislocation of the Femur into the Perineum. By Professors PARKER and POPE.

The following case was reported to the New York Medical Gazette, in 1841, by Prof. Parker, in whose practice it occurred; and being an entirely new form of luxation of this bone, we have thought it worthy of a republication in this connection.

The patient, Mr. E——, aged about thirty-five, was a caulker by occupation, and the accident happened while he was at work under the bottom of a canal boat, July 20th, 1831. The boat was raised upon props three and a half feet long. He was standing, bent forward very much, and his feet far astride. Between his feet there was lying a piece of round timber, a foot in diameter. While at work in this position, the props gave way, the boat came down, killing one of the workmen, and forcing the patient down by the side of the timber over which he was standing, in such a manner that the left thigh was placed between it and the bottom of the boat. On being extricated from this situation, the left limb was found standing at a right angle with the trunk, the toes were turned a little inward, the natural form of the nates lost, and the head of the bone distinctly felt in rotations in the perineum behind the scrotum, and near the bulb of the urethra.

Reduction was effected without much difficulty in the following manner: The patient was placed upon a table, resting upon his back, and the pelvis confined by passing a strip of muslin around it. Extension was then made downward and outward accompanied by moderate rotation, and in this way the head of the bone was made to surmount the ramus of the ischium, and to pass into the foramen thyroideum, changing the luxation from a perineal to a thyroideum. From this position, the bone was replaced in the acetabulum, by carrying the luxated limb strongly across the sound one. The patient soon recovered the use of the joint.

The only example of the luxation of the femur in perineo, to which we can refer, in addition to the one here recorded, was reported to the St. Louis Medical and Surgical Journal, July, 1850, by Prof. Charles A. Pope, of the St. Louis University. It is the following:

“J. B., an Irishman, aged forty, on entering the St. Louis Hospital, gave the following account of his accident, which had occurred six hours previously: He was engaged in excavating earth, and having undermined a considerable band, it unexpectedly fell upon his back, catching him in a bent position, with his legs stretched widely asunder. The weight crushed him to the earth, simultaneously fracturing both bones of his right leg, the lower extremity of the radius of the same side, and also dislocating the left hip. On examination, the fractures were readily recognized, and my attention was at once directed to the peculiar position of the left thigh. This was thrown quite at a right angle to the body, and somewhat forward. The normal site of the great trochanter offered a cavity in which the fist could easily be placed, while the head of the bone was both seen and felt below the skin, and raising the raphi of the perineum. On rotation, which was difficult, and caused intense suffering, the movements of the *caput femoris* were distinctly visible. There was experienced by the patient an inability of voiding urine, which was, doubtless, produced by the pressure of the head of the bone upon the urethra, and the difficulty continuing after reduction, relief was afforded

only by the use of the catheter. This trouble soon subsided. The fractures having been attended to, and the patient put under the full effect of chloroform, two loops were applied, interlocking each other in the groin, and using the leg as a lever, extension, by means of the pulleys, was made transversely to the axis of the body. A steady force was kept up for a short time, and the thigh-bone glided into its socket with a snap that was heard by every attendant and patient in the large ward, where, for the convenience of the pillars, the operation was performed. The patient, on recovering from the influence of the anæsthetic, seemed much delighted, and said that he had felt no pain whatever." Dr. Pope continues: "It might be supposed that there may have been, in these instances, some error, and that the displacement of the head was merely into the foramen ovale, and not into the perineum. I feel confident, however, that both (referring to a verbal report of Dr. Parker's case) were really dislocations, as reported. The various medical gentlemen who examined the case with me, all coincided in my opinion. The direction of the displacing force in these two varieties of luxation is precisely the same, and but little greater violence would be required to throw the head on, past the foramen ovale, and branches of the ischium and pubis, into the perineum. In the cases cited, there was the same position of the body, and of the outstretched legs, and the attendant cause and circumstances were just such as would most likely favor the production of such an accident. Had the head of the femur, in the case of J. B., been situated in the foramen ovale, it could not have been so plainly seen and felt, covered as it would have been, by the thickness of the interposed muscular attachments. Besides, the exaggerated position of the thigh, and the direct pressure of the head of the bone on the urethra, causing a retention of urine, are, to my mind, conclusive evidence of the nature of the case; for although, in the latter instance, some irritation might be propagated from the head, displaced in the foramen ovale, yet it would scarcely be so great as to cause retention of urine to the extent observed in the case above related."—*New York Jour. of Med.*

Dry Cupping. By Dr. B. H. WASHINGTON, of Woodburn, Ky.

DR. BOWLING: Please to allow me, through your columns, respectfully to invite the attention of the profession to a few items I have had the good fortune to stumble upon in my practice.

Having heard dry-cupping on the spine very highly recommended for its anodyne, alterative, and emmenagogue effects, by the late Dr. Prather, of St. Louis, and having seen its remarkable effects in his hands, I have used it freely and extended its use, and have never been able to hear or read of a superior remedy, though the last ten years have not been passed in idleness.

As an anodyne it relieves pain without checking any of the secretions; but on the contrary, it regulates the whole system, and brings every organ to the normal standard.

About five years since, a negro man cut his leg severely with a broad-axe, midway the tibia. The wound was dressed in the usual manner; but the next morning his wife informed me he had not slept a wink the whole night. There being some eight or ten children in the cabin, I thought it best not to

dry-cup him until night. In half an hour after he was dry-cupped, he fell asleep, never moved the whole night, and awoke next morning free from pain. The cupping was repeated every alternate night; freedom from pain was the result, and the leg soon healed. In almost every case of bruise or wound, I have used it with signal benefit; and, from my experience, have not the slightest doubt that, if it was added to the usual water-dressing, at least eight-tenths of the cases where mortification now occurs, could be healed without any such result. Many cases of its anodyne effects could be detailed, but one more will suffice. When called to see a negro woman, found her sitting up in bed; great difficulty in breathing; violent pain in left side, greatly increased on drawing a full breath, or coughing; tongue dry; pulse 125, hard and full; was informed she had had an attack of pleurisy about a year previous, and that the pain was in precisely the same spot. Independent of an auscultation, there was something in her looks and actions that convinced me it was not an inflammatory case. I took from a table a large tumbler, and applied successively nearly the whole length of the spine. When I was done, she said she was nearly well. To prevent the pain returning, and to assist in arousing the skin, two grains quinine and two of cayenne pepper were administered, and she was directed to have herself sponged with warm water from head to foot. Next day she was able to walk about well.

Having been dry-cupped a few days after a dislocated shoulder was reduced, I attempted to raise my arm to my head, while the cup was over the origin of the brachial nerves, and found my arms partially paralyzed. It immediately occurred to me that it would be a most excellent remedy for counteracting the resistance of the muscles in cases of dislocation. About six months afterwards, a boy aged seven years was thrown from a horse, and having thrown out his arm to save himself, dislocated the elbow joint. The dry-cup was applied over the origin of the brachial nerves, and the arm was easily reduced, apparently without much suffering; the water-dressing was applied, and the patient soon recovered. In my opinion, if the dry-cup was applied over the origin of the nerve distributed to the dislocated part, instead of the patient being nearly pulled to pieces with ropes and pulleys, as is sometimes the case, the joint could be reduced with far less suffering, and much easier.

Of the alterative effects of dry-cupping, I scarcely know where to begin detailing cases; it is incomparably superior to blue pill or any form of mercury. In January, 1848, I took charge of the case of a woman with chills, who had been in the hands of a distinguished physician for about six months. For the purpose of invigorating her health, so that the chills would *stay stopped*, dry cupping, with frequent sponging of the whole body with tepid water, was recommended. As a matter of course, the chills were cured, and, moreover, have never returned since. This case is mentioned, not because of the thorough cure of the chills, for in that there was nothing uncommon, but for another consideration. When put in charge of the case, I was informed she had had a tetter on her leg for thirty years, and a great number of physicians had prescribed for her without success. I concluded not to do any thing for the tetter until she was cured of the chills; but to my great surprise the dry-cupping had cured that also, and so thoroughly that it has never returned. The cupping was continued every alternate night for about four months. I took the hint thus accidentally given, and have since cured

a case of tetter of twenty years' duration, and it continues well; it is now about three years since the cure. Of course, I now recommend dry-cupping in preference to all other remedies, for tetter. If you wish to see the alterative effects of dry-cupping, the first time you ride out to Mill Creek, near your city, call on Mr. Edward H***. In '47 he was not able to walk across the room without a crutch, from rheumatism and an injury in the groin. For about four years he has been walking without his crutch, briskly, too, and can ride on horseback anywhere, while in '47 he could not ride on horseback at all; and a few weeks since he told me that he was in better health than he had been for twenty years. The course of treatment recommended was dry-cupping the whole length of the spine, with frequent sponging of the whole body with warm water. For ulcers it is superior to all salves and ointments, and even superior to the water-dressing.

For its emmenagogue effects, dry-cupping can be recommended with equal confidence. I do not believe any case of amenorrhœa or dysmenorrhœa would resist its steady application, accompanied with frequent bathing. Some years since, I had a severe case of dysmenorrhœa, of only five months' duration, however. Feeling anxious to afford prompt relief, a celebrated physician was consulted, but not taking his plan, concluded to try dry-cupping, as I had three weeks to go upon. The cups were applied every alternate night the whole length of the spine, more strongly over the origin of the nerves distributed to the uterus; and the result was, at the next period, only a slight headache for a few hours was felt, and the second period no inconvenience whatever.

* * * * *

The glasses commonly used for cupping are too small—tumblers with a thick rim answer much better. Each one should stay on about five or ten minutes, and when it is desirable to produce an impression on a given part, the cup should be more strongly applied over the origin of the nerves distributed to that part. With nervous patients only one or two cups should be applied at first, afterward gradually increasing the number. If they are applied the whole length of the spine at first, the next day perhaps the patient will not be able to hold his hands still. Like all other remedies, it requires judicious use.

I hope the reader will not consider dry-cupping my favorite hobby; I have one I ride in preference. To any one disposed to verify the above statements, names, dates, and residence will be given on application to me. *Nashville Journal of Medicine.*

Chloroform in Obstruction of the Bowels from Spasms. By D. J. CAIN, M. D.

Every physician meets, in the course of his practice, with cases of obstruction of the intestines, which has come on gradually or suddenly, generally, from some cause of irritation existing in them. The obstruction in these cases consists in spasmodic contraction of a portion, or of portions, of the intestines, generally the small. The plan of treatment which I formerly pursued was, to cease all attempts at forcing a passage by means of cathartics, if one or two brisk cathartics failed at the commencement, and to resort to opium freely, enemata of warm water, melted lard or butter, sweet oil, etc.,

the warm bath, fomentations to the abdomen, and other means of inducing relaxation. For more than two years past, I have used chloroform, as a more powerful agent than opium and its preparations, and as more certain in relaxing the muscular system. The chloroform, administered in inhalation, soon produces a greater or less degree of resolution, and, taking advantage of the relaxation thus affected, I give enemata, either stimulating, mucilaginous, or oily, which in a short time bring away fecal matter. The inhalation may be repeated as frequently as, in the judgment of the physician, the case demands.

Chloroform possesses the immense advantage over opium, of relieving effectually and promptly the pain, and in not leaving the bowels in a constricted state, the sedative effect soon passing off.

Seven cases have been thus treated by me, with highly satisfactory results. In one case, only, have I experienced any difficulty in inducing the requisite degree of relaxation of the bowels. The subject of this case was very slightly susceptible of its influence; but the pain was completely relieved by frequent inhalations, and the obstruction gradually overcome.—*Charleston Medical Journal.*

Speedy Cure of Drunkenness.—In the *Abeille Medicale*, M. Laloux mentions a novel, and, according to him, unfailing way of dissipating the effects of alcohol, viz: enemata of common salt. Among the most remarkable cases he narrates are the following:

“In 1840, he was summoned to three youths, who were said to be dead drunk. He found the most complete relaxation of the limbs, with total insensibility, cold extremities, livid countenance, and stertor. A clyster of salt acted like a charm, and in an hour they were able to walk without staggering.”

The effect of the enema is to produce copious stools.—*Prov. Med. and Surg. Jour.*

EDITORIAL DEPARTMENT.

A Table of all the known Operations of Ovariectomy from 1701 to 1851, comprising two hundred and twenty-two cases, including their Synoptical History and Analysis. By WASHINGTON L. ATLEE, M. D., Prof. of Medical Chemistry in the Med. Dept. of Pennsylvania College Philadelphia.

This truly valuable statistical paper is contained in the Transactions of the Am. Med. Association, but has been issued separately for private distribution. The title-page sufficiently sets forth the character of the work, which,

together with the numerous operations performed by Dr. Atlee, will always associate his name with the literature of the subject.

In an appendix to the table, Dr. Atlee presents certain facts which, in justice to his labors, should be generally known. It appears that the successful competitor for a prize on this subject, in London, Eng., Dr. Thomas S. Lee, appropriated from the Am. Journal of Med. Sciences the table of Dr. Atlee, published in that periodical in 1845, without the least acknowledgment. He uses, not only the facts collected by Dr. Atlee, but his arrangement, and even his very language, without giving any intimation that they were borrowed. Dr. Atlee, feeling justly aggrieved, addressed a note to Dr. Lee, appealing to his sense of honor, and calling attention to the omission of reference to the source whence all that was truly valuable in his memoir was derived. Dr. Lee acknowledged the injustice, and promised to make reparation in another edition.

The plagiarism of Dr. Lee hardly admits of any apology, but it seems that the injustice to Dr. Atlee was not limited to the British Author. Dr. Meigs, of Philadelphia, in his work entitled *Females and their Diseases*, etc., quotes in full the table from Dr. Lee's memoir, giving the whole credit to Dr. L., designating it "Dr. Lee's table." In so doing Dr. Meigs, it may charitably be supposed, was deceived, as were doubtless those who awarded the prize to Dr. Lee, and other readers. But in the second edition of his treatise on females, after his attention had been called to the subject, Dr. Meigs sees fit to omit altogether Dr. Lee's tables, as he styles them, but still alludes to them as belonging to Dr. Lee, making no mention of the name of Dr. Atlee in connection therewith.

These facts should be known. The reader will, of course, form his own conclusions respecting them.

Fever Report.—Perhaps the Editor should apologize for the space in this No. occupied by the concluding portion of his Third Report on Continued Fever. A desire not to extend the subject over several numbers, led to the insertion of the whole of the remainder of the Report in the present issue. In the issue for August he proposes to contribute two articles relating to Continued Fever. One will embrace a digest of the morbid appearances after death, exclusive of the intestinal lesions, from papers by Dr. Wm. Jenner, of London. The other will relate to "*Relapsing Fever*," including an analysis of the cases, contained in the Second Report, characterized by relapses. After the publication of these two articles, the subject will be dropped for other novelties.

Rank of Surgeons in the Navy.—The claim of the Surgeons of the navy to an assimilated rank, has been for several years before the profession and the country; but justice to our brethren connected with that arm of our national defence has not yet been obtained. Efforts should not, and it is to hoped will not be abandoned, until the end be attained; and perseverance will, we doubt not, sooner or later, be crowned with success. This subject was brought up at the late meeting of the Am. Med. Association, and referred to a committee, who submitted, by their chairman, Dr. Jackson, of Pennsylvania, the following report, which will be read with interest:

The committee to whom the resolutions relative to the rank of the medical officers of the United States navy were referred, respectfully report:

That the objects sought to be obtained, in respect to rank, by the medical officers of the navy, and the course they have pursued, were approved by this association at the meetings held in Baltimore 1848, in Cincinnati, Ohio, 1850, and in Charleston, South Carolina, 1851.

The attention of the association has again been directed to this subject by Surgeon Ninian Pinkney, of the navy, who has read a memorial he contemplates presenting to congress, and a bill providing by legislative enactment for the definite settlement of this unnecessarily debated question.

This association can have no hesitation in reaffirming its former opinions and action, and of approving the memorial of Surgeon Pinkney so far as it sustains the views of the navy medical officers, as also the provision contained in the bill which he has drafted for the adjustment of this controversy, calculated to disturb the harmony of the officers associated in separate departments of the same service, and whose united action is indispensable to the perfect fulfillment of their respective duties.

It is difficult to understand the opposition of the naval ship officers to the institution of military rank and grade for the navy medical officers. Rank and grade are things in themselves of no value—that philosophers may despise—but it is the universal custom of mankind to employ them as symbols of ideas expressive of honor and respect for individuals or stations. They do not necessarily, and need not confer command, or be connected with other than moral power and influence. The naval ship officers comprehend fully the value of rank and grade and forms of ceremony on the minds of the crews, who are for the most part uneducated, whom they command. They evince on this very subject great pertinacity, and it may also be said jealousy, in the opposition they make to the conferring of rank and grade unassociated with command or power, as marks only of honor and respect, on the naval medical officers. The government some years past, by the appointment of a navy and army examining board, elevated the standard of education for their navy and army medical officers to the highest point, not merely for professional knowledge, but on subjects of general information. It is a higher standard than that of any of our medical schools.

The navy department, acting in uniformity to this requisition of higher attainments in the medical officer, in the year 1847 conferred on him assimilated rank and grade, denoting solely the honor and respect due to him and

his position, bestowing no power that could interfere with the command and proper duties of the naval ship officers.

So far as this association has information, this system worked well—did not conflict with any duties connected with the command of the ship, and that no valid reason existed for its alteration. It was not permitted to remain undisturbed. It was subjected to a revision by a board of officers, in which the medical navy officers had not a single representative. The result has been a report that degrades the navy medical officers from their former rank, and establishes a lower one, and without assigning a reason for this course, or the advantages it possesses over that which had been previously established by the navy department. What renders this proceeding the more invidious is, that the army board, appointed at the time with the navy board and for similar enquiries, have retained the rank and grade of the army medical officers as previously established, which was the same as that of the navy medical officers under the regulation of 1847.

As a consequence of this different action of the boards of navy and army officers, this incongruity, if adopted, will be introduced into the military code of the United States—that the navy medical officers will hold a lower grade and rank than the army medical officers, and a consequent implication of the inferiority of the one to the other.

In view of the above considerations, the committee submit to the association the following resolutions as substitutes for those referred to them:

1. *Resolved*, That the American Medical Association, representing the medical profession of the United States, reaffirm the resolutions passed at the meetings held in Baltimore in 1848, in Cincinnati in 1850, and in Charleston, South Carolina, in 1851, by pressing their approbation and support of the establishment of the assimilated rank conferred on the navy medical officers by the regulation of the navy department in 1847.

2. That this association is not aware of any disadvantage attending on the regulation of 1847; that they can perceive no just cause for its alteration, and disapprove of the change proposed.

3. That it is the opinion of this association that it would be for the interest of the naval service that this question should be settled definitively during the present session of congress, and, if conformable with the usages of the military service, by legislative enactment, to which request they respectfully invite the attention of the honorable senate and house of representatives.

SAM'L JACKSON.

CHRIS. C. COX.

On motion of Dr. Corbin, of Va., the report was received and adopted, and the secretaries were instructed to forward a certified copy of the same to the presiding officers of both houses of congress, and also to the secretary of the navy.

Medical Reform and Private Instructors.—In view of the clamor for various plans embodying supposed improvements in medical education, usually presented under the *ad captandum* title of *Medical Reform*, we have, on several occasions, endeavored to call attention to the fact, that the most important and efficient step, in behalf of the elevation of the character of the

profession, is to be taken by its members individually; in other words, that the measure of so called reform which is most needed at the present time, applies directly to every practitioner who receives into his office private pupils. The burthen of the prevailing complaints of the system of medical instruction, in this country, is directed toward the colleges. Without contending that these institutions are faultless, it is to our mind sufficiently clear, that far greater blame, (if blame is to be attached any where,) lies with the private teacher, who admits pupils into his office wholly unprepared to enter on the study of medicine, and leaves him to steer his way without rudder or pilot, in the intervals of his collegiate courses. In the following extract from an article on medical reform, contained in a late number of the *Transylvania Medical Journal*, this topic is discussed in a manner which will, we think, commend itself to the common sense of the medical reader:

When Lycurgus was consulting with the people in reference to the reform of the State of Sparta, one advised the establishment of absolute popular equality: Lycurgus replied to him, "Sir, begin it in your own house." In like manner we would say to every medical man, who is really anxious for medical reform, "Sir, begin it in your own office." And we do not say it, as did Lycurgus, in the spirit of retort, with a view of indicating the impracticability of the thing, but in all sincerity, because it is in truth and in fact in the office of the private practitioner that the desired reform can be most appropriately and effectually initiated. Medical students, almost without exception, enter upon the study of the profession under the immediate advice and direction of some private practitioner, who is regarded as their personal friend or the friend of the family, or who is selected on some account or other as a suitable person from whom to seek advice as to the propriety of engaging in the study of medicine, as well as guidance in its prosecution. They reach the schools usually after they have been for some time studying under the direction of their private preceptors, and, of course, after their minds are fully made up as to the propriety of their choice of pursuit. At this late period it would be utterly idle in the professors of the Schools to attempt to change the determination of such as may seem to them to have acted unwisely in conceiving themselves prepared to enter upon the study of their choice; because any such advice as might look to the abandonment of their purpose, would be not only unthankfully received by the pupil, but justly regarded as an improper, if not impertinent, interference with the prerogatives of the private preceptor. Medical colleges are public institutions, chartered by the proper authorities for the purpose of affording facilities for the acquisition of medical education, and not for the purpose of deciding who shall or who shall not study medicine. Their managers should consider themselves bound, it seems to us, to receive the pupil of any respectable practitioner, who may wish to prosecute the study of the profession with a view of obtaining the degree which the schools alone are authorized to confer. The duties of their teachers consist in a faithful endeavor to present to the consideration of the pupils the ascertained and reliable knowledge of the profession, and an earnest effort to impress it as permanently as possible

upon their minds, and in the honest examination of the candidates, and a just decision upon the propriety of admitting them to the honors of the Doctorate. They have nothing to do, in their capacity of public teachers, with the determination of the choice of pursuit by the various pupils who resort to their lecture rooms. This has been decided, as it should be, by the private preceptor, and with him, and him alone, rests the responsibility which depends upon the issue. Were the schools to undertake the ungracious task of changing the purpose of pupils who resort to their lecture rooms, under the advice of their private preceptors, there would be one universal outbreak of indignation, on the part of practitioners, and the schools would be justly denounced as arrogant monopolies. For ourself, having the utmost confidence in the general intelligence and honesty of our professional brethren, we are more than willing to concede to them the exclusive right to decide upon the fitness or unfitness of all applicants to engage in the study of medicine, and, when they come to the schools with such endorsement, no inquiry should be made as to the correctness or incorrectness of this primary decision. We are unwilling that the responsibility of advising men to engage in the study of medicine should be shifted from the private preceptors, where it properly belongs, to the public teachers, who, as such, should have nothing to do with it. On the contrary, we would say to the practitioners of medicine throughout the Union, upon you, gentlemen, must rest all the responsibility connected with the admission of pupils to the study of the profession. Your advice is first sought, and by it the course of the pupil generally determined. This first step is one of paramount importance, as it involves not only the question of preparatory education, but also of moral, physical and intellectual qualification for the responsible ministry of our noble profession. The time for making the effort to change the purpose of such as are not possessed of the requisite qualification, is when the purpose is first formed, and before the personal pride and interest of the novice have become involved.

But, we may be here met with the suggestion that these errors can be corrected by the refusal of the schools to admit incompetent or unsuitable persons to the honor of the doctorate. It may be said, it matters but little who is received into the office of the private practitioner, since he has the right to look to the schools for a correct decision upon the merits of the candidate for graduation. To this suggestion, however, there are two conclusive replies, to say nothing of others. In the first place, it is by no means certain, that the pupil will not engage in practice before he attempts to graduate, and thus deprive the schools of the opportunity of passing judgment upon him. And, in the second place, the rejection, as it is technically termed, does not drive the candidate from the profession, but rather forces him, in self-defence, either to engage in practice, or to persevere until he can, charge his memory by a system of grinding, with a sufficient knowledge of details to enable him to obtain, from some source or other, the desired degree. So that, as a general fact, the mere admission of an unsuitable, unqualified person into the doctor's office as a pupil, decides the question of his ultimate introduction into the ranks of the profession; and, in this view of the matter, how important does it become that medical practitioners should exercise a judicious discretion in receiving pupils into their offices? How indispensable, indeed, is the exercise of such discretion to anything like a successful, safe or just scheme of medical reform? We say again, then, to our brethren, who are clamorous for reform — "Gentlemen, begin it in your own offices."

Committees appointed at the meeting of the American Medical Association, May, 1852.—The following are the names of the members of the several committees to report at the meeting of the Association in 1853:

1. Dr. D. F. Condie, of Philadelphia—On the Causes of Tubercular Disease.
2. Dr. James Jones, of New Orleans—On the Mutual Relations of Yellow and Bilious Remittent Fever.
3. Dr. R. S. Holmes, of St. Louis, Mo.—On Epidemic Erysipelas.
4. Dr. Charles D. Meigs, of Philadelphia—On Acute and Chronic Diseases of the Neck of the Uterus.
5. Dr. J. P. Jervey, of Charleston, S. C.—On Dengue.
6. Dr. Daniel Drake, of Cincinnati, O.—On Milk Sickness, so called.
7. Dr. A. Lopez, of Mobile, Ala.—On the prevalence of Idiopathic Tetanus.
8. Dr. Geo. B. Wood, of Philadelphia—On Diseases of the Parasitic Organs.
9. Dr. R. D. Arnold, of Savannah, Ga.—On the Physiological Peculiarities and Diseases of Negroes.
10. Dr. Joseph Carson, of Philadelphia—On the Alkaloids which may be substituted for Quinia.
11. Dr. S. D. Gross, of Louisville, Ky.—On Results of Surgical Operations for the Relief of Malignant Diseases.
12. Dr. James R. Wood, of New York—On Statistics of the Operation for the removal of Stone in the Bladder.
13. Dr. Alexander H. Stevens, of New York—On Sanitary Principles applicable to the Construction of Dwellings.
14. Dr. F. Peyre Porcher, of Charleston, S. C.—On Toxicological and Medicinal Properties of our Cryptogamic Plants.
15. Dr. G. Emerson, of Philadelphia—On Agency of the Refrigeration produced through Upward Radiation of Heat as an exciting cause of Disease.
16. Dr. Henry J. Bigelow, of Boston, Mass.—On the Best Means of Making Pressure in Reducible Hernia.
17. Dr. A. T. B. Merritt, of Richmond, Va.—On Cholera and its Relation to Congestive Fever—their analogy or identity.
18. Dr. Usher Parsons, of Providence, R. I.—On Displacements of the Uterus.
19. Dr. H. F. Campbell of Augusta, Ga.—On Typhoid Fever.
20. Dr. Worthington Hooker, of Conn.—On Epidemics of New England and New York.
21. Dr. John L. Atlee, of Lancaster, Pa.—On Epidemics of New Jersey, Pennsylvania, Delaware and Maryland.
22. Dr. Robert W. Haxall, of Richmond, Va.—On Epidemics of Virginia and North Carolina.
23. Dr. Wm. M. Bolling, of Montgomery, Ala.—On Epidemics of South Carolina, Georgia, Florida and Alabama.
24. Dr. Edward H. Barton, of New Orleans, La.—On Epidemics of Mississippi, Louisiana, Texas and Arkansas.
25. Dr. W. L. Sutton, of Georgetown, Ky.—On Epidemics of Tennessee and Kentucky.

26. Dr. Thomas Reyburn, of St. Louis, Mo.—On Epidemics of Missouri, Illinois, Iowa and Wisconsin.

27. Dr. George Mendenhall, of Cincinnati, Ohio—On Epidemics of Ohio, Indiana and Michigan.

Committee on Volunteer Communications—Drs. Joseph M. Smith, John A. Swett, Willard Parker, Gurdon Buck and Alfred C. Post, of New York

Pennsylvania Medical College.—The following notice of new appointments in the above mentioned institution, is copied from the Med. Examiner:

We learn, with much pleasure, that the faculty of this institution is about to be strengthened by the accession of Drs. F. G. Smith, J. M. Allen, and J. J. Reese, who have accepted the Professorships of *Institutes of Medicine, Anatomy, and Medical Chemistry and Pharmacy*. The two last named chairs have been lately vacated by death and resignation; the chair of *Institutes* is a new creation. These appointments are in every respect excellent, and cannot fail to advance the interests of the school in question. Drs. Smith and Allen have for many years filled lectureships in the Philadelphia Association for Medical Instruction, on the branches to which they are now translated in the Pennsylvania College; and the reputation which they have established, as eloquent, erudite, and judicious teachers, is second to none in the country. Dr. Reese has been for some time connected with the Philadelphia Medical Institute as a lecturer on *Materia Medica and Therapeutics*, and has been most favorably distinguished, both as a teacher and as a writer on this and other branches of medicine. His success in the chair of *Materia Medica* is a guarantee of his ability to do full justice to the kindred subject which he now undertakes.

We may also say, in noticing these appointments, that the high personal character and professional standing of the gentlemen named, cannot but give strength and popularity to any school with which they are connected. The Pennsylvania Medical College has already attained a most respectable position, and under this new organization, may rapidly anticipate a brilliant career. We are sure that it will have the cordial good wishes of the entire profession in Philadelphia. B.

Spongio Piline.—The new article called *spongio piline*, seems admirably adapted to the purposes for which it has been prepared. It is composed of a layer of sponge a third or a half an inch in thickness attached to india rubber cloth. Soaked in hot or warm water it is designed to serve as a poultice, or a fomentation, and it may also be used as a vehicle for medicated lotions. For each of these objects it will be found very convenient, and useful. A. I. Mathews, of this city, is supplied with the article.

American Medical Society in Paris.—The number of Americans resorting to Paris for pursuits connected with medical science, is so large that a society has lately been organized, the objects of which are briefly set forth in the accompanying communication made at the last meeting of the Am. Med. Association, by a delegation from the new society. These objects must at once commend themselves to the cordial approbation of the profession in this country. Such an organization, aside from other advantages, will prove highly useful to our countrymen in Paris, by facilitating the special purposes of their residence in that city. We have received a letter from the secretary of the society, which we hereby acknowledge. It will afford us pleasure to send the Buffalo Journal, as desired, and to contribute, in any mode within our power, to the success of the undertaking. The communication above referred to is as follows:

The undersigned, delegates of the "American Medical Society in Paris," beg leave to submit a few remarks upon the origin, intention and present condition of this institution.

This society, convened for the first time in the month of November, 1851, has for its object the bringing together of American medical men residing in Paris, and the consequent diffusion of American medical and scientific knowledge among them. This institution, as yet in its infancy, has been sanctioned by the French government, and already recognized as a society by the numerous institutions of a similar character in the city. It numbers already fifty regular, active, besides a number of honorary and privileged members. The society, when it becomes more permanently fixed, will undoubtedly publish a journal, containing, in addition to its own original articles, the most interesting foreign medical news of the day, and from the nature of its position, must promise great advantages to the American practitioner in the United States. It most respectfully requests the usual interchanges of the numerous and able medical and scientific journals in the United States.

Circulars requesting this favor have already been addressed to the editors of their respective journals, and the society flatters itself that their request will not be in vain.

The library and reading room attached to the society being open to scientific gentlemen of all nations, will be the means of more thoroughly diffusing American medical literature, and correcting numerous absurd ideas prevailing abroad with regard to our scientific institutions and general attainments.

ALEXANDER J. SEMMES, M. D.,

WM. H. BERRY, M. D.,

R. M. JONES, M. D.,

Delegation of American Medical Society in Paris.

Paris, March 27th, 1852.

New Publications.—Our table is loaded with new books, and various publications requiring editorial attention. Absence from our post for a couple

sound theory, that give so much value to his results. He himself insists particularly on the fact that experiment alone is not sufficient to determine physiological questions. It is necessary that the facts which are perceived by the senses, should be interpreted with judgment; otherwise they might often lead to false conclusions. Particularly with regard to so complicated a process as digestion, is the greatest circumspection requisite, both in contriving experiments and in drawing conclusions.

Every one is familiar with the old theories of digestion, which being almost entirely hypothetical and not based on any accurate experiments, followed each other in rotation according to the fancy of scientific men and the changes of theory; so that the process was supposed in turn to be one of an entirely vital and unexplainable character, one of simple disintegration, of coction, of fermentation and of putrefaction. The discovery of an acid fluid in the stomach, by Reaumur and Spallanzani, and more satisfactorily by Dr. Beaumont, which would dissolve alimentary substances even out of the body, replaced all these by the theory of chemical solution. A great approximation to the truth had been made, and the progress, so far as it went, was sure, since it was the result of the observation of nature. Still the theory of digestion was very imperfect. It was too *exclusive*. It attributed the process entirely to the action of an acid fluid, secreted by the walls of the stomach, which was considered as the universal solvent of all digestible alimentary substances. The peculiarity of the modern views on the physiology of digestion, and particularly of those entertained by M. Bernard, is that they regard it not as a simple but a compound process, resulting from the united action of several different secretions which are poured into the alimentary canal. These different secretions are all recognized to be digestive fluids; but digestive fluids which are destined to act successively on the food, and to digest some one portion and some another, until the whole has been reduced to a state fit for absorption into the circulation. Beside this, Bernard regards digestion not as a mere *solution* of the food by these digestive fluids, but as a process in which the food also undergoes a peculiar chemical modification. It has previously been supposed by many, that the whole object of digestion was to reduce the solid food to a fluid state in order that it might be absorbed. Liquid food, according to this idea, required no digestion. It was already capable of being absorbed, and required only to be taken into the alimentary canal to pass directly and unchanged, into the circulation. Soluble albumen, as the white of egg for example, casein in milk, or sugar in solution in water, would therefore be substances which required no digestion, but only needed to be introduced into the circulation to be employed as usual

for the purposes of nutrition. Therefore these substances might be injected into the veins artificially, without ever passing through the stomach, and undergo afterward their ordinary changes in the circulation. This, however, Bernard shows not to be the case. He takes a living rabbit and empties its bladder by compressing the abdomen in the hypogastric region. By means of a syringe, furnished with a sharp pointed canula, he then injects about an ounce of a watery solution of cane sugar into the subcutaneous cellular tissue of the back, and then allows the animal to remain quiet. In a little less than an hour the bladder is again emptied, and the urine, tested by boiling with tartrate of potass and copper, shows abundant evidence of the presence of sugar; while that passed at the commencement of the experiment gave no trace of it whatever. It makes a great difference, therefore, whether sugar, in solution, be digested by passing through the stomach, or whether it be introduced directly into the circulation. In the former case it is destroyed in the blood vessels and rapidly disappears; in the latter it remains in the circulation and is excreted unchanged in the urine. It is known also by experiment that cane sugar is converted in the stomach into grape sugar, a change which appears to be necessary to prepare the sugar for the part which it is afterward to perform in the processes of alimentation. And it is a change which cannot take place in the vessels but only in the stomach; for in the above experiment of injecting cane sugar into the cellular tissue of the rabbit, it was still cane sugar which was present in the urine, and not grape sugar, as in ordinary cases of diabetes. The process of digestion, then, is one in which the food is dissolved, and at the same time undergoes a certain chemical modification; a modification which is different for different kinds of food.

Bernard's classification of the alimentary principles has nothing in it peculiar. He divides them into three classes, viz: — 1st. The albuminoid substances; containing fibrin, albumen and casein, and also the modification of albumen which exists in the gelatinous tissues, and which, after boiling, appears as gelatine. 2d. Amylaceous substance, — starch, the various kinds of sugar, and dextrine; and 3d. Fats and oils. Since all these substances exist both in animal and vegetable food, it is not necessary to suppose any essential difference in the digestive processes of carnivora and herbivora. We have animal and vegetable albumen, fibrin and casein; there is animal sugar as well as vegetable; and oily matters exist equally in both the two classes of substances. The method followed by Bernard is to investigate in detail the effect produced on the different alimentary materials by the various secretions which are poured into the digestive cavity. These fluids are five in

number, viz., saliva, gastric juice, bile, and the pancreatic and intestinal fluids. They are taken up in the order in which they are met with in following the course of the intestinal tube, and in which the food is naturally subjected to their influence. He begins, then, with the saliva. In examining the properties of the saliva, Bernard is not contented with taking this fluid from the cavity of the mouth, nor even with obtaining it directly from the parotid by a salivary fistula. He collects the secretion from *all* the distinct glands, by opening the ducts and inserting a silver canula, provided with a stopper. He keeps constantly several animals (usually dogs) prepared in this way, and is enabled at any time to obtain pure saliva from the different glands, by exciting their action with sapid substances introduced into the mouth. He recognizes the existence, in this way, of four different kinds of saliva; different in their place of secretion, in their physical properties, and in the function which they are to perform, viz: 1. The saliva of the parotid; which clear, alkaline in reaction, and perfectly fluid and watery, is without the least degree of viscosity. 2. The saliva of the sublingual, which is also clear, and alkaline, but which differs from the first in being exceedingly viscous, as much so as white of egg. This saliva, when placed in a test tube will not run out by its own weight, but remains adherent to the glass. 3. The saliva of the submaxillary, which is intermediate in the degree of its viscosity between the other two. 4. The secretion of the muciparous glands of the mouth, the labial, buccal, and palatal glands, which is very viscous like the sublingual secretion, and does not differ from ordinary mucus. There might also be said to exist a fifth variety, viz., the "mixed saliva" of the mouth, resulting from the union of the four other varieties. It was on this mixed saliva that most of the earlier experiments were performed.

In following out the plan of investigation announced above, and examining the effect of saliva on the different alimentary principles by means of artificial digestions, it is ascertained that, 1st. Saliva has no chemical effect whatever on albuminoid substances; and 2d. That it is equally without influence on oily matters. There remains, then, only the class of amylaceous substances on which it can be supposed to produce any important alteration. And, in effect, it was announced some years ago, that if an emulsion of starch were treated with saliva, and the mixture kept at a moderately elevated temperature for a short time, the starch became completely converted into sugar. This experiment, which was perfectly correct so far as it went, led to the opinion that *saliva had the property of converting starch into sugar*, and that this was in reality the part it was intended to perform in the digestion of the food. This opinion, however, which is based on an experiment in

artificial digestion, cannot be maintained when similar experiments are tried on the natural process. In the first place, pure saliva, taken by means of an artificial fistula from the parotid, is entirely destitute of this property. The same is true, also, of the pure saliva of the sublingual and submaxillary glands. Neither of them are capable of converting starch into sugar alone. They must all be united and the mixed saliva taken from the mucous surface of the mouth in order that the change may take place. This accounts for a discrepancy that at one time puzzled most of the experimenters on saliva, viz., that although the saliva taken from the mouth of the human subject converted starch into sugar, the saliva of the horse, taken from a parotid fistula, had no such effect. Yet it was necessary to suppose that these animals who take so large a quantity of starch in their food, had some means of digesting it; and it was not easy to understand why the process should be different in them and in the human subject. It was simply because the pure saliva from the parotid has never any effect on starch, but only the mixed secretion, taken from the mouth.

In the second place, although saliva has this effect on starch in *artificial* digestions, it is not so in the natural process. In point of fact an emulsion of starch, taken into the stomach, is not converted by the saliva into sugar. If it were so, the starch, mixed intimately with saliva during mastication and deglutition, would rapidly disappear, and could not be detected in the contents of the stomach after a few moments. Starch mixed, out of the body, with the saliva of the mouth, and kept at the temperature of the stomach, becomes entirely converted into sugar in from seven to ten minutes. But if starch be swallowed and subjected to the natural process of digestion, it does not disappear in this rapid manner, but may be detected a long time afterward. To demonstrate this fact Bernard feeds a dog with a thick emulsion of starch which is mixed with pieces of meat, in order to render it palatable to the animal, and twenty-six minutes after the commencement of the meal, a quantity of fluid is drawn from the stomach through a fistula, which has been previously established for the purpose. This fluid, tested with tincture of iodine, shows abundant evidence of the presence of starch.

In the third place, saliva, even out of the body, cannot convert *raw* starch into sugar, but only that which has been cooked and is in the form of an emulsion. Cooked starch, in fact, is already nearly converted into sugar, and requires only a very slight influence to complete the change; so much so, that if left to itself for some days at a moderate heat, the conversion will take place without any other addition. But raw starch is neither converted into sugar spontaneously, nor can the change be effected by mixing it with

saliva. Yet man is the only animal that takes starch in a cooked form. All the herbivora, that consume so large a quantity of starch in grain, potatoes, &c., take it always in a raw state. It is certain, therefore, that they must have some other means of converting this substance into sugar, than simply the influence of the salivary secretion.

Thirdly. This property of the saliva of converting cooked starch into sugar, becomes much more active in cases of mercurial stomatitis. When the saliva is in an unhealthy condition it acts more promptly than usual, and we cannot, therefore, consider that a true digestive property which is increased in activity by a morbid condition of the secretion. This property is, in fact, nothing more than an accidental one. It is considered by Bernard as owing to the change effected in the saliva by commencing putrefaction; and for this reason it is that the pure saliva from artificial fistula does not possess it, but only that which has been already sometime secreted and mixed, on the mucus surfaces of the mouth, with debris of epithelium, &c. Pure saliva, if kept for ten or fifteen days, till it shows signs of commencing putrefaction, then also begins to exhibit this property. It is a property, too, which belongs to almost all the pathological fluids of the body: of ovarian cysts, of dropsy, of peritonitis, &c., all exhibit it to a greater or less extent. The conversion of starch into sugar by the saliva, must, therefore, be regarded only as an accidental, and not as a normal physiological occurrence. During the process of digestion, as it goes on in the living body, saliva has on starch no action whatever. It appears, then, that the saliva is not the important fluid in digestion that has been supposed, having no chemical action on either albuminoid, fatty or amylaceous substances. If we suppress its secretion, in the dog or the horse, by tying the ducts of the parotid, submaxillary and sublingual glands, the *chemical phenomena of digestion suffer no change in consequence*. They go on precisely as before.

Nevertheless the saliva is certainly intended for some purpose; and, in effect, after the suppression of the secretion in the manner indicated above, it is found that the animal suffers in three particulars. A disturbance is produced in the mechanical operations of mastication and deglutition, and at the same time there is a considerable increase of thirst. It is evident that for the complete mastication and deglutition of many kinds of food it is necessary that they should be mixed with a certain quantity of fluid; and that they will require more or less fluid according to their physical conditions of dryness or moisture. It is this fluid which is supplied by the salivary glands; and it is found by experiment that during the processes of mastication and deglutition, the amount of saliva absorbed by any article of food, is in

proportion to its physical characters, and has no relation with its chemical composition. The œsophagus of a horse, for example, is tied at the lower part of the neck, and opened above the ligature. A certain quantity of fluid, which has been previously weighed, is then given to the animal, and as it passes out by the opening in the œsophagus is collected and again weighed. If it was of a dry character like hay, oats, &c., it is found to have absorbed, during mastication and deglutition, from one to five times its weight of saliva. Green fodder, on the contrary, requires only half its weight; and if the food have been previously mixed with a sufficient quantity of fluid, its weight is not perceptibly increased during its passage through the mouth and œsophagus. The mode in which the saliva acts, as a whole, then, is sufficiently evident. Its importance may, also, be demonstrated by tying the salivary ducts and observing the difficulty with which certain kinds of food are afterward masticated and swallowed. A little over nine ounces of oats, e. g., were given to a horse, and the whole was masticated and swallowed at the end of ten minutes. Afterward, the ducts of the parotid glands having been tied, the same quantity of oats was again given to the animal, and it was found that the mastication and deglutition of the food was not complete at the end of three hours; so much had the difficulty of the process been increased. The quantity of saliva, however, is not only different for different kinds of food, but the different kinds of saliva are intended for somewhat different purposes. According to M. Bernard, the saliva of the parotid assists more particularly in the process of mastication; that of the submaxillary has a relation with the function of taste; and that of the sublingual, and the muciparous glands of the mouth, being more viscous and mucous in character, is required for deglutition. All these points may be demonstrated on the same animal, where we have distinct fistulæ established in the ducts of the three principal glands. On introducing into the mouth of the animal strongly sapid substances, such as vinegar or pepper, M. Bernard shows that the flow of saliva is more abundant from the submaxillary duct than from elsewhere. On the other hand, if it be a substance requiring mastication, it is the parotid which becomes excited to unusual activity; and finally, during the act of deglutition, the sublingual supplies the largest proportion.

Another proof of the speciality of function of the different glands alluded to by M. Bernard, is that they are under distinct nervous influences. If the lingual nerve be divided and its cerebral extremity irritated, the sense of taste is excited, and at the same time a flow of saliva takes place, which comes exclusively from the submaxillary.

The saliva, then, cannot be considered as taking any important part in the chemical processes of digestion, but as intended simply to assist in the purely physical operations of mastication and deglutition.

In studying the processes which take place in the digestive cavity, it is found that they become more and more chemical in their character the deeper they are situated in the interior of the body. Those which are performed in the mouth and fauces have been seen to be entirely physical in character. In the stomach, the operations are partly physical and partly chemical;— and finally, it is in the duodenum that the food suffers an alteration of a purely chemical nature.

Gastric Juice.—The digestive fluid which comes next in order is the gastric juice; a fluid much more important in digestion than saliva, since its action is chemical and not simply physical. The food is subjected to its action immediately on passing from the mouth, the pretended œsophageal and pharyngeal glands being no more than muciparous follicles, a continuation, as it were, of the muciparous glandulæ of the mouth, and distinct for a similar purpose.

The gastric fluid does not seem to be secreted, at least in the same proportions, by all parts of the stomach; but to be produced more particularly, if not entirely, by the pyloric portion. The secretion of the splenic portion is apparently rather of a mucous character, and destitute of the acidity and other peculiar properties which distinguish the gastric fluid proper. An ingenious experiment of M. Prevost, showed this fact directly in the dog. After establishing a gastric fistula in the animal, he evacuated the stomach, cleaned it thoroughly, by washing, and then filled it, through the fistula, with a wad of paper, or other convenient substance colored by a solution of litmus. At the end of a short time the animal was killed, and the paper was found reddened by the gastric fluid only in that portion which corresponded to the pyloric portion. That which had filled the splenic portion of the stomach still retained entirely its blue color. It is probable that this is the case with all the simple stomachs. In animals who have a compound stomach, as the ruminantia, it is well known that the secretion of the true gastric fluid takes place only in the last; that which is nearest the pylorus. In the horse, again, we have a stomach intermediate between the simple and compound; not divided into different compartments, but still showing a striking difference in the appearance of the mucous membrane at the splenic and pyloric extremities. The splenic portion is covered with a tough and opaque epithelium; that in the neighborhood of the pylorus being softer and more mucous in character.

The existence of the gastric fluid, as has already been mentioned, was first discovered by Spallanzani and Reaumur, who obtained it by means of sponges which they compelled the animals to swallow, and which they afterward withdrew from the stomach by a cord attached to them. It is evident, however, that in this way they must have obtained an exceedingly impure fluid, and that it must have been a matter of considerable doubt how much of it came from the stomach, and how much from other parts of the alimentary canal. The question was much more completely established by Dr. Beaumont, by experiments on the person of his Canadian Voyageur, who had a gastric fistula in consequence of a gunshot wound. Dr. Beaumont took pains to extract the gastric fluid in a state of purity, by exciting the walls of the stomach, when empty, with the end of a thermometer tube, or a flexible catheter. He ascertained distinctly that when the stomach is empty of food, it contains no acid fluid whatever, but its walls are covered with a thin layer of mucus, which is neutral or alkaline in reaction; and that whenever it is excited by the presence of food, or by the irritation of a foreign body, like the gum elastic catheter, it immediately begins to secrete an acid fluid which has the property of digesting food, even out of the body. M. Bernard imitates, artificially, on animals the lesion which Dr. Beaumont was fortunate enough to encounter, accidentally, in the human subject. He takes a healthy dog, makes a short incision through the walls of the abdomen, just below, and to the left of the sternum, hooks out the anterior parietes of the stomach, and, after making in it a perforation about an inch long, inserts a short canula armed with a shoulder at each end; precisely, to use his own expression, as one would insert a stud into a shirt bosom. The canula is prevented from being drawn inward by the outer shoulder, and from being loosened from the stomach by the inner. The walls of the stomach are in this way kept in contact with the walls of the abdomen at the place of the incision. They are still further secured by one or two ligatures, the canula is provided with a cork, and the apparatus is then complete. The animal is usually entirely recovered from the effects of the wound in a few days, and is afterward ready for experiment. M. Bernard has already done the operation upon a dog in presence of his class, since the commencement of the present course of lectures, and used him afterward for the purposes of experiment.

The gastric fluid obtained in this way is clear, watery, and strongly acid. It has no marked odor, except that which is peculiar to the animal from which it was obtained. The gastric fluid of the dog, according to M. Bernard, has the following composition:

Water holding an organic substance in solution,	- - -	989.73
Lactic and Muriatic Acids,	- - - - -	1.95
Chloride of Potassium,	- - - - -	1.44
“ “ Sodium,	- - - - -	4.59
“ “ Calcium,	- - - - -	0.40
“ “ Ammonium,	- - - - -	0.27
Phosphate of Lime,	- - - - -	0.40
“ “ Magnesia,	- - - - -	0.18
“ “ Iron,	- - - - -	0.01
		Loss, 1.03

Among all these ingredients of the gastric fluid there are only two which are absolutely essential. These are, first, the acid; and second, the organic matter, which is held in solution by the water. As to the acid, it is absolutely necessary that it should be present in the gastric fluid to enable it to digest food; since, if it be neutralized by the addition of carbonate of soda, this property is immediately lost. As for the particular kind of acid, it does not seem essential that it should be one or the other. If gastric fluid which has been neutralized, as above, by carbonate of soda, be again acidulated with either acetic, sulphuric, or muriatic acid, it regains completely its digestive properties. Although the gastric fluid is an acid solution, it does not act *simply* as such. For water, which is merely acidulated with muriatic acid, softens bones, by dissolving the calcareous salts and leaving behind the organic basis. Bones are affected by the gastric fluid, however, in a very different manner. The animal matter, in this case, is dissolved, and the calcareous salts left in a state of simple disintegration. A dog, fed upon bones, discharges by the bowels large quantities of nearly pure phosphate of lime; showing that this substance has not been dissolved by the gastric fluid as it would have been by a simple acidulated solution.

The gastric fluid, then, has an action which is altogether peculiar. This peculiarity is owing to the presence of the second essential ingredient, *viz.*, the *ferment*, i. e. the organic matter, held in solution in the water, which has been known under the name of "*pepsin*." This matter, like all the ferments, is precipitable by absolute alcohol. After its precipitation, it may be redissolved in water; and if the solution be acidulated, it then exhibits the digestive property;—so that we have a kind of artificial gastric juice. In the laws which regulate its action, also, pepsin resembles the ferments. Its action is stopped, for example, by an excessively low, or excessively high

temperature. At 0 (centigrade) it is inactive. At $10^{\circ}\times$ it begins to exhibit its properties, and its action becomes more and more energetic with the increased temperature, till it finally reaches its maximum 35° and 45° , above this point a further increase of heat diminishes its energy, until finally at $75^{\circ}\times$ its action is again stopped.

The active principle, then, of the gastric fluid, is an organic substance, similar in its mode of action to the ferments; and which it is necessary should be dissolved in an acidulated fluid. When these two essential ingredients, the organic principle and the acid, are both present, the gastric fluid has the property of dissolving the *albuminoid and gelatinary matters*; but all other *alimentary substances, amylaceous, fatty, and saccharine, are entirely unaffected by it.*

Now comes an exceedingly interesting and puzzling question. How is it that this fluid which dissolves muscular fibre, mucous membrane, cellular tissue, &c., &c., does not attack the walls of the stomach itself in which it is contained? How is it possible for a fluid, destructive of albuminoid substances, to be contained in an organ which is itself composed almost entirely of albuminoid tissues. This difficulty has usually been explained by referring to the vitality of the tissues of the stomach, by which they are rendered capable of resisting the chemical influence of the solvent; in the same manner as we know that the living body resists the extremes of heat and cold, while after death it immediately takes the temperature of the surrounding atmosphere. M. Bernard, however, explains the matter in a very different way. According to him, the gastric fluid resembles in its mode of action, not only the ferments, but also a large class of poisonous substances, such as the venom of serpents, the vaccine virus, the Woorara poison, &c., which are exceedingly active when introduced into the circulation, but still may be taken into the stomach with impunity. It is some years now since M. Bernard undertook a series of experiments for the purpose of ascertaining, if possible, the reason of this singular peculiarity; and the conclusion at which he arrived was that these poisons had no effect when taken into the stomach, simply because they *were not absorbed*. The fact had been previously explained by supposing that the gastric fluid acts upon the virus and destroys immediately its noxious properties;—that the poison is, in fact, *digested* and chemically altered immediately upon entering the stomach. Bernard, however, shows most conclusively that this is not the case. Last week he brought into the lecture-room a dog with a gastric fistula, and introducing a glass tube extracted a small quantity of gastric fluid, with which he inoculated a sparrow, by pricking it into a fresh wound in the thigh. This experiment was merely

preliminary, and intended to show that the gastric fluid alone is not noxious. In effect, the sparrow appeared to suffer no inconvenience from the operation. Bernard then introduced into the stomach of the dog, about ten grains of the Woorara poison [dried extract] and allowed it to remain. Between seven and eight minutes afterward another quantity of gastric fluid was taken from the dog's stomach, and a second sparrow, of the same size and appearance as the first, inoculated with it; and at the end of two minutes the bird was dead. Notwithstanding, the dog, as well as the first sparrow, remained perfectly unharmed.

It is proved, therefore, that these poisons are not destroyed by the gastric fluid, but are simply dissolved in it and retain their activity; and that it is possible for an animal to have in its stomach, without suffering any harm, a poison which, if introduced into the circulation, would be fatal in a few seconds. Now for the reason why these substances are not absorbed and consequently do not prove injurious. M. Bernard considers the stomach as defended from their action simply by the layer of mucus which covers its internal surface; in precisely the same manner as the skin is defended by its epidermis. The Woorara cannot penetrate the epidermis, and consequently is not absorbed when held in the hand. Neither can it penetrate the mucus of the stomach and is therefore innocuous when swallowed. To exert a poisonous action it must be introduced into the circulation.

According to Bernard, the gastric fluid, also, to exert its solvent power, must be *absorbable* by the substances with which it is placed in contact; and, like the Woorara, is prevented from attacking the walls of the stomach by the mucus which covers its surface. But there is still a great difficulty remaining, viz., that as the gastric fluid is secreted by the mucous membrane of the stomach, it must necessarily be in contact with it when first produced. In order to reconcile this difficulty we must recollect the manner in which Bernard considers the gastric fluid to be formed.

There are two essential ingredients in the gastric fluid, viz., the organic matter, the ferment, called "pepsin," and the acid fluid in which it is dissolved. There are also two secretions in the stomach, viz., mucus, which is exuded from the surface of the mucous membrane, and the acid fluid which is secreted by the gastric tubules, or glandules. The layer of mucus is constantly being renewed; a fresh supply being continually exuded from the surface of the stomach, and gradually pushed farther and farther from it by that which is secreted afterward underneath. The secretion of the gastric tubules, when first produced, is simply acidulated fluid, incapable of digesting alimentary substances. It does not contain any of the organic matter.

This organic matter does not exist until the acid fluid has traversed nearly the whole thickness of the layer of mucus, and is consequently separated from contact with the mucous membrane. *The active principle of the gastric fluid is, in fact, simply the gastric mucus which has become altered while moving from the surface of the stomach toward its cavity; which has lost, in the mean time, its viscosity, and become soluble in the acid fluid.* It is, then, very easy to understand how the gastric fluid may be said to be secreted by the mucous membrane of the stomach, and yet never to have been in contact with it. So certain is it that the active digestive principle is only altered gastric mucus that we may make an artificial digestive fluid by mixing the mucus of the stomach with acidulated water. After a few days, when the mucus begins to undergo a putrefactive change, it loses its viscosity, dissolves in the water, and the solution then exhibits all the digestive properties of a true gastric fluid. It may be considered then, as almost demonstrated that the active principle of the gastric fluid and the altered mucus of the stomach are identical.

Such is the explanation of M. Bernard. It is open, perhaps, to one or two objections, though it is hardly possible to conceive of one more ingenious. It does not seem, for example, altogether clear, if the Woorara poison is prevented from entering the circulation, when swallowed, simply by the layer of gastric mucus, why opium, strychnine, belladonna, &c., &c., should not be prevented by the same means. Hydrocyanic acid, and a multitude of other poisonous substances, do not find any obstacle in this layer of mucus, when introduced into the stomach. Again, if this layer of fresh mucus is so impenetrable to the gastric fluid, it would seem almost certain to interfere very considerably with the process of digestion. When a quantity of food is taken into the stomach, and subjected to the peristaltic motion, by which it is rolled about from one extremity of the organ to the other, the mucus, which is sufficiently abundant to protect entirely the walls of the stomach, must be mixed, in no small quantity, with the food itself; and, wherever it is applied must necessarily prevent the contact of the gastric fluid, and completely interfere with the solution of the food.

The innocence of Woorara and similar poisons, when swallowed, has generally been referred to a vital property of the mucous membrane rather than to its physical condition. The mucous membrane seems to have the power of absorbing some substances in solution, and rejecting others; in the same way as the roots of plants take up only certain salts from the soil, though others, equally soluble, may be abundant in the neighborhood. It has been supposed, at the same time, as already mentioned, that it was the vitality of

the stomach which defended it from the action of the gastric fluid. This idea was confirmed by the fact that immediately after death, solution of the stomach, in its own digestive fluid does, in reality, take place. The existence of all sorts of intestinal parasites, many of which are not only capable of living in the stomach of an animal, but are habitually developed there, and during a certain stage of their existence, find in the cavity of this organ the most favorable conditions of life, seemed hardly to be explained in any other way. M. Bernard, however, not only considers the impunity with which these animals inhabit the stomach as owing to another cause than their vitality, but he also denies the fact that vitality alone does protect an animal from the action of the gastric fluid. Certain substances, he says, which are perfectly digestible in themselves, are rendered indigestible by the tough and impenetrable nature of their external envelope; kidney beans, for example, in the new state, will pass through the intestinal tube undigested, because the gastric fluid cannot penetrate their coriaceous envelope. The horny epithelium of certain insects also defends them in the same manner, and enables them to live as parasites in the interior of the stomach. They are indigestible, alive and dead, for the same reason. If, however, we take an animal whose epidermis is permeable to fluids, it is no longer protected by its vitality from the action of the gastric juice. Such an animal, according to Bernard, may be digested alive. To prove this he takes a living frog, or an eel, and introducing the posterior half of the animal into a dog's stomach through the gastric fistula, fastens it in that position, with the head and anterior part of the body projecting from the opening. After an hour or more, he finds the parts which have been immersed in the dog's stomach, more or less completely digested; their vitality destroyed, and their consistency almost entirely gone. Notwithstanding, the animal is yet alive, respire, and is sensible to irritations of the skin. This experiment is a new one, brought forward by M. Bernard for the first time, at the present course. I have seen it tried several times, in the lecture room and elsewhere, but so far, I acknowledge, it has not been, in any instance, altogether satisfactory. In some it evidently failed, though the frog was left in the dog's stomach more than an hour. In all cases, too, the animal which has been subjected to this process of semi-digestion, though living at the end of the experiment, is very feeble, and dies no long time afterward. There are so many precautions to be observed, also, in so complicated an experiment, in order to avoid all sources of fallacy, that it can hardly be considered yet as entirely successful. There are, no doubt, several points in the physiology of digestion, which are still obscure. Others, however, which were formerly as much so, have been now satisfactorily

settled; and a great gain has certainly been made, though something still remains to be done. There seems to be no doubt regarding the action, for example, of the first two digestive fluids. The saliva acts simply as a more or less viscous fluid, in assisting the mechanical processes of mastication and deglutition; while the gastric fluid attacks and dissolves the albuminoid substances of the food. The two remaining alimentary principles, the fatty and amylaceous, remain to be acted on by the fluids beyond the pyloric orifice.

Yours truly,

JNO. C. DALTON, JR.

ART. II.— *Painful Tubercle; a Report of Two Cases, and the Result of a careful Dissection, by which it is shown not to be a Neuroma, or an Enlargement of the nervous tissue, as has been generally believed.* By FRANK H. HAMILTON, M. D., one of the Surgeons of the Buffalo Hospital.

I do not remember to have met with more than the two following cases of "painful tubercle," and as the tumor is exceedingly rare, and its pathology does not seem to have been generally understood, I shall claim for its consideration some space.

CASE I. I. L. D., aged 26; from Fayette, Seneca county; in good health. Nine months since, he first discovered a small hard tumor on the back of the left shoulder, immediately under the skin. It was then, and has been ever since, extremely sensitive. Four months since, it was accidentally hit, and the pain was intense. It will not bear even the chafing of his clothes without pain. It never has been inflamed. It is now about the size of a large pea; hard, irregular, of a brownish color, and movable.

I removed it by excision, cutting away the cellular texture freely about it. I could not discover any nerve or other vessel communicating with it, or passing through it. Its texture was firm and fibrous.

CASE II. Hannah Duchett, aet. 25 years; healthy. About five years since, she began first to feel a "deep" pain, at a single point, on the fibular side of the calf of the right leg. The pain was then, and has always been since, paroxysmal, occurring at first at intervals of a month or so. The sensation was like electricity, shooting from the point, as a focus, in several directions up and down the leg, but not extending usually farther than four or five inches. Such was the pain, so peculiarly diagnostic of the "painful tubercle."

All this preceded the existence of any tumor, or of any mark by which the spot could be designated; and it was not until a year ago that she first began to perceive a very small, hard tumor, at the point of suffering.

The tumor has now attained the size of a large pea; the skin is slightly elevated, but not discolored. Upon the center of the summit of the tumor is a small black spot, of the size of the point of a pin, which looks like the obstructed lacuna of one of the cutaneous follicles. The tumor is hard, and slightly movable. It is exquisitely sensitive; the gentlest brush producing a severe pain. The pains are still paroxysmal, but recur almost daily and often many times during a single day.

May 3, 1852. I excised the tumor with a considerable amount of cellular texture around it.

Examination of the Product.—The tumor lay imbedded upon the under surface of the skin, in which it had formed a cup-like depression, but to which it had no attachments except a very loose cellular. The skin over it, was reduced to less than half its usual thickness. The most careful dissection, aided by a glass, on all sides of the tumor, did not disclose the smallest filament of a nerve, or any thing except the same loose cellular texture.

It was covered with three investments, formed, as I have usually found them in non-malignant tumors occurring in cellular textures, only that the number is sometimes greater, viz., *first*, a tunic formed of condensed cellular texture, the result of the gradual encroachment of the tumor upon this texture; *second*, its immediate and proper investment, which was more dense and pearly; *third*, loose flocculi, or delicate cellular structure uniting the two.

The tumor was firm and elastic. When cut it looked to the naked eye semi-cartilaginous, or firm gelatiniform. With the eye glass small white spots were visible, but no fibres. Its color was very white.

Dupuytren, in his oral lectures, first demonstrated that such tumors were not neuromata. The symptoms and pathology are explained by him in a manner so graphic that we need, I think, never be in doubt; yet it will be seen, that very few have observed his distinctions; and it is for this reason that I have thought it proper to give these cases so much in detail, as additional confirmations of the correctness of his opinions.

ART. III. — *Morbid Appearances after Death, (exclusive of intestinal lesions,) distinctive of the Typhoid and Typhus types of Continued Fever.*
By THE EDITOR.

The investigations upon which were based my Clinical Reports on Continued Fever relate chiefly to events belonging to the *living* history of the disease, that is to say, to the symptoms and circumstances developed during the continuance of life. In the fatal cases, inquiries concerning the morbid effects impressed upon the organism, were generally prosecuted but, to a limited extent, attention frequently being directed only to the presence or absence of the intestinal lesions regarded as characteristic of the Typhoid type. It is needless to say that the appearances after death form an important portion of the natural history of any disease; and with reference to these, as well as the phenomena manifested during life, it is important to examine *all* the organs of the body, noting the absence, as well as presence of morbid changes. The post mortem history, like that pertaining to the phenomena of life, is to be based on analyses of recorded observations, sufficient in number, and made at different periods and places. Studied in this way, the morbid anatomy of Continued Fever offers a field for inquiry, as yet by no means exhausted. The researches of Louis embrace an account of the appearances in a pretty large number of cases of *Typhoid fever*; and within the past few years, facts have been contributed by various observers relative to the changes distinguishing this from the *Typhus* type. A point of absorbing interest, however, has been the presence of intestinal lesions in *Typhoid*, and their absence, or relative insignificance in *Typhus*. Attention appears to have been in a great degree engrossed by these lesions, to the exclusion of a more extended comparison of the two types embracing the appearances found in other organs.

Dr. William Jenner, Prof. of pathological anatomy in University College, London, has lately contributed some observations on this subject which are highly interesting and valuable. They are contained in a series of articles communicated for the *Monthly Journal of Medical Science*, thus entitled:— “On Typhoid and Typhus fevers — an attempt to determine the question of their identity or non-identity, by an analysis of the symptoms, and of the appearances found after death in sixty-six fatal cases of Continued Fever, observed at the London fever hospital from Jan., 1847, to Feb., 1849.” It will be perceived by the foregoing title that the articles embrace an analysis of the symptoms, as well as post mortem appearances. The course pursued by Dr. J. was to select only fatal cases, and to determine the type by the presence

or absence of the intestinal typhoid lesions. Having in this way arranged them into two groups, he then proceeded to analyze the symptoms and appearances after death in both groups, and compare the results. The articles by Dr. Jenner have fallen under my notice since my third Report was written. The data for comparison of the morbid appearances in the two types (typhus and typhoid) are fuller and more complete than, so far as I know, have been contributed by any other observer; and in order to supply a manifest deficiency in the analyses which I have made, I shall take the liberty of appropriating, for the benefit of those who have honored my Reports with a perusal, a summary of the results of his investigations. The synopsis which follows is copied from the Journal already mentioned, and is given as it is there presented by the author. The intestinal lesions are excluded. The propriety of this is apparent when it is considered that upon the presence or absence of these lesions the post mortem diagnosis was based. Moreover, as respects these lesions, the fatal cases in my collections were studied with considerable care.

Synopsis of morbid appearances distinctive of Typhus and Typhoid fever, based on an analysis of sixty-six cases.

“*Cadaveric rigidity*, ceased much more quickly in subjects dead from typhus than from typhoid fever.”

“*Discoloration of the walls of the abdomen and of the skin covering the larger veins*, was much more frequently present in those dead from typhus than typhoid fever.”

“*Emaciation* had made greater progress in the typhoid than in the typhus subjects.”

“*Spots*. The spots observed during the progress of the cases of typhus fever continued after death; no trace of the spots visible during life could be detected after death from typhoid fever.”

“*Head*. After typhoid fever, the pia mater and arachnoid separated from the convolutions with abnormal facility in one only of nine cases examined with reference to this point. The vessels of the pia mater were abnormally filled with blood in one-third of the cases, but intensely congested in one only of the fifteen cases; the cerebral substance was congested in one-seventh of the cases. After typhus fever, the pia mater and arachnoid separated with abnormal facility in nine of eleven cases of which notes on the point were made. The vessels of the pia mater were congested in nearly half, and intensely congested in one-fifth of the whole of the cases; while the cerebral substance itself was abnormally congested in half”

"*Hemorrhage into the cavity of the arachnoid*, which was not found in a single case of typhoid fever, had occurred before death in one-eighth of the cases of typhus fever." "The amount of sesosity found within the cranial cavity was decidedly greater after typhus than typhoid fever."

"*Pharynx*. After typhoid fever, this organ was found ulcerated in one-third of the cases. After typhus fever, ulceration of the pharynx was not detected in a single case."

"*Larynx*. Ulceration of the larynx was found in one of fifteen subjects dead from typhoid fever; in one of twenty-six from typhus fever."

"*Œsophagus*. After typhoid fever, ulcerated in one of fifteen cases in which it was examined. After typhus fever, the œsophagus was free from ulceration in all the twenty-four cases in which it was examined."

"The epithelium separated from the œsophagus spontaneously at an earlier period after death from the latter, than the former disease."

"*Stomach*. In none of the fifteen cases examined after death from typhoid fever was the mucous membrane of the stomach softened throughout the whole extent; in no case did softening of the cardial extremity approach perforation. In four of thirty-seven cases of typhus fever the whole mucous membrane of the stomach was softened; and in four others there was such extreme softening of the whole of the coats of the great *cul de sac*, that they were perforated by the slightest violence.

"*Small Intestines and Mesenteric Glands*. The presence or absence of lesion of these organs was the ground on which the cases of typhoid and typhus fever here analyzed were divided from each other,—consequently they were invariably diseased in the one, and sound in the other."

"*Large Intestines*. After death from typhoid fever, the mucous membrane of the large intestines was found ulcerated in rather more than a third of twenty cases. In no instance after death from typhus fever.

"*Peritoneum*. As peritonitis was in typhoid fever secondary to, and dependent on the entero-mesenteric disease, it may here be excluded from consideration."

"*Spleen*. This organ was enlarged in all the cases of typhoid fever—softened in one-third of the cases only. Before the age of 50, it was as large after typhus as typhoid fever; after that age, it was decidedly smaller in the former than in the latter affection. After the age of 50, it was as soft in typhus as in typhoid fever; before that age it was frequently softened."

"*Gall Bladder*. There was ulceration of the lining membrane of the gall bladder in one of fourteen cases of typhoid fever; in none of thirty-one

cases of typhus fever. In the latter disease the bile was much thicker, and of a darker green color than in the former."

"*Liver, Pæreas, Kidneys.* These organs were more flabby in the cases of typhus than in those of typhoid fever."

"*Urinary Bladder.* This viscus was ulcerated in one of the cases of typhoid fever—in none of the cases of typhus fever."

"*Pericardium.* This cavity contained a small amount of yellowish transparent serosity in all the cases of typhoid fever examined. The contained serosity was red, from transudation of a solution of hæmatosin, in five of thirty-one cases of typhus fever, in which the pericardium was examined before the termination of the fever.*

"*Heart.* The muscular tissue of this organ was much more frequently and decidedly flabby, and its lining membrane was much more frequently and deeply stained of a dark red color, in the cases of typhus fever, than in those of typhoid fever."

"*Lungs.* Granular and non-granular lobular consolidation were very frequent in the subjects dead from typhoid fever—rare in those dead from typhus fever. The reverse was the fact with reference to congestion of the most depending part of the lung."

"*Pleura.* Recent lymph or turbid serosity was found in six of fifteen cases of typhoid fever—i. e., between half and one-third, or in the proportion of forty per cent. The same lesions not much less in amount were found in two only of thirty-six cases of typhus fever—i. e. one-sixteenth, or in the proportion of 55 per cent."

The foregoing is copied from the Monthly Journal of Medical Science for April, 1850.

Dr. J. adds, after the foregoing parallel of pathological appearances, as follows: "The particulars here briefly recapitulated, and still more those fully detailed in the foregoing papers, appear to me to prove indisputably that the symptoms, course, duration, anatomico-pathological lesions, and the tendency to cadaveric changes, are different in typhoid fever from what they are in typhus fever."

* There is evidently a mistake in the last sentence.

ART. IV. — *Relapsing Fever.* By THE EDITOR.

In the distribution of cases for analytical investigation hitherto, I have practically assumed that they were either of the *Typhus* or *Typhoid* type. Are all the varieties or species of Continued Fever embraced in these two forms of the disease? This is an interesting and important question which perhaps cannot, in the present state of knowledge, be answered positively.

The distinctive nosological features of *Typhus* and *Typhoid* fever have been but recently established, and it would not be surprising if further investigation should lead to other subdivisions of continued fever, showing that we have been accustomed to include in one or both of these forms, affections which should be considered distinct.

The opinion has been advanced by some British writers that already there are sufficient grounds for such a subdivision. A form of fever observed at different epochs in Edinburg and London is described as possessing, in common, characters differing from those of *Typhus* and *Typhoid fever* sufficiently to constitute it a distinct affection, which has been named, *Relapsing fever*. The following extract from an able article in the *British and Foreign Medico-Chirurgical Review*, No. for July, 1851, furnishes a brief historical notice of the occurrence at different times and places of the kind of fever which has received this title:

* "In the early part of 1843, a febrile disease appeared in Edinburgh and Leith, so different in its course, in its symptoms, and in the amount of its mortality, from any Continued Fever which had been recently observed there, that it was at once and unhesitatingly declared to be a peculiar and new disease. It was soon known that the same disease had appeared in Glasgow a month or two prior to its outbreak in Edinburgh; it was more or less prevalent also in Dundee and in other large towns in Scotland, whether it appeared in London or in other English towns is doubtful; at any rate it was not described. It was observed with great accuracy, and recorded in the periodicals of the time, by Craigie, Alison, Arnott, Henderson, Halliday, Douglas, Jackson, and Mackenzie; and it was made the subject of two special and excellent treatises by Cormack and Wardell.

"It was soon discovered, however, that although this fever had not been seen in Edinburgh for many years, it was not altogether a new disease. Dr. Christison expressed an opinion that it was similar to the fever witnessed by

* We omit the various authorities to which the writer refers. For these the reader may consult the Review.

him in 1817-18, and recorded by Welsh, in his well-known work, and by himself in the 'Library of Practical Medicine.' That this opinion is correct, and that the Edinburgh epidemic of 1817-18, was in great measure made up of this disease, no one can doubt, who will attentively collate the descriptions of Welsh and Christison, with those furnished by the observers of the attack in 1843. But not only in Edinburgh was evidence of its former existence brought forward. It was observed that it had evidently formed part of the Irish fever of 1817-18-19, which had been so minutely recorded by Barker and Cheyne. It appeared also that this fever had been prevalent in Ireland for many years. Epidemics in 1739 and 1741 were described in unequivocal terms by Ruddy; and other epidemics during the eighteenth century, and those in Dublin in 1806 and 1826, presented, among other forms of fever, the peculiar and unmistakable features of the disease in question.

"The fever thus distinguished and elaborately described by the Scotch observers in 1843, became again epidemic in Glasgow, Edinburgh, and other towns in Scotland, in 1847. It did not by any means prevail so extensively, and there was a simultaneous occurrence of other species of fever; among which, however, the eyes of Steele, Paterson, Orr, and others, trained by the epidemic of 1843, had no difficulty in singling out this peculiar form. In 1847 it was also epidemic in London; and having come under the observation of Dr. Jenner, has been very carefully described. Before this time, in 1846, and in previous years, sporadic cases had been witnessed in the metropolis; and since 1847 a case has every now and then presented itself at the London Fever Hospital, and at other institutions.

"Instructed by the experience of these observers, it is also easy to perceive that this disease is not confined to these islands. It appears, although imperfectly described and confounded with other forms, in the pages of the celebrated treatise of Hildenbrand; and in the epidemic, in 1847, in Upper Silesia, which we intend presently to describe, it evidently formed in some places the great bulk of the cases. Yet it must be said, that although its characters are so striking that the most superficial observer could not overlook them, the German systematic writers have made no allusion to it as a separate disease; and even those who observed it have failed to draw that obvious inference to which the Scotch physicians unanimously came, viz., that it is a disease altogether distinct from ordinary Continued Fever.

"We are not aware that any perfectly satisfactory evidence is to be found in French writers of the existence of this fever in France; it has, in fact, considering the elaborate descriptions of the Scotch physicians, been somewhat

singularly overlooked in that country, as in Germany. In America it is not, I believe, known, or at any rate it has not been described. There is reason from Dr. Bell to believe, that it is known in Persia."

The subject of *Relapsing fever* has, as yet, received little or no attention in this country. At the time of writing my second report, I confess I was only aware of the fact that this name had been employed to designate a form of continued fever. I was not acquainted with the distinctive characters attributed to it.

In reference to that Report the reader will perceive that among the forty-eight cases analyzed, were several (fifteen) which were characterized by the occurrence of a *relapse of fever*, after convalescence appeared to be distinctly declared.* The occurrence of this second febrile career in so large a proportion of that collection of cases, was the more surprising from the fact that nothing of the kind had been observed in the cases previously analyzed, and I can now add that the same is true, as a general remark, of the cases upon the analysis of which my Third Clinical Report is based. The article in the British and Foreign Medico-Chirurgical Review, from which the above extract was taken, appeared about the time I had completed my second report. After reading that article the inquiry naturally arose whether the cases of fever characterized by the occurrence of relapses in the collection I had just analyzed, might not be cases of *Relapsing fever*. With reference to this inquiry I now propose, as I then promised, to subject them to a separate analysis, with a view to ascertain if, in other points than the occurrence of relapses, they differ from *Typhoid* and *Typhus* fever, and, if, at the same time, they exhibit traits which are said to distinguish *Relapsing fever*. Before entering on this analysis, however, inasmuch as it will probably not be an act of injustice to our readers to presume that many, if not most of them are but little acquainted with the subject, I will premise some account of the diagnostic points which are said to distinguish this form of fever. In so doing I shall avail myself, without giving more particular credit, of the article in the British and Foreign Medico-Chirurgical Review already referred to, and a paper by Dr. William Jenner, on the "identity or non-identity of the specific cause of Typhoid, Typhus and Relapsing fever," contained in the Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London." [Second series vol. 15, 1850.]

Evidences of a form of fever supposed to be identical with *Relapsing fever*, have heretofore received different names, such as the "short fever;" the "five day fever;" the "seven day fever;" "bilious remittent fever;" "remittent icteric fever;" "mild yellow fever," etc.

The disease is stated to affect all ages, and both sexes in about an equal ratio.

The *access* does not present any very distinctive features. The attack is oftener abrupt than in Typhoid fever, and the muscular and articular pains are apt to be severe.

Delirium, and other head symptoms are represented to be oftener absent, and, when present, less in degree than in the other forms of continued fever.

The absence of the abdominal symptoms which are so generally present, in a greater or less degree, in *Typhoid* fever, is an important point distinguishing *Relapsing fever*. Diarrhoea, with notable meteorism, and tenderness in the iliac regions, do not belong to the latter. It is, however, characterized by certain symptoms pertaining to the digestive system rarely found in *Typhoid*, viz: nausea and vomiting, which are frequently prominent symptoms, and tenderness over the epigastrium. The matter vomited is stated to be "bright grass green," and sometimes like coffee grounds, approximating to the black vomit of yellow fever.

The eruptions characteristic of *Typhus* and *Typhoid*, are not found in *Relapsing* fever. Sudamina and occasionally petechiæ have been observed. An eruption consisting of "small spots, round, purple, unaltered by pressure" has been described by some observers as an element of the affection. These spots are stated to be extremely like flea bites, and it does not appear to be fully ascertained that they were not, in fact, flea bites. The chest symptoms, i. e. cough and bronchial rales, so generally present in *Typhoid* fever, are said to be less commonly present in this disease. *Epistaxis*, so frequently observed in *Typhoid* fever, sometimes occurs. The pulse, seldom falling below 100, in more than half the cases is 120, and in a considerable number the frequency is still greater.

Profuse sweating is stated to occur pretty uniformly, preceding the apparent convalescence, and also when the relapse of fever is about to terminate. Another distinguishing feature referable to the skin, is more or less yellowness, occurring on the fourth or fifth day. In severe cases the jaundice is often a prominent symptom. The most distinctive event, however, is that indicated by the title of the disease, to wit: the occurrence of *relapses*. The first febrile career continues for a period rarely less than *four*, nor more than *ten* days, and then ends, generally after profuse sweating, which is considered to be critical, leaving the patient free from febrile movement, and apparently convalescent. After an interval varying in duration from *five* to *eight* days, another attack occurs, generally abruptly, and frequently preceded by a chill. The recurring febrile movement is equally or more severe than the first. It continues from

four to five days, and then terminates usually after profuse sweating. Usually after a single relapse, the patient becomes permanently convalescent. A second, third, and even a greater number of relapses, however, have been observed to occur.

The disease is seldom fatal. Patients almost uniformly recover, unless some complication takes place, such as pleurisy, pneumonitis or dysentery.

The most distinctive characters derived from autopsical examinations, are the absence of the intestinal lesions belonging to *Typhoid* fever. The peyerian glands are unaffected. The spleen is generally enlarged and softened.

It may be communicated by contagion, and Dr. Jenner, in the article already referred to, has collected observations tending to show that Relapsing fever cannot be derived from patients laboring under the other forms of Continued Fever, but that it alone produces the special miasm for its propagation. How far the facts contributed by him go to establish this important point, it does not fall within my present purpose to inquire.

This form of fever does not secure immunity from the other forms; nor from subsequent attacks of the same form.

These are, briefly, the more prominent of the traits distinctive of *Relapsing* fever which are mentioned by the two writers to whom I am indebted for the foregoing summary. They appear to have been deduced from an examination of reports of epidemics, supposed to be of this character, by various authors. If they are sufficient to render probable the position that the disease to which they relate is a peculiar form of fever, distinct from the *Typhus* and *Typhoid* forms, it is certainly desirable that collections of cases should be subjected to careful numerical analyses. The question can only be definitively settled by the results of such analyses.

So far as I know, we have no account of a form of fever prevailing in this country, analogous to the relapsing fever as just described. Were the cases characterized by relapse, among those observed by me during the winter of 1850-51, cases of *Relapsing* fever? With a view to arrive at the answer to this question, and, if the conclusion be in the affirmative, to furnish a small contribution toward the natural history of that form of fever, I will now proceed to analyze the cases referred to.

Analysis of Fifteen Cases of Continued Fever, characterized by Relapses.

In my Second Clinical Report I have stated that *fifteen* of the *forty-eight* cases upon the analysis of which that Report was based, were characterized

by the occurrence of relapses. Of these fifteen cases *nine* were included among those of *Typhoid* fever; *one* was considered a case of *Typhus*, and *five* were among the cases of *Doubtful type*. Upon a fresh examination of the cases I find that one case in the *Typhoid* group was rejected from the list in consequence of the first convalescence occurring about the time the patient was admitted into the hospital. I am satisfied that this case may be included in the category. The single case of *Typhus* characterized by a relapse, was undoubtedly a case of that type, the characteristic eruption being well marked. The apparent convalescence in this case occurred on the *eleventh* day, and the relapse of fever was of two days duration, after an interval of only three days. These facts show that this could not have been a case of *Relapsing* fever, and I shall accordingly now exclude it. The whole number of cases, therefore, will remain the same as stated in the Second Report, viz: fifteen; and of these fifteen cases *ten* were distributed among those of *Typhoid* fever, and the remaining *five* among the cases of *Doubtful type*.

Of the fifteen cases, *eleven* were males, and *four* were females.

The age of the youngest patient was *nine* years; of the oldest, *thirty-five* years. The average age was a fraction over *twenty* years.

All the patients were from Ireland. *Six* had recently arrived in this country—i. e. within the space of five weeks. *Six* had lived in this country several months—i. e. between six and sixteen months. *One* had resided in this country *four*, and *one* five years. In one case the period of residence is not stated.

Two of the cases were admitted in the month of October; *four* during the month of November; *eight* in December; *one* in January, and none in February and March, my term of service ending with the latter month.

Access. The duration of the access was not ascertained in *seven* cases. Of the remaining *eight* cases, the attack was sudden in *four*; there was an access of a single day in *one* case, and of *two* days in *two* cases. The duration of the access here, as heretofore, is measured by the period elapsing from the commencement of illness, to the time of taking to bed.

Of *thirteen* cases in the histories of which the presence or absence of a chill, or of chills is noted, the access was ushered in by one or more in *twelve*. The chill was followed by perspiration in *seven*, and was not thus followed in *three* cases.

Pain in the head, back and limbs was complained of during the access, or at the onset of the disease in *nine* cases; in the head alone in *three* cases; in the head and limbs in *one* case, and in the back only in *one* case.

Of *thirteen* cases in which the presence or absence of vomiting during the

access, or at the commencement, was noted, it existed in *eight*, and was absent in *five*.

Diarrhoea is not noted to have been present during the access, or at the commencement, in a single instance.

Three of the patients stated that they had previously had an attack of Continued Fever.

Two of the patients were sons of the patient affected with *Typhus* fever, in which an imperfect relapse occurred, this case being that already referred to which was included among the cases characterized by relapse in the Second Report.

General Aspect. Passive capillary congestion of the face was noted in all cases but one, and in the excepted instance the congestion was active, in other words the face was flushed. The passive congestion was considerable in *three* cases, moderate in *eight* cases, and slight in *three* cases.

The eyes were more or less injected in *five* cases; in *one* case there was no injection; and with respect to the remaining cases nothing is stated relative to this point.

Yellowness of the conjunctiva existed in two cases. These were the only two cases in which this symptom was noted out of the forty-eight cases composing the collection upon the analysis of which the Second Report was based.

Nervous System. Of *fourteen* cases there were no manifestations of delirium in *five*, and more or less delirium existed in *nine*. The manifestations of delirium consisted in incoherency only, in one case. In the remaining eight cases there were efforts to get out of bed. In several instances, however, the delirium was of very brief duration. In three cases it was manifested only on a single night. In not a single case was this symptom at all prominent.

Deafness is noted but in *two* cases.

Digestive System. In *ten* cases there was nothing like diarrhoea. Of the remaining *five* cases the evidences of diarrhoea consisted in *one* case of too frequent dejections for the first three days, the patient having taken a dose of salts the night before entering the hospital; in one case the dejections were too frequent on the fifth, sixth and seventh days; in two cases this was the fact on a single day only. In but one case was diarrhoea a symptom of any prominence. These results are remarkable when it is considered that *ten* of the cases were embraced in the *Typhoid*, and none in the *Typhus* group. More or less *meteorism* existed in *nine*, and this symptom was absent in *six* cases. The meteorism was moderate in *four*, and slight in *four* cases. In *one* case only was it considerable.

More or less *abdominal tenderness* was present in all but three cases.

There was tenderness in the right iliac region in *ten* cases. In all of these ten cases the degree of tenderness was slight, and in several instances it was present only for a short period, in some but for a single day. *Tenderness over the epigastrium* existed in *six* cases. By reference to the Second Clinical Report it will be seen that of the forty-eight cases upon the analysis of which that Report was based, epigastric tenderness was noted in six instances. Thus all the cases in which it was present in that collection were characterized by relapses! The tenderness was universal over the abdomen in *one* case.

Vomiting is noted to have occurred in but three cases; this is exclusive of the access or commencement of the disease, to which reference, as to this symptom, has been already made.

Eruption. Each of the fifteen cases was unattended by an eruption. This result is certainly remarkable. Recollecting that ten of the cases were included in the cases of *Typhoid* fever upon the analysis of which my Second Clinical Report was based, it is interesting to refer to the facts contained in the section of that Report devoted to eruptions. It is there stated as follows: "The characteristic eruption was present in *twelve* of the *twenty-nine Typhoid* cases. This result differs from that obtained by the first analysis. The latter developed a larger proportion of cases in which this symptom was present, viz.: in *twenty-three*, of *thirty* cases; the ratio, thus, being *two-thirds*, while in the present collection it is less than *one-half*. I can offer no explanation of the above disparity."

If we were to deduct from the twenty-nine Typhoid cases in the second collection, the ten cases characterized by relapses, in which there was no eruption, the ratio of the cases in which the eruption was present would be nearly the same as in the first collection — the disparity with respect to this symptom would disappear; or, in another point of view, of the seventeen cases in the second collection in which no eruption existed, *ten* may be suspected to have been cases of Relapsing fever. The disparity in comparing the second with the third analysis, as respects the eruption, will be found to be even more striking.

Respiratory Apparatus. Epistaxis was noted in *six* cases. Pneumonitis existed in *three* cases. In two cases, spasmodic inspiration occurred. Aside from these facts, there was nothing worthy of note referable to the respiratory system.

Circulation. The average mean frequency of the pulse was a fraction over 75. The highest mean in any case was 108; the lowest, 69.

As respects the average mean frequency, it was considerably less in these

cases than in the *Typhoid* cases in either collection; and the disparity is still greater when compared with the cases of *Typhus*.

Skin. Sweating occurred in ten of twelve cases, the histories of which contain information on this point. The sweating was profuse in *eight* cases. It was noted but once in nine cases, and three times in the remaining case.

In each of the cases in which sweating occurred, it took place at a period more or less remote from the date of the first or second convalescence.

Moisture of the surface, i. e., a degree of perspiration less than sweating, was noticed in twelve of fourteen cases. It occurred on one day in *six* cases, twice in *two* cases, and on several days in *four* cases.

Both sweating and moisture, thus, were present in a large proportion of cases; but, it is a point to be noticed, that they did not occur as critical events.

Duration. The average duration of the febrile career, from the time of taking to the bed to the date of the first apparent convalescence, was about *nine* days. The longest period was *twelve* days, and the shortest *six* days.

The average duration of the interval between the date of the first apparent convalescence, and the recurrence of fever, was precisely five days. The longest interval was *eight* days; the shortest *three* days.

The average duration of the second febrile career was *six* days. In one case, it is noted that the relapse of fever lasted only two days. Exclusive of this case, the shortest period was *four* days, and the longest *ten* days.

Mortality. All the cases terminated in recovery.

Sequelæ. Nothing coming under this head worthy of note is contained in the histories.

Concluding Remarks.

On reviewing the results developed by the foregoing analysis, and comparing them with the distinctive traits of *Relapsing fever*, as presented in the account preliminary to the analysis, the correspondence is certainly striking. The access was abrupt in one-half the cases in which this point was ascertained, and unusually short in duration in the remainder. In 5-14 of the cases, there were no evidences of delirium; and in all the cases, this, together with other head symptoms, was slight.

In 10-15 of the cases there was entire absence of diarrhoea, and in one case only was this symptom in any degree prominent or persistent. Vomiting was not a prominent feature, being present at the access or commencement in but 8-13, and afterward in but three instances. Iliac tenderness

was very slight in every case, and in six cases tenderness over the epigastrium, which is extremely rare in *Typhoid* and *Typhus* fever, was marked. Meteorism was considerable in but a single instance, and absent in several cases. The abdominal symptoms, thus, were absent or slight in a remarkable degree, unless the cases were of the *Typhus* type, and it is to be borne in mind that none of the cases were considered to be of that type. The chest symptoms, and the circulation offer nothing worthy of particular note, except that the frequency of the pulse was considerably less than is stated to belong to the history of *Relapsing* fever. Sweating and moisture occurred in a very large proportion of the cases, in this respect presenting a contrast with the average occurrence of these symptoms in *Typhus* and *Typhoid* fever; but differing from other observations with respect to *Relapsing* fever in the fact that these symptoms did not occur at the time of the temporary or permanent convalescence. Mild jaundice, a very rare event in *Typhus* or *Typhoid* fever, was present in two of the fifteen cases. But the most striking of all the points of correspondence relate, first, to eruptions, and, second, to the relapses. In none of the cases was there an eruption, a fact which would certainly be very remarkable with respect to the same number of cases of the *Typhus* or *Typhoid* type occurring successively. The circumstances pertaining to the relapses, viz., the duration of the first febrile career, of the interval, and of the second febrile attack, accord with the previous description of these events as they are stated to occur in *Relapsing* fever. Finally, the absence of a fatal tendency is exemplified by the fact that all the cases ended in recovery.

In view of these facts the conclusion seems unavoidable that the cases of fever characterized by relapses, among those which came under my observation in 1850—51, presented the distinctive traits attributed to *Relapsing* fever sufficiently marked to entitle them to be ranked in the class of cases which have been described by different observers as a peculiar form of Continued Fever. This conclusion does not necessarily involve the position that the traits distinguishing the cases authorize their separation as cases of an affection entirely distinct from *Typhus* or *Typhoid* fever. It of course follows either that this position is correct, or that the *Typhoid* or *Typhus* forms of Continued Fever occasionally exhibit, as peculiar modifications, the symptoms which are considered the diagnostic features of *Relapsing* fever by those who regard the latter as a separate form of the disease. I am not prepared to discuss the relative merits of these inferences.

ART. V.— *Case of supposed Retention and Development of the Placenta after Decomposition and Discharge of Fetus.* By S. MITCHELL, M. D., Cameron Mills, Steuben Co., N. Y.

Was called, April 9, 1852, to see Mrs. B——, aged twenty-five, nervous lymphatic temperament, full habit, good constitution, having enjoyed uniformly good health previous to the present difficulty, and borne two children.

She informed me that she became "epicente," about the last of July, 1851, and experienced the usual symptoms attending the early periods of pregnancy, such as suppression of the menses, nausea, vomiting, &c. The abdomen, she thinks, increased in size, about the same as it had done in her previous pregnancies. Sometime in November, as near as she could remember about the middle, she had a fall, which was followed by very great soreness across the abdomen and loins; but was attended with no hæmorrhage or pains denoting uterine contractions. She had felt no motion previous to the accident which must have occurred between the third and fourth months.

The soreness gradually subsided so that she was enabled in the course of two or three weeks to attend to her usual household duties.

The uterus slowly enlarged after this, and rose above the pelvis near the umbilicus, but she never felt any motion. Some three weeks previous to my seeing the patient, she commenced having a dark offensive sanguineous discharge from the uterus. This continued unabated up to the time the case first came under my observation. By a vaginal examination I found the os firmly closed, not even admitting the tip of the finger, and of a firm and healthy feeling except a slight abrasion, or ulcer, upon the anterior lip. As before described, the uterus reached to near the umbilicus, was flaccid, and quite tender upon pressure, and yet she had no febrile action, the patient only complaining of a constant feeling of lassitude and debility; the countenance rather anemic, still able to be about house.

Diagnosis. If I placed any reliance upon her statement that she became pregnant the last of July, and notwithstanding the accident that befel her, having had no hæmorrhage, or pains denoting uterine contractions by which the ovum could have been expelled, I must of necessity come to the conclusion that it still remained in the uterus, and that the injury was sufficient to suspend its further development, and yet not sufficient to cause its expulsion, consequently it had been retained, harmlessly enclosed within the membranes, until the period when the discharge commenced, when, undoubtedly, the membranes were ruptured, and a decomposition and gradual discharge of

the embryo commenced, giving rise to the dark and offensive evacuation before mentioned.

Treatment. After deciding that the uterus contained a fetus in a putrid, decomposing state, which it was making no efforts, by natural contractions, to rid itself of, the question arose in my mind which would be attended with least danger to my patient, to seek by artificial means to dilate the os uteri, and then excite uterine contractions, by the use of ergot, or wait the action of nature — "*the vis medicatrix naturæ*" — closely watching the case to guard against every unfavorable indication. I was induced to pursue the latter course by observing how little the system was suffering from the protracted sanguineous discharge.

The ulcer upon the os I touched with nit. silver, and directed the vagina to be injected with chlorine water several times per day, to correct the fetor; volatile liniment to be freely rubbed over the region of the uterus, and advised rest in the horizontal position, and to *wait*.

I saw the patient from time to time; the sanguineous discharge continuing unabated, but becoming much lighter colored and less offensive. The uterus slowly diminishing in size and becoming firmer and less tender to the touch; the patient complaining more and more of debility, countenance becoming more and more anemic, pulse more frequent and smaller, extremities inclined to be cold, and appetite much impaired, I had been for several days thinking of the propriety of abandoning this "*expectant*" plan of treatment and resorting to some more active measures to procure the evacuation of the uterine contents, when I was summoned in great haste April 29, 1852. I found the patient with strong uterine contractions occurring at frequent intervals, which expelled in the course of two hours what appeared to be a fleshy mass of a pyriform shape, in fact being a perfect cast of the uterine cavity, some five inches in length, and three inches its greatest diameter.

The mass I found to be hollow, its walls being made up of a placenta-like structure which was from one to one and a-half inches in thickness at the fundus, and gradually becoming thinner toward the apex, which was membranous. The membranous portion was ruptured, through which the discharge had evidently escaped. From the fundus there was a pyramidal fleshy mass suspended by its base, which nearly filled the cavity. It was soft and friable between the fingers, and of a nearly black color, denoting that decomposition was taking place. It had no appearance of an organized fœtus except its being covered by a smooth membrane which was also reflected over the interior of the cavity. This undoubtedly was once the fetus,

but its further development being suspended by the injury, it degenerated into this anomalous structure. After the evacuation of the uterus the patient became speedily convalescent.

The case is of interest to me by its proving that the placenta may continue its growth and development up to the full period of utero-gestation, even when the fœtus, which it is intended to nourish, is destroyed. The reader will observe that the time of the expulsion of the mass was just nine months after the time fixed upon by the patient as the period of impregnation.

ART. VI.—*Case of Abdominal Tumor, containing Hair, Teeth, etc.* By
T. R. HUNTINGTON, M. D., Perry, N. Y.

The following is a brief history of a case occurring in the practice of Dr. Wells, of Moscow, by whose request I offer it to the columns of the Journal, hoping it may be of some interest to your readers. Having seen the patient but once, I am mostly indebted to Dr. Wells for its history. The patient, a female, unmarried, æt. twenty-two, was of a healthy family, herself healthy previous to the past winter, and able to perform her usual household duties until the first of March last, when she began to feel quite indisposed, but called no medical aid until the 18th, at which time Dr. W. found her suffering from a dull pain low down in the right side and in the lumbar region, the circulation very rapid, the tongue and the secretions generally natural, the urine being scanty and a little turbid, the menses had been regular and normal, the last having occurred about the first of March.

At a subsequent visit soon after, he found the abdomen somewhat enlarged, and could feel through its parietes several hard masses which were mostly confined to the iliac regions. About this time, or a little later, his attention was called to a small tumor just above the clavicle, on the outer edge of the mastoid muscle, free from pain and tenderness, but which rapidly increased in size. The size of the abdomen, also, was rapidly increased by the growth of the hardened masses, as well as by an evident effusion into the peritoneal cavity, until on the 30th of April, the distension being so great as to threaten a speedy dissolution, by the advice of Drs. McIntyre, of York, J. Huntington, of Perry, Wells and myself, a trocar was introduced and about eight pints of serous fluid allowed to escape. A large tumor then falling against the walls of the abdomen was pierced by the trocar, but its contents were of too dense a nature to pass the canula, as only a small quantity of gelatinous fluid, intermingled with caseous matter, escaped. Previous to

this, different opinions were entertained by the medical advisers, in regard to the nature of the abdominal disease: some believing it to be mesenteric, but others regarding it as ovarian. All present at the operation, however, were agreed in pronouncing it ovarian. After recovering from the fatigue of the operation, the patient expressed herself somewhat relieved, but the abdomen rapidly became distended again, and the tumor in the neck so greatly increased as to occasion much difficulty in swallowing, and death supervened on the 5th of May. A post mortem examination was made on the 7th. On opening the tumor in the neck, it was found to extend as far as the upper lobe of the lung, to which it seemed to be attached, and its contents had about the consistency of softened brain, which they resembled, being mixed with an abundance of grumous blood. About twelve pints of serous fluid were then drawn from the abdominal cavity, and an incision made from the ensiform cartilage to the pubis, which exposed to view a dense, light-colored, lobulated mass, that seemed to fill nearly the entire cavity, pushing the viscera high up under the ribs, and greatly diminishing the cavity of the chest. On raising this mass it was found quite firmly adherent to the walls of the abdomen in several places, but it was attached to the left broad ligament by a flat thin neck. The uterus and right ovary were natural, but the left ovary was degenerated into this mass of disease, which, when removed, weighed eighteen pounds.

Upon an examination of the tumor, it was found to consist of many cysts, which contained a great variety of substances. Some of the larger were filled with serous or gelatinous fluid, mixed with caseous matter. Others contained an abundance of matter similar to that found in the tumor of the neck. Some of the smaller cysts contained an abundance of hair, while many others were found to contain bones and teeth.

No regularity of form or arrangement to the bones could be observed, but the teeth, of which between twenty and thirty were found, were more or less perfectly formed, and were found in groups of five or six, with their fangs impacted in bones. The walls of the cysts were in some places ossified, and cartilaginous, in others thin and membranous. Behind the peritoneum and just below the left kidney, another tumor was found, containing about a pint of matter, precisely similar to that found in the tumor of the neck, and in some of the cysts of the ovarian tumor. The lungs appeared healthy, although somewhat compressed, there being some fluid in the cavity of the chest.

ECLECTIC DEPARTMENT,
AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Treatment of Internal Strangulation of the Intestines.

By R. ROBINSON, M. R. C. S.

In the treatment of internal strangulation of the bowels, the following indications present themselves. 1. To open the bowels. 2. To subdue inflammatory action. 3. To support the strength. 4. To remove the obstruction.

1. *To open the bowels.*—As constipation is one of the most constant, and earliest symptoms, to relieve the bowels is the earliest indication and thus may be attempted,—*a*, by purgative; *b*, injections; *c*, the warm bath; *d*, nauseating remedies; *e*, bleeding.

a. Purgatives.—Great judgment is required in the selection of purgatives, as if they fail they do much mischief. Had I grounds for suspecting intestinal obstructions, I would give one full dose of calomel to liquefy the motions, and follow it up by drachm doses of sulphate of magnesia. If this did not succeed, I would rub croton-oil on the abdomen, or try one drop internally. Failing with these I would give up purgatives altogether.

b. Injections.—Purgatives failing to procure action from the bowels, recourse is naturally had to injections, which should, in the first instance, consist of the compound extract of colocynth, dissolved in water; but when a tympanitic state of the bowels occurs, turpentine, or the tincture of assafetida is to be preferred. So far, however, as my experience goes in these cases, injections avail but little; and I shall hereafter relate a case where seventy-two injections were administered without any apparent beneficial result.

c. The Warm Bath is a remedy which it would be very proper to use in the earlier stages; but, as the disease advances, the strength of the patient being much exhausted, would scarcely justify its trial. Fomentations, often useful, and always soothing, should be used in all cases, especially where there is pain.

d. Nauseating remedies, in a strong and robust individual, might deserve a trial. Of these, perhaps, the two best are tartarised antimony; and the tobacco injection: but I should not be very sanguine of obtaining success by their means.

e. Bleeding may be now and then resorted to in the earlier stages, in the robust and plethoric; but bleeding, carried to any great extent, I consider objectionable, for reasons to be stated hereafter.

2. *To remove inflammation* is a point to which particular attention should be directed; and, perhaps, there is but one thing of more consequence than this, for if it be allowed to go on unchecked, it may destroy the patient; and yet, if too vigorously attacked, (by reference to the cases it will be seen that, in some, it was entirely absent, and, in almost all, limited, and by no means.

severe,) the patient may sink from other causes. I think, therefore, that general bleeding will seldom be called for on this account; the application of leeches, fomentations, and the administration of small doses of calomel and opium, will be all that is required for removing or controlling peritonitis brought about by this cause.

3. *To support the Strength.*—I am very anxious to lay great stress upon this point, because I do not think it has been sufficiently attended to in practice; and I feel sure that patients have been largely bled, who would have had a better chance, had the vital fluid been less unceremoniously abstracted. If the bleeding be not sufficient to effect its object, either by opening the bowels by its depressing effect, or by removing the peritonitis by its antiphlogistic power, it must, if carried to a great extent, do infinite harm, as it will tend to depress the vital powers, already at a low ebb, and thus take away, I may say, every chance, either from the efforts of nature, or the resources of art. In the absence of fever, and where the stomach would allow of it, I would give, from time to time, barley-water and chicken broth in small quantities. Where there is great restlessness and want of sleep, and where I had failed in the use of purgatives, and determined no longer to administer them, I would certainly advise the administration of opium, as, by so doing, I should hope to tranquillize the nervous system; for nothing tends to exhaust the frame so much as long continued nervous irritability; and cases are on record showing the good that has been done with it occasionally. If I employed opiates, I should prefer solid opium to every other form,—first, as being more likely to be retained by the stomach; and secondly, as possessing a stimulating as well as sedative property, which, I think, would, in these cases, be decidedly advantageous.

4. *To remove the obstruction* is one of the most important and difficult questions connected with this subject, and one upon which much difference of opinion exists. It is clear that this can only be done with certainty by means of an operation; and two cases have lately occurred, in which encouragement has been given to this plan. In one of the cases (Mr. Hilton's,) the bowel was so far liberated, that the intestinal contents passed through the obstructed part. In neither of the cases, however, did the patient long survive. In considering this subject, four points are to be entertained: A, the likelihood of finding and removing the obstruction; B, the place of performing the operation; C, the time at which it should be undertaken; D, the chance of success that may attend the attempt.

A. *The likelihood of finding and removing the obstruction.*—Upon this point there can be no doubt that there is great uncertainty; for although, in both the cases to which I have alluded, the obstruction was detected and removed, yet in one (Mr. Hilton's case,) very considerable difficulty was experienced; and in case XVI, had an operation been undertaken, the incision would, in all probability, have been made to the *right* of the umbilicus, as a decided induration was evident there, and not elsewhere, and a pouch immediately above it. It was natural, therefore, to suppose that this was the point of obstruction; whereas, on dissection, this proved to be merely hardened cecum; and the fatal incarceration was situated in the upper and posterior part of the *left* side of the pelvis.

B. *The place of performing the operation.*—Mr. Phillips says: "There are some cases where the seat of obstruction is so clearly indicated, that no doubt remains. In such cases, I apprehend, the rule is evident,—the incision

should be made as near as is prudent to that point. But supposing the point of obstruction to be only obscurely marked, or indeed not discoverable at all, then I consider the incision should be made on the median line, because an opening in that situation may be found most convenient for liberation, if that be practicable; or, for the establishment of an artificial anus, supposing liberation of the intestine be not accomplished." The case, however, to which I have just alluded, induces me to think the central incision, as recommended by Dr. Crisp, preferable in all cases where the obstruction is seated in the small intestines.

c. *The time at which an operation should be undertaken.*—It is very difficult to lay down any decided rule for this. Mr. Phillips says, that interference by surgical operation is justifiable when three or four days have passed without any relief from the bowels by ordinary means, providing constipation be complete, and fæcal vomiting continue. I scarcely think we are justified in operating so soon; partly because persons have recovered from constipation of twenty-three days' continuance, and partly for reasons which will appear in the sequel.

d. *The chance of success* I regard as a very important consideration before undertaking any operation; and I cannot think that cases of internal intestinal obstruction, even under favorable circumstances, offer much chance of a successful issue from operation.

In the cases to which I have referred, it has been said, that, had an operation been performed earlier, a different result might have been obtained; and, no doubt, in both cases, delay was caused by the unwillingness of parties interested to give their consent. Can that be a matter of wonder, when it is recollected, that if an operation be undertaken, both the patient and surgeon must be prepared to go *all* lengths? The obstruction may *not* be at the part suspected, it may be *some way* from where the operation was commenced, and a *very large abdominal section* may be required to complete the operation; and it may possibly not be completed at all. How can this be undertaken without very considerable risk? How can such an operation be *proposed early*? And how can any better justification be urged for such a proceeding, than that long since advanced by Celsus: "Satius est anceps remedium experiri quam nullum?"

If it be true, that great difficulty attends the finding of these obstructions, and great danger follows the attempt at removing them by the knife, so that we cannot conscientiously recommend it but as a forlorn hope, it is but reasonable to inquire, whether any other expedient can be resorted to for a similar purpose. And this naturally induces me to ask whether nature ever produces a cure, or whether spontaneous relief is ever obtained? Several are on record, where persons with the symptoms I have described, and, to all appearance, sinking from internal obstruction, were suddenly relieved in the bowels, and gradually recovered. A case is alluded to by Mr. Cooper, where this happened. An elderly lady, residing at Norwich, was under the care of Mr. Colman, suffering from constipation of the bowels, having had no evacuation for four days. The usual purgative remedies were prescribed, but without effect; enemata and more drastic cathartics were tried, but still ineffectually; vomiting and immense distension of the abdomen supervened; the symptoms became more and more urgent, and on the twelfth day from her attack she had had no relief from her bowels. Dr. Alderson was then called in, and asked *what purgative* he would recommend; to which he

replied—"None; but a large dose of opium." It was given, and *in a few hours* the bowels were freely opened, and the patient recovered.* What was the precise condition of this lady must ever be open to doubt; but I venture to suggest, that this might have been a case where the bowel was strictured by false membrane, that inflammation and ulceration of this band ensued, and that then the gut was liberated; and I feel convinced that, had the patient lived a little longer, it would have entirely given way, and the patient might possibly have recovered. It is this conviction which makes me lay so great stress upon keeping up the strength of the patient; for as newly-formed parts are less organized than those formed originally, there is a hope, if the strength of the constitution be kept up, that the band may give way before the bowel, and the patient's life be saved. It is from this case that I particularly recommend the renewal of the old plan of metallic mercury; I think its use has not been rightly understood. That it will remove an intromission, or enable a portion of bowel to be drawn out of these bands, I agree with Mr. Hilton, is not at all likely to happen; but that it might in favorable cases (Mr. Hilton's was not one of that sort,) by exciting a pressure upon the bowels; break through a false band, I verily believe; and I am more strengthened in this idea by the good that has occasionally followed its use. I have heard of a case, which I believe was of this kind, where metallic mercury appeared to remove a very obstinate constipation, and the patient recovered; and my friend Mr. Lawrence, of Brighton, has mentioned to me, and kindly allowed me to make public, the two following instances, which are cases in point. In one, a boy, *æt.* 10, was seized, without any apparent cause, with constipation of the bowels, but with no sign of inflammation. He was bled, leeches, took drastic purgatives, and had seventy-two clysters administered. On the twenty-first day of the disease, no motion having been procured, ℥ij of metallic mercury were swallowed; no effect following, the same quantity was repeated on the twenty-third day, after which he felt great weight and pain in the abdomen, and voided with much forcing, an immense quantity of *fæcal* matter, and all the mercury, minus ʒss; almost fatal syncope followed, but the boy eventually recovered. The other was a case of similar kind, of shorter duration; it occurred in an elderly lady. All purgatives proving unavailing, two doses of metallic mercury, of ℥iv each were given; several motions (and all the mercury, minus ʒj) followed its exhibition after six hours, but the exhaustion and the depression occasioned, were such as to destroy the patient. For these reasons, I think metallic mercury again worthy of a trial; it *can* do no great harm, and *may* do good. That it will often fail, I have not the least doubt, especially where the obstruction is low down, and has been so great as to ulcerate or destroy the coats of the bowel; but where, on the contrary, the band is thin, and high up in the canal, where the constriction is not so great as seriously to engorge or injure the part constricted, where the system does not sympathize much with the local malady, and where the powers of life remain vigorous, I am not without hope that it may *occasionally* succeed; and if *but one case* should occur in which, from what I have said, a trial of this remedy should again be made with success, I shall consider my observations not altogether out of place, and I hope I may with truth be permitted to say—"Est quiddam prodire tenuis si non detur ultra."—*London Jour. of Med.*

A Case of Poisoning with Oil of Tansy—Death at the end of three hours and a-half—Quantity of the Drug taken about ℥j and ℥iii. By JOHN C. DALTON, Jr., M. D. (Read to the Boston Society for Medical Observation, June 2d, 1851.)

E. S., a fine, healthy-looking girl, about twenty-one years of age, died at the house of Mr. A., in Boston, on Wednesday, the 7th of May, 1851. She had been employed in Mr. A.'s family as a seamstress since some time in the previous winter, living in the house during the week, but going away on Saturdays to a cousin's in Pleasant street, and returning to Mr. A.'s on Monday morning. She had been for some months receiving the attentions of a young man who was reputed to be engaged to her. None of her friends, however, suspected anything to be wrong with her until Monday, May 5, when her cousin, with whom she had been spending Sunday as usual, perceived the odor of tansy in the room which she had occupied; whereupon it occurred to her that the girl might have become pregnant, and used the drug for the purpose of producing abortion.

On Tuesday, she was engaged in her ordinary employment, and dined heartily a little after five o'clock in the afternoon. She went up stairs to her room about half-past nine o'clock. The cook, who occupied a room above, went up with her and stopped in her room, conversing, for some fifteen minutes. The girl's manner was perfectly natural and cheerful, as it had been throughout the day. About a quarter before ten o'clock the cook left her preparing for bed, and went up to her own room.

Nothing more was heard from her till about eleven, when Mr. and Mrs. A., who were sitting in the basement-room, heard a scream, which they supposed to come from one of the children. Mrs. A. went immediately up stairs, and on entering Miss S.'s room found her on the floor, by the side of the bed, insensible and in violent convulsions. She had evidently fallen out of bed, as she was undressed, and the bedclothes were disturbed, and had been partially dragged on to the floor with her. Dr. Morrill was immediately sent for, and arrived in about ten minutes. He sent also for me, and I arrived at the house at half-past eleven o'clock.

The girl was then lying on her back by the side of the bed, and presented the following appearances: Total unconsciousness; cheeks flushed, of a bright red color; eyes open and very brilliant; pupils of equal size, widely dilated and immovable; sclerotics injected; skin warm, not remarkable as to moisture; respiration hurried, labored, stertorous, and obstructed by an abundance of frothy mucus, which filled the air-passages, and was blown from between the lips in expiration; the breath had a strong odour of tansy, as had been already observed by Dr. Morrill; pulse quite full, forcible, 128; at intervals of five to ten minutes the body was convulsed by strong spasms, in which the head was thrown back, the respiration suspended, the arms raised and kept rigidly extended, and the fingers contracted. After this state of rigidity had continued for about half a minute, it was usually succeeded by a tremulous motion, often sufficient to shake the room, together with very faint and imperfect attempts at inspiration. The whole interval from the commencement of the convulsion to the first full inspiration, varied from a minute to a minute and a-half. Occasionally, the tongue was wounded by the teeth, and the

saliva slightly tinged with blood. Immediately after a convulsion the countenance was very pallid and livid, from the suspension of respiration, and the pulse exceedingly reduced in strength and frequency. The pulse and color then gradually returned until the occurrence of the next spasm. It was very common, a few seconds after the termination of a convulsion, for the head to be drawn slowly backward, and the eyelids, at the same time, stretched wide open. In the intervals of the convulsions, the limbs were mostly relaxed, but the jaws remained clenched.

A vein was immediately opened in the right arm, and about Oij of blood taken away. After this, the pulse became much softer and the face lost its bright color. There was, however, no change in the condition of the pupils, nor return of consciousness, nor other improvement in the appearance of the patient. It being impossible to get anything down the throat, two injections of an ounce of wine of antimony, with about ℥ss of powdered ipecac., were thrown up the rectum at intervals of about half an hour, but produced no apparent effect.

On searching the room, a ℥ij phial was found in the pocket of the girl's dress, wrapped in a piece of paper, labelled "Oil of Tansy," and marked with the name and address of an apothecary in Pleasant street. The phial contained ℥v of oil of tansy of the ordinary purity. A mug was also found, from which she had apparently drunk the oil mixed with water, as it smelt very strongly of the drug, and still had a drop or two of it at the bottom.

The condition of the patient continued much the same for about an hour. The convulsions, however, gradually became less protracted, and the failure of the pulse after each attack, more complete, at the same time that it recovered strength less perfectly in the intervals. The countenance also became somewhat sunken and the temperature of the skin reduced. About 1 o'clock, six leeches were applied to the forehead and temples, and sinapisms put on the calves of the legs. The leech-bites bled freely.

Toward 2 o'clock the alteration for the worse became quite rapid. Pulse 124 and feeble; respiration 36, and attended with less muscular effort than at first; the left cornea was glazed, but the right continued brilliant; a little inward strabismus of the right eye, and the mouth and nose drawn a little to the right side. Occasionally, a slow, lateral, rolling motion of the eyeballs. At five minutes past two she had the last convulsion, which was much less violent than the earlier ones, and lasted only half a minute. There was no recovery of the pulse after this attack, and she died at a quarter-past two o'clock, A. M.

Autopsy ten hours after death.

Countenance natural; cadaveric rigidity very strong; only slight purplish discoloration of dependent parts; no ecchymoses anywhere; no effect had been produced by the sinapisms on the legs.

Head.—Scalp not injected; distinct, but not excessive dryness of arachnoid over hemispheres of brain; no effusion, congestion or other unnatural appearances anywhere about encephalon.

Chest.—Heart and pericardium natural; left ventricle firmly contracted; blood everywhere unusually fluid; interior of heart exhaled a distinct odor of tansy, as also cut surface of pectoral muscles.

No alteration of pleura; lungs rather shrunken, crepitated perfectly everywhere, and were not at all engorged; air-passages not remarkable except for a little redness of posterior surface of epiglottis.

Abdomen.—Strong odor of tansy in peritoneal cavity; a few drachms of thin fluid in pelvis; peritoneum natural in appearance.

Esophagus natural internally, except that epithelium was somewhat deficient in lower part.

The stomach contained about ℥xij of a semifluid, yellowish-gray substance, consisting of partially digested food, potato, cranberries, onions, &c., mixed with an abundance of small brownish-yellow, glistening oil globules, and exhaling an excessive odor of tansy; mucous membrane generally pale, not vascular in any part, but throughout nearly the whole of great pouch brownish and much thinned and softened, so that for a considerable space it is nearly or quite destroyed. There was an old, whitish, slightly puckered cicatrix of the mucous membrane on posterior wall of stomach, near small curvature. No other morbid appearance.

The lacteals of the mesentery were very distinct, and distended with milky chyle.

Small intestines were natural internally throughout. They contained, at their upper part, pasty masses of dusky-colored chyme, mixed with oil of tansy.

Below, the contents were less abundant, and were unmixed with oil.

Large intestine contained yellowish feces, and small masses of a brownish powder, apparently ipecac. Mucous membrane natural.

Spleen rather shrunken, flabby, and deficient in blood. Other abdominal organs not remarkable except for slight paleness.

Urinary bladder contained ℥ii to ℥iii of urine.

The uterus was enlarged, so that its upper edge came two and three-quarter inches above level of symphysis pubis. It contained a well-formed female foetus, about four months old.

There was not the least appearance anywhere of the foetus or membranes having suffered any disturbance.

The left ovary, which hung down a little lower than the right, had near its external extremity a small conical prominence, where the fibrous coat was wanting, and its place occupied by peritoneum alone. There was a very slight appearance here of a cicatrix, visible only on close inspection. There was no unusual vascularity here, or at any other part of the ovary. Beneath this prominence the corpus luteum could be felt through the ovarian tissue, tolerably firm and well defined, and having the form of a sphere, compressed laterally, much like that of the crystalline lens. On dividing the ovary longitudinally through the prominence, the corpus luteum was exposed. It presented a nearly circular section, measuring seven-eighths of an inch in its long diameter, and three-fourths of an inch in its short. It consisted externally of a convoluted wall, of a dull-yellow color, measuring at its deepest part a little over three-sixteenths of an inch in thickness. The space enclosed by the yellow wall was occupied by a colorless, reticulated, fibrinous coagulum, which possessed a few minute vessels. This central coagulum was much compressed laterally; so that, although it presented a cut surface of about half an inch in diameter, it had hardly more than one line in thickness. There was no cavity nor fluid anywhere.

Both ovaries were carefully divided in every direction, but only one other body was found having any resemblance to a corpus luteum, and that was so small and imperfect as to be hardly recognizable. There were many Graafian vesicles in the interior of each ovary, varying in diameter from three-

sixteenths of an inch downward, but none at all prominent on the surface. Both ovaries were quite healthy.

It was subsequently ascertained that the oil of tansy was obtained, at the shop of the apothecary whose label it bore, on the evening of Friday or Saturday preceding the girl's death. The apothecary's clerk, who recognized the bottle, testified at the inquest that he put up in it ζij of oil of tansy, and delivered it to a girl about fourteen years old, who stated that the family that sent for it wished to take it into the country. The patient, therefore, undoubtedly took ζi and ζiij of the drug. It seems probable that the violent action of the poison commenced at eleven o'clock, at the time the family heard the patient scream; and if we allow fifteen minutes for the absorption of the oil after it was swallowed, it would give three hours and a-half from the time of taking the drug till the patient's death. Fifteen minutes may seem rather a long time for the operation of a volatile oil to be delayed, but it is probably no more than should be allowed. In a case which recently came under the notice of Dr. Dalton, of Lowell, a girl took a quantity of oil of tansy just before dinner. She then went into the dining-room, sat some time at the table, ate with apparent relish, felt sick, left the table, went into the yard, vomited what she had eaten, and immediately fell down insensible and convulsed. She recovered, after remaining a long time unconscious. The only other recorded fatal case of poisoning with this oil that I am acquainted with also occurred in Boston, under the care of Dr. C. T. Hildreth, and was published in the *American Journal of Med. Sciences* for May 1835. In that case the woman took ζss of the drug, and did not lose consciousness entirely till three-quarters of an hour afterward, although she was convulsed at intervals before that time. After unconsciousness became complete, she did not again recover it, and died rather less than two hours after taking the poison.

The present case is another instance of the extreme violence to which the system may be subjected even in the early months of pregnancy, without inducing abortion. Though all the muscles, both of the body and limbs, were for three hours and a quarter subjected to a succession of the most violent contractions, there was no sign of abortion, and after death the ovum was found in the uterus entirely undisturbed. In Dr. Hildreth's case, also, pregnancy existed but a few weeks advanced, and the drug was undoubtedly taken for the purpose of producing abortion, but nothing of the kind took place. The general symptoms in that case were similar to those described in the foregoing, the most remarkable difference being the more gradual loss of consciousness, and the more rapid death after a much smaller dose.—*Am. Jour. Med. Sciences*.

Poisoning by Oil of Tansy. By W. W. ELY, M. D., of Rochester, N. Y.

The subject of the following painful occurrence, was a respectable young lady, in ordinary health, engaged at the time in teaching school. Having arrived at her menstrual period, she procured what she supposed was the essence of tansy, designing to take it to promote the catamenial discharge. On the evening of August 15, 1836, she took *one teaspoonful* of the medicine, which proved to be *oil of tansy*. From the speedy supervention of

alarming symptoms a messenger was sent for me, a distance of two miles. Being unable to attend personally she was promptly visited by my partner. The oil, however, had operated so energetically and rapidly that on his arrival nothing seemed likely to be of any avail, and nothing of any consequence was done.

From the record which I made at the time, it appears that she first complained of dizziness and became insensible in about ten minutes—a succession of convulsions supervened, with frothing at the mouth, laborious respiration and irregular pulse, and she died in *one hour and a quarter* after taking the oil.

It may be proper to add that another young lady in the family, also took of the medicine at the same time, but vomited very soon, and suffered no inconvenience.

Kouso on the Tape-Worm. By J. A. FRYDINGER, M. D., New Orleans.

In compliance with a promise made to you some time since, I now give you the result of my experience with the new remedy for Tape-worm—Kouso. I have used it in two cases only, which I will relate as briefly as possible.

The first was a young gentleman, about the age of twenty-one years, a resident of this city. He had suffered from it from childhood, and had been treated for it by a number of physicians, both in England and in this country, previous to my acquaintance with him. He states that no treatment heretofore used had afforded him any but partial relief, and at last would mitigate his suffering only for a few days. The entire catalogue of anthelmintics had been thoroughly tried in his case, particularly the terebinthinate preparations, and pushed to an extent, in several instances, to seriously affect his general health, and as he states, in one or two, to endanger his life; still the animal was not destroyed and his sufferings continued.

I have treated him for the last three years, at intervals, with varied results, but invariably gave him some relief for a time; at times he would discharge from a few separate joints to several hundred; at others, portions of the worm measuring in length from three or four inches to seventy or eighty feet; and I may here state, that since I commenced treating him, he has discharged over a *thousand feet of worm*.

In February last it annoyed him very much, and I determined to try the Kouso on him; accordingly, half an ounce was administered in water at bed time, followed in the morning by a seidlitz powder. In a few hours his bowels were moved, but no signs of worm or the Kouso could be discovered in the evacuations. During the following night the Kouso came away bringing with it a large quantity of the worm, broken, and torn, and mangled to such a degree, that it was some time before it could be recognized in the mass of matter. Such portions as could be selected from the mass, of sufficient size and form, worthy of preservation, can be seen at my office, with specimens of entire worm from the same person.

With a view of making it as certain as possible, and to give it a full trial, after the lapse of a week the dose was repeated; but this time it was premised by a dose of castor oil, for the purpose of emptying the bowels, so as to

give the remedy entire control of the alimentary canal, and to encounter nothing but the animal itself; but no traces of worm could be observed in the operations of the bowels; nor since the operation of the first dose. The young man is positive that the animal has been entirely destroyed.

The other and last case was also a resident of this city, a lady, about thirty-five years of age. She had passed portions of worm for fifteen or eighteen years. She suffered greatly from indigestion, derangement of the bowels, menses, etc., mostly all her life.

A half ounce of Kouso was given her about the first of March last, which brought away a large amount of worm, broken up as in the former case. In this case no cathartic medicine was given either before or subsequent to the administration of the Kouso.

Her digestion has since greatly improved; her bowels and menses have become regular; and she has gained considerable flesh and color; in a word her general health has greatly improved. She states she never was as well in her life before. So far she has had no return of the symptoms of the worm, and believes she is entirely relieved.

I am not prepared to give a decided opinion as regards the action of the Kouso upon the worm; but you will readily infer from the foregoing (the appearance of the worm as brought away by the remedy) that I am inclined to the opinion that it acts mechanically upon it; analogous to the *Dolichos Pruriens* on the *Lumbricoides*.

Upon reflecting on this method, after having made the promise, I came to the conclusion to defer this communication longer than was originally intended, for the reason that I wished some time to elapse, to see whether there would be a return of it in either of the cases, as I desired to give a statement that could, to some extent, be relied on.

Although several months have elapsed since the medicine was given, and no symptoms of the existence of the worm have reappeared, still I am not yet satisfied that they will not return. Should they return in either case, I will advise you of it.

In conclusion, I would say, that although not certain that this article will or can entirely eradicate the worm, and drive it from the system, I am satisfied that it is the most effective remedy I have ever tried in its removal, and I have used all the remedies I ever heard of, and have treated a good many persons suffering from Tape-worm. I would further add, that there is no danger in using it whatever; no unpleasant symptoms occurred in either of the cases; and did not even disqualify them for their ordinary pursuits.—*New Orleans Med. and Surg. Jour.*

Spiritual Writings.—The extracts under this head in our last number, occupied more space than we supposed they would, and as a consequence we were not able to conclude what we wished to say in regard to the *spiritual writings*; but our readers need not be alarmed—we shall not weary their patience much longer with our dissertation on this subject.

We have said that there appears to us to be a striking analogy between the condition of the nervous system which leads to these writings, and that which existed in the persons who were affected with the "jerks;" and some further facts which we have now to add, will, we think, render this still more

apparent. Thus, while this singular affection was not confined to any class or sex, but men and women, black and white, were its subjects, still it was observed that women were much more apt to fall into it than men; and it was also remarked that those who had once been seized were particularly liable to a second attack, and jerking or swooning readily became a habit. "Women," it is stated, "had their nerves so weakened by the frequency of these attacks, as to fall while walking to or from the meeting-house, engaged in narrating past exercises without any uncommon emotion, and drop from their horses on the road."

Many instances of this acquired habit of the nervous system are recorded by the writers of that period. Thus, Dr. Cleland, an estimable and pious clergyman, relates that riding one day with a lady, the wife of a Presbyterian elder, who had been some time previously affected with the jerks, it occurred to him to try whether they might not be renewed simply by starting a particular train of ideas in her mind. The conversation just before had been of an indifferent character; he changed it abruptly to devout and solemn subjects, and adds, that "before two minutes had elapsed, her body began to be violently agitated, pitching upward and forward, from the saddle half way to the horse's neck six or eight times in a minute."

There were those who struggled long and earnestly against the disposition to fall, but were forced to yield at last. One fell, after bitterly opposing what was esteemed a "divine work," and another, exclaiming that it was "an unfortunate sight and a great mortification." "One dropped, as if shot, just after expressing his fears that the work was not right." A father threatened his swooning daughters, that he would beat them if they ever came to such a place again, and fell with the words in his mouth. A man fell at Lexington, "who had told an acquaintance if he fell he might put his foot on his neck and keep him down."*

Not only were there these involuntary motions, the result of sympathy, but in many of the subjects there was also the unconsciousness and insensibility presented by the mesmeric state. Persons, to their great surprise, found themselves unable to move when they wished. One young lady is mentioned who was not aware of any change in her condition, and was amazed to find the people flocking around her; but then making an effort to move she found herself powerless. Some, while in this state, were both conscious and capable of conversing; others were speechless. The most energetic stimulants, as in artificial somnambulism, made no impression upon the sentient nerves. A phial of hartshorn was applied by a clergyman to the nose of a stout young man, who was lying flat on his back, and by accident some got into his nostrils; "but he took not the slightest notice of it."

On one occasion, Lorenzo Dow, while preaching in the court house at Knoxville, Tenn., the governor of the state being present, saw one hundred and fifty persons exercised with the jerks. At another meeting, where the excitement had risen to a wilder pitch, three thousand persons were reported to have fallen. The influence by which these strange manifestations were induced, as every one must be prepared to learn, was held by the multitude to be supernatural. It was esteemed, as we have said, a divine work, which it was hazardous and sinful to oppose. The subjects were often in an ecstatic state, and had visions and revelations. They saw dazzling light such as

* Davidson's History Presbyterian Church in Kentucky.

they could not behold. "Two women," says a historian of the times, "have fallen into trances, and one has passed a golden bridge to heaven; the other has been in heaven," &c. &c.

No doubt there were sensible and discreet men, probably physicians, who believed that these people were in communication with the spiritual world. No one believes so now; and yet the "spiritual writers" may be defied to bring out anything more marvelous than the phenomena afforded by the "jerks." These things—mesmerism, the jerks, spiritual writing, all in them that is not fraud and deception—belong, then, in our judgment, to the same category, and have their origin in a peculiar perverted state of the brain. It is a state easily induced in some individuals, while others are capable of resisting it. The subjects of mesmerism are found to be the apt spiritual writers, as the believers in clairvoyance are those who yield the readiest credence to its being the work of spirits.

We do not deem it worth while to write against the thing. It *will* have its day. The populace will be carried away with it; some will lose their senses, and commit crimes, or get into mad-houses; and then, after a time,

"—— all this derision
Will seem a dream, and fruitless vision."

As an apology for the length to which we have extended our remarks upon this subject we have said, that the miserable superstition has ended, in not a few instances, in deplorable insanity. In this city we already hear of persons who are fully persuaded that they hold daily converse with the spirits of their departed friends, and one young man is understood to have been impelled to suicide by these spiritual writers. We may not be able to arrest the delusion, but it ought, at least, to be exposed.—*Western Journal of Medicine and Surgery.*

Erysipelas: Treatment of, by Muriated Tincture of Iron. By HAMILTON BELL, Esq., F. R. C. S. E.

The author's intention being solely practical, he does not enter minutely into the pathology of erysipelas; but in order to explain, in some measure, the principle by which he was actuated, in employing a powerful tonic in a disease accompanied with so much fever and excitement, he records his opinion, that in inflammation, the capillary vessels having apparently lost the power of separating or electing the component parts of the blood which are necessary for functional purposes, and become, to a certain extent, inert tubes, a stream of blood is admitted for the circulation of which they are not calculated.

The treatment which he has adopted for many years, without failure in a single case, is the exhibition of the muriated tincture of iron. The first object is to have the bowels freely acted upon. If the disease be mild, fifteen drops are given every two hours; if more severe, twenty-five to thirty drops, persevered in night and day, whatever be the degree of fever or delirium. His only local applications are flour or cotton-wadding. Cases are appended in illustration.

The brother of the author, Dr. C. Bell, confirms the favorable report above given. He says he has given it in the idiopathic form, and in that consequent on injury, with equally good results, and he has found it useful at all ages, from infancy to advanced age.

So beneficial is this treatment in erysipelas, that Dr. C. Bell expresses his conviction that if boldly given in puerperal fever, many lives would be saved.—*Edinburgh Monthly Journal*.

Case of Acute Laryngitis. By DIXIE CROSBY, M. D., Prof. of Surgery and Obstetrics in Dartmouth College.

There are two distinct forms of this disease recognized by pathologists, viz., mucous and submucous. A case of the mucous variety having occurred recently in this vicinity, possessing more than common interest on account of its progress before medical aid was called, and the unexpected recovery of the patient, I send it for publication in your Journal.

Mr. A. D., 30, was out in the rain, Dec. 31, 1851, from which resulted a "common cold and sore throat." These symptoms increased up to the time I was called, and found that another physician had just bled him some eight or ten ounces, which produced incomplete syncope. He was sitting erect with respiration so loud as to be heard in an adjoining room. Could scarcely articulate in a whisper loud enough to be heard. Had not swallowed for several hours, and refused to try. Confusion of ideas with dizziness, approaching stupor, and perspiration starting from every pore. Cannot protrude the tongue much beyond the teeth.

4, P. M. Four vigorous leeches were applied over the larynx. After they fell off, the throat was enveloped with hot wet cloths to promote the flow of blood. I made a solution of forty grs. nit. silver to the ounce of water, and against every disadvantage, had the good fortune to pass a sponge saturated with the solution into the larynx. Directed more leeches and promote the bleeding as before.

9, P. M. Breathes a little easier. Leech bites, bleeding freely. Pulse quick and feeble. Respiration audible in any part of the room. Passed the sponge as before. Pediluvium.

2d. 8, A. M. Rested but little. Breathes easier. Speaks in a whisper, and asks to have the sponge used again, which I did. Does not swallow any thing. Apply leeches, followed as before, by warm fomentatives, to be followed by cold after leech bites have ceased to bleed.

2, P. M. A little improved. Sleeps semi-recumbent.

9, P. M. General improvement. Has not swallowed, and refuses to try. Says the cold affected him unpleasantly, increasing the difficulty of breathing. Apply a blister on the larynx.

3d. 9, A. M. General improvement; has slept somewhat. Has a good blister. Thinks he will try to swallow a dose of castor oil. Says the sponge relieves him more than every thing else, and hopes I shall pass it again.

10, P. M. Improved in every particular. Oil operated well. Can lie horizontal. Swallows with difficulty; breathes without noise; voice husky.

℞ G. Arabic,	ʒss.
Nit. Potass.,	ʒii.
Ant. Tart.,	gr. iii.
Hot water,	half a pint.

Dissolve and give a table spoonful every two hours. *

4th. Has had a good night. Can swallow with increasing ease, and now for the first time thinks he will take some thin gruel, not having swallowed any food or drank since my first visit. From this time he convalesced rapidly, and is now, Jan. 20th, quite well.

The sponge used was fastened to a crooked whalebone handle.—*New Hampshire Journal of Medicine.*

Death while under the influence of Chloroform.—The following are the particulars, as learned from various and direct sources, of a melancholy case which recently occurred at Hooksett, in this state:

A girl, of about fifteen years of age, had a tumor upon the thigh, which was examined by Dr. Timothy Haynes, of this town, and its removal was advised and strongly urged. After some time the patient consented that it should be done, and a day was fixed for the operation. At the appointed hour Dr. H. arrived, attended by his student; but the patient was so much terrified in the prospect of the operation, and of taking chloroform, that she ran away and hid. After some time she was found, and with a good deal of force was brought to the house and the room, where the operation was to be performed. She entreated to be allowed to go, but still more, that she might not be obliged to inhale the ether; saying that she would bear the pain of the operation, but she knew she should die if they made her breathe it. The doctor, however, insisted upon her taking it, and she was held, and concentrated chloric ether was administered by her uncle, not a physician. An unusually large amount was required before she ceased struggling violently, but finally the operation was commenced, and almost at the same time the patient was found to be exceedingly prostrated. The tumor was removed, and the doctor exerted himself to revive his patient, but in vain; she died in a very few minutes.

We regret that we cannot give the *precise* quantity of ether used and the *precise* time that elapsed between the commencement of the inhalation and the death of the patient,—but only one medical man was present, attended by a student, not at all advanced in his studies, and he proposes to “live down,” that is, wait till people forget the case, not to report it. The impression of friends in such minutæ, is perhaps, not perfectly reliable.

In view of these facts, shall we attribute the fatal result to the anæsthetic agent? We say at once, no. The patient was extremely terrified at the idea of the operation, but she was far more so at the thought of being rendered insensible. She entreated to be allowed to suffer the pain, reaffirming that she *knew* she should die if she breathed the ether. Under such circumstances, would she not have died if she had inhaled the vapor of water, believing it to be ether? Our own impression is that she would, and fright was in fact the cause of her death.

The errors in this case were, first, that the operator insisted upon the inhalation, or even *consented* to it. In fact, under such circumstances almost any man would have deferred the operation to a subsequent day, (there being no immediate danger to life from the tumor,) and then proceeded without placing the patient in a state of anæsthesia.

Second, that he allowed it to be administered by an unprofessional person, not at all acquainted with its use. An agent so powerful should be used with the greatest caution and skill, and no one should operate without placing the patient under the charge of a reliable physician, so far as this is concerned. The operator should be at liberty to devote all his attention to the operation, and not be distracted by watching the influence of the anæsthetic.

We regret that by this case, discredit has been cast upon a most useful agent, and difficulties thrown in the way of its use. The following sentence from Professor Gilman's preface to Beck's *Materia Medica*, exactly expresses our views of the use of anæsthetics:

"Used with constant care, watched with unceasing vigilance, they are safe and most beneficent agents;—used rashly and thoughtlessly, they are so dangerous, so almost certainly fatal to life, that such use of them involves, in my judgment, an amount of moral guilt little short of that which attaches to manslaughter."—*New Hampshire Journal of Medicine*.

Spencer's Objectives for Microscopes.—The pre-eminent success of Chas. A. Spencer, residing in Canastota, Madison county, New York, in manufacturing objectives for microscopes, deserves a notice in this place. It is now fairly conceded that Spencer, though an American, has considerably excelled the best English and European opticians in this most difficult department of practical optics. The American Association for the Advancement of Science have given him this award, and English microscopists have borne testimony to the same effect.

The preceding observations were made with a glass of exquisite workmanship, one of Spencer's latest and best productions. I gave him an order for the finest objective of high power which he could make, expressly without limit as to price. He sent me the instrument [the essential parts of which the smallest thimble would contain] in May, 1852, writing me at the same time that it was the best he had ever made, and charging me for it, what I consider a most moderate sum, \$120; for its defining power is so great and so wonderfully accurate, that a sum of money greater than I choose to name, would not deprive me of its possession. It is rated by Spencer as one-sixteenth of an inch focus, though the available working focal distance is probably less than 1-200th of an inch, requiring the very thinnest of Chance's thin glass, for covering objects to be seen. Its angle of aperture is full 174°! a figure at least forty units beyond what the best European opticians have, until quite lately, considered practicable. Upon this, as well as upon the general perfection of workmanship, its great excellence depends. That most beautiful test object, the *Grammatophora subtilissima*, of Bailey, is, by this glass, readily and clearly resolved into black beaded lines.—*Dr. J. L. Riddell, in New Orleans Med. and Surg. Jour.*

Leucocythæmia.—Prof. G. B. Wood called the attention of the college to a form of disease described by Dr. Bennett, of Edinburgh, under this name, the leading characteristic of which is an excess of the white corpuscles of the blood. He stated that there was a case of it in the Pennsylvania Hospital. The patient, a male of about seventeen years of age, was admitted laboring under symptoms of anæmia, with some anasarcaous effusion, general debility, and great enlargement of the spleen. The blood was examined by Dr. Adenell Hewson, resident physician of the hospital, who is accustomed to microscopic investigations, and found by him to contain a great excess of white corpuscles. There could be no doubt that it was a case of the leucocythæmia of Dr. Bennett. The patient was put upon the use of iron and quinia, with a good diet. Under this treatment, the dropsical symptoms and enlargement of the spleen rapidly diminished, and the patient soon became restored to a degree of robustness, which was in strong contrast with the debilitated appearance he presented at his entrance into the institution. The spleen was evidently diminished in bulk; but at the same time the liver was found to have become enlarged. On examining the blood under the microscope, it still exhibited the same excess of white corpuscles. Dr. Wood supposed that the patient had been overstimulated by the treatment to which he had been subjected. A less invigorating diet was directed, small doses of blue mass were administered, and a blister was applied over the right hypochondriac region. Under this treatment the condition of the liver became improved; but, as the symptoms of anæmia reappeared, and the spleen began again to enlarge, a blister was applied over the left hypochondrium, and nitro-muriatic acid was substituted for mercury. The visceral disease now rapidly diminished; but as the anæmia continued, recourse was again had to chalybeates. All the symptoms now improved; and an examination of the blood showed a very considerable diminution in the number of white corpuscles. When the patient was last seen by Dr. Wood, the spleen was of nearly its natural dimensions, and the anæmic symptoms had almost disappeared; but the liver still remained somewhat enlarged.—*Quart. Sum. Trans. Coll. Phys. Philadelphia, Jan. to April, 1852.*

EDITORIAL DEPARTMENT.

Medical Department of the University of Buffalo, Session of 1852-3.— On reference to the advertisement (see advertising sheet, cover) it will be perceived that the Faculty of this institution have made some changes in the commencement of the terms, the fees, etc., for the Session of 1852-3. The preliminary term, devoted mainly to the study of practical anatomy, and hospital instruction, will commence, for the next session, on the *fourth Wednesday (24th) of November*, and continue to the opening of the regular term. The regular term will commence on the *first Wednesday (5th) January, 1853*, and continue for the usual period, viz., four months. This change of time has been deemed advisable for the present collegiate year, and it is believed that it will meet the convenience and wishes of medical students, as well as, if not better than the former arrangement. A considerable number of students heretofore have found it difficult to attend so early as the month of October, or even to enter at the beginning of the regular term in the early part of November. The postponement adopted for the present year will accommodate those who are unable to come at an earlier date, and will afford advantages to those with whom the time of commencement is a matter of indifference, not less than if no change of time had been made.

In view of the course pursued by other institutions, with which, from their geographical situation, the school at Buffalo is necessarily brought into competition, the Faculty have resolved to adopt, for the next session, a change with respect to fees. The aggregate fees for the tickets of the seven professors will be *fifty dollars*; and in particular cases, a note, satisfactorily endorsed, upon interest, will be received. The latter new feature in the management of the institution is adopted, for the present collegiate year, for the same reason that the reduction in fees has been made.

The only change in the members of the Faculty that has taken place, is the resignation of Prof. Palmer, in consequence of his having accepted an appointment in the University of Louisville, and his engagements there and at Woodstock being such as to prevent his giving the anatomical course in the school at Buffalo. The well known ability of Prof. P., and his high reputation as a

teacher, render the necessity of his resignation greatly to be regretted. The Faculty, however, have been fortunate in securing the services of Prof. E. M. Moore in that important branch of medical instruction. Prof. Moore has for many years been a teacher of anatomy and surgery, holding the latter chair in the medical institutions at Woodstock, Vt., and Pittsfield, Mass. His reputation and experience furnish an ample guaranty that the department of anatomy will be thoroughly and efficiently taught.

Since the last session the subject of morbid anatomy has been added to the chair of Physiology, and will be taught by Prof. Dalton, the incumbent of the chair. Prof. Dalton, immediately after the close of the last session, sailed for Europe, and is passing the summer at Paris, prosecuting investigations relating to the departments of instructions assigned to his chair. He will return by the commencement of the regular term.

With the present organization of the school; the admirable opportunities for clinical hospital instruction which are placed at its disposal; the conveniences of the college edifice; facilities for practical anatomy, etc., there is every reason to look for its continued and increasing prosperity. So far as a judgment can be formed in anticipation, the prospect is that the class for the next session will be large.

It may be proper to add, that the professor of the principles and practice of medicine will give the course in that department during the next session. This notice seems to be required in consequence of the incumbent having accepted an appointment in the University of Louisville. The course will be given, at Buffalo, during the last half of the term.

A Treatise on the Diseases of the Chest; being a Course of Lectures delivered at the New York Hospital. By JOHN A. SWETT, M. D., Physician to the New York Hospital; Member of the New York Pathological Society. New York: D. Appleton and Company, 200 Broadway. 1852. 8vo. pp. 585.

Some ten years since, a series of lectures on the diseases of the chest, by Dr. Swett, were published in the *New York Lancet*, a weekly medical journal, issued at that time at New York, which has since been discontinued. They were received with marked favor by the medical profession, and served at once to establish the reputation of Dr. Swett as an able practitioner and teacher in the department of physical diagnosis. Upon these lectures the treatise, the title-page of which is prefixed, is based, the author yielding to the repeated solicitations of his medical friends to publish them in a separate

volume. In the present republication not only has the subject been revised, but the experience of ten years of hospital and private practice has been added. The author states that during most of his professional life he has kept a register of the most important cases of chest disease that have fallen under his notice, and the materials therein contained have been freely used in the preparation of this work. "The statements," to quote the language of the preface, "are founded upon registered and carefully observed facts, rather than upon vague recollections." This recommendation of the work will be duly estimated by those who appreciate the wide difference between the loose hap-hazard assertions of those who draw their conclusions from recollected experience, and the results deduced by a rigorous analysis of recorded data. The former originate and give currency to a host of false facts to be slowly and with difficulty corrected by the labors of the careful observer and analyst. In an appendix to the treatise, Dr. Swett has given a translation from Lebert's work on pathological physiology, containing the discoveries of that distinguished microscopist in the structure of tubercle and of cancer, with well executed illustrations.

We have held this work in reserve for some time, designing to give it extended analytical review; and it is with considerable reluctance that we feel obliged to forego the execution of that intent. As an American, and we may add a New York book, it claims thus much at our hands, but we find that the design must either be abandoned, or indefinitely postponed, and it is an injustice both to the author and our readers that the work should continue longer to lie on our table unnoticed.

The scope and character of the treatise will be understood from what has been already stated. The name of the author is too well known to require aught from us on that point. We have only to add, that after a tolerably attentive examination, we would warmly commend it to the perusal and study of our professional brethren. In saying this, it is hardly necessary to remark that we do by no means hold ourselves committed to all the views contained in the work. This is a disclaimer which we often take occasion to repeat, in order to avoid misconception. In point of fact, while perusing Dr. Swett's treatise, having in contemplation a critical review, we had marked several portions respecting which we should venture to differ from the able author. One of these we cannot forbear referring to in this brief notice. Speaking of the physical signs of early tuberculosis, Dr. S. says with respect to percussion, "posteriorly no advantage can be derived from percussion; the muscles are too thick about the shoulders to admit of any decided advantage even in cases in which the pulmonary condensation is much greater than in incipient.

phthisis." (Page 258.) We are greatly surprised at this statement by so skillful an auscultator as we know Dr. S. to be. Our own experience leads us to a different opinion. We have not infrequently met with cases in which a marked disparity in the sonoriety of the percussion sound was apparent over the scapular region, above and below the spinous ridge, when the resonance was nearly or quite equal in the infra clavicular regions, and under circumstances in which the fact was an important physical sign in determining the presence of tubercle. If attention be paid to the pitch, as well as amount of resonance, we are almost prepared to say that the scapular region is but little less important than the infra clavicular in seeking by percussion for the physical evidences of tubercle. This is a matter of sense and tact in which, it is evident, the appeal must be, not to argument, but demonstration.

In treating of diseases of the heart we are gratified to find that the author simplifies the subject by including the different varieties of hypertrophy and dilatation under a single class, to which he applies the simple term, enlargement. This course renders the subject much less complicated for the student, a point of considerable importance with respect, not only to these, but all other forms of disease. In giving public instruction we have been led to pursue a similar plan. Having thus alluded to this part of the work devoted to diseases of the heart, we cannot avoid expressing surprise that the author omits to refer to the fact that pericarditis is occasionally masked by cerebral symptoms which, without knowledge of this fact, will be almost sure to lead the practitioner to regard the brain as the seat of the disease. Cases illustrating this important point have been contributed by Watson, Burrows, and others, and a striking instance was reported by the Editor of this Journal in the April No., 1850. An instance somewhat similar has more recently fallen under our observation in which the diagnosis of pericarditis was predicated on sudden death taken in connection with the character of the cerebral symptoms, the previous history being unknown, and the patient dying before a careful examination of the case was made.

Assuming the validity of these, and other criticisms which we should presume to offer were we to enter deliberately on the task of a review, the merits of the work (which are generally left for the reader to discover without the aid of the critic) are such that, in mercantile phraseology, the balance in its favor should satisfy all reasonable claims of authorship.

The style of the work is lucid, but plain to a degree which sometimes seems to denote, not only neglect, but contempt for elegance of diction. We would not quarrel with the author on this score, and yet, we think attention to composition is not without its advantage even in a scientific production.

We should have preferred, also, to have had the work divided into sections, and chapters, rather than into lectures, with a corresponding adaptation of style to the former arrangement. We know not how it strikes others, but whenever we sit down to read a volume of lectures, we cannot divest ourselves of the idea that we are stationed behind the door, or in an adjoining apartment, and listening, somewhat clandestinely, to words addressed, not to ourselves, but to others.

In taking leave of Dr. S. we would express the hope that a second edition of this treatise will be speedily demanded.

A Treatise on the Practice of Medicine. By GEORGE B. WOOD, M. D., Prof. of the Theory and Practice of Medicine in the University of Pennsylvania, etc., etc., etc. Third Edition. In two volumes. Philadelphia: Lippincott, Grambo & Co., No. 14 North Fourth Street. 1852.

The success of this treatise, evidenced by the call for a third edition, shows, in the first place, the acceptableness of works of that description to medical students and practitioners. Their convenience and value, indeed, cannot be doubted; and yet, they are not without their disadvantages. Presenting a compendium of the contributions to science which are scattered among diverse publications, the reader is apt to be satisfied without going to the original fountains whence the authors derive a large share of their materials. So far as treatises on practice are compilations (which they usually are for the most part) the result just mentioned is an evil offsetting, in some measure, their good effects. Another evil is, that readers are apt to look to a popular practical work too much in the light of an authority, and to attach the same importance to the notions which the author may enunciate on his own responsibility, as to the opinions or doctrines which he appropriates from others. It does not require an argument to prove that a person may be well fitted to systematize and condense the knowledge which has been rendered available by the numerous laborers in the field of science, while his personal authority is entitled to but little weight. For the one, industry and tact are alone requisite; the other involves opportunities and faculties which do not follow as a matter of course.

But, in the second place, the success of Dr. Wood's work shows that, in the estimation of the medical public, it is well adapted to fulfill the objects for which it was prepared. We have heretofore expressed our opinion of its character. It bears internal evidence of care, and industry, and contains the

most approved modern views and doctrines relating to the various subjects embraced in practical medicine. As such it commends itself to the continued approbation of the profession. That the principles and rules of practice which the learned author inculcates are in all instances unexceptionable, we are by no means willing to admit. We are free to say that we find in the two volumes much that, as a practitioner, we cannot adopt; but after saying this, we are not inconsistent when we avow, in our critical capacity, the opinion that it is well adapted to serve as a text book and work of reference, in other words a repository of current facts and opinions, which deserves to be studied by the medical novitiate, and consulted by the practicing physician, but not to supersede special treatises, nor to be taken as a guide, to the prejudice of independent thought and investigation.

The present edition has been enlarged by the addition of nearly eighty pages. The author remarks in the preface that "the reader will find evidences of modifications of the treatise scattered here and there throughout the work. Among the additions may be mentioned brief notices of several diseases, which, from their rarity, or total absence in this country, or from being unknown as distinct affections, were not described in the former editions. Such are the relapsing fever of Dr. Jenner, the leucocythemia of Professor Bennet, the dengue, and certain cutaneous affections, as trichosis, lupus, and pellagra. The chief modifications, however, have reference to subjects previously more or less discussed, as, for example, to inflammation, fatty degeneration, carcinoma, epidemic cholera, the treatment of phthisis, the nature of hemorrhage, Bright's disease, etc."

Lectures on the Principles and Practice of Surgery. By BRANSBY B. COOPER, F. R. S., Senior Surgeon to Guy's Hospital, etc. Philadelphia: Blanchard & Lea. 1852. 8vo. pp. 771.

This volume embraces a series of lectures delivered before the pupils of Guy's Hospital, London. It does not profess to be a systematic work on the Elements of the Science of Surgery, but a compendium "in which the student may meet with a clear account of the practice of that science, established, not only on the author's own experience, but likewise upon the best acknowledged authorities." It is evidently a practical work, for which the large experience of the author, having been surgeon to Guy's Hospital for twenty-five years, must furnish abundant materials. As such it will commend itself to the surgical practitioner.

The Principles of Surgery. By JAMES MILLER, F. R. S. E. F. R. C. S. E., etc., etc., etc. Third American from the second and enlarged Edinburgh Edition. Illustrated by two hundred and forty Engravings on Wood. Revised, with additions, by F. W. SARGENT, M. D., Member of the College of Physicians of Philadelphia, etc., etc. Philadelphia: Blanchard & Lea. 1852. 8vo. pp. 751.

The present edition of this work contains numerous improvements. The subjects have been restudied, and the opinions and doctrines added to, and remodeled wherever recent observations and discoveries have seemed to the author to render expedient; so that "as it now stands," in the language of the American editor, Dr. Sargent, the volume "furnishes the most satisfactory exposition of the modern doctrines on the principles of surgery that is to be found in any single book on the subject in any language." The American editor has contributed, in foot notes, various annotations relating chiefly to inflammation, suppuration, tubercle, cancer, tumours, aneurisms, and ankylosis.

An important improvement upon the previous editions consists in the introduction of a great number of admirable wood engravings, well calculated to explain and illustrate the text.

Obstetrics: the Science and the Art. By CHARLES D. MEIGS, M. D., Prof. of Midwifery and the Diseases of Women and Children in Jefferson Medical College at Philadelphia, etc., etc., etc. Second edition, revised. With one hundred and thirty-one illustrations. Philadelphia; Blanchard & Lea. 1852. 8vo. pp. 759.

It were insulting to the intelligence of our readers to inform them that, albeit certain extravagances of opinion, among which is to be included a repudiation of chloroform in advance, and eccentricities of diction, the author of this work holds a position as a teacher and practitioner, in which his national brethren have just grounds for pride; and which should command, as it does, for his writings on the science and art of midwifery prompt and extensive circulation.

The first edition of this large work was published three years ago. That edition, although a large one, was exhausted, so that the author was called on to prepare a second which has recently been issued. The latter is considerably augmented, and, as the author states, improved by "recasting some parts, cancelling others, and by an earnest attention to improvements in the literary execution of the whole."

The author apologetically alludes to the fact that his works have been written "amidst the agitations, the distractions and the fatigue of a physician's life." How much is contained in these expressions the physician alone can appreciate.

A Complete Treatise on Midwifery; or the Theory and Practice of Tokology; including the Diseases of Pregnancy, Labor, and the Puerperal State. By ALF. A. L. M. VELPEAU, M. D. Translated from the French by CHARLES D. MEIGS, M. D., etc., etc., etc. Fourth American, with the additions from the last French edition, by WM. BYRD PAGE, M. D., Lecturer on Obstetrics in the Philadelphia Medical Institute, etc., etc. With numerous illustrations. Philadelphia: Lindsay & Blakiston. 1852. 8vo. pp. 652.

Velpeau's Midwifery translated and edited by Prof. Meigs, has for many years been a standard work in the country. The present edition embraces the latest additions by the author, of course embodying all the modern developments and improvements in the department of medicine of which it treats. It should be added that the task of the revision of the present edition, and comparison with the last French copy, was confided to Dr. Page, with the approbation of the original translator, Prof. Meigs.

The work, in its present form, must prove an acceptable addition to the literature of the subject of which it treats.

The History, Diagnosis, and Treatment of the Fevers of the United States.

By ELISHA BARTLETT, M. D., Professor of Materia Medica and Medical Jurisprudence in the College of Physicians and Surgeons of the University of the State of New York, etc., etc. Third edition, revised. Philadelphia: Blanchard & Lea. 1852. 8vo. pp. 595.

Having noticed at considerable length in this Journal the first and second editions of the above work, we need now but express gratification that so worthy a contribution to the medical literature of this country continues to meet with unabated favor. In justice to the work, and to ourselves, we feel bound to say, what our attentive readers already know, that we have found cause since writing the notices just referred to, for changing our views respecting an important question pertaining to continued fever, viz., the identity or non-identity of typhus and typhoid fever. We were disposed to take issue with Prof. B. on this question when the former editions of this work appeared. As an unforeseen and unexpected result of a careful investigation of the subject by means of the analyses of recorded cases under our own observation, we have been brought to the conclusion, which heretofore we had

contended against, viz., that the two varieties of fever just named (typhus and typhoid) are distinct forms of disease. The satisfaction of arriving by logical methods of investigation at the truth, in our estimation far outweighs any gratification of pride in an unchanging consistency of opinion; and it is with real, not affected pleasure, that we declare our conviction of the non-identity of these two forms of fever, a doctrine ably advocated by Prof. B.

Elements of Chemistry; including the Application of the Science in the Arts. By THOMAS GRAHAM, F. R. S., Prof. of Chemistry in University College, London, etc., etc. Second American from an entirely revised and greatly enlarged English Edition. With numerous Wood Engravings. Edited, with notes, by ROBERT BRIDGES, Prof. of Chemistry in the Philadelphia College of Pharmacy, etc., etc. Philadelphia: Blanchard & Lea. 1852. 8vo. pp. 331.

The former edition of Graham's *Elements of Chemistry* has been generally recognized as a valued addition to the text books on this science. In this new edition the size of the work is increased, and the illustrations have been changed in style and nearly doubled in number. The work, in its present form, has been for some time in progress of publication in London in parts, and the entire publication is not yet completed. The American publishers, in consequence of repeated inquiries for the new edition, have issued the first half separately, and the balance may be expected during the present year.

The portion now republished treats of the imponderables, heat, light, etc., and of the elementary substances.

Review of Materia Medica, for the use of Students. By JOHN B. BIDDLE, M. D., formerly Prof. of *Materia Medica* in the Franklin Medical College, of Philadelphia, etc., etc. With illustrations. Philadelphia: Lindsay & Blakiston. 1852. 12 mo. pp. 322.

The author was led to prepare this little volume in consequence of the dearth of elementary works on the *Materia Medica* proper, text books like the United States Dispensatory, and Preira's *Materia Medica*, being too voluminous to be conveniently used for that purpose. The chief object of the work is to furnish a "condensed *review* of the elements of *materia medica*, as a guide to the courses of lectures delivered on this department in the United States." Various illustrations of *indigenous* and *naturalized* plants are introduced.

The work appears to us to be well adapted to fulfill the end for which it was designed.

Miscellaneous Publications.—The following is a list, with occasional brief notices, of a number of publications which have been received during the last two or three months, for which we beg to return our acknowledgments:

Valedictory Address to the Graduating Class of the Medical College of the State of South Carolina, delivered, by appointment of the Faculty, at the Public Commencement, March 12th, 1852. By E. GEDDINGS, M. D., Professor of Surgery.

An Introductory Lecture delivered at the opening of the Thirty-second Session of the Medical College of Ohio, October 15th, 1851. By R. D. MUSSEY, M. D., Professor of Surgery in the Medical College of Ohio.

An Essay on Empirical Remedies; read before the Medical Society of the State of Georgia, April, 1852. By ROBERT CAMPBELL, M. D.

First Annual Report of the Board of Trustees of the Williamsburg Dispensary, for the year ending Feb. 4th, 1852. "To do good, and to communicate, forget not."

Report of the Pennsylvania Hospital for the Insane, for the year 1851. By THOMAS S. KIRKBRIDE, M. D., Physician to the Institution.

State of the New York Hospital and Bloomingdale Asylum, for the year 1851.

A History of the Art of Midwifery; a Lecture delivered at the College of Physicians and Surgeons, Nov. 11th, 1851, introductory to a course of private instruction on Operative Midwifery; showing the past inefficiency and present natural incapacity of females in the practice of Obstetrics. By AUGUSTUS K. GARDNER, M. D., Author of "Old Wine in New Bottles; or, the Spare Hours of a Student in Paris," etc., etc.

This brochure gives an interesting historical account of the rise and progress of the science and art of midwifery. The writer, however, is desirous to forestall the decline and fall of this department of medicine, by pointing out the sources of danger. We give the following extract, involving a reflection upon medical education in this branch, which must be admitted to be founded in fact:

"Two dangers now menace the practice of midwifery in this city, either of which threatens to do it an irreparable injury. The one which particularly concerns us is the carelessness and inefficiency of the profession itself. The student at graduation knows little or nothing of the subject. Practically, they are entirely ignorant of its simplest forms. They have no methodised habits, no illustrative reminiscences to throw light upon the obscurities which may occur in their subsequent practice. The student may graduate without

ever having attended a single bedside of a parturient woman. After the first few cases, in which he is sufficiently alarmed, but which, fortunately, pass by successfully, he concludes that all the talk of position, presentation, rotation, and such like terms, is all nonsense, or at the best, theoretical, and he joins the "expectant" practitioners, trusts to the *vis medicatrix naturæ*, confidently expects that spontaneous evolution will always occur, and by a dull inactivity, resulting from ignorance, loses the child, and not unfrequently the mother also, and injures the reputation of the profession, the capabilities and attainments of which he does not understand.

"The next great danger springs from the love of mammon. Ignorance and presumption, which thrive upon ignorance and credulity, when 'fools rush in where angels fear to tread.'"

Amputation of the Entire Lower Jaw, with Disarticulation of both Condyles. By J. M. CARNOCHAN, M. D., Prof. of the Principles and Operations of Surgery in the New York Medical College, etc., etc.

We confess a lack of admiration for what are frequently called brilliant achievements in operative surgery, while, at the same time, we disclaim, most emphatically, any deficiency of respect for surgical science and the true surgeon. To go a step farther than his predecessors in mutilation, does not seem to us the highest mark of distinction to which the surgeon should aspire. The author of this paper remarks, "To Dupuytren was reserved the glory of having, in 1812, first removed, by a methodical operation, a portion of the body of the inferior maxilla." In what, we would ask, consists the *glory* of having done this? Dupuytren had the boldness to do what others before him had feared to do lest they should place the lives of their patients in too great jeopardy. There may have been merit in the exercise of this boldness, but for the glory, let it rather be reserved for him who decides, in a critical emergency, to spare an important part of the body, and, by assiduous and skillful treatment, preserves life and the body in its native integrity. It is not contended that the operation of Dupuytren required any very superior knowledge or genius for its performance. He *dared* to do it, or his position and reputation were such that he could afford to run the risk of doing it. We know that this kind of distinction is fascinating, and we honestly believe that the influence of this false admiration for operative surgery has rendered that noble department of medicine too often a curse and a disgrace, instead of an honor and a blessing. We have not time nor space to continue our remarks on this subject, but we wish, for one, to be most distinctly on the side of *conservative* surgery. So far from boasting of a bloody, mutilating feat, we think the surgeon in reporting such cases should always feel

most solicitous to show abundant reason for the necessity of it, and should write a history of the operations to which he has been reluctantly compelled to resort, in a strain of sadness rather than exultation. Providence deliver ourselves, and the rest of mankind, from the scalpel of the surgeon whose chief ambition is to be a brilliant operator! Far better is it to be at the mercy of the ruffian, for the latter may be content to take our money and spare life.

This strain of remark has been suggested by the title of the paper which is prefixed, but we disclaim any pointed reference to the author of the paper, or to the particular operation which he describes, in what has been said. We have written, as usual, on the spur of the occasion, and should be very sorry to have any personality imputed to us. The feelings we have expressed pertain to abstractions, not to any individual or individuals. God grant that what we have said may have nothing but idealism to rest upon!

In justice to Prof. Carnochan we should state that the terrible operation of removing the entire lower jaw appears to have been skillfully performed. From the history of the case it is charitable to presume an operation to have been proper and necessary; and it resulted in complete success, the patient, ten months afterward, pursuing his vocation as a drayman in a state of perfect health.

It should be added in connection with the notice of this paper, that the award of the credit of the first operation of removing a large portion of the lower jaw to Dupuytren, is called in question by Dr. George C. Blackman, who asserts the honor to be due to Dr. Deadrick, of Rogersville, Tennessee, who performed a similar operation in 1810, and published a report in the '*American Medical Recorder*.' The validity of this claim in behalf of Dr. Deadrick, appears to be fully substantiated by Dr. Blackman, and, indeed, has been admitted by some of the surgical writers in Great Britain.

The Annual Discourse before the Philadelphia County Medical Society, delivered February 10, 1852. By the President, SAMUEL JACKSON, M.D., formerly of Northumberland.

A sententious, pithy, excellent discourse.

Tableaux of New Orleans. By BENNET DOWLER, M. D.

In this memoir, the author evinces that thirst for scientific research for which his contributions to various departments of science have rendered him so distinguished. The curious reader will find in these pages an interesting collection of facts pertaining to the geographical, commercial, geological and sanitary history and relations of the city of New Orleans.

Contributions to Experimental Physiology. By BENNET DOWLER, M. D.

This is another paper by the same distinguished observer, first issued in the *New Orleans Med. and Surg. Journal*, and republished for distribution in a separate form. Experiments are described in this paper tending to show "That the ligation of the trachea, the divisions of the spinal cord in the cervical and dorsal regions, the removal of the viscera, the destruction of the ganglions and plexuses of the sympathetic nerve, the ligation section and removal of the brachial and ischiatic plexuses, including the nerves of the limbs, do not prevent intelligence, sensation and motions, which are accurate in design, perfect in execution, being simultaneous and altogether voluntary in all parts of the body, even though connected only by muscles." These results are certainly surprising. We shall find space for a condensed account of these experiments in a future number.

Thirteenth Annual Report of the Directors and Superintendent of the Ohio Lunatic Asylum, to the Fiftieth General Assembly of the State of Ohio, for the year 1851.

The report by the superintendent, D. S. Hanbury Smith, is a comprehensive, carefully prepared, valuable document.

The Vital Statistics and Sanitary Condition of Memphis, Tenn. An Anniversary Address, delivered, by appointment, before the Memphis Medical Society, on the 5th of February, 1852. By GEO. R. GRANT, M. D.

If similar investigations and reports could be made in different sections of the country, data would soon accumulate which might lead to valuable inductions.

Dr. Talcott's Address to the Candidates for the Degree of Doctor in Medicine in the Medical Institution of Yale College, January 15, 1852.

A sound, sensible discourse.

Report of the Eastern Lunatic Asylum in the City of Williamsburg, Virginia, 1851.

This, as well as previous reports by Dr. Galt, the superintendent of the institution, show him to be an able and accomplished physician.

Semi-Annual Meeting of the New York State Medical Society.—We copy the following from the *New York Medical Gazette*:—

The semi-annual meeting of this body was held in the Hall of the Crosby-street College, on Tuesday, 29th June. Professor Clark presided, and read

the usual address, which was an able and judicious performance. As it will no doubt be published, we refrain from any comment till it appears. Dr. Cock was appointed secretary, *pro tem*.

Invitations were then read, inviting the Society to visit our various public institutions, and proffering private hospitality at the residences of Drs. Cheesman, Mott, and Stevens, all of which were accepted.

By a liberal usage of this Society, all the physicians present were made honorary members of the State Society for the present session, and privileged to take part in its deliberations.

The attendance was large, and the meetings for business, as adjourned from day to day, were highly interesting.

Dr. Tuthill read a paper on the Statistics of Births, Marriages, and Deaths, which merits high commendation, for its thorough analysis of the objections made to the law, and their refutation.

Professor Lee followed on the causes of the circulation of fluids in vegetables and animals.

Dr. Swett, on Bright's disease of the kidneys, as the next paper, and was followed with remarks by Professor Clark, and illustrations with his powerful microscopes, during a recess.

A discussion on the varied forms of Quackery and the duty of the Society, on motion of Dr. Cash, of Orange county, brought forth remarks from a large number of the members, which concluded the business of the first day.

In the evening the members partook of the elegant hospitality of Dr. Cheesman, in Broadway.

The second day was opened by Dr. Spencer's long and ingenious paper, on his theory of Vegeto-Animal Heat and Life, which he illustrated by colored diagrams, and upon which topic he has evidently bestowed much labor. Of its merits we are not prepared to speak at present, nor until its publication.

The rest of the day was taken up in visiting the Asylums of the Deaf and Dumb, the Blind, Croton water works, &c., until the evening, when the hospitable mansion of Dr. Mott, at Bloomingdale, welcomed the Society to agreeable recreation and refreshment, under the roof of this "Napoleon of Surgery." After which they attended the Pathological Society.

The third day was chiefly spent in visiting the Alms House Department at Bellevue, Blackwell's Island, and Randall's Island, terminating the day by a feast at Dr. Stevens's country seat in Astoria.

The fourth day was devoted to varied business, a paper on the Statistics of Surgery, by Prof. Van Buren, another by Dr. Peet, on the instruction of Deaf Mutes, &c., when the Society adjourned.

The announced report on the experimental treatment of Puerperal Fever, though ordered to be the special business of this semi-annual meeting, was passed over, although several gentlemen were present from day to day prepared to report papers, and for the discussion had it taken place.

The daily papers generally published brief and correct notices of the proceedings of the Society, and this semi-annual meeting may be regarded as highly satisfactory and useful, so that it will probably be repeated next year. Many gentlemen from different and distant parts of the state remained in attendance throughout the entire session.

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ORIGINAL COMMUNICATIONS.

ART. I. — *Letter from Prof. John C. Dalton, Jr.*

PHYSIOLOGICAL RESEARCHES OF M. BERNARD.

PARIS, June 21, 1852.

DEAR DOCTOR, — According to M. Bernard's view of the action of the gastric fluid and the changes which the food undergoes in the stomach, it will readily be understood that the "chyme" will vary considerably in composition with the nature of the alimentary materials. It has been formerly supposed that the chyme was a homogenous fluid, resulting from the indiscriminate solution of *all* the digestible substances which had been taken into the stomach. In reality it is not so; — since it is only the albuminoid matters which are digested in the stomach, and the remaining alimentary principles pass into the intestine in the same, or nearly the same condition in which they were swallowed. The chyme, then, is a mixture of digested albuminoid matters, and undigested, oily, starchy, and saccharine substances. It remains to be seen what becomes of these last. Our method of ascertaining where and by what agents these matters are digested is to follow them downward in the intestine and discover at what point of the alimentary canal the oil, or the starch, loses its natural physical and chemical properties and

becomes absorbable. Oily matters, for example, enter the duodenum unchanged;— but at a certain distance from the pylorus they suffer an alteration, are digested, in fact, and are no longer to be recognized by their ordinary characters. This change commences in many animals immediately below the opening of the biliary and pancreatic ducts, and as the bile appeared to be the most abundant and important intestinal fluid, the digestion of fatty substances has been attributed by some experimenters to this secretion. In some animals, however, the ducts are separated by a considerable distance; and in all these instances it is the biliary duct which comes first,— the pancreatic duct afterward. In the rabbit, for example, the biliary duct opens at the upper part of the duodenum, while the pancreatic joins the intestine some eighteen inches farther down. In all cases where the ducts are so separated the fat can still be recognized in the intestine after it has passed the opening of the biliary duct, and disappears only when it has been subjected to the action of the pancreatic fluid. The digestion of oily matters, in other words, always corresponds, in place, to the opening of the pancreatic duct. It occurs high up in the intestine when the duct is situated high, and lower down when the duct opens lower down. There is, then, the most complete proof that can be drawn from comparative anatomy, that it is the pancreatic fluid that accomplishes the digestion of oily substances.

But a still more convincing proof is obtained by the method which M. Bernard has already followed with the saliva and the gastric juice, viz., by obtaining the fresh secretion from a living animal and trying, by direct experiment, its action on the alimentary principles. For this purpose he takes a dog in whom the processes of digestion are actively going on, makes a short incision into the abdomen a little to the right of the median line, finds the pancreatic duct *by feeling*, introduces a slender silver canula and allows the secretion to drain away into a small India rubber reservoir, until enough has been accumulated for experiment. This requires only a short time if digestion is going on when the operation is done. He then takes fresh bile, saliva (the different varieties) and gastric fluid, obtained in the same manner. Olive oil shaken up with these different fluids in test tubes is only mechanically mixed with them; but when it is poured into a test tube containing fresh pancreatic fluid, it is immediately *emulsioned* in the most complete manner;— and the fluid, which was transparent and limpid like water, becomes at once white and opaque as milk. If the emulsion be exposed for some time to the air at a temperature of 40° cent. it suffers a further change and from alkaline becomes acid, in consequence of the fat being decomposed into glycerine and a free fatty acid. This last change, however, is entirely

artificial and does not take place in natural digestion. In the intestine the oil is simply *emulsioned*, and still retains its peculiar *chemical* characters. It is, therefore, absorbed as oil, but in a state of minute subdivision.

When the fresh pancreatic secretion is obtained from a living animal in the manner indicated above, it is a clear, watery fluid, with a distinct alkaline reaction. It has the following composition:

Water,	91.28
Organic Matter,	0.44
Ashes,	8.28

100.00

Free Soda,
 Chloride of Sodium,
 " " Potassium,
 Lactate of Lime,
 Alkaline Carbonates,
 Phosphate of Lime.

To which of these ingredients does the pancreatic fluid owe its peculiar property of emulsifying fat? It is not the free soda or the alkaline carbonates, since the saliva and the bile are equally alkaline but have no similar effect on oils. The contents of the intestines also are in many animals, constantly acid, and would therefore effectually prevent any action that depended on the alkaline qualities of the secretion. The pancreatic fluid, like the gastric juice, owes its digestive properties to the organic matter which it holds in solution. This organic matter, like that of the gastric juice, is precipitable by alcohol; and the precipitate, drained, washed, and redissolved in water, retains all its original properties. Its solution has the property of emulsifying oily substances in the same manner as the natural pancreatic fluid.

The active principle of the pancreatic fluid has some points of resemblance with albumen, since it is precipitated both by heat and alcohol. It is not albumen, however, since neither the white of an egg nor the albumen of the blood have any similar action on oils. It differs from albumen, also, in some chemical characters; as its alcoholic precipitate is easily soluble again in water, while albumen once precipitated cannot be again dissolved.

It resembles casein, also, in some respects. It is precipitated, for example, like casein, by sulphate of magnesia in excess. As the casein in milk apparently seems to hold its oily part in emulsion, so the pancreatic fluid in the

intestines exerts a similar action on the oleaginous ingredients of the food; so that there is, in reality, considerable resemblance in the physiological properties of the two substances. They are not, however, identical, since the pancreatic fluid is coagulated by heat, which has no effect on a solution of casein. Bernard finds that fresh pancreatic fluid constantly coagulates by heat. Other experimenters, particularly in Germany, have stated the contrary, and maintained that the secretion was unaffected by heat. This difference in the result of the experiments Bernard explains by the fact that the pancreatic secretion becomes altered very soon after the operation of making an artificial fistula. Even in dogs, who bear these operations on the abdomen with greater impunity than most animals, the partial peritonitis, which is soon established about the wound, vitiates the secretion of the pancreas to such a degree, that it will no longer coagulate by heat, nor exert its proper action on oily substances. For purposes of experiment, it is always necessary to take the *first fluid secreted* after the performance of the operation.

The active principle of the pancreatic fluid must, then, be considered as a peculiar organic matter, which gives to the secretion the power of emulsifying oily substances.

There is still another class of alimentary principles, namely, the amylaceous, which acquire to be modified by the action of the intestinal fluids. Starch is no more absorbable in its natural condition than fat, and, to become absorbable, it is transformed first into dextrine and next into sugar; and it is finally absorbed into the circulation under the form of sugar. There is one thing, however, remarkable about the digestion of starchy substances. While the digestion of both the other orders of alimentary principles, albuminoid and oleagious, is strictly *localized*, so to speak, i. e., is performed in particular parts of the alimentary canal, and by means of special secretions, the digestion of starchy substances is not so;—but takes place indiscriminately throughout the whole length of the digestive tube, below the stomach. *All* the intestinal fluids have, more or less, the property of converting starch into sugar. Simple contact with any part of the intestinal mucous membrane is alone sufficient to effect the change.

There are, then, three different digestions, so to speak, carried on in the alimentary canal; a different process being required for each of the three principal orders of alimentary substances; and at the same time there are three different products resulting from their modification. First, the albuminoid matters are dissolved by the gastric fluid in the stomach and converted into "albuminose;" a substance which is not coagulated by heat or

the strong acids, but only by some metallic salts. Albumen, fibrin, and casein, are all three converted by stomach digestion into this secondary principle. Second, fatty substances are converted into an oily emulsion by the pancreatic fluid in the duodenum; and thirdly, starch is transformed into sugar by the action of the intestinal fluids generally.

It will be seen that no account has yet been given of the nature and action of the bile; a secretion which seems particularly difficult of study, notwithstanding its great abundance and the ease with which it may be obtained for purposes of experiment. M. Bernard's explanation of its physiological properties will not, probably, appear by any means so clear and satisfactory as that which he gives of the other digestive fluids. One cannot help suspecting indeed, that he is not entirely satisfied with his own ideas on this point. Such as they are, however, you shall have them, and form your own opinion as to their merits.

It has already been said that the bile has the property of converting starch into sugar. But this property is one which it possesses in common with all the other intestinal fluids and cannot be considered as at all characteristic. The bile, in fact, has by itself no special action on any of the alimentary principles. Neither oleaginous nor albuminoid matters, in their natural condition suffer any change by being placed in contact with it. But if albuminoid matters, which have already passed through the stomach be mixed with bile, an immediate action becomes evident. In all animals the opening of the biliary duct is situated at a very short distance from the pylorus; so that the food, on passing out of the stomach, comes immediately in contact with the biliary secretion. The effect of this contact is to produce a copious yellow precipitate. Matters which were held in solution by the gastric fluid are thrown down by the bile. In other words, the chemical actions which had been going on in the stomach are arrested as soon as the food enters the duodenum. At this part of the alimentary canal, a new set of actions is about to commence; and in order that they may be properly accomplished it is necessary that those which have preceded them should be checked. For there is an essential difference, a kind of opposition, between the changes which the food undergoes in the stomach, and those which are to take place in the intestine. The processes of stomach digestion are essentially antiseptic. They are analogous to those produced by the prolonged action of fire. Fibrin, for example, is transformed into albuminose. Fat which is not chemically changed, is simply melted. Starch in the stomach merely swells up, and becomes hydrated, exactly as it does by boiling in water. On all the alimentary matters the effect of stomach digestion is analogous to a kind

of cooking; and is entirely *anti-putrefactive*. The gastric fluid is itself *anti-putrefactive*, and very little liable to change. It may be preserved for an indefinite length of time without losing its digestive properties.

On the other hand, the pancreatic fluid is extremely liable to putrefaction and changes very rapidly by exposure; so that a very short time after it has been secreted it can no longer be used for purposes of experiment. Since there is this opposition between the actions of the gastric and pancreatic fluids, they would necessarily interfere with each other, were there not some secretion intermediate between the two, which should neutralize the action of the gastric juice before the contents of the stomach come to be mingled with the pancreatic fluid. The bile is such a secretion. It immediately destroys the gastric fluid, and arrests its action; and, in fact, it is found by direct experiment, that if bile be injected into the stomach of a living animal, it effectually stops, for a time, the digestive process.

Another effect of the presence of bile in the intestine, seems to be to regulate the chemical changes which go on there, in such a way that the products of these changes are not the same that they would be out of the body. The decomposition of azotized organic matter, for example, out of the body, always gives rise to ammoniacal products. On the other hand, the substances resulting from the decomposition of non-azotized matters are always acid. Starch is transformed first into dextrine and sugar, and a continuation of the process produces lactic acid. Fats are decomposed into glycerin and a free fatty acid. In the intestine, however, exactly the contrary is the case. The internal surface of the intestine of carnivorous animals has always an acid, that of herbivorous animals an alkaline reaction. The azotized matters give rise to acid products, and the non-azotized to alkaline. This modification of the chemical changes, as they take place in the intestine, is referred to an influence exerted by the presence of the bile.

Such is M. Bernard's account of the character and functions of the bile. The secretion is so evidently one of a very complicated character, that perhaps it is not surprising that we have not yet entirely mastered its physiological history. Unlike other secretions, a large portion of it, after being once poured out by the excretory duct is reabsorbed, during its passage down the intestine. In the rabbit, for example, it is estimated that a quantity of bile is secreted daily amounting to one-eighth or one-tenth the weight of the whole body. But four-fifths of this quantity are reabsorbed before it reaches the end of the intestine, and it is only a small portion, consisting mainly of the bitter principle and coloring matter which is finally rejected with the refuse parts of the food. The liver is not only a secretory and an excretory

organ, but is destined at the same time, to accomplish certain other processes in the preparation of the blood which are still more obscure and complicated; as will be seen from what follows on the absorption of the digested portions of the food.

The three alimentary substances, which have been subjected to different digestive processes in the stomach and intestine, and which have respectively been converted into albuminose, sugar and an oily emulsion, are afterward absorbed into the circulation. But they are not all absorbed by the same vessels. There are two different routes which these substances follow in leaving the cavity of the intestinal canals: 1st, the portal vein; 2d, the lacteals. *All the albuminoid and amylaceous substances pass by the portal vein; all the fatty matters are taken up by the lacteals.* The chyle, which was formerly supposed to contain all the products of digestion, in reality contains only one class of them, the oleaginous. The two other classes are absorbed by another system of vessels. The anatomy of the portal vein on the one hand, and of the lacteals on the other, make it evident that all the alimentary materials, after absorption, and before entering the general circulation, are compelled to pass through certain preparatory organs. The oleaginous matters, entering the subclavian vein by the thoracic duct, are taken directly to the lungs. The albuminoid and amylaceous substances, taken up by the portal system, must pass through the liver before they are mingled with the rest of the blood in circulation. In these organs, the substances which have been absorbed, are destined to undergo a further modification before they can be used for purposes of assimilation. Even albumen is not assimilable until it has passed through the hepatic circulation. If pure albumen be injected into the *jugular* vein of an animal, it is not assimilated, but is excreted in the urine, as albumen. But if it be injected into the *portal* vein, it passes through the liver, becomes assimilable, and no longer appears in the urine. Cane sugar, absorbed by the portal system is converted in the liver into grape sugar. Oleaginous matters suffer some analogous change in the lungs, by which they are rendered fit to be used in the processes of nutrition. For these reasons it appears at least doubtful whether it be possible to support life to any extent by means of "nutritive baths," which have sometimes been used for the purpose. Nutritive enemata are undoubtedly useful, since the albuminoid matters are taken up by the portal system and carried to the liver. But when absorbed by the vessels of the skin, they are not yet fit for assimilation, and must therefore be excreted in the same manner as when injected into the jugular vein. The digestive processes, therefore, or that by which the elements of the food are prepared for conversion into blood, far from

being a simple process, performed in the stomach alone by the gastric juice is not even completed in the interior of the alimentary canal. But the nutritive matters, after being rendered absorbable have still other changes to undergo in the lungs and liver by which they are made assimilable; and these must necessarily be considered as essential parts of the process of digestion.

A few days ago, I had the pleasure of seeing, in M. Bernard's laboratory, two experiments which I had heard of before, but which it is difficult to believe in thoroughly, without the evidence of one's own senses. The first was a demonstration of the manufacture of sugar in the liver; the second, the production of diabetes mellitus in a rabbit, by wounding the posterior part of the medulla oblongata. Both experiments were completely successful.

M. Bernard maintains that one of the constant and normal functions of the liver is the production of sugar. In all animals, dead suddenly while in good health, in the human subject under similar circumstances, in executed criminals, &c., the blood of the liver and of the hepatic vein, and the substance of the liver itself is found to contain a very appreciable quantity of sugar (glucose); and this, when no sugar or starch has been taken for food, and when it cannot be discovered in the contents of the intestines nor in the blood of the portal vein below the liver. Any serious indisposition checks this production of sugar, in the same way as it checks the secretion of gastric juice, of cutaneous perspiration, &c. But in a state of health it is a constant phenomenon. The sugar thus produced by the liver is destroyed in the lungs; consequently it is not found in the general circulation, nor in any other organ in the body than the liver.

To prove this fact M. Bernard took a young dog that had been fed all the morning on animal food, and killed him instantaneously by destroying the medulla oblongata with a kind of "garrote." The abdomen was immediately opened, and a ligature placed on the portal vein just below the liver, another on the hepatic vein just above, and a quantity of blood taken from each of these two situations to be tested. Each portion was subjected to the same processes of coagulation, decolorizing, &c. — and afterward treated by the same reagents; tartrate of potass and copper, with heat. The blood from the portal vein, which had not yet passed through the liver showed no alteration whatever; in the other specimen a copious precipitate of the suboxide of copper took place, as abundant as is often seen in cases of diabetes mellitus. The substance of the liver, brayed in a mortar and extracted with water, showed the same reaction, while the substance of the lungs, treated in a similar manner, showed nothing of the kind. The fermentation test was also

applied, but was altogether superfluous, as the results of the first were completely satisfactory.

The second experiment is as follows: a rabbit is taken and the bladder emptied by compressing the hypogastrium. The urine is tested for sugar, and, as might be expected, shows no trace of it. A small steel instrument is then passed through the posterior part of the skull, into the substance of the medulla oblongata, and is immediately withdrawn. The instrument has a transverse cutting edge, like a chisel, a little over one line in length. From the middle of this edge a fine steel point runs out in the axis of the shaft for about two lines. This point is to prevent the cutting edge from passing through the medulla, and wounding its anterior fibres, which would destroy the animal. It is the posterior portion of the medulla alone which is to be wounded by the cutting edge. The instrument is then passed forward in the median line until the steel point rests upon the basilar process of the occipital bone, when it is immediately withdrawn. If the puncture has been made accurately in the median line, the animal makes no struggle during the operation, and appears simply feeble and exhausted afterward. He soon attempts, indeed, to use his limbs, and in a few days is generally quite recovered. If the instrument has divided considerably to either side, the animal presents the singular phenomenon of constantly turning toward the wounded side whenever he attempts to move. In the course of two hours after the operation, the urine tested by the tartrate of potass and copper, shows an abundance of grape sugar in solution; and the blood taken from the jugular vein also contains a considerable quantity. This state of artificial diabetes continues, in the rabbit, from thirty-six to forty-eight hours. At the end of that time the sugar disappears from the blood and secretions, and the animal returns to its natural condition, after which the state of diabetes may be again produced by a fresh puncture. Indeed the experiment, if carefully performed, may be repeated several times on the same animal. The explanation of this singular phenomenon is not altogether easy. It is considered by M. Bernard as illustrating the connection between the cerebro-spinal and sympathetic nervous systems. But however it may be interpreted, the fact itself is incontestible.

Yours truly,

JNO. C. DALTON, JR.

ART. III. — *Case of Perforation of Intestine.* Reported by EDWIN
R. MAXSON, M. D.

Mr. H——, a respectable merchant, aged 57 years, had been troubled for several years with dyspepsia, attended with acid eructations, and vomiting, generally for a week or two, in March and September of each year.

As the difficulty rather increased, he had been induced, by the advice of a quack, to take, for a long time, a composition containing capsicum. This he took daily for near twelve months, at the end of which time he grew much worse. His vomiting became very obstinate. In this condition I was called to see him in March, 1848. I ordered sinapisms over the stomach, and gave Hydrg. Mit. gr. $\frac{1}{4}$ with Pulv. Dov. gr. $\frac{1}{2}$, every two hours. In eight hours the vomiting was checked, so that I gave him Bismuth Sub. Nit. gr. iii, every six hours. This was discontinued at the end of three days, after which I gave him a weak infusion of Columbo. He soon recovered his strength, and his health continued tolerable till December 1st following, when he felt a slight uneasiness in the epigastric region, caused, as he supposed, by lifting. He kept about his business till December 4th, when he was suddenly distressed in the right and lower part of the epigastric region; and being faint he called for brandy, which was administered with little effect.

I saw him in half an hour. He was in great distress, groaning at every breath; the pain occupying the small spot above referred to. I applied a sinapism over the bowels, and gave Tr. Opii, gtt. xxx, with Gum Camph. gr. i, which dose was repeated in one hour, with little or no effect. I then gave Hydrg. Mit. gr. xii, in half an ounce of castor oil. This was followed by Castor Oil \mathfrak{z} ss. every two hours; and this by an enema, which produced a slight movement of the bowels. During all this time, he had not breathed without a groan. He had taken a bowl of warm gruel, which gave him considerable pain through the bowels, and there began to be some tenderness, and slight distension of the abdomen.

His pulse had been continually, and was now rapidly sinking. Brandy had no effect; and in twenty hours from the time he was taken he expired. All the symptoms induced me to believe there was perforation of the intestine. This I declared to the friends, and requested an examination, which

I was permitted to make forty-eight hours after death, in the presence of several gentlemen. The cavity of the abdomen contained about two quarts of a darkish fluid. The peritoneum was nearly of the same color; and there was a smooth circular perforation of the duodenum, the size of a half dime.

Adams Centre, Mass., August, 1852.

ART. II.—*Fracture Tables. Being a Supplement to the Fracture Tables published by Dr. Hamilton in the Buffalo Medical Journal, April, 1849.* Compiled and arranged from DR. HAMILTON'S notes. By J. BOARDMAN, A. B., Buffalo, N. Y.

In the Buffalo Medical Journal for April, 1849, and afterward in pamphlet form, Dr. Frank H. Hamilton published "tables" showing the results of treatment in one hundred and thirty-six cases of fracture for the "purpose of determining the average results of treatment in fractures."

Those tables were compiled from notes made by Dr. Hamilton of such cases as came under his own observation, and which he had the privilege of examining, not drawn exclusively from his own practice, or from the practice of this city, but collected from various sources.

The following additional tables have been compiled from notes of nearly three hundred more cases collected by him since that time, and as in the first tables, no case has been admitted which was known to have been treated by an empiric, or which did not come under surgical treatment.

Dr. Hamilton's measurements have been made with great care, rarely those taken from two points being considered sufficient until proved to be correct by at least one; and often two other measurements. Also in these notes by recording the name of surgeon, name of patient, time, and place of accident, care has been taken to avoid the repetition of any case, and to have them show the true results in cases of fracture.

In the tables the names of *females* are printed in *italics*, *y.* is written for *year*, *m.* for *month*, *w.* for *week*, *u.* for *united*, *n. u.* for *not united*, *in.* for *inch*, *p.* for *perfect*, *ip.* for *imperfect*. A limb is called "perfect" when there is no striking deformity, shortening, or maiming.

NAME OF BONE.	Name of Patient.	Age of Patient.	Cause of Accident.	Point of Fracture.	Character of Fracture.	Result of Accident.	What operation was made and what instr. used, trephine, saw, etc.	How soon after the Fracture.	For depressed bone, effused blood, or pus.	Result of Operation.
137. Os occipitis, 138. "	D. L. L.	13 y. 50 y.	Hit by a clam shell, Fall on back of head	Near center, Sup. angle,	Comp. Simple, with concussion	Sinus, Coma,	Knife, Trephine,	3 w. 16 d.	Bone, Pus,	Recovery. Died on the 19th day or 48 hours after operation. Died in 18 hours after operation. Died in 1 week after operation. Epilepsia for five years.
139. Ossa parietalis, 140. Os parietal,	J. W.	9 y.	Struck by a loco- motive.	Both sides,	Comp. com- minuted,	"	Treph. and lester,	1 h.	Bone,	"
141. Ossa parietalis, 142. Os parietal of left side, 143. Os parietal of left side, 144. Os parietal,	W. P. P. K. S. H.	30 y. 30 y. 3 y. 40 y. 9 y.	Fell into the hold of a vessel. Fall of tree upon top of head. Fall of scull's end- wise upon head. Explosion of steam boiler,	Near sagittal su- ture, Near sagittal su- ture, W. side, Near ant. fonta- nelle, Middle, Near ant. fonta- nelle,	" " " " " " " Comp.	" Coma and recov. Coma, Epilepsia and idioty, Coma and para- lysis and epi- lepsy, Abscess, Coma and para- lysis, Coma and recov. Recovery, Epilepsia 15 yrs. Epilepsia 23 yrs.	" No op. Forceps and levator, Forceps and levator, Forceps, Trephine, No op. No op.	7 d. Immediately, 1 h. 1 h. & 5 w.	" " Bone, Bone, " Pus, &c.	" Epilepsia for five years. Died in 24 hours. Epilepsia for five years. Died in a few wks Died 23 hours af- ter operation.
145. Os parietal of left side, 146. Os parietal, 147. Os parietal of left side, 148. Os parietal, 149. Os parietal of right side, 150. Os parietal, 151. " 152. Os parietal of left side, 153. Os parietal,	McG. C. B. S. P. W. M. H. T. S.	14 m. 18 m. 3 y. 9 y. 2 y. 7 m.	Blow from a club, Buck shot disc'd from a gun, Thrown from sleigh Fell six feet, Fall from bed, Fall from a horse, Kick of a horse, Fell against a stove,	Over ear, Near the middle, Ant. inf. angle, Near post. infer- angle,	" " " " " "	" Epilepsia 15 yrs. Epilepsia 23 yrs. Abscess,	Forceps, Trephine, No op. Cut around cicatrix, Trephine, Forceps, " "	11 d. 4 d. 20 y. 24 y. Immediately, 6 w.	" Pus, &c. To insulate cicatrix, Bone, "	" Recovery. Recovery. Temporary im- provement. Nervous pains for 19 yrs. and epi- lepsy, &c. for last 4 years. Recovery.

137. This was rather a cut, or slight scaling up of the outer table, which healed as soon as it was freely laid open and the loose scale was dislodged.
138. The patient was at first "stunned;" then he got up and walked about, but his head felt "big" and "numb." On the 12th day he began to grow dull, on the 16th he was completely comatose, and was trephined by Dr. Hamilton, at the seat of injury. Half a dram of pus found, and slight fracture of internal plate. The autopsy also showed pus at other points.
140. The bones were depressed, but no signs of compression occurring the surgeon in attendance did not think it proper to trephine. Coma supervened on the 6th or 7th day, and the trephine was used by Dr. Hamilton, but no relief was obtained.
141. Fourteen fragments of bone were removed. Epilepsia commenced two years after the fracture occurred and still continues at the end of five years.
- 142, 148, 149. Are similar to No. 154, differing only in the severity. Reported in Buffalo Med. Jour.
144. He is now 29 years old. When he was 23 years old he married and soon after began to have epileptic fits. They have now continued 7 yrs.
145. Occurred five years since. Four pieces of bone removed then, yet he remained speechless, and palsied on the *right* side five weeks; at which time eight more pieces were removed; after this he recovered his speech and the use of his limbs. From the time of the accident until now he has had epilepsia. Until within a year he had the fits as often as once in two weeks, but since he ceased the use of intoxicating drinks, he has had them only once in two or four months. No pain or tenderness at seat of injury. There is an irregular depression at the seat of fracture, but the pulsations of the brain cannot be felt.
146. The bone gradually exfoliated and was removed in pieces from the 11th day until the 18th, when he left the hospital. It is known that he has since died.
147. The buckshot passed through the brain. The trephine was applied to give more free exit to the matter when he began to grow comatose.
150. If this was a fracture, it was unaccompanied with depressions, at least in the outer plate. An abscess formed, however, at the seat of injury, under the scalp, and continued to discharge about two years. Soon after it closed, when he was about five years old, he began to have spasms, vertigo, various and very peculiar hallucinations; his body and mind were gradually becoming enfeebled, when, as nothing else offered a prospect of relief, Dr. Hamilton operated. He was then 23 years old. The operation, which is reported at length in the Buffalo Med. Jour., vol. V, page 460, consisted in brief, in circumscribing the old scars on the scalp with the knife; cutting quite to the bone. The result was for a time a most complete cure, but after a year his old symptoms returned, but in a much milder form. No depression of the skull was discovered in the operation.
151. The wound did not close until, about one year after the accident. Then he began to have fits, which have continued ever since. Dr. Hamilton commenced his operation with the intention of circumscribing and lifting the scar, but finding a slight depression of the outer plate he determined to trephine. No inconvenience resulted from the operation. His recovery was rapid, and the fits were for several weeks less frequent and less severe. It is believed, however, that they have returned at length to their old condition.
152. Occurred 19 years ago. *Wound has never closed.* When 3 years old he had hemiplegia of *right* side. Has had nervous pains from that time to this, commencing in right hand and extending to head. At 17 he had epilepsia, which has continued until now. The pulsations of the brain can be felt distinctly.

NAME OF BONE.	Name of Patient.	Age of Patient.	Cause of Fracture.	Point of Fracture.	Character of Fracture.	Result of Fracture.	What oper. made, and what instrum. used, &c.	How soon after the Fracture.	For depressed bone	Result of Operation
154. Os parietal of left side,	W.	5 m.	Thrown from a carriage.	Near the center,	Simple,	Recovery,	No op.			
155. Os parietal,	J. C.	3 y.	A rail fell upon her,	Near sagittal suture,	"	"	"			
156. Ossa parietalis		6 y.		Near occiput,	"	Pain in scar for 12 years,				
157. Os frontis of left side,		15 y.	Explosion of a cannon,	Over orbit,	Comp. commin.	Coma,	Llevator,	30 m.	Bone,	Died in 4 months.
158. Os frontis,	M. W.	9 y.	Fell ten feet,	Over outer angle of orbit,	"	"	Llevator and forceps,	1 h.	Bone and extravasated blood,	Recovery.
159. "		6 y.	Kicked by a horse,	Near ant. fontanelle,	"	"	"	12 h.	Bone,	"
160. "	H. F.	3 y.	Fell three feet upon a stone,	Over ext. angle of orbit,	Comp.	Epilep. and coma	Trochine,	38 d.	Pus or bone,	Died in 26 hours after operation.
161. "		20 y.	Struck by a locomotive,	Near center of forehead,	"	Coma,	Llevator,	2 h.	Bone,	Died in 24 hours after operation.
162. "	P. M.	28 y.	Fell twenty feet,	Over orbit,	"	Paralysis, idiocy, and death on 31st day,	No op.			
163. "	J. S.	33 y.	Tree fell on his head	Near ant. font.	Simple,	Mental imbecility	Screw levator,	1 h.	Bone,	Recovery.
164. "	J. F.	35 y.	Brick fell upon him from a house,	Over orbit,	Comp.	Coma,	"		"	"
165. Os frontis of left side,	D. P.	65 y.	Fall of a limb of a tree,	Over center orbit,	"	Coma,	Llevator,	Immedly.	"	"
166. Os frontis and os occipitis.	T. B.	22 y.	Thru'n from wag'n		Comp. commin.	Coma,	Heys' saw and screw levator,	1 h.	"	"
167. Os frontis and ethmoid bone,	H. G.	14 y.	Kick of a horse,	Just over the nose	"	Coma and death in 15 hours,	Llevator,	Immedly.	"	Died in 3 months.
168. Os frontis, ethmoid, sphenoid, and temporal,	L. E.	35 y.	Fell forty feet.	At base of skull.	Comp.	Paralysis partial and tardy recovery,	No op.			
169. Probably temporal, &c.	M. M. C.	20 y.	Head traversed by wheel of wagon.	At base of skull.	Simple,		"			

154. The bone was depressed half an inch, yet there was no coma or other signs of injury. He was sent to Dr. Hamilton two weeks after the accident, who advised no operation. The bones gradually resumed their places. Eight years after, he is well and intelligent (see report of this case in vol. II, p. 347 of Buffalo Med. Jour., article, "Fracture of skull in children," by Dr. Hamilton.)
155. This is similar to 154, differing only in the severity, and it is reported with that case.
156. He is now 24 years old, and has never been able to lie on the back of his head since the accident. Scalp is tender. Bone depressed quarter of an inch.
157. A piece of iron $2\frac{3}{4}$ inches long by $\frac{3}{4}$ of an inch thick, entered sideways and parallel with the superciliary ridge. Considerable brain escaped and it was broken into and disorganized to the depth of two inches. The wound closed in two months. He was then perfectly well. No pain or defect in the *phrenological organs*. Two months after he was seized with phrenitis, and died in 48 hours. The autopsy disclosed a cavity containing two ounces of pus. This abscess must have existed while he was at work and was apparently well.
158. This was an extensive fracture with considerable depression, but the recovery was rapid and perfect. (See Buff. Med. Jour. vol. vi, p. 152.)
160. The child did not seem to be seriously injured until the 34th or 35th day after the accident. He then became gradually comatose. The wound had never healed, but there was a slight swelling and discharge still continuing. On the 36th his coma was complete, but no paralysis. Other means having failed he was trephined by Dr. Hamilton, at the seat of injury. The outer plate was broken and loose; the inner not broken. No pus. The dura mater was not penetrated. The patient was not relieved. The autopsy disclosed no pus within the skull.
162. A fissure without depression. He was never comatose, but soon became idiotic and had paralysis of sphincter of his bladder and rectum. [Buff. Med. Jour., vol. vi, p. 151.]
163. He was admitted to the hospital five weeks after the accident. A slight depression at the point of injury could then be felt. It is not certain that the skull was broken. He was knocked down by the blow, but soon recovered and walked home. Has since had occasional paroxysms of vertigo and has fallen down; has had paralysis of facial muscles; also partial of hands, feet, &c. Left side of the retina of the right eye is insensible. His intellect is impaired. This was his condition several months after the accident.
164. A piece of bone exfoliated and was removed by Dr. Hamilton.
165. Occurred three years since. The outer plate was removed, and the inner being depressed, was lifted to its place, but not removed. It did not exfoliate, but it was a year before it ceased to discharge pus. It is now perfectly well.
167. The os frontis was much depressed and the cribriform plate of the ethmoid was also pushed upward against the brain. Some brain escaped. He was not comatose. He recovered rapidly except that some pus continued to discharge from a small orifice. He was bright and strong. He went to school regularly and was able to cut and split a cord of wood per day. Eight days before he died the fistula closed. Two days before he died he was found in the yard comatose, and from this he never recovered. The autopsy showed an abscess involving a large portion of the anterior lobes of the brain and containing several ounces of pus. The bones were still considerably depressed, and completely united by bony matter.
169. He remained insensible three hours after the accident, and bled from the ear for several days. His right eye became turned in — his hearing impaired and his voice raucous, and both of his arms were for a time paralyzed. At the end of about four months he was still dead, &c., and his right arm remained paralyzed. The fact of a fracture at the base of the skull, with extravasation, is not known, but assumed. (Buffalo Med. Jour., vol. v, p. 68, Dr. Hamilton's Hospital Reports. See also case No. 18, reported in same page.)

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Union or not united	Amnt. of shortening,	Perfect or imperfect
170 Ossa nasi,		compound	T. B.	17 y.	14 y.	u.		ip.
171 "		"	G. C.	"	4 w.	u.		ip.
172 "		simple	C. D.	11 y.	"	u.		ip.
173 "		"	N. B.	25 y.	2 y.	u.		ip.
174 "		"	H. S.	3 w.	12 y.	u.		ip.
175 "		"	I. A.	8 y.	15 y.	u.		p.
176 Sept. nasi,		simple	L. L.	19 y.	33 y.	u.		ip.
177 "		"	J. B.	26 y.	4 y.	u.		ip.
178 "		"	J. M.	28 y.	3 y.	u.		ip.
179 Sup. max.	under mal'r	simple	M. P.	34 y.	1 y.	u.		p.
180 Inf. max.	shft.bth.sds.	comp. com.	J. K.	17 y.	8 m.	u.		ip.
181 "	shaft	"	A. G.	11 y.	3 y.	u.		p.
182 "	angle	"	R. B.	39 w	7 w.	u.		ip.
183 "	ang. & shaft	simple	A. M.	26 y.	5 w.	n. u.		ip.
184 "	shaft	"	J. W. S.	25 y.	1 y.	u.		p.
185 "	angle	"	J. McE.	22 y.	1 y.	u.		p.
186 "	"	"	A. V.	47 y.	15 y.	u.		p.
187 "	symp.ment.	"	N. M.	25 y.	10 y.	u.		ip.
188 Clavicle,	out $\frac{1}{2}$	commin.	B. L. D.	"	"	u.	in.	ip.
189 "	"	simple	S. U.	47 y.	11 y.	u.	in.	ip.
190 "	"	"	W. P. L.	26 y.	13 y.	u.		p.
191 "	"	"	H. S.	56 y.	1 y.	u.	in.	ip.
192 "	"	"	E. M.	14 y.	31 y.	u.		p.
193 "	"	"	J. R.	44 y.	1 y.	u.	in.	ip.
194 "	"	"	G. W. R.	48 y.	2 y.	u.	in.	ip.
195 "	"	"	C. A. W.	47 y.	4 y.	u.	in.	ip.
196 "	"	"	Mrs. —	28 y.	12 y.	u.	in.	ip.
197 "	"	"	P. T.	51 y.	4 m.	u.	in.	ip.
198 "	"	"	M. McC.	12 y.	23 y.	u.	in.	ip.
199 "	"	"	A. A. H.	3 $\frac{1}{2}$ y.	6 m.	u.		p.
200 "	"	"	B. G. C.	40 y.	4 m.	u.	in.	ip.
201 "	middle	"	I. L.	32 y.	8 y.	u.	in.	ip.

170. The deformity is very conspicuous.

173. Displaced laterally; ulceration of septum nasi, has occurred and still continues; health bad.

174. Nose nearly flat at its middle and upper part.

175. Slight prominence of cartilage at end of nasal bones.

176. Nose perfectly flat.

177. Nose much flattened.

180. Central fragment slightly lifted and displaced.

182. Posterior fragment displaced upward and anteriorly. Necrosis has occurred.

187. Fracture vertical; left side displaced upward and posteriorly.

188. Deformity striking. The usual form of displacement of fractures at this point, i. e. the outer end of inner fragment over rides the inner end of outer fragment.

189. 190. 191. The displacement of same character as No. 188.

192. There was a projection at seat of fracture five years. None now.

193. 194. 195. 196. Displacement of same character as No. 188.

197. The arm was nearly paralyzed, from use of axillary pad.

198. Deformity at seat of fracture for long time great; now very slight.

200. Bent forward at seat of fracture. Arm not as strong.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	United or not united	Am't. of short'n'g.	Perfect or imp' r'g'e
202 Clavicle,	middle	simple	R. M. F.	12 y.	2 m.	u.		p.
203 " "	"	"	R. M. F.	"	"	u.		p.
204 " "	"	"	J. D. S.	2 y.	24 y.	u.		p.
205 " "	"	"	S. McN.	37 y.	10 y.	u.		p.
206 " "	"	"	J. C.	43 y.	"	u.		p.
207 " "	out $\frac{1}{2}$	"	I. E.	35 y.	5 y.	u.		ip.
208 " "	"	"	M. K.	50 y.	1 y.	u.		ip.
209 " "	"	"	H. M.	44 y.	6 m.	u.		ip.
210 " "	"	"	I. W.	15 y.	1 y.	u.		p.
211 Scapula,	coracoid P.	simple	I. W.	30 y.	3 m.	n. u.		ip.
212 " "	below spine	"	W. V.	54 y.	1 y.	u.		p.
213 Humerus,	low $\frac{1}{2}$	comp.	J. A.	12 y.	10 y.	u.	$\frac{3}{4}$ in.	ip.
214 " "	surg. neck.	simple	M. B.	23 y.	2 m.	u.		p.
215 " "	"	"	M. C.	30 y.	7 w.	u.		p.
216 " "	up. $\frac{1}{2}$	"	J. T.	54 y.	9 w.	u.		p.
217 " "	middle	"	J. M. C.	63 y.	3 y.	u.	$\frac{5}{8}$ in.	ip.
218 " "	"	"	E. F.	18 y.	25 y.	u.		p.
219 " "	"	"	J. B.	44 y.	5 m.	u.		p.
220 " "	"	"	S.	13 y.	7 m.	u.	$\frac{1}{2}$ in.	ip.
221 " "	"	"	J. H. S.	36 y.	41 y.	u.		p.
222 " "	low $\frac{1}{2}$	"	W. C.	4 y.	20 y.	u.		p.
223 " "	"	"	S. McN.	32 y.	15 y.	u.		p.
224 " "	"	"	E. S.	39 y.	8 y.	u.		p.
225 " "	low $\frac{1}{2}$	"	F. N.	59 y.	7 m.	u.	$\frac{1}{2}$ in.	ip.
226 " "	"	"	C. F.	8 y.	8 m.	u.		ip.
227 " "	"	"	H. A.	34 y.	4 m.	n. u.		ip.
228 " "	"	"	E. H.	$\frac{2}{12}$ y.	3 m.	u.		p.

202. 203. Was a *tend* of right and left clavicle.
 207. Shoulder half inch lower than other ; use of arm perfect.
 210. Inflammation extended to articulation of head of humerus, producing for a long time false ankylosis.
 211. Process moves with the head of the humerus.
 213 A small piece of the bone was removed. Has false ankylosis of elbow.
 214. See No. 283.
 217. Seven weeks after accident bone had not united and the fragments were displaced. Same patient as No. 274.
 216. Result of fragilitas ossium.
 219. Result of lues.
 220. Motions of arm nearly perfect.
 222. At same time fractured radius and ulna. See No. 271.
 223. Cannot straighten arm perfectly.
 225. Lower end of upper fragment is in front of the upper end of lower fragment, flexion and extension imperfect.
 226. Fracture immediately above condyles. Treated with two right-angle splints, one anteriorly, one posteriorly ; and also two short, lateral splints, for five weeks. At which time, the upper fragment was found to project in front of the upper end of lower fragment ; motion of elbow perfect ; soon, the hand became forcibly flexed upon forearm, the first phalanges, upon metacarpal bones, and he lost the power of pronation and supination. The projecting portion, eight months after the fracture, was removed by Dr. Hamilton, but with little or no benefit to patient.
 227. Has slight motion of arm, but motion of fore-arm nearly perfect.
 229. False ankylosis, lower fragment displaced backward.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unit'd or not united	Amt. of shortning.	Perfect or imp'rfect
229	Humerus,	low 1-6		9 y.	2 m.	u.		ip.
230	"	in condyle	P. Y.	5½y.	6 m.	u.		ip.
231	"	"	G. B.	5 y.	1 y.	u.		p.
232	"	"	B. S.	12 y.	38 y.	u.		p.
233	"	"	A. B. II.	43 y.	6 m.	u.		ip.
234	"	out condyle	E. R.	infcy	ad't	u.		ip.
235	"	"	J. R. D.	y.	11 w	u.		ip.
236	"	"	R. W.	88 y.	2 m.	u.		ip.
237	Radius,	lower end	O. P.	15 y.	2 y.	u.		ip.
238	"	lower ½	W. H. M.	16 y.	15 y.	u.		p.
239	"	neck	I. R. A.	25 y.	10 m	u.		ip.
240	"	upper ½	J. B.	9 y.	10 y.	u.		p.
241	"	middle	W. J.	10 y.	19 y.	u.		p.
242	"	"	M. O. B.	45 y.	8 w.	u.		ip.
243	"	"	D. S.	21 y.	50 y.	u.		p.
244	"	lower ½	P. B.	15 y.	3 m.	u.		p.
245	"	"	W. L.	2½y.	6 m.	u.		ip.
246	"	"	P. D.	1½ y.	4 m.	u.		p.
247	"	"	W. H.	33 y.	21 y.	u.		p.
248	"	"	H. H.	14 y.	24 y.	u.		p.
249	"	"	S. G.	15 y.	25 y.	u.		p.
250	"	"	M. W.	8 y.	1 y.	u.		ip.
251	"	"	T. B.	22 y.	3 m.	u.		p.
252	"	"	H. D.	52 y.	3 y.	u.		ip.
253	"	"	I. L.	43 y.	3 m.	u.		p.
254	"	"	R.	29 y.	1 y.	u.		ip.
255	"	"	J. D. B.	6 y.	4 m.	u.		ip.
256	Ulna,	3 points	J. L.	25 y.	2 y.	u.	½ in.	ip.
257	"	up. ½	J. C.	18 y.	1 y.	u.		p.

230. Had been broken before at same point and left deformed; remained in same condition after second fracture; false ankylosis of elbow joint.

232. Could not flex fore-arm upon humerus for six months; now motion perfect, but arm pains when used.

233. Also fracture of lower end of humerus at same time. Cannot extend, or flex fore-arm, more than to right angle; flexion of fingers imperfect. Arm pains when she attempts to use it.

235. There was false ankylosis of elbow joint for a long time; now motion free.

236. Outer condyle is separated half an inch; deformity evident; cannot straighten arm perfectly.

237. Much deformed; hand forcibly drawn toward radial side.

239. Upper end of lower fragment displaced forward; supination lost; flexion very imperfect.

242. Did not employ a surgeon for the first five weeks.

243. Pronation and supination imperfect.

244. Somewhat bent at seat of fracture.

246. This was a bend of radius and fracture of ulna. No. 261.

250. Radius was bent forward at seat of fracture when splints were removed, but, as is often the case with young persons, time has made it nearly perfect.

252. Deformity great; hand bent backward and very weak.

254. Slightly bent; motions of hand impaired.

255. Slight displacement of ulna; hand stiff, swollen and partly paralysed.

256. Remains bent.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	United or not united	Amt. of shortning.	Perfect or imp'fect
258 Ulna,	up. $\frac{1}{2}$	simple	U. L.	40 y.	1 y.	u.		ip.
259 "	"	"	R. S.	46 y.	9 w.	u.		ip.
260 "	middle	"	S. G.	30 y.	10 y.	u.		p.
261 "	"	"	P. D.	18 y.	4 m.	u.		p.
262 "	"	"	M. O. B.	3 y.	6 w.	u.		p.
263 "	up. $\frac{1}{2}$	"	E. C.	38 y.	2 m.	u.		ip.
264 "	low $\frac{1}{2}$	"	O. H. P.	26 y.	11 w.	u.		ip.
265 "	low $\frac{1}{2}$	"	P. W.	39 y.	1 y.	u.		ip.
266 "	olecranon P	"	J. C.	18 y.	9 y.	u.		ip.
267 "	"	"	P. C.	14 y.	69 y.	u.		ip.
268 "	"	"	S. D.	14 y.	5 w.	u.		p.
269 "	"	"	C. A.	15 y.	6 m.	u.		ip.
270 Rad. & ulna	low 1-6	compound	A. F.	11 y.	1 y.	u.		p.
271 "	up $\frac{1}{2}$	simple	W. C.	4 y.	20 y.	u.		p.
272 "	middle	"	D. M.	14 y.	4 y.	u.	1 in.	ip.
273 "	"	"	I. S.	9 y.	17 y.	u.	1 in.	ip.
274 "	"	"	J. M. C.	63 y.	3 y.	u.		p.
275 "	"	"	T. B. S.	11 y.	11 y.	u.		p.
276 "	"	"	J. W.	10 y.	40 y.	u.		p.
277 "	"	"	W. P.	12 y.	14 y.	u.		p.
278 "	"	"	J. B.	15 y.	25 y.	u.		p.
279 "	"	"	I. H. L.	37 y.	40 y.	u.		p.
280 "	"	"	G. B.	11 y.	5 w.	u.		p.
281 "	"	"	M. C.	9 y.	10 y.	u.		p.
282 "	low $\frac{1}{2}$	"	J. J.	10 y.	42 y.	u.		p.
283 "	"	"	M. B.	23 y.	2 m.	u.		p.
284 "	"	"	M.	3 y.	8 w.	u.		p.
285 "	"	"	H. A. C.	12 y.	6 y.	u.		p.
286 "	"	"	P. W. T.	32 y.	4 m.	u.		p.
287 "	"	"	E. McL.	1 y.	3 y.	u.		p.
288 "	low $\frac{1}{2}$	"	P. H.	30 y.	10 y.	u.		ip.

258. Head of radius dislocated forward, unreduced; motion nearly perfect.

259. Slight bend at seat of fracture; lower fragment overlaps upper; wrist ankylosed.

261. See No. 246.

264. Anterior luxation of head of radius, unreduced, ulna bent; motions perfect; head of radius does not strike humerus.

266. United by ligament; arm as strong as before.

267. Cannot straighten arm perfectly; pronation and supination imperfect.

268. Union appears to be perfect; four weeks before had a fracture of radius of same arm, which was treated by an empiric.

269. Radius dislocated forward; cannot straighten arm.

271. See No. 222 This was a bend of both bones and not a complete fracture.

272. Arm not as strong as before and at times painful.

274. Same patient as No. 217.

277. Flexion and supination imperfect.

279. Pronation and supination imperfect.

283. Same patient as No. 214. Both fractures caused by the crank of a hand car when under full speed; slight bend at seat of fracture.

284. This was a bend of both bones.

285. Radius bent forward at seat of fracture.

286. Four months since, broke radius and ulna, and three months after refractured them at same point. Now but slight bend of ulna, at seat of fracture.

288. Use of arm perfect; did not know it was shortened.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unit'd or not united	Amt. of short'ning.	Perfect or imperf'ct
289 Rad. & ulna	low $\frac{1}{2}$	simple	M. C.	40 y.	5 w.	u.		p.
290 " "	"	"	P. McE.	8 y.				p.
291 Carpal		comp.	S. L.	18 y.	1 y.	u.		ip.
292 Metacarpal of thumb	up $\frac{1}{2}$	simple	M. H.	26 y.	2 m.	u.		ip.
293 do. of finger	low $\frac{1}{4}$	comp.	H. H.	21 y.	1 y.	u.		p.
294 " "	low $\frac{1}{4}$	simple	W. P.	27 y.	14 y.	u.		ip.
295 2d and 3d phalanges		comp.	J. T.	7 y.	1 y.	u.		ip.
296 Ensiform cartilage	base	simple	H. B. R.	28 y.	12 y.	u.		ip.
297 7th cerv. vertebra	spin. process	"	R. L.	50 y.	9 w.	u.		ip.
298 Lumbar "		"	—	45 y.	36 y.	u.		
299 Lumbar		"	B.	25 y.	15 y.	u.		
300 Os innominatum		"	P. C.	40 y.		u.		ip.
301 Acetabulum		"	M. S.	40 y.			1 $\frac{1}{2}$ in	ip.
302 Femur	middle	comp. com.	J. A. B.	14 y.	17 y.	u.	1 in.	ip.
303 " "	"	"	P. H.	23 y.	38 y.	u.	1 $\frac{1}{2}$ in.	ip.
304 " "	low $\frac{1}{2}$	"	D. W.	20 y.	5 y.	u.	7 in.	ip.
305 " "	middle	compound	J. S.	18 y.	10 w.	u.	1 in.	ip.
306 " "	neck of f.	simple	J. B.	52 y.	3 m.	u.	1 $\frac{1}{2}$ in.	ip.
307 " "	"	"	T. K.	77 y.	1 y.	u.	3 in.	ip.
308 " "	"	"	J. S.	39 y.	6 y.	u.	2 in.	ip.
309 " "	"	"	L. F. T.	42 y.	6 w.	u.	$\frac{1}{2}$ in.	ip.
310 " "	"	"	W.	66 y.	5 w.	u.		ip.
311 " "	troc. mag.	"	B. R.	25 y.	2 m.	u.		ip.

291. While holding right hand over muzzle of a gun, it was discharged, and a charge of shot passed through the *semilunare, cuneiforme and unciforme*, removing part of these bones—can move all fingers except little finger.

292. Upper end of lower fragment is displaced slightly inward.

294. Fragments somewhat displaced.

295. Anchylosis—slight bend at seat of fracture; still very tender.

302. Did not use limb for one year. After eighteen months a piece of bone was removed. No halt.

304. Right femur was crushed by a heavy weight. Several pieces came out. Did not know limb was shortened more than three inches; by means of one inch added to the heel of right shoe, he walks with hardly any perceptible halt. When standing the ala of right side of pelvis is four inches lower than left and becomes almost six inches when he walks. This inclination of the pelvis is accomplished by a lateral curvature of the lumbar vertebrae.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unit/or not united	Amt. of short'ning	Perfect or imperf'ct
312 Femur	up	simple	M. C.	9 y.	31 y.	u.	1 1/2 in.	ip.
313 "	"	"	A. G.	35 y.	6 w.	u.	3 in.	ip.
314 "	"	"	J. C.	49 y.	7 w.	u.	3 in.	ip.
315 "	"	"	I. H. F.	30 y.	16 d.	u.	3 3/4 in.	ip.
316 "	"	"	J. R.	40 y.	1 m.	u.	3 in.	ip.
317 "	"	"	E. R.	42 y.	4 y.	u.	3 in.	ip.
318 "	"	"	J. M.	1 y.	1 y.	u.		p.
319 "	middle	"	J. G.	8 y.	22 y.	u.	1 in.	ip.
320 "	"	"	J. G.	14 y.	16 y.	u.	1 in.	ip.
321 "	"	"	M. H.	15 y.	19 y.	u.	1 in.	ip.
322 "	"	"	H. C.	16 y.	25 y.	u.	1 in.	ip.
323 "	"	"	D. K.	15 y.	8 y.	u.		p.
324 "	"	"	A. A.	22 y.	9 w.	u.	1 in.	ip.
325 "	"	"	A. S.	13 y.	19 y.	u.	1 1/2 in.	ip.
326 "	"	"	S. B.	3 y.	63 y.	u.		p.
327 "	"	"	J. G.	13 y.	6 w.	u.		p.
328 "	"	"	G. W. H.	43 y.	6 w.	u.	1 in.	ip.
329 "	"	"	M.	3 y.	5 w.	u.		p.
330 "	"	"	P. J.	15 y.	30 y.	u.		p.
331 "	"	"	G. S. L.	22	1 y.	u.	1 in.	ip.
332 "	"	"	E. S.	8 y.	5 y.	u.	1 in.	ip.
333 "	"	"	R.	9 y.	3 m.	u.		p.
334 "	"	"	M.	18 y.	8 w.	u.		p.
335 "	"	"	J. McE.	22 y.	1 y.	u.	3 in.	ip.
336 "	"	"	J. L.	35 y.	1 y.	u.	1 in.	ip.
337 "	"	"	J. L.	35 y.	3 m.	u.	1 in.	ip.

312. The overlapping of fragments at seat of fracture is very perceptible ; has but slight halt.

313. Had been fractured before at same point ; was shortened then, but more now ; straight splint was used and adhesive plaster extension.

315. Fourteen years since, broke left femur by a fall, while walking ; an empiric treated it nine weeks, with short splints, without obtaining union, patient then went to another empiric who treated it with pasteboard and leather splints, and in five weeks he was about. About one year after he refractured same bone ; it was treated in same manner by the empiric ; patient was up in fourteen days. Eight years since, he hurt same limb ; empiric said it was broke. Sixteen days since, he broke it a fourth time by jumping from a wagon. A surgeon was then called for the first time. Now united, but much bent at seat of fracture.

318. Bent forward at seat of fracture ; toes turned in a little.

319. 320. Same patient ; both fractures near same point ; straight splint was used.

322. No halt can be perceived in his walk.

323. Straight splint was used.

324. Straight splint and adhesive plaster extension.

325. No deformity ; no halt.

327. Straight splint and starch bandage was used.

328. Straight splint, and after three weeks the double inclined plane.

329. Dressed with roller and laid in a pillow.

332. Double inclined plane was used.

333. 334. Straight splint used, also in No. 331.

335. Union was delayed about eight weeks ; four months from the first fracture patient refractured it, by turning in bed, to which he was confined by a nonunited fracture of tibia and fibula. See No. 388. Union took place with great deformity after each fracture.

336. 337. Same patient. Straight splint was used each time.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Union'd or not union'd	Amt. of short'ning.	Perfect or imp' rect
338 Femur	middle	simple	D. C.	29 y.	1 y.	u.		ip.
339 "	"	"	J. T.	14 y.	12 w	u.	^{starch} in.	ip.
340 "	"	"	S. S. S.	27 y.	6 w.	u.		ip.
341 "	low $\frac{1}{4}$	"	A. M.	24 y.	30 y.	u.	$1\frac{3}{4}$ in	ip.
342 "	"	"	J. B.	8 y.	4 w.	u.		p.
343 "	"	"	P. H.	29 y.	4 m.	u.		p.
344 "	"	"	E. S.	42 y.	5 y.	u.	1 in.	ip.
345 "	"	"	H. H.	42 y.	7 w.	u.	^{starch} in.	ip.
346 Patella	trans.	"	E. L.	20 y.	7 w.	u.		p.
347 "	"	"	J. McC.	33 y.	2 y.	u. by lig.		ip.
348 "	"	"	W.	40 y.	9 w.	u.		ip.
349 "	"	"	C. A.	31 y.	4 m.	u.		ip.
350 "	"	"	J. D.	36 y.	3 m.	u.		ip.
351 Tibia	middle	comp.	R. P.	53 y.	4 y.	u.		p.
352 "	"	"	S. C.	28 y.	7 m.	u.	$\frac{1}{4}$ in.	ip.
353 "	low $\frac{1}{3}$	"	P. M.	28 y.	4 m.	u.		ip.
354 "	"	"	J. T.	17 y.	6 y.	u.		ip.
355 "	"	"	J. McD.	38 y.	3 y.	u.		p.
356 "	up $\frac{1}{4}$	simple	J. E.	42 y.	10 y.	u.		p.
357 "	up 1-6	"	R. P.	55 y.	7 y.	u.	$\frac{1}{4}$ in.	ip.
358 "	middle	"	R. J.	46 y.	10 m	u.		p.
359 "	"	"	G. P.	11 y.	6 w.	u.		p.
360 "	"	"	G. E.	17 y.	11 y.	u.		p.
361 "	"	"	M. H.	30 y.	4 y.	u.		p.
362 "	"	"	H. W.	17 y.	4 y.	u.		p.
363 "	"	"	J. T.	11 y.	16 y.	u.		p.
364 "	"	"	G. G.	18 y.	7 y.	u.		p.
365 "	"	"	L.	50 y.	2 m.	u.		p.
366 "	low $\frac{1}{3}$	"	M. C.	19 y.	2 m.	u.		p.
367 "	small. int.	"	W. G.	35 y.	6 w.	u.		p.

338. Leg pains him when used; not as strong as before.

339. Union did not take place in eleven weeks. Straight splint used.

340. Limb was shortened. Straight splint used.

341. Double inclined plane used.

342. 344. 345. Straight splint used.

347. Had splint on twenty-five days. Union by ligament $\frac{1}{4}$ inch in length.

348. Had splint on twenty-eight days; five weeks after first fracture refractured it while walking.

349. Sixteen weeks since, fractured patella, and eight weeks after refractured it at same point. Union by ligament $\frac{1}{4}$ inch in length as in No. 348 and No. 350.

352. Seven months after the accident, limb was quite lame, but improved after the removal of a small fragment of bone which was found to be loose.

353. Fracture extended into joint; astragalus and lower end of tibia became necrosed; ankle but little stiff.

356. A little bent at seat of fracture.

357. Same patient as No. 351, and also same leg. Head of fibula displaced; tibia bent at seat of fracture; limb pains when he walks; also see No. 374.

358. Slight bend at seat of fracture; pains him when used.

366. This patient at same time fractured the tibia and fibula of other leg, see No. 399. The starch bandage was here used as in case of No. 359 and No. 365.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unit'd or not united	Amt. of short'ning.	Perfect or imperf'lect
368 Fibula	middle	comp. com.	T. M.			n. u.		ip.
369 "	"	comp.	R. C.	20 y.	5 y.	u.		p.
370 "	"	"	H. W.	28 y.	14 y.	u.		p.
371 "	up $\frac{1}{8}$	simple	—	60 y.	3 w.	u.		p.
372 "	low $\frac{1}{8}$	"	C. B.	34 y.	14 y.	u.		p.
373 "	"	"	I. S.	34 y.	25 y.	u.		p.
374 "	low $\frac{1}{8}$	"	R. P.	57 y.	4 w.	u.		p.
375 "	"	"	J. R.	32 y.	3 m.	u.		ip.
376 "	"	"	H. B.			u.		ip.
377 "	"	"	J. S.	10 y.	4 y.	u.		ip.
378 "	"	"	J. S.	32 y.	20 y.	u.		ip.
379 "	ext. mall.	"	A. G.	21 y.	8 m.	u.		ip.
380 Tib. & Fib.	middle	comp. com.	J. O.	48 y.	3 m.	u.	$\frac{1}{2}$ in.	ip.
381 "	low $\frac{1}{2}$	"	H. K.	14 y.	7 m.	u.	2 in.	ip.
382 "	low 1-6	"	C. S.	50 y.	6 m.	u.		ip.
383 "	low $\frac{1}{2}$	"	J. B. F.	39 y.	6 y.	u.	1 in.	ip.
384 "	up $\frac{1}{2}$	comp.	I. R.	34 y.	8 y.	u.	3 in.	ip.
385 "	middle	"	T. C.	4 y.	23 y.	u.	3 in.	ip.
386 "	"	"	J. W.	11 y.	1 y.	u.		p.
387 "	"	"	W. K.	35 y.	4 m.	u.		p.
388 "	"	"	J. McE.	22 y.	1 y.	u.	1 $\frac{3}{4}$ in.	ip.
389 "	low $\frac{1}{2}$	"	A. R.	30 y.	2 y.	u.	$\frac{1}{2}$ in.	ip.
390 "	"	"	M. F.	24 y.	3 m.	u.		ip.
391 "	"	"	G. W. B.	33 y.	1 y.	u.	$\frac{1}{2}$ in.	ip.
392 "	"	"	W. H.	32 y.	22 y.	u.		p.

371. The tendency to displacement was so little, that no splint was used ; the horizontal posture was sufficient to keep fragments in place—caused by direct blow—no displacement of tibia.

374. See No. 351 ; 357 same patient.

375. Tibia was dislocated inward ; still remains slightly displaced. Fibula bent at seat of fracture. Ankle swollen.

376. Tibia dislocated inward, unreduced.

377. Foot turned slightly out ; some enlargement above malleolus externus.

378. There is some deformity ; ankle at times becomes swollen and painful.

379. Foot turns in ; lower fragment displaced downward.

380. Starch bandage and the double inclined place was used. Union was not complete on the 25th day, but patient was allowed to get up on 39th day. Dr. H. saw him again two years after and found ankle and knee stiff.

382. Bones united in four weeks. Had ulcer on heel.

384. Both bones much bent at seat of fracture.

385. Has remained fistulous, fragments of bone have frequently been removed from the wound.

386. Starch bandage used.

388. On the first day of work after an attack of bilious fever, a bank of earth fractured his left tibia and fibula, and also his right femur, see No. 335. Soon ulcers formed on both heels ; about thirteen weeks after accident erysipelas appeared on the left leg. Union took place in eleven months, with deformity ; the lower end of upper fragments override the upper end of lower fragments.

389. Bones project at seat of fracture. Six months since a slight injury at that point produced an ulcer, which is still open.

390. Bones much bent, lower end of upper fragment projects anteriorly. Heel bent backward. Skin over seat of fracture red and tender, appears as if it would ulcerate.

391. Ankle stiff and swollen. Patient did not know limb was shortened.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unf'd or not unf'd	Amt. of short'ning.	Perfoot or jump/ribs
393 Tib. & Fib.	low $\frac{1}{2}$	comp.	I. O. B.	26 y.	5 m.	u.	$\frac{1}{2}$ in.	ip.
394 "	"	"	P. B.	25 y.	6 m.	u.	$\frac{1}{2}$ in.	ip.
395 "	"	"	M. S.	40 y.	—	n. u.		ip.
396 "	" & up 1-6	"	B. G. McK.	39 y.	17 y.	u.	$1\frac{1}{2}$ in.	ip.
397 "	low $\frac{1}{2}$	"	M. F.	31 y.	2 y.	n. u.		ip.
398 "	up $\frac{1}{2}$	simple	—	25 y.	8 w.	u.		p.
399 "	mid. & low $\frac{1}{2}$	simp. & com.	M. C.	19 y.	2 m.	u.		p.
400 "	up $\frac{1}{2}$	simple	E. T.	21 y.	5 y.	u.	$\frac{1}{2}$ in.	ip.
401 "	"	"	T. L.	—	—	u.	$\frac{1}{2}$ in.	ip.
402 "	middle.	"	W. B.	38 y.	8 m.	u.	$\frac{1}{2}$ in.	ip.
403 "	"	"	E. V.	10 y.	15 w.	u.		p.
404 "	"	"	W. McC.	19 y.	3 m.	u.		p.
405 "	"	"	W. R.	38 y.	5 w.	u.		p.
406 "	"	"	J. M.	16 y.	8 w.	u.		p.
407 "	low $\frac{1}{2}$, up $\frac{1}{2}$	"	R. S.	46 y.	4 m.	n. u.	$\frac{1}{2}$ in.	ip.
408 "	low $\frac{1}{2}$	"	I. H. L.	52 y.	25 y.	u.	$\frac{1}{2}$ in.	ip.
409 "	"	"	J. H. L.	15 y.	5 w.	u.		p.
410 "	"	"	E. McD.	35 y.	6 w.	u.	$\frac{1}{2}$ in.	ip.
411 "	"	"	A. T.	49 y.	2 y.	u.	$\frac{1}{2}$ in.	ip.
412 "	"	"	T. F.	38 y.	2 y.	u.	$1\frac{1}{2}$ in.	ip.
413 "	"	"	T. S.	40 y.	8 w.	u.	$\frac{1}{2}$ in.	ip.
414 "	"	"	T. H.	3 y.	6 w.	u.		p.
415 "	"	"	I. C. C.	14 y.	8 y.	u.		p.
416 "	"	"	J. K.	16 y.	8 m.	u.		p.
417 "	"	"	J. J.	22 y.	6 m.	u.		p.
418 "	"	"	A. A. H.	32 y.	1 y.	u.	$\frac{1}{2}$ in.	ip.
419 "	"	"	J. H.	25 y.	2 y.	u.	$\frac{1}{2}$ in.	ip.
420 "	"	"	J. P. W.	—	4 m.	u.		p.
421 "	low 1-6	"	E. B.	42 y.	8 y.	u.	$\frac{1}{2}$ in.	ip.
422 "	low $\frac{1}{2}$	"	E. B.	50 y.	3 m.	u.	$\frac{1}{2}$ in.	ip.
423 "	low 1-6	"	I. T.	44 y.	24 y.	u.		p.

393. A fragment of bone was removed.

394. Health bad before accident. Much bent at seat of fracture, has an ulcer over seat of fracture.

395. The attempt was made for fifteen days to save limb, then amputation was performed.

396. Lower end of upper fragment of tibia projects over lower fragment. Patient did not rest any weight upon his limb until after four months, and it was not until after the fifth month that he attempted to do without his crutches. Pains him at seat of fracture; motion of ankle not quite as free as the other.

397. Amputated five weeks after fracture.

399. See No. 367: starch bandage used in both fractures.

402. Upper end of lower fragment is anterior to lower end of upper; foot weak: ~~uses~~ crutch. Here as in No. 266 and No. 267, the starch bandage was used.

406. There was no displacement of the bones.

407. Lower end of upper fragment is anterior to upper end of lower fragment; union not yet complete; used extension twenty-five days, then starch bandage. Had ulcer on heel.

408. Bent at seat of fracture; ankle stiff, enlarged; pains more and more each year.

410. There is an anterior and lateral displacement of the lower end of upper fragment.

417. It remains much swollen.

413. 416. Slightly bent at seat of fracture.

421. 422. These are one person.

NAME OF BONES.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time since it occurred.	Unif'd or not united	Amt. of shortning.	Perfect or imp'fect
424 Tib. & Fib.	low $\frac{1}{2}$	simple	A. V.	37 y.	25 y.	u.	$\frac{1}{2}$ in.	ip.
425 "	"	"	H. M.	43 y.	10 w.	u.	$\frac{1}{2}$ in.	ip.
426 "	"	"	E. H.	27 y.	6 w.	u.	"	ip.
427 "	"	"	M. H.	54 y.	4 w.	u.	$\frac{1}{2}$ in.	ip.
428 "	low 1-6	"	J. H.	6 $\frac{1}{2}$ y.	4 m.	u.	"	p.
429 "	low 1-6 and int. mall.	"	I. B.	33 y.	30 y.	u.	"	p.
430 "	up $\frac{1}{2}$	"	F. C. T.	35 y.	8 w.	n. u.	$\frac{1}{2}$ in.	ip.

424. Foot turned in very much.

429. Ankle ankylosed. Pains him more now than it did years ago ; limb emaciated.

430. This patient, never a stout, robust man, is now pale, skin cold and moist ; under excitement his pulse varies in a short time, from 100 to 140. Leg enormously swollen and œdematous ; no union ; upper end of lower fragment is in front of lower end of upper fragment. Has been treated with the lateral splints.

The following *thirty-one* cases are drawn also from Dr. H.'s notes, being cases in which death or amputation followed the fracture, and on that account were not admitted to the other tables :

NAME OF BONE.	Point of Fracture.	Character of Frac.	Name of Patient.	Age then.	Time of Amputa'n.	Point of Amputa'n.	Result.	Time of Death.
431 Inf. Maxilla,	up $\frac{1}{2}$	comp. com.	J. G.	31 y.		up $\frac{1}{2}$	d.	12th d.
432 Humerus,	"	"	K.	3 y.	17 d.	"	"	"
433 "	middle	"	E. H.	21 y.	19 h.	"	r.	"
434 "	"	"	E. H.	21 y.	18 h.	"	r.	"
435 "	"	"	W. L.	28 y.	4 h.	shoulder	r.	"
436 "	"	simple	J. C.	35 y.	"	" joint	r.	"
437 Radius & Ulna,	low $\frac{1}{2}$	comp. com.	I.	27 y.	6 h.	middle	r.	"
438 "	middle	"	F. P.	19 y.	14 h.	humerus	d.	15 h.
439 Carpus, etc.,	"	"	Hollander,	11 y.	soon	low $\frac{1}{2}$ of rad. and ulna	d.	36 h.
440 "	"	"	H. C.	20 y.	"	rad. & carp. artic.	r.	"
141 "	"	"	H. C.	22 y.	12 h.	wrist joint	r.	"
442 Phalanx,	2d	"	Boy,	12 y.	18 h.	2d artic.	r.	"
443 "	2d	"	"	12 y.	"	mid. 2d phal.	r.	"
444 "	3d	"	B.	20 y.	soon	last artic.	r.	"
445 "	1st	"	P. H.	28 y.	"	m. carp artic.	r.	"
446 "	1st	"	"	28 y.	"	"	r.	"
447 "	2d	"	"	28 y.	"	1st phalanx.	r.	"
448 "	3d	"	B.	40 y.	6 h.	2d artic.	r.	"
449 "	2d and 3d	"	S. C.	25 y.	5 w.	1st artic.	r.	"
450 "	"	"	L. R.	30 y.	12 h.	"	r.	"
451 "	"	"	"	30 y.	"	"	r.	"
452 "	"	"	"	30 y.	"	"	r.	"
453 "	1st, 2d & 3d neck	"	J. W.	45 y.	4 w.	metacarpus	r.	"
454 Femur,	middle	comp. com.	J. O'K.	40 y.	"	"	d.	6 h.
455 "	"	comp. com.	H. B.	21 y.	"	"	d.	34 h.
456 "	"	simple	"	21 y.	"	"	d.	34 h.
457 "	"	commn.	J. N.	30 y.	5 h.	up $\frac{1}{2}$	d.	8
458 "	"	comp. com.	A. M.	30 y.	5 w.	middle	r.	"
459 Tibia & Fibula,	"	"	J. S.	30 y.	"	"	"	"
460 "	"	"	I. B.	3 y.	soon	up $\frac{1}{2}$	d.	9 d.
461 "	up $\frac{1}{2}$	"	D. R.	40 y.	7 h.	above knee	d.	3 h.

SUMMARY.

OF THE CASES REPORTED BY DR. HAMILTON.	POINTS OF FRACTURE.			CHARACTER OF FRACTURE.			RELATIVE NO. AT ALL AGES.													
	No. of Fractures.	No. of Perfect.	No. of Imperfect.	Upper or outer †.	Middle.	Lower ‡.	Comp. & Commin.	Comp.	Simple	To 15 years.		15 to 30 years.		30 to 45 years.		45 to 60 years.		60 to 82 years.		
										No.	P.	No.	P.	No.	P.	No.	P.	No.	P.	
Osse Nasi,	3	..	3	1	2	2	..	1
Vomer,	1	..	1	1	1
Inf. Maxilla,	5	3	2	*	5	1	1	2	2	1	..	1
Clavicle,	18	6	12	15	3	4	14	3	2	7	2	7	2	1
Scapula,	1	..	1	†	1
Humerus,	14	5	9	1	2	11	2	..	12	8	4	2	2	3	..	1	1
Radius,	8	5	3	1	1	6	8	4	3	2	2	3
Ulna,	9	8	1	2	3	4	..	2	7	4	4	3	3	2	1
Radius & Ulna,	13	7	6	1	1	10	..	1	11	8	5	2	2	2
Femur,	29	4	25	10	13	1	27	7	2	6	1	9	1	3	4	..
Patella,	2	..	2	2	1	..	1
Tibia,	2	1	1	1	..	1	..	1	1	..	1	1	1
Fibula,	4	2	2	4	..	2	2	1	3	2
Tibia and Fibula,	22	5	17	2	9	10	9	5	7	6	2	5	1	10	2	1
Amount,	131	46	85	33	32	47	16	16	95	46	23	34	13	39	8	8	2	4
<i>Of the Supplement.</i>																				
Osse Nasi,	6	1	5	2	4	3	1	2
Septum Nasi,	3	..	3	3	2	..	1
Sup. Maxilla,	1	1	1	1	1
Inf. Maxilla,	8	4	4	1	3	1	1	1	5	2	2	1
Clavicle,	23	9	14	17	6	23	7	6	2	1	7	..	6	2
Scapula,	2	1	1	†		2	1	1	1	..
Humerus,	24	12	12	4	5	15	..	1	24	10	4	3	3	7	4	2	1	2
Radius,	19	12	7	2	3	14	..	2	17	8	6	5	3	4	2	2
Ulna,	14	6	8	§	3	2	13	6	4	5	2	2	..	1
Radius and Ulna,	21	18	3	1	10	10	..	1	20	15	13	2	1	3	3	1	1	..
Femur,	44	10	34	13	25	6	3	1	40	16	8	4	2	10	..	2	2	..
Patella,	5	1	4	5	..	1	1	4
Tibia,	17	13	4	2	10	5	..	5	12	2	8	5	3	3	4	3
Fibula,	12	6	6	1	3	8	1	2	9	1	4	2	3	2	2	2
Tibia and Fibula,	51	18	33	5	11	35	4	14	33	5	3	10	4	12	3
Amount,	250	112	138	45	76	92	8	31	207	76	48	53	26	58	19	19	8	6	2	..

* 5 Shaft. † Acro. p. ‡ Corocoid p. 1. § Olec. 4. ¶ Below spine.

SUMMARY OF TABLES OF ALL THE CASES FROM THE NOTES OF DR. HAMILTON.

NAME OF BONES.	POINTS OF FRACTURE.			CHARACTER OF FRACTURE.			RELATIVE NO. AT DIFFERENT AGES.													
	No. of Fractures.	No. of Perfect.	No. of Imperfect.	Upper or outer $\frac{1}{2}$.			Comp. & Commin.			To 15 years.		15 to 30 years.		30 to 45 years.		45 to 60 years.		60 to 82 years.		
				Middle.	Lower $\frac{1}{2}$.	Comp.	Comp.	Simple.	No.	P.	No.	P.	No.	P.	No.	P.	No.	P.		
Ossa Nasi,.....	9	1	8	3	6	5	1	3
Septum Nasi,.....	4	1	3	4	3	1
Sup. Maxilla,.....	1	1	1
Inf. Maxilla,.....	13	7	6	6	3	2	2	7	4	3	1	1
Clavicle,.....	41	15	26	32	9	4	37	10	8	9	3	14	2	7	2
Scapula,.....	3	1	2	3	1	1
Humerus,.....	38	17	21	5	7	26	2	1	2	36	18	8	5	3	10	4	3	2	2	..
Radius,.....	27	17	10	3	4	20	2	2	2	25	12	9	7	5	6	2	2
Ulna,.....	23	14	9	10	6	6	2	20	10	8	8	5	4	1	1
Radius and Ulna,.....	34	25	9	2	11	20	2	31	23	18	5	3	5	3	1	1
Femur,.....	73	14	59	23	38	7	3	1	67	3	23	10	10	3	19	1	5	..	6	..
Patella,.....	7	1	6	7	2	1	4	..	1
Tibia,.....	19	14	5	3	10	6	6	13	3	2	9	5	3	3	5	4
Fibula,.....	16	8	8	1	3	12	1	4	11	2	2	..	4	2	6	4	2	2
Tibia and Fibula,.....	73	23	50	7	20	45	13	19	40	11	5	15	5	22	5	1
Carpal,.....	1	..	1	1	1
Metacarpal,.....	3	1	2	1	..	2	1	2	1	..	2	1
Phalanges,.....	1	..	1	1	..	1
Ribs,.....	4	2	2	4	1	..	3	2
Vertebra,.....	3	3	2	1
Pelvis,.....	2	..	2	1	2
Amount,.....	395	161	231	87	108	144	26	49	312	126	71	103	40	102	28	29	10	10	2	..

SUMMARY OF BONES OF SKULL FROM THE NOTES OF DR. HAMILTON, &c.

NAME OF BONES, &c.	No. of Fractures.	CHARACTER OF FRACTURE.					RELATIVE NO. AT DIFFERENT AGES.													
		Recovered.	Died.	Comp. & Commin.	Compound.	Simple.	To 15 yrs.		15 to 30 y.		30 to 45 y.		45 to 60 y.		60 to 82 y.					
							N.	R.	N.	R.	N.	R.	N.	R.	N.	R.				
Os Occipital,.....	12	1	1	..	1	1	1	1	1
Os Parietal,.....	12	13	5	7	8	3	12	11	3	1	3	1	3	1
Os Frontis,.....	9	5	4	3	5	1	4	2	2	..	2	..	2	1	1	..
Os Temporal,.....	1	1	1	1	1
Occipital and Frontal,.....	1	1	..	1	1	1
Os Frontis and Ethmoid,.....	1	..	1	1	1
Os Frontis, Ethmoid, Sphenoid and Temporal,.....	1	..	1	..	1	1
Amount,.....	33	21	12	12	15	6	18	14	7	3	7	3	7	3	1	1
Fractures of the Cranium at the N. Y. Hospital, by Dr. T. D. Lente,.....	117	21	96	13	4	64	11	39	6	9	..	2

SUMMARY OF BONES OF SKULL.—I have taken the liberty of forming a summary from the valuable article "Lente's Statistics of Fracture of the *Cranium*," published January last in the "New York Journal of Medicine and the Collateral Sciences." In those tables *one hundred and twenty-eight* cases are reported, of which the result in *three* cases was not given, and in *eight* others the age was wanting; on this account I have omitted these in my summary of his tables. These results differ from those obtained from Dr. Hamilton's notes, but the reader, if he examines the cases in both tables will perceive that those recorded by Dr. Lente are such as have been brought into the *New York Hospital* during the last twelve years, the result of *recent* injuries, and also that the proportion of those under *fifteen* years of age is very small compared to the other ages, while in Dr. Hamilton's notes more than one half occurred in the *first fifteen* years of life; also Dr. Hamilton's notes are *in part* composed of cases, which have come under his eye, after the lapse of several years from the time of the accident, and they cannot therefore so faithfully represent the average result of such injuries, since none but survivors are accounted for.

In a medico-legal point of view, the following suggestion of Dr. Lente, in the same article, is of value: "In no case did death follow the receipt of the injury until after the lapse of some hours, even in the most desperate cases; nor does it appear to be possible for an ordinary blow upon the head, producing fracture of the skull, to cause immediate death." "In a recent criminal trial of great interest, it will be recollected that, at one stage of the proceedings, it was much discussed whether a blow upon the head with an ordinary weapon capable of inflicting death, could produce this result instantaneously; many eminent surgeons were examined, and the general impression was that the thing was exceedingly improbable, if not impossible, and the question was thus decided." A suggestion, that the reader will see is equally applicable to the notes of the thirty-three cases by Dr. Hamilton.

In the March number of the same Journal, Stephen Smith, M. D., in an article entitled "Smith on Surgical Treatment of Epilepsy," exhibits one of the frequent results of fractures of the cranium, as illustrated also by Dr. Hamilton's tables. In twenty-seven cases of epilepsy reported by Smith, twenty-four were known to have resulted from fracture of the bones of the head.

The following additional remarks, although somewhat desultory, seem sufficiently pertinent:

Children, or for the sake of a convenient reference, I will say those of the *first period* in Dr. Hamilton's tables (the first fifteen years) show a large percentage of perfect results, in all cases of fractures.

Ossa Nasi.—Dr. Hamilton has observed that the accident called “fracture of the ossa nasi” is generally rather a displacement; the fracture being only at the serrated margins, where they unite with each other and with the neighboring bones.

It is a curious fact that only a small proportion of these patients apply to a surgeon for treatment. In the notes I find recorded fourteen cases, one of which died within a few days from other injuries, and seven others employed no surgeon, and have not therefore been admitted to these tables.

Clavicle.—It will be seen by the summary, that a large proportion of the fractures of this bone, are at the inner end of the outer $\frac{1}{3}$. Also that the number of perfect, after the first fifteen years of life is very small.

A great proportion of the fractures of carpal, metacarpal, phalangeal, tarsal and metatarsal, and also vertebral bones, occurring in Dr. Hamilton’s practice, or coming under his observation, have not been recorded. The tables are not, therefore, a correct representation of their relative frequency.

The New York Journal, before alluded to, vol. iii, 1840, speaks in very high terms of “Remarks on Fractures, by A. L. Pierson, M. D., of Salem, Mass.,” which contains not only remarks, but also tables “drawn from the records of the Mass. General Hospital,” “of all the cases of fracture ever treated in that institution.” Also the Boston Med. and Surg. Jour., Aug., 1840, makes honorable mention of those tables. I regret that I have not had the opportunity of examining them.

In the same Journal, vol. iv., page 473, is a short article entitled “Cases of Fracture in the General Hospital of Hamburg, during the year 1838, by Fricke.” “The whole number treated was *seventy-two*. Of these, forty-two were cured, ten died, and twenty remained under treatment.” Between 10 and 30 years there were twenty fractures; from 30 to 50 years, twenty-four fractures; 50 to 70 years, twenty-two fractures; 70 to 90 years, six fractures. Also the tibia and fibula were broken *twenty-two* times, the femur *seventeen* times, os brachii *seven*, the clavicle *three* times — the *shortest term of treatment* was for the os nasi, *nineteen days*; and the *longest* for the neck of os femoris, *one hundred and thirty-nine days*.

The author states that of the seventy-two, six remained from the last year; of the sixty-six who entered, fifteen were females.

Lente, N. Y. Jour., vol. iii, page 167, gives a statement of 1548 fractures, of which 1366 were males, and 182 females. I have not been able to determine the relative proportion in which fractures occur of the sexes: these, being the only statements I have been able to find on this point.

The only records I have found of the number of fractures drawn from

body of men, were the books of the Auburn State Prison. They show that of *five hundred and eighty* convicts admitted, *one hundred and twenty* reported at time of admission, ancient fractures.

The subject of *results*, which occupies the most prominent place in Dr. Hamilton's tables, is not considered in Fricke's table, nor am I informed that it constitutes a point in Pierson's tables, or in any others hitherto constructed. I am at least quite certain that by no one else, have the results been determined by measurements.

"Lente's Statistics of Fractures" published in the September number of the N. Y. Journal, give the "age, sex, season, and seat of fracture, &c.," in those fractures occurring in the N. Y. Hospital for the last twelve years, but in his tables, he makes no mention of the amount of shortening in the different fractures. To show the relative frequency of fractures of the different bones, he institutes "a comparison with the statistics of Malgaigne, at the Hotel Dieu of Paris, Lonsdale, at the Middlesex Hospital of London," and Norris, at Pennsylvania Hospital of Philadelphia; from which, as well as from Fricke, at the General Hospital at Hamburg, and Dr. Hamilton's notes, I shall make the following abridgement by way of comparison.

NAME OF BONES.	N. Y. Hospital.	Pennsylvania Hos- pital.	Hotel Dieu.	Middlesex Hospi- tal.	General Hospital at Hamburg.	Dr. Hamilton.
Thigh,.....	230	134	308	181	17	73
Tibia and Fibula,.....	42		515	197	22	73
Tibia,.....	45	293	29	41	5	19
Fibula,.....	92		108	51	4	16
Arm,.....	161		310	118	7	38
Radius and Ulna,.....	90	227	107	93	2	34
Ulna,.....	36		29	64	..	23
Radius,.....	143		160	197	1	27
Clavicle,.....	158	84	225	273	3	41
Inf. Maxilla,.....	65	19	27	32	1	13
Pelvis,.....	23	6	7	7	3	2
Patella,.....	30	16	45	38	1	7
Scapula,.....	17	10	4	18	..	3
Sternum,.....	12	5	1	2	1	1
Skull,.....	128	46	53	48	..	33
Amount.....	1722	840	1939	1392	67	403

It is thus by comparison of the cases occurring both here and elsewhere, that I have endeavored to arrive at the general truths, which can only be obtained from facts collected from various sources. It is by comparison of the *results* in the tables first published by Dr. Hamilton, with those I have

compiled from his notes, and also by comparing both with other tables, that I have sought to prove their accuracy as statistical records. There will be seen a striking similarity in the average results between the tables in the case of almost every bone; the relative frequency of fracture being nearly identical, as stated by the different writers. It is true the statistics of Middlesex Hospital present a much larger proportion of fractures of the superior extremities than the other tables; but when it is ascertained that the number of beds in this Hospital is limited, and that a large proportion of those treated are "out-door patients," this disagreement will find a sufficient explanation.

ART. IV.—*Report on the immediate or exciting cause of the Asiatic Cholera, which suddenly appeared with great malignancy, on Ellicott street, in Buffalo, on Monday, Tuesday, and Wednesday, the 26th, 27th, and 28th of July, 1852. Read before the "Buffalo Medical Association," at its regular meeting, Aug. 2d, 1852.* By FRANK H. HAMILTON, M. D.

On Monday, Tuesday, and Wednesday of last week, the 26th, 27th, and 28th ult., the Asiatic Cholera made a sudden and fearful irruption into one of our most healthy neighborhoods: and after a brief but fatal sojourn its departure was announced almost as suddenly as its accession.

In this simple event, those who have read the history, or themselves witnessed the eccentric wanderings of this scourge, will see nothing new or extraordinary. The circumstances which herald its approach, and the laws which determine its direction, hitherto been only feebly ascertained. It has defied everywhere, at all times, quarantines, cordon sanitaires, climate, season, and meteoric influences — no person can be so vigilant and prudent in his habits as to secure an insurance — no place is so favored as to possess an unqualified immunity. If to-day it seems to be restrained by natural or artificial lines, to-morrow it may overleap all barriers, and swift as a passing cloud, may throw its shadows successively upon the valley and the mountains, upon the city and the plain.

Yet, notwithstanding all its apparent irregularities, no intelligent man doubts, I presume, that the Asiatic cholera possesses habitudes as precise and distinctive as those which belong to any other disease; and that all its movements are directed by laws as exact and inviolable as those which control any other events. All its paradoxes are the common results of consistent causes. It never makes a foray without provocation, or retires until compelled by overpowering forces. The causes exist, and it only remains for us

to ascertain, and having ascertained, to remove them, that the effects may cease.

I remind you, gentlemen, of these indisputable truths, which no one is simple enough to question, because we do not always act as if we fully believed them. We see in the cholera so many phenomena yet unexplained, that we are inclined to distrust every thing pertaining to it — and to doubt those rational explanations which fortunate coincidences, and careful investigations have furnished us. We become perplexed and bewildered in its seeming inconsistencies, and finally declare the whole a mystery — and refuse any longer to accept of arguments. In this way I seek to explain the incredulity which some of my professional brethren, whose opinions I am accustomed to respect, have manifested in relation to the immediate source of the cholera as it appeared on the 26th, 27th, and 28th ult., in Ellicott street.

To me it seems too palpable to admit of a question, that it had its cause in the ditch which, commenced at Eagle street on Saturday, was finished through to South Division on Wednesday morning.

Permit me, gentlemen, if you please, to give my reasons for this belief.

Not many years ago a marsh occupied the ground where this street is built, covered with a deep, soft, alluvial mould. The marsh extended from near Washington street to about where now Michigan street lies, and from Goodell street to Swan. It had its outlet toward the corner of Swan and Michigan, or in that direction. This marsh was the result of a peculiar formation of the clay bed, which to the depth of ten or twenty feet was nearly all that portion of the city which is east of Main street and below High street. The strata having a dip toward Main and Michigan, from Goodell and Swan toward a common centre, this clay basin, only partly filled in its deepest portions with sand, there was no actual outlet, except the slight depression toward the south-east: and it remained therefor, until intersected by ditches, the depot for all the surface drainage of the higher neighborhoods — a general receptacle for all the rain water and sewers. Where the Irish catholic church now stands there was formerly a pond, and lower down along where Ellicott extends toward South Division the ground was nearly, but not quite as low.

Upon this soil much of that portion of the town is built; for in paving the streets, with few exceptions, none of the surface earth was removed, but the sand was deposited for pavements, above it. The streets thus became higher than the adjoining lots, and the water being thrown back upon them the owners found it necessary to fill them up. So this whole bed of alluvium, &c., was at length buried up, and there it has remained to the present time. No

less rich and fertile, and redolent of disease, however, to-day, than before it was inhumed — when it was regarded as unsafe for any family to live within the reach of its miasms.

Since the pavements were laid, the lots filled up and the sewers made, this part of the town has been as healthy as any of those portions which are underlaid with clay — indeed much more so, I think. Ellicott street, especially, and particularly at this southern extremity, has been regarded as healthy. In 1849, fewer deaths from cholera occurred in this street than in Washington, Elm, Oak or in any other parallel street of equal length; and I am informed by a resident that not one death occurred from this cause, in that portion of the street of which we are now speaking. And this fact may be explained by the size and comfort of the dwellings, which are mostly brick; by the neatness and spaciousness of their yards, which affords them sufficient ventilation; by the cleanliness of their street, and the completeness of their sewerage, which last possesses also, I am told, this remarkable advantage over other sewers, that it has running through it most of the year, if not constantly, a fresh current of water, which finds its supply in springs around the foot of court house hill, and from other parts of the clay basin in that vicinity.

In short, for several years the occupants of these houses have enjoyed that immunity from epidemic and other diseases which the science of etiology would have taught us to expect for them, and to which their own diligence in the abatement of the usual causes has eminently entitled them.

During the present season the reputation of this locality for healthfulness remained unchanged. While the cholera has been here and there in other portions of the city, and especially in the low parts of the town, and at the eastern extremity toward the “Hydraulics,” where the same geological structure prevails, viz., a clay subsoil, with less pavements and sewerage, and less comfort in the style of the pavements, still here were no indications of disease; not even the ordinary diseases of summer were known to have prevailed.

The long and persevering drought, with an unusual degree of heat brought no change.

On Saturday, July 24th, a ditch was commenced at Eagle street, four and a half deep, and two feet wide, for the purpose of laying water pipes. The work was regularly carried on through Saturday, Monday, Tuesday, and a part of Wednesday forenoon. On Monday night it was partly open near the corner of North Division and Ellicott. Wednesday morning it was opened to South Division street.

The length of the ditch was about 200 yards, and the number of dwellings fronting upon the street, from Eagle to South Division was twenty.

The soil through which the ditch was dug, was, directly underneath the pavement, a coarse sand of about one foot in depth, then a rich loam averaging about one foot also; and underneath this, sand of a reddish or yellow color, either coarse or fine at different points. The clay bed beneath was not reached.

Mr. I. P. occupies a recently built and very comfortably constructed brick house, on the north-west corner of Ellicott and North Division streets. His house is well ventilated and his cellars sufficiently drained.

On Monday evening, July 26th, Mrs. P., who had been in feeble health for some months, but with no intestinal disease, was attacked with a slight diarrhoea. On Tuesday it returned with increased severity, and on Wednesday during the forenoon, cholera was distinctly announced. She died Friday morning at 2 o'clock.

No other members of the family were attacked.

Mr. O. H. P. W., with his family, consisting of himself and wife, three children, an apprentice and a servant girl, lived in the new brick house, No. 35 Ellicott, on the east side, and three doors north of North Division street. The house is two stories and a half high, with an open, clean yard, and is well constructed for ventilation. The cellar is well drained.

Monday night the family all slept in the house, with more or less of their windows open.

Tuesday morning Mr. W. arose feeling ill, and with a diarrhoea. During the day he had ten or twelve copious watery evacuations, but having taken medicine and rest, the disease was arrested, and never returned with severity.

On the same morning, Tuesday, Mrs. W. awoke with a severe headache, and early in the afternoon a diarrhoea commenced, which followed her, accompanied with unequivocal symptoms of Asiatic cholera, until the next day, at 5, P. M., when she died. The signs of cholera did not supervene, however, until about midnight of Tuesday.

John Berry, the apprentice, slept at Mr. W.'s Monday night, and Tuesday morning went to the Falls — returning Tuesday night on the boat, he felt sea-sick, as he thought. He reached home between 9 and 10, and retired without mentioning his illness. In the night, about 11 o'clock, he had occasion to get up and the family discovered his condition. From this time until 10, A. M., of Wednesday, when he died, he had unequivocal Asiatic cholera.

No other members of the family fell sick on Tuesday.

It has been thought that the currant pudding eaten by some of the family

on Tuesday, at dinner, might have caused the cholera; but Berry was not at dinner at Mr. W.'s. Mr. W.'s diarrhoea commenced in the morning; and Mrs. W. awoke quite ill, although her diarrhoea did not commence until after dinner. Nor was it a thing unusual for the family to have currant puddings and other similar fruit deserts at dinner.

Wednesday morning I. W., about two years old, was attacked as early as 6 o'clock, with vomiting and purging. In the afternoon he was sent out of the house, and died on the Sunday or Monday following, the 1st or 2d of August. His symptoms were finally those of dysentery.

H. W., aged about four or five years, was attacked at his father's house, as early as 12 o'clock, at noon, of Wednesday — perhaps earlier, and died about 5 o'clock the next morning.

M. W. was attacked some time during the day on Wednesday — probably in the forenoon. He was removed from the house, and is now, August 2d, convalescing.

Mr. E. C., aged about sixty years, was attacked at 2 o'clock, A. M., of Wednesday, and died at half past 1 of the same day. He occupied the house No. 27, east side of Ellicott.

Mrs. C., his wife, was attacked about 11, A. M., of same day, Wednesday, and died at 11, A. M., on Thursday.

Miss C., daughter of Mr. C., aged twenty-four years, returned from a visit of a week at the Falls, on Saturday afternoon. Tuesday, before the cholera had appeared in the family, she felt quite ill, but had no cholera symptoms. Thursday she left town with the family for Lima, and was taken with Asiatic cholera at this latter place, on Friday evening, and died about one week after.

Elizabeth Tres, the servant girl of Mr. W., (35 Ellicott,) aged seventeen years, was attacked Wednesday night, or early Thursday morning, and died at about 5 o'clock, P. M., on Thursday.

Mr. J. R. E. resides at 31 Ellicott, N. E. corner of North Division and Ellicott, in a two and a half story frame house. Mr. E. and two of his children were attacked, I am informed, with diarrhoea and dysenteric symptoms, on Wednesday or Thursday, and are recovering.

Mr. E. T. B., resides at 24 Ellicott, on the west side. His daughter, aged nine years, was attacked with diarrhoea, copious and watery, on Thursday, P. M., at about five or six o'clock.

Mrs. B. was attacked Thursday at midnight, in the same manner. Both are now convalescent.

Mr. M. resides at 25 Ellicott, east side. Mrs. M. was attacked Thursday morning at three o'clock, with nausea, prostration and diarrhoea.

Her daughter had a slight illness of the same character and at the same time.

It is entirely pertinent to include in this enumeration the case of Mrs. T., who having eaten freely of cherries on Sunday, was attacked at her house, No. 22 Ellicott, west side, with a very profuse diarrhoea of the rice water character; and the same returned, with more or less severity, on two or three successive days. I think on Monday, Tuesday, and Wednesday.

Mr. P. resides at 37 Ellicott, and Mrs. P. was ill on Sunday with diarrhoea; and the servant girl fell sick about Thursday noon, with the same malady.

The dwellings to which I have referred, are all, with one exception, brick, and generally new. They are also, I believe, all well drained and well ventilated, having generally spacious yards, and in most cases being detached from the adjacent buildings.

I have heard of one other case as having occurred a few doors above Mr. W.'s, on Wednesday, but I have not been able to learn the facts.

We have thus occurring within the distance of a few rods each way from the centre of the ditch, near the intersection of North Division with Ellicott, nineteen cases of diarrhoea, with manifest cholera tendency, (all being so ill as to require medical attendance) or with actual cholera; and of these, nine have died. Of the six whose illness commenced on or before Tuesday, four have died. Of the six attacked on Wednesday, five have died; and of seven attacked on Thursday, none have died. Since Thursday no new cases have occurred in that neighborhood. In twenty families living upon the street the epidemic has shown itself in nine or ten, possibly in others.

Early Thursday morning the attention of the Board of Health was called to the probable connection between the opening of the ditch and the existence of the cholera in this street, and they promptly issued an order to have it closed. The contractor had, however, commenced replacing the earth at the request of several residents of the street; and by one o'clock, P. M., of Thursday, July 29th, it was completely closed.

My conviction is, under all these circumstances, that these cases, all had their source, more or less directly in the miasms from the ditch. I have no doubt other causes may have concurred and materially promoted the result—eating sour, or unripe fruit—alarm—even contagion I admit; yet neither one nor all of these are sufficient to explain many of the cases. They did not all eat fruit—several were attacked simultaneously in the night before it was known that the cholera was in the neighborhood—children and almost infants were in many instances its subjects.

The weather was very warm, and immediately when this old bed was

opened and brought to the surface, a rapid decomposition and elimination commenced. During the day the heat of the sun so rarified the air, that the mephitic or poisonous gases arose rapidly and were borne off; but during the night, when most of the attacks commenced, these exhalations settled and hung upon the houses and their unsuspecting occupants, like the heavy vapors of a pit. The men who dug the ditch were unharmed, and I believe also that such as occupied these houses only during the day might have been comparatively secure.

It is no reply, that the same has not occurred in other streets where ditches have been opened. It *has* occurred, as I think I have it in my power to convince you by an examination of the facts—but if it had not, it would only show that there did not exist in those cases the same catenation of circumstances. The soil thrown up, might have been more clean and destitute of vegetable matter—the heat to which it was exposed might have been less intense—the atmosphere might have been more disturbed by winds, by which the miasms might have been moved off as rapidly as they were evolved. For it will not be forgotten that during these days, from the 24th to the 29th, there was scarcely a breath of air.

Thursday evening there was a slight shower, and during the night a terrific thunder storm, with wind and rain. The wind, which for several days had blown gently from the south, changed now suddenly to a brisk west wind. Since then the temperature of the atmosphere has been much lower.

I have no confidence in the electric origin of cholera, and should not, therefore, under any circumstances, ascribe its accession or departure to electric influences. But here it is certainly a very insufficient explanation of either event. The disease was too limited to have any connection with the general electric state of the atmosphere—and it had already nearly or quite disappeared when the electric explosions occurred.

For myself, I do not doubt, but that the usual concurrence of wind with lightning, has led many to ascribe to the electricity the decrease of the cholera which has occasionally followed; when the *wind* has been in fact the purifying agent to which we were indebted.

In corroboration of the view which I have taken of this matter, I may mention that in 1849, during the summer months, a ditch was commenced in Genesee street, near Michigan, and carried through to Hickory. The ditch was deep and wide, being intended for a sewer. I am informed, by good authority, that as the ditch progressed up the street, the cholera accompanied it, or a little preceded it—and that the cholera continued

street with extraordinary malignancy until the sewer was finished and the earth removed.

The cholera has definite exciting causes, I affirm, and if we will look we shall sometimes see them. Although it occasionally seeks new localities in its successive annual returns, yet it soon and certainly finds out its old haunts, and there mainly fixes its residence.

In this city we hear of it first, or very soon after it is announced, on the low grounds bordering the canals and the lake; then it creeps along upward and slowly, through the "Hydraulics" to Batavia street, Genesee, and into that portion of the city known as the German quarter—and thence west toward Main street. Finally, as the dry and hot season advances, when its local causes are most intense and abundant, it is found more or less at all points of the city. But nowhere are its victims so numerous, or its attacks so fatal, unless, as in the instance before us, some special local cause is interposed, as in the portions of the town first mentioned.

It prefers Rock street and Evans, because its inhabitants are for the most part poor and debauched, and ripe for any disease; and because its streets are dirty, and its dens reeking with filth. It visits most of the streets lying upon the flats, because the soil being underlaid with clay and not sufficiently drained, produces vegetable miasms, or because the land, by recent excavations and deposits, is so irregularly filled in that a multitude of pools are formed, from which similar emanations arise. It spreads itself tardily into the eastern portion of the town because, although the soil is the same as upon the flats, and although here also much of the water which falls upon it is compelled to disappear by evaporation, yet the land is more elevated and much better drained. It avoids, most of all, the western and northern portions of the town, because, from the peculiar exposure, the ventilation is there most complete. But not only because our prevailing winds are from this direction, does the north-western portion of Buffalo possess a comparative immunity from epidemic diseases, cholera included, but also in part because in such streets as Pearl, Franklin, Delaware, &c., the soil is sandy and dry, and water does not evaporate even where there are no drains, but rapidly disappears beneath the surface. The same amount of ventilation does not give the inhabitants of Fifth street, nor indeed, I am inclined to believe, of Prospect Hill, the same immunity as is possessed by Pearl, Franklin, and Delaware; and for this reason, that in the places first named the soil will not permit the water to percolate and escape with the same rapidity.

It is probable that the usual concurrence of a clay soil with lime rock, has to the very common belief that lime water produces cholera. The fac

that cholera is found so frequently where lime water is usual, is only another confirmation of my belief that a clay subsoil, by detaining the water upon the surface produces cholera miasms. I wish it to be distinctly understood that I do not regard the clay itself as a source of disease, but rather the water or moist alluvium, or other decomposing matters which lie above it. Where clay is not present, this alluvium alone is sufficient to its production if it is exposed to moisture.

The remote cause of cholera, like the remote cause of the plague, of the yellow fever, &c., has never been ascertained, and I venture to say never will be. It spreads over us like an invisible spirit, whose presence we mysteriously feel, but whose form we can never grasp. It is probable that at the same moment its breath overspreads broad territories, and that its influences are operating no less in the country than in the city. Yet alone and unaided, it is seldom sufficient to produce such results as enable us to recognize its presence. Of what consequence is it to us then, whether we are able to determine the remote cause or not? Of how much less consequence, certainly than that we determine well its near or exciting causes, without whose concurrence the arrows of this pestilence are pointless.

The immediate sources of cholera are before us in this instance, and in a thousand others, and it is our duty as in some sense conservators of the public health, to see and understand them, and by explaining them fully to the people accomplish their removal.

Clean and ventilate the houses; sweep often and thoroughly the streets; fill up the low basins, if possible, with sand; drain well the clay and alluvial soils; remove early in the season all nuisances from the city; and do not leave the cess-pools to be opened and their contents carried through the streets in hot midsummer nights; expose as little as possible during the prevalence of this epidemic, or at the usual seasons of its incursions, the fresh soil to the heats of a fervid sun, especially where alluvium is known to have been buried; let the people live temperately and wisely, and I fully believe this dreadful pestilence would soon cease to visit us, or that at least it would return so shorn of its strength as no longer to baffle the science and skill of our art.

I wish, gentlemen, if you are persuaded that these are truths, that you would do your duty, for this is no trifling matter, and to you the people look for direction. If the causes are visible, so represent to the public officials with whom the sanitary regulations of the city rest, and thus perhaps by your good and timely counsels save many valuable lives.

ART. V.—*Mercurials in Dysentery.* By S. B. HUNT, M. D., Mendon, Monroe Co., N. Y.

In the *Journal* of September last, is an article on dysentery, from the pen of Dr. Morse, of Tuscarora, Livingston Co., followed by some editorial comments. In the following number the eclectic department furnishes the opinions of Dr. Parrish of New Jersey, on the same subject.

It occurred to me at the time, in reading those articles, that they were (especially the first named, with the comments following) rather heterodox in sentiment, departing somewhat from the old landmarks, and discordant with my own (in this disease) somewhat varied experience. I treated, during the months of July, August and September of the year 1848, one hundred and thirty cases of dysentery, occurring in the town of Portage, Livingston Co., in the Cashequa Valley, some eight or ten miles above Tuscarora. In that region, which is annually devastated by this disease, the epidemic of 1848 will long be remembered.

In the summers of 1846-'47-'49 and '50, in that region, and to a less extent for the past two seasons in this my present location, I had also ample opportunities for testing the various methods of treatment, and arrived at the same general conclusions taught me by the epidemic of 1848. This epidemic, commencing about the 20th July, continued till the close of September. During that time, sixteen deaths occurred of the disease in the township, six of them in my practice. I have full and accurate minutes of the cases as they occurred. I have no intention of afflicting you with a full account of them, but shall rather state the conclusions at which I arrived at the time, with a few deductions forced upon my mind by the fatal cases. After this prelude, you will guess that I am about to reassert the time-honored fame of calomel in this disease, and moreover, to place myself for the nonce, in the ranks of the Hepatic Pathologists, a much abused class of "old schoolers," carrying their conservatism a trifle too far perhaps, yet still possessing many of those sterling attributes of veneration for the past — a sturdy determination to "hold fast to that which is good," and who show a degree of energy in attacking a disease, shocking to the sensibilities of all homoeopaths, and their cousins, the expectants.

In taking the epidemic of 1848 as my theme, I do so because it is more convenient, and presents types of all the modifications of dysentery I ever met. I lay down the following propositions to start with.

1st. It was malarial in its origin.

2d. There were unmistakable signs of hepatic disturbance preceding the disease, sometimes by many weeks.

3d. That it was *not* purely inflammatory in its character.

1st. As to its malarial origin. The cases occurred principally upon the borders of the Cashequa Creek, which, having a rapid fall, was interrupted in its course by several mill ponds, which were frequently drawn off and slowly filled again, leaving a mass of decomposing vegetable matter, exposed to the rays of a hot sun. Other cases occurred at the Deep Cut of the Genesee Valley Canal, a high and breezy location, but where men, women, and children, pigs and poultry, lived huddled together in miserable shanties, and where vast quantities of earth were daily turned up to the sun. Other cases occurred on the road leading south from the Deep Cut, in a location apparently healthy, but really exposed to the malaria brought over it by westerly winds, from a large swamp a short distance west. Lying to the west of the town, and forming its boundary, is the Genesee river, with wide flats or bottoms pregnant with malaria, and where intermittents and dysentery are household names.

2ndly. Signs of Hepatic Disturbance.—Every one who has seen a case of dysentery, recollects the parti colored stools so indicative of "*bilious*" disturbance. [I have still left moral courage enough to say sometimes to a patient, "*You are bilious, Sir!*"] But here the objection of Doct. Parrish meets us, this is a sympathetic derangement depending upon, and cured with the inflammation below. It would seem that portal congestion, setting back the blood upon the capillaries of the rectum, would be quite as likely to cause engorgement and inflammation there, as the *vice versa* process would be to cause portal congestion; but, dropping this argument, let us look at the premonitory symptoms of dysentery.

I extract from my note book:—"This class of cases was indicated by symptoms of hepatic disturbance, *preceding sometimes by weeks* the onset of the disease. Yellowness of skin and conjunctiva, sleepiness at noonday, bitter taste in the fauces, pain in the right shoulder, with uneasiness in the hypochondrium, and consequent disposition to lie on the *right side*, were generally present."

The third proposition that it was not purely inflammatory in its character, follows naturally the other statements, and requires little proof. Many cases were essentially typhoid in their character, and the patients were prostrated at the onset of the disease to such a degree as no purely inflammatory disease manifests.

And now I wish to put in a word or two lest I may be *misunderstood* as

to the treatment of dysentery. In the class of cases which I have been describing, and which I believe make up a large modicum of the cases coming under treatment, calomel was the sheet anchor. Given, combined with sufficient opium to control its cathartic tendencies, repeated at intervals of three or four hours, and continued till bilious stools followed its exhibition, it was followed with the happiest results. In cases where the liver did not seem to be implicated, a favorite plan of treatment was to give at night \frac{ss} grains pulv. Dover, repeated in two hours should the first dose fail to quiet the tormina. This was followed in the morning by \mathcal{Z} sal Rochelle, given in light mutton broth. Frequently this was sufficient for a cure; if not, the plan was repeated. In other cases, a full dose of opium, (3 or 4 gr.) cut short the disease.

One word about the use of sal Rochelle. It is less nauseous, and more reliable than castor oil, and much more rapid in its action, which is a very desirable quality. I have never seen any untoward symptoms of irritation following its use, and its stimulant effect upon the duodenum, assists in rousing the liver.

As to astringents, I have given the whole round of them a more thorough trial than I shall ever venture upon again. I do not know but Dr. Condie can check the discharges with his pulv. acet. plumbi ipecac. et opii, but I cannot. I have failed entirely a hundred times in trying astringents, and so wasted valuable time.

As to the use of cathartics, I do not think that any person can prescribe a set rule for their administration. I am partial to them, but my rule is to wait until I find that the contents of the bowel are trickling down over the inflamed surface, and then to give the sal Rochelle, sweep out the bowels thoroughly, and then close up with opiates, or should the hepatic symptoms be present, with calomel and opium, until the indication for cathartics comes round again.

My note book furnishes me another text, and a solemn one to preach from, as to the use of mercurials. I will give merely an abstract, as I have already said more than I intended.

Two cases of dysentery occurred in children of the same age, in houses a quarter of a mile distant, but exposed to the same miasmatic causes. In each case, my little patients had careful nursing. In the one, I suspended mercurials on the evening of the 28th August, on account of the apparently moribund condition of my patient. But he lived, and bore the most exhausting discharges for four days thereafter, when he died. The other case had symptoms no less severe. He, too, was moribund; but I followed

him up with calomel to death's door, and when, not expecting to find him alive, I visited him, and gave him a large injection of cold water, which was followed by a copious forced evacuation, he convalesced rapidly. The added experience of four years has given me no reason to change the opinion expressed in my note book at the time, and which I append as a clincher to my argument. I quote—"In this case, the use of mercurials was continued much longer, and through greater debility than in the first; and however disagreeable it may be to acknowledge faults in a fatal case, yet the conclusion forces itself upon me, that had the use of calomel been continued through the night of the 28th, in the first case, a different result would have been obtained. I believe it to be good practice in this form of dysentery, to continue mercurials until approaching death, if death must come."

MENDON, July 19, 1852.

ART. VI—*Arabian Method of treating Stone in the Bladder.*

August 10th, 1852.

In an old work, published in London in the year 1743, entitled *A View of the Levant, &c.*, by Charles Perry, M. D., there is described a curious method of treating a very formidable disease, which is practiced by the Arabs in the city of Cairo. Thinking it might interest your readers, as a medical curiosity, I have copied it out word for word, merely modernizing the spelling a little.

Yours truly,

G. H.

"Being the other day in discourse with one Signior Gabrieli, a Venetian who has practiced here as a Medico for many years, he entertained us with an account of a great cure which he had lately performed. His story was thus, or to this purpose: That a certain Effendi, a person of great affairs and consideration, and about fifty years old, had been tormented with violent and continued pains in his reins, for twelve years past, without intermission: that during this long and irksome time of twelve years misery, he had applied himself to all the doctors, (whether real or nominal,) that he could meet with or hear of in this city, but without any sort of benefit; for they all alike mistook his case, judging it to be no other than a cold, which had determined and fixed itself upon that region. At length, about eight months ago, good luck or providence directed him to his Signior Gabrieli. He was no sooner called, and fully instructed of the patient's complaint, than he judged and pronounced it to be of the hepatic kind; but he judged much better of the disease than of the

medicines he applied for the cure, for he gave nothing but mallow water in large quantity, with oils and syrups to lubricate. These indeed were very innocent remedies, and, as one would be apt to think, equally impotent, as in fact they proved. But Signior Gabrieli, having experienced those, and such like things, for a considerable time, without any fruit or effect, and being acquainted with an Arab, who was famed for his dexterity in blowing wind up the penis for the cure of stone and gravel, he went in search of him, carried him to the patient, and ordered him to perform the operation without delay, in the best manner he could. The operator, having his instruments about him, went to work directly. He first ordered the patient to stand up in an erect posture; then he put the end of a common cane pipe, (which was about three inches long, and cut taper, after the manner of our penis syringes,) into the urethra; and the instrument being adjusted in such a manner as he thought proper, he blew into it, with all his might, for a considerable time; then, holding the urethra, to prevent the wind's flying out again, he played about the bottom of his belly with the other hand, (especially above the os pubis and near the groins,) for a considerable time; then, relieving the urethra, he let the wind discharge itself, beating his belly gently with his hand, to force the wind out with a greater impetus. When the wind was pretty well discharged, he applied a pipe again into the urethra, and then sucked with the same force as he had before blown. By this first operation the patient voided eleven stones near as big as vetches; and the same operation being repeated every morning and evening, till he was entirely freed from pain, and from all further emissions of stone or gravel, the whole quantity of stones discharged, (besides an incredible quantity of gravel,) amounted to near three tea cups full; and besides these, he excreted a great deal of white viscous matter in his urine.

“However, we confess, we were rather pleased and diverted with this story, than satisfied about it; because people are generally partial in their own favor; or at least will exaggerate in their accounts of things which tend to their own glory and honor. We therefore desired Signior Gabrieli, for our full satisfaction and conviction, to carry us to see the person. Signior Gabrieli replied, without the least scruple or hesitation, that we were masters to go whenever we would; and no longer ago than yesterday, we went and had an interview with this Effendi. We saw the stones which he had voided, and had all the other circumstances of the cure confirmed by the patient's own mouth. Most of the stones are as big as vetches, and somewhat of the same figure; they are all of a dark yellow color, and of a

friable texture. The Effendi told us that he had not been able to mount his horse, nor scarce to move about the house, for the space of twelve years before, but was now pretty well, and very easy. He said, however, that when he urined, he had yet a burning pain in the urethra, near its extremity; and examining his urine, we found it of a wheyish color, abounding with a number of white filaments."

ART VII.—*Remarks on Bandaging the Abdomen after Delivery.*

By J. H. BEECH, M. D.

I was surprised to see an article in the *Buffalo Med. Journal*, "On Bandaging the Abdomen after Delivery," without editorial comment in reference to the sentiment. We believe the article calculated to mislead students and inexperienced practitioners, in regard to the importance of the bandage in question, and that serious consequences might result to many while experiments were establishing facts which are almost universally received by the profession.*

Either Mr. Kesteven or myself have certainly misunderstood the object expected to be gained by the bandage, and we believe that physicians generally have no such idea of the "rationale of its usefulness" as he supposes. We think few expect to "promote the contraction of the uterus by the bandage," but rather to maintain the contraction, by supplying the accustomed pressure of the abdominal walls, until the muscles shall have regained their power of contracting upon the viscera alone. We find bandages necessary for the comfort and safety of patients, whenever we abstract fluids from the thoracic cavity, from hydrocele or anasarca, after evacuating large abscesses, and whenever we remove fluids or solid substances from the abdominal cavity of males or females.

We have never ventured to apply a bandage to a puerperal woman, nor leave her until the uterus was firmly contracted; but after this, the bandage properly applied, reaching from the ensiform cartilage to the os pubis, laced or pinned smoothly, is usually a comfortable means of supplying the place of that tension which had been so suddenly lost by parturition, and without which faintness and exhaustion are apt to be experienced.

Without this support, we believe the uterus is inclined to become relaxed, the fatigue of its contractile tissue allowing it to yield to the lochial

* We do not limit our selections of eclectic matter to articles in which the views expressed accord with our own. We concur fully with Dr. Beech in his reasons for the use of the bandage.—*Editor Duff. Med. Jour.*

exhalation, until the veins of the placental surface became patulous, and blood begins to ooze, or flow *in uteri*, in proportion to the dilatation.

If the uterus makes efforts to arrest the flow by regaining its contracted condition, the pains (after pains) will be more or less severe in proportion to the neuralgic condition of the uterus, and to the firmness or size of the coagulum to be expelled.

It is no uncommon circumstance to see patients express great relief from syncope and distress upon the proper adjustment of a bandage, even where the uterus had kept firmly contracted.

The muscles of the "*laboring poor*" contract more readily than those more easily circumstanced, and they are not so apt to suffer from neglecting the bandage; but in this country they are far from being ignorant of its use and value. We understand that even the Indians formerly inhabiting this vicinity made use of such support.

We have had no experience with "pads, basins, &c., &c.," except so far as to have seen enough to satisfy us, that they may be so used as to interfere with the very object intended; but removing such, have found, after proper preliminary treatment, that a suitable bandage placed the patient in a better condition to leave in the care of a nurse.

I am very respectfully yours, &c.,

COLDWATER, MICH., July 15, 1852.

J. H. BEECH, M. D.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

On Nitric Acid in Hooping Cough and Asthma. By F. C. T. ARNOLDI, M. D., Lecturer on Midwifery and Diseases of Women and Children, St. Lawrence School of Medicine, Montreal, &c., &c.

The few following remarks I take the liberty of communicating to the profession, through the pages of this excellent Journal, feeling perfectly confident they will be read with pleasure, inasmuch as they are somewhat novel as regards the alleviation of hitherto supposed intractable diseases, viz: hooping cough and asthma. The *modus operandi* of the remedy I will not at present attempt to explain, but from the results of my own practice, and those of my medical confreres who have watched it and adopted it, I confidently recommend its application to all such as meet with similar cases. In hooping cough, at whatever age, whether it be a child at the breast, or a full grown adult, I administer nitric acid in solution, as strong as lemon juice, sweetened ad libitum. I had given to a child of two years of age, as much

as one drachm and a half of concentrated nitric acid, in the above manner per diem, and I have never known the disease to resist its use beyond three weeks. In one instance, that of a child at the breast, only seven months old, the disease disappeared within eight days. In another instance of a young lady fifteen years of age, the paroxysms were subdued within the first twenty-four hours, and the disease disappeared within ten days. Again, in the cases of two boys about ten years of age, living at a great distance from one another, who had had the cough for several weeks, and to such a violent degree, that both of them had the circumference of their eyes echymosed as though they had been pumelled in pugilistic combats, the acid acted positively like a miracle. A medical confrere of mine had four of his children severely affected with the same disease in the middle of winter, and although they had to be kept in-doors owing to the inclemency of the weather, they were nevertheless all perfectly cured within three weeks. I might go on to cite a hundred similar instances, but these, I am satisfied, will prove sufficient to induce the profession to adopt this treatment. As regards asthma, the use of nitric acid has proved, not only in my own practice, but in that of others who have adopted it, truly marvellous, and I trust that the profession will remain satisfied by my quoting two special cases. One is that of an elderly person, who had been for five years a frequent inmate of the Montreal General Hospital, a thorough victim to this disease. He generally remained under treatment the winter, and used to be discharged when the disease seemed to have exhausted itself. This patient, about eighteen months ago, was again admitted into the hospital, under the care of my friend Dr. David, who, observing the obstinacy of the paroxysms, resolved on trying the use of nitric acid. The result was, that the first night was passed tolerably; the second night he slept well; the day after the third night he reported himself perfectly convalescent, and on the fifth day he was discharged at his own request, since which he has never been heard of. The other case is that of a stout plethoric servant girl, about thirty-five years of age, who applied to me in the early part of December last. She was then laboring under very severe asthmatic distress, and told me that she had been a martyr to repeated attacks, equally severe, for four or five years past; that she had consulted many medical men, but could never obtain any relief, until, as she said, the disease had spent itself. I gave her a prescription containing half an ounce of concentrated nitric acid, and I have never seen her since, but during the New Year holidays, happening to call at the house where she served, I made enquiry about her, when I was told, much to my merriment, that the reason why she never came back to see me was that she thought that I had bewitched her. She had often taken medicines which gave her no relief, but that the very first night after taking the acid, she slept perfectly sound, and had not, up to that time, had any return of the symptoms. Now, these are obstinate facts, and I trust that this familiar method of communicating them will not diminish their value, nor need any of the profession to be too sceptical to follow the treatment.—*Canada Med. Jour.*

A Few Hints in treating Urinary Affections.—Dr. Bird concludes his work on the above subject by the three following rules:—1. Whenever it is desirable to impregnate the urine with a salt, or to excite diuresis by a saline combination, it must be exhibited in solution, so diluted as to contain less than

five per cent. of the remedy, or not more than twenty-five grains in an ordinary draught. The absorption of the drug into the capillaries will be ensured by a copious draught of water, or any diluent, immediately after each dose. 2. When the urine contains purpurine, or presents other evidence of portal obstruction, the diuretics or other remedies employed should be preceded or accompanied by the administration of mild mercurials,—taraxacum, hydrochlorate of ammonia, or other cholitic remedies. By these means, or by local depletion, especially by leeches to the anus, the portal vessels will be unloaded, and a free passage obtained to the general circulation. 3. In cases of valvular disease, or other obstructions existing in the heart and large vessels, it is next to useless to endeavor to excite diuretic action, or appeal to the kidneys by remedies intended to be excreted by them. The best diuretic will, in such cases, be found in whatever tends to diminish the congested state of the vascular system, and to moderate the action of the heart, as digitalis, colchicum, and other sedatives, with mild mercurials.—*Brit. and For. Med. Chir. Rev. from the Dublin Med. Press.*

Artificial Production of the Flavors and Odors of Fruits and Flowers.—One of the most surprising achievements of modern chemistry is the artificial production of the flavors and odors of fruits and flowers; the imitation in the crude laboratory of the chemist of the most delicate of the productions of nature, and one which it might have been supposed was beyond the reach of art.

Dr. Playfair, in his lecture on the great exhibition of 1851, furnishes us with the following interesting information on this subject:—

“The jury in the exhibition, or rather two distinguished chemists of that jury, Dr. Hoffman and Mr. De la Rue, ascertained that some of the most delicate perfumes were made by chemical artifice, and not, as of old, by distilling them from flowers. The perfume of flowers often consists of oils and ethers, which the chemist can compound artificially in his laboratory. Commercial enterprise has availed itself of this fact, and sent to the exhibition, in the form of essences, perfumes thus prepared. Singularly enough, they are generally derived from substances of intensely disgusting odor. A peculiarly fetid oil, termed ‘fusel oil,’ is formed in making brandy and whiskey. This fusel oil, distilled with sulphuric acid, and acetate of potash, gives the oil of pears. The oil of apples is made from the same fusel oil, by distillation with sulphuric acid and bichromate of potash. The oil of pine-apples is obtained from the product of the action of putrid cheese on sugar; or by making a soap with butter, and distilling it with alcohol and sulphuric acid, and is now largely employed in England in the preparation of pine-apple ale. Oil of grapes, and oil of cognac, used to impart the flavor of French Cognac to British brandy, are little else than fusel oil. The artificial oil of bitter almonds, now so largely employed in perfuming soap, and for flavoring confectionery, is prepared by the action of nitric acid on the fetid oils of gas tar. Many a fair forehead is damped with eau de millefleurs, without knowing that its essential ingredient is derived from the drainage of cow-houses. The winter-green oil, imported from New Jersey, being produced from a plant indigenous there, is artificially made with willows and a body procured from the distillation of wood. All these are direct modern appliances

of science to an industrial purpose, and imply an acquaintance with the highest investigations of organic chemistry. Let us recollect that the oil of lemons, turpentine, oil of juniper, oil of roses, oil of copaiba, oil of rosemary, and many other oils, are identical in composition, and it is not difficult to conceive that perfumery may derive still further aid from chemistry."—*Medical News*.

Expulsion of Tape Worm by Pumpkin Seeds. Communicated for the Boston Medical and Surgical Journal.

Having recently had an opportunity to administer the remedy for tapeworm recommended in the Journal for October 8, 1851, I take the liberty to send you a brief account of its operation.

The patient, an adult, had taken, several weeks since, by direction of a physician, some extract of male fern followed by castor oil, which expelled about four feet of worm, together with a number of fragments. The remedy was repeated, but no further benefit was obtained.

There being sufficient evidence, however, that the difficulty was not overcome, I determined, as the case fell under my charge, to try the pumpkin seed orgeat, which was prepared and administered as follows: Six ounces of common pumpkin seeds were thoroughly bruized in a mortar, without removing the outer shells, and a sufficient quantity of water was added to afford by straining and expression one pint of liquid. At six o'clock, A. M., the patient took one half of the liquid, or orgeat, and in two hours after half an ounce of castor oil. A slight movement of the bowels followed, with a few fragments of the worm. At ten o'clock, half an ounce more of oil was given, the abdomen was rubbed with sulph. ether, and cold water was directed to be used freely. No food to be taken until after the operation. At twelve o'clock the bowels were evacuated, and an entire worm discharged, eight feet and seven inches in length.

Although the patient is quite feeble from the effects of pulmonary and hepatic disease, no inconvenience has resulted from the remedy.

ROCHESTER, N. Y., July 13, 1852.

W. W. ELY.

On the Merits and Demerits of the Ovarian Section.—The following are the conclusions which the "British and Foreign Medico-Chirurgical" Reviewer arrives at after a careful consideration of the whole of the cases bearing upon this point, although they must only be considered on the whole as approximations to truth:

1st. That in any case in which it is considered advisable to remove an ovarian tumor, it is justifiable to make a small preliminary incision into the abdomen, for the purpose of determining whether the tumor be inherent or not.

2d. If the tumor be adherent, the incision is to be immediately closed entirely, or to such an extent as merely to leave an aperture the size of that made by an ordinary trocar, and we may then expect that this operation will not, on the average, be followed by much more fatal results than common tapping.

3d. That where the tumor consists of a simple cyst or cysts, with but small solid deposit, it may be extirpated with as good a chance of success as attends the performance of the more serious surgical operations, and with the further prospect of the cure remaining permanent.

4th. The existence of much solid deposit, or of extensive adhesions, absolutely forbids the operation, which should be brought to a termination immediately on the discovery of either.

Finally, we may add our belief, that the plan proposed by Mr. Wilson, of tying each bleeding vessel separately, so as to dispense with the ligature round the pedicle is an important improvement; and that, if experience shows that it is sufficient for the cure of the disease to remove only a part of the cyst, the operation will be rendered considerably more hopeful.—*Brit. and For. Med. Rev.*

We transfer to our pages, with great pleasure, the following extract from a review of Dr. Gross's work on the Urinary Organs, in the July No. of the British and Foreign Medico-Chirurgical Review, as evidence of a kindlier feeling on the part of foreign journals towards American authors. The reviewer has been speaking of an English work on a kindred subject, by Dr. Coulson, in connection with which he says:

"It has remained for an American writer to wipe away this reproach; and so completely has the task been fulfilled, that we venture to predict for Dr. Gross's treatise a place in the literature of surgery, worthy to rank with the best works of the present age. Not merely is the matter good, but the getting up of the volume is most creditable to transatlantic enterprise; the paper and print would do credit to a first-rate London establishment; and the numerous wood cuts which illustrate it demonstrate that America is making rapid advances in this department of art. We have, indeed, unfeigned pleasure in congratulating all concerned in this publication, on the result of their labors; and experience a feeling something like what might animate a long expectant husbandman, who, oftentimes disappointed by the produce of a favorite field, is at last agreeably surprised by a stately crop, which may bear comparison with any of its former rivals. The grounds of our high appreciation of the work will be obvious as we proceed; and we doubt not that the present facilities for obtaining American books will induce many of our readers to verify our recommendation by their own perusal of it."—*Med. Examiner.*

Royal Medical and Chirurgical Society — An Analysis of One Hundred Cases of Cancerous Disease of the Uterus. By ROBERT LEE, M. D., F. R. S. &c.

The conclusions to which the author arrived from this analysis were:—1st. That cancer may commence in any part of the mucous, muscular, or peritoneal coats of the uterus; but most frequently in the os and cervix. 2dly. That the earliest symptoms of the disease, in a large proportion of cases, were discharges of sanguineous, serous, or white-colored fluid from the vagina, with sense of uneasiness or pain more or less acute within and around

the pelvis. 3dly. That cancerous disease of the uterus presented itself most frequently in the form of induration and ulceration of the os and cervix uteri and vagina, or ulceration without induration, or in the form of fungoid tumors, usually called cauliflower excrescences, growing from one of the lips or the whole os uteri, being often associated with encephaloid or colloid masses and true scirrhus of the remaining portions of the uterus and contiguous viscera. 4thly. That in no case could cancerous disease of the uterus be referred to inflammation; and that its fatal progress was never arrested by cauterizing the morbid structures through the speculum, nor by any other course of treatment.—*Dublin Med. Press.*

Death by Chloroform.—I have this moment received a note from you requesting an account of the unfortunate case of etherization that occurred in my practice on Friday last. It was my intention to have drawn up a statement of the case for your Journal, and so will at once comply with your request.

Henry Keyser, a German by birth, aged 17, was brought to my office yesterday about noon for surgical aid. The middle finger of his left hand had been caught in the gearing of the machine at which he was at work; the last phalanx was carried away and the soft parts badly lacerated and torn. It was thought best to remove a portion of the next phalanx, so that when the mangled muscles, ligaments, &c., should be removed, the extremity of the bone might still be covered. The young man looked pale, and appeared to suffer great pain; but beyond this I observed nothing unusual in his appearance—and saw no reason why ether might not safely be administered, and the further infliction of pain obviated. I made the proposition, and he at once began to inhale a mixture of chloric ether and chloroform. There was about ten per cent. of chloroform contained in the mixture. The inhalation was continued for about four or five minutes, with now and then an interruption by his pushing the sponge away with his hand. Appearing insensible, the operation was commenced, but the first stroke of the knife made him start with a loud expression of pain. He began to vomit, and now sensibility was in a great measure restored. The sponge, replenished, was again applied, and he complained of its making his lips smart. I at once removed it and applied ol. oliv. The sponge was again applied over the nose and mouth, and inhalation carried on as before, and for about the same length of time as at the first attempt, or it may have been a minute or two longer. He again appeared unconscious; and to prevent his waking up as before, I gave the sponge—now containing very little ether—into the hands of Mr. Merrill, directing him to hold it still over the face; but there was again retching and an attempt to vomit, from which cause he but illy succeeded in retaining the sponge as directed. I again commenced the operation, Mr. Venner, who came with the young man, holding the hand.

Up to this time, there was observed nothing remarkable either in his general appearance, the pulse, or the respiration. I had made only two incisions, and was attempting to tie the outside digital artery, which was bleeding, per saltem, showing that up to this moment the circulation was good, when my attention was directed by Mr. Merrill to the appearance of the patient.

I saw at once that he was either dead or dying, and directed my assistants to help me lay him at once on his back. I sent one of them for medical counsel, whilst the other assisted me in applying restoratives. I found the pulse at the wrist gone, the action of the heart very feeble indeed, and respiration in a moment ceased. But by the application of strong ammonia to the nose, dashing ice-water over the head, &c., he again began to gasp, and was breathing convulsively when Dr. Parcher arrived. But a few heaving inspirations, at long intervals—the action of the heart meanwhile growing more and more feeble—and all was quiet. My patient was dead. Dr. Parcher assisted me in the diligent application of the usual means for resuscitation in cases of suspended animation—but all to no purpose, the vital spark had fled.

I regret that I am unable to give the post-mortem appearances of the internal vital organs, the friends objecting to an autopsy being made. And yet I very much doubt whether the knife would have revealed anything new. Such, then, are the facts in this unfortunate case. I had read of deaths from chloroform, but had hoped never to have seen one. A coroner's inquest was held at my request over the body of the deceased, a few hours after death, and the jury returned a verdict in accordance with the facts above stated.

But the question will arise in the minds of all who may read this article, how was death caused in this particular case? Was it owing to an impure article used? Was it unskillfully administered? Or was there something peculiar in the organization of the patient, or the state of the nervous system at the time, rendering this agent toxic with whatever precaution used? That the last position is the true one, is my honest conviction.

On the first question, I have only to remark, that both the chloric ether and chloroform were obtained at W. B. Little's apothecary store, on Hanover street, where I have usually supplied myself for several years past. And in addition, I can state this important fact, that from the same bottle I had administered the ether in a number of cases previously, with the usual effect. And furthermore, the mixture in the phial, containing the small per cent. of chloroform mentioned, had been tested only a few days before, in a case where a similar operation on a finger was required. This patient inhaled from two to three times the quantity that Keyser did, with the happiest effect, and walked home after the operation.

Secondly, I would remark that I have habitually used anæsthetic agents in my practice, since their first introduction in this city, and never before witnessed any alarming or injurious effect from their use. At first I employed sulphuric ether; then, for nearly two years, chloroform exclusively. But finding that, according to the experience of others, ether seemed more safe, I have for almost two years relied upon this agent in obstetric and surgical practice, using chloroform only in cases where the ether appeared not to induce anæsthesia very readily. My plan of administering these agents has been uniform. I have never used an inhaler of any kind whatever. The sponge is the only article I have ever used, unless it was the handkerchief or napkin in a few instances when no sponge was at hand. I have always made it a point to admit an abundant supply of atmospheric air into the lungs, and when the patient complained of suffocation permitted him to push the sponge away for a few moments, and then go on with the inhalation. The sponge I used the other day is of small size, not holding above two fluid ounces, and was seldom filled beyond half its capacity. It is the

same sponge that I have used for more than five years, and is so open, that of itself, it is no obstacle whatever to respiration when placed over the mouth and nose.

In regard to the quantity used in the present instance—the phial had on it an apothecary's label; and, before moistening the sponge, the fluid did not reach the lower edge of said label. This fact was remarked by Mr. Venner as well as myself, and there still remains in the phial nearly one ounce of the fluid. In the opinion of the physicians—Drs. Parcher and Thorndyke, who were on the inquest—the quantity used was about two ounces or a little over. By weight it may have been more.

Finally, I learned from the mother of the young man, that he had never been sick, but had been a child of penury and want, suffering at times for the necessaries of life; also, that he had met with an accident some years since, on account of which he lay in a fainting condition for some time. One of the men who came to my office with him, but who passed out as I came in, told me, the day after, that he “had no doubt but that Henry died from the effects of fear;” that he trembled like an aspen leaf when he was coming from the shop. We all know what a terrible influence *fear* has over the vital economy. Why, a friend of mine mentioned, in connection with this case, that he once knew an artisan, who died in this way without taking ether. His knife slipped in his hand, and he *thought* he had inflicted a serious wound on his thigh. He swooned away, and all efforts to revive him were unavailing. The surgeon found only a slight scratch of the skin—but his patient was dead.

My conclusion, then, is, that the fatal consequence attending etherization in the present instance is not owing to any inferiority in the article used, to want of care in its administration, nor to any organic disease in the patient; but that we must look for it in the naturally delicate organization of the subject, rendering him very sensitive to external impressions, in the shock that the nervous system had sustained in the injury, and last, but not least, in the influence of *fear*. Not in any one of these singly, but in the three combined.

In haste, yours most truly,

DAN'L V. FOLTS.

37 Maverick Square, E. Boston, Aug. 9, 1852.

Boston Med. and Surg. Journal.

External Use of Cod Liver Oil.—Dr. A. H. David recommends (*Canada Medical Journal*, May, 1852) the cod-liver oil as a local application in various cutaneous affections, and states that after a trial of it in such cases for upward of two years, he has found it to act almost specifically.

In ringworm of the scalp, Dr. D. says he has used it in more than twenty cases with entire success. Some cases, which had resisted other methods of treatment for weeks, were cured in four or five days.

He has also used it in tinea capitis with equal success; and he cured one case of psoriasis inveterata of three years standing, by this application, in seven weeks.—*American Jour. of Med. Sciences.*

EDITORIAL DEPARTMENT.

Pathology and Treatment of Dysentery. — Our readers will find in the original department of this No., a communication from an esteemed correspondent, Dr. Hunt, which we insert with pleasure, notwithstanding we are obliged to dissent from the theoretical and practical views therein contained. Free discussion, opposing arguments, and facts pertaining to both sides of mooted questions, contribute to the development of truth. It is one of the most important of the functions of a medical journal to afford facilities for these objects. We are always glad to receive articles designed to controvert opinions advanced either by our correspondents, or ourselves, if written in that spirit of candor and courtesy which is an essential element of a philosophical temper.

Dr. Hunt attributes the cases of epidemic dysentery occurring in his neighborhood to malarial causes. The facts which he adduces, however, appear to us only sufficient for a bare presumption on this point. The malaria to which he attributes the origin of the disease, is that supposed to emanate from vegetable matter in a state of decomposition. It has been customary to attribute morbid emanations to this source for a long period, but it is questionable whether the doctrine has much to sustain it beyond tradition. There is abundant evidence to prove that the special cause of intermittent and remittent fevers, commonly known as marsh miasmata, is, to say the least, not uniformly produced by vegetable decomposition. Whether it is ever due to the latter alone, admits of doubt. But not to pursue this topic, the supposition is made by Dr. H. as bearing on the hepatic pathology of dysentery. He apparently takes it for granted that if the disease be malarial in its origin, the liver is implicated as a matter of course. This, too, is an idea which has the sanction of tradition; but, for one, we must withhold assent until it is shown to rest on facts, instead of mere theoretical assumption. The term *bilious* is often enough used by patients and physicians to create for it some significance, if there be any virtue in the aphorism *vox populi est vox Dei*. This criterion of truth, however, if it be applicable to politics, is certainly not to medical science. For us, the term is devoid of any special meaning. It does not express any well defined pathological condition. It

has only a loose, conventional, popular signification, which would make it convenient sometimes in order to satisfy the very natural desire of patients that their disorders should have some name. With our notions, we cannot ever say to a patient "Sir, you are bilious." Our correspondent with his views can doubtless consistently speak thus; but for us it would be more than an absurdity. Dr. H. is by no means alone in the use of the term; but we should be glad to have some one of the many who daily employ it, reduce to writing a rigorous definition of the sense in which they understand it. Do they mean too much or too little bile; or bad bile; or obstructed bile?

A term used to denote a morbid condition must have reference to something tangible or appreciable. Now what is the ensemble of symptoms which characterises the bilious state? When this question is answered, we shall be ready to ask upon what grounds the symptoms mentioned are assumed to be derived from the pathological change which is assumed to exist. The truth is, as it seems to us, the indefinite notions so generally entertained on this subject are entirely gratuitous, and the sooner they are discarded, the more favorable is the attitude for real progress in pathological knowledge.

Our correspondent mentions certain precursors of an attack of dysentery, which he appears to think favors the supposition that this disease is preceded by "hepatic derangement," to use an expression often employed by physicians as a synonym of the more popular phrase "*bilious disorder*." So far as our impressions go, the cases of dysentery that have fallen under our own observation, have not usually presented the premonitory symptoms mentioned by Dr. H. We have found the attack generally to be ushered in by simple diarrhœa of greater or less duration, or occurring abruptly, i. e. the dysenteric symptoms exhibited at the outset. It is true we have not looked for precursory symptoms referable to the liver; that is to say, we have not judged of the prodromes by this pathological preconception. But we are not prepared to admit that, on this account, our impressions are less likely to be correct.

Our correspondent states that dysentery, in the cases observed by him, has not consisted of pure inflammation. If he means by this that the pathology of the disease involves something ulterior to the local affection, we are not prepared to take issue with him on that point. This is very far from an admission that the ulterior morbid condition is hepatic congestion. Here we may ask for positive proof that congestion of the liver obtains in dysentery. Assuming, for the sake of argument, that this condition does exist, we may then ask what proof is there that the dysenteric affection is dependent on the hepatic congestion. Not only is there no positive evidence of such a

dependence, but facts lead to a different conclusion. We know that congestion of the liver does exist under certain circumstances; for example in the lesion of that organ called cirrhosis. The *portal circle* then without doubt becomes engorged, and what is the consequence? Does dysentery supervene? Are the symptoms of dysentery embraced among the legitimate results of cirrhosis? Not at all, even when the portal vessels are distended sufficiently to occasion a mechanical expression of more or less of their serous contents through the coats of the vessels into the peritoneal cavity. Ascites ensues, but not dysentery. Dysentery probably involves an occult proximate cause inducing the intestinal affection; but this is true of other local inflammations frequently, if not generally. The disease is local in the same sense that typhoid pneumonia is an inflammation of the lungs. It is a disease characterized by inflammation of the mucous membrane of the large intestine, but involving something more than, and prior to this, especially in its epidemic form; but in what this prime essential pathological condition consists, in the existing state of the science, we cannot say. It is easier, it must be confessed, to show that it is not hepatic congestion, than to substitute any other explanation, which, with our present knowledge, would, of necessity, be equally hypothetical. We do not understand Dr. H. to deny the existence of inflammation in all cases of dysentery. Had he examined *post mortem* in his fatal cases, he would have found abundant evidences thereof. From the tenor of some articles that have occasionally fallen under our notice, we have been led to think that the fact of local inflammation being present in this disease, is not sufficiently appreciated by all practitioners. If so, it doubtless arises from examinations after death not having been made or witnessed. The morbid anatomy of dysentery should be well understood, with reference not only to the pathology, but the treatment of the disease. In this connection it may not be amiss to quote the results deduced from 231 dissections made in the Prague Hospital, by Dr. Finger, for which we are indebted to the British and Foreign Med. Chir. Review, No. for July, 1852.

This large number of dissections were made by Dr. Finger between the years 1846 and 1848, during which period dysentery prevailed, more or less, as an epidemic, at Prague. We give the following abstract by the reviewer, which we think will interest the reader:

1. The first or elementary form is termed intestinal catarrh, with especial implication of the follicles. In this stage, the large intestine viewed from without appeared more or less reddened, and was moderately distended with air; if the disease had been of short duration, its coats did not appear thickened. When the intestine was opened, a yellow, brownish, grayish red, or

sometimes purulent-looking fluid, more or less intermixed with fæcal matter, flowed out. The mucous membrane was reddened in patches of variable size, and was usually covered with a grayish-white or grayish-red pulpy substance; underneath this the membrane itself was softened, and was easily removed in the form of a grayish-red jelly-like pulp. The solitary glands were swollen to the size of millet or hemp seeds (*zu Hirse bis Haufkorn-grose*), by distension with transparent jelly-like mucus, which could be forced out by pressure; and were often surrounded by vascular zones. Afterward the mucus became opaque, thick and purulent, and then arose like (hemp-seed or lentil-sized) ulceration of the glands; these were at first solitary, then ran together by the destruction of the intervening mucous membrane, and formed ulcers varying in size from the original ulcer to that of a sixpence and larger; the borders of the ulcers were loose, ragged, swollen, and usually undermined. By the farther extension of these ulcers, very frequently large ulcerations arose, which spread over great part of the colon, and were connected by bridge-like portions of mucous membrane. Here and there little pits were formed by suppurated follicles, which sometimes passed through and even perforated the peritoneum. The submucous tissue formed, however, the most frequent floor of the ulcers.

When the process extended into the small intestines, it was found in an earlier stage than in the large, showing the affection of the ilium was subsequent to that of the colon. The follicles were here also affected. In two cases of the 231, the jejunum and ilium were diseased, while the large intestines were healthy. Of the large intestines, the transverse colon and the sigmoid flexure were most affected. In some cases there was extensive catarrhal redness (*catarrhalischer Rothung*) of the small and large intestines, with numerous small, isolated, usually round aphthous erosions.

The tendency to gangrene of the intestines was not so great in the cases of pure follicular disease, as when the next form was combined with it; and when it did occur, it attacked chiefly the undermined mucous membrane. By longer duration of the follicular disease, which sometimes lasted even for many months without attendant croupous exudation on the mucous membrane (which was chiefly the case in children and feeble anæmic adults), the intestinal coats were thickened and anæmic, and the mucous and submucous coats were softened, infiltrated with serum, and thereby thickened, either pale as if washed, or of dark, smutty, or slate-grey color from pigment, or they presented yellow or gray projections (*Buckeln*), between which ran enlarged and varicose vessels. The follicles were often collapsed, with sometimes a brown or black circle of pigment. The process could, however, be seen in various stages; in some parts the follicles contained fresh infiltration-matter, or had suppurated; in other parts they had healed. By successive attacks of portions of the mucous membrane, the disease thus lasted sometimes for months.

The cicatrization occurred by the borders of the ulcers becoming covered with a white more or less thick callus, which joined on to a more or less shining smooth tissue, which spread over the base of the ulcer. The cicatrices sometimes formed hard fibroid intersecting cords and elevations, by the side of which were often still swollen and suppurated follicles. The submucous tissue was, like the mucous, changed into a firm, thick, fibroid formation.

The mesenteric glands were found unchanged in cases of short duration,

they subsequently became swollen, injected, succulent, and softened, but without evident infiltration.

2. The foregoing changes were present in every case but one; but in addition there existed in many cases what Finger calls a "croupous exudation," which was either spread uniformly over great part of the mucous membrane, or was collected near the folds of the colonic mucous membrane. In color it was whitish, grayish-white, or reddish; it had a granular (*korniges*) or scaly appearance, and was usually easily detached. Under the microscope, besides epithelium, and mucous corpuscles, there was an amorphous molecular mass, often mixed with blood-discs, or at a later date with pus-cells. According to the varying condition of this exudation, the intestinal contents presented various characters, and sometimes, by the suppuration of the exudation, were chiefly made up of pus mixed with blood. Underneath the exudation the mucous membrane was often softened, sometimes to a great extent, was at first reddened, and afterward in chronic cases became anæmic, presented pigment-discoloration, and was thickened from serosity. The follicular lesion was combined with this exudation in 232 cases, and was absent in one, and was generally far advanced before the croupous exudation commenced on the other portions of the mucous membrane. Sometimes the follicular ulcers were themselves covered with croupous exudations, which contained no pus-cells. Many stages were generally seen in the same intestine; in some places the mucous membrane, deprived of epithelium and covered with exudation, presented many ecchymosed vessels running through it, and was easily detached in the form of a reddish pulp; at other places the membrane was bluish or slate colored, elevated in knobs, and with its submucous tissue converted into a thick callus-tissue, while the follicles of the neighboring membrane were filled with purulent infiltration, or were destroyed by ulceration.

The tendency to gangrene was considerable when there was croupous exudation, but was much less marked in the follicular disease. It occurred in about one-third of the whole number of cases. It was confined to small follicular ulcers in only three or four cases; in other instances it spread over a considerable extent of surface. The gangrene often occurred in a very short time, especially in persons previously healthy; and it was observed with surprise, that it was less marked in persons who, before the attack of dysentery, had labored under a blood-disease, such as puerperal fever, typhus, gangrenous tuberculosis or cancer. The coincident or subsequent conditions of the intestines were, peritonitis without perforation, peritonitis with perforation and effusion, and narrowing of the intestine from healing of the ulcers.

The reliance on calomel in the treatment of dysentery is predicated, theoretically, on the supposed causative connection of the latter with an affection of the liver. They who disclaim the hepatic hypothesis, of course must reject the inference which leads to the mercurial treatment. We are not prepared to deny in toto that calomel exerts any curative influence in dysentery; but we see no rational grounds for its salutary action in this disease aside from those which apply generally to the treatment of inflammations. The value of this remedy in the disease under consideration must be

determined mainly by the results of experience. Dr. H. gives a single case in which mercury being continued while all the symptoms were of a very unpromising character, the patient recovered. The reflection which the case suggested was natural, but he will hardly claim that its correctness is proved. He states that in some instances in which the liver did not seem to be implicated, a full opiate appeared to cut short the disease. But if the liver be implicated in a portion only of cases of the disease, it certainly does not furnish an essential element of its pathology. To be an essential element the hepatic congestion must be constant; otherwise it is only an incidental or accidental event. It must be considered rather inconsistent with the hepatic doctrine that a remedy supposed to lock up the biliary and other secretions, like opium, should be competent, in any cases, to arrest at once the disease. Not to dilate on the subject of treatment, for which we have not, at present, either time or space, we will reiterate our conviction that mercury in the treatment of dysentery has been vastly overrated. This conviction is based on comparative observations of the treatment of cases with, and without this remedy; and we venture to say that if our correspondent will employ opiates, without the addition of calomel, in a series of cases, his own observations will lead him to the same conclusion.

In estimating the effects of remedies in this disease, it is to be borne in mind that in its ordinary sporadic form it is very rarely fatal. Cases will recover pretty uniformly under different, and opposite plans of management. It is also to be considered that in its graver epidemic forms the intestine is apt to suffer lesions, which, in a certain proportion of cases, render recovery almost impossible. The relative proportion of cases may not, therefore, be a reliable test of the efficacy of management. We will add that our observations have led us to regard opium as emphatically the efficient remedy in the treatment of epidemic dysentery. The efficacy of opium in the graver forms of the disease, depends on its administration with reference not to quantity, but effect. There are certain conditions of disease in which the narcotic power of opium appears to be antagonized in a remarkable degree. This is true of some cases of dysentery. It may be given largely without any evidences of narcotism, and *must* be given largely to ensure efficient service. On this subject we may report some facts hereafter. Our desultory remarks are already so extended that we must bring this article somewhat abruptly to a close. We had no intention, when we commenced writing, of occupying more than a few lines. The subjects, however, are of so important a character, that it would be far easier to continue writing, than to find a convenient stopping place, were the article much longer than it is.

We are much obliged to Dr. H. for this, as well as former contributions to our pages; and should he do us the favor to practice upon the suggestion we have thrown out, we hope he will give us the results. If he resolves to give the opiate treatment a fair trial we beg him to bear in mind the principle to give the remedy with a view to effect; that is, in grave cases to prescribe doses which will be borne without narcotism, however large. If he does this he will be surprised at the amount which may be given with safety and advantage.

Health in Buffalo. — Epidemic cholera has prevailed, to some extent, in this city during the last two months. For the most part, it has been confined to the improvident and reckless portion of the community, comparatively few cases having occurred among the better classes. The sections of the city in which the disease has most prevailed, are low, and were formerly marshy. It has been observed, also, that, in these sections, cases were most frequent in houses, or blocks, over-crowded with inmates. In one street the disease suddenly broke out in several families coincidently with an excavation through the street for the purpose of laying water pipes; and its development in this situation is attributed by Prof. Hamilton, and others, to emanations from the upturned soil, acting in conjunction with the unknown special cause which is essential to the production of the disease. The reader will find in the article by Prof. H., on this subject, in the present number, a report of the facts upon which this opinion is based. The considerations adduced by Prof. H. merit careful attention. It is clearly enough established that the special cause of cholera, whatever it may be, rarely acts with sufficient efficiency to produce the disease, unless auxiliary causes be super-added. Cholera may thus be said to depend usually on local causes. These local causes, it is to be presumed, are diverse in their character, and generally several are combined. The most important are insalubrity of situation, over crowding, intemperance in food and drink, over exertion, mental excitement, fear, etc., etc. Notwithstanding the great amount that has been written on this subject, a collection of recorded facts embracing all the appreciable circumstances attending the origin of the disease in particular districts, and in individual cases, is still a desideratum. With adequate data of this description, it might be practicable to deduce, not only the influence of combined local causes, but their relative agency separately.

The sanitary reports of the Registrar General of Great Britain during the years that the cholera prevailed in England and Wales, have lately been

analyzed, and some important results determined, an account of which may be found in the Brit. and For. Med.-Chir. Rev., July, 1852. We allude to them thus briefly at this time, as it is our intention to transfer a digest of the important conclusions arrived at, in a subsequent number.

With respect to the number of cases that have occurred in this city, and the ratio of fatality, we are without definite information. The Board of Health did not begin to receive reports until some time after the disease made its appearance; and they have since adopted the silent policy — that is, no bulletins have been issued. Some cases are still occurring daily. After the cessation of the epidemic we shall endeavor to obtain the facts relating to its progress and history which the city records furnish.

Aside from cholera, the ordinary diseases of the season do not appear to be more frequent or severe than usual. Dysentery prevails to some extent, but, so far as our observations extend, the type of this disease is not malignant.

Gross on Urinary Diseases, etc. — Medical and other writers of our country have frequently complained of a disposition on the part of British critics to disparage productions emanating on this side of the Atlantic. If there are just grounds for such complaints, the signal favor with which the late work by Prof. Gross, of the University of Louisville, has been received, is some indemnification for former acts of injustice. The British and Foreign Medico-Chirurgical Review, in speaking of the admirable treatise on Diseases of the Genito-urinary system, a treatise of which the American medical profession may justly be proud, holds the following language: — “Dr. Gross is quite correct in asserting that all the treatises on this subject as yet published in the English language, are mere outlines, which no one has attempted to render at all worthy companions to such works as those of Lawrence on ‘Hernia,’ Mackenzie on the ‘Diseases of the Eye,’ Budd on the ‘Liver,’ and Culling on the ‘Testis.’ It has remained for an American writer to wipe away this reproach; and so completely has the task been fulfilled, that we venture to predict for Dr. Gross’s treatise a permanent place in the literature of surgery, worthy to rank with the best works of the present age. Not merely is the matter good, but the getting up of the volume is most creditable to transatlantic enterprise; the paper and print would do credit to a first-rate London establishment; and the numerous wood cuts which illustrate it, demonstrate that America is making rapid advances in this department of art. We have, indeed, unfeigned pleasure in congratulating all concerned in this publication, on the result of their labors; and experience a

feeling something like what might animate a long expectant husbandman, who, oftentimes disappointed by the produce of a favorite field, is at last agreeably surprised by a stately crop which may bear comparison with any of its former rivals. The grounds of our high appreciation of the work will be obvious as we proceed; and we doubt not that the present facilities for obtaining American books will induce many of our readers to verify our recommendation by their own perusal of it.*

Again, in comparing the work by Dr. Gross with a treatise on similar subjects by an English writer, Coulson, the reviewer adds:—“ We have already expressed our high appreciation of Professor Gross’s treatise; and it gives us much regret to feel compelled to state that the comparison of the two is, on almost every point, disadvantageous to Mr. Coulson. We acknowledge that we have tried his work by a high standard; but we think that we are fully justified in so doing, considering how long its author has been before the public as a surgical writer, and how high a position he evidently desires to attain. There is scarcely any point on which his work is not inferior to Dr. Gross’s; and we have been continually led to feel, in our examination of it, how seriously defective it is, alike in method and in completeness; so that its perusal has left us with the conviction, that scarcely any one subject has been satisfactorily treated, scarcely any point of pathology or practice fairly and fully disposed of. We can assure Mr. Coulson that we state this with regret, and with no other motive than that which arises from our desire to discharge our critical duty faithfully and impartially. That our verdict will be confirmed by any competent judge, who may take the same pains that we have done in the comparison of the two works before us, we have not the smallest doubt; and we have even a sufficiently good opinion of Mr. Coulson’s own candor, to believe, that if he will examine Professor Gross’s treatise for himself, he will admit our case to be so strong, that he will not feel it necessary to impute to us any bias to his disadvantage.”

Curators of Med. Department of University of Buffalo.— We are requested to state that the names of the curators of the Med. Dept. of the University of Buffalo, residing in this city, were inadvertently left out of the list of curators in the annual circular for 1852. The names of the gentlemen referred to, are as follows:— Drs. E. Wallis, Samuel Carey, S. F. Mixer, and J. B. Samo.

* Not having read the proof of the eclectic department, it had escaped us that this extract is already copied (page 242.) It will, however, bear repetition.—Ed.

Buffalo Medical College.—By the circular appended to this No. of the Journal, it will be perceived that the dissecting rooms in this College will be opened on the first Wednesday in November, for those students who desire to commence their winter labors at that time. This will give two months for prosecuting practical anatomy before the beginning of the regular term in January. Hospital instruction will also commence on the first Wednesday in November. We are authorized to state that anatomical material will be promptly supplied to any number of classes that may be formed after the dissecting rooms are opened.

Dr. Hugh B. Vandeventer, who has been appointed demonstrator of anatomy since the last session, will devote his entire attention to classes from the time of opening the dissecting rooms, through the preliminary and regular terms.

We are gratified to state that the portion of the college edifice originally designed for the museum, and hitherto unfinished, is now in progress of completion, and will be in readiness before the next session commences. When finished, it will be one of the most spacious and best arranged apartments for such an object in the country. It occupies one quarter of the entire building, is to be arched to the roof, surrounded by a gallery, and having space for a second gallery if required hereafter.

Buffalo Hospital of the Sisters of Charity.—We are gratified to announce that this admirably conducted charity has lately received from the state appropriation for hospitals out of the city of New York, nearly fourteen thousand dollars. This has enabled the trustees to purchase a large portion of the vacant lots surrounding the hospital, leaving a balance sufficient to erect another wing, which will considerably enlarge its accommodations for the sick. The new addition is under contract, and already in progress. The increase of room will render it practicable to isolate fever cases more effectually than has hitherto been possible owing to the limited number of wards. The flourishing condition of the institution is a source of great satisfaction to its numerous friends.

Dr. Hamilton's Fracture Tables.—We commence in the present No. the publication of fracture tables prepared under the direction of Prof. Hamilton. These tables have been prepared with care, and embody facts which


Prof. H. has taken great pains to collect. They will form a most valuable contribution to surgical science, and especially will be found useful for reference in suits for mal-practice, a large proportion of which concern cases of fracture. We cannot doubt that the profession will hold in high estimation the service which Prof. H. has rendered by this laborious undertaking.

Transactions of the American Medical Association. — We copy the following notice from the *Stethoscope*, edited by Dr. Gooch, one of the Secretaries of the Am. Med. Association:

“The readers of the *Stethoscope* are hereby informed that the volume of the *Transactions of the American Medical Association*, containing all the interesting papers and reports presented at the meeting held in Richmond in May last, and making about 1000 pages, will soon be issued. Members, or institutions represented in that body, will be furnished with one copy for \$3, or two copies for \$5, *if the amount is remitted to us, or to Dr. D. Francis Condie*, at Philadelphia, *previous to the 1st September*. After that time the price of the volume will be *five dollars*. We advise those desiring to be prompt.”

We are informed in a letter from Dr. Condie, that the volume will be ready for distribution early in October.

Medical Schools. — A new medical school has been organized in Cincinnati. Prof. Mussey, lately connected with the Ohio Med. College, has become one of the faculty of the new school. The chair of anatomy and of the theory and practice of medicine, in the Medical College of Ohio, made vacant by the resignation of Drs. Mussey and Bell, have been filled by the election of Drs. Drake and Cobb, lately connected with the Med. Department of the University of Louisville. The vacant chairs in the latter institution have been filled by the election of Drs. Palmer and Flint, of the Medical Department of the University of Buffalo.

 A communication from Dr. Rogers, received too late for insertion in this No., will appear in our next issue.

Appointment. — We learn that Dr. Robert E. Rogers, of the University of Virginia, has been appointed professor of chemistry in the University of Pennsylvania, in place of his brother, the late Dr. James B. Rogers. This is a first rate appointment, for we do not know a man who *teaches chemistry* better than Dr. R., in the whole country. — *Stethoscope*.

BUFFALO MEDICAL JOURNAL

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NO. 5.

ORIGINAL COMMUNICATIONS.

ART. I.—Prize Essay.* *On Variations of Pitch in Percussion and Respiratory Sounds and their Application to Physical Diagnosis.*
By AUSTIN FLINT, M. D., of Buffalo, N. Y.

"Happy am I in my own estimation if I have thrown any light, in this Memoir, upon any clinical question; and especially if I have stimulated the zeal of our young practitioners for the diagnostic studies which constitute, in my mind, one of the most beautiful parts of our art."—ANDREY, *On Diseases of the Heart.*

Very little attention has hitherto been paid to variations in the pitch of sounds heard in the practice of percussion and pulmonary auscultation. The sibilant and sonorous râles of bronchitis, it is true, are distinguished from each other chiefly by a contrast in pitch, but, as respects the remainder of the physical signs pertaining to pulmonary disease, they appear not to have been much studied in this aspect, and even the facts that have arrested notice do not seem to have been applied, save in a very limited degree, to

* In deprecation of the charge of egotism which the reproduction of the following essay in this place may occasion, the editor begs to state, that he has been repeatedly asked if it would not appear in this Journal, and wishes to that effect have been expressed by a number of subscribers. The idea, however, had not been entertained until

physical diagnosis. By most writers on physical exploration, pitch modifications, except* in the sibilant and sonorous râles, are not recognized, no allusion whatever being made to them. In the second edition of the able and comprehensive work by Dr. Walshe, of London, recently republished in this country, the subject is noticed more distinctly than by any other author with whose writings I am acquainted. Dr. Walshe enumerates among the elements involved in the different modifications of respiratory sounds in health and disease, variations in pitch; he also mentions several important facts with respect to these variations. But he apparently loses sight of their practical applications, making no reference to them in connection with the diagnosis of individual thoracic diseases. Barth and Roger state, as briefly as possible, the fact that the bronchial respiration is higher in pitch than the cavernous. But, in general, as just remarked, nothing is to be found relating to this subject in standard treatises† on percussion and auscultation.

What facts are disclosed by the study of percussion and respiratory sounds, in health and disease, with reference to variations of pitch? How far are these facts available in diagnosis? The latter question is much the more important of the two; or, rather, the importance of the first question depends mainly upon that which belongs to the second. The subject of physical exploration has already suffered from over-refinement. To be practically

information was received that, owing entirely to delay in making application on the part of the author, the portion of the Transactions of the Am. Med. Association embracing the essay, was already printed and the type distributed, so that no extra copies could be furnished. In order to circulate it among those of his friends who may not procure the volume of Transactions, or who might like to have the essay in a detached form, no alternative remained but to insert it in the Journal, or to reprint it separately. A medium course has been adopted. The essay proper is inserted as a contribution to the Journal; and the appendix to the essay, occupying nearly as much space as the essay itself, is introduced as supplementary matter, at the expense of the editor and publishers. The reader will please to observe that the size of the present No. is increased by the number of pages which the appendix occupies. The editor ventures to hope that to this course the majority of his readers will not be disposed to take exceptions.

* The vocal sign *ægophony* should perhaps also be excepted.

† In connection with this statement, it is proper to give the bibliography to which it applies. The works consulted are as follows:—

Laennec, edited by Forbes; Walshe on the Heart and Lungs; Hughes's Physical Diagnosis of the Lungs and Heart; Barth and Roger's Practical Treatise on Auscultation; Gerhard on Diseases of the Chest; Prize Dissertations on Physical Exploration, by Drs. Holmes, Bell, and Haxall; Blakiston on Diseases of the Chest; Latham on Auscultation and Semeiology; Swett on Diseases of the Chest, edition of 1852; Bowditch's Young Stethoscopist; Lawson's Lectures, in the Western Lancet.

available in the hands of all intelligent practitioners, the art must be simplified as much as possible. New distinctions, unless obviously tending to enlarge or facilitate our means of diagnosis by physical signs, were they not only true, but ever so interesting to those whose attention is specially directed to this department of medicine, would be of questionable utility, so far as they serve to render the pursuit more complicated and difficult. In proposing, therefore, to submit the results of my observations and experience, thus far, in answer to the foregoing inquiries, I am influenced by the belief that, considered in this point of view, the physical signs of pulmonary affections admit of being enlarged in their application to diagnosis, and rendered more readily available for practical purposes. A single additional remark by way of introduction: the pitch modifications of sound, as before intimated, opening a field of study in physical exploration as yet but little cultivated, and to which, so far as relates especially to auscultation, my attention has but recently been directed, propriety and prudence dictate, not only caution for the deductions from a number of data somewhat limited, but a certain amount of distrust in a kind of observation in which the liability to error cannot be at once fully estimated. In view of these considerations, the conclusions which I shall present are advanced as propositions to be confirmed by further researches, the object of this paper being, in a great measure, to invite the investigations of others in the same direction.

The subject naturally resolves itself into the study of — First, *percussion* sounds; and, second, the pulmonary sounds disclosed by *auscultation*.

SECTION I.

ON ATTENTION TO PITCH OF SOUND IN THE PRACTICE OF PERCUSSION.

It is hardly necessary to gather recorded data with reference to the variations of pitch of *percussion* sounds, since observations can be so easily made, and repeated, to any extent, by every one. I shall, therefore, give my experience under this head briefly, and in a general way, *i. e.*, without citing particular cases. The use of the term *pitch*, as applied to percussion sounds, may be questioned, and questionable. Whether it be correctly applicable, in a musical sense, to these sounds, or to those of auscultation, I shall not stop to inquire. It suffices if the idea it conveys be intelligible, and if it denote an appreciable distinction. A percussion sound elicited from the chest, if not entirely flat, has a certain amount of resonance. This resonance may differ in degree; in other words, the sound is more or less clear, or dull. ▲

peculiar quality of tone arises from the fact that the chest contains air in vesicles, and hence the sound may be distinguished as the *vesicular resonance*. Percussion sounds present, occasionally, deviations from this vesicular quality; an example the most familiar being the change occasioned by the presence of air between the pleural surfaces, or in a large excavation, constituting what is known as *tympanitic resonance*. Now, in addition to these classes of variations, the resonance on percussion in different parts of the chest, in morbid conditions, present a disparity analogous to, if not identical with that which constitutes the distinction called *high* or *low* in comparing musical tones — in other words, a disparity in pitch. This kind of disparity is recognized by directing attention to, and comparing the sounds elicited from different regions, just as is done with reference to musical notes in determining whether they are relatively *higher* or *lower* in key. The faculty of distinguishing discords, in other words a musical ear as it is called, doubtless assists this discrimination, and it is probably true that the ability to detect a slight variation will correspond with the delicacy with which deviations from the harmony of musical tones are appreciated.

What pathological significance belongs to the fact of a variation in the pitch of resonance? The practical importance of the subject depends, of course, on the answer to this question. The question leads to the enunciation of the following law: *An elevation of pitch always accompanies diminution of resonance in consequence of pulmonary consolidation.* In other words, *dullness of resonance is never present without the pitch being raised.* This proposition is to be verified by observations which may be readily made. Cases of tuberculous deposit, with marked disparity of resonance at the summit of the chest on one side, will furnish examples, and such cases are sufficiently numerous. But the law may be tested in the healthy subject. The præcordial region presents well-marked dullness compared with the corresponding region on the right side. The percussion sound in the former situation will answer as a type of resonance, modified by an increased proportion of solid material incident to tuberculous or other disease.

A practical advantage of attention to pitch, in employing percussion, consists, thus, in its confirming the existence of dullness. It furnishes additional evidence thereof, and adds positiveness to the conclusion, when the mere diminution of resonance might not be with certainty determinable. To the musical ear, more especially if skilled in discriminating musical tones, a disparity in pitch is more quickly, as well as more clearly distinguished. It is far easier to appreciate a contrast in this point of view, than to determine a slight, or moderate preponderance of resonance in the percussion sound on

one side of the chest. I have frequently illustrated this in teaching physical exploration. A person just essaying to distinguish relative dullness of resonance on percussion often fails in its recognition, even when it is pretty strongly marked. He is unable to perceive a difference which is sufficiently apparent to the practiced percussor. Under these circumstances, I have frequently inquired if the learner were able to sing, or play on any musical instrument. If he replied in the affirmative, I requested him to compare the sounds on the two sides of the chest as if they were musical notes, with reference to pitch, and the disparity then became immediately manifest. If I am misled with respect to the assistance to be derived in this way, by the results of my own experience, the error involves a kind of self-deception which it is very easy to impart, for many to whom I have pointed out this method of determining dullness, have assured me that they have found it of great utility in practice. By directing attention to the pitch, an intelligent student, with some musical powers and cultivation, will become an expert in appreciating a slight dullness sooner than he can attain to proficiency in the manipulating tact of percussion.

Another practical advantage, certainly equal to, if not greater than the foregoing, is derived from the fact that a distinct disparity of pitch may be apparent, in some instances, when a disparity in the amount of resonance is inappreciable. The correctness of this statement my observations lead me not to doubt. Others, however, are to be satisfied by the evidence of their own perceptions. This fact is stated by Dr. Bowditch in his work entitled the *Young Stethoscopist*, and I do not recollect meeting with a similar statement by any other writer. He says, "A difference of note or of pitch between two corresponding parts is not uncommon, when there is no real flatness in either. It occurs in cases in which the lung is not by any means impervious to air. Sometimes in the early stages of phthisis, and in pneumonia in its early or latest stages:" (page 60, second edition.) In another place, he remarks: "Any degree of dullness, even the slightest difference of note or of pitch, if confined to the upper part of the chest, between the portions equidistant from the spine or sternum, augments my suspicion of the existence of tubercles:" (page 88, *ibid.*) To measure the exact amount of mere resonance, so as to make an accurate comparison of two sounds that differ but slightly in this respect, it must be evident on a little reflection, is not easy. The truth probably is, that the difference in pitch is perceived when a slight disparity exists which is ordinarily called dullness. And this leads me here to remark, it is not to be supposed that practical chest explorers have not been accustomed to be guided by pitch modifications of sound.

Variations in this respect have been perceived without recognition, if this antithesis is allowable. The disparity has been apparent, but attributed solely to diminished resonance or dullness. This remark will be found to apply equally to respiratory, or to percussion sounds. This, however, does not prove that the practice of physical exploration will not be materially aided by directing the attention to the variations in pitch *as such*.

A contrast in percussion sounds in many of the diseases of the chest is sufficiently obvious, requiring no special delicacy of discrimination. This is the case in pleuritis with effusion, in the second stage of pneumonitis, and in phthisis when the tuberculous deposit is abundant. Attention to pitch, under these circumstances, may be said, with truth, not to be of much importance. A flat, and a notably dull sound are readily enough discovered. It is in connection with the diagnosis of the early stage of tuberculosis that the point under consideration is particularly important. Of the importance of the diagnosis of the disease in this stage it is not necessary to speak. In view of the employment of means to arrest the further progress of tuberculization, and the fair prospect, in many instances, of effecting that object, there is perhaps no end in practical medicine more desirable than to determine positively the presence of tubercle before the disease has made much progress. Any addition to our means of giving precision to the diagnosis of incipient phthisis is a valuable contribution, not only to our science, but to our art, inasmuch as the prospect of saving or prolonging life is greater in proportion as the affection is earlier recognized.

A disparity at the summit of the chest, however slight, is a sign to which great weight should be attached in deciding on the presence of tubercle. Practical auscultators generally will concur in the statement above quoted from the treatise by Dr. Bowditch, relative to this point. If, therefore, by attention to the pitch of resonance, we are better able to appreciate a slight shade of difference, or to recognize its existence with greater certainty, there can be no question concerning the service thereby rendered to physical exploration.

It is hardly necessary to say, that the usefulness of the means of early diagnosis in phthisis is to be regarded in both a positive and negative aspect. In other words, it is quite as important to be able to exclude tuberculosis, by the absence of physical signs, as to determine its existence by the presence of these signs, and, hence, improvements in physical exploration have practical relations of equal value, in either direction.

Another advantage to be derived from attention to pitch consists in the facility of developing a disparity in situations in which but little resonance *can be elicited*, owing to the intervention of bone, muscle, and fat between

the integument and walls of the chest. A late writer on diseases of the chest says, that the mammary region in females "is of no value in percussion."* The same author farther states that, "posteriorly, no advantage can be derived from percussion; the muscles are too thick about the shoulders to admit of a decided advantage even in cases in which the pulmonary condensation is much greater than in incipient phthisis." These statements are to me surprising. A disparity of pitch is frequently very apparent in the scapular region, both above and below the spine, in cases of tuberculosis, and I have met with it here when it was not marked in the infra-clavicular region. I have demonstrated this fact repeatedly during the short period that has elapsed since the publication of the work from which the above extracts are taken. To dispense with the information from percussion sounds over the scapulæ would considerably impair the diagnostic resources in incipient phthisis. The thickness of the muscular coating in other parts of the chest, and even the mamma of the female, do not prevent a comparison of sounds with reference to pitch; but, for reasons obvious enough to the pathologist, the comparison in these situations is less important in diagnosis, save in diseases in which the coarsest exploration would suffice to develop flatness, as in pneumonitis and pleurisy.

Another advantage relates to the diagnosis of heart diseases. Percussing from the clavicle, the acromial angle, and the axilla, in a direction toward the site of the heart, the points at which there is a distinct modification of pitch will mark the limits of the organ in these directions, while the point of impulse, if perceptible, will give the lower boundary. This method, thus, will be found useful in determining the degree of enlargement in hypertrophy and dilatation, and the amount of effusion in pericarditis.

The significance of variations in pitch, as of other points of disparity, in the diagnosis of thoracic diseases, of course, is predicated on the equality of both sides of the chest, in this respect, in health. Does this equality exist? I cannot answer this question quite so explicitly as I could desire. Pitch modifications will necessarily result from causes which at the same time occasion obvious dullness, such as spinal curvatures, former pleurisies, arrested tuberculous deposit, &c. These causes, and others, moreover, may occasion, in some instances, a disparity in pitch, when the changes are not sufficient to produce obvious dullness. Comparison in pitch affording a more delicate means of discovering a slight deviation from the symmetry of the two sides, it is not improbable that healthy chests may be less uniformly equal in this

* A Treatise on Diseases of the Chest, by John A. Swett, M. D., 1852, pp. 17 and 256.

particular than with respect to a marked diminution of resonance on one side. The examination of a large number of individuals presumed to be free from pulmonary disease would alone afford the data for answering the above question positively. I have frequently examined chests, at the summit more particularly, with reference to this point, but without recording the results. Within a few days I have noted observations in twenty-two persons. In all save *three*, the pitch was equal in the infra-clavicular region, to which attention was more particularly directed. In *one*, there was elevation of pitch on the right side, apparently owing to greater development of the muscles on this side. In another, the same disparity existed, the same explanation not being so obvious. There was no reason to suspect disease of the lungs in this latter instance. The patient was a young female in perfect health. In the third instance, the percussion sounds were clear and equal at the summit anteriorly, but over the right scapula there existed evident dullness compared with the sound over the left scapula. This person was a mechanic, and the muscular mass in the interscapular space was notably thicker on the right side. It is undoubtedly desirable that examinations of this kind should be multiplied.

In practicing percussion with a view to *pitch*, observance of the rules which are familiar to every practical auscultator is peculiarly important; I allude to the position of the patient, care to strike successively on the two sides at corresponding points, and with an equal force, &c. Tact in eliciting a loud distinct sound is desirable. A smart stroke is frequently more effective for this object than a light feeble blow, not, however, using sufficient force to occasion pain. The only pleximeter and percussor with which I have any practical acquaintance are the fingers. It has often occurred to me that other instruments might in some instances be more satisfactory, but I have nothing to say on this topic from my own experience.

SECTION II.

ON ATTENTION TO THE PITCH OF SOUND IN THE PRACTICE OF AUSCULTATION.

My attention has been directed to the variations in the pitch of respiratory sounds more recently, than to those belonging to percussion. It is for a few months only that I have made the former objects of study. During this time, as leisure and opportunities have permitted, I have been accustomed to note observations on the different cases of thoracic disease coming under my notice; and the views which I shall submit in this section will consist of

deductions from the data thus collected. Considering the novelty of the subject, as well as its importance, I have thought it best to present, not only the conclusions based on what data I have gathered, but a transcript of the data themselves. As an appendix to this section, therefore, I shall give a brief synopsis of the observations which have been made in cases of disease, amounting to over forty in number. A larger collection of data for generalization is undoubtedly to be desired; but it is to be borne in mind that the aim of this memoir is by no means to exhaust, but rather to open the subject, as one claiming attention, with a view to its practical application to physical diagnosis. I would here repeat that what I shall advance as the results of observations made up to this time, I wish to be considered in the light of propositions to be confirmed, enlarged, and perhaps corrected by continued investigations. As already intimated, to be able to estimate fully the liability to error of observation in a new field of study like this, may require the fruits of a longer experience. I am led to make this remark the more, in consequence of being sensible that a facility in distinguishing pitch variations in the respiratory sounds is much increased by practice. After what I have said, it is but justice to myself to add that, in making the observations I have noted, I have spared no effort to render them reliable.

The point of departure for the study of pitch modifications, as of other physical signs of disease, is the examination of the chest in health. The first inquiry, therefore, in presenting this branch of the subject, will be, *what variations in pitch belong to healthy respirations?* To this question I will devote a distinct division of this section.

VARIATIONS IN THE PITCH OF SOUNDS IN HEALTHY RESPIRATION.

With a view to the study of the variations to be found in a healthy chest, I have noted the results of physical explorations, more or less complete, of twenty-seven individuals, presumed to be entirely free from any thoracic disease. This number of observations, although too few to settle the numerical ratio of the occurrence of particular phenomena, will probably suffice for the present object, which is merely to determine some general principles to serve as the basis of the study of morbid deviations.

Of these twenty-seven individuals twenty-one were males, and six females. The ages were various, all, with a single exception, being above childhood, and none being advanced in life. The majority were young persons from twenty to thirty years of age.

The normal respiratory sounds are resolvable into three divisions, according

to the parts of the pulmonary apparatus whence they emanate, viz., the trachea, the bronchi, and the vesicles. Named from these their anatomical relations, they are the *tracheal*, the *bronchial*, and the *vesicular*. The character of pitch belonging to the sounds produced in these three situations may be considered under distinct heads.

1. *Tracheal Sounds*.—On placing the stethoscope over the trachea, the respiratory sounds are found to be notably high in pitch. The development, that is to say the loudness of the sounds, in ordinary respiration, varies considerably in different persons. In some persons they are quite intense, in others feeble, and even indistinct until the persons are requested to breathe forcibly, when they become much increased. The relative altitude of pitch is immediately perceived on comparing the tracheal sounds with the vesicular respiration heard on listening over the chest. Two sounds are heard uniformly in this situation, viz., the sounds of inspiration and expiration. An interval occurs between these two sounds. The inspiratory is relatively shorter than the expiratory sound. The sound of expiration is higher in pitch than that of inspiration. These results have been uniform in all the observations that I have made.

2. *Bronchial Sounds*.—Explorations for the bronchial respiration were made anteriorly near the claviculo-sternal junction, and, in a smaller portion of the cases, in the interscapular space posteriorly. Of *twenty-three* examinations in the former of these situations, the bronchial sound was appreciable in *twenty*, and not discoverable in the remaining *three* instances. Of *fourteen* examinations in the latter situation, *i. e.*, the interscapular space, it was appreciable in *all* but *one* instance. As respects the two situations, in some cases, the sounds were more developed posteriorly than anteriorly, and in other cases the reverse. There was considerable difference in different cases in the degree of development, or intensity of the sounds. In several, no sound was discernible during ordinary respiration, but it became apparent on increasing the force and quickness of the respiratory movements.

The pitch of the bronchial sounds is high, probably not much below that of the tracheal sounds, but it did not occur to me to compare the two with reference to pitch, until I began to write on the subject. The point has not much importance. The elevation of the pitch of the bronchial, compared with the vesicular murmur, is to be borne in mind. A comparison of the former, as heard in the interscapular space, and near the sternum anteriorly, with the latter as heard over other parts of the chest, showed, in every *instance*, a distinct and notable disparity.

Other interesting points of distinction pertain to the *bronchial sounds*.

Recollecting the relations, in size and length, of the two primary bronchi to each other, the inquiry arises, if the bronchial respiration heard in corresponding situations on both sides of the chest, before and behind, is uniformly equal in intensity and pitch? With respect to intensity or loudness, of *thirteen* examinations it was thought to be somewhat more developed on the *left* side in *four* cases, on the *right* side in *five*, and in *four* cases no difference in this particular was apparent. As regards *pitch*, the result was different. Of *twenty* examinations, the pitch is noted to have been distinctly higher on the *right* side in *fifteen*, no difference being appreciable in the remaining *five* cases. This disparity in pitch between the two sides was not observed in all instances, both anteriorly and posteriorly. Excluding *seven* instances in which it is merely noted that the pitch was higher on the *right* side, without specifying whether before or behind, the notations in the remainder of the cases are as follows: 1. No disparity in *front*, but notably higher on the *right* side *behind*. 2. Higher on the *right* side *before* and *behind*. 3. The same. 4. The same. 5. Higher on the *right* side *behind*, where the bronchial sound is alone heard. 6. Slightly higher on the *right* side in *front*, no difference being appreciable *behind*. 7. Higher on the *right* side *before* and *behind*. 8. The same.

The existence of a disparity in the bronchial sounds of the right and left side, attributable to the difference in size and length of the bronchial divisions of the trachea, has been pointed out by Dr. Gerhard, of Philadelphia, but the fact has not been recognized by all auscultators. Dr. Stokes, on the contrary, has taught that a disparity frequently exists, but that the bronchial character is apt to be more apparent on the *left* than on the *right* side. Dr. Gerhard speaks of the respiration being more *blowing* or *tubular* on the right side. If my observations are correct, both Dr. Gerhard and Dr. Stokes may be right, the discrepancy arising from confounding different elements which enter into the bronchial respiration. It is occasionally more developed, that is to say, louder on the left than on the right side. So far Dr. Stokes is correct, but in a very large majority of persons the pitch is higher on the right side, while this relative elevation never obtains on the left; and, as the pitch is one of the most prominent of the elements of the bronchial respiration, the foregoing results accord with the fact pointed out by Dr. Gerhard. The observations of these two distinguished writers are thus apparently not really inconsistent with each other.

In degree, or loudness, the normal bronchial respiration presents in different individuals great variations. In this point of view, it has no uniformity.

It is never intense, and, in some persons, is quite faint, requiring a forcible respiration to develop it sufficiently to study its characters; and it is not appreciable in all persons.

In a certain proportion of the cases in which the normal bronchial respiration is heard, not uniformly, a sound of expiration is appreciable. Of *thirteen* instances in which the inspiratory sound was present, a sound of expiration also existed, the latter being absent in *four*. The sound of inspiration may be heard behind and not in front, or on the right side and not on the left. The facts with respect to these variations contained in the few observations I have noted are as follows: Of the above *thirteen* instances, in *six* the sound of expiration was perceived only on the *right* side, either in front or behind, and in *seven* it was appreciable behind, and not in front.

The sound of expiration appeared to be higher in pitch than the sound of inspiration, in every instance in which attention was directed to this point. This fact is noted in *nine* observations, an exception thereto not being noted in any. Thus, in this trait the normal bronchial respiration resembles the tracheal.

In every instance in which attention was directed to the succession of the sounds of inspiration and expiration, an interval between was observed. This feature belongs to the bronchial, in common with the tracheal respiration.

It will thus be noticed that, in the more important of the elements of the tracheal and bronchial sounds, they are similar. They both want a distinctive quality which will be seen to characterize the vesicular respiration. They are both high in pitch, in this respect probably not differing much from each other. The inspiratory sound in both is short. The expiration in each is higher in pitch than the inspiration; the difference with respect to this point between the tracheal and bronchial respiration being that in the latter an expiratory sound is heard in a certain proportion of cases only, while it is uniformly heard in the former. In each, an interval occurs between the sounds of inspiration and expiration. The characters distinguishing the two kinds of respiration consist in the greater intensity of the tracheal sounds, the uniformity with which they are heard in different persons, and the constant presence of an expiratory sound.

3. *Vesicular Respiration.* — The vesicular respiration, as is well known, differs from the tracheal and bronchial in having a peculiar quality, not capable of being very definitely expressed by language, but which is readily enough appreciated by the practiced ear. The terms *breezy* and *expansive*, perhaps, approach as near a definition as can be done by words. This

quality of sound is *sui generis*, and familiarity with it is very necessary to the practical auscultator. The peculiarity alluded to may be characterized as, *par excellence*, the *vesicular quality*. I shall have occasion to refer to it hereafter by this title.

In what other particulars does the vesicular respiration differ from the two varieties already noticed?

The difference in pitch is striking. The pitch is uniformly and notably lower than that of the tracheal or bronchial respiration. This was true of all the cases examined with reference to healthy variations. As a point of difference, it is one to which, in connection with the subject of this memoir, special attention is desired.

An expiratory sound is appreciable in a less number of instances than is the case with the bronchial respiration. Of *nineteen* examinations with reference to this point, the expiration was heard in *nine*. The pitch of the expiratory sound, when it is heard, compared with that of the inspiration, is another point of special interest in the present inquiries. Of *eight* observations in which the facts relating to this comparison were noted, the pitch of the expiration was lower in *all* but *two* instances. In these two instances, the pitch of expiration was higher in the right infra-clavicular region toward the sternum, no expiratory sound being appreciable, in either instance, except in the situation first mentioned. So far as these examinations go, then, the rule is, that the sound of expiration in vesicular respiration is lower in pitch than the sound of inspiration, with occasional exceptions* at the summit of the chest on the right side toward the sternum. This rule, without the exceptions, is stated by Dr. Walshe in the last edition of this work on the heart and lungs.

Aside from the foregoing differences, the duration of the inspiratory sound, in the vesicular respiration, is longer than in the tracheal or bronchial. The expiratory sound on the other hand, is as notably shorter; and when a sound of expiration is appreciable, it is nearly or quite continuous with the sound of inspiration. The latter statement I give on the authority of others, and my own unrecorded experience, not having taken pains to note the facts relating to the point in the healthy observations made with reference to the subject

* The sound of expiration is oftener heard when the vesicular sounds are exaggerated in supplementary respiration. In the clinical observations of cases of pneumonitis, the presence of a sound of expiration on the healthy side, and the lowness of pitch compared with the sound of inspiration, are noted in several instances. See Appendix, First Series. Cases of Pneumonitis.

under consideration.* There are some other variations in pitch which are of practical interest. The pitch of vesicular respiration at the upper part of the chest is higher than at the lower. This I have noted in *eleven* observations, which were all that have been made relative to this point. The pitch does not appear to be sensibly raised by increasing the force of the respiratory movements. It seems to remain the same in ordinary and forced respiration. This I have noted in *eight* observations, being all that were made relative to the point. The vesicular respiration has a sensibly higher pitch at the summit of the right than of the left chest, in a large proportion of individuals. Of *fifteen* examinations relative to this point, the disparity just noted was found to be more or less marked in *eleven* instances, no difference between the two sides being apparent in the remaining *four*. The practical bearing of the several facts stated in this paragraph on physical exploration in disease will be at once obvious.

In conclusion, while the tracheal and bronchial sounds were found to be essentially the same in character, the circumstances distinguishing each from the other being rather incidental than intrinsic, the vesicular respiration, on the other hand, contrasted with the two former, exhibits some striking points of dissimilarity. It has that inexpressible peculiarity distinguished as the vesicular quality. The inspiratory sound is lower in pitch. The expiratory sound, when heard, save in a limited situation, is lower than the sound of inspiration, the reverse of this being true of the tracheal and bronchial sounds. Distinctive features less prominent, but important, are the greater length of the inspiratory sound, the shortness of the sound of expiration, the continuity of these two sounds, and the fact that in a large proportion of cases the sound of inspiration alone is appreciable.

VARIATIONS OF PITCH IN RESPIRATORY SOUNDS IN CASES OF DISEASE.

In treating of morbid variations, it will be most convenient to consider, under separate heads, the different diseases in which they have been studied. The divisions will then correspond with those of the clinical observations given in this memoir.

Pneumonitis. — The present inquiry respecting pitch variations of sound has no reference to râles. The crepitant râle in the first stage of pneumonitis

* The continuousness of the expiration with the inspiration is noted in several of the clinical observations, in which the lungs on one side were free from disease. See Appendix, First Series. — Cases of Pneumonitis.

frequently drowns other sounds. The modifications which relate to the present subject are incident to the solidification belonging to the second stage of the disease. More or less crepitation, as is well known, may continue into this stage, being heard at the end of the inspiratory act. Under these circumstances, enough of the respiratory sound may be heard, before it is lost in the crepitation, for the pitch to be compared with that of the respiratory sound on the healthy side.

The observations pertaining to pneumonitis, among those which I have collected, amount to *twelve*. This number might have been increased since my attention has been directed to this subject, but it is probably sufficient for the present object. In each of these observations, the pitch of respiration was notably high, presenting a striking contrast, in this respect, with the respiration on the healthy side. The disparity, as I have noted in one of the observations, is sometimes as obvious as between a fife and German flute. Of these *twelve* observations, an expiratory sound was more or less developed in *eight* instances. In *two*, a sound of expiration was not appreciable; in *one* instance, it was appreciable, but too feeble for its characters to be studied, and in *one* instance the fact as to the presence or absence of the expiratory sound is not noted. The pitch of the sound of expiration was higher than that of inspiration in every instance in which attention was directed to this point, save *one*. In the excepted instance, the observation was made in an early stage of the disease; the crepitant râle persisting, the expiratory sound was feeble, and the fact of its being lower is stated somewhat distrustfully. The expression is, it appears to be lower in pitch. The correctness of this observation seems to me open to doubt, for, as I have had occasion to notice, there is a liability to err in estimating the pitch of a faint sound of respiration. Its feebleness may, without due attention, give rise to the impression that it is lower in pitch. With this doubtful exception, the elevation of the pitch of expiration over that of inspiration was uniform, wherever the expiratory sound was sufficiently appreciable to study its characters.

Aside from what relates to the above pitch modifications, the facts noted with respect to other characters are as follows: The inspiration was shortened in every instance in which attention was directed to this point. This is noted in *five* observations. An interval was observable between the sounds of inspiration and expiration in all the observations containing information on this point: viz., in *six*. The expiration was prolonged whenever attention was directed to this point. It is noted in *five* observations.

The characters, then, belonging to the respiratory sounds incident to pulmonary solidification in pneumonitis, are, elevation of the pitch of both

inspiration and expiration; a higher pitch of expiration than of inspiration, whenever the former is heard, almost if not quite uniformly; a shortened inspiratory sound; a prolonged sound of expiration; and an interval between the sounds of inspiration and expiration.

The respiration in the second stage of pneumonitis, as is well known, is sometimes characterized as *tubular*, and sometimes as *bronchial*. As contrasted with the vesicular respiration, the first term is certainly significant. The sound may also, with great propriety, be called bronchial, for, on comparing the several points just mentioned with the different elements entering into the normal tracheal and bronchial sounds, more especially the latter, it is perceived that they are essentially identical. The bronchial respiration in pneumonitis is neither more nor less than the bronchial respiration heard in the healthy chest in certain situations. One of the chief points of difference between the normal bronchial and tracheal sounds, it has been seen, is the greater intensity of the latter. The respiratory sounds in pneumonitis vary considerably, in different cases, in degree. The intensity appears to be a contingent, rather than an intrinsic element. In some instances, the development of sound is fully equal to that heard over the trachea, and it may be even louder than is the tracheal respiration in some individuals. This comparison of the respiration in pneumonitis with the tracheal and bronchial sounds would naturally enough lead to inquiries respecting the physical philosophy of the identity in their characters. Such inquiries, however, are entirely foreign to my present purpose. To contrast the respiration of pneumonitis with the normal vesicular murmur would be to repeat the points of difference already noticed in connection with the variations incident to health. The disparity is, of course, based on the same circumstances as that between the tracheal and bronchial, and the vesicular sounds.

Pleuritis.—The observations relating to pleuritis were made but in *three* cases. So far as these cases go, they show that, when considerable effusion exists, the sound over the compressed lung is high in pitch, presenting, also, more or less of the other characters belonging to the bronchial respiration. After the fluid has been removed nearly, or quite, by absorption, more or less pleuritic adhesions limiting the expansibility of the affected side, the respiratory sound, having resumed the vesicular quality, is relatively feeble when compared with the sound on the other side which is exaggerated, but a disparity in pitch may not be appreciable. If, however, the quantity of effusion has been large, so as to have distended greatly the chest, and led to considerable contraction after the absorption is effected, then, in connection

with some permanent dullness on percussion over the affected side, the pitch of respiration may be higher than on the other side, although the vesicular quality and other characters of the vesicular respiration are resumed.

For the exemplification of these statements, the reader is referred to the Appendix, Second Series of Observations.

Gangrenous Excavation, and Pneumothorax with Perforation. — I have had an opportunity of observing, since my attention has been directed to this subject, the respiratory sounds in but one instance of gangrenous excavation, and one of pneumothorax, both occurring in the same case.

Over, or near the site of the gangrenous excavation, which was considerable in size, the sound was noted to be non-vesicular, and low in pitch; as respects the latter, presenting a striking contrast with the sound heard at the summit of the chest, in the same case, where the lung was solidified by compression. Over the latter, the sound was equally non-vesicular, but high in pitch.

The entrance of air into the pleural cavity through a perforation of about the size of a goose-quill occasioned a sound, non-vesicular, and low in pitch.

For a brief account of the case, together with the autopsical appearances, the reader is referred to the Appendix.

Tuberculosis. — The physical signs incident to tuberculosis have relation not merely to the presence of the morbid deposit, but to its amount, and the stage of the affection. The study of the disease, with reference to the principles of diagnosis, thus presents itself under somewhat different aspects. During the past two months, I have recorded observations, with respect to variations in the pitch of respiration, in *twenty-five* cases of tuberculosis. I propose to examine the results of these observations under four heads, conforming to the distribution of the cases into four groups in the Appendix to this memoir. Following this arrangement, I shall consider, *first*, the observations in cases in which the tuberculous deposit was supposed to be small in amount; *second*, when the quantity of the deposit was comparatively abundant; *third*, when the disease had proceeded to the stage of excavation; and *fourth*, when the affection appeared to have been arrested.

I. Observations in Cases of Small Tuberculous Deposit. — *Eleven* of the cases fall under this subdivision. In each of these cases, the pitch of respiration was found to be higher over the site of the tuberculous deposit,

as determined by relative dullness on percussion. In order to test the reality of this variation, and ascertain whether the perceptions might not be influenced by preconceptions, in several of the cases included in this and the next subdivision, auscultation with reference to the pitch was practiced prior to percussion, or any other examination of the chest. In every instance, the pitch modification indicated the side on which relative dullness on percussion was afterward found to exist. I have frequently caused this experiment to be made by several young gentlemen, medical students, who have accompanied me in my hospital visits, and with uniform success. That is to say, the difference in the pitch of respiration furnished a ready criterion of the existence, or greater abundance of the deposit on one side, before resorting to percussion or other signs.

In some of the observations, the mere fact of elevation of pitch on one side is stated. I was at first satisfied with simply ascertaining this fact, without attending particularly to the expiration, or other points. In several of the cases, however, the observations were more comprehensive.

It is noted that the sound of expiration was appreciable in *six* cases, and inappreciable in *two*; in *three* of the observations, nothing being stated on this point. The expiration is stated to have been prolonged in *four* cases. In *five* cases, the expiration was thought to be either equal to, or higher in pitch than the inspiration: or, to be exact, in *three* of these five cases it was thought to be higher; in the other *two*, it is stated to have been as high, if not higher.

In some cases the vesicular quality was obviously impaired, in connection with the elevation of pitch; but in other cases, disparity in this respect was not obvious. So, with regard to development or intensity of sound, it was sometimes diminished, but not invariably.

To determine the presence of a small amount of tuberculous deposit, in other words, the diagnosis of incipient phthisis, is frequently one of the most difficult problems in practical medicine. Certainly, the employment of physical exploration with reference to this point requires as much accuracy, care, and skill as in any of its applications. Several of the auscultatory signs which have more or less importance as indicating the presence of tubercle, are only occasionally present, and the evidence furnished by them is circumstantial, rather than positive. This remark applies to the signs denoting circumscribed bronchitis, pneumonitis, or pleurisy, in proximity to the morbid deposit; the sibilant or mucous râles at the summit of the chest; the crepitant râle, or friction sound, heard in the same situation. Other signs which

are doubtless of importance in this connection, must be regarded as somewhat equivocal, such as the jerking respiration, and a prolonged expiration. The most direct and constant of the signs incident to a small tuberculous deposit is the modification known as the *rude*, sometimes called the *harsh* or *rough* respiration. This modification has relation to the present subject, while, to consider the other signs referred to, in this connection, would be irrelevant.

For the novice in the study and practice of physical exploration, the *rude* respiration is generally, of all the signs, the one most difficult to be apprehended. There is probably no sign which causes the teacher or writer more embarrassment in his efforts to explain clearly. Take, for example, the description by Dr. Hughes, author of a very lucid treatise on physical diagnosis. Speaking of the *rude*, compared with the *vesicular* respiration, he says, "it is the *forte* of the same note, but on a loose and jarring string;" and, again, contrasting it with the normal vesicular murmur, he says, "In the one, the same soft breeze passes through a greater number of trees; in the other, the breeze is increased to a moderate gale." Not only is there a singular indefiniteness in the idea conveyed by this language, but the analogies selected for illustration are defective in correctness. The *note* is not the same in *rude*, as in normal respiration, and the comparison to the gale is calculated to give the erroneous impression that the *rude* respiration is necessarily louder than the normal, which is so far from being true, that it may be in a notable degree less developed; the intensity being a variable element, having nothing to do with the distinctive character of the sign. The truth is, these terms *rude*, *rough*, and *harsh*, are unfortunate; they are not only inexpressive, but tend rather to mislead in the apprehension of the modification referred to. The sound is not intrinsically *rude*, or *rough*, or *harsh*. Even a well-marked bronchial respiration, to which the sound under consideration is an approximation, can hardly be said to have the qualities indicated by these terms. The bronchial respiration is not very unlike in character the endocardial sound, which is characterized as a *soft* bellows murmur. Whatever appropriateness the designation has, is based chiefly on the fact that, in the *rude* respiration, the peculiar expansive, breezy attribute of the vesicular respiration, which has been referred to as the *vesicular quality*, is more or less impaired.

To form a correct idea of the modification usually termed *rude*, &c., it must be analytically decomposed, and the nature of its elements determined. It is an approximation to the bronchial respiration. It exhibits an incipient

development of the character distinguishing the bronchial from the vesicular respiration. One of the most striking of these characters is the change in pitch. The pitch is raised. The vesicular quality is diminished; hence it approaches to a tubular or blowing respiration. The inspiration may be somewhat shortened, and occasionally a sound of expiration becomes developed and prolonged, constituting an important rhythmical variation. By attention to these several points, much will be gained in practically recognizing the modification; but it is not easy to find a satisfactory title to be substituted for the names generally in use. Of the several elements mentioned here, as in the case of the bronchial respiration, it seems to me the pitch modification is the most striking, and the most readily appreciated, while it is probably the most constant.

The expiration deserves to be distinctly noticed. Considerable importance has been attached to a prolonged expiration, since its occurrence was pointed out as a frequent sign of early tuberculization, by James Jackson, the younger, of Boston, Mass. The best practical authorities have recognized this sign as a valuable contribution to the art of physical exploration. It is not, however, a constant modification. Its absence, therefore, does not furnish ground for the conclusion that tubercle does *not* exist. Moreover, it may exist as a normal peculiarity, and consequently alone it is not perfectly reliable. In the few instances among the cases I have collected in which a prolonged expiration was present, and the pitch noted, it was found to be higher, or as high as the sound of inspiration. Now, in health, over the greater part of the chest, if a sound of expiration be appreciable, it is found to be distinctly lower in pitch than the sound of inspiration. May it not be that the elevation of pitch in expiration has a diagnostic value fully as great, or even greater than when the inspiration is thus modified? May it not be that, in some cases in which the inspiration is not sensibly raised in pitch, the expiration may exhibit a change in this respect, and thus the latter furnish a more delicate sign of the presence of the disease? These are interesting, and possibly important questions, which are to be satisfactorily answered only by an accumulation of observations.

It occurs to me here to remark (what would have been more appropriate in connection with pneumonitis,) that the observations of Jackson and others have showed the expiration to become earliest, and in the most striking degree changed, in the development of the bronchial respiration. This fact would lead to the presumption that, in the modification generally known as the rude respiration, the change would be first and most distinctly declared

in the expiration. The expiration in the development of bronchial respiration is said first to exhibit the bronchial character. Although not hitherto recognized as such, I can have little doubt that the change thus noticed consists chiefly in the elevation of pitch. This produces a more striking change in the expiration than in the inspiration, because the change is really greater in amount. The expiration in health is lower in pitch, while in the bronchial it is higher than the inspiration. The degree of modification is, therefore, greater than in the case of the inspiratory sound.

In conclusion, it is highly important to bear in mind that the pitch of respiration at the summit of the right chest is frequently higher than on the left side, and that this may, or may not coexist with a greater development and prolongation of the sound of expiration on the right side. The practical inference is that these modifications, when present on the right side, are, in themselves, less significant than when they occur on the left side, and, in the former situation, are to be less confidently relied upon, especially in the absence of marked rational evidences of tuberculous disease.

II. *Observations in Cases in which the Tuberculous Deposit was abundant.*—The cases arranged under this head are comparatively few in number, amounting to but *five*. In each of these cases, on the side in which the tuberculous deposit was most abundant, as declared by the relative dullness on percussion, and other signs, the respiratory sound was found to be higher in pitch than on the side in which the morbid product was less in amount. In *three* of the cases, the disparity in the pitch of respiration is simply noted in general terms. In the fourth observation, it is stated that there existed a prolonged expiration, with an interval between the sounds of inspiration and expiration. The expiratory sound, however, was so feeble that its pitch was not ascertained.

In the fifth observation, an expiratory sound is stated to have been present, higher in pitch than the inspiration, with an interval between the two sounds.

So far as these observations go, they lead to the conclusion that the respiratory sound over the site of an abundant tuberculous deposit, when it is appreciable, or not obscured by râles, will be found to be elevated in pitch, and presenting more or less of the other characters distinguishing the bronchial respiration.

III. *Observations in Cases of Tubercle advanced to the Stage of Excavation.*—The cases coming under this head are of special interest, having

reference to the study of the variations in the pitch of respiration occasioned by the presence of a cavity or of cavities within the chest. It has been seen already, in the single case of perforation of the lung and gangrenous cavity, presented in another division, that cavernous respiration, under these circumstances, was low in pitch. This was true in all the cases of tuberculous excavation embraced in this subdivision. The cases studied with reference to the cavernous respiration are *seven* in number. In *one* of these cases, however, the existence of cavities which had been predicated on physical signs, was disproved by post-mortem examination. This case is included in the series for convenience, and serves to illustrate certain points in diagnosis which are to be carefully attended to, in order to avoid an erroneous conclusion. Of the remaining *six* cases, in *two*, autopsical examinations were made, showing the existence of cavities, and their situation with reference to the parts of the chest where the physical signs of excavation had been determined, as noted in the recorded observation before death. The dissection, in both instances, was made at my request, by Dr. John C. Dalton, Jr., to whom I am indebted for reports of the morbid appearances. The attention of the reader is particularly invited to the cases which include autopsies. (See Observations 1 and 3, under same head in Appendix.) In the *four* cases without autopsies, the existence of excavations, and their particular situations, are not, of course, demonstrated. The evidence consists of the physical signs, and other facts, pertaining to the histories.

So far as the observations go, they show, as already remarked, that the pitch of the cavernous respiration is low, contrasting in this respect strongly with the high-pitched bronchial respiration. This is not claimed as a newly discovered fact. It is stated by Dr. Walshe, in the last edition of his treatise on diseases of the lungs and heart; and is implied in the language used by Barth and Roger. It is not, however, dwelt upon by either with much emphasis, and, if I am not mistaken, it does not generally receive, with practical auscultators, attention as a diagnostic criterion.*

The pitch of the expiration, when it enters into the cavernous respiration, does not appear to have attracted notice. In the cases I have observed, in which the sound of expiration was appreciable, the pitch was lower than that of inspiration. This forms a striking feature distinguishing the cavernous from the bronchial respiration. In the latter, as has been seen, the

* For example, in a treatise on disease of the chest, issued during the present year, I find the following statement: "A naturally cavernous respiration exists over the trachea and larynx."

sound of expiration, when appreciable, is higher, or at least equal in pitch to the sound of inspiration.

The cavernous sound is, of course, devoid of what has frequently been referred to as the *vesicular quality* of normal respiration. This point is generally noted in the observations. It is a point important to be borne in mind in determining the presence of a cavernous respiration; else the vesicular murmur, with its low pitch of inspiration, followed by a still lower expiration, may be thought to denote an excavation. Obs. 7 (see Appendix) illustrates the liability to error incident, in part, to not observing sufficiently this precaution.

The following elements, then, combine to form the cavernous respiration: **A blowing sound**, that is to say, a sound in which the vesicular quality is absent; a low pitch compared with that of the bronchial sound; an expiration, if present, lower in pitch than the inspiration.

Keeping in view these characters, it is probably easy, in most instances, in which there are cavities of any considerable size, to determine, with precision, their particular situations, by ascertaining the limits circumscribing the space over which a well-marked cavernous respiration is found to be present. We are assisted in defining the boundaries of these spaces by the fact that excavations are generally surrounded by tuberculous consolidation which will be likely to raise the pitch, and thus exhibit the cavernous sound in stronger contrast. If, however, this were not true in any instance, but, on the contrary, supposing a cavity to be surrounded with lung giving a vesicular sound, we can generally satisfy ourselves of the lowness of pitch (having already satisfied ourselves of the non-vesicular character of the sound in question,) by comparing it with the normal bronchial respiration heard at the sterno-clavicular junction, or with the tracheal respiration. A cavity at the apex of the lung will be more readily discovered by the respiration if it be anteriorly and superficially situated. It is hardly necessary to add that it must be more or less empty, and communicate more or less freely with the bronchial tubes. To ascertain the pitch and other characters, also, the respiration in the surrounding lung must be free from râles.

Tuberculous excavations, as is well known, generally coexist with a greater or less amount of crude tuberculous deposit. In cases, therefore, which present the physical signs of cavities, we have a high-pitched non-vesicular sound, *i. e.*, the bronchial respiration, more or less, over parts of the chest, at its summit, in which the cavernous variations are absent. The observations in the cases of tubercle advanced to excavation furnish illustrations of this fact.

In searching for the sites of cavities, the stethoscope is obviously preferable, since it enables us to circumscribe better the source of sounds. In immediate auscultation, the sounds are received from a wider circuit. The character of the respiration, as heard by the latter method, will depend on the predominance of excavations or solidification within a certain distance of the part to which the ear is applied. If a large cavity exists, or several cavities, although the intervening tissue be solidified so as to give dullness on percussion, the pitch of respiration may be lower than on the other side in which the tuberculous disease is less advanced. There, the combination of a low, non-vesicular respiration, with dullness on percussion, affords presumptive evidence of the stage of excavation. This is illustrated by Obs. 1. See Appendix.

The importance of attention to the pitch variations in determining the presence of the cavernous respiration is enhanced by the fact that other physical signs of excavations are inconstant and unreliable. A tympanic resonance on percussion cannot be depended on. It may be absent although cavities exist, and may be present without excavations. The cracked pot variety is rarely discoverable, and may be due to the expulsion of air from the bronchial tubes or other morbid conditions. Pectoriloquy, which was regarded by Laennec, as a pathognomonic sign, is not only frequently absent, but does not possess that distinctive significance attributed to it by the illustrious founder of auscultation. Between pectoriloquy and intense bronchophony there is no intrinsic difference. In other words, pectoriloquy is but a variety of bronchophony, and may occur not only in cases with excavations, but when the lung between a bronchial tube and the ear is in a state of solidification from tubercle or effused fibrin. For this statement I have the authority of Dr. Walshe. But, if it be substantiated by a sufficient number of observations that a cavernous respiration is uniformly low in pitch, with a sound of expiration lower than the inspiration, these traits, taken in connection with its blowing or non-vesicular character, will suffice for its recognition, and consequently, not only for the diagnosis of the stage of excavation, but the localization of the cavity, the estimation of size, &c.

IV. *Arrested Tubercle*.—During the time I have been engaged in making observations with reference to the present subject, two cases have fallen under my notice in which the tuberculous disease appeared to have been arrested; in other words, the morbid product had not continued to accumulate, nor had the primary deposit advanced through the processes of softening and excavation. The cases have not any special importance in connection

with the subject of this memoir. I have given them under a distinct head in the Appendix, because they are distinguished from the other cases by the fact of the disease having ceased to make progress. In this point of view, the cases are not without interest. In the first case, the disease was declared a year previous, and some cough and expectoration still remain. In the second case, the rational symptoms marking the period of the tuberculous deposit occurred several years ago, and the patient is entirely free from any symptoms of pulmonary disorder. In both instances, the disease has left permanent evidence of slight injury to the lung on one side, consisting of disparity of resonance on percussion, and an elevation of the pitch of respiration.

The arrest of tubercular disease is doubtless much more frequent than has been heretofore recognized; and with the views now entertained by the most intelligent practitioners respecting the pathology of tubercle, and the proper ends of treatment, there is reason to hope that cases of this kind will become more numerous. As one of the causes of a lasting disparity between the two sides of the chest in the physical signs developed by percussion and auscultation, this is to be borne in mind in the examination of patients with reference to existing pulmonary affections.

In the practice of auscultation with the view to pitch variations, the same rule of course obtains which is applicable to physical exploration in general, viz., the sounds of the two sides of the chest, in corresponding situations, are to be listened to in succession, and compared with each other, always recollecting to make due allowance for certain natural deviations which are determined by observations made on persons in health, and also, as just remarked, bearing in mind the changes incident to pre-existing disease. That the respiratory sounds may present strongly-marked variations in pitch will be readily understood, if it be observed for a moment how easy it is to breathe audibly with the mouth in unison with musical notes, and even in this way to hum a tune. Almost any one who has any fondness* for music is practically familiar with this. Or, as an illustration, let the pitch of sound be observed when different words or letters are whispered, after the ingenious method of representing the variations in pitch of the bellows murmur of the heart, suggested by Bouillaud and Hope. The letters *R S*, and the syllable *w/ho*, thus exhibit

* The pitch of a respiratory sound may be readily imitated by modulating, with the lips, breath sounds in the mouth. I have sometimes found this useful in comparing the pitch of sound in the two sides of the chest.

quite different degrees of altitude in pitch. It is easy to distinguish the difference in pitch among endocardial sounds. There is no greater difficulty in appreciating variations in this respect in pulmonary sounds. A musical ear is doubtless an advantage, as also some musical cultivation; but neither is absolutely essential. A medical student who has been accustomed to follow me in my examinations, and who is unable to distinguish one tune from another, finds no difficulty in recognizing the pitch variations in respiration, and in several instances has made and noted observations by himself which have corresponded with mine. The practice of comparing the pitch of respiratory sounds doubtless leads to an increased facility in directing the attention to, and clearly perceiving variations. This I have found in my short experience, since I have been particularly interested in the subject.

In conclusion, the more important practical deductions submitted in this section are recapitulated in the following summary. These deductions, I would again repeat, are submitted as propositions to be confirmed, enlarged, or corrected, by further investigations:

1. In the second stage of pneumonitis, the inspiratory sound is high in pitch, followed by an expiratory sound, which is frequently, if not generally higher in pitch than the sound of inspiration; these traits being found in conjunction with more or less of the other characters which belong to the bronchial respiration.

2. In cases of small tuberculous deposit, or incipient phthisis, the most striking modification of the respiratory sound is the elevation of pitch. This elevation of pitch is an important element of what is generally known as the *rude, rough, or harsh* respiration. If an expiratory sound be appreciable under these circumstances, it may be as high, or higher in pitch, than the sound of inspiration, and the variation of pitch in the former is greater, inasmuch as the pitch of expiration in the normal murmur is lower than that of inspiration. Elevation of the pitch of expiration, therefore, may be found to be valuable as a sign of incipient phthisis in some cases in which the variation in the inspiration is not marked.

3. If the tuberculous deposit be more abundant, the pitch of respiration is in a more marked degree elevated. The expiratory sound, if appreciable, will be likely to be as high, or higher in pitch than the sound of inspiration. More or less of the other characters of the bronchial respiration are at the same time present.

4. In pleurisy with effusion, the pitch of respiratory sound is elevated, in conjunction with more or less of the characters of the bronchial respiration,

over the parts of the chest lying above the compressed lung. In cases of large effusion, after its complete removal by absorption, the affected side may continue to present a variation in pitch, the symmetry of the two sides being permanently impaired, in this respect, after the vesicular quality of respiration is regained.

5. In cases in which tubercle has advanced to the stage of excavation, the site of a cavity of considerable size is indicated by a blowing sound, low in pitch, with an expiratory sound (if appreciable) lower in pitch than the sound of inspiration. These traits constitute the elements of the cavernous respiration, and the cavernous respiration is the most constant and reliable of the signs of an excavation.

If the cavity be very large, or there are several cavities, the respiration may be modified to such an extent that, on immediate auscultation, over the whole summit of the chest, it may present the cavernous characters. This may be the case while dullness on percussion shows the existence of more or less solidification in connection with the cavities. The coexistence of relative dullness on percussion, and a low-pitched blowing respiration, denotes the predominance of excavation.

The cavernous respiration may also be present in cases of excavation from circumscribed gangrene, and in pneumothorax with perforation.

6. In arrested phthisis, the traces of the disease may be manifested by a permanent variation in the pitch of respiration, in connection with more or less dullness on percussion at the summit of the chest on either side.

APPENDIX.

CLINICAL OBSERVATIONS.

THE following account of clinical observations embraces a synopsis of the characters of respiratory sounds relating to the subject of the foregoing essay, as noted at the time the examinations in the cases severally were made. Aside from these, details pertaining to the histories of the cases will be introduced, only so far as they seem to be important with reference to the diagnosis, the stage of disease, or the appearances found after death. The object will be to condense as much as practicable, with due regard to the points just mentioned.

For convenience of reference, the observations are distributed into different nosological groups; cases of each disease forming a distinct series. The classification of the observations corresponds with the arrangement of subjects in the foregoing essay, so that reference from either to the other may be easily made.

FIRST SERIES.—PNEUMONITIS.

Observation 1.—Dec. 30, 1851. Hospital patient. Pneumonitis affecting lower lobe of right lung. Second stage of the disease. Loud tubular respiration over the site of the solidified lung. Pitch of respiration notably high.

Obs. 2.—Jan. 27, 1852. Wm. Hasmer. Hospital patient. Disease of seven days' standing. Flatness on percussion over the lower third of chest, posteriorly, on the *left* side. The respiratory sound is bronchial or tubular, the inspiration somewhat shortened; a prolonged expiration is present, with an interval between the sounds of inspiration and expiration. Over the inferior third of the *right* side, posteriorly, the respiration is vesicular, supplementary, without any sound of expiration. The pitch of sound on the left side, compared with the right, is in a marked degree high, the contrast nearly as striking as between the sound of the fife and German flute.

Obs. 3.—Jan. 29. George Young. Hospital patient. Has been confined to bed four days. Dulness on percussion exists over the inferior third of the left chest. Over this portion the respiratory sound is notably higher in pitch than over the corresponding region of the right side. The inspiration is somewhat shortened. There is a faint but prolonged expiration, and an interval between the two sounds.

Obs. 4.—Jan. 30. Mary Gaball. Hospital patient. Disease occurring as a complication of continued fever, third day after taking to the bed.

Pneumonitis confined to lower lobe of the right lung. Inferior third of right chest, posteriorly, flat on percussion. The pitch of respiration over this region is notably higher than on the corresponding region of the left side. The expiration is somewhat prolonged, and higher in pitch than the inspiration. A short interval between inspiration and expiration.

Obs. 5.—Wm. Wrick. Hospital patient. Disease affecting upper and middle lobes of the right lung. Respiration on the right side, posteriorly, bronchial; the pitch high, inspiration shortened, and the expiration prolonged. The pitch of the expiration is notably higher than that of inspiration. Anteriorly, the respiratory sounds on the right side too feeble to determine the pitch, &c.

On the 9th of February, the respiratory sound on the right side anteriorly sufficiently developed to study characters. It is bronchial, higher in pitch than on the left side; expiration prolonged; inspiration shortened, and an interval between the two sounds. The pitch is distinctly higher than on the left side. On the left side the respiration is supplementary. A faint sound of expiration is appreciable, continuous with the sound of inspiration, and apparently lower in pitch than the inspiratory sound.

Obs. 6.—Feb. 10. Catharine Finn. Hospital patient. Disease affecting middle lobe of right lung. In first stage. Crepitant rale in mammary and axillary regions. The crepitation obscures the recognition of pitch. On the left side the respiration is supplementary. An expiratory sound is present on this side, continuous with the sound of inspiration, and notably lower in pitch than the inspiratory sound.

Feb. 11. Crepitant rale still heard in mammary region of right side. Posteriorly, over the middle third of the right side, there exists moderate relative dulness on percussion. A feeble crepitant rale is heard in this region, but not enough to obscure the pitch of respiration, which is notably higher than over the corresponding region on the left side. The expiratory sound, over the middle third of the right chest, posteriorly, is feeble, and appears to be lower in pitch than the sound of inspiration.

Obs. 7.—Feb. 12. Michael Russell. Hospital patient. Eighth day of disease. Posteriorly, over middle and lower thirds of right chest, flatness on percussion. In these regions respiration notably higher in pitch than over the corresponding situation on the left side. The sound of respiration is too feeble to determine the pitch.

Obs. 8.—Feb. 12. Philip ———. Hospital patient. Disease of a week's standing. Marked dulness on percussion over the lower and middle thirds posteriorly. Respiratory sound in these regions, in ordinary respiration scarcely appreciable; on forcible respiration more developed, without an appreciable sound of expiration. The inspiration is higher in pitch than on the left side.

On the left side the inspiration is followed by a continuous sound of expiration, which is lower in pitch than the sound of inspiration.

Obs. 9.—Feb. 12. James Whalen. Hospital patient. Disease affecting the upper and middle lobes of right lung, of nine days' standing. Flatness on percussion exists over the upper and middle thirds anteriorly, and over the whole side posteriorly. Percussion over the lower third anteriorly, on the right side, elicits clear resonance. Anteriorly, over the upper and middle thirds, a crepitant rale is heard at the end of the inspiratory act.

Over the right side, posteriorly, the respiratory sound is notably high in

pitch, and quite intense over the whole side. The sound of expiration is prolonged to an equality, in duration, with the inspiration, and there is an interval between the two sounds. The expiratory sound is higher in pitch than the inspiratory. This is the more marked the higher up the chest the ear is applied.

Obs. 10.—Feb. 13. Jane McNolty. Hospital patient. Disease occurring as a complication of typhus, having taken to bed six days ago. Marked relative dulness on percussion exists over the middle third of left side, posteriorly, and in the axillary region of same side. Over the inferior third, anteriorly and posteriorly, a gastric tympanitic resonance is transmitted. The crepitant rale is heard in the axillary region at the end of the inspiration. Over the middle third, the inspiratory sound is moderately loud, with no appreciable sound of expiration. It is somewhat shorter than the sound of inspiration over the corresponding region of the right side. It has less of the vesicular quality, and is notably higher in pitch. In the latter respect the disparity between the two sides is most marked. A faint crepitant rale is heard at the inferior portion of the right chest posteriorly.

Obs. 11.—Feb. 13. John Jarry. Hospital patient. Disease of a week's standing. Marked relative dulness on percussion exists over the lower third of right chest posteriorly, and distinct bronchophony. No rales observed. The respiratory sound on both sides quite feeble. Over the lower third of right side the pitch is notably higher than over the corresponding region of the left side. It is notably higher than over the superior portion of the chest on the same side. The inspiratory sound over the lower third of right side is relatively shorter in duration. There is a sound of expiration obviously higher in pitch than the sound of inspiration, with an interval between the two sounds. On the left side there is a short, continuous sound of expiration, which is obviously lower in pitch than the sound of inspiration.

Remarks.—In this case, two medical students,* who had been present at previous examinations of patients, and were accustomed to verify the results of my observations, were requested to explore and compare the relative pitch of respiration on the two sides, and the relative pitch of the inspiration and expiration on both sides. Without being acquainted with the result of each other's exploration, or of mine, the conclusions in the three instances were found to be the same. The foregoing statement is noted in connection with the observation.

Obs. 12.—Feb. 19. Fitz Morris. Hospital patient. Fourth day of disease. Disease seated in the lower lobe of right lung. Crepitant rale was apparent yesterday, and is now heard at lower part of chest posteriorly. Bronchophony over these regions. The respiratory sound is loud, non-vesicular, on high pitch compared with the sound on the left side. An expiratory sound is heard, higher in pitch than the sound of inspiration, with an interval between the two sounds. On the left side, no sound of expiration is appreciable.

* Mr. J. R. Smith and Mr. Charles Ap. A. Bowen.

SECOND SERIES.—PLEURITIS.

This series will embrace but *three* cases; in *two* of which, the patients were laboring under the subacute form of the disease, with in *one* case considerable, and in the *other* large effusion. In the remaining case, the patient had recovered from chronic pleurisy, and the variations, were, therefore, those incident to the permanent changes left by the disease.

Obs. 1.—Jan. 4. Conrad Reushling, Hospital patient. Chronic pleuritis affecting the left side. The lower and middle thirds of this side, anteriorly, are flat, and the upper third relatively dull on percussion. Posteriorly, lower third flat, and the upper and middle thirds dull. Over the upper and middle thirds, posteriorly, respiration is feeble, and no sound of expiration appreciable. Over the right side the respiration is supplementary. The pitch of sound on the left side, posteriorly, is notably high. This, aside from feebleness, is the most striking point of disparity on comparing with the left side. Anteriorly, over the right chest, no sound of respiration appreciable, except just below clavicle, and here the pitch was obscured by dry crackling.

Feb. 1. Up to this date, the patient progressively improved, and had become sufficiently restored to be discharged from the hospital. On the left side, at the summit, there is now slight dulness on percussion; at the inferior portion, the dulness is greater. The left chest is one half an inch less in circumference than the right. The left shoulder is somewhat depressed. The left side expands less at the summit than the right. The respiratory sound, on the right side, is exaggerated, supplementary. On the left it is relatively feeble. In both sides, the respiratory sound has the vesicular quality, a feeble sound of expiration being appreciable, and the duration of the respiratory sounds being apparently equal. After careful examination, I cannot satisfy myself of a disparity of pitch between the two sides.

Remarks.—The following remarks were appended at the time of the last observation. This case affords an opportunity of comparing an exaggerated, with a relatively diminished vesicular sound. The physical and rational signs show that the patient has labored under subacute pleurisy, from which he has nearly recovered. There is slight condensation of the left lung from compression, and less expansibility from pleuritic adhesions. Here are two opposing circumstances as respects their probable influence on the respiration. The defective expansibility might be expected to occasion a pitch of sound somewhat lower than that on the right side, the respiration in the latter being, at the same time, exaggerated. On the other hand, the condensation would be expected to elevate the pitch. These apparently conflicting circumstances serve to antagonize each other, the fact being that the respiratory sound at the summit of the chest on both sides is not far from equal. It may be inferred from this case that exaggeration of the vesicular murmur does not tend, in a marked degree, to elevate, nor, on the other hand, a feeble vesicular murmur to lower, the pitch of sound.

Obs. 2.—Feb. 8. Ortmann. Hospital patient. The effusion, in this case, was large, compressing the lung into a small space at the upper and posterior part of the chest. The disease was in the right side. No respiratory sound is to be heard save in the clavicular, post-clavicular, supra-spinous, and interscapular regions. Over the supra-spinous, and interscapular regions, the sound is feeble, non-vesicular; the expiration louder and higher in pitch than the inspiration. The inspiration is shortened, the expiration prolonged, with an interval between the two sounds.

On the left side, in the corresponding regions, the respiratory sound is supplementary. A sound of expiration is heard, which is about one-fourth the length of the inspiration. The sound of expiration follows that of the inspiration without any appreciable interval. In pitch, the expiration on this side is distinctly lower than the inspiration.

In pitch, the inspiration, as well as expiration, is higher on the right than on the left side; but this is more especially marked in the expiration.

Obs. 3.—Feb. 10. Eliza Moore. This patient, eighteen months ago, had subacute pleurisy of the left side. The chest, on this side, was greatly distended, the heart being dislocated, so that its pulsations were seen and felt to the right of the sternum. She has remained in hospital up to this present time, but is now quite well, and has been employed as a domestic, performing hard labor, such as washing, scrubbing floors, &c. The left chest is considerably contracted. The impulse of the heart is now felt somewhat above the normal situation.

The left chest is relatively dull on percussion. The respiratory sound has the vesicular quality, but is higher in pitch than on the right side. There is a short, feeble, continuous sound of expiration. The pitch of the expiration appears to be lower than that of the inspiration. Near the right sterno-clavicular junction, the respiration is bronchial; the inspiration high in pitch, with an expiration higher than the inspiration. It is more intense, and on a higher pitch on the right than on the left side, near the sterno-clavicular junction; this being the reverse of the vesicular respiration, which is higher in pitch on the left side.

The pitch of the vesicular respiration is not increased by a deeper and stronger act of respiration.

† THIRD SERIES.—GANGRENE AND PNEUMOTHORAX.

Under this head, a single case only will be embraced. This case occurred in private practice. The patient, aged about forty-five, had suffered, for between one and two years, with occasional attacks of epilepsy. He had for many years been addicted to the use of stimulants. The mental faculties were somewhat impaired, the memory especially. On the 28th January, after a severe epileptic paroxysm, pulmonary symptoms became developed. The physical signs of pneumonitis and pleurisy being absent, the pulmonary affection was supposed to be bronchitis. On the 5th February, when I again visited in consultation, there were the physical evidences of solidification of lung at the middle third of the right chest anteriorly and posteriorly, and a dirty expectoration with a characteristic fetid odor. It was evident that the patient was laboring under pulmonary gangrene. On the 8th, I again saw the case, and at this time the fetid expectoration had ceased, and he presented the physical signs of pneumothorax, viz: tympanitic resonance at the upper and middle thirds of the chest anteriorly, and metallic tinkling at the inferior

third, just below the nipple. On the 9th, I noted as follows: "The physical exploration was brief, owing to the great prostration of the patient. On the right side, over the upper and middle thirds, anteriorly, and on the upper third, posteriorly, percussion gives a tympanitic resonance. Below, flatness on percussion. The respiration over mammary region is non-vesicular, and low in pitch. Above this region, in front, the sound is indistinct. On the upper third, posteriorly, the respiration is high in pitch. Below, feeble, but low in pitch."

The patient died on the 10th, and the chest was examined on the 13th, by Dr. John C. Dalton, Jr., who has furnished the following statement of morbid appearances:—

"Autopsy of Mr. S——, February 13th, 1852. On opening the cavity of the right pleura, a considerable quantity of rather fetid gas escaped, and on raising the sternum the same cavity was found to contain about two pints of a dingy, yellowish-gray purulent fluid. The pleural surface at the anterior part of the chest was covered by a thin coating of recent lymph.

"The right lung, reduced to about one-fifth of its natural size, was compressed against the spine and the posterior wall of the chest. Before removing the fluid from the pleural cavity, a pipe was inserted into the trachea, and on inflating the lungs the air escaped freely in large bubbles, from a point situated toward the posterior part of the right lung, about the junction of its upper and middle thirds. The right lung, removed from the chest, was found to be completely carni- fied by pressure, except in those parts occupied by disease. No air-cells were visible anywhere, and the whole lung was destitute of crepitation. The lobes were adherent to each other by recent lymph. The upper third of the lower lobe was occupied by gangrene, which had reached an advanced stage, the pulmonary tissue forming a soft, shreddy, disintegrated mass, of a fetid odor, and a dirty, grayish color and infiltrated with purulent fluid. A gangrenous cavity of considerable size, had apparently existed at this spot before the compression of the lung. The opening by which it had communicated with the pleural cavity was about the size of a goose-quill, and situated posteriorly at the very uttermost portion of the lower lobe. The limits of the gangrenous portion of the lung, were well defined, but only a very little inflammation existed in its neighborhood, the solidification of the pulmonary tissue, except just outside the limits of this gangrenous cavity, being entirely of a passive character, and due to its compression by the pleuritic effusion. The left lung and pleura were healthy. There was no tubercle anywhere. (Signed) J. C. D."

Remarks.—On comparing the above appearances with the physical signs noted before death, it is evident that the non-vesicular respiration, with a high pitch, which was heard at the summit of the chest posteriorly, was due to the consolidated lung in this situation. The non-vesicular sound, low in pitch, heard in the mammary region, is attributable to the entrance of air into the pleural cavity through the perforation. The low pitched non-vesicular sound heard at the inferior posterior part of the chest was probably owing to the entrance of air into the gangrenous cavity.

FOURTH SERIES.—TUBERCULOSIS.

For convenience of reference, the cases of tuberculosis will be distributed into four subdivisions, according to the amount and stage of the disease, as follows:—

1. Small tuberculous deposit.
2. Abundant tuberculous deposit.
3. Tubercle advanced to excavation.
4. Tubercle arrested.

1. *Small Tuberculous Deposit.*

Obs. 1.—*January 4.* George Laver. Hospital patient. Slight dulness at summit of right chest. Respiration at summit of right chest relatively feeble, without any aberration of rhythm. The elevation of pitch, compared with the left side, is distinct.

Remarks.—The symptoms in the history of this case pointing to the existence of tubercle are as follows: cough for one and a half years, with slight expectoration. Slight hæmoptysis. Respirations increased in frequency ranging from twenty-four to thirty-six. The patient was not much emaciated nor greatly debilitated; had not been subject to night perspirations or pleuritic pains. Countenance not notably pallid; no febrile movement.

This was one of the earliest observations relative to the subject, when I was satisfied simply to ascertain the pitch of the respiratory sound over the site of tubercle as determined by percussion, without directing attention to comparative pitch of expiration, and other points. This remark will account for the limited scope of the observations in several other instances.

Obs. 2.—*Jan. 4.* Slayter. Hospital patient. At the summit of right chest marked relative dulness on percussion. The pitch of respiration is higher than on the left side. Aside from this, a disparity is not apparent, except in the relative feebleness of sound on the right side. No murmur of respiration is appreciable.

Jan. 27th. The following was noted as a distinct observation, without reference to the former: I find relative dulness at the summit of the right chest anteriorly. Over this situation the respiratory sound is relatively feeble. I cannot discover a difference of inspiration between the two sides, nor is there any sound of expiration appreciable on either side. Aside from relative feebleness, the elevation of pitch appears alone to distinguish the respiration on the right from that on the left side.

Feb. 9. The difference in vesicular quality between the two sides is not marked. The marked difference is in pitch.

Remarks.—The symptoms, in the history of the case, pointing to tubercle, are as follows: cough of four months' continuance, with small expectoration. Night perspirations. Is debilitated and pallid, but not greatly emaciated.

Pulse 88. Respiration 18. Has not had hæmoptysis, nor complained of pleuritic pains.

Obs. 3.—January 28th. Aaron Young. Hospital patient. Slight relative dulness on percussion in right infra-clavicular region. The respiration in the right infra-clavicular region is higher in pitch than in the corresponding region of the left side. The expiration is prolonged, with an interval between the inspiration and expiration. In the left infra-clavicular region the respiration is lower in pitch. There is a short sound of expiration, with a short interval between the two sounds. The inspiration is longer than on the right side. Slight bronchophony exists in the right infra-clavicular region.

March 2d. At summit of right chest, the expiration as long as the inspiration. Inspiration and expiration about the same in pitch. Near the sterno-clavicular junction, on both sides, loud bronchial respiration, but much more developed, and higher in pitch on the right side. Expiration more developed on this side, the pitch about the same as that of the inspiration.

March 3d. Expiration and inspiration at summit of right chest apparently about similar in pitch, the expiration notably prolonged. At the summit of the left chest, a sound of expiration is appreciable, notably lower in pitch than the inspiration. The contrast between the two sides consists in an elevation in the pitch of both sounds on the right side; a prolonged expiration, about equal in pitch to the inspiration on the right side, while the pitch of the expiration is lower than that of inspiration on the left side.

Remarks.—I give this among the cases of tuberculosis, but I do not feel positive with respect to the diagnosis.* On another examination made on the day I am writing, viz., March 8th, I find the same physical signs, with marked relative dulness over the right scapula below the spinous ridge. The symptoms in the history which relate to phthisis are cough, since January, with moderate expectoration. No hæmoptysis, nor pleuritic pains. He entered the hospital for cough and general debility, January 13th. Afterward he contracted typhus, from which he is now convalescent. His cough and expectoration are less at the present time than when he entered. The physical signs at the summit of the right chest may possibly be due to a normal disparity which, as has been seen, frequently exists to a greater or less extent—in this instance, perhaps unusually marked; or they may be due to uniform enlargement of the bronchi on the right side.

Obs. 4.—Jan. 30. Felata Kyser. Hospital patient. Slight relative dulness of resonance in left infra-clavicular region. The dulness is more marked over the left scapula, and in the latter situation there is distinct bronchophony. The pitch of respiration is in a marked degree higher on the left than on the right side, at the summit of the chest anteriorly and posteriorly. The expiration is not prolonged. The elevation of pitch is the chief source of disparity between the two sides.

* The correctness of the diagnosis was subsequently demonstrated.

Remarks.—The symptoms pointing to tuberculous disease are cough, with small expectoration; pleuritic pains; occasional night perspirations; respiration 28; the pulse is 70. She had not had hæmoptysis.

Obs. 5.—Feb. 1. Harmon Kramp. Hospital patient. Relative dulness of resonance, and distinct bronchophony, in the right infra-clavicular region. The pitch of respiration, in this situation, is notably higher than on the left side. The inspiration is somewhat shortened, the expiration prolonged, and higher in pitch than the inspiration, with an interval between the two sounds.

Obs. 6.—Feb. 2. Mrs. R. Out-patient. On auscultation, before percussing, the respiration is found to be notably higher in pitch in the right, than in the left infra-clavicular region. Aside from this disparity in pitch, the inspiration on the right side is somewhat shortened, with a short sound of expiration; on the left side, no sound of expiration is appreciable.

Slight relative dulness of resonance exists in the right infra-clavicular region, and distinct bronchophony.

Remarks.—The symptoms in the history of this case pointing to tuberculosis are, cough, troublesome at night, almost entirely without expectoration; pleuritic pains; considerable emaciation; pallid complexion; night perspirations; tuberculous fever. Respiration 28.

Obs. 7.—Feb. 12. Patient at almshouse; name not noted. Slight dulness on percussion at the summit of chest on left side. Below, resonance equal on both sides. The respiratory sounds, at the summit of the chest, on both sides, are about equal in intensity. There is an equally short, faint, scarcely appreciable expiration on both sides. The pitch, however, on the left side is notably higher. The vesicular quality is also somewhat diminished on the left side.

Remarks.—The symptoms in this case were not noted. It is simply stated that the history and symptoms point to tuberculosis.

Obs. 8.—Feb. 16. Mrs. R. Private patient. Marked relative dulness on percussion over the right scapula, with bronchophony. Slight relative dulness in right infra-clavicular region. The respiratory sound at the summit of the chest is notably higher in pitch than on the left side. There is a faint sound of expiration, which is prolonged, and appears to be higher in pitch than the inspiration. On the left side, the sound is lower in pitch, the vesicular quality is more marked, and no sound of expiration is appreciable.

Remarks.—In this case, I was led to the site of dulness on percussion, by the signs developed by auscultation, resorting to the latter method before the former. There are few symptoms denoting, directly, pulmonary disease in the case. The patient has been recently confined, having had three children in the space of five years; lactation protracted during the last pregnancy up to the period of quickening. Since her confinement, she has had chills,

with irregular paroxysms of febrile movement. She is considerably emaciated and prostrated, presenting a morbid aspect. Cough has been present, only occasionally, and is not a prominent symptom. The expectoration is insignificant; she has not had hæmoptysis, nor distinct pleuritic pains; the secretion of milk was tolerably abundant, but weaning was speedily resorted to, and she has since improved in strength and appearance, without any further development of pulmonary symptoms. These remarks were written three weeks after the above observation was noted.

Obs. 9.—Feb. 22. Murdoch Gillis. Hospital patient. On immediate auscultation, the pitch of respiration is higher on the right side at the summit. This is more marked on mediate auscultation. The pitch in the right infra-clavicular region is highest toward the acromial angle. There is here a prolonged expiration higher in pitch than the inspiration.

Remarks.—The signs on percussion are inadvertently not embraced in the observation. It is noted that Mr. Smith, one of the resident students at the hospital, had made and recorded the results of his examination prior to mine, which were afterward compared with the above and found to correspond.

The symptoms in the history pointing to tubercle, are as follows: cough of eleven months' standing; moderate expectoration; considerable emaciation, pallor and debility, and, of late, slight perspirations at night. Has not had hæmoptysis nor pleuritic pains. Pulse 108; respiration 30.

Obs. 10.—Feb. 24. Robert Evans. Out-patient. Slight dulness on percussion in right infra-clavicular region. The pitch of respiration, in this region is higher than on the left side. There is no appreciable sound of expiration in this situation.

Posteriorly, over the scapula above and below the spinous ridge, there is marked relative dulness on percussion. The respiratory sound in these regions is very feeble; but, in forced respiration, the pitch is determined to be notably higher than on the left side, and a sound of expiration is heard as high or higher than the sound of inspiration. Moderate, but distinct bronchophony exists at the summit of the right chest, anteriorly and posteriorly, and not on the left side.

Remarks.—The patient has had a cough for nine months, with small expectoration. Has lost considerable flesh and strength, and is incapacitated for labor, but is able to be up, and to walk out of doors. Respiration 24. Other details pertaining to the history were not recorded.

Obs. 11.—March 3. Louisa Stately, aged 8 years. Hospital patient. Distinct dulness on percussion, in left infra-clavicular, and over the scapular regions. The respiratory sound is more developed in the left than in the right infra-clavicular region. A difference in duration of the inspiration, or

in the vesicular quality is not appreciable. There is a feeble sound of expiration on both sides. The chief point of disparity pertains to the pitch, and this is marked. It is higher on the left side. The expiration on this side appears to be about equal in pitch to the inspiration, while it is lower than the inspiration on the right side. The pitch is also notably higher on the left than on the right side, over the scapular region.

Remarks.—This patient has had cough for several months, and, once, slight hæmoptysis. She is thin and pallid. Before my examination in this case, Mr. Bowen, medical student, had made an examination with reference to the pitch of sounds, and recorded the results, which were found afterward to correspond with those obtained by me, save so far as the latter relate to the expiration, of which he had made no note.

2. *Abundant Tuberculous Deposit.*

Obs. 1.—Jan. 4. Bieslay. Hospital patient. Marked relative dulness of resonance at the summit of the left chest, posteriorly, and of the right chest anteriorly. Respiration, on both sides, tubular, with prolonged expiration.

Distinct relative elevation of pitch at the summit of the left chest posteriorly, and of the right chest anteriorly.

Remarks.—In the above, as in other observations made shortly after my attention had begun to be directed to the study of the pitch of respiration, the fact of the disparity of pitch alone was noted, without reference to the expiration and other points.

Obs. 2.—Jan. 4. Julia Moniva. Hospital patient. At summit of right chest, anteriorly, marked relative dulness on percussion. On auscultation, sonorous râles occasionally, on both sides, at the summit of the chest. Respiratory sound more developed on the right side. No expiratory sound on either side. On the right side, distinct elevation of pitch. Distinct bronchophony on the right side.

On another examination after the foregoing was noted, the patient in the mean time having coughed and expectorated, I find the respiratory sound more developed at the summit of the left chest, and on comparing the pitch, the elevation is more strongly marked on the right side than before. The disparity in pitch is the most striking point of difference between the two sides in this case.

Obs. 3.—Jan. 5. Welch. Hospital patient. Marked relative dulness on percussion at the summit of the left chest. Respiratory sound feeble, and tubular on the left side, presenting a marked elevation of pitch compared with the respiration on the right side.

Obs. 4.—Feb. 19. Daniel Cuit. Hospital patient. On immediate auscultation, prior to examination by percussion or inspection, the respiration at the summit of the right chest is found to be notably higher in pitch in comparison with that at the summit of the left chest. The inspiration on the right side is shorter than on the left. On the right side there is a feeble,

prolonged sound of expiration, with an interval between the two sounds. The expiratory sound is too feeble to determine the pitch. On the left side, there is a feeble short sound of expiration, which is continuous with the sound of inspiration.

Afterward, it was found that the summit of the right chest is notably dull on percussion, and marked bronchophony exists on this side.

Obs. 5.—Feb. 20. Evelina Potter. Hospital patient. On examination by auscultation, before practicing percussion or inspection, with a view to determine on which side tuberculous deposit might be present, or more abundant, I found, at the summit of the right chest, the inspiratory sound higher in pitch than on the left side; the vesicular quality less; a sound of expiration higher in pitch than the sound of inspiration, prolonged to the same length as the inspiration, and an interval between the two sounds.

At the summit of the left chest, the sound of inspiration is feeble, but lower in pitch than on the right side. A prolonged expiration exists on this side, high in pitch, with an interval between the two sounds.

On percussion, there is marked relative dullness on the right side over the infra-clavicular and scapular regions.

An occasional sibilant r le is heard in the right infra-clavicular region.

Bronchophony exists on both sides, but is much more marked on the right side.

3. *Tubercle advanced to excavation.*

Obs. 1. With Autopsy.—Jan. 25. Hugh McMullin. Hospital patient. Marked relative dullness on percussion, and bronchophony at the summit of the right chest. Respiration over right chest, at summit, presents a notable elevation of pitch on comparison with the summit of the left chest. The expiration on the right side is prolonged, exceeding in duration the sound of inspiration. The inspiratory sound is shorter than on the left side, and there is a longer interval between the two sounds. A sound of expiration is heard on the left side, but less in degree and duration than on the right. There is also on this side, an interval between the two sounds, but less in duration than on the right side. On the left side, the sound of expiration is higher in pitch than that of inspiration.

Jan. 31. Over a space in the right infra-clavicular region, in this case, about midway between the acromial and sternal sides of this region, the respiratory sound is lower in pitch than in every direction surrounding this space. A feeble sound of expiration is heard over this space. No pectoriloquy. Above this space, directly upon the clavicle, the respiration is intensely tubular, with prolonged expiration, the pitch quite high, resembling tracheal respiration.

In the left infra-clavicular region, over the space corresponding to that just referred to, (*i. e.* just below the clavicle, about midway between the two extremities,) the respiratory sound has a high pitch, the expiration being prolonged.

The right side presents, on percussion, a higher pitch of resonance, but a hollow or tympanitic sound.

On immediate auscultation over the summit of the chest, the right side gives the lower pitch. It is to be observed, in this case, that the side on which the percussion pitch is highest, has the lower pitch of respiration.

Feb. 1. A *bruit de pot fêlé* is to-day distinctly apparent over the space in the right infra-clavicular region, characterized by the gravity of the pitch of respiration. The latter fact is also again verified.

Feb. 18. Confirmed again the statements made in the foregoing observation, and observed, in addition, that over the space in the right infra-clavicular region presenting a low pitched sound of respiration, the expiration is lower in pitch than the inspiration. On examination of the left infra-clavicular region yesterday, and to-day, I find a space about midway between the acromial and sternal extremities, and about half an inch below the clavicle, in which the sound is blowing, *i. e.* non-vesicular, and lower in pitch than in every direction surrounding it. There is a sound of expiration here which is lower in pitch than the sound of inspiration.

This case proving fatal on the 21st of Feb., the lungs were examined on the 23, by Dr. John C. Dalton, Jr., who has furnished the following statement of morbid appearances:—

“*Right lung.*—The upper, and one-half of the middle lobe are entirely without crepitation, being completely solidified except at situation of cavities, by deposits of yellowish, cheesy tubercle, and gray infiltration. The greater part of the upper lobe is occupied by a superficial irregular cavity nearly empty, twice or more as large as a hen’s egg, situated quite at the apex, and rather toward the outer part of the lobe. No other cavity of any considerable size in the right lung. The lower lobe crepitates pretty freely, but contains also many small yellowish tubercles. Many military tubercles sprinkled over the pleural surface of the lower lobe.

“*Left lung.*—Apex of upper lobe crepitates freely, and is nearly healthy, but its middle portion is extensively solidified by yellowish tubercle, and by well-defined spots of light-red hepatization. About the middle of the anterior portion there is a superficial cavity about the size of a walnut, containing a drachm or so of pure pus. Remainder of the left lung somewhat tuberculous, but less so than other parts.

Remarks.—It is evident, on comparing the foregoing morbid appearances with the preceding observations, that the low pitched respiration (consisting of a low inspiration and a lower expiration,) heard over a circumscribed space in either infra-clavicular region, was due to the cavities existing in the lungs on both sides, while the high pitched respiration, heard in the neighborhood of these spaces in every direction, was owing to the solidification of the lung surrounding the excavations. The latter, in other words, was the bronchial, and the former the cavernous respiration. It may seem, at the first glance, that the results of the first of the several examinations conflict somewhat with those of the subsequent examinations. The apparent inconsistency may be readily explained. Auscultation with the stethoscope in certain portions of the right infra-clavicular region, that is, over the portions of lung solidified, would, of course, give a high pitch of sound. That the low pitched respiration, denoting excavations, was not discovered at the first exploration, may have been because the whole of the infra-clavicular region was not

carefully examined, or the cavities may have been filled with fluid secretions, and therefore the conditions for the cavernous respiration were not present.

Obs. 2.—Jan. 26. James Bell. Hospital patient. On the left side, marked relative dulness on percussion at the summit.

Over the right side, at the summit, the respiration is evidently supplementary, the murmur being intense, but vesicular, without any sound of expiration. The resonance on percussion on this side is quite clear. The pitch of respiration, however, is rather high.

Over the left chest, at the summit, the respiratory sound is lower in pitch than over the right side. The pitch, however, is not uniform over the whole of the summit of the left side. Toward the acromial angle of the infra-clavicular region, over a space of about the dimensions of the thoracic extremity of the stethoscope, the pitch is low. At a short distance, in a direction toward the sternum, the pitch is notably higher. Over the latter space, compared with the former, the sound on percussion is in a marked degree dull. The resonance over the former space seems to me somewhat tympanic in character, and the tone is communicated through the stethoscope with a resonance approaching to pectoriloquy.

On applying the ear immediately over the summit of the left chest, the pitch is lower than on the right side.

The history of this case shows copious muco-purulent expectoration.

Obs. 3. With Autopsy.—Feb. 2. O'Donnell. Hospital patient. On the right side the resonance is relatively dull in a marked degree. On immediate auscultation, the pitch of respiration is found to be notably elevated, with a prolonged expiration, the latter being quite high in pitch. About an inch and a half, on the right side, below the clavicle, toward the acromial angle, the pitch is low, devoid of vesicular quality. At the same distance below the clavicle, toward the sternal extremity the pitch is high.

Feb. 13. On examination of this patient to-day, I observed at the summit of the right chest, during the first part of the act of respiration, a tubular respiration loud and high in pitch; and during the latter part of the inspiration, the inspiration abruptly became low, the blowing character being still preserved. The contrast between the two portions of the inspiration was striking. There is a feeble sound of expiration which is low in pitch.

This case terminated fatally a few days after, and the following statement of the morbid appearances of the lung is furnished by Dr. John C. Dalton, Jr.:—

“The right lung is closely confined by old and very firm adhesions throughout its upper portion. Whole of upper and most of middle lobe destitute of crepitation, and solidified by gray (tubercular) infiltration. Lower lobe full of air, but shows also many small, well-defined cheesy and softened yellowish tubercles. Upper lobe toward median line, solid, or with only one or two small cavities. Toward sternal portion there is a large superficial cavity, with irregular walls, capable of holding an egg or more, containing a few drachms of pure pus, and lined throughout by a grayish, semi-transparent false membrane, about a line and a half in thickness; easily scraped off by the edge of the scalpel. This cavity communicates with other smaller ones at posterior part of upper lobe, and also with a branch of the right bronchus about the size of a goose-quill.

“The left lung shows many small, yellowish, cheesy, and softened tubercles, and one cavity at its posterior part nearly or quite empty, and exhibiting the

appearance of having been for some time inactive. But very little gray infiltration in the left lung, and it is altogether much less diseased than the right. No effusion in either pleural cavity. (Signed) J. C. D."

Remarks.—The low pitched sound of respiration, with the expiratory sound lower than the inspiration, heard over a circumscribed space in the right infra-clavicular region, was doubtless due to the cavity discovered in the lung on this side at the autopsy. The high pitched sound in a direction from the cavity toward the sternum was produced by the solidified lung. The latter sound, in other words, was bronchial, and the former cavernous. The combination of the bronchial and cavernous respiration in the act of inspiration is an interesting point in this case, the former due to the current of air in the bronchial tube passing through solidified lung to communicate with the excavation, and the latter produced by the entrance of air within the cavity.

The examinations were limited to the anterior part of the chest, owing to the great feebleness of the patient. A careful exploration over the posterior summit of the chest, on the left side, might have led to the pitch variations incident to the cavity in the posterior part of the left lung.

Obs. 4.—Feb. 22. John Jervis. Hospital patient. On immediate auscultation, the pitch of respiration is found to be higher in the right infra-clavicular region. The inspiration is short and non-vesicular. On immediate auscultation, over a space near the sterno-clavicular junction, the pitch is low, and the sound non-vesicular. A sound of expiration is not sufficiently appreciable to determine the pitch. The sound over the space just referred to contrasts strongly with the high pitched bronchial respiration over the sterno-clavicular junction. The chest is depressed on the right side in the infra- and post-clavicular regions. It expands less in these situations, on inspiration, than on the left side.

On percussion, the right infra-clavicular region is found to be relatively dull. Over the space referred to, the sound is lower in pitch than over surrounding portions, and somewhat tympanitic. Over this space there is forcible vocal resonance, but not marked pectoriloquy.

Over the right mammary region there exists marked relative dulness on percussion, almost amounting to flatness. There is not much resonance, but comparatively a greater degree on the left side. The respiration on both sides over this region is feeble, but on the right side notably higher in pitch.

Remarks.—The disease in this case had existed for about six months. The respirations were 32, and the pulse 128. The comparative lowness of the pitch of respiration, with the absence of the vesicular quality, over a space in the right infra-clavicular region, is presumed to denote the existence of a cavity in that situation. In other words, the respiration in this situation is cavernous.

In this case, at my request, Mr. Bowen, medical student, examined and recorded the results, which were found to correspond with mine.

Obs. 5.—March 5. Carlos Aberd. Hospital patient. This patient entered in the last stage of the disease, and was so greatly prostrated that the physical examination was very cursory. Death occurred the second or third day after his admission, and an autopsy was not practicable. It is simply noted, that on the right side, at the summit of the chest, before and behind, the respiration is non-vesicular, low in pitch, with an expiration lower in pitch than the inspiration.

Obs. 6.—Feb. 26. James Kelly. Hospital patient. The examination for pitch is difficult on the left side, owing to the presence of râles. There exists marked relative dullness on percussion over the left side, at the summit of the chest anteriorly and posteriorly. On the right side, posteriorly, the respiration over the scapula is high in pitch, with a prolonged expiration higher in pitch than the inspiration.

On the left side (the side in which relative dullness exists) over the scapula the sound of respiration is lower in pitch, and devoid of the vesicular quality. An expiration is present lower in pitch than the inspiration.

In the right infra-clavicular region toward the sternal boundary, the pitch is high, and the sound loud, without an appreciable expiration. Toward the acromial border, in the same region, the pitch is low, with an expiration lower than the inspiration. The sound here, however, has the vesicular quality. Percussion over the half of this region in the direction of the sternum is dull, while over the other half it is clear.

In the left infra-clavicular region, râles obscure the pitch save over a space near the sternum. Here the pitch is low, with an expiration lower than the inspiration.

Obs. 7, with Autopsy.—Jan. 27. This case is included among the cases of tuberculosis advanced to the stage of excavation. It was not, however, really a case of that description. The existence of a cavity was predicted upon certain pitch characters, and other signs, but the autopsy proved the diagnosis to be incorrect. Candor requires the observation to be given in connection with the post-mortem appearances. The case is interesting and instructive, showing circumstances which were perhaps well calculated to lead to error of diagnosis. It also illustrates, incidentally, certain points connected with the present subject.

James Caytor, aged 5 years. Hospital patient. On percussion, the resonance in the left infra-clavicular region appears to be somewhat tympanitic, but the pitch of resonance is distinctly higher than on the right side. There is a distinct *bruit de pot fêlé* in the left infra-clavicular region. The pitch of respiration is distinctly lower in the left infra-clavicular region than in the right. There is a feeble expiratory sound on this side, not uniformly, but occasionally appreciable. The pitch is lower in the above situation than on the same side over the mammary region.

Over the right infra-clavicular region, the pitch of respiration is higher than over the left mammary region. Pectoriloquy is not present. In the left infra-clavicular region the second rib yields with very little resistance to pressure, in this respect contrasting strongly with the resistance over the corresponding region on the right side.

Jan. 29. Confirmed the foregoing facts. The *bruit de pot fêlé* is distinct.

The case terminated fatally early in the present month (March,) and the following statement of the morbid appearances of the lung is furnished by Dr. John C. Dalton, Jr.:

"The pleuræ of both lungs were rather plentifully sprinkled with tubercular granulations—no effusion or adhesion. There were also a few small tuberculous masses in substance of middle and lower lobe of right lung, yellowish and firm in consistence; no cavities and no softened tubercles. The largest collection of tubercle was just behind the root of the left bronchus, firm and cheesy—about the size of a small English walnut. Bronchial glands slightly tuberculous. Bronchi of natural relative size and length. There was considerable redness of mucous membrane of the left bronchus, with three or four shallow white ulcerations. No tuberculous deposit perceptible at base or in neighborhood of these ulcerations. No similar affection of right bronchus. Apices of lungs nearly or quite free from tubercle. No pneumonia anywhere. (Signed) J. C. D."

Remarks.—A cavity was sought for in the left infra-clavicular region, supposed to be due to excavated tubercle or pouch dilation of the bronchus in this situation. This expectation was based on the physical signs above mentioned, viz., the low pitch of respiration, the tympanitic resonance on percussion, and the *bruit de pot fêlé*. The diagnosis was formed with considerable hesitation, because the rational system did not denote extensive tuberculosis advanced to the stage of excavation. The respirations were not accelerated, nor labored. There did not appear to be much expectoration. The age of the patient militated against the supposition. The patient was reduced to marasmus, and died by asthenia, evidently from defective assimilation. In spite of these circumstances, the pitch characters, and the cracked-pot, tympanitic resonance, were thought to denote the existence of a cavity.

The source of the error is intelligible. The thoracic walls were very thin, and the respiratory sound was largely developed. The respiration on the right side, at the summit, was higher in pitch from the predominance of the bronchial element as a natural disparity. I was not, at that time, so well prepared to attribute a disparity so strongly marked to a normal difference, as since the examination of a number of healthy chests with reference to this among other points. The relative lowness of the pitch on the left side did not arise from a depression on this side, but because the normal elevation of pitch on the right side was in this instance strikingly marked. Instead of a cavity, there existed in the right lung a solid tuberculous deposit of about the size of an English walnut. This was situated directly behind the left bronchus. In this situation, it would not evidently be expected to raise the pitch of the bronchial respiration in front on this side, and by making some pressure on the tube it would be likely to render the sound less developed.

How is the *bruit de pot fêlé* to be accounted for? It is well known that this modification of tympanitic resonance does not universally require a cavity

[The following page of the appendix to the prize essay, was inadvertently omitted in the republication of this present number, and the omission discovered too late to remedy it. We therefore insert it in this manner:]

It is possible that the comparison, in these situations, was not made with sufficient care.

This case furnished an interesting illustration of the effect of a moderate tuberculous mass, surrounded by vesicles, in modifying the pitch of resonance on percussion. The pitch of sound on percussion, in the left infra-clavicular region, was distinctly raised.

4. *Arrested Tubercle.*

Obs. 1.—Jan. 27. Martin Welch. Hospital patient. This patient presented, a year ago, the evidence, physical and rational, of small tuberculous deposit. The disease, in the mean time, has apparently made no progress. He remains in the hospital as a porter. Some cough and moderate expectoration continue. He has not lost in weight during the year, and his aspect is not notably morbid.

On auscultation, the respiration at the summit of the right side is relatively feeble, and the pitch is elevated. There is no sound of expiration, nor does the respiration present any abnormal characters, save feebleness and elevation of pitch.

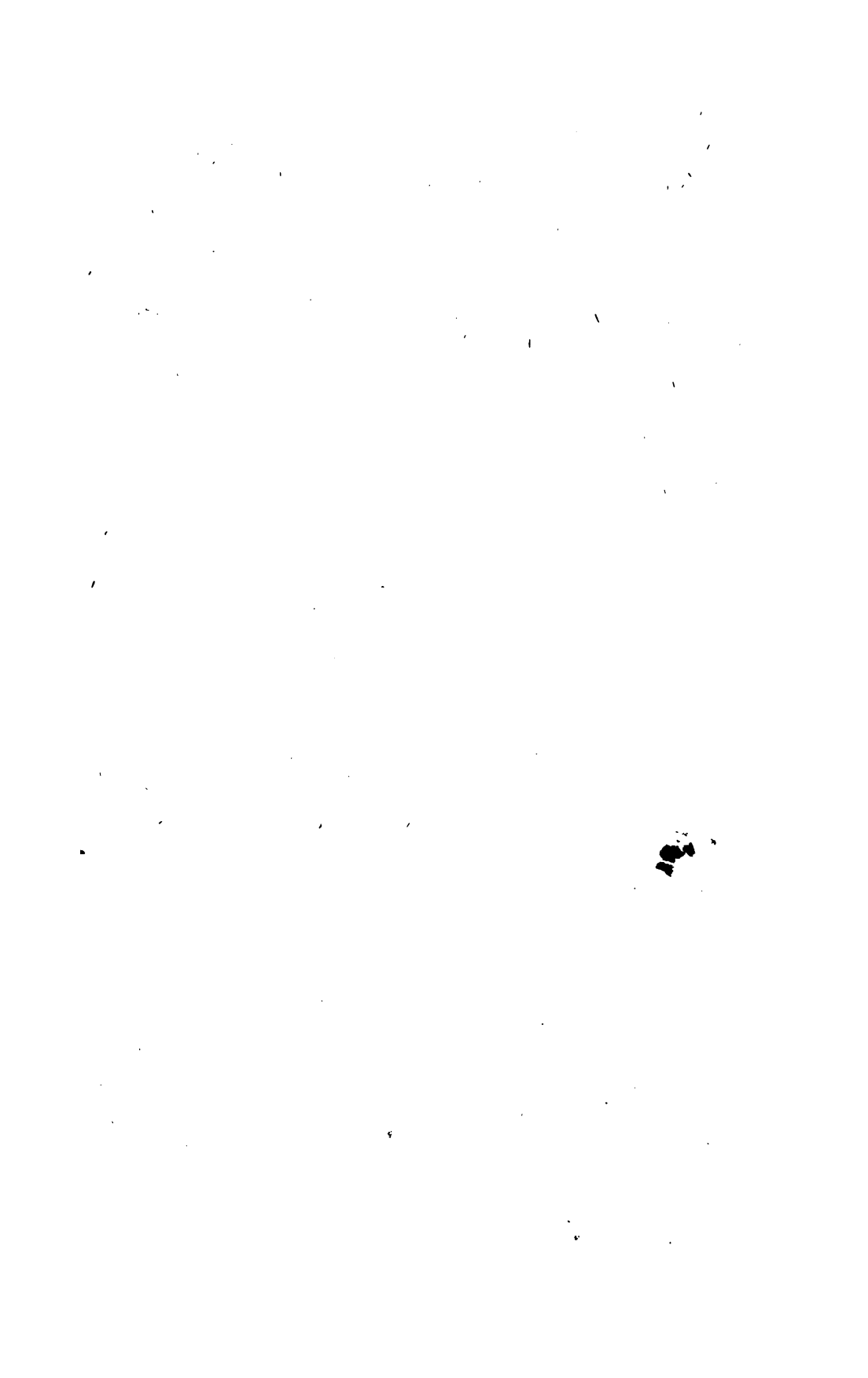
On percussion after obtaining the foregoing results, I find slight relative dullness in the right infra-clavicular region.

Obs. 2.—March 4.—The subject of this observation is a professional friend of the writer. In 1844, he suffered for several consecutive months from cough with moderate expectoration. Hæmoptysis occurred, during this period, once. His weight was considerably diminished. He gradually recovered without medical treatment, and with the exception of a cough, which lasted for two months in 1849, he has enjoyed good health, being entirely free from any symptoms of pulmonary disease.

There is distinct elevation of the pitch of resonance on percussion at the summit of the chest on the left side. The respiration, in this situation, is slightly elevated in pitch, and the vesicular quality is somewhat less developed than on the right side.

Slight vocal resonance exists on both sides, in about an equal degree.

Remarks.—The variations in resonance and respiration being on the left side, are, on this account, the more significant of changes, probably due to arrested tubercle.



for its production. It has been observed in the second stage of pneumonitis. Without discussing the mode of its production as a rule in cases in which cavity does not exist, I will offer an explanation of its occurrence in this particular case. It will be seen, by reference to the observation, that the second rib on the left side was remarkably yielding to pressure. The solid tuberculous mass behind the bronchus furnished a point of resistance, or as it were a fulcrum. On percussion, the ribs and the lung anteriorly to the bronchus yield readily, and the tuberculous deposit formed a kind of anvil causing the air in the bronchi to be expelled in such a way, and with sufficient force to yield the cracked-pot sound. Of the existence of this sound in the case there was no doubt. While the patient was in the hospital, it was frequently demonstrated, by myself and others, as a good illustration of the sign.*

I can only account for the fact that the pitch at the summit of the left chest appeared lower than over the mammary region by supposing that the presence of the small tuberculous masses in the lower lobe on this side, exalted the pitch more than the deposits in the upper lobe. Why this should be so, it is not easy to understand.

ART. II.—*Obstetrical Auscultation. Signs of Pregnancy.* By M. M. RODGERS, M. D., Rochester, N. Y.

It is not designed, in this brief article, to describe in detail, all the signs of pregnancy, or to consider them in the order of their relative value. We shall only notice briefly those signs which usually accompany utero-gestation, and which in the aggregate furnish strong presumptive evidence of this condition. As these signs, however, are all more or less equivocal, whether taken individually or collectively, we propose to consider the value of those results furnished by auscultation. If, by this mode, we are able to arrive at a sign, which, taken alone, and independently of all others, will at all times give unequivocal *positive* evidence,—its importance in a medico, legal, moral, and scientific point of view will be admitted by all. The signs upon which we have formerly been accustomed to rely for a diagnosis in suspected pregnancy, may be noticed, for the purpose of showing, not what they indicate, but what they do *not indicate*, for their evidence is entirely negative.

* Since this was written, a child admitted into the hospital, with affection of the mesenteric glands, reduced to extreme emaciation, but without cough or other symptoms of pulmonary affection, presents a very distinct *bruit de pot fêlé* on the left side.

1. *The general condition* of a woman *enceinte*, may lead to this suspicion, especially if she be primiparous. 2. *The cessation of the menses*, but there are so many exceptions to this, that it cannot be relied upon: menstruation may continue in cases of pregnancy, until nearly the close of the term, and may cease in women not pregnant. 3. *The morning sickness*, which occurs usually, between the sixth and twelfth week, is often absent. 4. *Salivation* occurs in some cases, but may arise from other causes, and is not often present. 5. *Enlargement of the mammae*, is a pretty constant sign, but is occasionally absent, and may occur under other circumstances also. 6. *The areola and enlargement of the follicles*, are also nearly constant, but occur under other circumstances. 7. *Secretion of milk* usually takes place during the latter half of the term, but not always: it may occur also, in women not pregnant, and even in girls and men. 8. *Increased size of the abdomen*, when taken in connection with other signs, is of value, but it may be a consequence of disease also. 9. *Condition of the umbilicus*, is a sign of little value. 10. *Dullness on percussion*, over the abdomen must occur in pregnancy, but may be found in other conditions. 11. *Quickening*, or the motions of the fetus, usually occurs at the end of the fourth month, and is a very constant sign, but it may sometimes be produced by the voluntary or involuntary action of the abdominal muscles, and is sometimes never felt, in cases of real pregnancy. 12. *Ballotement*, next to auscultation, furnishes the most unequivocal evidence, and is considered by some authors, infallible. But, on the authority of Prof. Depaul, of Paris, it has led to the error of pronouncing a hydatid tumor a case of pregnancy. In cases of twins, and where there is a small quantity of amniotic fluid, it is sometimes impossible to obtain this result. 13. *Violet color of the vagina*, is very generally present in pregnancy; but the writer has seen this test made extensively in Paris, when it occasionally failed both ways. 14. *Changes in the uterus*, may occur similar to those of pregnancy, from disease. 15. *Buffy coat on the blood*, is considered by some authors a sign worthy of confidence, but this occurs in so many diseases of both sexes that it must be of little value as a test. 16. *The urine* is said to contain an unusual quantity of uric acid in pregnancy, but this occurs also in diseases of both sexes. 17. *Kiesteine* is usually present in the urine of pregnant women, but is also found in the urine of men and children, as a result of peculiar diet and disease. 18. *Palpation* of the abdomen sometimes affords very strong evidence of the presence of a fetus, but is seldom reliable alone. 19. Besides these, there are several minor signs which are of some value, considered with the others: such are, variations of the pulse, the appetite, maculae on the face, vaginal secretions, venereal

desires, organic sympathies, mental conditions, temper, age, presence of the hymen, certain diseases, &c.

Now we see that these signs, taken together or singly, yield equivocal evidence, — evidence which at best is only negative. We want a sign which will, in all cases where it is present, give positive proof; and this sign is furnished by auscultation.

The *bruit placentaire*, is an intermittent, whizzing sound, resembling the *bruit de soufflet* of the heart, and synchronous with the maternal pulse. It may usually be heard from the end of the second month of utero-gestation, until the last pains of labor. This sound is sometimes simulated by the ovarian vessels, the uterine sinuses, abdominal vessels, the vessels of fibrous tumors and aneurismal varix; but it need not be confounded with any but the latter sound. This sound is now supposed to be in the uterus, and not, as formerly, in the placenta; it does not, when present, indicate pregnancy positively,—nor when a fetus is present, does it indicate its life, as it may continue for sometime after death takes place. The *funis soufflet*, may sometimes be heard intermitting synchronously with the foetal pulsation,—but it cannot exist independently of the action of the foetal heart, and when this can be detected it is of no use in diagnosis. The *foetal tic tac*, or pulsation of the foetal heart, consists of short, double, regular pulsation, resembling those of the new born infants, varying in velocity from 120 to 140 in a minute. This sound cannot be simulated by, nor confounded with any other; so, when it can be distinctly heard, it is proof *positive*, and the only one, of the presence of a living fetus; where there is the *tic tac*, there must be a heart to produce it, and where there is a heart, there must also be a fetus. Its absence proves only negatively, that there is no fetus, or if any, that it may be dead.

There is no known sign by which we can determine that a woman is not pregnant. This fact makes this sign the more valuable, as in nearly all cases of actual pregnancy with a living fetus, we may at once verify it. The foetal *tic tac*, according to different authors, may be heard from three and a half to five months after conception. The location of the sound and the manner of obtaining it, we need not indicate.

This sign alone furnishes the means of diagnosing twin pregnancy, determining any thing in relation to the foetal health, or the presentation to be expected when labor commences. If, then, this is a true test, as we assume, how do those physicians appreciate its value, who never auscult the abdomen at all? What confidence ought to be placed in the testimony of a medical witness in court, who should base his opinion of pregnancy entirely upon

those signs, every one of which he knows to be equivocal? No matter what may be said of ballottement, aggregate signs, age, experience, learning, they all vanish like vapor before the sunbeam, when compared with this. We may be pardoned then, if we exhort those who wish to be considered "read up," and who still entertain "peculiar views," to *study obstetrical auscultation by the bedside, study it, where alone it can be learnt, on the abdomen of woman.*

ART. III. — *Report of a Committee appointed by the "Buffalo City Medical Association," August, 1852, to "investigate the influence of upturning the soil in the causation of Cholera." Read at a Special Meeting, held Sept. 8, 1852, and ordered to be published.*

GENTLEMEN OF THE ASSOCIATION :

The committee appointed by you at your last regular meeting "to investigate the influence of upturning of soils in the causation of the Asiatic cholera," would respectfully report.

First. That upturning of the soil is a sufficient and frequent cause of intermittents, and of other forms of malarious disease.

The correctness of this first proposition we presume no one will dispute; yet as it constitutes in some measure the basis of our argument, we think it proper to refer you to a few of the very common and notorious facts by which it is sustained.

The pioneer who builds his cabin in the wilderness seldom suffers in health until with his axe he has cleared away the forests from around his dwelling, and exposed the surface of the earth, covered with decaying vegetable remains, to the action of the air and of the sun.

In this instance the soil is not disturbed; but removing the forests which have for centuries buried the surface of the earth in its deep shadows, is the same in effect as bringing to the light a similar soil, long hidden, with the spade or the plough.

It is just as well known, however, that the emigrant who settles upon the prairie, lives in comparative health until the soil is broken; and that the surest protection against autumnal fevers is to place the garden and the ploughed fields remote from the dwelling. An extensive breaking of the fallow ground is almost invariably succeeded by a sickly season.

Among a multitude of local incidents which might be adduced to further corroborate our proposition, we will ask your attention to only that one which

constitutes in part the subject of a memorial from the citizens of Rome, Oneida Co., to the Senate and Assembly of the State of New York: and especially to the letter accompanying the memorial, from Dr. H. H. Pope to Benjamin Enos, Canal Commissioner.

ROME, 30th December, 1843.

HON. BENJAMIN ENOS, Canal Commissioner, &c.

Dear Sir — By request of our citizens, I herewith transmit a list of such cases of sickness and deaths as occurred under my observation in 1842, the apparent result of the suspension of the then unfinished public works through our village: and I would first respectfully state, that the bed of the present enlarged canal, in most of its course, occupies the ground along side of the old Lock Navigation Co. Canal, and where most of the wood, timber, roots, and brush, graded from said canal was deposited. By excavating and exposing to the rays of the sun, this vast amount of decomposed and decomposing vegetable material, which had been covered in from five to eight feet of the swamp muck excavated from said canal some forty-five years since, a most prolific source of disease was produced. To the above, and the fact that in the partial excavation of the new canal previous to the suspension, the old canal was only partially filled up, leaving many places for pools of stagnant water, I attribute most of the sickness of that year: and by excavating and spreading over the vegetable matter above mentioned, the three or four feet of clean gravel and sand which forms the bottom of the bed of the enlarged canal, aided by the frosts of last winter, and the drain resulting from perfecting the bottoming out of the canal, I attribute our almost entire exemption from bilious disease the past summer. Should it be desirable, I might furnish a diagram of the lots along the canal, and the names of their occupants, and thus conclusively show that from ten to fifteen cases of disease occurred in the immediate vicinity of the canal, where one occurred in the upper part of the village.

I am, very respectfully, yours,

DR. H. H. POPE.

LIST OF CASES OF SICKNESS AND DEATHS.

Mrs. H. H. Pope and son, sister, and mother.
 Mr. John Eddy's wife and two children.
 Mr. Edward Eddy, wife, and family.
 Mr. S. Hungerford and two children. One death.
 Mr. Oliver C. Grosvenor. One death.
 Mrs. R. Woldby, son, sister, and apprentice.
 Mr. John Stevenson and family.
 Mr. Luther Moltesar and family.
 Mr. S. Martindale and family.
 Mr. A. Seymour, wife and daughter.
 Mr. W. T. Hungerford, wife, and son.
 Mr. M. Rowley and family. One death.

Wm. Young—Joseph Shield's son, wife, and father. One death.
 Mrs. Thomas Dugan and brother. One death.
 Mr. Sandford Adams and wife. One death.
 Mrs. Elonzo D. Lewis and family.
 Mr. Steel, and wife and daughter. One death.
 Mr. Snow—Mr. John E. Henderson and family. One death.
 Mrs. McConich and child.
 Mrs. Giles Haaby.

To this list I might add very many cases mostly of a mild form, which submitted readily to medical treatment; and I should also add, almost every Irish family whose shanty was situated in the immediate vicinity of the public work.—(Sen. Doc., No. 90, p. 35, April 15, 1845.)

The Hon. Henry A. Foster, senator, by whom the memorial was presented, and who was at that time a resident of Rome, informs your committee that all of this sickness occurred within twenty rods of the canal.

Confident, however, that you do not deny the competency of these causes in the production of fevers, we shall proceed to affirm

Second. In all these cases, where newly opened soil occasions fevers, it is the old and decaying vegetable matter thus brought to the surface, which chiefly or alone produces the diseases which result. (See Appendix, B.)

We believe that you will not demand of us an argument in defense of this position.

It is not quite certain, however, that aluminous soils freshly exposed, may not produce malaria; and if so we shall find, perhaps, an explanation in the fact that such soils generally contain more or less organic vegetable matter in an intimate state of mixture, especially where they lie underneath alluvium.

To be more definite, then, we affirm that the malaria, or disease-producing emanations from a soil freshly exposed, and the malaria from low and marshy soils are identical—the results alike of vegetable decomposition.

We shall have immediate occasion to apply this conclusion in considering the influence of upturning of the soil in the causation of Asiatic cholera.

Third. Marsh malaria produce not only intermittent fevers, and various zymotic diseases, such as yellow fever, typhus, &c., but also Asiatic cholera.

If this proposition shall be sustained, and our reasoning has been hitherto correct, then, by a plain rule of logic, is it at once established that upturning of the soil may produce cholera—the object of our commission is already reached and the question proposed by the "Association" determined.

Whether marsh malaria may develop cholera is a point in our argument upon which we might easily occupy much time, since the facts upon which its elucidation depends, cover the progress and history of this epidemic from the day of its first irruption from Jessore, in India, to the present time.

Here again, however, your committee finding the question already and now for a long time decided in the affirmative, do not feel at liberty to detain you by a lengthy parade of testimony. But as the latest and by far the most complete summary of the causes of the cholera is contained in the British Registrar-General's Report for 1848-9, we shall copy from a critical notice which we find in the July No. for 1852, of the British and Foreign Medico-Chirurgical Review, such facts and remarks as seem pertinent.

"Of all the causes influencing the spread and the mortality of cholera, none has so great an effect as elevation. This fact known for a long time," says the reviewer, "has been worked out by Mr. Farr so completely, that it may be received like the solution of a problem."

"The mortality from cholera is in the inverse ratio of the elevation. The mortality of the 19 highest districts was at the rate of 33 in 10,000; and of the 19 lowest 100 in 10,000." (Report.)

"On further examination," the reviewer concludes that in spite of other disturbing causes "the mortality from cholera bore a constant relation to the elevation." Density of population, over-crowding, poverty, general insalubrity, are all taken into the account as disturbing influences, yet by the side of elevation they have comparatively little effect, and not only in London but in the rural districts equally were the same conclusions obtained — still "in every place, elevation exerted a paramount effect."

Finally, Mr. Farr proceeds to explain how this happens:

"As we ascend, the pressure of the atmosphere diminishes; the temperature decreases, the fall of water increases, the vegetation varies, and successive families of plants and animals appear in different zones of elevation. The waters roll along the surface of the rocks, or filter through them and the porous strata of the earth, to burst out below—the sources of rivers or of tributaries, which carry disintegrated rocks with the remains and excretions of vegetables, animals, or men, in every stage of decomposition. The deposits in stagnant places, and at the estuaries, show the kind and quantity of mixed matter which the laden rivers carry down and deposit on the low margins of the sea at the tidal confluences of the fresh and salt waters. * *

"As the rivers descend, the fall of their beds often grows less, and the water creeps sluggishly along or oozes and meanders through the alluvial soil. The drainage of the towns is difficult on the low ground, and the impurities lie on the surface or filter into the earth. The wells and all the water are infected. Where the houses are built on hill-sides and elevations, as in London, the sewage of each successive terrace flows through the terrace below it, and the stream widens, the ground becomes more charged, every successive step of the descent, until it is completely saturated in the parts lying below the high-water mark.

"The river, the canals, the docks, and the soil of a port may be viewed as a large basin full of an almost infinite variety of organic matters, undergoing

infusion and distillation at varying temperatures; and as the aqueous vapor which is given off ascends, it will be impregnated with a quantity of the products of the chemical action going on below, variable in amount, but necessarily greatest in the lowest and foulest parts. * * * The amount of organic matter, then, in the atmosphere we breathe, and in the water, will differ at different elevations, and the law which regulates its distribution will bear some resemblance to the law regulating the mortality from cholera at the various elevations. It has been seen how rapidly in London the mortality from cholera diminishes a few feet above the low ground on a level with the Thames, while several feet of elevation in higher regions produces no sensible effect. * * *

"It is established by observation, that cholera is most fatal in the low towns and in the low parts of London, where, from various causes, the greatest quantity of organic matter is in a state of chemical action; and it may be admitted that cholera, varying in intensity with the quantity, is the result of some *change* in the *chemical action* of this matter. Further inquiry must determine whether in England that change is spontaneous, or the result of the introduction of a zymotic matter from beyond the seas; whether the poison enters the human frame in air or water, through the skin, the mucous membrane, or the air-cells of the lungs." (pp. 69-70.)—(p. 44.)

"This law of elevation," the reviewer remarks, "is perhaps the most important practical point brought out in the Report, and is well worthy the attention of the authorities of the East India Company; for the fact, though long recognized, has never been so definitely shown before." And he concludes by saying,

"The readers of our journal need scarcely be reminded how frequently we have advocated views identical with these, and how we have over and over again pointed out, that all observers who have regarded cholera with an unprejudiced eye, from the days of Jameson downward, have adopted opinions of a similar kind. Let us hope that this reiterated assertion—an assertion based on observations so numerous and so accurate, may at last have some weight with the rulers of this and other countries; and that we may at length commence in good earnest those works of sanitary improvement, the neglect of which is the opprobrium of the present generation, and the fatal legacy which it seems is to be inherited by the next." (p. 44.)

Your committee believe, and we think you will fully concur with us, that while the statistics and tables of Mr. Farr establish conclusively the *general* relation of elevation to the cholera, yet as the summaries only show the results of aggregates, it is very probable that among the materials from which the whole is gathered, might be found many exceptions. Our own experience would show that such exceptions do occasionally occur, in which elevation affords little or no immunity. The site of Bellary, in India, upon which an English fort is built, "is a granite rock, half a mile in diameter, and 500 feet in height. In its neighborhood are no marshes, no rivers, no dense and

exuberant vegetation, which may afford to cholera a congenial soil; and yet, for thirty years, the pestilence has never been absent a single season from Bellary." (Trans. Amer. Med. Assoc. 1849. Report of committee on Med. Sci., p. 72.) But we shall find a sufficient explanation in other circumstances which are present. The barracks are over-crowded and uncleanly; close by are "two dirty bazaars, which have long been considered a public nuisance. *A large tank, which becomes dry during the hot season, taints the air with its effluvia.*" "When we take into account the climate of the country, it is not surprising that these causes are productive of cholera from one year to another. The heat during the months of March, April, May, and part of June, is described as being insufferable. The unclouded sun glares from a sky of brass upon the parched earth, and its fierce rays acquire additional force by being reflected from the granite rock; the thermometer rises, at midday, to 90°, and even 98° in the shade, and to 130° in the sun; and the heat is rendered more oppressive by the sultry stillness of the atmosphere. The winds which occasionally interrupt this calm, burn as if they had passed over a furnace, and are more intolerable than the still atmosphere itself." (Ibid p. 73.)

We would infer, therefore, that the condition of elevation upon which the immunity from cholera depends, is certainly not the diminished pressure of the atmosphere, to which Mr. Farr has, among other conditions alluded, since this condition is uniform. One other condition, however, we find here, which is unusual to such localities, and which is so constant in lower situations, viz., stagnant and putrid water. To this, therefore, in the present instance, most intense cause, with the concurrence of several other causes, such as idiomiasm, filth and excessive heat, your committee choose chiefly to refer the existence of the cholera at Bellary. It is the drainage, ventilation, and complete absence from malaria, which give to elevated situations their remarkable immunity; and where these accompanying conditions are absent, we venture to say, the same immunity will not be found.

For the same reason, also, the observation of Dr. Jackson, will be found generally true, viz., that the epidemic has never been destructive in granite regions; while others have remarked that in the spread of the cholera through the western states, "it has seemed to assume its highest malignancy in regions of country where the older limestone rock is the geological formation."

Granite regions are not generally miasmatic regions; but where lime-rock prevails the soil is mostly alluvial and is underlaid with clay, presenting thus the very conditions most essential to exuberant vegetation and to the detention of the water upon the surface. Lime-rock regions are therefore most

often malarious, and, as others have observed, and as we have observed also, most often the chosen regions for cholera. Bellary is built upon a rock of granite, yet granite separated from those other conditions which elsewhere so almost universally accompany this formation, affords her no protection. Nor is it *necessary* that towns built in valleys, at the estuaries of rivers, or upon the margins of basins of water, or, indeed, wherever the geological formations are alluvium, clay, and lime-rock, should suffer perpetually the visitations of this pestilence; since money and labor, judiciously expended, may always conquer these natural disadvantages of situation.

But we need not look abroad for attestations of the fact that cholera seeks miasmatic regions. Its terrific assaults upon our neighboring towns, Sandusky and Toledo, in 1849, are well enough remembered. Sandusky, especially, was more than twice decimated. Nor have these places escaped during the present season. Wherever, also, in this state or in the neighboring states, in the city or the country, like causes exist and in equal intensity, there you shall learn that the cholera, true to its instincts, has not failed to pay its unwelcome visits; and if any places escape now that did not escape in the previous epidemics, it has been because some essential improvement has been made in their sanitary system, or in some way ancient malaria have been made to cease. Of this fact we might easily furnish you with several marked illustrations.

In our own city, each successive return of the epidemic has found it earliest and in greatest severity on the "flats;" in the vicinity of the canals, and stagnant pools which every where dot that portion of the town; at the "Hydraulics," and over that broad, level, yet quite *elevated* section which reaches eastward from Main street — throughout all of which localities there has always been, and there remains to the present day, when the cholera is not present as its substitute, intermittent fevers.

In other parts of the city, also, might be indicated many similar localities, which furnish similar evidence of their insalubrity; and investigations instituted by the vigilant officers who composed the "Board of Health" in 1849, seldom failed to discover, wherever the disease for a time fixed itself, an undrained cellar, an unfilled lot, an obstructed street gutter, or some like circumstance, as the source of the sickness.

It is not alone those, however, who live above or adjacent to these causes who are made to suffer; but if the winds are favorable, those living remote, and in the more salubrious portions of the town may fall under their poisonous influence.

So now there are operating about us, and surrounding us in such a manner

that we cannot for our lives escape them, a thousand efficient causes, in all those standing pools covered with green fungi — of whose pestiferous agency few have thought, and for the removal of which none have cared.

We will not omit to mention, also, as having a clear relation to the point under consideration, that the cholera occurs ofteneft in those houses which are destitute of cellars, and in which the floors are laid close upon the ground, or in dwellings the cellars of which are damp, not ventilated, or contain vegetables in a state of decomposition. (Appendix, C.)

Equally pertinent, also, is the well known fact that cholera, like malarial fevers, makes its attacks most frequently at night, or early in the morning, when the atmospheric vapors, holding miasmatic poisons in suspense are nearest the surface of the earth.

Those employed, therefore, in the excavation of alluvial earth during the day, have relatively little or nothing to fear — the vapors heated and rarified by the sun, arise rapidly and pass off. Hence the argument which has been occasionally urged before us, viz., that the workmen employed in these excavations do not fall sick, has no weight. The same is known to be true in relation to marsh fevers. It is the inhalation of the marsh atmosphere at night which proves so especially fatal.

There is more seeming value, however, in the statement that men have been known to work all night over these ditches and in the midst of beds of vegetable mould, who have yet continued in excellent health. Mr. Brick, the intelligent superintendent of the Buffalo Gas Works, assures us that he has thus employed a number of laborers during the present summer, where mould was three feet deep, yet they have escaped the cholera.

But may not these facts only corroborate what has been a generally received opinion, also, in relation to the influence of marsh miasmas in the production of disease, viz., that the system is more susceptible to their influence during sleep, and that they are comparatively inoperative during wakefulness. The traveler on the Pontine marshes, in Italy, is cautioned constantly against yielding to the seductions of sleep, lest he should thereby fall a victim to the malaria, and medical men, with others, have long sanctioned the belief.

But if no such explanations could be offered of these apparent exceptions, their number is too small altogether to weigh a feather against the cumulative and overwhelming evidence opposed.

In short, we are now prepared to reaffirm that marsh malaria do prove an exciting cause of Asiatic cholera; and indeed, your committee are disposed to regard such malaria, if not the most energetic, at least as of all others the most widely operating cause.

We thus arrive again at the conclusion which we had already anticipated, and which constitutes our *fourth* proposition, viz: *That upturning of the soil may prove an exciting cause of Asiatic Cholera.* This conclusion, it must be seen, is the inevitable corollary from our argument.

We suppose that in the strict interpretation of the resolution under which we act, we might properly have here terminated our inquiries. But we believe that the spirit of your instructions extended farther — and that you proposed also, that we should inquire whether in *our city* there had been any cases in which the upturning of the soil had produced cholera; or whether, indeed, the soil was generally of that character from which in future danger might be apprehended, if the practice of opening and exposing the earth in the season of the cholera should be pursued.

To this subject we have therefore diligently addressed ourselves: yet we are forced to confess that here our labors have been less fruitful of results than we could have desired.

First. In relation to the cholera as it appeared during the last week in July, in Ellieott street, and its probable connection with the opening of a ditch in that street at about the same time, we find no farther room for investigation. The facts are before you in a report presented, at the regular meeting in August, by the chairman of this committee.

Second. In consequence of a reference made incidentally in the report just mentioned, to a similar occurrence in Genesee street, in 1849, we sought at once all the sources of information which at this late day were accessible, to determine the facts.

From Mr. Harraden, the contractor, we learn that a ditch was commenced in Genesee street, at the intersection of Michigan, about July 1, 1849, and that it was opened and completed through to Hickory by about the 20th of August; the work of opening, laying the sewer, and refilling, being carried on simultaneously — so that it was closed in its entire length very soon after the excavation was completed. The excess of earth was, however, not removed until about the 14th of September. The ditch was 1200 feet long, two feet wide and from eight to ten feet deep.

The soil through which the ditch was excavated was first one foot of paving sand, then clay to the depth of two or three feet, and finally hardened quicksand, or clay and sand in mixture.

From the files of the cholera reports kept in the office of the city clerk we ascertain that forty fatal cases of cholera were reported from Genesee street during the summer of '49; but in only three or four cases is the number of the house given, so that we were unable to determine from this source,

the fatality of the disease along the line of the ditch. We will remark, however, that all the reports of fatal cases for this street were between the 19th of July and the 4th of September; a period wholly within that occupied in the opening and closing of the ditch.

We have also addressed circular letters to all those physicians, chiefly Germans, who we supposed had been the principal medical men employed in that district. But from none of them have we obtained any information which is of value, or pertinent to the question. Generally, they assure us they kept no records of these cases.

As the only remaining alternative, we secured the services of a very intelligent young gentleman, Mr. Augustus Jeyte, son of Dr. Jeyte, a German, who, at our request, promptly undertook to ascertain the facts by direct, personal inquiry. Accordingly Mr. Jeyte visited every house on Genesee street, between Michigan and Mortimer, and in the tabular form which we had arranged for him, reported to us the following results.

We give the tables as reported, that an opportunity may be afforded for correction, if there is any error. It will be seen that there are in all seventy-nine families referred to, of which twenty-four have removed, nearly one-third; among which number, doubtless, some sickness and deaths occurred. This must render the tables somewhat inaccurate, yet not sufficiently to affect materially their value.

In company with one of the aldermen of our city, the chairman of your committee has visited many of the dwellings, and we have confirmed, as far as we examined, the faithfulness of Mr. Jeyte's returns, with only two exceptions, which we have corrected. We also ascertained that from Michigan to Elm, the cholera occurred in but one family. In this family three died.

FROM MICHIGAN TO HICKORY STREET—28 FAMILIES.

No.	NAME OF OCCUPANT.	Before July 1.		Between July 1 and Sept. 14.		After Sept. 14.	
		Sick.	Died.	Sick.	Died.	Sick.	Died.
157	Changed occupant,						
158	“ “	--	--	--	--	--	2
159	Lorenz Gillig,						
160	Frederick Wuest,						
161	Changed occupants,						
164	“ “						
168	— Loecher,	11	2				
169	Changed occupants,						
171	Peter Schmal,	--	--	1			
172	— Reinald,	--	--	3	1		
174	— Wepner,	1					
176	— Angelmiller,	--	--	6	3		
178	— Back,	--	--	1			
179	— Spengler,	--	--	9	4		
180	Changed occupants,						
181	“ “						
182	“ “						
184	— Webster,	--	--	2			
188	— Washington,	2					
189	Wm. Messing,	--	--	5	2		
190	— Jackson,	--	--	--	--	1	1
190	Fried. Kibler,						
192	Bodamers House,	--	--	7	2		
195	Joseph Hartman,	1					
199	Andrew Guenther,	--	--	--	--	2	
200	Vincens Messmer,						
202	John Sabel,						
206	Joseph Ambs,						
209	George Beckle,	--	--	1			
210	Mathias Lutz,	1					
211	John Wolf,	--	--	1			
212	Changed occupants,						
214	Franz Droll,						
216	— Lutz,						
218	Changed occupants,	--	--	1	1		
219	Wm. D. Tute,						
225	Friederich Emerich,	4	2				
228	— Bickel,	--	--	1	1		
		20	4	38	14	3	3

FROM HICKORY TO EAST SIDE OF PRATT—10 FAMILIES.

No.	NAME OF OCCUPANT.	Before July 1.		Between July 1 and Sept. 14.		After Sept. 14.	
		Sick.	Died.	Sick.	Died.	Sick.	Died.
233	G. Fisher,	--	--	5	4		
234	Christopher Schmah,	--	--	3	2		
237	— Bauer,	--	--	8	6		
238	Changed occupants,						
239	— Schandall,	--	--	--	--	2	1
241	Changed occupants,						
242	“ “						
243	— Uebelacher,						
244	John Dehent,						
246	— Hauenstein,	--	--	1			
248	Nicholas Brick,	--	--	5	2		
248	— Klotz,	--	--	4	1		
250	Frederick Geib,	--	--	4	1		
				30	16	2	1

FROM EAST SIDE OF PRATT TO MORTIMER—17 FAMILIES.

254	Peter Seibert,						
255	Changed occupants,						
“	“ “						
“	“ “						
“	“ “						
“	“ “						
256	“ “						
257	Adam Schauf,						
259	Philip Schauf,						
“	Jacob Scheu,						
“	Joseph Meyer,						
“	Valentine Schneider,						
262	Changed occupants,						
“	“ “						
“	“ “						
277	Adam Guth,						
279	Wm. Borger,						
“	Heinrich Thal,						
“	H. Ekel,						
284	Andres Bodamer,	--	--	1			
“	Changed occupants,						
“	Christian Dismar,						
286	Wm. Bedinger,						
287	Anton Trot,						
288	Christian Vogel,						
289	F. Jacob,						
295	Reinhardt Philip,						
				1			
	Total from Mich. to Mort.,	20	4	69	30	5	4

The summary may be stated as follows:

Whole number of cases from Elm to Mortimer during the season, 97. Whole number of deaths, 41. Of the deaths, 3 occurred between Elm and Michigan, 21 between Michigan and Hickory, 17 between Hickory and east side of Pratt, and none between east side of Pratt and Mortimer. Again, of the deaths, 30 occurred between the 1st of July and the 14th of September; four before the 1st of July, four after the 14th of September, and three are not determined. (See diagram in Appendix, A.)

It will not escape your observation that nearly all the deaths were along the line of the ditch, or within 300 feet of its north-eastern extremity, in which direction our winds would be most likely to carry the miasms. If the water courses were obstructed also, the reflux would be in the same direction, as the street has a declination from east to west and south.

Such are the facts as near as we have been able to ascertain them; and while your committee do not regard such evidence as conclusive, yet, when taken in connection with all the circumstances, we cannot avoid a belief that to the presence of the ditch ought to be ascribed in some measure the extreme malignancy of the cholera in its neighborhood.

Whether its agency depended upon direct emanations from the upturned soil, or upon the obstructions caused by it to the water courses, we cannot positively determine; yet, considering the character of the earth disturbed, we think the latter altogether the most probable supposition. The malaria from a soil like that through which this ditch was carried must have been, we apprehend, too inconsiderable to be regarded as in any manner an adequate source of the sickness.

Attempting to carry our investigations into other streets, through which ditches were opened during the same season, and in which it had been said that similar consequences had followed, we found our inquiries ending in no satisfactory results, and we therefore soon ceased our examinations. Ditches were made generally for the purpose of removing nuisances, in many streets, and in one instance at least, by request of the inhabitants. Such was the fact in Cherry street. The street was covered in various places with stagnant pools of water, and the lots had no means of drainage. In this condition the cholera broke out among the inhabitants, and they soon petitioned the Common Council to have a drain built in the hope that the disease might be thus arrested — but the cholera continuing to increase in severity after the work was commenced, and the completion being somewhat delayed, they again petitioned hastily to have it closed.

In this instance, also, if the opening of the earth had any connection with the increase of the cholera, it was probably in consequence of its increasing the water accumulations along the street.

If we had been disposed, we might have prosecuted our inquiries upon a larger scale, by an examination of the influence of the removal and deposit of many acres of earth upon the "flats;" a circumstance which has resulted from the necessity of lifting that portion of the town to protect it from inundations, and from the excavations of the Hamburg street canal, and the **Ohio basin**. But we are persuaded that no conclusions of value could be drawn where so many modifying circumstances need to be taken into the account. Here was originally a marl covered with vegetable mould, and it is well known to have been a source of malaria. It is now in part the same, and in part it is composed of irregular basins of clay, containing pools of water, and we presume, also, that it is now, as before, a source of malaria: but to what relative or actual extent we have not the means of determining. One thing at least must have greatly favored this district during the present season, viz., the general prevalence of south-west winds, which, while they have swept off the miasms generated upon the flats, may possibly have contributed materially to the greater fatality of the cholera in the north-eastern portions of the town, over which these winds must pass, bearing their vapors and their poisons.

To us it seems that while few cities have grown as rapidly as Buffalo, in few cities, also, has it become necessary to the same extent to disturb the soil for the purposes of grading, filling, paving, ditching, &c., &c., a circumstance which might have proved harmless where sand was the earth removed, but which, as we have shown, must be directly or indirectly a source of disease where clay and alluvium preponderate.

Recapitulation. — The conclusions to which we have arrived then are,

- First.* That upturning of the soil, under certain circumstances, will produce certain forms of disease, such as intermittents, &c.
- Second.* That in all these cases where newly opened soil occasions fevers, it is the old and decaying vegetable matter thus brought to the surface which chiefly, if not alone, produces the diseases which result.
- Third.* Decaying vegetable matter may produce not only fevers, but also, probably as an *exciting* cause, Asiatic cholera.
- Fourth.* That as an inevitable inference, upturning of the soil may, in the same manner, produce Asiatic cholera.
- Fifth.* That in our own city there are many localities under which vegetable

soil has been buried; and that in all those parts the exposure of this under soil to the air may become an exciting cause of cholera.

Sixth. That upturning of a clay, or sandy soil, which is impregnated with vegetable matter, may also, in proportion to the amount of such organic materials contained, prove a source of cholera.

We wish it to be understood, however, that we regard this cause as **quite**, inconsiderable in the great majority of cases.

Seventh. That independent of the nature of the materials composing the soil the obstructions occasioned thereby to the free passage of water along the gutters, renders upturning of the soil indirectly a source of cholera; and not alone in the manner now indicated, but also by forming irregular basins which contain water, as in many ploughed and unpaved streets, and upon the "flats."

Finally, the practical inferences to be made from all we have said, are,

An active, and intelligent Board of Health, with the co-operation of the Mayor and Common Council and citizens, can do more for the arrest of the Asiatic cholera, than the most able body of medical men: since with them rests the task, so comparatively easy, of removing the causes; while with physicians only remains the work, so often impossible, of applying the remedy.

To this end, and that our city may become again as salubrious as it was known to be before its rapid growth had turned its pastures into pools, and its streets into muddy sewers—as healthy, and as free from epidemics as Utica, Schenectady, Troy, Albany, or indeed as any of our elder sister cities, which are now enjoying an enviable exemption from the present epidemic; and for which they are indebted, no doubt mainly, to their complete drainage, and to the perfection of their public works, such as grading, paving, sewerage, &c.—to this end, we repeat, it is necessary that no narrow system of party interest, or mean economy should prevail; but influenced by enlarged views, which will esteem the lives of our fellow-citizens paramount to all other considerations, pecuniary or political, a plan must be devised, and a work carried out commensurate with the magnitude of the evil to be abated.

Were it necessary, however, we might easily demonstrate that even in a pecuniary point of view, the owners of property would find such expenditures remunerative. The reputation of increased salubrity which our town must soon enjoy, would, we have no doubt, return a quick interest for every dollar thus laid out.

By such measures as the following are these results only to be attained:

- By a system of sewerage, perfect and coextensive with the city limits, or at least with occupied dwellings. (Appendix, D.)
- By draining every cellar, and every lot whose level is below the street, or
- By filling up vacant and undrained grounds.
- By paving every street and lane, at least in those portions of the town where clay or alluvium preponderate.
- By removing all obstructions which arrest or retard the flow of water through the gutters. For it ought to be strongly impressed, that a very small basin of stagnant water is, at certain seasons, the source of a very large amount of poisonous malaria.
- By sweeping the streets *thoroughly*, once or twice a week; and not allowing the dirt to be again sifted and scattered along the way by the loose wagons with which its removal is attempted.
- By requiring builders to occupy less room with their materials; and especially by requiring them so to confine their sand, &c., as that it may not be worked and blown about, rendering it impossible to keep cleanly swept any portion of the street in that neighborhood.
- By opening cess-pools in winter alone; or if they must be opened in hot summer nights, by at least insisting that they shall not be emptied through the whole length of our most public avenues.
- By permitting no vegetables, kitchen slops, or other offal, to be deposited for one moment in any street, or to be thrown into the sewers; but requiring that it shall be deposited in barrels or tubs conveniently placed, to be removed daily by persons employed for that purpose.
- By even a system of domestic espionage, by which the health officers shall feel themselves authorized to enter private premises, and order the abatement of nuisances in private yards and cellars.
- By not disturbing the earth during the summer and early fall months, especially where vegetable mould has been buried, unless upon urgent necessity; and where the necessity actually exists,
- By returning the earth to its place again before nightfall, and at no time permitting it to obstruct the gutters, or water courses.

And if hereafter the cholera shall return to us, it will, we trust, find only here and there a victim, chosen from among that class whom poverty, or "over-crowding," or habits of persona filth, or intemperance, or other

ORIGINAL COMMUNICATIONS AND REVIEWS.

asing vices, or habitual and gross imprudence, have most eminently pre-
sed to the disease. The causes will be confined to every man's own
ling, and to every man's own self; and against such causes, thank God,
who are above want, may provide.

All of which is respectfully submitted.

FRANK H. HAMILTON, Chairman,
PHINEAS H. STRONG,
CHARLES H. WILCOX,

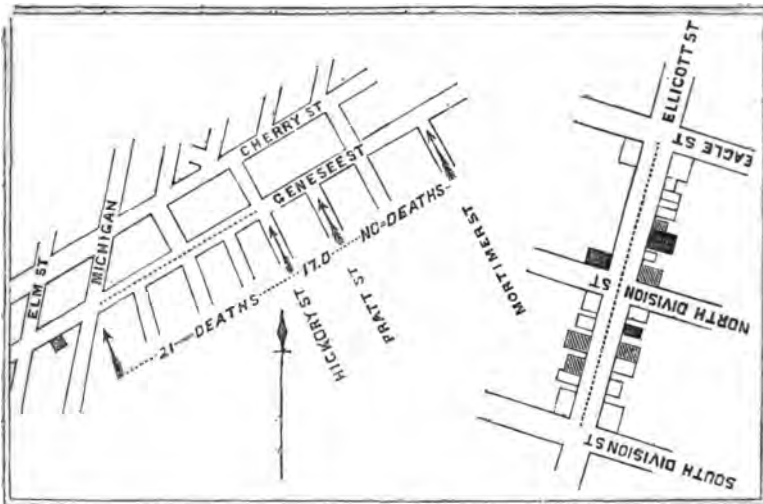
Committee.

APPENDIX,

BY FRANK H. HAMILTON, M. D.

A.

MAP OF PORTIONS OF GENESEE STREET AND ELLICOTT'S.



----- Ditches.

Houses in which deaths from cholera occurred, are shaded black.

Houses in which cases of cholera occurred, but no deaths, are shaded in half tint.

B.

The agency of decaying *animal* matter in the production of malarial fevers
is pretty universally denied; and we regard, also, the evidence of its agency
in the production of Asiatic cholera as quite equivocal. The odor from

putrid animal substances, is certainly far from being agreeable, and we would choose always not to be obliged to respire it; yet every one knows that the offensiveness of an odor is no test of its unwholesomeness. Few odors are more offensive than sulphuretted hydrogen, yet at Avon at least, it is regarded as innocuous—on the contrary, it is snuffed up by the valetudinarians from early morning until night, as the very essence of life and health.

Decaying animal matter may, we know, under circumstances of extraordinary concentration, produce asphyxia, and possibly, yet of this we are not quite certain, low typhoid fevers: but no evidence of its competency to the production of malarial diseases or of cholera has, to our knowledge, ever been furnished. It is, therefore, that in our report we declare decaying *vegetable* matter to be, in our opinion, where earth is upturned, the chief or sole cause of the cholera which may result.

In confirmation of this opinion, we refer our readers to the very able and elaborate work on "Hygiene Publique," by Parent Duchatelet, published in Paris in 1836. The work is in two vols. octavo, and comprises over 1200 pages.

M. Duchatelet, formerly a physician, had devoted fifteen years of his life exclusively to the prosecution of the subject of Public Hygiene. He was also for many years one of the most active members of the "Conseil de Salubrité," and his reports, in reply to the various inquiries of the government, are very numerous, and are esteemed every where as authority. It will be understood that the "Council of Health," in Paris, is usually composed of the most distinguished members of the medical profession, who hold their places for a succession of years; and such confidence have the municipal government in their counsels, that seldom or never has their advice been rejected.

M. Duchatelet himself visited repeatedly all the workshops of the tanners, the manufacturers of animal grease, glue, music strings, Prussian blue, the slaughter-houses, and especially Montfaucon, that immense establishment for the disposal of old and worn-out horses. Here annually are brought, either dead or alive, from 12 to 13,000 horses. Every portion of this carrion is worked over and used up for various economical purposes. The main and tail, the hides, the meat, the bones, the hoofs, the shoes, the fat, the intestines, and their contents even, are piled into separate heaps, and worked up, or immediately sold. Nothing can exceed the filthiness of this "Chantiere d'Equarrissage." The enclosures and the air of the country for some extent around, are saturated with the most disgusting odors. Yet M. Duchatelet has

ascertained that the laborers enjoy both here, in the tanneries, and in the manufactories of gut strings, as good health as others of the same class in other occupations. He tells us of the "fécondité remarquable" of the women, and of the "strength and "good looks" of the children, who are often cradled as it were "in the interior of a carcass." But what we shall notice in this connection as most curious and pertinent, is that all families in these various establishments enjoyed, says M. Duchatelet, *a remarkable exemption from illness during the destructive prevalence of the Asiatic cholera in Paris.*

We have ourself, in 1844, visited the same establishment, now removed to a greater distance from the city, for the purpose of verifying the observations of M. Duchatelet, and we can attest that among the children employed in picking over and assorting the various offals, we saw no indications of bad health, except the pallor which would naturally result from confinement and sedentary habits; for these children, male and female, sit in the midst of large heaps of offal, upon benches or stools, and thus are employed from morning until night. We can certainly attest that nothing could exceed the offensiveness of the odors with which the building, and the grounds, shut in by a high wall, were impregnated.

"M. Duchatelet," says the reviewer, "examines at great length the question, whether the existence of anatomical amphitheatres in a city is at all injurious to the public health; as some years ago, it was often urged against these establishments, that they were so many 'foci' of infectious emanation. He has quite satisfied himself that there is not a shadow of truth in this statement; and among other arguments, he appeals with confidence to the healthy state of the Hotel Dieu in former years, when this hospital was surrounded by numerous dissecting rooms, some of which were even within its walls. He next adduces the testimony of a great number of eminent authorities to confirm his opinion. The first he quotes is the late M. Lallemand, who has left us an account of the dissecting rooms first established by Dessault. They were, it appears, situated on the top story of an old decayed house. The number of bodies usually on the tables was from fifty to sixty, and the number of pupils 200 or more. The rooms were very seldom cleaned, and even the debris of the bodies was not removed oftener than once a month. Nothing could exceed the abominable stench diffused over the immediate neighborhood; and yet, says M. Lallemand, we never heard of any diseases, which might be fairly attributed to the presence of the dissecting rooms, either among the students themselves, or among the inhabitants of the adjoining houses. Dessault himself used to say, that he really believed that the *odorous* air of his dissecting rooms saved him from attacks of epidemic and other disorders, of which those hospital physicians and surgeons, who seldom or never dissected a dead body, seemed to be much more susceptible than himself. He took pleasure in repeating the old saying — 'morte la bete, mort le venin;' and therefore he did not feel at all desirous, he said, to alter the state of the rooms under his charge.

"M. Duchatelet next appeals to the testimony of Dubois, Dupuytren, Boyer

and of a host of others still alive, who have been, or still are teachers of anatomy. They all agree in opinion, that it is quite an error to suppose that the air of a neighborhood is ever contaminated—so as to induce disease—by the emanations from dissecting rooms, or that the students ever suffer from breathing the impure air of these places.

“M. Duchatelet applied to M. Andral, among many others, for his opinion on this subject. The following is an extract from his letter:—‘As to the diseases which medical students contract during their dissections, we have no good grounds for attributing them to cadaveric emanations. Gastroenterites, meningites, and typhoid fevers, are very frequent among the students during the first year of their stay in Paris; but these diseases depend so little upon the mere circumstance of the ‘sejour’ of the students in the dissecting rooms, that they are more common among those who have not begun to dissect. Indeed the mortality among the medical pupils is certainly not greater, than among an equal number of any other class of young men. The fatigues of study, the late hours, the intellectual exertion at home for the concours, &c., are much more hurtful to the constitution, than the labors of the dissecting room. I have taken the trouble to ascertain the general health of the servants of the amphitheatres—and some of them pass day after day there without once going out—and it appears that they are quite as healthy in every respect, as other men.’

“With respect to the health of the students themselves, M. Duchatelet has taken great pains to discover, whether there are any good grounds for supposing, that it is affected by the air and the labor of the dissecting room. According to his account, the most frequent complaint among these youths is dyspepsia, attended often with colicky pains and diarrhoea. But these symptoms are, says he, attributable much less to their engagements in the dissecting rooms, than to the imperfect or improper diet, which the ‘*res angusta domi*’ imposes upon many of them. Not a few of the French students, especially those who arrive from the provinces, are but scantily provided with even the absolute necessaries of life. M. Duchatelet assures us that he has known several, who have lived for weeks, aye, and months too, upon stale bread, and an occasional glass of brandy.

“It may be worth while to notice, also, the usual good health of the servants of the anatomical theatres. Many of them live within the premises; yet they do not suffer any direct inconvenience; and it has been often remarked that these men are singularly exempt from febrile diseases.” (L. M. C. R. vol. xxvi. pp. 424–26.)

M. Rousseau, who for thirty-six years had superintended the dissections in the extensive rooms devoted to comparative anatomy, connected with the “Garden of Plants,” declares to M. Duchatelet that neither he nor any of his assistants had ever suffered from their occupations even during the summer, although the bodies of the animals were often in a highly putrid condition.

Mr. Lawrence, of London, also confirms the same opinion, in a letter to Dr. Bancroft, quoted by M. Duchatelet, and cited by Dr. Warren, in the Boston Med. and Surg. Journal.

With such testimony before us, and with no knowledge of any conflicting

evidence, we are forced to the conclusion that decomposing animal substances are not often a source of disease, and especially of the epidemic forms.

If, however, we find that vegetable decomposition is in some way the general cause of malarial diseases, and not unfrequently of cholera, dysentery, &c., yet we do not affirm our belief in any particular doctrine of malaria. In what the morbid agent consists to which, after the Italians, writers generally have applied this term, we have as yet no positive knowledge. It may be of a "fungous" or "cryptogamous" nature, a poisonous vegetable which inoculates itself upon the animal; it may be "gaseous," the result of a chemical separation and decomposition, or "animalcular," or any thing else upon which it is easy to speculate, but impossible to determine. The general concurrence of certain diseases, including epidemic cholera and dysentery, with the rapid decay of vegetable substances is all we pretend to assert: a concurrence which is sufficiently constant to furnish strong presumptive evidence that they stand in the relation of cause and effect. Nor is our confidence in this belief at all shaken by the facts stated by Linnæus in his thesis entitled "*Hypothesis nova de febrim intermittentum causa*," or by the observations of Von Aenwank, or of Bell, all of which establish that periodic fevers are common where an argillaceous or clayey soil prevails, as well also as in alluvial districts. On the contrary, such facts and opinions only confirm the doctrines to which we hold, and which we now advocate.

C.

The following are briefly the circumstances, so far as we can ascertain them attending the sickness in the house No. 513, east side of Main street, to which the attention of the "Association" has been called by a verbal statement. The house is a very old frame building, composed of four or five rooms, situated in a healthy neighborhood, and with neat, clean premises:

The occupants were John Nugent, wife and infant — Irish. Mr. Wilson, wife and two children — English, and Sarah, an adult Irish girl.

John Nugent and Wilson were carpenters, and worked under cover: both temperate, steady, and healthy — and the same was the fact with all the family.

John Nugent was attacked Thursday, September 2, 1852, at 6, A. M., and died at 11, P. M.

Wilson was attacked at 3½, P. M., September 3, (Friday,) and died at 2, A. M., of Saturday.

Catharine, wife of John Nugent, was attacked Saturday (4th.) and died at the hospital Sunday, P. M.

Sarah was attacked after reaching the hospital Sunday (5th,) at 10, A. M., and died there the same day.

John Nugent's infant was attacked Sunday and died several days after.

Wilson and family had occupied the house three weeks; Nugent and family one week, and Sarah had been in the house, when she died, three days.

Having made careful inquiry, we learned that there had been no imprudence in diet.

Impressed with the belief that some local cause existed in or about the house to which the sickness was due, we inquired whether there was a cellar, and learned that there was a hole called a cellar, under the front room, but that Mr. Wilson having found it "close and musty" when they first occupied the house, they had determined not to use it, and had left it closed from that time.

Dr. Merriam, who was in attendance also, and the writer, immediately descended into the cellar, which was entered by a trap door from the dining room.

The air was very warm, damp, and suffocating. The soil in which the cellar had been made was clay; and the sides had been originally lined and walled up completely with boards. Most of these boards, however, had decayed, and the earth had fallen in, leaving one or two openings where some air and light were admitted. Nearly all the boards, in addition to being very much rotted, were covered with thick masses of vegetable mould, or fungi, of the cryptogamous order.

It is, perhaps, rather a curious than practical fact, that a careful microscopic examination made by Dr. Merriam, showed ten different species of cryptogamæ.

We did not doubt that in the vegetable malaria of this cellar we had found the source of the disease.

D.

In many respects our system of sewerage is eminently defective. The mains are not sufficiently large — the branches are not sufficiently numerous, but especially is it defective in the almost complete absence of stench traps, which alone, we are informed, can effectually prevent the return of the gases into the streets and dwellings.

This is a great and rapidly increasing evil, and while we wish to give due prominence to the other causes, and especially to the surface and underground miasms, of which we have now spoken at length, we deem it proper to refer also to these sewer miasms as, in our city at least, an equal, possibly it may

be, a much more abundant source of disease. It is the more proper to speak of them in this report, because it not unfrequently happens that in upturning the earth to lay gas and water pipes, the inlets or mains of the sewers are cut across and their gases thus escaping may produce those diseases which result. Such, however, we ought to say, was not the fact in Ellicott street, nor in Genesee and Cherry street. In Ellicott the inlets were below the bottom of the ditch, and in Genesee street and Cherry, the excavations were for the purpose of building sewers.

These sewers receive a large amount of offal, chiefly vegetable; and to be convinced that they may eructate villanous odors, one has only to place himself over one of the open mouths, or inlets, at any corner of the streets.

Most of the main trunks have their outlets above the surface of the water, on the creek or canals, open toward the prevailing winds, so that they are daily and hourly, when the winds are favorable, blowing their impurities up into the houses and into the streets. *At all times*, whether the wind favors or not, they do not cease to breathe out poisons and dispense it silently and imperceptibly into our dwellings. If we do not always recognize it, it is because we have in general become too much accustomed to it.

We are informed by Mr. Brick, that his men often fall sick and are obliged to leave their work when accidentally or by necessity, they tap one of these inlets or house drains.

That we should continue to be exposed to this source of disease, and of cholera no doubt, where the remedy is so easy, implies great neglect, if not actual criminality, on the part of our city authorities, as well as of the citizens themselves. Stench traps, constructed at a trifling cost, have been proven to be an effectual remedy. At the expense of the city they should be immediately placed at every street inlet; and no person ought to be permitted, under a suitable penalty for the omission, to connect his premises with a sewer without a similar sink. The expense to the city of keeping the street traps sealed by a sufficient supply of water from the hydrants, would be inconsiderable.

How effectual these stench traps prove, a large experience in other cities has fully determined, and some of our citizens have ascertained by having laid them in their own premises. But many of you, perhaps, will remember that when the gas works were first completed in the fall of '48, our whole town was made to celebrate the event by a sudden irruption into every house and street having a drain, of an intolerable odor of sulphuretted hydrogen.

The refuse gas from the gas works' establishment had been emptied by a chimney into one of the main sewers near its outlet, and a favorable wind

had carried it upward into every artery of the city sewers, and people were driven from their houses into the streets, and even there the stench followed them.

Immediately, by order of the superintendent, Mr. Brick, the main sewer was cut off above the point where the gas pipe connected with it, and a stench trap was laid, and the offence at once ceased.

Something might also be accomplished, and probably at a cost to the city of less than \$300 annually, by flooding the sewers occasionally during the summer season by turning several hydrants simultaneously into a single main.

ART. V. — “*Mercurials in Dysentery.*” By S. B. HUNT, M. D.,
Mendon, N. Y.

You will, I am assured, pardon me for again trespassing upon your kindness. The subject of “*Mercurials in Dysentery*” is, at present, so far as the pages of your Journal are concerned, left in a shape which I do not like. My communication in the September number was brief and hastily written and the propositions which I advanced were too ill digested to withstand your searching criticism. I am, however, rather antagonistic in my nature, and am, as yet, unwilling to abandon my ground.

The question at issue is whether mercurials are necessary or proper in the treatment of dysentery. Now much depends upon what we call dysentery. If you rule down the term in its application to simple, unmixed, acute dysentery, I acknowledge myself at once *hors du combat*. But inasmuch as we of the country see but little of this simple dysentery, and that little easily controlled by opiates and rest, we do not, when speaking of dysentery, have this form of disease in our minds, but rather a disease wherein the symptoms of unmixed dysentery are present in great force, but accompanied and modified by a train of symptoms, which add greatly to the difficulty of its management. These symptoms are those to which universal consent has given the name — *bilious*. Do not accuse me of having shifted my ground. My previous article had reference, mainly, to the Portage Epidemic of 1848. My precise ground on this point of etiology is this: I suppose that portal congestion, pre-existing in a constitution exposed to extreme heat, or sudden changes from extreme heat to cold, will, in the majority of cases, determine the production of dysentery in preference to diarrhœa. Commona crapulous diarrhœa may result from the irritant nature of the ingesta, or it may depend upon an irritable condition of the intestinal mucous membrane produced

by the common causes of bowel complaint, mentioned above. This irritable condition of the bowels may run into follicular enteritis, and in constitutions enfeebled by city residence, and less frequently in those removed from that influence, may even go farther into that pronounced form of muco-enteritis which we call dysentery, and this may and does happen without any portal congestion. But it will happen far more frequently when portal congestion exists.

The term "*bilious*," it is true, has been woefully misapplied, but it still has, in my mind, a single definite meaning — confined to that state of the liver where, be the ultimate cause what it may, the bile is not properly excreted, and manifests its presence in the circulation by discoloration of the conjunctiva and skin, and frequently the urine and sweat — by a whitish tongue — somnolency at noon-day — headache — langour — dull pain in right side, nausea and loss of appetite. Much as we deplore the indolence and want of research which would make the liver the common progenitor of all disease, we should no less avoid that contrary error, which would ignore the very existence of a liver, and confine its disturbances in their effects to the organ itself, forgetting that through it pours the whole vast tide of the abdominal circulation; of which it stands the headgate and governor.

Should you insist, as you intimate you might do, upon my producing *positive* evidence as to the malarial origin of the disease in question, I might find it difficult to convince you. It is extremely difficult to reason from a class of facts so intangible as the ultimate causes of disease. A large class of minds in their zeal for making medicine an exact science, rule out everything which would not be evidence in a court of justice. In regard to this particular epidemic at Portage, I can only state my firm conviction that malarial causes were at work. I have, in my previous article, supplied you with the medical topography of the township — I will only add that at the Deep Cut of the Genesee Valley Canal, the same causes were at work upon a grander scale, as those described by Prof. Hamilton, as, in his opinion, the exciting causes of cholera in Ellicott street, Buffalo. I confess that I had supposed it to be a settled point that malarial causes produce hepatic disease; no tissue of reasoning can stand up against the well known fact, that hepatic diseases have their favorite haunts in low, warm situations, along the banks of sluggish streams, upon the flats and bottom lands adjoining great rivers, and in the forest newly laid open by the axe of the pioneer to the solar rays. Wherever I have seen fever and ague. I have seen accompanying hepatic disease, and the universally received nomenclature, which has given to miasmatic disorders the familiar generic term of bilicus disease, is an evidence of a deep

fixed conviction in the medical mind of the existence of such a thing as a bilious state of the system.

In the summer of 1847 I was attending the family of B. W. French, living on the verge of Mendville flats, on the Genesee river. Six or seven of ten families were down with dysentery at a time. The house stood at a slight elevation above the flats, and each were every morning covered by a dense fog, the smell of which was positively *rotten*. This fog was stratiform, and I have frequently, in climbing a slight knoll, found myself above its level, and seen it stretched like a lake across the wide fertile valley, while here and there an isolated tree or the roof of a barn, lay like an island on its surface.

I had the idea at the time, that this fog was surcharged with miasma, and that to it Mr. French and his neighbors were indebted for their dysentery which prevailed through the valley, while others of them were shaking with the ague from the same cause. At any rate they were decidedly *bilious*, having all those symptoms which I have enumerated as peculiar to that condition, and which they possessed in common with their fever and ague neighbors. People living on the hill-side above the level of this paludal exhalation were healthy. Supposing the exhalation from the flats to be the cause of the disease, we would expect patients nightly under the influence of that cause to be difficult of cure. Such was the case. They were the most intractable cases I had that season, and only recovered when I ordered the sick to the upper part of the house, and the doors and windows to be kept closed until such an hour in the morning as the fog was dissipated. Then they got well with as much rapidity as the cholera disappeared from Ellicott street, when that ditch was closed.

My good old preceptor, Prof. Trowbridge, said, that no one took fever and ague in the daytime, and it was his practice when spending the night with his patients in the malarial district on the Grand River below Painesville, Ohio, to sleep in an upper room, and to this he attributed his entire immunity from malarial disease.

And now about the inflammatory nature of the disease. I certainly had no intention of denying the existence of ulceration, deposits of lymph, etc. I have seen some post-mortem appearances in dysentery. Instead of the term "purely inflammatory," I should have said that the disease, as it has more usually presented itself to my notice, is not *simply* inflammatory in its nature, i. e., that the inflammation is so modified by its causes, as to present different symptoms from any that could result from a simple muco-enteritis. The grade of fever was low, in many cases a periodical tendency to chill could be detected; in many, too, the patient was completely prostrated at the

outset of the disease, and, in a state more resembling congestive fever than any other pathological condition. I meant to convey the idea that in cases depending as I believed on paludal origin, the disease was not a simple muco-enteritis yielding to opium, entire rest, and the abstraction of blood, but an inflammation modified by the causes which gave it birth, and requiring some remedy reaching beyond the locale of the disease, to the causes in which it originated. That such a form of dysentery exists as you describe, I cannot doubt, for I have seen it and allude to the opium treatment in my article as successful in those cases. But I think it is in cities where they mostly obtain.

In this lengthy discussion of etiology and pathology it has been my purpose to keep an eye to the main question at issue, viz, the propriety of the exhibition of calomel.

In coming to the discussion of therapeutical effects we tread at once on more satisfactory ground. We administer many drugs empirically, without any pathological reasons therefor. When we derive benefit from the exhibition of a drug, it is a matter which our own eyes can see. When we refuse to administer it from preconceived notions as to the pathology of a disease, we are working in the dark, because our notions of pathology are much more liable to error than is an opinion as to the therapeutic effects of a drug derived from experience. No one could argue *a priori* that capsicum would be a good gargle for a sore throat, yet experience has taught us its value.

A difference of opinion as to the point in question is no new thing. Writers are so divided that a collection of authorities would yield no light. Calomel has long been used in the whole class of bowel diseases, and it seems still to have as strong a hold as ever. In dysentery the modes of its administration have varied. I have myself usually given it in doses of three grs., combined with Dover's powder in ten grain doses for three or four doses in succession, and have followed it with some mild cathartic. I have known it given in full cathartic dose at the outset of the disease, and have the word of respectable physicians as to its efficacy. This is, I am informed, a very common practice in those malarial regions in Maryland and eastern Pennsylvania, where dysentery is a formidable yearly visitant. It is also sometimes given with a view to its sedative effect, in doses of from ℞j. to ℥ss. half hourly. It is claimed that this practice has been attended with unrivaled success in tropical dysentery. Of these latter modes of administration I have no personal knowledge. Of the first mentioned manner of exhibition I have an experimental knowledge, which is to me satisfactory. In 1846, when my acquaintance with the disease commenced, I was in very limited practice, and saw but few cases. There were, no deaths. In 1847 I attended about

twenty cases, but I do not know the exact number. There were no deaths. In 1848 I attended 130 cases, and had six deaths; one of these was a feeble old man of over seventy years, the others were children. I was, at the close of the epidemic of the opinion, that had I been more thorough in the administration of calomel, I might have lost fewer cases. In 1849 I attended twenty-eight cases; of these one was fatal, a man eighty years of age.

Since my removal to my present location, I have seen less of the disease by far, than when at Portage. I have, however, carefully watched the effect of the remedy, and am more than ever convinced of its efficacy. I think that even in the medium doses which I administer, it has something of that sedative effect attributed to the heroic dose. I always consider that when I have once obtained a fœcal evacuation the disease is under control. Aside from its effect upon the biliary secretions, it assists the sudorifics with which it is combined in directing to the surface, it has its usual specific action upon the inflammation, and what is very important, it prepares the way for a cathartic. There is in many severe cases of dysentery a stricture at the caput coli, which prevents the passage of anything from the upper bowel, save in quantities so small as to add to the irritation at the seat of the disease. Accumulations of fœcal matter and vitiated secretions form above this stricture, and it is frequently difficult to get cathartics past this point without calomel preceding their administration.

During the Peninsular war, and especially in those disastrous retreats made by the British army, the troops, sleeping upon the ground uncovered, and exposed to all the forms of miasma, were visited by dysentery in its unmanageable form.

In two years and a half the British army in the Peninsula lost no fewer than 4,717 men by this "scourge of armies." Sir James McGrigor, who had the superintendence of the medical department in these campaigns, and in the no less disastrous Walcherean expedition, after opportunities for observation unequalled in the history of the disease, recommended to the hospital surgeons a treatment, in which calomel, followed by a saline cathartic, figured as the leading prescription. It was given upon alternate days, the bowels being closed with Dover's powder immediately after the operation of the cathartic. I believe that this will still be found a satisfactory treatment in all cases where portal congestion exists, and I have already expressed my belief as to the frequency of these cases.

There has been much dispute as to the propriety of cathartics. I consider them as an important adjuvant. Constipation is one of the most constant concomitants of dysentery. If we give no cathartics we have, in spite

opiates, a constant dribbling of fœcal matter from above over the inflamed surface. *With* a cathartic we sweep them out at once, and are thus enabled to keep the bowels quiet with opiates after the operation. The cathartic chosen should be one quick and thorough in its operation. I prefer the Rochelle salt to any other.

The use of astringents I have already objected to. They are unphilosophical in every point of view, unless we regard the disease as a passive hæmorrhage. *En passant*, does opium in the dose which you propose "lock up the secretions of the liver" or any other organ? In a case of persistent spasm of the intestines under treatment last week, I was obliged to keep the patient under the most powerful doses of opium. I gave him twelve grs. of sulph. morph. in twelve hours. Afterward I gave sixty drops of laudanum, hourly, for twenty-four hours; and after that forty-five drops, hourly, for another twenty-four hours. During this time I procured, by enema, free evacuations of slime and mucus, gradually changing as the disease declined, to fœcal matter. There was certainly no deficiency of secretion in this case, for large quantities of bile were present in the stools.

I have only one thing more to say as to the treatment of dysentery, and that is about injections. I have found the cold water enema of far greater service than the more usual mixture of starch and laudanum given warm.

In conclusion, I beg that you may not rank me as an indiscriminate advocate of calomel. I believe I use less than the average of practitioners, and have even, the past summer, treated hypertrophied liver with the alkaline carbonates, and without calomel, successfully.

EDITORIAL DEPARTMENT.

Cholera at Buffalo. — In our last issue we stated that epidemic cholera was then prevailing to a considerable extent in the city. Cases of the disease began to make their appearance early in July, and continued to increase in number during that month. As no reports were made to the Board of Health, the number of cases occurring in July cannot be ascertained. During the month of August there were reported, by physicians and sextons, 326 deaths from cholera. This does not embrace cases of death by cholera infantum, of which 128 were reported during the month of August. The

epidemic continued with unabated severity up to about the middle of Sept. At this time the weather became cold; a rain storm of several days' duration occurred, which was followed by fine autumnal weather, and the disease rapidly subsided. At the present time, September 25th, the city is nearly free from cholera; and, excepting some cases of dysentery, there is very little sickness. The number of deaths, reported by physicians and sextons, from September 1st to 12th, inclusive, was 300. The whole number of deaths reported from August 1 to September 12, thus, amount to 626. Including the unknown number of cases occurring prior to August, the fatality of the disease has been nearly, if not quite as great as in 1849. In that memorable season the number of deaths reported between June 25th and September 7th, was 858.

The duration of the epidemic during the past season, was about the same as in 1849, commencing somewhat later, and continuing longer into September, occupying in its career a little over two months.

The disease prevailed most in sections of the city comparatively insalubrious. At first it was most rife at a part formerly known as the "*flats*," the ground being, as the title implies, low, and requiring, for the most part, to be filled up to protect it against inundations from the lake during high winds. During the latter part of the time there was most suffering toward the eastern limits of the city where the ground is low and marshy. Occasional cases, however, occurred in all parts of the city.

The poorer classes of society furnished a large majority of the victims. At a rough estimate, nineteen-twentieths of the fatal cases were among the foreign population, viz., Irish, Germans, and Norwegians.

The tendency to disorder of the digestive system, among all classes, during the prevalence of the epidemic, was observed, as in 1849. Other affections were extremely infrequent. Medical practice consisted almost exclusively in prescribing for diarrhoea; so much so, that to meet with a case which did not involve special attention to the bowels was almost a luxury, by relieving the monotony of the daily duties of the practitioner. It is common to regard the diarrhoea which coexists with a cholera epidemic as denoting a proclivity to the latter form of disease. Whether this view be correct, or not, the safest policy, undoubtedly, is to act upon the presumption of its being so. With medical treatment it is certain that these *premonitory* symptoms, as they are termed, very rarely eventuate in cholera. As respects this point, our own late experience accords with that which we gave in our report of the epidemic in 1849, (*Buffalo Med. Jour.*, Nov., 1849.) Out of the host of cases of diarrhoea which have come under treatment during the past sumn

well marked cholera supervened but in a single instance;* and, on the other hand, in the few cases of cholera that have occurred in our private practice, in all the disease was either developed abruptly, or the premonitory symptoms had been neglected.

In treating the prevailing diarrhoea during the past season, we employed anodyne remedies almost uniformly and exclusively, to wit, opium in tincture, or substance, and morphia, given in pretty full doses; repeated according to circumstances, quietude, and care in dietetics being enjoined. This course we found sufficiently reliable, without exposing patients to the risk of ptyalism.

Not having had the misfortune to meet with many cases of fully developed cholera, our experience in its treatment during the past season has been limited. So far as our observations go, they furnish some striking illustrations of the efficacy of morphia, given promptly in pretty large doses, in arresting, at once, the progress of the disease. We have kept notes of some cases treated after this plan which we may report at a future time.

A conclusion at which all practical observers have arrived, with respect to diminishing the fatality from epidemic cholera, is, that it is a disease to be prevented, or arrested very early in its progress, rather than cured. If it advance to what is generally known as the stage of collapse, recoveries, under any plan of management, will be few. The blood sustains lesions which set the resources of therapeutics at defiance. The important question, then, how shall we stay the ravages of this terrible pestilence, involves another question, viz., how is it to be averted or arrested, in individual cases, during the continuance of the epidemic constitution? Facts show that in the vast majority of cases the development of the disease is preceded by ordinary diarrhoea, and that if the prodromic disorder be treated by simple remedies, and proper precautions observed, a person is nearly secure. The disease prevails chiefly among the classes who do not understand the importance of, or who obstinately neglect to resort to prophylactic measures, and it occurs to a very limited extent only among intelligent members of society who appreciate, and are able to avail themselves of the security afforded by seasonable management. Now, if the whole community, during the prevalence of cholera in a place, could be placed on an equal footing with those whose prudence thus renders them almost exempt from a liability to the disease, it is clear that the number of cases would be greatly diminished. How is this to be effected: in other words how is the disease to be averted, or arrested, among

* In this case the patient was an infant.


the class particularly exposed by their want of intelligence, poverty, and social position? The only effectual plan is to institute a system of sanitary surveillance, to be steadily maintained so long as the epidemic constitution lasts. It is not enough to give public advice by handbills, etc. The object can be fully accomplished only by organizing a voluntary police, subdividing a city into wards, and providing for one or two daily visits by a health officer at every house, his duty being to inquire particularly after the health of every inmate, give directions in case of disorder, and supply remedies. If this plan were to be thoroughly carried out, we verily believe it would render the fatality from this disease small in comparison with what it would otherwise be.

We would not be understood by these remarks to undervalue other sanitary measures for preventing cholera and staying its progress. Facts abundantly prove that the special cause inducing the disease is rendered efficiently active mainly by the co-operation of other causes which we can, to a certain extent, determine, and control. The importance of due attention to the latter is obvious. The plan which we have proposed, however, has never, to our knowledge, been fully adopted, and as it is altogether probable that the disease will continue to recur in different parts of our country, it is to be feared there may be future opportunities to test its merits.

On the subject of malaria as an exciting cause of cholera, Prof. Hamilton has drawn up an elaborate paper, contained in the original department of this No., which it is quite unnecessary for us to commend to the careful perusal of our readers.

Mercurials in Dysentery. — We insert with pleasure a second communication by Dr. Hunt, on the above subject, which will be found in the original department of this No. If our criticisms always succeed in provoking a repetition of the favors of correspondents who write as ably as does Dr. H., we shall have every reason to be satisfied with the results. Although we cannot concur with him in the hepatic pathology, we would commend his practical views to the candid consideration of our readers.

Curators of Med. Department of University of Buffalo. — The name of Dr. Morgan G. Lewis, of Black Rock, Erie Co., was inadvertently omitted in the annual announcement for this year.

 Dr. Maxson's communication, received too late for insertion in this No., will appear in our next.

Resident Students at the Buffalo Hospital of the Sisters of Charity.— We are requested to give notice that on the first of November next, two resident students at the Buffalo Hospital of the Sisters of Charity are to be appointed, one for a term of six months, and the other for a year. The students are required to pay to the hospital two dollars per week for board. They must have studied medicine for two years, and attended one course of medical lectures, to be eligible. The situation offers a fine opportunity for acquiring practical knowledge of medicine and surgery. Applications *in the hand writing of the applicants*, with testimonials, may be addressed to Dr. E. Wallis, Buffalo, chairman of a committee to select suitable candidates.

New Medical Journal.— *The Philadelphia Medical and Surgical Journal*, is the title of a new monthly, with the name of James Bryan, M. D., as editor. Dr. B. has contributed largely to the medical periodical literature of this country for several years past. Some valuable communications from his pen have appeared in this Journal. He has our best wishes in behalf of his enterprise.

New Medical Times.— Dr. Adams has resigned, on account of ill health, the editorial charge of this Journal, and is succeeded by Dr. Bulkley. Dr. Bulkley is known to the medical public as an able writer and teacher. We need not state that he is particularly distinguished for his attainments in the province of dermatology, our readers doubtless being familiar with his valuable contributions to the literature of that subject. We cordially wish him success in his present undertaking.

Notice.— From the 15th of October till the last of February, next, the address of the editor of this Journal will be Louisville, Ky. Letters and communications may be directed to him at that place. With the aid of his colleagues and other friends, the publication of the Journal will be continued under his supervision, as heretofore.

Notice. Notices of Transactions of several Medical Associations are crowded out of this No.

Notice. We beg leave to remind our readers that the usual limits of the Journal are exceeded in this No. by sixteen pages.

BUFFALO MEDICAL JOURNAL

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NO. 6.

ORIGINAL COMMUNICATIONS.

ART. I.—*Clinical Report on Chronic Pleurisy, based on an analysis of Forty-seven cases.* By AUSTIN FLINT, M. D.

The study of Chronic Pleurisy by means of the analytical investigation of recorded observations, offers a field of scientific research which, as yet, has been cultivated but to a very limited extent. In proof of the correctness of this remark, it is sufficient to refer to the cursory and loose manner in which the subject is treated by the compilers of the most approved systematic works on practical medicine, and even in treatises devoted specially to diseases of the chest. Without professing to have taken pains to institute an extensive bibliographical examination with respect to this point, I am unable to cite any work which presents numerical results deduced from a larger collection of data, than that by Dr. Blakiston.* This author gives the facts elicited by an analysis of seventy-eight cases. His enumerations are confined to a few only of the points pertaining to the natural history of the disease, a very small portion of the work being devoted to the subject. The late Dr.

* Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation. By Peyton Blakiston, M. D., F. R. S. Republished by Lea & Blanchard, Philadelphia, in 1848.

James Hope, during his last illness, was engaged in arranging the materials for a memoir on this subject, which should embody the fruits of his large experience, and the posthumous publication of the brief notes relating thereto, dictated on the bed of death, tends to enhance regret that he did not live to execute his design, and add another treatise to the works which will render his name ever illustrious in the annals of medical science.

It is needless to say with respect to this, as well as other diseases, that accumulated clinical observations constitute the true basis of its symptomatology, diagnosis, and our knowledge of the laws regulating its development, progress, and results. This is not less clear than the fact that our present knowledge of it in these relations is imperfect. There are several pathological questions of practical interest which remain to be settled by numerical investigations, such as the connection of the chronic with the acute form; its dependence on tuberculosis or other antecedent affections; the consecutive production of Phthisis or other diseases; the ratio of its fatality, and the influence of remedial agencies, separately and combined. On all these points the notions to be derived from current medical literature are wanting in precision, sometimes discrepant, and, as a matter of course, to a greater or less extent, erroneous.

There is another aspect in which the importance of the study of Chronic Pleurisy is strikingly apparent. The disease, although infrequent in its occurrence, is not so rare but that it occasionally falls under the notice of most practitioners of medicine, however restricted the circuit of their observations. But in a very large proportion of instances it is not recognized. As will be seen in the sequel, a great majority of the cases that I have collected, had been under the treatment of one or more physicians who had failed to discover the seat and character of the affection. It is not probable that these cases are notably peculiar in the fact of their escaping diagnosis, for it is remarked by Dr. Hope that "there is no class of affections more habitually overlooked by the bulk of the profession than this." It necessarily follows from this fact, that the disease very frequently fails to receive appropriate management, and, perhaps, nearly as often is treated by measures which are more or less injurious.

The considerations just adduced have led me to examine the facts relating to Chronic Pleurisy contained in the histories of the cases coming under my observation during several years that I have been accustomed to keep notes of public and private practice. The number of cases is not very large, and, on the other hand, not so small as to be insignificant. A more serious defect is the incompleteness of the histories. A considerable proportion of the

patients were either seen in consultation, or applied simply for a physical exploration of the chest. Under these circumstances a general sketch of the previous history only was noted, and in several instances the farther progress and issue were not procured. The cases, moreover, came under my notice at different stages of the history, and the notes of some were made after recovery from the disease. The data which I have collected are, thus, manifestly inadequate for the positive and negative results of a complete numerical analysis. To this character the present Report can have no claim. My only expectation is to furnish a small contribution toward the cultivation of a field of clinical study, of great practical interest, which, from the difficulty of aggregating facts, or other causes, has hitherto been neglected.

The number of cases of which I have preserved notes, is *forty-seven*. These cases have occurred, some in hospital and others in private practice, for the most part during the last eighteen years. A few are of an older date. For convenience of consideration and reference, they admit of being distributed into several groups. One of these groups will embrace the cases which came under observation while the disease was progressing either favorably or unfavorably. Another group will include cases in which the diagnosis and history were retrospective, that is to say the facts were obtained at a period more or less distant from the occurrence of the disease, being restricted in a great measure to its consequences or sequelæ. Another will embrace cases of circumscribed, as distinguished from general pleurisy. Another will embrace cases of pleurisy from perforation, constituting the affection known by the incorrect title of pneumo-hydro-thorax. I shall speak of these several groups, excepting the first, under distinct heads, but with reference to some points of inquiry it will be proper to examine the cases collectively, disregarding the foregoing divisions. Before considering these several groups, therefore, distinctly, I shall take up the points just alluded to, devoting separate consideration to the symptomatology, the physical signs and diagnosis, and I shall, in conclusion, offer a few remarks on the management of the disease.

Age. Excluding the cases in which the disease occurred subsequently to perforation of the lung, and a single case in which the pleurisy was circumscribed and probably due to external injury, the maximum of age is *forty-five*. One patient, only, was as old as this. One patient was *forty-one*; one was *thirty-eight*, and two patients were *thirty-five*. In all the remaining cases the age was below thirty. The youngest patient was five years of age. Two were of this age. The next youngest was six years; and the next twelve. The average age in *thirty-six* cases in which the age was ascertained

as near as practicable, is twenty-two and a quarter years. The disease, according to these results, is not without the influence of age. It is a disease incident to early life, occurring, in a very large proportion of cases, before thirty, and very rarely after forty years of age.

Sex. *Thirty-four* of the cases were males, and *eleven* were females. In the two remaining cases the sex is not noted, the patients being children.

Season. Of eighteen cases in which the date of the commencement of the disease is noted, excluding the cases in which the disease was due to perforation, *two* occurred in June; *three* in February; *one* in April; *one* in May; *five* in June; *one* in July; *three* in October, and *two* in December.

Previous health. Of twenty-five cases in which the health of the patients prior to the occurrence of the disease was ascertained, it was good in *fifteen*. The cases due to perforation are excluded from this enumeration. In the remaining ten cases in which the previous health was poor, the following are the facts in the cases respectively: One patient had rubeola three months before, with a continuance of cough and debility up to the time of coming under treatment for pleurisy. Another had a severe attack of scarlatina anginosa, a month before coming under observation with pleurisy. In two cases, females, the health had been delicate for some time, with no special malady except leucorrhœa; one of these patients had had lichen a year before. Another, a lad, aged fifteen, had been subject, from infancy, to two or three attacks yearly of difficult breathing, considered to be asthma. Another had suffered for several months from a chronic indolent ulcer on the leg. Another had parotitis shortly previous. In *three* cases the history rendered it probable that tuberculosis existed prior to the pleurisy; and in *one* of these cases the pleurisy was preceded by articular inflammation of the ankle joint, of brief duration.

The foregoing results, coupled with the fact that in 3-5 of the cases the previous health of the patients was supposed to be good, go to show that the disease becomes developed without reference to any particular class of affections as antecedents. The small number of cases in which the evidences of tuberculosis were contained in the histories is worthy of notice. It is, indeed, not improbable that tuberculosis may have existed, in some instances, prior to the pleurisy, and the history furnish no distinct evidence of the fact. In no case was the patient subjected to physical exploration to determine the non-existence of phthisis before coming under observation for pleurisy. And, on the other hand, there is the absence of the demonstrative proof of tubercle afforded by the physical signs in the few cases in which it is supposed to have existed. The pre-existence of phthisis is only rendered probable by the

rational symptoms entering into the early histories. It seems fairly deducible that, judged by these observations, a very large proportion of cases of Chronic Pleurisy do not involve, as an antecedent affection, tuberculosis. The occurrence of tubercle subsequent to the pleurisy, is an interesting point of inquiry, but this will be deferred for the present.

Character of the disease at the commencement. In *six* cases the facts noted with respect to the previous history are insufficient to determine whether the disease was acute, or subacute at the commencement. Excluding these, together with the cases in which the disease was consecutive to perforation, and those in which it was circumscribed, the remaining number of cases is *thirty-five*. Of these thirty-five cases the pleurisy was subacute from the first in *twenty-nine*; it was acute before it became chronic in *two*; and it was subacute at first, after a few days assuming an acute form, and then becoming chronic in *four*. According to these results subacuteness in the inflammatory action from the date of the attack is decidedly the rule with respect to Chronic Pleurisy. This does not agree with the statements generally made in works on practical medicine, and on diseases of the chest, relative to this point. The chronic form of the disease is said usually to follow the acute. To avoid misconception, inasmuch as the terms acute and subacute allow some degree of latitude in their application, it should be added that in all the cases which are considered to have been subacute from the commencement, severe pain occasioning embarrassment in respiration, and high febrile movement were absent. The pain, in fact, was usually slight in degree, and sometimes scarcely present at all, the patients frequently not taking to the bed, or applying for medical aid, till the affection might be considered to have become chronic.

Seat. Excluding four cases in which the disease followed perforation, and one case in which it was circumscribed and probably due to mechanical injury, the remaining number is *forty-two*. Of these forty-two cases the disease was seated in the *right* side in *nineteen*, and in the left side in *twenty-three*. The disparity as respects the side affected is not nearly so great in this collection of cases, as in those analyzed by Dr. Blakiston. The latter gave, of seventy-eight cases, the right side affected in *twenty*, and the left in *fifty-eight*.

Complications. In three cases the histories render it probable that tuberculosis coexisted. The disease occurred in connection with typhoid fever in one case, and with what was regarded as infantile continued fever in another case. Chronic laryngitis coexisted in two cases, in one of the cases tubercle probably also existing, but not in the other case, as proved by the autopsy.

A chronic ulcer of the leg in one case. Ascites, pericarditis, and a slight degree of chronic inflammation in the other side of the chest, in one case. Pericarditis was supposed from the physical signs to be present, in one case, and was demonstrated after death in another case. These are all the complications that can be ascertained from the histories. The cases in which perforation preceded the pleurisy are excluded.

Causes. In four cases the disease was occasioned by perforation of the lung. In two of these cases the perforation occurred in consequence of circumscribed gangrene, and in two instances in connection with tuberculosis. In one case in which the pleurisy was circumscribed, it was probably caused by external injury. This case will be given at length under another head. Of the remaining cases, in the great majority, not even probable causes are mentioned in the histories. It is stated in the history of two cases that the patient was attacked after unusual exertion and exposure. In another instance the patient was attacked suddenly, after exposure to cold. Another patient was seized after getting the feet wet and apparently taking cold. These are all the causes assigned, but it is proper to state that I am by no means certain that pains were generally taken to obtain information from the patients or friends relative to this point. The probable origin of the disease in one of the cases claims particular notice. The patient, a girl, twelve years of age, was attacked suddenly, without any assignable cause, with pain in the back, extending laterally over the right side, but not to the anterior surface of the chest. No medical attendance was obtained until a week afterward, when she was seen by Dr. Josiah Trowbridge, of this city. At that time Dr. T. discovered a swelling situated on the latero-posterior portion of the chest. He regarded it as phlegmonous inflammation, deeply seated, but exterior to the walls of the chest. At the expiration of another week, Dr. Trowbridge saw the patient again, and he then thought there was fluctuation, but did not deem it advisable to make an opening. The swelling was very tender to the touch. A few days after the second visit by Dr. Trowbridge, Dr. Sprague, of this city, was called in. He regarded it in the same light as did Dr. T., and directed poultices, which had already been applied, to be continued. I saw the patient three weeks after the date of the attack. There were, then, the evidences of Chronic Pleurisy, with considerable enlargement of the chest on the right side. In my note of the first examination, nothing is stated relative to the presence of a tumor, and as the case came under observation many years ago, I am unable to say, from recollection, whether it existed or not. If it were present, or in any degree prominent, it is singular that in the record made at the time, no reference should have been

made to it. A few days afterward, however, a tumor was observed occupying (as stated by Dr. Trowbridge, who visited the patient again with me) the site of the tumor present at his previous visits. The tumor was large and fluctuating, and after the lapse of a few days more, was opened by Prof. Hamilton, of this city, giving exit to a copious discharge of inodorous pus, which continued to discharge for several months. The abscess beneath the integument communicated freely with the pleural cavity. This case will be referred to again under another division of the subject.

From the history just given, it is suspected that in this case an abscess may have first formed exterior to the chest, which ulcerated in an inward direction, its contents being discharged between the pleural surfaces, giving rise to pleuritis and empyema.

Quantity of effusion. Excluding the cases which came under observation after the fluid effused within the pleural cavity had been nearly or quite removed by absorption, together with the cases in which the pleuritis was circumscribed, and the remaining number is *thirty-eight*. Of these thirty-eight cases, the quantity of fluid contained within the chest was relatively *moderate* in *four*. By the term moderate, I would be understood to mean that the chest was less than half filled with the fluid, as determined by physical exploration. The quantity was *considerable* in *eight*. That is to say, the chest was about half filled or somewhat more. The quantity was *large* in *sixteen*. In these cases the chest was full, or nearly so. The quantity *very large* in *ten*. In the latter class of cases the chest was not only filled, but its dimensions enlarged, the heart, in some instances in which the left side was the seat of disease, being pushed beneath, or beyond the right margin of the sternum, and in one instance in which the right side was affected, the liver being depressed below the false ribs.

So far as these observations go, they show that the effusion in general Chronic Pleurisy is never small in amount, but, as a general rule, it is rather large, or very large — a fact which gives to the diagnosis by means of physical signs in all instances simplicity and certainty.

SYMPTOMATOLOGY.

The record of the symptoms occurring during the career of the disease, in many of the histories, as already stated, is incomplete. In a large proportion, even of the cases which came under observation while the disease was progressing, the disease had already existed for a greater or less length of time. Moreover, the patients did not generally

remain constantly under my observation from the date of the first record. In several instances notes at a single examination only were obtained, and sometimes information of the farther progress, and the issue, is wanting, the cases being lost sight of. The number of complete histories is too few to furnish data for determining with precision the numerical ratio in which different symptoms are present or absent in this disease. Nevertheless, an analysis of the facts pertaining to symptomatology which the histories do contain, may be of some practical interest and value.

Pain. Most of the histories contain information respecting the amount of pain experienced at the commencement of the disease, and prior to the time that the cases came under my observation. If we exclude the cases in which acute inflammation preceded the chronic form of the disease, and those in which the disease supervened in consequence of perforation, it does not appear that in a single instance pain was severe. In the great majority of cases it was not sufficient in amount to render it prominent as a symptom; in several instances it was altogether wanting. When present it was generally confined to the access, or continued for a short time only after the attack; usually disappearing in the course of a few days. Its character was lancinating, described by patients as stitch-like, and in each instance in which its situation is specified, it was referred to the inferior part of the chest. With respect to the situation, it thus differs from the usual seat of the pains which accompany the ordinary subacute attacks of pleurisy occurring in the progress of tuberculosis. The latter are referred by patients to the upper part of the chest, very frequently beneath the scapula. Severity of pain, approximating to that degree which generally attends the early stage of acute pleurisy, evidently is rarely a symptom of the chronic variety, when the disease is subacute from the beginning; and its presence in ever so slight a degree, is by no means uniform. Without careful inquiry it would often escape the notice of the practitioner in obtaining the previous history of patients affected with Chronic Pleurisy, having been so slight as scarcely to attract attention at the time, or its occurrence been forgotten, and it is therefore mentioned by patients not of their own accord, but only after they have been requested to endeavor to recollect with regard to the point. This is stated in several of the histories in the present collection. Whenever cases come under observation after the disease has existed for several weeks, or months, the absence of pain is the rule. Occasionally a dull pain, or sense of uneasiness in the affected side is complained of; but, so far as my observations go, even these instances are exceptional. The practical bearing of these facts is too obvious to require remark.

Cough and expectoration. An examination of the facts noted with respect to these symptoms, leads to the following general conclusions: They are sometimes entirely absent, and are not, therefore, essential to the diagnosis. When present, they are generally not prominent symptoms. This, in fact, so far as these observations go, expresses the rule, the exceptions being very few. Cough and expectoration did not precede the development of the pleurisy except in the few cases in which it was inferred from the history that the patient, prior to the latter disease, was laboring under tuberculosis. The pre-existence of these symptoms, for a considerable length of time, should render the supposition of antecedent tuberculosis highly probable. Pleurisy occurring in patients not tuberculous, is oftener accompanied by cough without, than with an expectoration, or the latter is insignificant in amount. The cough is usually dry, and short or hacking. The expectoration, when present, usually consists of mucus, more or less modified. These facts show that Chronic Pleurisy is often uncomplicated with bronchitis, and if the latter affection coexists, it is of a mild grade.

A copious purulent expectoration sometimes occurs suddenly during the progress of pleurisy, and continues for a greater or less period. Under these circumstances ulceration of the pleura commencing on the free surface of the membrane, and perforation of the lung, opening a communication between the pleural sac and the bronchial tubes may be suspected. This happened in two cases in the present collection, in both instances the pleurisy being circumscribed; and it was supposed to occur in another case after the patient had passed from my observation. The two cases just referred to I shall give in full under another head.

In the cases in which tuberculosis appeared to be developed during the progress of, or subsequent to the pleurisy, cough and expectoration became more or less prominent. Prominence of these symptoms, when before they had been either absent or slight, provided they are not due to perforation, should give rise to strong suspicion of tubercle. This practical consideration is the more important, inasmuch as the physical signs of tubercle are rendered less available by the existence of pleurisy with effusion, and also by the permanent effects of the latter upon the chest.

Respiration. Excluding the cases of perforation, the frequency of the respirations was almost uniformly somewhat increased. To this rule, however, there are exceptions. In one of the cases, while the quantity of effusion was very large, removing the heart to the right side of the sternum, the respirations were but sixteen per minute. The increased frequency was not usually great, varying from twenty-five to thirty-five per minute. In one case

the respirations were forty per minute. I should remark that the enumeration per minute is noted in but a small proportion of the cases, descriptive expressions being used in the remainder.

The respirations were uniformly increased in frequency by exercise. This was more prominent, as a symptom, than the amount of acceleration while the patients were tranquil. In several instances the chief source of complaint by the patients was, that they were short-winded on walking, or any kind of physical exertion. They would sometimes declare that they were quite well, and able to labor except for this difficulty. The want of breath was also apparent in conversation. The patients were obliged to stop frequently in the midst of sentences to inspire.

Dyspnoea, or a painful sense of breathlessness, does not appear to have been present in any of the cases, while the patients were at rest. In cases in which the chest was filled with liquid effusion, the respirations rendered short and frequent by slight exercise, and imperfect oxygenation of the blood denoted by lividity of the prolabia, and other parts, there was not a marked degree of suffering from dyspnoea. In one of the cases only, is it noted that this symptom was in any measure prominent. This was a case of empyema in which the purulent contents of the chest having made their way through the thoracic walls were evacuated externally. In this case, for a time, the dyspnoea was considerable, causing the patient to preserve the semi-erect position. It was, however, relieved before the discharge of the contents of the chest took place. It is possible that dyspnoea may have occurred in other of the cases while they were not under my observation; but it may be safely stated that they show the absence, or non-prominence of this symptom to be the rule in Chronic Pleurisy.

Circulation. The pulse was found to be more or less accelerated, in the great majority of the cases, during the progress of the disease. The degree of acceleration differed considerably in different cases, ranging usually from 80 to 120, but in one case reaching 140 and in another 160 per minute. In a few instances, at the time the symptoms were noted, the pulse preserved its normal frequency, and in one instance was below the average standard, being sixty-four per minute. As respects other characters, in all the histories containing information on this point, the pulse was small, compressible, and sometimes quite feeble. It would be highly interesting to study the pulse, as well as other symptoms, in the different classes of the cases of Chronic Pleurisy separately: for examples, in the cases characterized by the coexistence of tubercle, and in which this complication was absent; in cases in which the liquid effusion became purulent, constituting empyema, etc. The

present data, however, are inadequate for the prosecution of these inquiries to an extent sufficient to render them of much value.

Lividity of the prolabia is noted in three cases, all of which proved fatal, and in two the disease was complicated with pericarditis.

In the history of one case in which marked capillary congestion of the surface of both extremities was observed, the left upper extremity presented this appearance to a greater extent than the right, and the temperature was sensibly less. In this case the left side was the side affected, and the amount of effusion was large.

Skin. Sweating is an event occurring pretty often in this disease. The sweating is frequently profuse, and is most apt to occur at night. The sweating is by no means uniformly preceded by a febrile paroxysm, or exacerbation. I think it probable that in but few, if any of the instances, was well marked hectic present, but on this point the histories are defective. To be precise with respect to sweating, it is noted, as a prominent symptom, in eleven of the cases. Of the remaining cases it is noted to have not been present in a few, but in most of the cases nothing is stated relative to it.

Aside from sweating, the skin is stated to have been cool, or cool and moist in several of the cases, but in a large proportion the condition of the surface is not noted.

Chills. In a small proportion of cases a chill, or chills, or chilly sensations are noted to have occurred. - Here, as with respect to other symptoms my data do not authorize the deduction of an exact numerical ratio. Chills may have been present in some cases after the periods in the progress of the disease to which my records extend, or they may have escaped attention in recording the previous history; in the latter instance they would not have constituted a very prominent symptom. In the cases in which chills are noted to have occurred, they do not appear to be uniformly associated with other events investing them with any special significance. They occurred in cases in which the progress and issue of the disease showed that the liquid effused within the chest did not become purulent; and also in cases in which there were no evidences of coexisting tuberculosis. In one case the recurrence of chills for several successive days preceded a sudden moderate purulent expectoration which continued but for a short time. This patient recovered, and has, for several years, been in good health. In two cases, the chills were so prominent in the early part of the disease that the patient was supposed to be laboring under intermittent fever.

Digestive System. Anorexia, or impaired appetite, enter into the history of many of the cases, but it is remarkable how small was the disturbance of

the digestive system in several instances. The appetite was tolerable, and sometimes excellent, and the digestion good, during the whole progress of the disease. In several of the histories it is noted that the tongue was perfectly clean. A few cases were characterized by the occurrence of diarrhoea; not colliquative, but apparently exerting a salutary effect, being followed by diminution in the amount of the liquid effusion within the chest.

Aspect and nutrition. The countenance presented, in some cases, marked pallor, but in other cases the aspect was not notably morbid, and, in some instances, while the quantity of liquid effusion within the chest was considerable, the general appearance denoted good health.

Progressive emaciation characterized the cases ending fatally, but of those in which recovery took place, the loss of weight was not always great, and in several instances considerable embonpoint was preserved.

Strength. The muscular strength was retained in a remarkable degree by some of the cases terminating fatally, the patients being able to sit up until within a short time before death. In one case, particularly, this preservation of strength was exhibited in a striking degree. The patient was considerably emaciated, there were anorexia, short and labored respiration, incessant hacking cough, coldness of extremities and oedema, lividity at the roots of the nails, frequent and intermitting pulse — these symptoms preceding dissolution — but he was still able to sit up, and insisted on being dressed. In several cases in which the quantity of liquid effusion was large, filling the chest, the patients were not confined to the bed at any time during the progress of the disease; but under these circumstances, in some instances, they were able to be out of doors, taking exercise and even performing a considerable amount of labor. For example, in the case of a patient who applied to me six weeks after the date of attack, having had no medical advice up to that time, the right chest was completely filled with fluid. He had, however, not kept the bed even for a single day, but had continued steadily in his occupation, embracing wood-sawing, taking care of a horse, and the varied duties of a house servant. He rapidly recovered after ceasing labor, being at no time prevented from going out of doors, and walking always a mile to my residence for medical advice.

As another illustration, a lad, aged ten years, had labored under the disease for two months before the diagnosis was made. During that time he had never been kept at home, but had daily been accustomed to play out of doors. He was allowed to continue his out-door sports, and he progressively improved and recovered, complaining only of being put out of breath on

active exercise. At the time of the examination, two months after the date of the attack, the left chest was completely filled with liquid effusion.

Urine. In a few cases in which the condition of the urine was ascertained, it was uniformly scanty and high colored. The information does not extend beyond the qualities just mentioned.

Retention of urine, requiring the catheter for several days, was a symptom in one case.

Œdema. Œdema of the lower extremities is noted in the histories of three cases, and in one of these cases the face was also œdematous. Two of the cases terminated fatally.

Hæmoptysis. This symptom was present in two cases. In one of the cases, it preceded, together with slight cough, the development of the pleurisy. The history renders it altogether probable that tubercle coexisted in this case. In the other case the hæmoptysis occurred twice in the early part of the disease. The patient, however, progressed favorably, and three years afterward, was in good health, being free from any pulmonary symptoms, except want of breath for active exercise. A few months afterward he had a third attack of hæmorrhage, which was quite copious. This was not followed by any new pulmonary symptoms, and at the present time, about four months after the last attack of hæmoptysis, he is actively engaged in business, is free from cough, and considers himself quite well. It is, however, by no means certain that the lungs are free from tubercle in the latter case.

PHYSICAL SIGNS.

The physical signs, in most of the cases, were determined only so far as was required to establish the diagnosi. They were not recorded, more than the symptoms, with a view to numerical analyses. I shall consequently give, in this division of the subject, as in that devoted to the symptomatology, the practical results of an examination of the facts contained in the histories, without aiming to ascertain the relations which the signs individually bear to each other, and the disease, as respects the ratio of their occurrence. In the following account I will exclude reference to the cases of perforation, of circumscribed pleurisy, and those in which the disease had existed at a period more or less removed from the time the cases came under my observation. These three groups of cases will presently be considered under distinct heads, and attention will therefore now be directed only to the cases of ordinary chronic, general pleurisy, during the progress of the disease.

Percussion. The signs developed by percussion, were quite uniform, viz., flatness extending from the bottom of the chest upward, either entirely, or to a greater or less extent over the side affected; resonance clear, apparently exaggerated, over the healthy side, and in cases in which the flatness was not universal over the affected side, relative dullness of resonance above the upper boundary of the flatness, compared with the corresponding portions of the healthy side.

The few deviations from, or additions to the above rule of the percussion signs, which are noted, are as follows:

In one case the resonance was tympanitic over the lower third of the left side, which was the side affected, a tympanitic resonance also being present over the epigastrium. This was undoubtedly due to flatulent distension of the stomach.

The flatness in two cases was observed to extend from the side affected, beyond the sternum, over a small portion of the healthy side. In both cases the quantity of liquid effusion was very large, distending the chest in every direction, and pushing laterally the mediastinum so as to encroach on the opposite half of the chest.

In two cases the resonance on the affected side above the line of flatness, was exaggerated, the clearness being relatively greater, in a marked degree, than on the healthy side. In one of these cases it is noted that the clearness of resonance was apparent over the upper third anteriorly and posteriorly. The chest on that side was enlarged, intercostal depressions lost, motions in respiration limited, and there was absence of all respiratory sound over the inferior and middle thirds. The respiration over the upper third was feeble, the vesicular quality absent, and the expiration prolonged. The greater relative clearness of resonance, at the summit of the chest on the left side, continued, and after the lapse of a month, the marked contraction of the affected side having taken place, the same disparity between the two sides was noted.

In the other case the infra-clavicular region of the affected side was bulging, and in this region the resonance was clearer than in the corresponding region on the healthy side. The respiration was relatively feeble; its qualities not being described. Flatness on the affected side existed below the second rib. The side (which was the left) was visibly enlarged, measuring one-fourth of an inch more than the right. In one case there was flatness over the left side *except posteriorly at the inferior portion of the chest below the angle of the scapula*, tubular respiration was heard over the situation just mentioned. The left side was comparatively immovable. The heart

was carried upward and to the right, the point of impulse being perceptible between the nipple and left margin of the sternum.

Tenderness apparent on percussion, is noted in several of the histories.

It does not appear that a change in the level of the liquid contents of the chest, in different positions of the patient, was observed in any instance, but I am not certain that pains were taken, generally, to examine for this sign, which is frequently found in cases of acute pleurisy and of hydro-thorax. The great amount of effusion in a large proportion of cases of Chronic Pleurisy renders it often unavailable.

Auscultation. It will suffice with respect to this, as well as the other classes of physical signs, to notice any peculiarities differing from the ordinary rule in this disease. The rule of auscultatory signs in Chronic Pleurisy, is as follows: Absence of all respiratory sound on the affected side from the bottom of the chest upward, as far as the flatness on percussion extends; respiration above this line on the affected side more or less developed, and tubular and bronchial in character, with bronchophony in some cases; occasionally egophony; on the healthy side the respiratory sound normal in character, and increased in intensity, constituting the respiration known as exaggerated, supplementary or puerile.

On examination of the facts contained in the histories, after writing the foregoing paragraph, I find but a single case in which any anomalous deviation from the usual signs is noted. In this case a faint and apparently distant bronchial respiration was heard generally over the chest on the affected side. There was flatness over this side except at the summit. The side was visibly enlarged, but no marked disparity in mobility. This was the case, already referred to, in which there was bulging of the infra clavicular region on the affected side, and a greater amount of resonance in that situation, than in the same region on the healthy side.

A friction sound was heard in three cases. In one of these cases it existed early in the disease before much liquid effusion had taken place. In another it was noticed by the patient himself, after the disease had existed for some time, and he was able to be about, having been for some time confined within doors. In the remaining case it existed during the progress of the disease, while the quantity of effusion was considerable. These were all the cases in which this sign was discovered, but it was not always carefully sought for, at different stages of the disease, in the cases individually.

Egophony was not often sought for. It was not distinctly noticed in any instance, but in one case there was an approach to it, the voice having a egophanous modification.

Bronchophony was observed in several of the cases at the summit of the chest on the affected side.

Adventitious sounds of respiration, or râles, are not noted to have been present in but two cases. In one of these cases sibilant and sonorous râles were heard at the summit of the chest on both sides; and in the other case crackling in the same situation. In both cases the history of the case showed the coexistence of tubercle.

Inspection. Visible enlargement of the chest on the affected side was a well marked sign in several cases. Permanent expansion of the affected side, rendering it comparatively immovable in respiration, was usually observed in the cases in which the amount of liquid effusion was large. The intercostal spaces, except in some instances in which the quantity of adipose tissue prevented depressions from being apparent, were elevated to the level of the ribs, and in some cases projected beyond this level. The contrast between the two sides in this respect, was frequently striking.

After the liquid effusion was absorbed, or considerably lessened, contraction of the chest was apparent in all the cases which remained under observation. The diminished semi-circumference on the affected side was frequently obvious; but the change was more marked in the depression of the shoulder, and generally, but not invariably, in the diminution of the interscapular space.

Oedema of the affected side is noted to have existed in some cases, and in other cases its absence is stated, but a large proportion of the histories contain nothing relative to this point. The same is true of enlargement of the subcutaneous views on the affected side.

Mensuration. Measurement showed, in several cases, the affected side to be distended while the amount of liquid contents was large; and afterward contraction to less than the normal size. The greatest increase in any of the cases in which measurement was practiced, was two inches. The greatest degree of contraction noted is three-fourths of an inch, but mensuration was not resorted to, after absorption was completed, in some instances in which the contraction was probably greater than this.

Palpation. The absence on the affected side of a vocal fremitus, appreciable on the healthy side, is noted in several of the histories. In two cases a sense of fluctuation was perceived in the intercostal spaces.

Displacement of the Heart. In the cases in which the disease was seated in the left side, and the quantity of liquid effusion large, the heart was displaced, to a greater or less extent, from its normal situation. This was shown, in some instances, by the impulse not being felt at any point, the

organ being too far removed from the walls of the chest, to be brought into collision by the systolic contraction. Under these circumstances the cardiac sounds were heard over a considerable space, but usually rather feeble and distant. This rule is not without exceptions. In one instance the sounds of the heart were heard with considerable intensity over the chest, and a shock communicated to the walls of the affected side, so that the attending physician, who was but little acquainted with physical exploration, supposed the case to be one of disease of the heart.

The displacement, in other instances, was rendered more obvious by the impulse being appreciable at a greater or less distance from the point where it is felt in health. In five cases the impulse was felt or seen beyond the right margin of the sternum. The quantity of fluid, in these cases, was sufficient to push the heart and mediastinum laterally, causing them to occupy a portion of the space belonging to the right side of the chest. The heart was removed in an upward as well a lateral direction in these cases, the impulse being apparent in the right mammary region in one case, and between the third and fourth ribs in another case. In the instances in which the displacement was not so great, the impulse was perceived at different points in a line running directly in an upward and lateral direction to the right. As the liquid contents of the chest diminished by absorption, the heart receded along the same line in a direction toward its normal position.

In the cases in which the right side was the seat of the disease, displacement of the heart is noted in but one instance. Pains were, perhaps, not generally taken to ascertain whether the relations of the organ were disturbed, as I find nothing relative to this point in the histories, save in the single instance referred to. In that instance the patient came under observation four months after the date of attack. The right chest then presented marked contraction, and diminished mobility. There was flatness on the right side below the nipple, and absence of respiratory sound. The impulse of the heart was found on a level with the left nipple and between the sternum. This displacement was probably due to the absorption of the fluid effused into the right chest, a result of Chronic Pleurisy on that side which was first noticed by Dr. Stokes, of Dublin.*

* This Report will be continued.

ART. II. — *Malignant Dysentery.* By EDWIN R. MAXSON, M. D., Adams Centre, Jefferson Co., N. Y.

About the first of August, 1851, a dysentery, of a malignant character, made its appearance in our village and vicinity. The symptoms of which were as follows: The incipient stage was characterized by occasional nausea, and slight griping pains in the bowels; which continued from six to twenty-four hours. This was followed by a chill, more or less severe, with dysenteric discharges. The cold stage continued from one to twelve hours, during which the prostration was very considerable.

Then came the febrile reaction, which, in most cases, was very marked, and continued from seven to nine days; after which came fatal prostration, or gradual abatement of all the alarming symptoms, and slow convalescence.

During the twelve weeks it continued, I saw and treated about one hundred cases, more or less severe, from which I drew the following conclusions as to treatment. These conclusions have been confirmed by the result of numerous cases, of a similar character, occurring in the same locality this year. Cathartics, in every stage of the disease, were decidedly injurious, rendering mild cases desperate, and malignant ones fatal.

Of the five cases which I lost in 1851, three had taken either pills, salts, or oil, before I was called; and the other two, I gave small doses of Rhei, and Hydg. cum creta. Small doses of calomel, or even Hydg. cum creta, could not be borne without injury.

During the incipient stage, Tr. opii, gtt. xv, saturated with camph., given every three hours, did most toward arresting the disease.

After the chill, and during the cold stage, the same doses, with a teaspoon full of brandy, and sinapisms to the bowels, appeared to do best.

When the cold stage was succeeded by febrile reaction, with continued griping and frequent dysenteric discharges, I found the following to do well:

℞ Pulv. Doveri. gr. v.
Plumbi Acetas., gr. i.

Given every four hours.

After the febrile stage subsided, and the sweating commenced, there was generally considerable prostration; I then gave

℞ Pulv. Doveri, gr. v.
 “ Tannin, gr. ii.
 “ Camph. gr. i.

Every four or six hours.

All my cases were not treated precisely according to the preceding plan. But that was the general course, and the one which was the most successful.

Some of the cases were congestive; in these I gave, in addition, quinine. Others were of a typhoid character, some of which I blistered over the stomach and bowels, with very good effect.

Several circumstances, during the prevalence of the disease, suggested to me the possibility of its being somewhat contagious. A lady came from Watertown to see a friend, sick with the disease, and staid several days; after returning to her home, in the village of Watertown, where no disease of the kind was prevailing, she came down with it, became typhoid, and died.

Another lady, who was teaching in our vicinity, was exposed, and left for her home, thirty miles distant, in a healthy locality where no disease of the kind was prevailing; she too fell sick with the disease, and died. And then two of her sisters, who were living at home, and had not been otherwise exposed, took the disease and died.

It is common for us to have mild dysentery in August and September, and some bilious fever. But I know of no local cause that might be supposed to have produced this disease, except that very much vegetable matter was thrown up and exposed to the air, in a swamp a little to the west of us, in grading for the Watertown and Rome railroad during the spring and summer of 1841.

ADAMS CENTRE, N. Y., Sept. 24, 1852.

ART. III. — *Case of Empyema, in which Paracentesis was practiced.*

Reported by J. H. BEECH, M. D., Coldwater, Mich.

Believing that the following case of paracentesis thoracis possesses some interest to the profession in the large amount of fluid withdrawn in proportion to the size of the patient; and that it affords much encouragement to undertake relief in forbidding cases, I am induced to offer the main facts, holding the full report of the treatment, &c., at the service of any who may take farther interest in the subject.

Wm., Jr., son of Mr. Wm. Fillison, had rubeola simplex, in February or March last. The father states that "the boy did not do well," but he could not learn from his physician that there was any complication to the measles.

Saw the patient first, May 28th, 1852. Was five years old yesterday. Much emaciated. Pulse 148, sharp and irregular. Respiration 37. Panting. Inspiration most difficult. Extremely restless. Prefers an upright position. Bowels costive. Tongue covered with thick yellow fur. Frequent hacking cough. Right shoulder higher than the left. This side of thorax much elongated; and edge of liver felt one and a half inches below the cartilages of the ribs. Whole of right thorax dull on percussion. Respiratory murmur faintly heard in upper part. *Cæ*gophony at right axillary, and infra-scapular region, when patient reclined toward left side, but not when perfectly erect. Tenderness over the infra-axillary region. Subcutaneous veins show much plainer on right than on left side. Skin hot and dry. Appetite very poor.

It seemed advisable to change the constitutional symptoms before risking an operation, and the result of medication was:

May 30, 10 o'clock, A. M. Pulse 138. Expectoration freely. Skin moist. Tongue clean at tip and edge, thin, moist, light-colored fur in middle. Appetite better. Physical signs as before. Introduced a small trocar and canula at infra-axillary region, above eighth rib, and drew with a pump fl. ζ xx. of sero-purulent fluid, when the patient complained of pain in his "side;" evinced nausea; and the pulse quickened. The canula was immediately removed, although the pleura was not quite empty.

Nine o'clock, P. M. Pulse 126, soft and full. Respiration 28. Has slept some. Tongue moist and cleaner. Sweats freely, but not excessively. Coughs less. Liver not felt below cartilage of ribs. Appetite good. Bowels have moved gently.

31st. Rested well during night. Complains of soreness of right side. No redness about the puncture. Slight excess of rotundity of right side. Crepitant r le throughout right thorax. Pulse 132.

June 4th. More cough, but still improving.

June 6th. Patient still improving in general appearance, but right thorax nearly as full as before. Introduced instrument near former puncture, and drew all the fluid which could be obtained by moving the canula about, and changing the position of the patient. It was greenish pus, too thick to have flowed through the canula without the pump. The last portions were tinged with blood. Patient did not complain after the trocar was introduced, and objected to lying down after the operation. Convalescence was soon fairly

established, and after a few days we saw no more of him until about the middle of September, when he was hastily examined, by auscultation and percussion. No abnormal sound, or sign, was detected. He has a robust appearance.

In the above operations about six inches of flexible tube was interposed between the pump and canula, after Doct. Bowditch's plan, which effectually prevented any jerking upon the patient. The last amount of fluid drawn was fl. ℥xxii, making in all fl. ℥xlii, seven days intervening between the operations.

COLDWATER, Mich., Sept. 8, 1852.

ART. IV. — *Case of Carbuncle.* Reported by CLINTON COLEGROVE, M. D.

April 18, 1852. Was called to see Mr. R. T., aged 71, temperament sanguine, and previous health good. Had, last February, wandering pains in his back and neck, followed by constant and excessive itching. On the 3d instant, a slight swelling was noticed along the back of the neck, about the junction of the cervical and dorsal vertebræ, which was accompanied by great pain and heat, and which rapidly became diffused over a large extent of surface measuring from eight to twelve inches in diameter, and reaching nearly to the head, purple and projecting outward from the base nearly four inches.

We noticed numerous small orifices which covered the face of the tumor, from which oozes a thin, purulent, sanguineous matter. The tumor is quite hard and seems to be of dense structure, communicating an impression to the finger like that of dry sponge.

Of the treatment heretofore I know nothing, it having been conducted by a *Root Doctor*.

We made a conical incision into the tumor, each cut being about two and a half inches in length, and applied a charcoal poultice, with liberal internal use of opium and Port wine.

April 29. Found our patient restless, particularly nights. Pulse 100 and feeble. Applied the knife more deeply in the line of the original incisions, which bled freely. The subcutaneous cellular tissue is cut with difficulty, and seems to have acquired a kind of cartilaginous hardness. The treatment consists in the application of a wine poultice. Internally free administration of carb. ammonia and opium — of the former, ℥jss, of the latter, gr. vi, dissolved in an ounce and a half of water. Dose, ℥i. every fourth hour.

May 9. Patient decidedly worse. The edges of the incisions made at

the last visits, are gangrenous, and the whole aspect of the tumor is livid. Large portions of the cellular tissue are sloughing, accompanied by a fœtid purulent discharge. Both feet, left hand and arm œdematous. Pulse 120, very feeble. Respiration somewhat hurried and laborious. General appearance languid and sunken. Has sharp lancinating pains extending through the tumor and upper part of the chest. Prescribed, opium grs. ii, every eighth hour. Port wine freely, and a solution of cream of tartar for a drink.

May 12. Very extensive sloughing of the cellular membrane, and excessive œdema of the feet, left hand and arm, with slightly erysipelatous condition of the skin. Applied a broad bandage to the feet and ankles, to be kept wet with cold water. Above treatment continued.

May 15. The sloughing has continued rapidly and obstinately. The bandages gave pain and were removed.

May 18. Patient seems on the whole to be better. The sloughing has continued so that the introduction of a probe under the edges discloses a large sulcus, two inches in diameter, around the whole circumference of the ulcer.

We now commenced the administration of tonic medicines, as large doses of quinine, &c. Also some diuretics.

May 23. Patient better. Sloughing has entirely discontinued.

May 29. Patient improving. The integument is slightly attached to the surface underneath. Adhesive straps were applied with a view to accelerate an approximation of the lips of the ulcer.

Aug. 15. Called on my old patient to-day, and am gratified to find him in excellent spirits and health, with the single exception of the sore upon his back, which, under a continuation of the treatment last adopted, has become smaller and smaller, and is now about one inch in diameter, with every prospect that it will soon be perfectly healed.

ART. V. — *Case of Prostatic Abscess.* Reported by Prof. JAMES P. WHITE, Buffalo, N. Y.

Below you will find a brief account of a case of acute inflammation terminating in abscess of the prostate gland, with the course of treatment pursued. It interested me by reason of the ambiguity of the symptoms and its rarity during its progress; and may not be entirely without interest to some of the readers of your valuable Journal.

On the 7th of September last, visited for the first time Mr. B., a gentleman, forty-eight years of age, of sedentary habits, but of general good health. He had suffered from irritation of the urethra or neck of the bladder, during the last two years, and had been treated by a surgeon in New York, for stricture. He had been supplied with bougies, of graduated sizes, and directed to insert them occasionally to prevent contraction. During this period he had passed a large bougie every fifteen or twenty days without difficulty, but it was always attended with great irritation and pain throughout the entire canal. The insertion of the instrument gave rise to considerable pain in the glans penis also. He had introduced one of these large bougies quite to the bladder, about a week before I saw him, which gave him more than the ordinary amount of pain at the time, and was succeeded by an unusual degree of suffering during micturition. Subsequently he had, however, traveled several hundred miles by railroad, and indulged more freely than usual in stimulating food and drinks. He had also suffered frequently from the descent of hæmorrhoidal tumors, one of which, as large as an almond, was now in an inflamed condition, attached to the posterior margin of the anus. The pulse was considerably accelerated; there was a general febrile movement with frequent micturition and tenesmus. The passage of the urine occasioned an acute burning sensation in the urethra, although external pressure did not produce pain. The urine was nearly natural in appearance, there being only a slight mucous deposit after standing.

Directed the patient to preserve the horizontal posture, applied cold to the hæmorrhoidal tumor, giving him freely of Dover's powder and morphine. He also took a teaspoon, three or four times each day, of a mixture of spirits of nitre and the muriated tinct. of iron. Under this treatment the fever subsided, and the acuteness of the pain was somewhat relieved for a day or two. On the 11th, however, the pains were increased in severity, and the patient found it exceedingly difficult to locate them. The whole perimeal region seemed to participate in the painful sensations, though pressure upon this part did not increase it. These pains were intermitting, increased in the after part of the day, and accompanied by a sense of weight in the pelvis. Apprehensive that the symptoms were of too grave a character to be entirely accounted for by the presence of piles, and doubting the existence of any considerable contraction of the urethra, a rectal examination was insisted upon. The finger being carried well up into the rectum, immediately came in contact with a hard tumor upon the right side of the neck of the bladder. This was, doubtless, the right lobe of the prostate gland. It was exceedingly sensitive to the touch, and as large as a Madeira-nut.

Continued the horizontal position, applied a dozen leeches to the perineum, and followed with warm anodyne fomentations. Gave also laudanum or morphine, by enema, in such quantities as to moderate the sufferings of the patient. The bowels were moved every second day with castor oil, and, it should be added, that defecation was attended with severe pain.

On the following day Dr. Austin Flint saw the patient, in consultation with myself, and concurred in the diagnosis and in the treatment which was being pursued. Continued the discutient treatment until it became evident by a repetition of the rectal examination that suppuration had commenced.

On the 17th, in order to verify the diagnosis, in the presence of Prof. Flint, carried an exploring canula and needle up beside the finger, and pushed it into the soft tumor, upon which the finger rested. A few drops of well-digested pus immediately followed the withdrawal of the stilet, and demonstrated the nature of its contents. The tenseness of the tumor was diminished and the pain became less severe, after this slight discharge of matter.

On the following day, September 18th, after evacuating the bowels, a small, long trochar was carried up upon the finger, and after placing it in contact with the sac, the stilet was thrust through its parieties, when it was immediately withdrawn and the canula pushed forward into its cavity. There now escaped something less than a gill of pure pus through the instrument, when a syringe was nicely fitted to the outer extremity, and suction applied, in order to effect its complete evacuation. The soft tumor immediately collapsed, and great relief to all the urgent symptoms followed. The bowels were left unmoved for several days, in order, if possible, to permit adhesion between the parieties of the abscess, and prevent the continued formation of pus and a fistulous opening into the rectum. The patient improved rapidly without the discharge of any more purulent matter, and an examination one month after the operation, enables me to detect the prostate somewhat enlarged, but not painful on pressure, and no trace of the orifice through which the pus was withdrawn to be detected. He has resumed his ordinary pursuits and has less pain and fewer of the symptoms which he attributed to stricture, than have been present at any time during the last two years. It may be added that he is, however, under treatment for the discussion of the chronic or senile enlargement which I believe gave rise to the symptoms attributed to stricture, and which, I doubt not, is diminished by the suppuration which has just terminated.

This acute form of disease in the prostate is rare, and, so far as my reading extends, the course to be pursued when matter is formed, not well established. I am not aware that the method pursued in this instance has ever before

been resorted to. Most standard works on surgery omit to mention the existence of this affection. Civiale and Gross, the best authorities in the French and English languages, recommend that, when detected, exit should be given to the purulent collection through the perineum, by puncture. Now this would be difficult to accomplish, on account of the amount of tissue to be divided, and would almost certainly end in fistula. Many authors insist that abscess of the prostate shall be left to rupture spontaneously. It is at the same time conceded that rupture into the peritoneal cavity would be certainly fatal; and that a connection (which is most likely to be established) between the abscess and the bladder would be scarcely less to be dreaded. Could we be sure that the matter would be discharged through the perineum, the demand for surgical interference would be less imperative. But the distance of the gland from this surface and the denseness of the tissues through which the matter must pass, render this result highly improbable. Again, should the abscess open into the rectum, the only other probable outlet, if left to rupture itself, it would unquestionably leave a large open sac to be filled with feculent matter, and result in a very bad form of fistulous connection. In view of all the circumstances it would seem very desirable that the fluid should be artificially drawn off at as early a period as practicable after its discovery, and prevent, if possible, any of the fearful consequences so likely to result from spontaneous rupture.

October, 1852.

ART. VI. — *On the arrest of Typhus Fever by Quinine.* By Dr. ROBERT DUNDAS, Physician to the Northern Hospital, Liverpool. A review thereof by SCHRAPENKUTTEL SMELFUNGUS, M. D.

Doctor Smelfungus has a long nose wherewith he "scents the battle afar off." He not only scents the battle, but his olfactories dilate readily to malaria and marsh miasm. Surmounting this nose of Dr. Smelfungus, is a head, said by some to be not so long as his nose. But that head, such as it is, has been lately much pothered by the question of the existence, or non-existence, of miasmatic emanations as a blood poison.

Dr. Flint intimates that it is an open question. Dr. Robert Dundas, of the Northern Hospital, Liverpool, Eng., says, point blank, that "ague and remittent fever do not originate in malaria, or marsh miasm."

Dr. Hamilton, *et multi alii*, says, that malaria does cause, not only intermittent and zymotic diseases, but also Asiatic cholera.

Dr. Smelfungus says, with an emphasis, "Amen," and fixing his eyes upon his little collection of medical authorities, (his best friends and solace of many an hour otherwise lonely,) declaims:

"If we are to throw away this time-honored doctrine of malarial poison, with it must go all measures of sanitary reform. We may with impunity build our dwellings on the easterly side of miasmatic swamps—a cottage by the border of the stagnant Bayou is as healthful as a home upon the breezy hillside. Dickens has written in vain his wonderful descriptions of the fever haunts of London, and 'Tom-all-alone's' may remain in chancery till the crack of doom without detriment to the public health—all notions of ventilation, and drainage, and cleanliness are obsolete—we may erect our temples of Cloacina beneath our bedroom windows, and inhale the odors of their vaults through the long summer nights, as we would 'the scent of orange bowers'—we may wallow in all manner of uncleanness, and revel in the filth of Asiatic cities, without the danger of their plagues."

"Now," says Dr. Smelfungus, growing milder, "I am extremely anxious that Dr. Dundas should make good his second proposition, which I find in the last number of 'Braithwaite,' viz., that intermittent, remittent, and continued fevers are but different varieties of the same disease, and all equally curable by the same means, viz., *quinine*, (glorious old *protege* of the Society of Jesus,) in doses large enough, viz., 10 grs. every two hours, continued for some thirty-six hours. I have faith in that latter idea. I have long been regardless of the existence of fever, when about to give a *large* dose of that drug. I know *quinine*, and there is good sense, and philosophy in supposing that given in these doses it would do what Dr. Pitcaim, and Southwood Smith, and Tweedie, and all the rest of the 'old fogies' said was impossible, *id est*, cut short a typhus fever. But," (growing warm again) "why in the name of all that is rational, should he block the way to trial of his mode of cure by connecting with it this *nouveau né* hobby of no malaria? Dr. Dundas! 'I know a vale,' where let me place you for a few hours at the rising of the sun and the going down of the same—you may take a nap if you please—all the better—and you shall be nicely covered up with a little fog blanket, which smells as if it had been slept in before—but you shall come out of that valley, with such evidence of the existence of malaria, as shall make you *shake* in your shoes!"

Dr. Smelfungus grows contemplative, and with a long drawn sigh of pity for human frailty¹ and short-sightedness, takes a chew of Nicotiana Tabacum. It is not his first, for already upon the floor is a miniature diagram of the whole chain of lakes. This fixes his eye, and he gently comments on the

different localities — “There is Chicago, and Racine, and Milwaukee, all having the four essentials of malaria, air, moisture, heat, and vegetable decomposition, all fever localities. And there is Mackinaw — high, breezy Mackinaw, where people have to move away to die; and there are Toledo, and Sandusky, and the deadly Maumee valley, where the arrival of the cholera predisposition was but the touching of the torch to the full magazine. There, too, is Cleveland, on its noble bluff, with sandy soil, and open parks, and freedom from disease. There, again, is Buffalo — 326 deaths from cholera there this healthy year, among those canals and mud-puddles, while the south-west wind sweeps up from Hamburg swamp with death upon its wings.

Finally, there is Rochester — city of Universities, and Athenæums, and Eclectic Medical Colleges! with its truculent board of health, which valorously tears down all nostrum handbills, and returns to a prominent physician his respectful communication, wherein he tells them that right in the heart of their city — in the dry bed of their river, is the source, and fount of that vial of wrath which has been poured out without measure on their devoted town.”

Dr. Smelfungus is satisfied with himself. He takes another chew, and thus doth cogitate: “Why is it that Dr. Dundas doubts, or denies the old theory of malarial poison? He has lived in Brazil — he has seen tropical diseases. He says that continued fevers do occur in hot climates, and that they not uncommonly mutate to the remittent form. Well, what of it? The old idea of a special animal poison, in the causation of continued fever, is tolerably well exploded. If all these forms of fever are the same, (and for aught I care you may add yellow fever to the list,) why may not they have the same cause, and where is the wonder in their changing forms?

“Remittent fevers assume a typhoid type. Dr. Fenner says that bilious remittent in the great heats, and close air of a city, may become yellow fever. Why, then, may not, under favorable circumstances, a continued fever become remittent, and if it does, what bearing has this on the great question whether marsh miasm causes intermittent and remittent. Marsh miasm is an unfortunate term. Dr. Caldwell told us twenty years ago, that a marsh was not necessary to the production of malaria, and proved it, too, in a certain prize essay. But malaria — is it not possible that Dr. Dundas has been wool gathering among researches as to what malaria is, till disappointed at its intangible nature, he has grown to deny its very existence. If so he has grasped at a fog, and found nothing but a shut and empty hand. Laucisi long ago upset the old animalcular theory, which supposed that myriads of little monads issued from the marshes to insinuate themselves with disgusting

familiarity into our mouths, and noses, and our very pores. But he did not tell us what the thing is. Others have tried to do so, and sulphuretted hydrogen, and carburetted hydrogen, and phosphoretted hydrogen, nitrous acid gas, and every other gas, and conceivable combination of gases, have had their earnest advocates, but in vain.

“Doctor Smelfungus opines, that it is (in very original phrase) a subtle essence, which, generated by the action of solar rays upon decomposing vegetable matter in a moist state, produces our ordinary autumnal fevers—that when acted upon by *long continued tropical heat*, it causes the deadly congestive and yellow fevers, but that when produced by artificial heat, in winter, it (conjoined with such other causes as unite in a crowded ship load of emigrants) gives birth to the continued forms of fever. Not only this—it may, under favorable predisposing causes, produce dysentery, diarrhoea, and cholera.

“Furthermore, Dr. Smelfungus saith not, save this, that his theory above announced is a very convenient one, and harmonizes all discrepancies. If you ask *why* quinine cures continued fever, he tells you because it cures remittent; if you ask why it cures remittent, he cunningly shutteth one eye, and propounds the following *hard question*—why does a chimera, ruminating in a vacuum, devour second intentions?”

Dr. Smelfungus looks at his bulls-eye watch, and finds himself among “the wee sma’ hours ayont the twal’.” He turns with a sigh to his bachelor bed, and breathes a prayer for the speedy coming of that day, when a Mrs. Doctor Schrapenkuttel Smelfungus shall be a blessing to his basket and his store, and a warming-pan to his couch o’ these chill nights. *Festina diem!*

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

A Memoir on the Pathology and Treatment of Leucorrhœa, based upon the Microscopical Anatomy of the Os and Cervix Uteri. Read before the Royal Medical and Chirurgical Society. By W. TYLER SMITH, M. D., Physician-Accoucheur to St. Mary's Hospital.

The author first directed attention to the minute anatomy of the os and cervix uteri; and here, at the outset, he was desirous of expressing his warmest thanks and obligations to Dr. Arthur Hassall, for his valuable assistance in the microscopical part of the investigation, and without which he could not successfully have prosecuted his researches. The mucous membrane of the os and cervix uteri, like the mucous membrane of other parts, consisted of epithelium, primary or basement membrane, and fibrous tissue, blood vessels and nerves. But as there were some special characteristics pertaining to this tissue, he proposed, for the convenience of description, to examine, first, the mucous membrane of the os uteri and external portion of the cervix; and secondly, the mucous lining of the cervical cavity or canal. The epithelial layer of the former of these situations was tessellated or squamous, and so arranged as to form a membrane of some thickness; by maceration, it could be easily detached, and it was then found closely to resemble the epithelial covering of the vagina, with which it was continuous. Beneath this epithelial layer was the basement membrane covering numerous villi or papillæ, which studded the whole surface. Each villus contained a looped blood-vessel, which, passing to the end of the villus, returned to its base, and inosculated with other blood-vessels of the contiguous villi. These villi had been mistaken for mucous follicles, usually described as covering the surface of the os uteri; but the microscope failed to discover any distinct follicular structure in this situation. When a thin section of the surface of the os uteri was examined by a low power, the points of the villi could be seen as dark spots through the epithelial layer. A careful examination exhibited these spots as slightly depressed in the centre, yet nevertheless slightly elevated in their margins, nipple-shaped, and containing red points, which were the terminations of the looped blood-vessels. These appearances were produced by the villi being obscured by their epithelial covering. The thick layer of scaly epithelium, and the villi with their looped vessels, were the principal anatomical features of the mucous membrane of the os and external part of the cervix uteri; and these structures played an important part in the pathological changes which occurred in the lower segment of the uterus in leucorrhœa. Between the margin of the lips of the os uteri and the commencement of the penniform rugæ, within the precincts of the cervical canal, a small tract of smooth surface was usually found, which to the naked eye seemed of more delicate

structure than the neighboring parts, and when examined by the microscope was found to be composed of cylinder epithelium, arranged after the manner of the epithelium covering the villi of the intestinal canal. The cylinder epithelium covered in this situation villi two or three times larger than the villi upon the surface of the os uteri—so large, indeed, as to be visible to the naked eye when viewed by transmitted light. Within the cavity of the cervix uteri, the mucous membrane contained four columns of rugæ, or folds, arranged in an oblique, curved, or transverse direction; and between these columns were four longitudinal grooves. The two sulci in the median line, anteriorly and posteriorly, were the most distinct; and of these, the sulcus of the posterior columns was the most strongly marked. In the normal state, the transverse rugæ, with the fossæ between them, were filled with viscid, semi-transparent mucus; and when this was brushed away, a reticulated appearance, caused by numerous secondary rugæ, was visible. The author gave a very minute description of these four rugous columns, and the furrows between them, which was illustrated by some very beautiful drawings of the cervical canal, displaying the rugous columns and fossæ of the natural size, and magnified nine and eighteen diameters. The latter power showed a large number of mucous fossæ and follicles, crowding the depressions between the rugæ and the rugous elevations also. The author mentioned that a healthy virgin cervix, of normal size, contained at least ten thousand mucous follicles. This anatomical arrangement secured a vast extent of superficial surface, which was still further increased by the presence of villi similar to those found in the lower part of the cervix; they were found in considerable numbers on the large rugæ and other parts of the mucous membrane in this situation. By this disposal of the mucous membrane of the canal of the cervix, a very large extent of glandular surface was obtained for the purpose of secretion. In effect, the cervix was an open gland; and in the opinion of the author, it was in this part of the utero-vaginal tract that the principal seat of leucorrhœa would be found to exist. There was an analogy here which should not be lost sight of, bearing, as it did, on the pathology and treatment of leucorrhœa, which was, the similarity which existed between the skin and the mucous membrane of the vagina and the external part of the os and cervix uteri. The resemblance, in these situations, was certainly much nearer to the cutaneous structure than to the mucous membrane of more internal parts. These analogies were strongly confirmed by what was observed of the pathological conditions to which these parts were liable, and by the effect of therapeutical applications. The author dwelt on the fact that the epithelium of the os uteri and external portion of the cervix was constantly squamous, and that the epithelium immediately within the os uteri was cylindrical but not ciliated, but that in the rugous portion of the cervical canal the cylindrical epithelium became ciliated. The mucus secreted by the glandular portion of the cervix was alkaline, viscid, and transparent; it adhered to the crypts and rugæ, so as to fill the canal of the cervix. It consisted chiefly of mucous-corpuscules, oil-globules, and occasionally dentated epithelium, all entangled in a thick, tenacious plasma; it was remarkable for its tenacity; while the mucus found in the lowest part of the canal was thinner in appearance. There was an essential chemical difference between the vaginal mucus and the secretion of the interior of the canal of the cervix; the first was always acid, and the latter invariably alkaline. Mr. Whitehead, of Manchester, had noticed this fact, and the observations of the

author confirmed his views. The acid of the vaginal secretion was more than sufficient to neutralize the alkaline secretion of the cervix, and when any secretion from the cervical canal entered the vagina it became curdled from the coagulation of its albumen. It was worthy of note, that that part of the mucous membrane of the uterus and vagina which resembled the skin was the only part which, like the skin, furnished an acid secretion. The vaginal mucus was a simple lubricatory fluid. But the uterine cervical mucus had other uses besides that of lubrication; in the healthy condition, in the intervals of the catamenia, it blocked up the passage from the vagina to the fundus; it thus defended the cavity of the uterus from external agencies, and from its alkaline character afforded a suitable medium for the passage of spermatozoa into the uterus. Having stated his views of the structure of the utero-vaginal mucous membrane, the author expressed his opinion that the glandular portion of the cervix uteri was the chief source of the discharge in leucorrhœa. Leucorrhœa, in its most simple and uncomplicated form, was the result of an increased activity of the glandular portion of the cervix. A follicular organ, which should only take an active condition at certain intervals, became constantly engaged in secretion. Instead of the discharge of the plug of mucus at the catamenial period, an incessant discharge was set up. At first the discharge was but an unusual quantity of the elements of the healthy mucus of the cervix. The quantity increases, and becomes a serious drain to the constitution, and the glandular cervix in some cases becomes so excitable, that any unusual stimulus, even mental emotions, provokes an augmentation. The author next referred to the conditions under which the epithelium of the os and external part of the cervix uteri and upper portion of the vagina might be partially or entirely removed. The mucous membrane then presented an intensely red color, from the presence of the naked villi, and an appearance of roughness or excoriation presented itself. He thought that among the causes which produced this aspect of ulceration, were eruptive disorders, similar to herpes or eczema, which strongly marked the analogy between this tract of mucous surface and the skin. He had observed cases in which an occasional herpetic eruption upon the os uteri always produced herpes præputialis in the husband. But the most frequent cause of denudation arose from the alkaline mucus discharge of the cervix irritating the acid surface of the os uteri, and causing the rapid shedding of the epithelium round the margin of the os. A microscopical examination was given of the various discharges met with in these affections, in making which the author was assisted by Dr. Handfield Jones and Dr. Hassall. In cervical leucorrhœa the discharge consisted of quantities of mucous-corpuscles, and in severe cases pus-corpuscles and blood-discs, with fatty matter, involved in a transparent plasma. The epithelial debris is constantly present, but in limited quantity. In vaginal leucorrhœa, including the secretions of the external portion of the os and cervix uteri, the plasma is opaque, and contains myriads of epithelial particles in all stages of development, with pus and blood globules when the villi are affected. When a circumscribed ulcer upon the os uteri is visible to the naked eye, after death, such as occurs in eruptive affections of the os and cervix, and is examined by the microscope, with a low power, it is found to consist of a base from which the villi are entirely removed, or in which only the scattered debris of villi remain; and surrounding this base there is a fringe of enlarged villi, partially or entirely denuded of epithelium. The character of the so-called ulceration of the os uteri was

detailed, and the nature of the discharges described. The author then observed that if any division of leucorrhœa were made, two principal forms must be recognized:

- I. The *mucous* variety, secreted by the follicular canal of the cervix.
- II. The *epithelial* variety, in which the discharge was vaginal.

With respect to the so-called ulcerations of the os and cervix, two kinds of morbid change would be observed:

- I. *Epithelial abrasion*, by far the most common, in which the epithelium alone was deficient.
- II. *Villous abrasion, erosion, or ulceration*, in which the villi are affected by superficial ulceration.

It was to the villi, denuded of epithelium and partly eroded, that the marked forms of granular os uteri were owing. The ovules of Naboth, often referred to by writers as obstructed follicles, the author had found to be in reality an eruptive disease of the mucous membrane analogous to a cutaneous affection. In these affections of the cervix uteri it frequently happened that the cervix uteri was partially everted, and the deep-red surface covered by vascular villi thus exposed, had frequently been mistaken for breach of continuity in the mucous surface. The author then offered some remarks on the practical deductions which might be drawn from the present investigation. The glandular structure of the parts from whence the leucorrhœal discharge arose, pointed to the influence of constitutional causes, and exemplified why this affection should be so common in women of strumous habit and leuco-phlegmatic temperament; it vindicated the importance of constitutional treatment, and directed attention to the more rational employment of topical remedies; and it was evident that the profuse application of caustics, as recommended by the French school of uterine pathology, was both unnecessary and unscientific. He admitted that leucorrhœa of the cervical canal was sometimes cured by the use of caustics to the os uteri, but in these cases they acted as counter-irritants to the glandular structure. The indications of treatment, based on a knowledge of the minute anatomy of the os and cervix uteri, and the study of its pathology in leucorrhœa, appeared to the author to require constitutional medicines and regimen, with local applications. Local measures, to be of any use in cervical leucorrhœa, should be applied, not to the vagina, nor the os uteri, but to the canal of the cervix. In vaginal or epithelial leucorrhœa, common injections were serviceable; but in cervical or mucous leucorrhœa no benefit could result unless the injection passed into the cervix. He mentioned the methods he adopted to secure this result, and concluded by expressing a hope that the prosecution of these researches might prove serviceable, by rendering a troublesome class of maladies more intelligible than they had hitherto been, and by tending to correct errors of practice, and to indicate the just value of constitutional and topical remedies.

[Dr. Tyler Smith's Paper was illustrated by a number of beautiful drawings, which excited great attention among the Fellows, representing the novel points described in the paper, and which were made under the superintendence of Dr. Hassall.]

At the conclusion of Dr. Smith's paper, the President observed that he should be happy to hear any observations upon it from the Fellows. After a short pause,

Dr. Locock rose and said that he regretted an appointment obliged him to leave the society immediately, but he could not do so without first offering

to Dr. Tyler Smith, for his very admirable paper. He could scarcely remember an occasion on which he had listened to a paper with greater interest, or from which he had derived so much instruction. The present communication was, in his opinion, a step in the right direction, and he felt convinced that researches of this kind would eventually lead to a better understanding and an improved treatment of what was most certainly a very intractable class of disorders. He was glad to learn the author intended to pursue the subject, and he should certainly look forward with great interest to the progress of his further investigations. (Cheers.)

A New Article for the Treatment of Tape-Worm. By ALFRED S. CASTLEMAN, M. D.

In 1844, I was at the "taking of a bee tree," at which a delicate child, of some ten or twelve years, ate freely of the honey comb, from which most of the honey had been pressed. Next day the child was purged, and sticking to the wax, which passed almost unchanged, were many pieces of tape-worm varying from half an inch to ten inches in length.

Since then, I have had opportunity of trying the honey in three cases, in which there was *no doubt* of the existence of the worm. In two of them it was entirely successful, in the third it failed; in each of them the patient was directed to eat, at short intervals, for twenty-four hours, as much new honey in the comb as the stomach would comfortably bear (otherwise fasting.) A cathartic was then administered, and the worm was ejected in short pieces, but alive.

A *country* practitioner might pass a lifetime in business and not meet with cases enough of this kind to prove or disprove the claims of any article to the title of remedy, but where a thing so simple, so easily obtained, so harmless in action, has proven successful in three of the only four cases in which it has been used, I feel that I am excusable in calling the profession to assist in ascertaining whether it has any claims to our confidence. I do not know that it is a remedy, nor do I recommend it as such; I barely state the facts which have come under my observation, and ask assistance in ascertaining whether they are worth preserving.

Delafield, Wis., August, 1852.—*North Western Med. and Surg. Jour.*

Medical Ethics.—The perusal of an article in the *London Medical Times*, touching a formidable feud which has recently sprung up in the good city of Edinburgh, forcibly reminds us of certain sins too common in our profession. The offense to which we allude is the too common practice of *criticising* the professional conduct of our cotemporaries. Examples, unhappily, of such misdeeds are too common everywhere, and we might readily find sufficient illustrations in our own country; but as it is no very pleasant duty to review our neighbor's conduct, and finding pertinent cases abroad, we prefer, on the present occasion, to cite a single instance of the latter. Moreover, as it is not our design to write a homily on medical ethics, we will mention only a single case, which, from its remoteness of location, can excite no personal ill feeling.

Several foreign medical journals have recently made allusions and contained articles in relation to an acrimonious personal controversy between Professors Simpson and Miller, of Edinburgh. It appears that the former performed some sort of surgical operation (the character not stated, but probably *vaginal*) on the wife of a medical gentleman. The patient died, *suddenly*, two days after the operation. An impression was at once produced that the patient died of hæmorrhage, but neither Prof. Simpson, nor Prof. Christison (who was called in consultation) detected the hæmorrhage. Many rumors were immediately set afloat in regard to the case, all more or less censuring the operator. A gossiping Doctor of the town, happening to have business with an upholsterer, was told by the latter that the mattress on which the patient lay had been sent to him to be cleaned, and that it was stained with blood. The gossiping Doctor immediately communicated this delectable morceau of intelligence to Professor Miller, who in turn mentioned it to Professor Henderson, the *homœopathist* who disgraces the University of Edinburgh. Nothing further was necessary to give the report currency. The homœopathist did not fail to circulate the statement, that a patient of Dr. Simpson's had been suffered to die of hæmorrhage, after an unimportant operation. Dr. Henderson's author being inquired for, the report was at once traced back to Mr. Miller. Dr. Simpson, as a matter of course, felt greatly aggrieved that his colleague, Mr. Miller, should be instrumental in circulating such a report, and, as it appears, an unfounded one. Of Dr. Henderson nothing better could be expected. The facts of the case show that the stain on the mattress, after all, was quite small, about such as would follow an ordinary operation, but not sufficient to cause death. Dr. Christison was of opinion that the patient died of low peritonitis.

Dr. Simpson, in a communication to Dr. Christison, complained of the conduct of Mr. Miller, and thus the quarrel began, which seems likely to have no end. Mr. Miller, in defense of his conduct, alleges that the story of the bloody mattress came to him incidentally, and that it was casually mentioned to Dr. Henderson, without any sinister motive.

It cannot be doubted that the conduct of Mr. Miller was in every sense wrong and unprofessional, whatever the motive. In the first place it was wrong to give currency to a report derogatory to a member of the profession, (and that member his *own colleague*,) on the mere statement of an unprofessional man, altogether unused to judge of such matters; and, even if *true*, it did not become his colleague to circulate the story; but, above all, it was inexcusable to make such a statement to an avowed *homœopathist*, although he, too, was a colleague. We can readily admit that Mr. Miller's remarks were uttered in carelessness rather than from sinister motives; but it is just such carelessness, (or recklessness more properly) which so often strikes at a man's reputation, under the shield of *inadvertence!*

In relation to the quarrel which has so unfortunately disturbed the harmony of the Edinburgh profession, the *London Medical Times* holds the following language:

"This dispute touches a vital doctrine of medical ethics; a doctrine admitted by all, but practiced by few. The first law of courtesy and professional honor is, that one medical man shall not discuss, criticise, much less condemn the conduct of another."

This doctrine is worthy of all commendation, and without it the profession becomes a disjointed assemblage of moral assassins. Gross blunders, or

obvious malpraxis, growing out of absolute ignorance, are not subject to such restrictions, but, on the contrary, deserve unlimited condemnation. If a surgeon, through ignorance of anatomy, destroy his patient by wounding a vital part; or, if a physician from gross incapacity, should administer corrosive sublimate in *ten grain* doses, and thus destroy life, no stringency of ethics can require silence, but all such acts deserve, and will receive universal condemnation. But the case is quite different when a mere *accidental* injury is inflicted; thus, the most skillful surgeon may accidentally wound the intestine in operating for strangulated hernia, and the most competent physician by administering a grain of opium in a case not precisely adapted to its action, may induce fatal cerebral congestion. These and similar accidents are the results of errors of judgment, or sheer casualties, which demand the sympathy rather than the censure of the more fortunate of the profession.

There are three classes of persons who injure their brethren by untimely criticisms: first, those who do it inadvertently and without improper motive; second, a class who indulge in criticism from a mere love of gossip, without a *desire* to injure the person who becomes the subject of remark; and, third, a class of wicked revilers who love detraction for the deed itself, and gloat over the defects, real or imaginary, of their fellow beings. These three classes exhibit widely different degrees of censurableness: the inadvertent man is censurable for his recklessness of the interests of his brother; the conduct of the gossiping man is reprehensible because he loves his neighbor's character less than his own gratification; and the malignant reviler deserves the severest condemnation for his wicked propensity.

How different would be the condition of the medical profession, if these critics could be induced to pause and *think* before speaking. It is a much more genial office to praise than censure a co-laborer; and the same keenness of optics guided by a judicious impulse, would find more to commend than disparage in the conduct of their brethren. And then the effects on the whole profession would be highly beneficial. We should, with this liberal course, present an unbroken front, which would appall the swarms of empirics, and command the entire confidence of the community.

Let us then be more guarded, and less censorious, and let us direct our efforts to the development of science, rather than the destruction of individual character.—*Western Lancet, Editorial.*

Treatment of Dysentery.—The following paragraph expresses, in our opinion, much truth. We have acted upon it for years, and strongly commend it to the notice of our readers:

“But if the abstraction of blood be not the remedy for dysentery, I can speak with equal confidence, from my own experience, upon a point on which theory might be expected to be no less explicit—I mean the worthlessness of *mercury* for the subdual of the disease. At the time of which I have been speaking, I found my brother medical officers and the civil practitioners of the island, for the most part, agreed about the pretensions of mercury as a cure for the disease—pretensions which had had their rise in the extravagant opinions entertained of its merits among the Indian medical men. Thus, the rule that associated dysentery and the mineral in their practice, I

found to be "absolute" and unquestioned. My own experience, however, of its control over the disease, at first suggested doubts of its curative influence at all; and, latterly, convinced me that it was not merely inoperative, but prejudicial in the extreme. So far from the tenderness under pressure giving way, the tenesmus and tormina subsiding, and the blood and "corruption" composing the stools, giving place to normal dejections, under its use, the very opposite of all these was the case, in proportion as the poison was pushed and continued; and this so much as a rule, in my own practice, that I began at length to suspect the mineral as at the bottom of the mischief, and in the end, to be unable to disconnect the two. In order to avoid jumping to hasty conclusions upon the point in question, I administered the mineral in varied forms and doses—by the mouth and by inunction—in two grain doses and in twenty; and as a general rule, with the same results in both cases—to wit, an aggravation of the irritative action already going on in the mucous membrane—results which, as I have already said, I should have looked for from *priori* reasoning; and for these considerations:—1. That the state of the blood in dysentery is the opposite to that in which mercury is found by experience to be beneficial, being one of dyscrasy, and in which abnormal elements are superadded. 2. That I believe the natural tendency of mercury is to irritate all mucous membranes even in health, and greatly to aggravate irritation and inflammation when already present in the same. Such I believe to be the explanation of the poverty of its influence over mucous inflammations in general—a position, I am well aware, in which I shall not be supported by the general voice of the profession; but of the soundness of which I, myself, entertain no doubt.

What I have had to offer in relation to bleeding, leeching, and mercury, in this disease, is not less appropriate to the use of blisters for the same; in a word, that they are eminently impotent, if not injurious, in their operation.

So much for the worthlessness of the three most potent and valued agents in healthy inflammation, when addressed to the disorder now under consideration; and if this alone be not a strong ground of suspicion, as to the specific character of the action giving rise to it, I know not what stronger to adduce.

But as I am not writing a *treatise* upon dysentery and its management, it would be foreign to my present object, to enter further into what its essence *does* consist in, or to discuss at greater length the question, what treatment is adapted to its cure?

I may briefly mention here in passing, that I believe *opium*, in one form or other, to offer a larger promise of success in every variety of dysentery, than all the other agents in common use for it put together, provided the system be not further depressed by abstractions of blood, and that attention, at the same time, be paid to the all-important consideration of supporting the patient's strength, instead of sinking it by evacuants. I have stumbled upon a remark in a respectable authority of the last century (Kirkland "on Child-bed Fevers,") very apposite to the present subject, as well as suggestive of the inference, that Dr. Kirkland, as a physician, was in advance of the age in which he wrote. It is this: "I believe there is no kind of fever which will not be moderated by its use (that of opium); for if, in inflammatory fevers, we join small doses of it with antiphlogistics, and in putrid fevers with antiseptics, we shall frequently prevent the disease having violent effects, and

give the proper medicines a better opportunity of accomplishing the intention desired."

"Were the same cases (of dysentery) again to be placed under my care, I would not hesitate to give opium in doses of four or five grains, as it was the opium chiefly that seemed to arrest the progress of the inflammation."—Cheyne, *Loc. Cit.*—*Lon. Med. Times & Gaz., from Western Lancet.*

Treatment of Dysentery. By GEO. S. UPSHUR, A. M., M. D., of Norfolk, Va.

We quote the following from an article on *Dysentery*, by Dr. Upshur, in the Philadelphia Med. Examiner.—ED. BUFFALO MED. JOUR.

No agents have been more overrated in the treatment of dysentery than mercurials. The combination of calomel, opium and ipecac., is the *alpha* and *omega* of therapeutics in this disease, with a very large and respectable body of physicians; and such is the testimony adduced in its favor that I doubt not beneficial results have followed its use in particular cases. I am disposed, however, to attribute the benefits to the *opium and ipecac. only*, and am satisfied from careful and ample observation, that they would have accomplished much more good without the calomel. It may be true that in the dysentery of tropical climates, where torpor of the liver is usually a complication, mercury is sometimes administered with benefit, but in the ordinary dysentery of this country I am persuaded it does more harm than good. My own observation in this regard is corroborated by some of the best authorities in medicine.

Dr. Cheyne, in 3d vol. Dublin Hospital Reports, says: "Mercury could not be depended upon, and did not relieve in numerous instances when the mouth was affected, and sometimes seemed to increase the disease."

M. Twining, who saw a vast amount of the disease in the General Hospital at Calcutta, says: "I speak without hesitation on this subject, from having too often seen the fallacy of trusting to the effects of calomel for the cure of severe acute dysentery, and having tried that medicine extensively in every form of the disease."*

Dr. Mackintosh, (*Practice of Physic*, vol. i. p. 366, 1837,) says: "My own experience in this country, as well as within the tropics, enables me to confirm the statement of Dr. Cheyne and M. Twining."

Dr. C. A. Lee, in a note to the art. *Dysentery*, in Copland's Dictionary, says: "With respect to the use of mercury in this disease, we believe that it generally proves injurious."

Dr. Dunglison says that the common remark, that if the disease proves obstinate it will yield, provided we can only 'touch the mouth' with a mercurial, does not accord with his experience. He has seen many cases in which the effect of mercury has been induced on the system, and, nevertheless, the disease has proceeded to a fatal termination. (*Practice of Medicine*, vol. i. p. 114.)

Dr. Batchelder, in the N. Y. Journal of Medicine for July, 1851, confirms

* Vide Diseases of Bengal, by W. Twining, Esq., p. 40.

these statements by adding his own reliable testimony against the calomel practice.

But it is useless to multiply authorities upon this subject. I will add a case or two in illustration from my case-book:

The cases are omitted.—ED. BUFFALO JOUR.

Saline Cathartics.—I have prescribed a combination of Epsom and Rochelle Salts, ℥ij. of each, after general blood-letting, with the happiest effects in many cases of acute dysentery—and am disposed to think very favorably of the practice. I think it is particularly applicable to cases occurring in robust persons where the tenesmus is very troublesome. Frequently have I seen this symptom entirely relieved after the copious watery evacuations produced by the saline.

Acids.—The nitrous acid mixture of Dr. Hope, (℞. Acid Nitrosi ℥i. Tr. Opii gtt. xl.; Aquæ Camph. ℥viii. M.) which was introduced to the profession in this country through the columns of this Journal, by Dr. C. D. Meigs, in 1838, is justly considered by those who have used it freely, a remedy of great value in dysentery. Among all the good things which Dr. Meigs has done for his brethren, he never did a better than calling their attention to this remedy. In my hands it has rarely failed to accomplish every thing that was desired. It is not a specific, but if fairly tried I think will less often bring disappointment than any other single remedy in this frequently intractable affection. Great care should be taken not to administer it in too small doses, an adult should not take less than ℥ii. at a time, and it should be continued at intervals of three or four hours until about eight doses have been given.

Opium and Acetate of Lead.—Since the publication of Dr. Batchelder's valuable article, before referred to, I have given the acetate of lead and opium a fair trial, and am prepared to speak as favorably of the combination as Dr. B. has done. My experience with it has been in those cases chiefly where Hope's mixture failed. I usually give as the first dose, one grain of opium with four of the acetate, and then half a grain of the one with two grains of the other, *immediately after each evacuation*, no matter if the bowels are moved every five minutes. Usually but a few doses are taken before relief is obtained.

Thoughts on Chloroform. By C. R. GILMAN, M. D., Professor of Obstetrics, &c., College of Physicians and Surgeons, New York.

If any apology is needed for him who attempts to interest the profession on the hackneyed theme of chloroform, it must be found in my case, in the deep feeling of responsibility which attaches to a public teacher, who is compelled by the results of his own experience to commend, and that earnestly, to his pupils, and thus, according to the measure of his influence, to extend the use of this agent, and yet can hardly open a medical periodical without learning that another and another life has been sacrificed to such use.

Is it to be expected—is it to be desired that such an one should turn from these harrowing narratives, and, satisfied that he has had no direct personal share in such horrors, dismiss the subject from his thoughts? Is not such

an one to be pardoned if over zeal induce him to be "instant in season and out of season" in his efforts to prevent such deeds of death? Yet, how prevent them? Shall we banish anæsthetics from our materia medica—proscribe their use? Plainly this is impossible.—We cannot and *will not* give up the use of an agent which in our hands relieves suffering, cures disease, saves life—as we know chloroform does—because other men abuse it. Nobody expects us to do this with the forceps; and yet how often is health, and even life sacrificed by the careless abuse of the forceps! We must continue, then, to use, and those of us who are public teachers must continue to commend them. This, in the lecture room, is easy. There is no difficulty in *there* teaching the use and guarding against the abuse of anæsthetics: the rules are well settled. But who can assure us *that*, be we ever so careful, be our commendations ever so guarded, our influence may not be for evil in this matter? Even among our hearers, some may remember the commendation, and forget the cautions; and if the story chance to pass from mouth to mouth, what chance is there that more will be repeated than that Doctor, this or Professor that recommended chloroform? Such are the reflections that have induced me to ask a place in your journal for a few thoughts on chloroform.

I shall say nothing in detail in favor of the practice, and therefore, perhaps, ought to premise that my confidence in its powers is undiminished, the number of cases in which I use it enlarges every day. Thus much for the bright, but my present business is with the dark side of the picture. First, then, let me state very briefly a case, which, as I suppose, throws some light on the varying degree in which it operates on different individuals. Mrs. J. was taken in labor early in the morning, January 17, 1852,—the child was born in a few hours, but almost immediately afterward the patient had severe convulsions. She was freely bled, had an enema, cold to the head, &c. I saw her in consultation, about three hours after the first fit; the convulsions were slight, patient very restless, consciousness *nul*. I thought it a case for chloroform, and the drug was sent for. During the absence of the messenger, a more careful examination of the pulse convinced me that she would bear more bleeding—the pulse, as I had anticipated, rose, and more than two pints were taken. Just as we were bandaging the arm, the chloroform was received, and at once administered. I gave it cautiously, my finger on the pulse, with no attempt to overwhelm the sensibility—a thing I never do; yet when the patient had taken four deep inspirations, the pulse fluttered—staggered—stopped. A dash of cold water, a few puffs of the face, and she gasped, her pulse staggered on—became regular, and all was well. Here the frightful symptoms were doubtless owing to the rapid absorption of the chloroform, consequent upon the profuse bleeding—the empty vessels were "*all agape*," and every particle of chloroform was eagerly caught up. This influence of blood-letting in absorption is well known, yet I do not remember to have seen it alluded to in this connection. Its obvious importance will strike every one. It should lead the obstetrician to double caution in the use of anæsthetics where flooding has occurred, and the surgeon, in all bloody operations; especially if these latter are protracted, absorption will be rapid and danger proportionably great.

Thus much for a particular point: now let me offer a few suggestions on the great end and aim of this paper: How to diminish the danger. And in the first place, let me urge on all who have not practical familiarity with

the course and symptoms of anæsthesia, to use ether—sulphuric ether, I mean, for I verily believe that chloric ether is worse than chloroform; it is more likely to be used freely, i. e., carelessly; and thus used, *it will kill*. Use, then, sulphuric ether till you are at home in anæsthetics, yet, even then, beware lest this advantage lead you to carelessness with chloroform—*carelessness is death*. But it is not beginners only, who should prefer ether. The state of the patient may render one or the other preferable.

Without going into detail, I should say that exhaustion and prostration call for ether—great nervous excitement and vascular activity, chloroform.

Now, as to the extent to which the drug should be carried. Here I find myself at issue with the great mass of the surgeons. They all—all at least of whose doings I am cognizant, carry anæsthesia to the complete abolition of sensation. Is not this attempting too much? Never in natural labor, and very rarely in obstetric operations, do I go beyond a decided benumbing of sensibility. This may or may not be attended with loss of consciousness, for there is certainly no regular order of succession, or at least no invariable order observed. I have seen consciousness perfect and sensibility entirely gone, and *vice versa*. The rule then should be, go always beyond the stage of excitement; till this is passed nothing can be done; but as soon as it is passed, pause, and arrest the process always short of stertor.

Such I believe to be the true practice in obstetrics, and under it *no fatal case has occurred*.

Now, may not the surgeons profit by our good and their evil fortune, and stop short of the deep, dangerous state of anæsthesia, into which they now plunge their patients? Something ought, nay, something *must* be done to prevent these ever recurring deaths from anæsthesia; and it does seem to me that those who can guide and control surgical opinion, ought to be willing to sacrifice the advantages of complete insensibility, that others may not jeopardize life. It is very true that one who has very large experience may again and again crowd patients down into deep snoring anæsthesia, and yet no harm come of it; but if he do it, the man of less knowledge and of narrower experience will do the same thing, and death to the patient and disgrace to the practitioner be the disastrous result.

There is another idea on this subject which presses upon my mind when I read these terrible cases, so strongly that I must give it utterance; and I hope that the thoughtful members of the profession, who feel as we all ought to feel, that nothing should be neglected that may by possibility free the skirts of the profession from the deep disgrace of these repeated deaths by chloroform, will not reject my proposition till they have well considered it. It is my deliberate opinion that, in every case of death from chloroform, the whole history of the case ought to be fully and impartially investigated by a coroner's jury. I know and feel the objections to this course. I know that it may—nay that it must fall occasionally with crushing force upon a brother practitioner, who may be entirely innocent of negligence or rashness—still I say it ought to be done. Let it fall where it may, the thing ought to be done. It will be a terror to evil doers, it will restrain the rash, it will punish the guilty; and that there has been fearful rashness and deep guilt in some of these cases, no man who has used chloroform often can doubt. If in every case the inexperienced would use ether; if, when chloroform was ventured on, a competent person had his fingers on the pulse, and his undivided attention fixed on the anæsthetic state of the patient, who believes that

the fatal cases would have occurred? *I do not!* If, then, these precautions are neglected, is not such neglect criminal? I believe it is, and that the profession owe it to themselves to have the question of guilt or innocence impartially investigated. The time will soon come, when, if we do nothing, the public will demand this in every case. It is better that the profession propose, than that they be hereafter forced to submit to such investigations.—*New York Medical Times.*

Expulsion of Tape-Worm by Pumpkin Seeds.—CASE II. Through the politeness of Dr. E. M. Moore, I had an opportunity, about two weeks ago, to administer the pumpkin-seed emulsion to a patient of his, affected with tape-worm. The remedy was taken at 6, A. M., followed by two doses of castor oil, which expelled the worm at noon. When I saw it, it was much corrugated by immersion in strong alcohol, so that its length must have been materially reduced. It measured eighty inches, and was composed of neck, body and tail, but no head. The patient, for a number of years, had expelled fragments of the worm almost daily, and quite recently had pulled off more than a foot in one piece. I proposed another trial of the remedy, with a view of obtaining the *head*, but the experiment has been necessarily postponed. In respect to the evacuation of the head upon which so much stress is laid, it is well to bear in mind, that from the extreme tenuity and fragile nature of the animal's neck, and the means which the head possesses of fastening itself to the bowels, together with the minuteness of the organ itself, various difficulties and disappointments must arise in our efforts to obtain ocular demonstration of this structure. Bremser mentions "an important practical fact, viz., that of the many hundred persons cured by him of tape-worm, not a single individual has seen the head come away."—(Cycl. Pr. Med., art. Worms.)

The seeds used in the above case were of the common pumpkin—*cucurbita pepo*—and were prepared as follows: Take three ounces of pumpkin seeds, bruise them thoroughly in a mortar, and add water until by expression and straining they afford eight ounces of emulsion. The whole to be taken at once on an empty stomach, followed in two hours by a sufficient purgative dose of castor oil. Cold water to be used freely as a drink, but no food until after the operation.

In Philipp Lorenz Geiger's *Hand buch der Pharmacie*, Heidelberg, 1829, 2nd vol., p. 1702, the *cucurbita occidentalis* (West-Indian pumpkin) is recommended, on the authority of D. Mongency, as a valuable remedy for tape-worm.

For *simplicity, cheapness and safety*, the "pumpkin seed remedy" has no competitor. That its *efficacy* may prove equal to that of kousso and other celebrated articles, is not an unreasonable anticipation, and my object in recording these two cases has been, to induce the readers of the *Journal* to test it and report their experience.

W. W. ELY.

Rochester, N. Y., Sept. 27, 1852.—*Boston Med. and Surg. Journal.*

Three Cases of Tape-Worm, with Remarks. By HENRY S. PATTERSON, M. D., Professor of Materia Medica and Therapeutics in Pennsylvania Medical College.

CASE I. *Successful use of Kwoosso.*—Miss W., æt. twenty-two years, consulted me in March, 1851, with tape-worm. She had been in failing health for several months, with headache, languor, variable appetite, nausea, and abdominal pains, but had not sought medical advice. Having taken a dose of citrate of magnesia, she observed in her discharges something peculiar, which proved to be joints of *tænia solium*. As I was at that time expecting some kwoosso from Europe, I postponed treatment until it should arrive. Early in April it came, and was administered, during my absence from the city, by my friend Dr. Gilbert. He gave the dose (3vj.) at once, the patient having fasted from the previous day. It excited some nausea but no vomiting. It was followed, in a few hours, by a dose of castor oil, which brought away a tape-worm several yards in length, but which unfortunately was not preserved for more minute examination. There can be no doubt, however, that the entire worm was expelled, as the patient rapidly convalesced, and has been in the enjoyment of uninterrupted health since that period.

CASE II. *Failure of Kwoosso. Successful use of Pumpkin Seeds.*—The subject of this case was for some time under my care, in consultation with my colleague, Dr. Darrach. I can aver that he was most thoroughly put through the entire routine of tape-worm remedies, before he left Philadelphia. He tells his own story so well, that I prefer to give the following extract from a letter announcing his restoration to health: "In the early part of January, 1836, I was rather suddenly attacked with what seemed to be an alarming diarrhoea, which continued for some weeks, resisting the usual remedies. My symptoms had been peculiar for some time previous to the attack. Indeed, I had all the prominent symptoms of *tænia* as laid down in the books, viz., dizziness; occasional false vision; variable appetite; pain in the lumbar region; pain in the knee-joint; swelling of the abdomen; hesitancy of speech; restlessness in time of sleep; unusual drowsiness during the day; varying strength, being sometimes quite strong and then again quite feeble. Somewhere about the middle of February of the same year, I discharged, at a morning stool, about nine yards of the *tænia*. From that time onward, for six years, I was more or less under medical treatment continuously. I took vast quantities of the spts. turpentine, (once or twice two ounces at the dose,) also the malefern, calomel and jalap, and Jayne's vermifuge; and was several times under homœopathic treatment. I took also iodide of potassium, iodide of iron, decoction of pomegranate, and the 'kousso.' I discharged large quantities of the worm, but no head could be perceived. When the kousso failed, I began to despair of being cured at all, but my sister, Mrs. —, sent me, in December last, two numbers of the Boston Medical and Surgical Journal, containing two several accounts of the cure of *tænia*, by the use of pumpkin seeds. Having previously abstained from usual food for a day, on the 20th of January last, I took, at 8 o'clock in the morning, two ounces of the kernels of pumpkin seeds pulverized with two spoonfuls of white sugar, commingled with a half pint of boiling water, making a very

pleasant drink for a fasting man. I kept my bed, drinking frequently of cold water, and at 9½ o'clock I took an ounce of castor oil. At 10½ I drank a cup of hot black tea, and, in about two minutes, discharged about eight yards of the tape-worm, *with the head*. O, how I wept for joy, that I was again a free man, after a servitude of six sad years to this awful complaint. Since then, I feel like a new being in a new world. My life had often been a weary burden, and yet I grew fleshy and looked healthful. For months in succession I had discharged the worm daily, in pieces of six to eighteen inches, and also in gourd-seed form. I suppose that, without any over-estimate, I discharged during the six years of my affliction, about *four hundred yards!* The remedy is very simple. Were I a practicing physician I would never administer the turpentine for tape-worm; I sometimes fear that I have experienced irretrievable harm to my kidneys from using it. There is virtue in pumpkin seeds, doctor, even if it be a *Yankee notion*."

CASE III. *Successful use of Xanthoxylon Fraxineum*.—For the following curious case I am indebted to my friend Dr. Thomas J. Turner, of Port Richmond:

J. R., æt. 41 years, is a workman in a chemical laboratory. In December, 1847, whilst a private in the British army in Ireland, he first perceived that he was afflicted with tape-worm. He states that he passed fifteen or twenty joints at almost every stool for a time, and on several occasions as much as thirty feet at once. The surgeon of his regiment treated him with *Ol. Terebinth.* ℥j. every other day. He also took tin-powder, malefern root, and "every other article he ever heard of." He finally abandoned the hope of a radical cure. The symptom most prominent was a sense of gnawing and beating at the epigastrium in the morning. He was obliged to eat before rising, as he otherwise became faint, and "had all sorts of queer feelings." His appetite was insatiable. While at Port Richmond, in the autumn of last year, he was attacked with tertian intermittent fever, for which he was recommended to take an infusion of prickly ash bark in brandy, a popular domestic remedy. He digested an ounce of the bark in a pint of brandy, and drank the whole during the apyrexia. The result was a most copious diaphoresis, as usual, and also some purgation, bringing away the entire worm. He has remained perfectly well since.

I have recorded the above cases, because of the intrinsic interest attached to each, and more especially because they serve to teach us moderation in our laudations of special remedies for tape-worm. There are many substances which, if given at the right time and in the right way, will destroy and expel the *tœnia solium*, but there is not one of them which will not frequently disappoint the practitioner. The subject of each of the foregoing cases is vehement in praise of his or her "cure," and yet the judicious practitioner will give either of them in his next case with an abundant reservation of doubt as to the result. Similar accounts of the accidental cure of tape-worm are by no means rare. We have recently had in the medical journals a history (from the *Ann. Univers. of Turin*.) of a case in which a shepherd, to whom valerian had been prescribed for a nervous affection, plucked and took by mistake, a quantity of *Conium maculatum*. He came very near losing his life, but fortunately recovered with the loss only of a tape-worm, which revealed the source of the nervous affection in question. The account is

headed "Efficacy of *Conium maculatum* in Tape-worm." Is it not true, however, that any acro-narcotic poison, given so close to the verge of life-taking, may dislodge and destroy a parasite in the bowels? I should certainly refuse to admit the conium into the list of anthelmintics on this showing. The remedies used in our cases are fortunately without danger, and may be tried safely.

As to the kwosso, the excitement that attended its introduction, appears to have subsided already, and it is to be presumed that the greater part of the 1400 pounds which the nephew of M. Rochet d'Hericourt admitted to Dr. Pereira that that gentleman had in store at Paris, still remains on hand. The first notice of this article I have met with, is in the *Annuaire of Bouchardat* for 1841, on the authority of M. d'Abbadie, somewhat notorious in African affairs of late years. It was subsequently brought to England by Dr. C. T. Beke, and was the subject of a paper read by Dr. Hodgkin, at a meeting of the British Association at York. (See *London Medical Times*, October 26, 1844.) The *Annuaire of Bouchardat* for the same year, contains a notice of an attempt at its chemical analysis by Stanislaus Martin. One of the most satisfactory accounts of it is found in a communication from M. Schimpfer, Governor of Adoa, in the *Gazette Med. de Strasbourg*, (*Bouchard. Ann.* 1849, p. 254.) He informs us that "it never completely expels the worm, or at least that rarely happens." He also states that it is but one of the remedies relied upon in tape worm by the Abyssinians, and he enumerates eight others. Dr. Beke (to whom I am indebted for an interesting note on the subject,) informs me that the natives with whom he conversed, did not rate the kwosso above several of their other remedies. An Abyssinian servant, brought to London by Dr. B., was treated by Dr. Hodgkin for tape-worm, first with kwosso, and *afterward* with *Ol. Terebinth.* The impression of Dr. B. was that the latter was the more efficacious, though neither finally removed the worm, and the man returned to Africa showing occasional symptoms of its presence. Dr. B. states that the natives who use the article, are in the habit of taking a dose every two months. He is also my authority for the orthography kwosso, as best representing the Abyssinian pronunciation.

There can be very little doubt that the recent excitement about the kwosso was, in a great measure, the result of a commercial speculation. It has subsided suddenly, and I presume that the originators of the plan have been disappointed. It is already perceived that, while the kwosso is undoubtedly an active vermifuge, it is by no means so infallible as was represented. On the whole I consider it inferior to the *Oleum Terebinthinæ*, which is perfectly safe when judiciously given, notwithstanding the protest of my correspondent. I have given it constantly in ℥j. doses, or in doses of ℥ss. with an equal quantity of *Oleum Ricini*, and have never seen any ill effect whatever. Should the kwosso be afforded at a reasonable price, it will probably take its place among our established means of cure, but I have no expectation of its being employed at the rate of \$5 for the dose of six drachms.—*Philadelphia Medical Examiner.*

Mineral Springs.—Notice.—Doctor John Bell (Philadelphia,) who is preparing a work on *Mineral Springs*, more especially on those of the United

States, is desirous of procuring, at an early day, all accessible information on the subject. With this view he requests his professional brethren to transmit to him all the facts in their possession which may throw light on the chemical composition and curative powers of the waters of the springs in their respective neighborhoods.

Proprietors of these waters would oblige by sending to Dr. Bell authenticated accounts on these points, and also of the topography of the springs, and the roads by which they are approached.—*Philadelphia Med. Examiner.*

Poisonous Chloroform.—The numerous deaths which have recently taken place from the inhalation of chloroform, seem to require that I should state what I know upon this subject, without waiting for more extended researches which I have now in progress; for a word in time may save human life, and I shall, therefore, present my views, even though some may think that I ought to wait until my work is completed to its full extent, before publication. I have formerly been charged with dilatoriness in presenting my discoveries to the public, and wish to avoid a repetition of this accusation, even though my work, in its present state, is not so complete as would be required for scientific purposes.

I have long had a strong suspicion that the very sudden deaths resulting from the inhalation of chloroform, must have been produced by the presence of some poisonous compound of amyle, the hypothetical radical of Fusel oil, or the oil of whiskey; and I began a series of researches upon this subject several years ago, but was called off from my work by unexpected persecutions. This work I have resumed, and I will now state what facts and inductions I am able to lay before the public.

1st. When chloroform, and the alcoholic solution of it called chloric ether, was made from *pure* alcohol, diluted with water, no fatal accidents took place from its judicious administration.

2d. When chloroform was made, as it now too frequently is, from common corn, rye, and potato whiskey, deaths began to occur, even when the utmost care was taken in its administration.

3d. In the Chelsea case, where this kind of chloroform was probably contained in the alcoholic solution incorrectly called chloric ether, death took place in a very sudden manner, and the post-mortem appearances of the subject indicated the usual effects of poisoning by chloroform.

From these data, it might justly be inferred that some poisonous matter exists in the cheap chloroform of commerce, and I suspected that it arose from the Fusel oil which exists in whisky. This opinion, at my suggestion, was published by two of my friends, to put the public on their guard, and those gentlemen urgently advised that physicians and surgeons should return to the use of pure sulphuric ether (oxide of ethyle,) as originally prescribed by me.

It is well known that I have always preferred my original anæsthetic agent to all the substitutes that have been proposed since; but still I have always been willing to give the proposed substitutes a fair trial, and did try them all, first upon myself, and then upon such of my pupils as felt willing to allow the experiment to be made upon them. I also in a measure compromised with that powerful anæsthetic agent chloroform, by mixing small

proportions of it, about one-fourth or fifth part, with sulphuric ether, so as to concentrate the anæsthetic agent into a smaller bulk, and I have extensively used this preparation in the production of anæsthesia, and without producing any dangerous or even unpleasant symptoms in any case, but I always took care to ascertain that the chloroform used by me was pure.

Having, during the last month, succeeded in procuring some very pure Fusel oil (of whisky,) I undertook the researches which have resulted in the conviction that it is this amyle compound that produces the poisonous matter of certain kinds of chloroform. When mixed with hyperchlorite of lime (bleaching powder) and water, in the same way as we prepare alcohol for the production and distillation of chloroform, I found that the mixture in the retort, after agitation and standing some time, became warm, indicating that a reaction was taking place between the Fusel oil and the hyperchlorite of lime.

After some hours the retort was placed in a water-bath and distillation was effected, the volatilized liquid being condensed by means of one of Liebig's condensers. A clear colorless liquid came over, which was at once recognized as having the peculiar *odor of bad chloroform*. It is, perhaps, a *ter chloride of amyle*, but has not yet been submitted to analysis. It is so powerful that merely smelling of it makes one dizzy, and working over it made me so sick that I was obliged to go out of doors for fresh air several times during my operation on it. In order to make sure that the Fusel oil was all decomposed, I again mixed the product of the distillation above mentioned, with a new lot of bleaching powder, and water; and after three hours, with frequent agitation, it was again distilled, and gave what I regard as the pure unmixed poison. This I am now to test on such animals as have proved good ether subjects, and shall make report of my results in this Journal.

If my views are correct, it follows:

1st. That all chloroform intended for *inhalation as an anæsthetic agent should be prepared from pure rectified alcohol*, to be diluted with water when used for distillation from hyperchlorite of lime.

2d. That no druggist should sell, for anæsthetic uses, any chloroform which is not known to have been properly prepared as above suggested.

3d. That the mixture of chloroform and alcohol, commercially known under the name of strong chloric ether, must be made with the same precautions as chloroform.

There is less danger of the existence of Fusel oil in sulphuric ether, which is always made from strong rectified alcohol.

There is more danger of the existence of sulphurous acid in this liquid, and that is a dangerous poison, but it is one readily detected; and persons will object to inhaling ether containing it, on account of its well known disagreeable odor of burning sulphur.

Fusel oil itself, according to the microscopic researches of my friend Dr. Henry C. Perkins, of Newburyport, appears to act as a poison. His experiments were suggested by an article published by Mr. Henry A. Hildreth, imputing the poisonous qualities of some kinds of chloroform to Fusel oil contained in it.

It is important, now that this Fusel oil has been introduced into medicine, as a remedy in phthisis, that the profession should know that when it is inhaled it may produce fatal results, and that great caution is necessary in the use of so powerful an agent. Administered, a few drops at a dose, by the

stomach, it does no harm, but is undoubtedly useful in some forms of disease. Experience will soon show how far it is remedial in tuberculous diseases; and this remedy is in good hands at present—Dr. Morrill Wyman and Dr. Perkins having engaged in the researches as to its medicinal use.

I annex a letter which I have just received from Dr. Perkins, deeming it an interesting contribution to physiological science.

Respectfully your obedient servant,

C. T. JACKSON, M. D.,

Assayer to the State of Mass. and to the city of Boston.

Boston, Sept. 1, 1852.

NEWBURYPORT, Aug. 27, 1852.

My Dear Friend,—Noticing, the other day, a paragraph in one of the papers, which attributed the evil effects of chloroform to the Fusel oil it contained, I tried an experiment upon a frog with a few drops of this oil dissolved in ether, and found that after inhaling it for a short time, the same effects were observable under the microscope as appear when chloroform is used, *viz.*, an *almost entire* suspension of the circulation in *all* the blood-vessels ramifying upon the web of his foot; there was, in fact, only a *very slight backward* and *forward* motion to be seen in *one* single vessel; in *all* the others the blood was *perfectly stagnant*. The frog was insensible for a much longer period than when the ether alone is used. He is now bright and ready for another experiment—to which I proceed.

I exposed him to the vapor of a few drops of Fusel oil dissolved in about a drachm of New England rum, for about six minutes, when he closed his eyelids and seemed under its influence. He was then placed upon the stand of the microscope, but not the slightest appearance of circulation was to be found in any of the vessels of the web; it was unusually pale and exsanguinous. He removed his foot twice or thrice from the stand, and gasped several times. I was now called away, and was absent about half an hour. Upon my return, the frog was found *dead*.

Several queries suggest themselves, which you will allow me to propose:

- 1st. Is there any Fusel oil in sulphuric ether?
- 2d. Can the Fusel oil be removed from the chloroform?
- 3d. Would the vapor of New England rum, rot-gut whisky (which contains this oil,) produce anæsthetic effects?
- 4th. In what other liquors is this oil found?
- 5th. Does it in small doses, as administered by our friend, Dr. M. Wyman, and as I am now trying it upon his recommendation, diminish the pulse and act as a direct sedative?

To the third and fifth queries I shall direct my attention. The others I leave for your investigation.

Very truly your sincere friend,

—*Boston Med. and Surg. Jour.*

H. C. PERKINS.

Acute Laryngitis.—Inclosed you have an exceedingly graphic account of a very interesting case, which you are at liberty to present to the profession should you deem it worthy of publication.

The extreme asphyxia, verging on death, presented to the surgeon, the

primary operation, its absolute success and efficiency, and the *length of time* which the tube has been worn with apparent impunity to the surrounding parts, are truly extraordinary features in the narration.

I would simply remark that the account is drawn up entirely by my friend and patient, Mr. Hamlin, of this city, late a highly worthy and respectable merchant of Philadelphia, and that his statements are implicitly reliable.

In conclusion, permit me to say, that although his general health, now fair, continues to improve, and physical system to develop, the local stricture yet presents a case of peculiar interest both to the patient and to science. We trust, however, that continued perseverance and care may yet overcome the remaining obstacle to complete success, and that he may be again restored to health and his accustomed occupation and usefulness.

With great respect, believe me truly yours, &c.,

PROVIDENCE, R. I., July 29, 1852.

J. M.

P. S.—Should be happy to receive hints from the profession adapted practically to the case.

M.

DR. JOSEPH MAURAN, Providence:

Dear Sir,—Supposing that a history of my case might prove of interest to you, I take this occasion to give a statement from the period of the first attack until the present time.

I have been subject to an occasional sore throat for the past eight or ten years, which heretofore had yielded to the prescribed remedies.

In the month of January, of this year, a violent cold left me with hoarseness, which gave but little trouble at first, and was of so light a character as to render medicine and advice unnecessary.

In the month of April I called upon a homœopathic physician of Philadelphia, as my breathing became affected and my voice very weak. Each respiration sounded as if coming from two individuals. He told me that I had an inflammation of the windpipe, and attended me until the eighth day of May.

On the day previous, I became so weak and exhausted from walking, or the least exertion, that confinement to my room became necessary, and my agony was so great that he was sent for in the night. His powders produced no effect. At 4 o'clock in the morning he pronounced me in no danger. I had spasmodic respirations which could be heard over the whole house. My eyes were fixed, and it was impossible to obtain sufficient breath to fill my lungs. I found that no position would give me any ease or comfort. It seemed as if every muscle of my face, chest and arms, was thrown into activity without giving me the least relief. I attempted to eat and drink, but found it impossible. The doctor gave me emetics and anodynes, but they produced no effect. He left me at six o'clock. At about 7 o'clock I became insensible.

Dr. Joseph Pancoast, of Philadelphia, reached me between the hours of 9 and 10 o'clock on the morning of the eighth May. He pronounced the disease "acute laryngitis." My pulse and respiration had ceased, and my face was black. He pronounced me dead! that it was too late for any operation to save me, but soon consented to try, and made the first incision without my showing any signs of animation. A discharge of very thick, black clotted blood very slowly followed. All the fresh air which was possible was admitted to the room. A slight twinge was observed of a muscle of the

mouth, and then a slight heaving of the chest displayed a partial respiration, which was gradually increased until the whole mechanism of the body was again in motion. The blackness of the face gradually rose upward from the chin, like the removal of a black veil.

My feet were placed in warm water, a mustard poultice applied to the stomach, and brandy and water placed to the mouth (in a teaspoon) which I could easily swallow. I remained totally insensible for five or six hours after the operation, and then very gradually aroused, but was not aware of the operation until the next day.

I was surprised at the number of strange faces around me, and supposed, from appearances, that I had been very ill, but the enjoyment of breathing with perfect ease, after such intense suffering, was so great a luxury that it rendered me perfectly satisfied with my situation, with but little inclination to make inquiry. We had no further trouble until the next day (Sunday, the 9th,) in the evening, when the orifice became clogged with mucus, and the parts were highly inflamed. I was soon relieved by the forceps opening the orifice. In the absence of Dr. Pancoast, his friend, Dr. Mutter, arrived, who immediately ordered fifteen or twenty leeches applied to the throat, and a medicine called "neutral mixture," a tablespoonful to be taken every hour. My pulse had run up to 130, but by the next morning the fever had abated and I was doing well. The orifice was so constantly clogged with the mucus as to require instant watching, and I am greatly indebted to the young gentlemen in Dr. Pancoast's office for their disinterested kindness and attention day and night.

For the first three days I took no nourishment but mineral water. Milk punch was found to excite my pulse too much. I was then allowed arrow-root for several days, with lemonade and weak claret. In about three days after the operation, Dr. Pancoast applied the sponge attached to whalebone, soaked in nitrate of silver, ten grains to the ounce. It was very severe, on account of my weakness. This application was made once or twice a day, and afterward increased to twenty grains to the ounce.

On Friday, the 14th May, six days after the operation, I again began to suffer for want of breath, as the orifice had begun to heal so rapidly that the passage was nearly closed, and I had no power to take breath through the natural passage. Up to this period we had hopes that a cure might be effected without the tube. Dr. Pancoast reopened the orifice and inserted the tube, when I was instantly relieved. During the second week my throat was swabbed in the morning with nitrate of silver, and in the evening with glycerin. A tablespoonful of syrup sarsaparilla and hydriodate of potash was taken three times a day. This was continued until the fourth week, when a solution of lapis divinus was applied with the sponge and whalebone: the latter at noon, and the others as before.

After three weeks of confinement to my bed, I sat up a little at a time each day, and was able in the fourth week to ride and walk out. My appetite was very good, and from light diet I gradually advanced to the most hearty food, and have not eaten or drank anything which appeared to do me any harm. Near the expiration of the fifth week I was able to go to Providence, in company with Dr. W. D. Southall, of Richmond, Va., who has been constant in his attention to me, and to whom I feel under lasting obligations for his indefatigable kindness from the day of the operation until my case was resigned to your hands.

After my arrival in Providence, you continued for the first two weeks (which were the sixth and seventh weeks since the operation) with precisely the same treatment as that of Dr. Pancoast, in Philadelphia: in the morning a swab of nitrate of silver, twenty grains to the ounce; at noon a swab of the solution of lapis divinus; and at night a gargle of the glycerin. I also continued a tablespoonful of the syrup sarsaparilla and hydriodate of potash three times a day.

The seventh week, the strength of the nitrate of silver was increased to thirty grains to the ounce, and an alterative pill of biniodide of mercury.

The eighth week continued the same, with the exception that we tried the lapis divinus in the morning in lieu of the nitrate of silver, and found it more effectual.

The ninth week continued the lapis divinus in the morning, and at noon and night a gargle of cod-liver oil, continuing the pill every three or four nights, and taking half a tablespoonful of syrup of sarsaparilla and hydriodate of potash three times a day.

The tenth week continued the same.

I have taken great care not to expose myself to the weather. I have particularly avoided the night air, and in damp weather been carefully protected. Residing in the country I have found beneficial, but of all the remedies, I have found none that appeared to do me more good than breathing the pure *salt air*. My lungs are perfectly sound, and consequently did not find it too bracing, but each respiration seemed to invigorate and refresh me.

Experience has proved that a crowded room would do me great injury. On one occasion I entered a large church, which was about two thirds filled, and although sitting near an open window for about two hours, my exhaustion was so great on going out, that I was scarcely able to inhale a breath for several minutes. Walking faster than the ordinary gait, walking up a hill or up stairs, produces a shortness of breath and wheezing in the throat. I have an excellent appetite, and have no difficulty in eating or drinking, nor has anything taken into the stomach done me the least injury. I am disturbed but two or three times in the course of a night by a single cough, which discharges the mucus through the tube. I find it of the utmost importance that my mind should be kept perfectly calm and free from excitement. Smoking a cigar I find very soothing to the throat. There is a sufficient quantity of air passing up through the natural passage, to enable me to smoke as comfortably as ever.

I feel under great obligations to Dr. Pancoast for his promptness and skill in the operation which raised me from the dead, but nothing that I could say would add to his exalted reputation. His kindness will ever be remembered.

I will say a few words about the *tubes*, as my experience may be of benefit to others who are *blessed* in the same manner as myself. My first tube was inserted on the 14th day of May, about one week after the operation, and gave me immediate relief in breathing. It was only intended to be temporary, as there was no orifice in the back of it, to allow the passage of air up through the trachea. In the course of a week I had another tube made of the same diameter and length, with an orifice in the back, and by placing my finger over the mouth of the tube, and allowing my breath to pass through it and the orifice, I was enabled to speak so as to be understood by all around me. I soon found, however, that although the orifice was effectual

in producing sound, it was so large as to take in the fleshy parts, which brought on bleeding and soreness, and in a few hours the tube would be so completely clogged as to require to be taken out. A third tube was made, but through mistake it was only three-fourths the size of the others, and I found it too small for breathing purposes.

I have tried various experiments with the size and position of the orifice in the back of the tube, and found that it would not bear to be more than two and a half lines long and one wide, and an oval shape was preferred; it should be cut about three-quarters of an inch from the plate. The fourth tube was made in this manner, and answered very well.

The opening in the throat has now so much healed, that I find it utterly impossible to breathe without the tube, and a duplicate of the fourth tube was made to be inserted as soon as the other was taken out. These I have been enabled to wear, only changing them night and morning. I place a piece of linen cloth, of the size of the plate, upon the back of it, covered with sweet oil or cod-liver oil, which prevents the plate from chafing the throat.

When the tube was first entered, I could only sleep well by lying upon my back. By turning upon either side, the clicking produced a hacking cough. This, however, was soon overcome by habit and experience, and I now find no difficulty in lying in any position.

A cough is often excited in changing the tubes, occasioned by the loosening of the mucus around them. The cough is immediately relieved on the discharge of the mucus. Some care is now necessary in inserting the tubes, in consequence of the healing of the orifice in the throat, it having diminished in size. I consequently prepare the duplicate tube, and have it ready to insert immediately after extracting the other, and before inhaling a breath, when the orifice is completely open. Having inserted the end of the tube, and waiting a few seconds for the spasmodic action to subside, it will pass home without any difficulty. If the orifice in the throat should be closed before the duplicate tube is inserted, I use the small silver forceps, which you designed for the purpose, and which I have constantly with me, as in case of accident they will afford relief until assistance can be obtained. These will easily enter the orifice and distend it, that the tube may enter, as well as if it had not been closed.

The tube may be easily cleaned by soaking it in water for a few minutes, when the mucus is softened, and a piece of whalebone and rag are passed through it. Great care should be taken that it be not bent or unsoldered; and I would here advise that all similar tubes be flanged upon the front side of the plate, and then passed through it when it may be soldered. My tubes are simply soldered upon the back of the plate.

On one occasion you will remember having been sent for, as the tube would not move all the way in, when you discovered that it had become unsoldered, and was merely hanging by a thread.

By placing my finger over the mouth of the tube, I am able to converse without pain or inconvenience, except that a strong effort is required, which would render a long conversation very fatiguing.

You have now a full history of my case and my experience, and I trust as my general health is now perfectly good, that with *your* usual kindness and attention, together with a due amount of patience and perseverance on *my own* part, we may yet be able to overcome the difficulty.

I remain, dear sir, yours very respectfully, W. E. H.

Providence, July 24, 1852.—*Boston Med. and Surg. Journal.*

Holland Gin as a Medicine.—In our last number we accompanied the publication of a circular on this subject, from our fellow-citizen Udolpho Wolfe, Esq., with a brief commentary, expressive of our own views. Since then we have been employing this agent, and thus far with favorable results. But we are in the receipt of several communications on the subject from medical men, which serve to show that Mr. Wolfe's Aromatic Schiedam Schnapps is very extensively in use, and, in the hands of physicians, is proving itself, as a stimulating diuretic, to be eminently successful, after other medication with this intent had been tried in vain. In one of the cases thus reported, abdominal dropsy has been cured, and the necessity of tapping averted; and in another, a distressing case of gravel, so called, has been entirely removed by the passage of a calculus of considerable size, which is ascribed to the use of only two bottles of this article.

We know not the object of Mr. Wolfe in designating his preparation by the singular uneuphonious name of "*Schnapps*," nor of his denominating it in his advertisements, the "concentrated Tincture of Juniper," instead of perpetuating its ancient title of Holland Gin. It is true that he admits it to be nothing else than the latter article in its pure state, unadulterated by noxious drugs, and hence he contradistinguishes it from Gin of commerce, nearly all of which, as is well known, is manufactured here and elsewhere from inferior whisky, and refuse drugs. The name he has given it, however, may serve the purpose of designating his article, as prepared exclusively for medical purposes, and thus commend it to physicians, for whose convenience it is on sale only by reputable druggists and apothecaries.

As respects its medicinal and curative effects, we understand him to claim only that it is a pure and reliable article of Holland Gin, and as such worthy of the confidence of physicians, in those diseases for which they are wont to prescribe it, and have hitherto only been restrained, by finding it impracticable to obtain the article in a pure state. Nor should any prejudice against alcoholic medicine deprive the afflicted of the benefit of this article, which from time immemorial has held its place among the remedial agencies of the *materia medica*, if it be found worthy of confidence by continued experience. At all events, those who persist in the employment and toleration of other alcoholic medicines, as tinctures, bitters, &c., and especially those who prescribe gin under any circumstances, must all unite in giving the preference to a pure article over the manifold adulterations so rife in the market. Mr. Wolfe liberally supplies physicians with a sample bottle for analysis and trial, as set forth in his circular, and stakes the reputation of the remedy upon the innocence, safety and efficiency of his Holland Gin, when used under medical advice; and pledges his own character in business that the article will not disappoint any who use it.

We shall take occasion hereafter to discuss the whole subject of alcoholic medicine, a topic which has recently been attracting much attention in Europe and America.—*New York Medical Gazette.*

Snoring Prevented by Excision of the Uvula. By THE EDITOR.

CASE. A. D.—, about five years of age, had for two or three years suffered from considerable enlargement of the tonsils, which impeded respiration so much during sleep, as to cause him to snore very loudly, and to seem

to be on the point of suffocation. About a year ago, I excised both tonsils, after which the respiration was very much improved, and the snoring nearly ceased. In March last, his respiration during sleep had become as bad as ever, and his parents apprehending that he might actually suffocate, again requested medical aid. Upon examination, I found that the tonsils had again become somewhat enlarged; that the uvula hung flabbily between them and rested upon the base of the tongue, and that this state of things taken in connection with the natural, yet extraordinary smallness of the bucco-pharyngeal aperture, was sufficient to account for the impediment in respiration. It should be remarked, however, that, although the uvula appeared flabby, it was not paralyzed, for it would sometimes retract spontaneously, and always do so when touched with an instrument.

As the tonsils projected but slightly beyond their proper limits, and their further excision was very difficult, if not hazardous, in consequence of the smallness of the mouth and extreme narrowness of the throat, I resolved to try the effect of simply clipping off the uvula. The child has not snored since, and has from that time slept without any impediment in his respiration.

Would it not be advisable to resort to this simple operation for the relief of snoring in adults? It is certainly worthy of trial, and might add very much to the comfort of those who are annoyed by a snoring bed-fellow.—*Southern Med. and Surg. Journal.*

Influence of Climate upon Consumption.—The value of a removal to the south, of persons affected in the northern states with consumption, has been heretofore very generally admitted; but it is now asked whether much, if any, advantage is to be derived from spending merely the winter months at the south and returning to the north in the spring—and it is added that if a temperate atmosphere be all that is needed, this may be obtained in New England by means of a well-regulated system of artificial heat. We believe it to be an error to suppose that the southern states owe their immunity from phthisis pulmonalis alone to the mildness of their winters. If such were the fact, all temperate climates ought to be equally exempt, and all cold latitudes alike unfavorable. Yet phthisis is much more common upon the seaboard and in the mountainous districts of the southern states than at intermediate points, and it is comparatively rare in the northern portions of Canada and Russia, whilst it makes frightful havoc in milder England, France and our northern states.—*Southern Med. and Surg. Jour., Editorial.*

Lupus cured by large quantities of Cod-liver Oil.—L'Union Medicale relates, on the authority of another French Journal, a case of lupus which was admitted into the hospital of Ghent; the disease had attacked the face and chest, and was of long duration. The patient at first took half a pound of the oil twice a day; this quantity was gradually increased to three pounds daily, with occasional interruptions when it disagreed. Generous diet was at the same time allowed, and the ulcerated spots were touched with tincture iodine, lemon-juice and nitrate of silver. In about seven months the cure was complete. About 265 pounds of the oil had been taken.—*New York Medical Times.*

Colored Medical Students at Paris.—Professor Eve, of Nashville, Tenn., relates the following anecdote in a letter from Paris:

“A few days before leaving Paris, we had quite a *nice* little professional anecdote to occur in one of its hospitals. France is essentially democratic, however she may tolerate a despotic ruler. All classes of society, and all *colors*, too, mingle freely there. Among the students of Velpeau, is one as black as can be, who observing a South Carolinian recently arrived, took peculiar and persevering pleasure in exhibiting the interests of the great *charité* hospital, much to the annoyance of our young countryman. During an operation, the negro asked him where he was from. Charleston, South Carolina, was the reply; when the black promptly observed, ‘Oh! ah! then we are *fellow-students* and *fellow-patriots*; for I am from Boston, Massachusetts.’”—*Nashville Jour. of Med. and Surg.*

EDITORIAL DEPARTMENT.

Medical life in London.—We have had occasion heretofore to allude to the habit, which has prevailed of late years in this country, among writers in medical journals and speakers at medical gatherings, of disparaging the moral and educational standing of American practitioners. The ranks of the profession, it is said, are filled by persons poorly educated; our medical schools are spoken of with contempt; the tendency to quackery, both within and without the profession, is a constant theme of querulous complaint. In all these particulars a comparison with other countries, of American medicine, is frequently drawn, in which the latter is made to appear greatly inferior. This habit of self-depreciation has become so confirmed, that whatever may be the progress made on this side of the Atlantic, many, it is to be feared, under the influence of ideas that have been so much reiterated, will never have courage enough to feel a national pride in whatever may be accomplished. A disposition to undervalue, in anticipation, any discovery, or improvement, originated in this country, is often apparent. An extract from a foreign journal containing something new proposed by some one wholly unknown at this distance, not infrequently passes current, or attracts attention, while a novelty, from a responsible source, which labors under the disadvantage of being a home production, may not only be overlooked, but, as it would seem, studiously kept out of sight. For example, a practitioner of long experience and high standing, announces a new method of reducing dislocations of the hip joint, without the aid of pulleys, etc. He adduces several cases in

which the plan has been successfully tried. It excites some attention, but chiefly by those who are anxious to prove that the author borrowed the idea of some one else. By some journalists, and surgical teachers, the subject meets with no notice whatever. They are not willing even to make trial of the plan, and considerable effort is necessary to bring it sufficiently before the profession, to secure a sufficient number of cases for a fair experimental test of its merits. Even the great discovery of the employment of anæsthetic agents in surgery, has met with an active opposition at home, which it has not had to encounter abroad.

With the low estimation of American medicine which appears to be cherished to a considerable extent among ourselves, it may contribute somewhat to a higher grade of self-respect, to read what the profession of other countries say, not of us, but of themselves. With this view, we quote a few paragraphs from a series of articles which have lately appeared in the *Dublin Medical Press*, headed, "Medical Life in London." Speaking of medical students the writer says:

"We said enough before, perhaps, as to the very insufficient education that medical students receive before they join the classes; it is not difficult now to perceive how likely they are to be misguided as to the true and noble calling in which they are embarked by the ridiculous books and essays which fall in their way, the product of the overgrown trade in these commodities. Accordingly, one meets them constantly at Guy's, at King's, and Bartholomew's, arguing, even with the chief men there, as to their 'convictions on mesmerism,' the marvelous cures they 'had read by homœopathy.' The trite creed of too many of these young men, that all 'physic is humbug'—*their* knowledge of physic no doubt being very nonsensical—is due to the utterly absurd books they had been reading, the result of this frightful trading principle of the book-trade, and the favoritism shown to special authors who happen to have money. * * * * * In London, the student sees the uneducated chemist and the College of Surgeons the only people making fortunes. He votes physic a bore; the College of Physicians, like the Court of Chancery, a great professional incubus to be avoided; advancement in professional life as impossible as the discovery on his own private account of perpetual motion. He knows half the money he has lost would get him a commission in the army, or set him up in a cotton-mill. He has heard of Apothecaries' Hall; but on looking into the books he thinks he might as well try to learn Dutch and Sanscrit, as Dr. Lindley's big words; he never does learn them, for he never learned Greek or Latin; he has been reading novels and the book about Egypt, and the salt-cellars, or the latest rubbish sent 'with the author's compliments' to his hospital library. Three years, four, and sometimes five, he spends in this mortifying way. In October, he comes up, like all his fellow-geese, poor fellow, to be plucked, and to hear the introductory lectures, at which he is told his profession is all pleasantness and all its paths peace, that he has only to follow the directions of each particular lecturer to make a solid fortune, and gain a commanding corner in the Temple of Fame.

If now he runs away without diploma or certificate, and sets up a chemist's shop, he is safe; if he waits for academic honors and College of Surgeons' *soirées*, he will rue it all the days of his life. Godfrey's cordial and chemicals carry the day; or if the trade in homœopathicals promises better, he has no scruples, for long since he has decided 'physic all humbug.' Even Dr. Pereira, who is considered a Jew, and who, one would think, should make money, if any body did, out of medicine, is of opinion all English physioc is nonsensical."

Of the profession he adds:

"We have just had a meeting of the Provincial Medical Association at Oxford; but with such a disjointed and dissociated mass as the profession in London, one looks in vain for anything very enlivening at these gatherings. If one could read in the signs of the times, or in any point of the professional zodiac, of a complete sweeping away of all present overgrown abuses, then might one indeed breathe freely the open air of Heaven. Like the reform in its next-door neighbor Chancery, brought about by the pen of honest, thoughtful men like Dickens, perhaps other parts of Lincoln's-inn-fields will yet undergo some change for the better; and when the pepper-boxes of the National Gallery at the West-end of London, the not very captivating or odorous abuses of the College of Physicians also next door, and two or three quack hospitals, are also removed, we may yet sing pœans of thanksgivings to what Mr. D'Israeli calls the 'spirit of the age.' A member of the College of Physicians of Dr. Paris, may be sent to Newgate if he perpetrates the crime of consulting with the president, or any member, of any Dublin or Edinburgh College of Physicians, or any other M. D. 'whatsomnever;' but they have been known to send nice three-cornered notes, appointing nice three-cornered hours to meet fashionable homœopaths. At the *soirées*, also, one sees emblazoned in all the papers, the sorriest kind of tuft-hunting is had recourse to, and strangers of Rome, 'Cretes and Arabians,' any body and every body, but exactly those for whom one would think royal colleges, medical and surgical, were erected, are invited; the pleasant ultimate result of all such corporate bodies here being that all struggling medical men might as well have a millstone around their neck as the excruciatingly absurd care and patronage of these big buildings. With money, of course, young medical men in London, will make a fortune. A slender apprenticeship to quackery, however, is as indispensable as kid gloves at the College of Physicians' tea parties. Any man in Ireland with £1000 to purchase a practice or partnership, with energy and ability to work, in the present rush of surgeons to the gold diggings, would make a very favorable percentage on the outlay, and bring up his family respectably. Let him think, only, of the presidents of the two colleges, as fabulous people, like Mopsus and Corydon; his diplomas as so much waste paper; the Hunterian oration every year as some blissful condition of things in the book of the Crusades; the medical weekly journals not so much paladins as pump-borers, pumping every man they can, and then throwing him one side as useless. If the man wishes to be happy and contented, and live among his patients, he will sedulously avoid all and each of these. If he is a quack, it is painful to repeat again, he is sure of a fortune. If he is honest, the millstone of the journals and colleges will be his destruction."

Again:

"The practice of physic and surgery in the hospitals is unexceptionable. The moral influence of the colleges and press out of doors, the most melancholy sham; quackery and trade existing in every department of the profession; but perhaps the lowest and the highest, in the court and highest circles, and in the daily drudgery of the lowest or union practice among the poor, the experience of every disinterested man is against quackery; but the homoeopathist or the traveling charlatan, when he makes money enough of his wares, goes up to the College of Surgeons and gets his diploma; the chemist boldly advertises for a medical student to prescribe over the counter; the College of physicians makes it impossible for any but such juveniles to make money, and they and the chemists wax fat on the credulity of the people. Cunning and incapableness, coroners' inquests, and Hunterian orations, white kid gloves, and flunkeyism, all going to make up the staple—the web and woof of English medical practice in this happy year of our Lord, 1852."

We cannot, of course, vouch for the correctness of the representation of medical life in London, as given in the above quotations. With respect to this point it is not to be forgotten that London and Dublin are different places, although situated in Great Britain. How much of the spirit of the articles referred to is due to rivalry of location, we cannot presume to say. We have given the quotations in order that those of our readers who would be glad to think better of medical life in their own country than they who appear to have a fondness for disparaging it, may be encouraged by the fact that in the great English metropolis, a writer on the spot finds as much scope for animadversion and ridicule, as the warmest advocate of American inferiority could claim in behalf of the medical profession on this side the Atlantic.

Operative Surgery, illustrated. Containing more than Nineteen Hundred Engravings, including Two Hundred Original, and Fifty Colored Drawings, with Explanatory Text. By R. U. PIPER, M. D. Also a Chapter on the use of Ether in Surgery. By H. I. BIGELOW, M. D., Prof. of Surgery in Harvard University. Boston: Ticknor, Reed & Fields. 1852. 12mo, pp. 384.

The plan and general scope of this work are set forth in the following extract from the preface: "A picture often presents to the eye, at a glance, more satisfactory and precise information than a protracted description. With this belief, and with a desire to make existing knowledge available and cheap, rather than to enlarge its boundaries, the present work was undertaken. It is not designed to be a methodical treatise, but rather a general storehouse

of matters connected with practical surgery; and although it really contains ample illustrations of all the regular operations, both of the theatre and the dissecting room, the surgeon will also find here a large amount of useful and practical information, upon a wide range of collateral subjects connected with surgical manipulation."

The work seems to us to be one which will prove highly acceptable to medical students, and to practitioners who are called on (as most practitioners are) to make surgical operations. It is filled with beautiful engravings representing every variety of surgical manipulation, instruments, bandaging, etc. As the author remarks in the quotation just given, a picture often conveys a more correct idea, and more quickly, than verbal descriptions, however vivid. Moreover, in this work is furnished, at a small price, a host of illustrations selected from numerous treatises which, from their cost, are not within the reach of a majority of either students or practitioners. The getting up of the work is admirable, being in the best style of Boston typography. The author, Dr. Piper, has been at great pains in its preparation, and we hope his labors will be rewarded by a prompt and extensive demand for it.

Practical Chemistry, a branch of Medical Education; considered in a brief letter to his class. By ALFRED L. KENNEDY, M. D., Lecturer in the Philadelphia School of Chemistry, etc., etc.

The object of this letter is to advocate the importance of a greater relative attention to practical chemistry in medical schools. The writer thinks that the medical student should devote a portion of the period of his attendance at medical lectures, to *working* in the laboratory under the direction of a "demonstrator in chemistry." There can be no question as to the superior value of this plan of instruction, nor as respects the advantage of giving more attention to chemistry than is generally done by medical students. It is clear that from organic or animal chemistry we are to look for many of the important revelations which are hereafter to enrich medical science. Investigations hitherto, for the most part, have related to the *effects* of disease — the changes produced in the solid structures. With the revival of humoralism (which is mainly owing to the labors of the chemist,) and the progress of analytical researches pertaining to the fluids in health and disease, we may hope to attain to a better knowledge of the intimate nature of the relations of morbid actions. There is certainly no province of medicine which promises richer fruits than this to reward the efforts of genius and industry.

The Physician's Visiting List, Diary, and Book of Engagements, for 1852. Philadelphia: Lindsay & Blakiston, 25 South Sixth-street, above Chestnut.

The idea of a book for the convenience of physicians in noting their daily visiting list, engagements, etc., is an excellent one. The plan of that issued by Lindsay & Blakiston, however, is susceptible of improvement. The spaces for noting daily visits are too small. It is often desirable to state the person visited and the nature of the service rendered. The copy which we have used for the past year, allowing twenty-five patients per week, is too restricted. We reckon there are few city practitioners in extensive practice, who do not have to make charges against more than this number. It strikes us that it would be better to appropriate a page to each day. This would leave room for everything relating to the business of the day which it would be desirable to note. To render the book less bulky, a considerable portion of the extra pages might be dispensed with. At present, we know no better, nor indeed any other book than this designed for the same object, and we accordingly commend it to the favor of our readers. It contains an almanac for the year; a posological table; a list of poisons and their antidotes; and an abstract of the proceedings of the last meeting of the American Medical Association. These will be found useful for reference.

New Work on Materia Medica and Therapeutics. — The Boston Med. and Surg. Journal states that it is proposed to publish, by subscription, a work on materia medica and therapeutics, by Prof. W. Tully, of New Haven, Ct. Our cotemporary enumerates the peculiar features of the work as follows: "*First.* It will be original, having none of the characteristics of a compilation. *Second.* It will contain a large amount of practical information not found in ordinary books on the subject. *Third.* The powers and operations of medicines will be described with minuteness and precision. *Fourth.* The doctrines it inculcates are not speculative or theoretical, but eminently practical. *Fifth.* It will be written with ability and learning, such as would do credit to the profession in any country." The plan of publication is to issue the work in numbers, 25 cents each — four, at least, to be subscribed for at one time. It will be printed in double columns, with good type and paper, and in the best style. The Nos. will be issued on the first

of every month, commencing in November. Subscriptions will be received by Dr. J. Church, Springfield, Mass.

Quackery and the Newspaper Press. — It is not often that we find instances of the effrontery of quackery receiving its just deserts at the hands of the newspaper press. The following notice, which we cut from the *New York Freeman's Journal*, is therefore a curiosity worth preserving. If editors, generally, would adopt the same upright policy, society would be much the gainer:

“ Dr. J. X. Chabert, Professor of Chemistry, &c., sends us a pamphlet just published, containing his ‘ Observations on the origin and treatment of Cholera,’ with certificates of various cures professed to have been performed by a specific of his preparing. In sending this to us, as we are not a Doctor in medicine, Dr. Chabert seems to desire of us an *extra-professional* notice or opinion. This notice, therefore, cannot be on the medical merits of his pamphlet, but on the aspect in which it is presented. We must say, then, that he pretends to cure cholera by a specific or mode of treatment which he keeps a secret; and that doing this is the thing known as *quackery*, and that quackery is an unprofessional, and is considered a not very honest nor reputable way of making money, and seems a violation of the oath taken by every man that obtains the degree of M. D. in any of the medical schools of this country. Being non-medical ourselves, this is all we are competent to say on the subject.”

Transactions of Medical Associations.

Transactions of the Medical Association of Southern Central New York, at the Annual Meeting, held at Owego, June, 1852.

The Transactions of this Association for the present year, embrace: 1st. The annual address of the president, Dr. James H. Jerome, of Trumansburg. The subject is Quackery, which, considering that the theme is rather hackneyed, is well handled. 2d. The vital statistics of Cortland county, by Dr. Caleb Green. This report contains meteorological tables for the year, and a succinct account of the prevailing diseases during that period. Similar reports, from different parts of the county, would supply data for important deductions. 3d. Vital Statistics of Owego, by L. H. Allan, M. D. 4th. Abstract of report of committee on Surgery for Cortland Co., by F. Hyde, M. D., chairman. This, like former reports by the same author, is an interesting

paper. 5th. Surgical report of Tioga Co., by R. O. Crandall, M. D. 6th. Report of committee on Surgery for the county of Broome, by Geo. Bun, M. D., chairman. This report contains an interesting case of fracture of vertebræ.

In addition to the foregoing, the volume contains reports of cases by Drs. S. E. Clark, N. R. Derby, T. H. Squire, J. H. Jerome, and Wm. C. Wey.

Our readers will perceive by this brief notice, that the Association of Southern Central New York, is sustained with unabated zeal. The example of our brethren in the counties over which the association extends should excite an effort to institute similar organizations in other districts throughout the empire state.

Transactions of the Medical Association of the State of Missouri, at its Second Annual Meeting, St. Louis, vol. II, 1852.

This is a volume of 116 pages, embracing communications and reports on various subjects. The association is of recent formation, but the volume before us gives promise of success and usefulness. Dr. W. M. McPheeters, of St. Louis, is president of the association.

Transactions of the Illinois State Medical Society for the year 1852.

This makes a volume of 94 pages, and contains some valuable papers, among which is an interesting summary of the contributions to practical medicine, by Prof. N. S. Davis, chairman of the committee. The society, although of recent formation, is in a flourishing condition.

The Transactions of the Third Annual Meeting of the Medical Society of the State of Georgia, held in the city of Augusta, April, 1852.

This is a volume of 100 pages. Among other papers of interest, it embraces a report of twenty-five cases of urinary calculus, in twenty-three of which the bi-lateral operation was performed, by Prof. Paul F. Eve; observations on the use of certain new remedies, by Prof. L. A. Duges, and an able address by H. F. Campbell, first vice-president of the society, on "the difficulties and the privileges of the medical profession."

The several volumes which we have briefly noticed, furnish evidence of an increasing disposition among members of the profession in different sections of the republic, to secure the benefits of union for professional and scientific objects, and they show a degree of activity and spirit pervading the profession which it may be confidently expected will lead to happy results.

Observations on the Origin, Treatment, and Cure of Asiatic Cholera and Cholera Morbus, etc. By J. X. CHABERT, M. D., Prof. of Chemistry, &c.

Allusion is made to this pamphlet in another article. Since writing that article we have been favored with a copy. The author proposes to sell the secret of a preparation which he asserts to be an infallible remedy for cholera. He affixes an M. D. to his name. Whether he has any right so to do, or not, we cannot say; but a perusal of the pamphlet will satisfy any medical reader that he is essentially a quack. We have observed several articles in a contemporary medical journal by the same author. After the publication of such a pamphlet it is to be hoped he will cease to be recognized among respectable contributors to legitimate medical literature.

The Duties of Medical Men to themselves, and to their Profession. An Address before the New Hampshire Medical Faculty. By the President, Prof. E. R. PEASLEE, A. M., M. D. Published by order of the Society.

This, like all the productions of Prof. P., displays the scholar and the sound thinker. We have read the address with pleasure and profit.

“*The Practice of Allopathy.*” — We fully concur with the editor of the New Hampshire Med. Jour., in the following remarks. Among the charges alluded to, this Journal is included, as the reader will notice by referring to the advertising sheet. The latter, however, concerns only the business department, and does not come under editorial supervision: *

“We notice in several of our exchanges an advertisement of a large drug house with this caption, and take the liberty respectfully to call the attention of editors to it. It seems apparent that to characterize the practice of medicine as *allopathy*, is to place it on the same ground as homeopathists and other *thists*; and, for ourself, we are not willing thus to be classified. We recognize but two classes among *doctors*, namely, physicians and followers of *systems*, as they are called. Claiming rank then with the former honorable body, we will not receive a title which shall confound us with the latter.”

* Since this article was in type we have observed that the heading of the advertisement referred to is altered by direction of the advertising house.

Medical life in Philadelphia.— In the following extract from the Philadelphia Medical and Surgical Journal, Dr. Bryan, the editor, gives a sorry picture of the condition of medical matters in Philadelphia. There is some comfort in the reflection that in most parts of the country the practice of medicine is in better estimation with the public:

“Two-thirds of the practice of medicine in Philadelphia, even among those who pay for everything else, is done gratis; and not unfrequently insult is added to injury. This is due to the crowded state of the profession, and to the *mock charity* inculcated by many of the public teachers. The average fee in our city for medical attendance *paid*, does not exceed *twenty-five cents* a visit. This is one great reason that quackery so thrives among us. Perhaps there is not a city in the Union where quackery, when, as it often is, is combined with talent, builds such large houses (vide the palace to Moloch, on Chestnut street,) as among the staid and demure denizens of the City of Brotherly Love. The getting of fees is a kind of game in which the honorable and upright are fleeced to pay for the mean and contemptible. *Vive la bagatelle.*”

Foreign honor conferred on an American Surgeon.— The honorary fellowship of King's and Queen's College of Physicians, in Ireland, has lately been conferred on Professor Valentine Mott. The honor of this distinction, measured by the number of persons on whom it has been conferred, is marked. It is stated that since the foundation of the college, nearly two centuries ago, but twenty-five medical men, of different countries, have received this degree — Prof. Mott being the only American.

Parsimony of Publishers.— The following article, with the above caption, is from the N. Y. Med. Gazette. In common with the Med. Examiner, we received an order for Bernard & Huette's work on operative surgery, which, the writer of the order was at pains to designate, was to be *plain*. We immediately returned the order, declining to receive it.

We do not entirely concur with the editor of the Gazette in the concluding paragraph of his article. The *American* publishers of the London Lancet, belong in the same category:

“The last number of the *Medical Examiner* contains a ‘palpable hit’ at the publisher of the two numbers which have appeared of Bernard and Huette's Manual of Operative Surgery. The reviewer complains that the copies sent to the editors of the medical journals are ‘in mourning,’ being without the ‘colors’ to the plates, which they are expected to introduce to their readers. He facetiously remarks that he ‘confesses to some weakness for the

feathers along with the *fuss*, and expects to see a book in the best dress that belongs to it.

"A neighboring contemporary of ours called on this same publisher to examine his stock of foreign books, and was shown a copy of the first number of the work in question, with colored plates. While looking at it the publisher solicited a good notice of it in the forthcoming number of his quarterly, at the same time apologizing for not presenting a copy for the purpose, by assuring him that this usual courtesy had to be withheld, as he could not afford it! The uncolored copy sent to our friend, at Philadelphia, was therefore a stretch of liberality, for which he should be truly grateful.

"Such littleness merits reprobation for its bad policy, and is never betrayed by any *American* publisher. They are wiser in their generation, and hence never begin a work without being able to finish it."

American Medical Association.—At the meeting of the Association held at Richmond, Va., May, 1852, the undersigned were appointed a committee to receive voluntary communications on medical subjects, and to award two prizes of \$100 each to the authors of the best two essays.

Each communication must be accompanied by a sealed packet, containing the name of the author, which will be opened only in the case of the successful competitors. Unsuccessful communications will be returned on application after June 1st, 1853.

Communications must be addressed, post-paid, to the chairman of the committee, Dr. Joseph M. Smith, 56 Bleecker street, New York, on or before the 20th of March, 1853.

JOSEPH M. SMITH, M. D.
JOHN A. SWETT, M. D.
W. PARKER, M. D.
GURDON BUCK, M. D.
ALFRED C. POST, M. D.

New York, September 17, 1852.

Editors of medical journals in the United States are respectfully requested to copy the above.

New Professorship.—We understand that John A. Swett, M. D., of the N. Y. Hospital, has been appointed Professor of Clinical Medicine, in the College of Physician and Surgeons of this city, and will deliver a course of lectures on this department during the approaching session.

We rejoice at this addition to the curriculum of this college, evincing as it does a just estimate of the indispensable necessity of thorough clinical instruction, in every medical school worthy of the name. Dr. Swett is a learned and able teacher, a successful practitioner, and has a reputation at home and abroad which will render him a real acquisition to the faculty of the college.
—*Medical Gazette.*

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BUFFALO MEDICAL JOURNAL

AND

MONTHLY REVIEW.

VOL. 8.

DECEMBER, 1852.

NO. 7.

ORIGINAL COMMUNICATIONS.

ART. I.—*Clinical Report on Chronic Pleurisy, based on an analysis of Forty-seven cases.* By AUSTIN FLINT, M. D.

(Continued from page 253.)

DIAGNOSIS.

It is evident that, so far as its symptomatology is concerned, exclusive of physical signs, Chronic Pleurisy is often remarkably latent in its character, as well as insidious in its development. Symptoms pointing to pulmonary disease, rarely prominent, are frequently quite insignificant, or altogether absent; and when present even in a degree to direct attention to the chest as the seat of the malady, they afford no criteria to indicate the particular structure involved, and the nature of the affection. The rational or vital phenomena, in a large proportion of cases, are insufficient for a positive diagnosis; and, inasmuch as physical exploration is still neglected by a large class of practitioners, the disease, in proportion to its frequency of occurrence, is, perhaps, as often overlooked and confounded with other affections as any in the nosology. The cases that I have collected furnish proof of the correctness of this remark. In a pretty large number of these cases the histories show errors of diagnosis on the part of the physicians under whose notice

they had fallen, and I am by no means assured that pains were taken in noting the history of each case to ascertain and record the facts with respect to this point. The propriety of embracing the facts pertaining to this subject in the present Clinical Report, would be more than doubtful, if they were not calculated to subserve an useful practical purpose. But inasmuch as they tend to enforce the importance of physical signs, it would be as improper to withhold them, as it would be to give them publicity simply as a matter of curiosity.

The number of cases in which errors of diagnosis are noted to have occurred, is eighteen. Of these eighteen cases, in four the disease had been treated as latent intermittent fever. This error was based, in two instances, on the fact that chills were pretty prominent, and in the other cases on the fact that the patients had been exposed to miasmata. In four cases the disease was supposed to be phthisis. In two cases the patients were thought to be laboring under continued fever, and in one case the disease was developed in the course of fever and overlooked. In the remaining seven cases, the supposed affection was in each case different. The following is a list of the diagnostic views that had been entertained: Disease of the heart, abscess between the pleura and walls of chest, bilious fever, hepatization of lung, liver complaint, general debility, and some pulmonary affection the nature of which was confessedly not known.

The diagnosis of Chronic Pleurisy, with the aid of physical exploration, is as simple and sure, as it is difficult and doubtful when the sole dependence is on the symptoms. Flatness on percussion extending from the bottom of the chest, upward, over the whole, or a considerable portion of one side, anteriorly, posteriorly, and laterally, and absence of respiratory sound, would alone lead to a correct diagnosis, with some very rare exceptions. A tumor filling the chest on one side, in part, or entirely, constitutes almost the only morbid condition in which the above combination of signs is presented, exclusive of liquid effusion. Solidified lung rarely, if ever, occasions the same absolute loss of sonority, in the same situation, and to the same extent, coupled with absence of all respiratory sound; and, on the other hand, the presence of a bronchial respiration in cases of large liquid effusion is an anomaly of which but a few instances are on record. But, aside from the above combination, if the liquid effusion be large, as it generally is in Chronic Pleurisy, we have the enlargement of the chest on the affected side, its comparative immobility, the widening and elevation of the intercostal spaces, the displacement of the heart affected in the left side, the contraction of the affected side after the liquid effusion has been removed wholly, or in part,

by absorption, the change of level of the fluid if the quantity be moderate and the pleural surfaces free, a friction sound in some cases, the absence of the vocal fremitus frequently appreciable on the sound side, and occasionally the cegophonous modification of bronchophony. With the assistance of more or less of these collateral signs, the discrimination is easy and certain. Reference is now made to Chronic *general* Pleurisy. Circumscribed pleurisy may involve difficulties in the way of diagnosis to which it will be more appropriate to refer in another connection. To treat of the diagnosis at any length, does not fall within the scope of this Report. After indulging, however, the few foregoing remarks, I may briefly allude to one or two points of practical interest which are incidentally connected with the subject.

Chronic Pleurisy and tuberculosis of the lungs occasionally coexist. This complication does not appear to be of frequent occurrence, but it is desirable to determine the coexistence in individual cases. How is this to be done? I am not prepared to answer this question by submitting conclusions deduced from an extensive series of personal observations. The few facts, however, which have been already presented, involve the practical rule to be adopted. Recollecting this law of tuberculosis, viz, that it invades the lungs successively, on both sides, after an interval of greater or less length, we are to seek on the side which is sound as respects the pleurisy, for the various physical indications of tubercle, such as crackling, jerking respiration, sibilant or mucous râles, bronchophony, etc. If any of these signs are found to be persistent, and confined to the apex of the lung on the nonpleuritic side, they show, as under other circumstances, the presence of a tuberculous deposit on that side. If the deposit be quite abundant, and especially if it have advanced through the changes of softening and excavation, the physical indications will of course be still plainer and more unequivocal. The symptoms, as well as signs, are to be considered in resolving the question under consideration. Hæmoptysis must be regarded as a very significant event. A prominent and persisting cough, more especially if accompanied with a progressively increasing expectoration, is also ominous, since these do not ordinarily belong to the natural history of simple pleurisy. Marked emaciation, and hectic paroxysms should also lead to strong suspicions of tubercle.

Phthisis, if it do not coexist, may be developed subsequently to Chronic Pleurisy. As a sequel it is probably far less frequent in its occurrence than the views of some late writers would lead us to suppose. Of fifty-three of the cases analyzed by Dr. Blakiston, in which the patients were under observation for some years after the attack of pleurisy, not one became phthisical. So far as my observations go, they show a very small proportion of

instances in which this result has followed. That it does occasionally follow, however, is not to be doubted, and the permanent changes in the chest due to the pre-existing pleurisy, offer some obstacles in the way of the physical diagnosis. It is well known to all who are practically conversant with physical exploration, that some of the signs of early tuberculosis are based on the correspondence of the two sides of the chest in a state of health. This natural harmony, which is sufficiently so for practical purposes, is permanently impaired, to a greater or less extent, by Chronic Pleurisy, as will be seen in the division of this Report to be presently taken up. The disparity which succeeds the disease, relates more especially to the resonance on percussion, the mobility, and the comparative development of the vesicular respiration. We cannot determine morbid signs involving the points just mentioned, by a comparison of the two sides of the chest, as we should do had the patient not been affected with Chronic Pleurisy. Under these circumstances reliance in forming a diagnosis must measurably be placed on the same combination of signs and symptoms which have just been referred to in connection with coexisting pleurisy and tuberculosis.

With reference both to coexisting and consecutive phthisis, the study of, and practical acquaintance with all the various indications, physical and rational, of incipient tuberculosis, are eminently important to the physician. It is hardly necessary to add, that the chief object in alluding to the points just noticed, is to show how the disease which is the subject of this Report, affects the importance of these indications.

Another point of practical interest connected with the diagnosis of Chronic Pleurisy, pertains to the conversion of the liquid contents of the chest into pus; in other words, the development of empyema. How is this to be ascertained in individual cases? The data afforded by the present collection of observations, bearing on this point, will be found to be insufficient for many practical deductions; but I may anticipate so far as to say that the persistent evidences of a large accumulation of liquid effusion within the chest, showing little or no efforts at removal by absorption, and the presence of symptoms denoting no progress toward recovery, but a tendency to a fatal issue, should occasion suspicions of a purulent change. This language is comprehensive, and rather indefinite, but to determine the special significance of particular symptoms, it would be necessary to analyze a series of cases of empyema, and compare the results with those attained by the analysis of cases of ordinary Chronic Pleurisy. The materials for the former labor are not at hand. The nature of the liquid contents of the chest, it may be remarked, can be demonstrated, in individual cases, in which information on

this point is desirable, by means of the exploring canula, which the recent experiments of Dr. H. I. Bowditch, of Boston, Mass., have shown may be resorted to with entire impunity, to say the least, for the purpose just mentioned. The withdrawal of the fluid by means of the canula, is another matter which relates to the subject of treatment.

FATALITY.

On examining the fatal cases in this collection, I find that they admit of being divided into several groups, as follows: *First*, cases of death from uncomplicated pleurisy; *second*, cases in which the pleurisy was associated with other grave affections; *third*, cases in which death was due to an intercurrent disease, and, *fourth*, cases in which a fatal termination occurred from causes acting shortly after recovery from the pleurisy. I will state the facts pertaining to the fatality under these several heads.

1. *Death from uncomplicated pleurisy.* The fatality which belongs solely and directly to Chronic Pleurisy, is to be determined by facts falling under this head. Excluding cases in which the issue was unknown, together with a few cases in progress at the present time, and the number of recoveries is *nineteen*. The number of deaths from uncomplicated pleurisy was *four*. Of these four cases the fact of the disease not having been complicated, was autopsically demonstrated in two. The fact was predicated on the history and symptoms in the remaining two cases. Thus, in four of twenty-three cases of Chronic Pleurisy, a fatal result was due, so far as could be ascertained, to the pleurisy exclusively.

2. *Death from pleurisy complicated with other grave affections.* Seven fatal cases are embraced in this category. Adding these to the cases which recovered, nineteen, and the ratio of fatality is as seven to twenty-six. The affections complicating the pleurisy were in three cases tubercle; in two cases fever; in one case laryngitis leading to œdema glottidis and sudden death, and in one case pericarditis, ascites and double pleurisy.

3. *Death from an intercurrent affection.* One patient, a child, died with a bowel complaint, which occurred while the pleurisy was progressing favorably.

4. *Death from subsequent affections.* One patient died shortly after recovering from the pleurisy, with what is said to be *bilious colic*. Another

patient died of a repetition of the pleurisy occurring several months after the recovery from the first attack which seemed to be complete. This case, judged by the symptoms, was complicated with pericarditis. The fact of the fatal termination in this case was never positively ascertained, but when last seen the patient was near the close of life, and it is therefore assumed that it ended fatally.

In the foregoing account are excluded four cases of perforation, all of which ended fatally, and two cases of circumscribed pleurisy one of which was fatal.

Adding together the fatal cases in all the above groups, including the four cases in which there was perforation, and the fatal case in which the pleurisy was circumscribed, and the sum is nineteen. In the same number of cases, nineteen, the patients were known to have recovered from the pleurisy, and in nine cases the termination is unknown.

From the results which have been submitted the general conclusions to be drawn are, that Chronic Pleurisy, *per se*, is fatal in only a very small proportion of cases; that the majority of deaths occurring during the progress of, or shortly after the disease, are due to its complications, or to intercurrent and subsequent affections; but taking into view all the circumstances connected with the origin, progress, and immediate tendencies of the disease, the rate of mortality is about fifty per cent.

DURATION.

Fatal Cases.

No.		Remarks.
1.	Two months after admission into hospital.	Associated with tubercle. Autopsy.
2.	Six weeks after coming under observation. Previous duration not ascertained.	Empyema. Uncomplicated. Autopsy.
3.	Several weeks after coming under observation. Previous duration not ascertained.	Uncomplicated. Autopsy.
4.	Seven days after admission into hospital.	Lesions of typhoid fever. Had a short time before had scarlatina. Autopsy.
5.	Seven weeks after date of attack.	Supposed to be uncomplicated. No autopsy.

6.	Five months.	Supposed to be uncomplacate l. No autopsy.
7.	Forty days.	Patient supposed to have fever. Empyema. Autopsy.
8.	Not ascertained.	Complicated with laryngitis, and sudden death from œdema glottidis. No tubercle., Autopsy.
9.	Two months.	Associated with tubercle. Autopsy.
10.	Not ascertained.	Complicated with ascites, pericarditis and double pleurisy.
11.	Ten months.	Associated with tubercle. No autopsy.
12.	Six months.	Died of what was called bilious colic. No autopsy.
13.	Several months.	Died with bowel complaint while apparently recovering from pleurisy. No autopsy.
14.	About two years.	Died from second attack after having appeared free from the disease for a year. No autopsy.

It is evident that Chronic Pleurisy, when it proves fatal, has no definite duration. When it is complicated with other affections, the issue, of course in a greater or less degree, is dependent on the nature and extent of the latter. When uncomplicated, however, so far as any conclusion on this point can be drawn from the foregoing data, the duration is variable.

Cases not Fatal.

In order to determine with precision the duration in cases ending in recovery, it is necessary to fix, in each case, the period when the processes of restoration are completed. These processes involve absorption of the liquid contained in the pleural cavity, the organization of the effused fibrin, etc. It must be difficult, with all the aid which physical exploration affords, to arrive at exact information on this point. An analysis of a collection of observations relating thereto would be interesting. The histories that I have preserved do not embrace data sufficient for such an analysis. As before stated, in a large proportion of the cases the patients did not remain under

my observation during the progress of the disease; and in those in which the opportunity was offered, pains were not taken to obtain and record the results of repeated examinations of the chest, while convalescence was going on. I have uniformly observed that at the time when, judged by rational signs, recovery was nearly, or quite established, the physical evidences of the presence of more or less liquid in the chest, persisted. In several instances the patients regarded themselves well, and were able to take active exercise, while the quantity of liquid effusion was considerable. I have already cited illustrations of this, and also of the fact that active occupation may be continued from the beginning of the disease during the greater part or the whole of its career. Even many months after apparent recovery, I have found flatness and absence of respiration over the lower third of the affected side. For the completion of the processes of restoration a long period is required. I regret that my recorded observations do not enable me to state the average period with more exactness.

There is another aspect under which the duration may be viewed, viz., from the date of attack to the time when patients are sufficiently recovered to resume their usual avocations. The nature of the occupation, and other circumstances, irrespective of the condition of the chest, it is obvious, will affect the duration in this aspect. I will, however, give the facts in the few instances in which the histories contain information relative to this point.

CASE 1. The patient was a soldier, and remained in hospital thirty-two days, when he was considered by the medical officer, and himself, well enough to be returned for duty. The chest, however, was two-thirds filled with liquid.

CASE 2. Recovered sufficiently to resume the duties of a physician in about three months. For several months afterward he was feeble, and subject to cough. Recovered at length his former health.

CASE 3. Was able in about four months, to engage in tolerably active business, but was short-winded on exertion, and the physical signs of considerable liquid effusion continued. He progressively improved, until he recovered perfect health.

CASE 4. Apparently quite recovered in about six months.

CASE 5. After a few months (precise number not stated) able to be up

and about, but not able to engage in active business for three years after date of attack. During all this time physical evidences of liquid effusion continued.

CASE 6. Patient considered himself quite well in about four months.

CASE 7. Entered hospital a week after attack, and was discharged convalescent in two months. Condition of chest at time of discharge not noted.

CASE 8. Able to be up and walk about freely in about six months. A year afterward reported quite well, and able to do housework, being a female domestic. The quantity of liquid effusion in this case was very large.

It appears from these cases that in Chronic Pleurisy, recovery sufficiently to resume the active duties of life, is not likely to occur within a very short period. Several months, it may be expected, will elapse before the patient will reach this stage of improvement; and complete restoration will require an indefinite period beyond apparent convalescence.

REMOTE EFFECTS.

What remote effects, local and general, follow Chronic Pleurisy? Is the chest apt to be permanently damaged? To what extent, if at all, are the physical powers of the organism usually impaired? Is there a liability to renewed attacks, or is the patient peculiarly exposed to the supervention of other affections? These questions, it is obvious, are to be answered by reference to the results of an analysis of a sufficient number of cases remaining under observation for a considerable length of time after recovery from the disease. The group of cases in this collection in which the diagnosis was retrospective, will furnish some facts pertaining to this subject. In a portion, also, of the cases belonging to the other groups, the disease occurred several years ago, the condition of the patients in the mean time being known. Several of the cases, however, are of too recent date to be studied in this point of view; and in several the patients were lost sight of directly, or shortly after coming under notice. In fourteen of the forty-seven cases the consecutive history extends over a sufficient period to furnish information relating to remote effects. The facts noted with respect to these cases, respectively, are as follows:

CASE 1. In this case the patient had experienced an attack of Chronic

Pleurisy forty years before her death in April, 1847. She was confined to her room with the pleuritic affection for several months, and remained an invalid for several years. The history with greater particularity of detail was not obtained. After recovering entirely from the disease, she enjoyed excellent health until a few years before her death, when she suffered from palpitations and paroxysms of sinking. She had no medical treatment for these symptoms until about a fortnight before her death, when she came under the care of Dr. Wilcox, of this city. I saw her in consultation with Dr. W. She presented the physical evidences of greatly enlarged heart, without valvular lesions. The action of the heart was exceedingly irregular, and accompanied by paroxysms of great suffering from orthopnoea. At the autopsy the heart was found to weigh 16 oz., the cavities greatly dilated, the walls not thickened, and the valves free from disease.

In the right chest, which had been the seat of the pleurisy forty years before, the pleural surfaces were united throughout, with firm adhesions. The pleural cavity was entirely abolished. Lungs on both sides free from disease. Moderate effusion of serum had taken place in the left pleural cavity.

CASE 2. This patient experienced the disease five years ago. After several months he recovered sufficiently to resume the duties of a medical practitioner, but he remained feeble, and was subject to cough for about a year. He has since continued in good health, and is now free from any evidences of pulmonary or other disease.

CASE 3. This patient was supposed, from the history of symptoms, to have had the disease three years prior to the time of examination. She was then, i. e., at time of examination, probably laboring under tuberculosis, and died a few months afterward.

CASE 4. This case occurred a little over four years ago. After a few months the patient began gradually to resume active occupation as an outdoor clerk in a lumber yard. After the lapse of one or two years he was able to endure as much physical activity as the most robust, and has remained up to the present time in perfect health. The quantity of liquid effusion in this case was very large, occurring in the left side, and removing the heart to the right of the sternum.

CASE 5. The patient was a child six or seven years of age. The case

was examined one and a half years after the date of the attack. The only ailment at that time was debility, and deficient growth. Four years after the attack, it had continued well, but was more feeble, and had grown less than the average of children of the same age.

CASE 6. This case came under my notice four years after an attack of Chronic Pleurisy. The patient was laboring under tuberculosis, which, from the history, appeared to have supervened about two years after the pleuritic attack. She died a few months afterward.

CASE 7. This patient came to me for an examination of the chest seven months after an attack of Chronic Pleurisy. There was at this time marked contraction of the chest, with the physical evidences of liquid effusion still at the lower part of the chest. This examination was made about three years ago. The patient has been very feeble for months, and it is not expected that she will live long. She is supposed to be in the last stage of tuberculosis. She resides at a great distance, and the above information is all that I have obtained relative to the progress of the case.

CASE 8. This case came under observation six months after the date of attack. The patient had then quite recovered; the liquid effusion was nearly absorbed, and the chest considerably contracted. It is now three and a half years after the attack. The patient, since the time of my examination, has been in good health, and so continues at this time.

CASE 9. The patient in this case was a lad ten years of age. He came under my notice two months after the date of attack. The chest was then filled with liquid effusion. He recovered after a few months, and has remained up to the present time, a period somewhat less than three years, in excellent health.

CASE 10. This case came under observation a little more than two years ago. The patient had had an attack of Chronic Pleurisy twenty months previous. He presented, still, evidences of considerable liquid effusion. The chest afterward exhibited marked contraction. Since coming under my observation he has had two attacks of hæmoptysis, in one of which the hæmorrhage was pretty profuse, and continued more or less for several days. He has, however, been steadily improving in flesh and strength, and at this time appears in perfect health, being able to engage in active occupation as an out-door clerk.

CASE 11. In this case the patient was examined by me three weeks after the date of attack. The quantity of liquid effusion was then moderate. This was fourteen months ago. He recovered sufficiently to resume the duties of an in-door clerk, but has always remained rather feeble, with more or less cough. On a second examination, a year after the first, the physical signs of tuberculosis were unequivocal. There is ground for the suspicion that the tuberculous deposit preceded the attack of pleurisy. My knowledge of the case was derived at the examinations just referred to. The patient has removed to a southern climate.

CASE 12. In this case I made an examination of the chest about fourteen months after the date of the attack. The patient was a farmer, 47 years of age. There was marked contraction of the chest. There were no symptoms of pulmonary disease present, the only subject of complaint being a diminished ability to perform active labor.

CASE 13. The patient in this case, came under treatment four weeks after the date of attack. This was a little less than two years ago. She has remained under observation up to the present time. The left side was the side affected, and the quantity of liquid effusion was very large, removing the heart to the right of the sternum. Great contraction of the chest followed the absorption of the effused fluid. The patient, a female domestic, for a year past has been able to do laborious house work, and is now in excellent health.

CASE 14. The date of the disease in this case was five years prior to the examination. The left side was still considerably contracted. There existed at the time of the examination, hypertrophy of the heart, to which the symptoms present in the case were referable. From the history it appeared probable that the pleurisy was complicated with pericarditis.

Reviewing the facts contained in the foregoing account of a limited number of cases, it appears that in precisely one-half (seven) the patients preserved for several years after recovery, so long as they remained under observation, or up to the present time, excellent health. In two other cases the health was tolerably good.

In two cases hypertrophy of the heart supervened, but in one of these cases the latter affection was discovered at a period quite remote, at an advanced age; and in the other case the pleurisy was probably complicated

with pericarditis. The cases furnish no evidence of a special tendency, after Chronic Pleurisy, to any disease except tubercle. In *three* cases the subsequent development of tuberculosis was highly probable, although not demonstrated, and in one case the latter disease was positively determined. It has been a current opinion that Chronic Pleurisy predisposes strongly to phthisis. There is reason to think that this opinion is incorrect, having been based on isolated observations. In the analysis by Dr. Blakiston, to which I have before referred, not one out of fifty-three cases became phthisical during the lapse of several years after recovery from pleurisy. This result is striking, for it might be expected that out of so large a number of cases of any disease occurring as does pleurisy, for the most part, at an early age, tubercle would be likely to affect, in the course of several years, a certain proportion. In estimating the influence of pleurisy, or any disease, in determining the subsequent development of tubercle, the liability to the latter, irrespective of preceding affections, is, of course, to be taken into account. Giving to this consideration a certain degree of weight, and bearing in mind that in four of the five cases neither the presence of tubercle subsequent, nor its absence prior to the pleurisy, were demonstratively established, and it follows that the few cases in the present collection analyzed with reference to this point, show but a slight, if any tendency to phthisis to be among the consequences of Chronic Pleurisy.

The remote effects of Chronic Pleurisy upon the chest, so far as they affect the results of physical exploration, constitute an interesting branch of inquiry. For several years after recovery, the symmetry of the two sides, as respects the equality of percussion sounds, and the relative intensity of the vesicular murmur, is impaired. It is probable that this effect frequently continues to some extent through life. A series of careful examinations of the chests of persons who had experienced pleurisy many years previous is a desideratum. The object, however, relates mainly to the subject of physical diagnosis. There is one point to which no allusion was made in connection with the physical signs, which may properly be ranked among the remote effects. This is the gradual expansion of the chest, on the affected side, after contraction has taken place in consequence of absorption of the liquid effusion. I have not taken pains to make careful examinations relative to this point. I have had occasion, however, to observe that this subsequent expansion does take place in cases in which there had been marked contraction. In case No. 4, in the foregoing group, four years after the date of attack, the chest on the affected side had enlarged so as to appear nearly equal in capacity to the other. In this case the quantity of liquid effusion was very great,

removing the heart to the right of the sternum, and contraction subsequent on absorption was present in a marked degree two years after the date of attack. In case No. 13, there also existed very large effusion removing the heart beyond the right margin of the sternum. Absorption was followed in this case by very marked contraction. At the present time, however, two years after the date of attack, the affected side has expanded so that on measurement on a line passing over the nipple it falls short by less than half an inch the dimensions of the right side, the relations of the two sides to each other being about normal. Considerable contraction is permanent in some instances. This was shown in case No. 14, in which it was observed five years after the date of attack. It is probable that age has an influence in determining the degree of expansion. In the case last referred to, the age of the patient is not stated, but he was in the neighborhood of forty years. In the two cases previously referred to the patients were young, both being twenty years of age.

CIRCUMSCRIBED PLEURISY.

The pleurisy was circumscribed in two cases. In one, the character of the disease was ascertained by the symptoms and signs during life, the patient recovering; in the other, the affection was supposed to be seated in the liver, and the diagnosis rectified by the autopsy, the case proving fatal. The circumstances belonging to the history of both cases, considering the infrequency of this form of uncomplicated pleurisy, seem to warrant a brief report of each case.

CASE 1. The patient was a female, fourteen years of age. The case was referred to me for examination by Dr. G. Conger, of Niagara Falls, in July, 1850. At the time of my examination the following were the facts noted: "She menstruates regularly. Aspect is not morbid. She is not emaciated, and has not lost flesh of late. She is highly nervous, weeping at the apprehension of a physical exploration. There is no spinal tenderness. Appetite and digestion tolerable. The friends state that, when an infant, she had hooping cough severely, which was followed by inflammation of the lungs. She has ever since been subject to a cough, loud and hard, and attended usually by small, transparent, frothy expectoration. Five weeks ago she suddenly expectorated a quantity of fetid purulent matter. Since that time she has had several (seven or eight) similar attacks. She continues to expectorate the same matter for a day or two after the attack commences, and then

the expectoration is no greater than usual. The cough has diminished since these discharges took place. She has kept her bed for a week during the past five weeks, and now her strength is regained so that she came to this city without much fatigue.

On examination of the chest I find moderate dullness on percussion over the upper third of the right side, with no distinct abnormal modification of the respiratory sound. Absolute flatness exists over the lower, and most of the middle third, on the right side, with absence of respiration, in front and laterally. Posteriorly, on the right side, the resonance is clear as low as is usual in a state of health. I discover no râles, nor cavernous nor bronchial respiration. There is tenderness on percussion over the right mamma. No tenderness, nor evidences of enlargement of the liver.

From the foregoing signs, and the history, I inferred the non-existence of tuberculosis; and that the purulent expectoration, occurring, thus, at several epochs, was due to circumscribed collections between the pleural surfaces, discharging through the bronchial tubes, rather than to abscesses in the substance of the lung, or in neighboring organs. I have given all the data which were noted, so that the reader may form his own opinion as to the correctness of the diagnosis.

I saw the patient again in March, 1851. She was then quite well. On a slight examination, however, I found that there was still flatness over the lower part of the chest on the right side. She was in good flesh, and had no cough. She had had no purulent expectoration for some time.

I have since been informed by Dr. Conger, that subsequent to the time of my last seeing her, she had another recurrence of purulent expectoration, from which she recovered as before, and afterward appeared in good health.

CASE 2. The patient was an Irishman, a peddler and laborer, aged forty-five years. He came under my care at the Buffalo Hospital of the Sisters of Charity, at the commencement of my winter service, Oct. 1, 1851. He had entered about a fortnight before. The previous history was as follows: Attacked with cough about two years ago, and has had more or less cough from that time to the present. Has had several attacks of hæmoptysis, the first having been early in the disease. Has occasionally had chills, and stitch pains in the chest frequently. Has had diarrhœa for the last ten days, which still continues. Has to-day attempted to sit up for the first time since his admission, and for two weeks previous. Was not attended by any physician before entering the hospital.

Present Symptoms: Not greatly emaciated. Aspect pallid. Pulse 72,

and extremely feeble. Two or three dejections daily. Expectoration copious and purulent.

Physical examination was deferred. The case was presumed to be one of tuberculosis.

Oct. 7, it was noted that the patient had expectorated during the past six hours about ten ounces of pus, and was still raising it freely at short intervals. Physical examination on this day gave the following results: Emaciation not sufficient to render the outline of the ribs visible. Respiration not labored, 24 per minute. Clear resonance at the summit of the chest on both sides; the pitch of sound being somewhat raised on the right side, but the resonance clear. Flatness on the right side extends from the bottom of the chest as high as the fourth rib. The chest on the right side, at the inferior part, *and below*, is tender to the touch. Some degree of tenderness, also, exists on the left side. He refers the pains to the inferior and lateral portions of the right side. The pain is not severe. It is lancinating.

Posteriorly, in the interscapular space, the resonance is clear on both sides, the pitch being elevated on the right side. Below the inferior angle of the scapula, and over the lower part of the scapula, on the right side, flatness exists.

Laterally, on the right side, there is flatness.

The respiration on the left side is exaggerated. Posteriorly, on the right side, the respiratory sound is feeble, tubular, and accompanied by a fine mucous or subcrepitant r le. At the upper third, on the right side, anteriorly it is feeble but not tubular. This obtains above the fourth rib. Below the fourth rib there is absence of respiratory sound, and a distinct, but not loud friction sound heard in both acts of respiration.

Oct. 12. Quantity of expectoration diminished by one-half, or two-thirds according to a rough estimation. Diarrhoea continues to be a troublesome symptom, and the house student thought he had discovered pus in the evacuations. The matter expectorated is pure pus, with little or no mucus.

In view of the foregoing facts, hepatic abscess, evacuating through the lungs, was suspected. The reasons for this opinion are set forth in the following quotation from the hospital records: "From the symptoms and signs in this case, the probable diagnosis is, purulent formation within the liver, evacuating through the bronchi. This presumptive diagnosis is based on the following points:

"1. The profuse purulent expectoration without the physical signs of a cavity in the lungs.

"2. The slight evidences of disease at the summit; consisting only of

slight elevation of pitch on percussion on the right side, the left side appearing perfectly healthy; the respirations not accelerated nor labored; the pulse unaffected; the emaciation not extreme, etc. — all these circumstances excluding tuberculosis.

"3. Empyema being excluded by the absence of the evidences of considerable pleuritic effusion, i. e., no signs that the right chest *has been* distended by liquid effusion, nor that it now exists in much quantity. In this connection it should be stated that distinct bronchophony exists at the angle of the scapula on the right side.

"4. Pneumonitis is excluded by the absence of evidence in the history that this disease has existed; by the improbability of this disease, had it existed, eventuating in abscess; and the absence of the physical signs of a cavity.

"5. The presence of tenderness below the chest on the right, over the site of the liver.

"6. Pus in the evacuations, if this be true. The presence of this symptom needs confirmation.

"7. All the signs and symptoms are rationally explicable on the supposition of a purulent formation in the liver, at the same time that other supposable affections adequate to the production of so large a quantity of purulent expectoration must be excluded."

It will be perceived that circumscribed Chronic Pleurisy was overlooked in canvassing the evidence in behalf of the several affections which might be supposed to be present in this case. The patient died on the first day of the November following, and the autopsy revealed the following condition of the chest:

At the inferior part of the right chest there existed a pleuritic abscess about five or six inches in width, extending from the lower part of the sternum quite around the right side of the chest. The pleural surface in the strip just mentioned, exhibited a rough granular appearance, and the membrane was much thickened. Above the strip the pleural surfaces were united by rather feeble adhesions. The lower lobe of the lung on the right side was solidified. The lungs otherwise healthy. The situation of the pleuritic abscess, or circumscribed empyema, was such as to coincide with the physical signs.

A circumstance mentioned by the patient, although not recorded in the previous history, may, perhaps, serve to explain the production of the circumscribed pleurisy in this case. For some time before being attacked with the disease, he had followed the avocation of a pedler, and had been accustomed

to carry a pack, resting heavily on the right side, and pressing particularly on the lower part of the chest on that side. He stated that it frequently occasioned severe pain in the situation in which he experienced pain during the progress of the disease, and over the site of the pleurisy. The patient referred the origin of the disease to this cause.

(To be continued.)

ART. II. — *Case of Convulsions depending on various Eccentric Causes.*

By S. B. HUNT, M. D., Mendon, Monroe Co., N. Y.

Not from diseases of common and acknowledged type, but from *exceptional* cases, derive we the strongest and most faithful light.

It is not my purpose in placing this case upon record, to add another to the useless histories of fancied anomalies in disease which crowd the pages of our journals — although in duration and intensity of pain, in violence and peculiarity of spasm, it has rarely been equaled — although it had many phenomena of almost inscrutable obscurity — yet these facts alone are worthless compared with the greater fact, that in it are illustrated and made plain many of the deeper mysteries of neuralgic disease.

The reader will pardon me for minuteness of detail, not only of symptoms, but of the various peculiarities of mental and physical organization which characterize this patient; for to those idiosyncrasies must we look for the rationale of symptoms, and the *origo mali* of disease. So strong is my conviction of the influence of mental causes in, (not producing) but in *determining the character* of disease, that I wish if possible to bring to light those influences, and the manner in which they operate.

WILLIAM SHERRY, the subject of this report, is a *farm* laborer, age twenty-one years. His temperament is nervo-bilious. Possessed of a saturnine disposition — a man of few words — slow and dull in scholarship — indifferent in his attachments — he performed his duties as a laborer without hurry or enthusiasm, but faithfully, and mechanically. His brain is neither large nor active, and his eccentricities of character being such as to attract much notice, he has been in some measure the butt of his schoolmates, and the subject of their practical jokes.

With such a cerebral organization, he has the usual accompaniment of a fully developed spinal system. His frame is heavy built and muscular; height five feet eleven inches; weight about 170, when in full health. Though rather slow in motion, he is possessed of more than the average

strength. It will be seen from this description that his spinal system is not so fully under the control of the brain as it should be when there is a proper balance of power between those two great nervous centers.

He applied to me in June last. His symptoms indicated hepatic obstruction, and on examination his liver was found considerably enlarged. The diagnosis was fatty deposit in the liver from a deficiency of the alkaline carbonates in the blood. Bicarbonate of soda was the leading prescription, and under this his side diminished in size, and his general symptoms improved correspondingly. Coming one day to my office he complained of strange feelings — lost for a moment his consciousness — breathed heavily — then slight cramps with violent trembling followed, and in a moment he was himself again. There was no change in the pulse, or paleness of surface. Having seen this form of convulsion in other cases of hypertrophied liver, I did not make it a subject of treatment. These slight convulsions occurred regularly, sometimes in church. During them his lips were blue, face flushed, and swollen. Somewhat epileptic in character, I considered them as the *petit mal* of the French.

While convalescent from his liver disease, an herpetic eruption appeared upon the testicles with a most intolerable sense of burning and itching, in bearing which he manifested a lack of fortitude, raving about the room during the paroxysms in a perfect frenzy of excitement. At one time I dressed the eruption, applying a cooling saturnine lotion. Notwithstanding its mild and soothing character, he manifested the keenest suffering from its application. Leaving the room for a moment, on returning I found him on the floor frothing at the mouth, but conscious and able to help himself on to the bed. The same evening I was called to him and found him in a convulsion. His countenance was tinged and swollen. He came out of it in great pain, and seeing a tendency to a return of the fit I bled him 16 oz. This relieved him, and I placed him upon the valerianate of zinc with blue mass.

This eruption receded in a day or two, and was followed by retention of urine which yielded to my favorite diuretic, the iodide of potassium. Soon after, the eruption came out again, then receded, and he came to my office hardly able to walk from distention of the bladder. The introduction of the catheter caused considerable pain, and soon after the urine was drawn off, he complained of "strange feelings" again, and went off in one of the most violent epileptic convulsions it was ever my lot to witness. I prescribed again iodide of potassium for the inflammatory action in the mucous coat of the bladder, with blue mass and zinc valerian as a general alterative and nervous tonic. This was early in August. After a few days' use of these drugs, he

apparently recovered and wished to return to his work but yielded to my persuasions to keep quiet.

Aug. 30th, evening. I found him in a most violent convulsion, which was rapidly repeated. Coming out of it he complained of cramp and pain in the stomach. On inquiry I found that he had eaten immoderately of green fruit during the day. An emetic dislodged a quantity of apples, green corn, cucumbers and beets, in large undigested, and unmasticated pieces. The relief was only partial. Calomel, grs. xx, jalap, grs. xv, was then given.

Later in the evening his symptoms were coldness of the extremities, extreme pain in the epigastrium, and cramp in the stomach and bowels, which organs could be felt beneath the abdominal muscles as rigid as iron bars. This cramp extended by sympathy to the voluntary muscles, until losing consciousness he was plunged into the most uncontrollable fits. Opiates and anti-spasmodics were administered *pro re nata*.

The case had now become pronounced. The small aberrations from muscular control which we have called *petit mal*, have magnified themselves into the *grand mal*. He has epilepsy from eccentric causes, and in a form of unusual severity. He knows no relief from pain so severe as to cause the most terrific outcry, save when he loses sensation in a convulsion equally terrible. It is no unusual thing for crude food to cause cramp, but why should this cramp extend to the voluntary muscles, until gathering strength, it sweeps down all the barriers of mental control and consciousness? The answer is to be found in the fact of his strong spinal system and weak brain. An intellectual man of more feeble frame, would bear this pain without yielding up to any great extent his control over the voluntary muscles. This statement will find confirmation in the further history of the case.

Aug. 31st. Vomited during the day quantities of undigested fruit with some relief. Movements of the bowels occurred and he was more easy though having occasional returns of convulsion till

Sept. 1st. All the symptoms returned with increased violence. There was, in the character of the fits some variations from true epilepsy. The *aura* was distinct in the shape of pain in the bowels, and sometimes prolonged for many minutes. Sometimes during the *aura*, I sat by him exhorting him to fortitude under his sufferings, and to efforts of the will to keep off the fits. Sometimes by bringing all his resolution to bear, he was able to stave off the paroxysm, but as the pain increased in intensity, he gave way, and found relief in the lethe of unconsciousness. For four or five days, at least one half of his time was spent in convulsion, the other half in pain. Generally after the fit there was heavy inspiration, and then delirium, during

which he recollected the events of his fit, but saw them in a distorted point of view, manifesting the greatest antipathy to those who had held him on the bed, and much affright when those persons came into the room — the fright being sometimes sufficient to cause renewed convulsion. Afterward there was during the paroxysm a kind of semi-consciousness, like that observed in hysterics — and this reminds me that his urine (which was removed entirely by the catheter) was very copious and limpid.

The treatment had two indications. First, to control pain and so prevent convulsion; and secondly, to get rid by calomel and cathartics of that inflammatory condition of the mucous membranes of the bowels and bladder which kept up the pain. Now this inflammation was the ordinary follicular enteritis of a mild grade. It is difficult to imagine how so slight an inflammation could cause so severe pain; unless it was by causing neuralgia of the internal viscera, and we find the counterpart to this in the intense pain and convulsions accompanying some trivial uterine affections. From the night of Sept. 1st up to Monday, Sept. 6th, morphine was given in *one grain doses, hourly*, decreasing, however, to half grain for the last day or two. Calomel was given in alterative doses, and free action of the bowels secured by the cold saline enema. Nothing afforded so much relief as these last. They brought away quantities of tough gum water phlegm, invariably followed by temporary relief of the symptoms. The stools gradually became more fecal and consistent as he improved. Of course all the usual adjuvants of mustard, friction, baths, and anti-spasmodics were put in force. On the 6th the Sulph. Morph. was suspended and 60 drops Tinct. Opii given hourly. Sept. 7th, 45 drops hourly. Sept. 8th, ordered to be given *pro re nata* with a decrease in quantity. Only $\frac{1}{4}$ oz. was taken during the ten days.

Friday Sept 10th. The symptoms now remaining are those of follicular enteritis.

℞ Calomel, gr. j., night and morning.
Salacine, gr. v., before meals.

Sunday, Sept. 12th. Patient is up and about the house, appetite good, tongue clean, but still some tenderness about the bowels. Continue treatment.

After a few days my patient resumed labor, and continued in good health till October 15th, when he was taken with a fit in a store. It was violent, and he had two others on his way home. I could learn of no imprudence of diet, but the symptoms (less the nausea) were those of the previous attack.

Calomel, gr. xx, with Jalap, gr. xv, was given at 7 o'clock, P. M., and repeated at 12. A pill of calomel and conium was given every hour. Towards morning he grew easier, and having free movements of the bowels he escaped with only one or two convulsions until evening, when they came on again. Accompanying, simultaneous, increasing, and decreasing with the pain in the bowels, was intense pain in the occipital region. There was tenderness of three or four of the cervical vertebræ. A large blister was applied to the nuchæ. At midnight, after six hours of continued convulsion and pain, he took 8 grs. of quinine, grew easy in about fifteen minutes, and though still suffering with the pain in the head, was tolerably easy.

I have neglected to mention, that during the evening of the 15th I took something more than two pints of blood from the arm, without materially affecting the force of the circulation, or benefiting the patient.

During the 17th, 18th, 19th and 20th, the treatment was made up thus, with occasional increase of the doses during severe pain.

Quinine, gr. viii, every four hours.

Laudanum, gtt. xlv, every two hours.

Calomel, gr. iii, every four hours.

Tinc. Valerian was also given in $\frac{1}{4}$ gr. doses.

The quinine was the most efficient and spasmodic given. The laudanum produced hardly any appreciable effect, even when given in doses much larger than that stated. The calomel acted with its usual certainty upon the inflammatory action, and was evidently an important item in the treatment. Under this management the fits decreased in frequency, and the pain assumed a periodical neuralgic character, coming on at sundown and lasting till 2 o'clock on the night of the 17th, but leaving earlier each night. Free action of the bowels was kept up by enema, and the urine was once or twice a day drawn off with the catheter. It was evident that the pain and spasm would not leave him so long as any inflammatory action continued.

The treatment was accordingly kept up until catharsis was obtained without the use of the syringe. His tongue cleaned off, and he passed the night of the 21st without spasm, and with but slight pain. On the morning of the 22d he reports freedom from pain in the head, and comparative comfort in the bowels. The calomel was suspended — the laudanum ordered given *pro re nata* — 5 grs. salacine to be given every six hours, alternating with a pill composed of Tinc. Valerian, gr. $\frac{1}{4}$, Pil. Hydrarg. gr. ii.

The patient is now again convalescent. The final issue of the case is not

in my mind doubtful. Should he continue a mild alterative, tonic, and nerve treatment, until every vestige of inflammatory action is subdued; and should he conjoin with this several months of rest from labor, with the exposure to wet and cold which farm labor involves, he will regain a state of robust health. But on the other hand, should he be guilty of a renewal of those imprudences which brought on this second attack, he will have a renewal of epileptic seizures, and with his peculiar *physique*, a complete destruction of his mental powers must be anticipated.

I feel that I have already extended this report to a length which may be tedious, but I am unwilling to leave the subject until I have stated those deductions which may be rationally drawn from it. But not on this case alone do these deductions depend. They are the offspring of some study of neuralgic disease in books, some at the bedside, and more than either in the lonely rides which are the lot of the country physician.

1st. There is such a thing as a proper balance of power between different organs having diverse but similar functions, and when one such organ predominates over the other in activity and strength, the result is a variation from true health.

2d. When, there being a want of proper balance and equality between the brain and spinal cord, the brain has the predominance, we shall find an infirm physical development, and the tendency will be towards diseases not convulsive.

3d. When the reverse obtains, (the spine having the predominance) we shall find a tendency to those diseases in which chill, congestion, and convulsion are prominent symptoms.

These statements involve a brief inquiry into the nature of those three conditions which I have linked together in the latter proposition. But prior to, and the precursor of these conditions, is another to which common consent has given the name of irritation.

Now what is irritation? I have heard serious objections to the word on the ground of its indefiniteness. A certain lexicon on my shelves defines it to be "the effect following the application of an irritant." This is very lucid — as clear as Carlyle — but the inquiring mind seeks for something less concise, and more satisfactory. To my mind, that which Kaltenbrunner calls the "period of incubation" — a lapse of time occurring after the application

of an irritant, and before the manifestation of congestion—defines the word. Its symptoms are pain, tenderness on pressure, and disordered sensations; the other essentials of inflammation being wanting.

Ubi irritatio ibi fluxus. Congestion follows the period of incubation. It is essentially distinct from irritation itself in being a *visible* change in the size and action of the capillary vessels. Closely connected with congestion is that phenomenon called chill. What is chill? Evidently the manifestation of a disturbance of the spinal system, consequent upon the presence of an undue quantity of blood upon the spinal and ganglionic systems, producing that tremor of the muscles, and (by derivation from it) that coldness of the surface, called chill.

This presence of blood in undue quantity is congestion.

If the congestion is intense and long continued as in congestive fever, the chill is permanent, and may never leave the patient until the chill of death is substituted. Or as in cerebro spinal meningitis, it may deepen into that more pronounced and alarming form of chill—convulsion.

These conditions then are but different stages of the same action. Irritation is the prodrome of congestion. Chill is a symptom of congestion, and spasm is but a magnified chill.

Such being the main facts on which we base our argument, let us support them by a cursory examination of the influence of sex, age, physical and mental conformation in the production of disease.

In the child the principal office of the brain is to govern the motive power. It has not yet attained to its higher intellectual function.

A child rarely has chill—in fact those causes which would suffice to cause chill only in the adult, will give us convulsion in the infant. Children are more subject to cerebral, than to spinal disease. The child has not learned to use its muscles—the will has no control over them—there is a lack of that proper balance of power between the brain and spinal cord, which alone can insure a quality of action. In the adult, chill takes the place of convulsion because the brain has acquired such power as enables it by efforts of the will to control those spasmodic actions to which the child must submit.

Women are much more liable to convulsion than men. Their wills are not so strong. Many funny things have been said of “woman’s will,” but the strength of their will lies in its very weakness. It is a fact worthy of notice, that rotundity of outline, and symmetry of figure are the children of the spinal system; while activity and strength of the cerebral functions pare down the rotundity, and shapen the outline of the figure. Witness the

shriveled limbs and sharpened faces of big-headed and precocious children. It is uncommon to find great beauty of person and strength of intellect in the same individual. Ever since Narcissus bending beside the fountain, became enamored of himself and changed into a daffodil, great beauties and great fools have been synonymous.

Female *savans* may have fine speaking countenances — faces which we *admire*, as we must admire intellect wherever found, but they are lacking in that softly moulded contour, that luxuriant but not redundant grace and fullness of figure, which we *love* in women and children. “Strong-minded women,” with high cheek bones, “probulgent” foreheads, door-handle clavicles, and very scanty mammæ — the Mrs. Jellyby’s of social life, whose “eyes are turned to Africa,” or to the “missionary fields,” or anywhere else save to their homes and firesides — with outline as angular and unyielding as a granite rock, are very little liable to convulsions. Through their compact forms the blood flows in scanty current through unyielding veins — the menstrual secretion is performed *guttatim* — congestive action is impossible.

There is another class of women, full to bursting with rich fibrinous blood, with bold dark eyes and hair, and a redundancy of figure which denotes strong sexual passions, who are peculiarly liable to convulsion. Their minds are generally of an inferior order, and so great is the predominance of the spinal system over the will and intellectual forces, that they are daily liable to the congestive and consequent convulsive action. Wet feet, or active exercise, or a little mental excitement are sufficient to break down the barriers of mental control, and subject the system to the unchecked influence of a spinal cord in full revolt from the government of the brain. But I have exceeded my limits. This vein of thought is but just opened — no divining rod is needed to see how great its influence might be, not only in our practice but in the great interests of education.

ART. III.—*Remarks on the Pathology, Causes and Treatment of Epidemic Cholera.* By J. LAWRENCE PAGE, M. D.

I take the liberty of inclosing to you an article on cholera, written by me about three years ago. My subsequent observations and reflections have strengthened the views therein expressed, and the recent report of Prof. Hamilton, of Buffalo, appears to confirm the opinion I have long entertained, that the causes of intermittent, remittent, and congestive (pernicious intermittent) fevers and cholera are identical.

If you think proper to give this article a place in your excellent Journal, with or without comment, you will oblige

Yours respectfully,

J. LAWRENCE PAGE.

RACINE, Wis., Nov. 1, 1852.

That *cholera* depends upon some miasmatic or aerial poison, is now pretty generally admitted. What that poison is we do not pretend to decide, nor does it signify. The nervous system is unquestionably primarily morbidly impressed; and whether by *koino* or *idio* miasmata is a question, the solution of which will aid us but little in arriving at a correct pathology, on which to predicate our therapeutics. The disease has generally prevailed more extensively and with greater violence in what are termed "malarial districts," where intermittent, remittent and congestive fevers prevail. The history of the disease from its first appearance on the banks of the Ganges, shows that its ravages have been greatest in *alluvial* countries, of *tertiary* formation, while those of *primary* formation have almost entirely escaped the epidemic. The great and fertile valley of the Mississippi being of the former character, has suffered severely, while amid the bleak and sterile hills and granite formations of New England, the disease cannot be said to have prevailed *epidemicallly*.

It would seem from this fact that the cause of cholera is somewhat analogous to that of the diseases just referred to, or at least that the epidemic is considerably modified thereby; and this opinion is strengthened by the analogy which exists between *Asiatic cholera* and congestive fever. Shrunken features, cold extremities, labored or hurried respiration, pulseless or nearly pulseless wrist, burning sensation at the pit of the stomach, great thirst, restlessness and serous dejections are symptoms common to both diseases. The experienced medical man cannot but be impressed with the resemblance of these two formidable diseases.

Oppression and consequent depression, a complete exhaustion of the nervous system, a retrograde motion of the *lacteals* and *lymphatics*, by which the fluids are poured out into the alimentary canal, constituting a sort of internal hæmorrhage, are the leading features of cholera.

What then are the indications of cure? Most clearly to relieve the oppression, call the circulation from the internal organs, determine to the surface, arrest the hæmorrhage and sustain the sinking energies of the system.

How can these be effected? The physicians of India resorted to *bleeding* to fulfill the first indications, and where this was effected early in the disease,

it was oftentimes successful, but the objection to this practice is, the radical tendency to prostration which characterizes this disease, besides, in many instances it is quite impossible to draw blood. A safer, more effectual and less debilitating method of overcoming the congestion is the cold affusion, than which nothing has been found more efficacious in arousing the energies of the system in the collapse of *congestive* fever. It is now very generally adopted in the treatment of that disease at the South, and the profession are familiar with the mode of its application.

Its beneficial effects are well attested in cases of poisoning by opium and other narcotics, and in *asphyxia* from the inhalation of deleterious gases. Who has not felt the delightful shock of a shower-bath, and the agreeable sensation of reaction which follows? It is this shock, or repeated shocks, that arouse the paralyzed nervous system and bring on reaction, equalizing the circulation and restoring warmth to the surface and extremities. Who, then, would hesitate to resort to its use in *cholera asphyxia*? Certainly no one who has compared the phenomena of that disease with congestive fever, and has seen its marked beneficial effects in the latter disease, even where the case was apparently hopeless.

But this is no longer an experiment. It has been tried, and the testimony of Parks, Giacomini, Goodlet and others, confirm its value as a therapeutical agent. I am satisfied that a more prompt and efficacious remedy does not exist wherewith to combat cholera, than the cold *douche*, or dashing cold water over the surface of the body. Its use, even where the surface is of an icy coldness is followed by heat of skin, elevation of the pulse, cessation of the cramps, and freedom of respiration. Here, then, we have the means of fulfilling the principal indications.

Opiates, astringents, and stimulants, to shut up the *waste gates*, and sustain the sinking energies of the system, fulfill the other indications. Caution, however, should be used in the administration of opiates that *narcosis* be not induced, which we have reason to fear has been the case in many instances. *Ice*, internally, is very grateful and doubtless very beneficial, and should be allowed *ad libitum*. Calomel, which has been so generally administered, is undoubtedly a valuable *adjuvant*, but it is in the *sequela* or fourth stage, or during convalescence (which is often expedited thereby) that we expect much benefit from this remedial agent.

To recapitulate — our theory is, that cholera *in every stage*, is a state of *congestion*, differing only in degree, analogous to the congestive fever of the malarial districts, and *somewhat* at least dependent on the same cause or causes. Our treatment, therefore, predicated on this view, cannot in many

respects, differ from that which the experience of years has approved in that disease.

If these views be correct, we can easily reconcile the suggestion of several European and American physicians, that *quinine* is a preventive or antidote to cholera.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

A Consideration of some of the Relations of Climate to Tubercular Disease. By W. J. BURNET, M. D., Boston.

There are two prominent facts which have made the subject of the climatic relations of tubercular disease, one under active discussion among the medical men of this country and Europe during the last few years.

These are: first, the almost alarming increase of disease of this nature; and, second, the facilities of travel, so that climate can be easily and cheaply changed. The time has been when only a few thought about distant travel for health. But now, almost every one who at all values his life, can easily put himself in a more genial atmosphere and beneath an almost cloudless sky. With the attention thus directed, the questions are—*what* climate is to be sought; and what are the reasonable expectations as to its effect upon tubercular disease?

Of late there has been published quite a number of works upon the climate of those European and insular countries hitherto quite celebrated as resorts for invalids of this character; and, as the most dissimilar views have been advocated, there has arisen much confusion among medical men as to the correct answers of the questions above referred to. Some, in fact, have become thorough skeptics as to the benefit of any change of climate out of the latitude in which the invalid has been accustomed to live.

From among these works recently published may be mentioned two, viz., that of Dr. Pollock, appearing in the London Medical Gazette of last year; and that of Dr. Burgess, not long since separately published. Both are upon the climate of Italy, and are well calculated to lessen the enthusiasm of invalids for a land which has always been made more sunny by the pens of poets than the favor of nature. I have no doubt that the conclusions of these men, and especially those of Dr. Pollock, upon the climate of southern Europe, are correct in the main; and as they were addressed to the English people, will no doubt lead many English physicians to hesitate before advising their usual migration.

But in this country, a misapplication and sometimes a misinterpretation of

these and similar opinions, has led very many physicians to be quite skeptical as to the real benefit to be derived by northern invalids, from a change of residence into the southern and more sunny states. This skepticism seems to be yearly increasing—and there can be but little doubt that it is as mischievous as it is really unfounded. It is certainly quite desirable that clear and distinct opinions should be entertained by northern physicians upon a subject fast getting to be one of such paramount importance. I make this remark, because I think that the reason of their doubts of climatic influence, is plain; in other words, that the cause of their unfortunate experience is becoming well understood. It is, that the climate has not been thoroughly tried. To make a clear and full statement of the whole matter, I will say that I am convinced that the shifting migratory course, South in winter and spring, and North the rest of the year, usually advised and followed, is an erroneous and mischievous one; and that if a northern consumptive can reasonably expect any benefit from this change of climate, this benefit will be obtained only from a continued southern residence for several years.

There is a grave error in thinking that, if one goes South in late autumn, and remains there until late spring, and then returns North to pass the summer and early autumn, he keeps himself in the train of favorable climate influences. It is not so; and the error is concealed in the fact that a summer at the North does not make a southern climate. This leads me to some considerations upon the peculiarities and differences of the northern and southern climates of this country.

As to the New England climate, it seems quite clear, that, taken as a whole, there is something in it highly predisposing to the development of tubercular disease. Not only do we see this disease here constantly peering out from hereditary predispositions, but the cases are quite numerous in which it seems purely indigenous, being engrafted upon an untainted stock. It is true that this may be said of other countries having an intemperate climate, but very far from the extent of what I think is true of New England. Statistics can be produced to show, that, take the whole year through, pulmonary diseases—inflammation of the mucous membrane of the air-passages—constitute a very large proportion of the disease. In fact, the tendency of disease here seems to be quite toward the pulmonary organs. Aside from the evidence of general observation, this statement has a very significant support in the fact, that in cases presenting some obscure aspects, the suspicion of the intelligent physician is quickly fastened upon the lungs, and an examination of the chest is made; thus showing that where outstanding local or temporary causes are absent, one is almost unconsciously led to suspect insidious disease referable to ever-constant general agencies.

An unequal fluctuating climate, in any latitude, tends to produce these effects. But the climate of New England, besides having this inequality and diversity in a very marked degree, possesses other characteristics having a great influence. Its atmosphere is dry and stimulating, and during the greater part of the year of a low temperature considering the latitude. The effect of such an atmosphere upon a sound constitution is highly bracing, leading to a mental and corporeal activity quite inconsistent with endurance and longevity. It is probably not an incorrect opinion that many of the moral and physical peculiarities of New England people, included under the terms enterprise and action, may be traced to these agencies.

In such an atmosphere, the constant vicissitudes of the temperature render

the functions of the skin imperfect, thus increasing the liability of congestions of the mucous membrane; and this mucous membrane, from the fact that it is ever in contact with an irritating medium, is generally that of the air-passages. On this account, mainly, the urgency of these conditions is considerably lessened by the use of flannel next to the skin; the importance of which worn in summer as well as winter, is now well recognized.

On the whole, New England climate has little in it that is sedative at any long season of the year. The winters are broken and unsteady, especially so on the sea-board, and it is only in the northern inland portions that there is that constant cold which has a far more favorable influence. The character of New England spring weather is too well known to need comment. Nothing could be more uncertain and less reliable. The months of May and June frequently change places, and one is not sure of warm weather until into July. As for the summer months, it is a great mistake, as I have before said, to suppose that they furnish a climate like that of the South. There is, to be sure, heat enough, but it is unsteady, and during July and August the thermometer not unfrequently falls 30° or 40° in a few hours. Intensely hot as it is frequently in midday, yet at midnight, if one is exposed, it is rare that over-clothes are not the more comfortable.

But a fact more significant than all the rest as to the influence of our summer weather, is that our consumptives do not generally improve in it; on the other hand, they lose ground. This is generally attributed to the depressing influence of the heat. No doubt there is much in this, for the heat is here often very intense; but more is probably due to the sudden and wide changes of temperature. That this is the correct version of the matter, would seem to be indicated by the influence of our early autumn weather, which is far the best and most genial we have. There is generally a season, commencing about the first of September, and continuing until the early frosts of October, when the weather of new England may be said to be truly fine. The atmosphere is warm and dry, presenting a hazy, quiet aspect, and the light wind is generally from the W. or S. W. It is then that we have those dreamy days that come and go so quietly as scarcely to leave a ripple-mark—reminding one of the sunny skies of the pine-lands of Georgia and South Carolina. Every one, and especially those out of cities, has felt the soothing, sedative influence of this weather.

It is well known that during this weather, our consumptive and other pulmonary invalids improve. The functions of their skin are more active, and the urgency of the cough and all the other pulmonary symptoms is decreased. The expectoration is less purulent, the appetite improved, and the spirits, strength and flesh increased. In many instances the improvement is as unexpected as it is remarkable—and there is often a melancholy pleasure in thus observing this temporary improvement, brightened as it always is by the patient with a thousand delusive hopes.

This short season is the only weather in New England with which I am acquainted, that is really favorable to consumptive invalids.* And in its

* The fine weather of a New England June has always been insisted on and highly recommended. But of late years this does not appear to have been true—for it has been unsettled, and often colder and more uncomfortable than May. If one can trust the testimony of elderly people, it would seem that in this and other respects, the climate has changed very perceptibly in the last quarter of a century. Now, they affirm, the winters have not that steady severe cold as formerly, but are more open and broken, running into the spring; and this last, in its turn, usurping a portion of summer.

favorable influence, and at the same time in its resemblance to that of the pine-lands of the South, there may be drawn something more than a hint as to the real agency of southern climate upon diseases of this nature. But broad as this hint is, it is not usually taken; or if so, not in time. For many invalids in the second stage of consumption, improved as they have, do not perceive the wisdom in taking means to continue in this same climate, but delude themselves with the hope that they will be well enough to remain North during winter; or, if they conclude to go South, defer it until they are obliged to, having two or three "colds upon their lungs."

The peculiarities of a southern climate as bearing upon its benefit to consumptive invalids, are far from being reterable alone to its elevated temperature. I refer here to the alluvial and pine-land portion of Georgia and South Carolina. It has other characteristics, which, though less well understood, are not the less important as to effects. The atmosphere has a decidedly sedative, soothing influence, which, due to whatever causes it may be, has a very desirable effect upon the mucous membrane of the air-passages—and this effect, once commenced, is not likely to be disturbed by sudden vicissitudes of temperature. There the general tendencies of disease seem to be changed; and that, too, from the thoracic to the cutaneous and abdominal organs; and it is through these changed relations that the cure is to be effected. But a fact more worthy of notice than all the rest, is the almost complete exemption from phthisis of the native inhabitants of this section of the country. It is true that consumption is there found; but a careful inquiry has shown that in almost every instance it had been immigrated either directly or indirectly. Other diseases, such as those of a miasmatic character, those of the intestinal canal and its appendages, seem to exist in the place of those of a tubercular nature; and were we better acquainted with that curious yet important subject—the *antagonism of diseases*—we might, perhaps, better understand how these relations are effected.

That these relations of disease are based upon climatic influences, might be here shown in many ways; but I will mention one fact, observed by myself, which is quite indicative. In northern and upland Georgia, the soil and aspect of the country quite resembles that of New England. There, as in New England, the primitive geologic rocks appear; and it has for a long time been remarked, that nowhere South is the climate so much like that of New England as in this section. The diseases follow in the same train, for they are pre-eminently those of the pulmonary organs. Consumption, lung fever, bronchitis, are common, and this, too, at the apparent exclusion of the diseases of the low and pine-land regions.

An additional fact of the same bearing, and which may here be mentioned, is, that, even in the pine-land country of upper South Carolina, a very severe winter (as the last, for instance) is quite productive of pneumonia or lung fever with those inhabitants living on creeks or in damp spots. The construction of their houses is little calculated to shield them from the adversities of cold and damp; and thus situated, it is rather a noticeable fact, that the disease assumes an acute form, exactly as is true of the Irish of New England, in whom tubercular tendencies are not common; whereas, among our native inhabitants, acute pneumonia is rather a rare disease, the pulmonary affections being generally of a more chronic and insidious nature.

If such are the influences of climate upon comparatively healthy constitutions, we should naturally infer that its tendency would be toward arresting

the development of tubercular disease, and favoring that condition of the general system leading to a permanent cure.

That this is so, I fully believe, and think it can be tolerably well shown, imperfect as the state of inquiry has hitherto been.

But if we sought proof in the results of migratory invalids, our case would truly be a poor one. If climate is to work a change, it is foolish to expect that that change will be effected unless the individual gets acclimated. It is, therefore, to the results of those cases of tubercular disease where the residence has been permanent, that we are to look for a correct version of the matter.

In my intercourse with many intelligent physicians at the South, many cases were described to me, in which individuals from the North, having phthisis in its first stage, had taken up their permanent residence there. Their pulmonary symptoms gradually disappeared, and now they are quite free from them, enjoying a very fair share of health. In the same manner, also, several cases were described to me, in which the disease had far advanced in the second stage—a cavity of small cavities having been produced in one of the lungs. These individuals remained there permanently, settling down into a quiet life. They recovered so as to enjoy tolerable health—the cure taking place, as indicated by physical signs, much in the way Laennec has described, by the partial cicatrization of the cavities, which yielded a blowing, dry, amphoric sound. In one of these instances the young man felt so much restored after a few years, that he hazarded a return to New England for a permanent residence. But in less than a year he was seized with a violent and unexpected hæmorrhage, and died soon after of ordinary phthisis.*

It is to be regretted that statistics upon this subject have not been made out; but as the matter now stands, the conviction left in the mind of the medical inquirer and observer is full and clear.

There is another fact, vouched for by an intelligent physician of Georgia, and which should be mentioned in this place. He affirmed to me that the negroes of Maryland and Northern Virginia, affected and broken down by pulmonary trouble, and perhaps scrofula, as shown in enlarged glands, &c., if sold to the Georgia and other far Southern planters, soon improved, losing their symptoms, quite often recovering, and growing strong and fat.

I was also struck with the fact of the long duration of phthisis with those negroes of the South, who, from quite ill conditions of life, had contracted the disease. It seemed to run a light, lengthy form, although perhaps fatal in the end. I recall to my mind one instance, where I examined the chest of a negro having tuberculosis of the apices of both lungs, and a cavity in the left one. To the physician with me I declared that he would die in three months. But he affirmed that he would live two to three years, and that, as property, this probability of life would be admitted.

But I need discuss this matter no farther. It now remains for me, in conclusion, to make a few general remarks.

The view I advocate is, that if a consumptive can reasonably expect benefit

* In citing these facts, I trust I shall not be misunderstood. I am very far from advocating the doctrine that all who have consumption in the first and second stages, can get well by living permanently at the South; but I do advocate that if benefit in these cases can be reasonably hoped for by this change of climate, this change should be permanent.

from a southern climate, his residence **there** must be permanent and not migratory.

Besides the arguments already adduced in support of this view, it may be worth while to notice the testimony given me by those physicians residing in the winter resorts of **northern** consumptives. Generally, they say, they (the invalids) do not arrive **there** until actually driven by the cold weather of the North. As soon as the warm, delightful weather of April has come, and they are, if at all, in a fair way for permanent improvement, they are uneasy about their return North; and the occurrence of two or three quite warm days in succession, soon settles their determination. By early May they have left, looking much better than when they came. The ensuing winter they appear again, but it is evident they have lost ground during their absence; they return home again in early spring as before, and here often is the end of their migrations. Others, having the disease in a more chronic form, appear regularly for many years; but at last are not seen or heard of again.

I am aware that invalids, on going South, expect too much in the way of climate. They picture in their minds cloudless skies over a land of the cypress and myrtle, and which will immediately effect their restoration. I need scarcely say that in this they are doomed to disappointment; and so will it always be, until the opinion is fully recognized—that it is not sunny skies that will alone benefit them, but rather a continuation under the aggregate of the influences of the climate.

At the present day numerous objections are raised by northern physicians against this southern migration. One class disapprove of it on the ground, both of the incurability of the disease, and a disbelief in warm climate, based upon an ill-digested theory, partly chemical and partly medical. Another class, and much the more numerous, although avowing a belief in southern climate, nevertheless quite object to the migration on the ground of humanity. They cry out against what they call the cruelty of sending people away from the comforts and attentions of home—and that too with a wide possibility to die among strangers. In its place they advise the patient to remain among the comforts of home—occupying a large chamber, which by various arrangements is to have a southern or summer atmosphere.

There is some force in a part of this objection, for sometimes there is great inconsiderateness in urging patients away. But, taken as a whole, it is not valid. Certainly no judicious person would advise the going away of a patient unable to bear the journey, or whose end is not far distant. But the conveniences of modern travel have taken away the former terrors of the transit. The journey now is easy and of short duration, and with mail and telegraph one can feel quite near home. With these conveniences there seems little necessity for the immuration of an invalid in a chamber—obliged all the while to take sedative medicines for cough—and however many and complete the home-comforts, yet in a fair way to depress the nervous system, and enervate the whole body.

In no disease is there so much danger of over-medication as in consumption. Experience has shown, that as a disease primitively of the nutrition, our object must be to strengthen the nutritive function, and to spare every unnecessary dose of medicine into the stomach, the tone and power of which, must be carefully nursed by proper food. I need scarcely say that these relations cannot be carried out by a winter's residence at the North, however favorable the circumstances.

In cases where the symptoms are not immediately threatening, and the patient has remaining considerable physical power, so as to be about in an easy way without fatigue, it will generally, I think, be judicious to advise, at least a winter's residence at the South, where one can be under the influence of pleasant days, and drink in balmy air instead of cough mixtures.

As to a summer's residence at the South, beside the objection of its being unnecessary, there is another generally urged—the enervating effect of its excessive heat. This objection is not well founded, and rests more upon ideas of a more southern latitude than any thing else. As to degree of heat, the mercury certainly rises higher in the New England than in the Southern States. For in these last it rarely exceeds 90°, even in the hottest season. It is true that the hot season is long, and, in the low sandy regions, its effect is quite depressing. But possessing such a variety of climates as does South Carolina and Georgia, the invalid need not thus be endangered, for there are resorts midway between the low and the mountainous parts of both of these States, where the summer climate is indescribably fine, having, perhaps, no equal in this or any other country.*

But in advocating the necessity of a permanent southern residence for the consumptive, I should be willing to do so only with some exceptions. There is a class of patients, generally of the so-called lymphatic and bilious temperaments, who bear heat badly; and what they gain in a decrease of local symptoms, they lose in general strength. I need scarcely say that this class of cases everywhere is the most intractable, and least amenable to treatment. It belongs to the judicious physician to perceive the relations of such cases, and advise accordingly.† As to variety of climate and climatic advantages, the United States are certainly more highly favored than any country. If this fact is known generally, it is not appreciated. No invalid need cross the water; for in our own borders, among our own people, who speak the same language as ourselves, we can, by a journey of less than eighty hours, be in a clime certainly not surpassed by any of the old world. Dissatisfied as the English are fast getting with their “sunny Italy,” or their “beloved Madeira,” it may not be regarded improbable that, when the communication shall have become easier and more direct, they will exchange these for the sunnier spots of Carolina and Georgia.

Boston, September 13, 1852.—*Boston Med. and Surg. Journal.*

Tourniquets on Railroads.—Several of the English railroad companies, and especially the Midland, have a supply of these very important instruments on board, which are often eminently serviceable in case of accident, till a surgeon can be called. They should be kept by the conductors of our American roads also. Many a death occurs from hæmorrhage in cases of

* Such is the character of climate of Greenville and its neighborhood in South Carolina, and of Stone-Mountain, in Georgia. In fact, there can be little doubt that the climate of both of these States is far better in summer for invalids than in winter.

† In this connection I may make a remark having an unrestricted application. It is, that in a disease so precarious as consumption, if an individual residing at the South is doing well, the wisdom of letting well alone and remaining there, should be recognized, however late in spring the time may be. They should not act up to the dictates of a common theory, before they have tested its value in their cases, by individual experience.

crushed limbs, wounds of blood-vessels, &c., before surgical assistance can be had. Should the public papers assist in promulgating this sentiment, the boon would soon be secured.—*Boston Med. and Surg. Journal.*

COLLOQUIA DE OMNIBUS REBUS.

COLL. V.—DE REMEDIIS NOVIS, SPECIFICIS, DIABETICIS, ETC.

The introduction of dramatic composition into medical literature, is somewhat of a novelty — an *innovation* — but it has, at least, the advantage of serving to relieve the dull uniformity of a style which generally characterizes the contributions to a medical Journal. The reader, at all events, will not be likely to take exceptions to the following colloquy, nor to others of the same sort, when he learns that it is taken from the Edinburgh Monthly Journal of Medical Sciences, and that the *Dramatis Personæ* are as follows:

<i>Obstetricus,</i>	PROF. SIMPSON.
<i>Chirurgus,</i>	PROF. SYME.
<i>Medicus,</i>	PROF. CHRISTISON.
<i>Physiologus,</i>	PROF. BENNETT.
<i>Chemicus,</i>	PROF. MACLAGAN.
<i>Editor,</i>	DR. WM. ROBERTSON.

ED. BUFFALO MED. JOURNAL.

Obstetricus (to *Chirurgus*.) Might a friend venture to inquire what has disturbed your equanimity this evening?

Chirurgus. Even yours would have been unsettled by the gentleman's story, who drove from my door as you arrived.

Chemicus. A tall, handsome, young fellow. I wondered to see him leaving your hospitable gate at such an hour.

Chirurgus. He is not in condition to enjoy hospitality, and came here for a very different purpose. He is one of the

VICTIMS OF MERCURY. Passing through Edinburgh with a mercurial sore-throat, a pocketful of mercurial prescriptions, and a mercurial belt, he felt uneasy traveling with three such unsafe companions, and came to see what I thought of him and them.

Chemicus. He would be surprised to learn that the root of his misfortune lay in his belt and recipes, and not in his throat.

Chirurgus. Very possibly. But I have not yet told you all. Led by incidental circumstances, he had been for some time indulging freely in wine and wassail, and living a life of hard exercise and constant exposure. On expressing my wonder at this, he told me, to my consternation, that the London surgeon, who advised him to poison himself with mercury, had not put

him on his guard, or under any rule or restriction, as to diet or regimen. You may judge what reason I had for appearing discomposed.

Chemicus. The traveler has cause to thank his stars and his constitution of "oak and triple brass," that he had not bid adieu to his nose and palate at least. What a fearful amount of misery must arise from the wagon-loads of mercurial pills and potions which are administered in London to all sorts of weak and scrofulous victims of venereal disease! It is a subject of painful reflection to every mind not proof against every humane consideration.

Chirurgus. The *Athenæum* tells us the other day, that medical men "have a vested interest in fever and cholera; their estate consists in the foul places, the bad drains, the putrid heaps of the city grave-yards." If this opinion, which is doubtless founded on acquaintance with the sentiments of the author's medical friends—should fairly represent the tone of metropolitan medical ethics, it would be unreasonable to expect the abandonment of the mercurial treatment of syphilis. But we must hope that things are not quite so bad as might appear from the *Athenæum*. In every medical community there must be numbers of professional men who are not so blinded by the pursuit of gain as to have their eyes shut to the truth, because it may affect their pockets. There are even some bright exceptions to the dogmatic mercurialism of London surgery.

Medicus. Do you mean to tell us, that, after what has been done and written about syphilis and mercury during the last forty years, a non-mercurialist is still the exception in London practice?

Chirurgus. Certainly. Have we not perpetual proof of this in the contents of the London Journals, and in such living illustrations as my belted traveler—whose case, I can assure you, is by no means a solitary one in my observation.

Medicus. This is deplorable. When I first went to London, in 1820, satisfied by frequent experience in our Infirmary here, of the soundness of the non-mercurial doctrine, first propounded by the medical officers of the army, and then systematized and powerfully advocated by Dr. John Thomson, I was shocked to find, as pupil of one of the great metropolitan hospitals, its "foul-ward" patients salivating, many of them for the second, third, or fourth time, and its surgeons ignorant or regardless of the glorious victory over mercury gained by our army surgeons, and conclusively followed up in the North. Returning thither in 1838, I expected to encounter truth at last in the ascendant; but in vain. After the lapse of eighteen years there were the same wards, the same fetid atmosphere, the same mercurial victims—other surgeons, but the same ideas. Is it possible that fourteen years more have wrought no decay in that old donjon keep of prejudice?

Physiologus. I can add my testimony that matters were in the same state in 1833, having found in its attics the same sort of patients, and spit-boxes, and atmosphere, and notions that year, while a pupil, as you did in 1820.

Obstetricus. When *Chemicus* and I accompanied the late Mr. Barnsby Cooper at his visit in Guy's Hospital, in 1836, we ascertained that every surgical patient in the hospital was taking mercury in one shape or another; and there is no reason to suppose that matters are any better yet, so far as syphilis is concerned.

Chirurgus. The more need, then, for us to show the contrast; which the Managers of the Infirmary have just put it in our power to do. The great

additions now made in the new buildings will afford ample accommodation for venereal patients, who for many years have been excluded from the hospital. We shall thus enable the student, as well as others, to learn from personal observation the truth of the principles, which have been so long taught and practiced in Edinburgh:—that “Hunterian chancres” and other primary affections may be cured by simple local treatment, without any mercury; and that in most secondary cases, mercury, instead of being an antidote for venereal infection, is another poison, and nothing else.

Editor. But would you consider so slight a matter as a Hunterian chancre a fit subject for hospital treatment?—it is such a trifle now under the non-mercurial method.

Chirurgus. The more occasion to prove to our unbelieving neighbors that it is so.

Editor. And where will you obtain in Edinburgh secondary cases of such severity as to instruct pupils or convince skeptical Southrons?

Chirurgus. Edinburgh can still supply a few of indigenous growth, thanks to one or two surviving home believers in the specific virtues of mercury against syphilis; and any want of native produce will be amply made up by arrivals from other parts still groaning under the mercurial curse.

Editor. To what do you ascribe so great a disregard of advancement in therapeutics as this dogged perseverance of our London brethren in the mercurial delusion?

Chemicus. To metropolitan indifference for improvement originating from without;—Roman contempt for everything barbarian.

Physiologus. Don't you think it may be rather referred to the prevalence there of a blind, degrading faith in Specifics, of which this mercury in the cure of syphilis has long been the chief?

Medicus. To both the one and the other concurrently, but at the bottom to an imperfect, unsound therapeutical education.

Chemicus. Why look farther than to metropolitan apathy toward “outside” improvement. For example, there has not been a single improvement of any importance made here in the treatment of diseases during the last five and twenty years that has been admitted into London practice, except tardily and imperfectly, if admitted at all.

Medicus. That is a bold proposition, yet true, and which, I doubt not, you can substantiate, if it be called in question. It may well rouse our metropolitan friends to serious reflection. But meanwhile, look a little beyond this state of things, and I think you will find its origin to be mainly a radical defect of tuition in therapeutics.

Chemicus. It was a marvelous step backward, when in 1850 the whole Boards of medical education in London, by incomprehensible common consent, reduced their requirements in materia medica to a course of lectures of three months.

Medicus. A heavy blow and discouragement truly to therapeutics. And more than this:—it is a proof to me that the nature and scope of therapeutics have not yet been duly appreciated in the London schools, or by the board of education there.

Is it possible to estimate too highly the importance of this branch of medical science? What is the ultimate object of medicine but the cure of diseases? What then ought to be the ultimate object of all medical education, if it be not the knowledge of the means of cure? To what purpose should

we teach anatomy, physiology, chemistry,—why pathology and diagnosis,—if we did not possess remedies, medical and surgical, which we could put into the hands of students when so instructed? But fortunately we do possess them,—indeed in too lavish profusion. And the best of them are hard to obtain, difficult to know, variable in quality, puzzling to select, nice to prepare, but above all most wonderful in action,—energetic, multifarious, complex, versatile, and singularly influenced by co-operating circumstances.

The ancients knew all this: Therapeutics, indeed, with semeiology, constituted almost their whole circle of medical science. The early modern physicians knew it also: Witness Matthioli's great folio *Commentationes*, which went through eleven editions during half the sixteenth century. Alston, the first British professor of materia medica, knew it. He stated it in this University in 1738, with a course of lectures of six months in duration, and I have never heard that either professor or student has since found the period too long. In all great medical schools of the present day, except one, the same opinion has prevailed. In Britain, under the united name of *Materia Medica*, on the Continent under the separate heads of Pharmacy and Therapeutics, the means of curing diseases are taught in just equilibrium with the other branches of medicine. In London alone has it entered into the understanding of man to conceive that pharmacy, therapeutics, diet, and regimen may be mastered by a student in sixty lectures. When, indeed, University, College, and afterward King's College, were founded on the model of that of Edinburgh, an attempt was made to place the materia medica on a satisfactory footing, and other London schools followed the example. But after a twenty year's trial the attempt, it seems, has signally failed: and in 1850 both the London College of Surgeons and the Apothecaries' Company, reduced their requirements in materia medica to the old miserable standard.

Chirurgus. Possibly they thought that all which is at present positively ascertained on the subject may be taught in three months.

Chemicus. If professors of medicine and surgery were to teach only what is positively known in their several departments, few of them would require more time. It is the very uncertainty of materia medica, and especially of therapeutics,—the number of doubtful points to be discussed, the quantity of falsehood to be cleared up, the amount of fashionable humbug to be exposed, that entail the necessity of deliberate tuition.

Medicus. Exactly so. But unfortunately, in the London system there has long been no time left for anything but hasty tuition in this and some other equally important branches. The dominant influence of the College of Surgeons as an educational body,—the partial, narrow views of their Council, who now, as in time past, will look to nothing but anatomy and surgery as deserving of earnest attention,—have been the main cause of this. With the Council of the College, anatomy and surgery have been everything; at least everything else is little more than nothing. Even physiology and pathology by their regulations mere offsets or appendages to anatomy, and to be taught as branches of it,—a very natural error for a body composed entirely of hospital surgeons and lecturers on anatomy and surgery, and in which no other branch of medical science or art is represented. And as for the Apothecaries' Company, it is easy to see why they do not encourage the science which they ought peculiarly to foster; they cannot even yet overcome the old hallucinations that apprenticeship is education, and that a

student, who is constantly handling drugs, must necessarily come to know all about them.

The consequences of all this might have been foreseen. What their directors undervalue, students do not prize. What the magnates of the profession do not cherish, the masses neglect. Therapeutics has ceased to be an object of inquiry, or is cultivated without method or principles. No one seems to care to improve our knowledge of old remedies. There is an incessant thirst for new ones. But these are sought for by the rule of chance; and not so much because they are needed for the purpose to which they are applied, and for which there is no want of acknowledged means; but apparently to satiate a morbid public craving for novelty, or to serve as a periodical invitation and advertisement. A wide-spreading empiricism broods over medicine, penetrating even into high places; and quackery of all kinds grows rank under its shade, pervading even the regular profession.

Obstetricus. You take a gloomy view of things. But the very magnitude of the evil will by and by work out its own reformation.

Medicus. It is not easy to avoid despondency, when one beholds, in relation to so essential a branch of medical science and practice, the ignorance of the profession, the advance of quackery, the sneers of the public, and the apathy of our medical rulers.

Chemicus. "Apropos des Charlatans," I see.

* * * * *

TREATMENT OF DIABETES. Having attained something very like a true pathology of the disease,—having discovered that it is not a disorder of the kidneys but a depraved digestion,—and having ascertained the chemical composition of all the principal articles of man's food,—by theory it was at once inferred, that a number of old remedies in the shape of physic, and many new ones still proposed from time to time, may be allowed to sink into oblivion. By theory, too, we know that a peculiar regulation of the diet constitutes the only sound treatment; and we know also what articles compose that diet,—thus already making a great stride towards the cure. For, by the substitution of gluten-bread and cakes made of bran, butter, and eggs, for ordinary bread and other farinaceous food,—and by allowing such vegetables as spinage, cauliflower, brocoli, and cabbage which contain little or nothing capable of conversion into sugar, we have rendered a permanent nitrogenous diet practicable, which it was not before,—and so we effect sometimes a cure, and often a most material amendment, which may be maintained indefinitely by due dietetic observance.

Obstetricus. Have you seen any one recover entirely in that way?

Medicus. A gentleman of sixty-five recovered entirely three years ago, and continues well, unless he exceeds at table; and another of twenty-five, and a third, a boy of thirteen, are greatly improved,—the latter, indeed, might be thought in all respects well, except that the urine continues saccharine.

Obstetricus. Although we do not now know any medicine to improve this state of things by directly controlling the morbid peculiarity of digestion which constitutes the disease, who knows that theory may not soon direct us to one?

Medicus. It is much more likely to do so than empirical trial, that is, accident,—which has been hitherto followed as the main guide. Indeed,

know not but that it may have actually pointed out a remedy already. At least I have just received some very apposite information, which may interest you, relative to an entirely new remedy, derived strictly from theoretical considerations,—namely, the

TREATMENT OF DIABETES BY RENNET, which seems to promise well. Dr. Gray, of Glasgow, was lately induced to make trial of this substance by the following theoretical views. Diabetes consists in the process of digestion stopping at the conversion of other organic principles into sugar, which cannot be oxidated in the lungs, and is therefore thrown off as excrementitious by the kidneys. But rennet out of the body converts sugar into lactic acid, and it may therefore do so within the body likewise. Should such conversion take place, however, the disease will be brought to an end, if Liebig be right in his opinion, that lactic acid is one of the principles of the organic world which can support respiration, by becoming oxidated in the lungs. Resting on these views, Dr. Gray tried rennet in the case of a patient so much reduced by diabetes, of at least twelve months' standing, as to be unable to work. Dietetic treatment had been only of partial benefit. Medicines of various kinds had been of little use. The urine was copious, 1045 in density, and strongly saccharine. On the 30th of last July, a teaspoonful of rennet, prepared as for the dairy, was given thrice a day. In eight days the density of the urine was reduced to 1025, and it contained lactic acid, but only a trace of sugar. In twenty-five days the quantity was sixty-four ounces, the density 1022.5, and the sugar gone entirely. In six weeks the urine continued free of sugar; the man had gained weight considerably; his strength was such as to enable him to return to his employment; he thought himself in as good health as before his illness; and nevertheless he had been ten days on nearly his usual allowance of wheaten bread.

Now I am far from meaning to say, nor does Dr. Gray say, that rennet is thus proved to be a remedy for diabetes by its apparent success in a single case. But it is surely the most feasible remedy that has been proposed for many a day;—so feasible, that I hope many will give it at once a fair trial, which is his object in allowing me to give this brief notice of it to you all. Should it prove as successful in other hands as in his, we shall owe another therapeutic discovery to therapeutic theory.

Obstetricus. Were all inventors in the materia medica as well trained in therapeutics as Dr. Gray appears to have been, we should have fewer new remedies to deal with, and probably more good ones. It is certainly a striking confirmation of your criticism on London therapeutics, that, among the many new London remedies, not one has been announced for some years, which has stood the test of experiment elsewhere.

Medicus. A very natural consequence of the contempt manifested everywhere in London for therapeutic instruction. By the way I forgot to advert to a most extraordinary circumstance connected with the discountenancing of this branch of medical knowledge by the London boards of education,—viz., the complete and universal silence and submission with which their degrading regulations have been received. Not a single teacher has publicly uttered a single remonstrance. Not a journal has issued one word of criticism. Therapeutics, it seems, has not a patron in the whole metropolis. But enough of this for the present.

Physiologus. You mentioned a little ago that we had arrived at something like a sound pathology of diabetes, and that it seems to be a disease of

digestion. But you are aware that this view may require revision, since the recent discoveries of Mr. Bernard, relative to the functions of the liver, by which he has proved that

SUGAR IS A NATURAL PRODUCT OF THE LIVER.

Medicus. That is possible. We do not yet see how the singular observations of Bernard are to affect the pathology of diabetes; but that they must have important bearings on it we can scarcely doubt. His inquiries have received too little attention in this country as yet. You have studied them carefully, and indeed have witnessed its leading experiments. Will you give us some account of them?

Physiologus. Within the last two years M. Bernard has brought forward a theory as to the production of sugar in the blood, which is supported by an amount of experimental proof that cannot be easily set aside. He admits that sugar may be formed in the process of digestion, and that a certain amount of it may, as the result of absorption from the alimentary canal, find its way into the blood. But he has shown that in man and animals of various orders, even so low down in the scale of creation as acephalous mollusca—if they are even fed entirely upon flesh—the blood from the hepatic vein invariably contains sugar. It is the result, however, of digestion of the food: for it disappears when an animal is starved, and it re-appears when the food is again given. He further observes, that sugar is found in the liver independently of the nature of the aliment. In dogs fed exclusively on animal food for several months, though he could find no sugar in the intestines or portal blood at its entrance into the liver, he always found it in the liver itself, and in the hepatic vein. In the spring of 1851 M. Bernard was good enough to perform the following experiment in my presence, during a visit I paid to Paris: A ligature was tied round the vena portæ where it enters the liver, and the dog was immediately killed by dividing the medulla oblongata. On opening the abdomen, the portal blood below the ligature, and blood from the hepatic vein, were immediately collected in separate glass vessels; and it was at once demonstrated, by applying the same test to both, that the latter contained sugar in abundance, but the former none. Sugar was also found in water in which a piece of the liver had been boiled in chips. Such an experiment seems decisive of the fact, that sugar is formed in the liver, and not conveyed to it with the blood through the vena portæ. Subsequently M. Bernard found that sugar is formed even by the fetal liver; for he detected it in that organ both in mammals at different stages of intra-uterine life, and in birds before being hatched.

In all cases the sugar so formed presents the characters of grape-sugar. In all cases it is quickly decomposed on coming in contact with the blood and animal tissues. Hence, even in the livers of animals, it can be discovered only for a short time after death.

M. Bernard next discovered, that section of both pneumo-gastric nerves, as well as any violent shock to the nervous system, destroys the power of the liver to form sugar. The most interesting, however, of his observations, and that which bears most pointedly on the pathology of diabetes, is, that irritation of the root of the pneumo-gastric nerves in the fourth ventricle of the brain increases the formation of sugar in the liver, and causes it so to abound in the blood that is secreted with the urine; in short, this operation produces artificial diabetes. M. Bernard showed me this remarkable experiment. Having squeezed some urine from the bladder of a healthy rabbit, he proved

that it did not contain sugar. He then passed a needle through the skull in such a way as to irritate the pneumo-gastric roots, and let the animal rest for an hour after the slight convulsions excited by the injury. Sugar was then found largely in its urine. On then killing the rabbit, it was found that the needle had wounded the intended part. I have since repeated this interesting experiment, with the same result; and so has my former assistant, Dr. Drummond: so that there can be no doubt of the fact.

Medicus. It has also been lately repeated with success in many trials by Dr. Schrader, as announced to the Royal Society of Sciences at Gottingen, in the beginning of the present year.

Physiologus. M. Bernard has since informed me of the results of his farther researches on this subject. He has now discovered, that, although section of the pneumo-gastric nerves destroys the formation of sugar in the liver, it is restored by artificially irritating their cut extremities; and that diabetes is produced exactly in the same manner as by irritating their origins in the brain. He was therefore led to conclude that the nervous action on the liver, necessary for the secretion of sugar, is not direct along the pneumo-gastric, as he formerly supposed, but indirect, or reflex, through these nerves as incidents, the medulla oblongata as the center, and the spinal cord communicating with the solar ganglion as the excident channels of communication. And following out this theory, he likewise found that whenever the respiratory function is violently stimulated, sugar appears in the urine, and that whenever ether or chloroform is given, a temporary diabetes is occasioned. It follows that the formation of sugar by the liver is analogous to those kinds of secretion which are produced by reflex action through the agency of a sympathetic ganglion, and the influence of certain stimuli—such, for instance, as the secretion of saliva caused by the presence of sapid bodies in the mouth, where the sensitive and motor branches of the fifth pair operate in a reflex way through the agency of the submaxillary ganglion. In this case, stimulating the tongue is necessary to cause a flow of saliva; and in like manner, a certain stimulus of the lungs (normally by the air) is necessary to cause the formation of sugar by the liver. M. Bernard further supposes, that in the same way that the lungs thus act by reflex nervous influence on the liver, so does increased action of the liver act upon the kidney; consequently, that the sugar, produced in excess by one organ, is excreted by the other.

Such is the present state of the question. Various pathological considerations might be stated which seem to show that Bernard's liver theory of the origin of diabetes is as consistent with facts as the theory which ascribes it to disorder in the stomach. But further inquiry is necessary before we can positively settle the real cause of that very mysterious disease. Meanwhile, it is not easy as yet to see how the discoveries of Bernard will enable us to improve the treatment of diabetes, unless the well-known symptom of dryness of the skin, by exciting the lung to increased transpiration, be connected with the cause of the disorder, in which case diaphoretics, though they have been often used with some benefit, would be more strongly indicated. But I think something will be learnt on this head ere long.

Editor. Gentlemen, I must beg you to excuse me for breaking up this colloquy so soon. I must prepare for an early start to Rotterdam.

Physiologus. And I to Paris.

Chemicus. And I to the Doune of Rothiemurchus.

Chirurgus (aside.) And *Medicus, Obstetricus,* and I, to the top of The Cobbler.—*Edinburgh Monthly Journal.*

Experiments on the Livers of Birds, in relation to the presence of Sugar.

By GEORGE D. GIBB, M. D., L. R. C. S. I., Hon. Fellow Med. Soc. Va.;
Lecturer on the Institutes of Medicine, St. Lawrence School of Medicine,
Montreal; Physician to the Montreal Dispensary.

During my residence in France, in 1848, M. Claude Bernard published a paper in the *Archives Générales de Médecine*, upon the source of sugar in the animal economy. I was presented with a copy of this paper through the politeness of the author.*

His experiments on the liver, demonstrating the existence of sugar as a natural constituent of that organ, were principally confined to the dog species, and I have repeatedly confirmed them in the human subject, and in many other animals of the class Mammalia.

The healthy liver of man and animals is now proved to contain sugar as a normal constituent; but in certain diseases, particularly those of a tuberculous character, as a pulmonary phthisis for example, where we sometimes find the liver enlarged, and in the condition termed "fatty" by Louis, the amount of sugar present appears to be very great indeed.

To determine whether this rule, the natural existence of sugar in the livers of Mammalia, would stand good with respect to another class of the Vertebrata, namely, birds, which rank next in importance to Mammals, I instituted a series of experiments.

It will be remembered, that, in birds, the liver is a viscus of considerable magnitude, consisting of two principal lobes, and firmly suspended in situ by ligaments and membranous processes. The vena porta, supplying that venous blood from which the bile is elaborated, is formed by vessels, derived from numerous sources, receiving not only the veins of the stomach, spleen, and intestines, as in Mammalia, but likewise the renal and sacral veins.†

There is also a difference in the amount of fat contained in the livers of the different orders of birds. Thus in the Palmipedes or webfooted birds, and the Grallæ or Waders, the larger number of species possess quantities of fat in their livers. In the Gallinæ or Poultry, again, in very many species there is a notable absence of fat.

The quantity present, or absent, of the fat, influences the amount of sugar to be detected, at least such is the conclusion numberless experiments lead me to.

If, again, the hepatic cells of the livers of birds are examined with the microscope, they are found even more free from fat globules than are those of Mammalia, and they are almost entirely filled with amorphous biliary particles.‡

In the following experiments, the livers were pounded in a clean mortar to a pulp, then boiled in a very small quantity of water for some minutes, and filtered. After cooling, the filtered fluids were examined. They are examples selected from a number:

* An abridged translation is published in 5th volume *British American Medical and Physical Journal*.

† Rymer Jones' *Comparative Anatomy*.

‡ *Principles of Physiology, General and Comparative*, by W. B. Carpenter, edition 1851.

No. 1. *Small Chicken.*

Moore's Test gave the merest trace of sugar.

Trommer's shewed its presence satisfactorily, but still in small quantity.

Cappezuoli's was also satisfactory, the yellow deposit of oxide of copper being pretty clear, after the lapse of some hours.

No. 2. *Larger chicken* than the last was killed by dividing the jugular vein, collecting the blood as it flowed. This fluid was allowed to separate into its two portions, and the serum examined for sugar, but no satisfactory results were obtained.

The liver was treated in the usual way.

Moore's Test shewed the presence of sugar greater than in the last experiment, but still in small quantity.

Trommer's was pretty satisfactory, and shewed the presence of a tolerable quantity.

Cappezuoli's was also equivocal, more so than in the last experiment.

No. 3. Liver of a *Fowl.*

Moore's Test light brown.

Trommer's, very marked indeed.

No. 4. Liver of *another Fowl.*

Moore's Test pale brown.

Trommer's, darker brown.

No. 5. Liver of a *Turkey.*

Moore's Test light brown.

Trommer's, darker brown.

No. 6. Liver of a *Goose.*

Both Tests very much marked indeed, indicating a large quantity of sugar.

No. 7. Liver of a *Duck.*

Both Tests marked in a similar degree to the goose.

Sugar was detected in every bird's liver I examined, the quantity being in proportion to the amount of fat present, and this was invariably large in the webfooted or water birds. There is a striking analogy to this in the Phocida, among the mammalia animals living almost entirely in the water, as the Walrus and Seal, and in which their livers are found to be almost masses of fat, and the quantity of sugar in that of the Seal is enormous.

M. Bernard, in his experiments, examined the blood as well as the liver, and found sugar to be a normal ingredient in both. I was unable to examine the blood, excepting in one instance, and discovered none.

To pursue these investigations further, experimental examination should be made on the livers of reptiles and fishes, which are store-houses of fat and oil; the livers of cod and other large fishes prove this from their yielding a considerable supply of the latter. And the great bulk of the liver in the Crustacea, Mollusca, and cold-blooded Vertebrata just mentioned, has reference apparently, not to a large production of bile, but to an accumulation of fat.

The deductions to be drawn from the fact of sugar existing in the liver and blood, cannot as yet be satisfactorily arrived at, until our knowledge is farther advanced on the subject. M. Bernard considers that a regular function of the liver is the formation of sugar, and that the liver alone has the power of producing sugar without starch. The sugar, as it is formed, is conveyed away by the hepatic veins, the vena cava inferior and right side of the heart; and as none is found in the pulmonary veins returning from the lungs,

Magendie infers, that it must have undergone destruction in the lungs and the carbon eliminated.*

The presence of sugar in the blood of the portal vein, which takes venous blood from the intestines and other viscera to the liver, is accounted for by M. Bernard, by the regurgitation of the blood from the liver, when the pressure of the abdominal parietes is removed on opening the abdomen; and this is permitted, he says, by the absence of valves. In this view, I cannot altogether coincide with the author, but do believe that the sugar found in the vena porta is totally independent of that in the liver itself, probably arising in most instances from the mesenteric veins.—*Canada Med. Journal.*

On the benefits that may be derived from placing Medical substances on the Tongue instead of into the Stomach. By MR. WARDROP.

In a practical science like that of Medicine, an insulated fact often forms a connecting link with other facts that had appeared equally unimportant. The Medical observer does well to collect such facts, and it is one not of the least advantages of societies, that they stimulate members of the profession to record observations which might not have been deemed of sufficient importance to be brought before the public in any other channel. With such an impression, I venture, on the present occasion, to call the attention of my fellow-members to a subject which seems to be worthy of their consideration, and which has not hitherto, as far as I know, claimed much attention. There are many circumstances which might be mentioned, in order to show the influence which some medical substances have on the animal economy, when they are placed upon the surface of the tongue, these effects being caused by the absorption of the medicine, and its subsequent admixture with the mass of blood. Such phenomena are quite analogous to the effects produced by mercury or arsenic, whether these pass into the blood by the pulmonary, by the cutaneous, or by the absorbents of the alimentary canal. A gentleman, subject to what are usually called bilious headaches, had, during many years, seldom failed to obtain relief by taking sometimes two, and sometimes only one grain of calomel. He repeatedly found that there was a distinct difference in the length of time which the calomel took to relieve the headache, according as it was taken in the form of a powder put upon the tongue, or of a pill taken into the stomach. Another gentleman, who had for many years suffered from dyspepsia, and who, for some years before I saw him, was in the habit of regulating his bowels by taking a pill composed of a couple of grains of aloes with myrrh, accidentally discovered that there was a remarkable difference in the effect of the pill when swallowed or when allowed to dissolve in the mouth. When taken into the stomach, it always created a good deal of pain in the whole course of the alimentary canal, and the evacuations were irregular both in number and in quantity; but when the pill was dissolved in the mouth, no other sensible effect was ever produced than one natural evacuation. Further experience convinced me of the difference in the efficacy of medicines placed upon the

* See Dunglison's Human Physiology, for a clear consideration of these experiments, vol. ii, 1850, a work that ought to be in the library of every inquiring physician.

tongue, or taken into the stomach, and led me to inquire into the cause, and endeavor to explain so important a phenomenon. The structure of the tongue pointed out that it possesses an abundant supply of absorbents. "The spirituous parts," observes the illustrious Haller, "more especially of vegetables, are received either into the papillæ themselves, or into the absorbing villi of the tongue, as appears from the speedy renovation of strength by liquors of this kind, even when they are not taken into the stomach." This structure satisfactorily explains how medicinal bodies, when placed upon the tongue, are absorbed and carried directly, by the absorbent vessels of that organ, into the venous circulation; whereas, when the same substances are taken into the stomach, they are necessarily mixed with the food and juices contained in the alimentary canal, so that a more lengthened period must be required to separate them, and convey them by the absorbents into the thoracic duct, and thence into the venous system. Or they may pass unchanged, as has often been observed, out of the stomach, and in this unaltered state they are evacuated along with the excretions from the alimentary canal. This remarkable effect of medicines when placed upon the tongue, is strikingly illustrated in the administration of calomel, and it will be found that placing a very small quantity of it, say the sixth or even the twelfth part of a grain, at short intervals, upon the tongue, such as every half hour, the mineral is rapidly absorbed, and ptyalism more quickly produced than by any other mode of employing the calomel. These results of medicines, I may also observe, are well known by the effects which croton-oil produces when applied to the tongue; and it is by no means improbable that the good effects of some medicines, when used in the form of lozenges, may be attributed to their absorption by the vessels of the tongue. All the circumstances regarding the difference and the effects of medicinal bodies, when conveyed to the venous system directly by the vessels of the tongue, or when they reach the blood by the more uncertain and circuitous course by the absorbents of the alimentary canal, appear to be worthy of being noticed, and may, it is not too much to hope, lead to some practical improvement in the mode of administering remedies. How far such differences will be found to result from exhibiting chloroform, the hydrocyanic acid, and the sulphates of quinine, iron, copper, and zinc, in the form of lozenges, and the advantages of using these medicines in such a manner, well merits further inquiry.—*London Lancet.*

On the Treatment of Typhus and Typhoid Fevers. By DR. TODD.

The general principles contained in this brief article will be found to accord in a striking manner with the views relating to the same subject submitted by us in former numbers of this Journal.—EDITOR BUFFALO MED. JOURNAL.

One important feature of fever, whether it be Typhus or Typhoid, whether diarrhoea be present or not, is *depression*. The disease is adynamic, and great attention must therefore be paid to supplying the patient with a proper nutriment. The basis of his diet should be proteinaceous matters, in such a

state that the stomach shall have little or nothing to do to bring them to a condition fit for absorption. In the animal broths, well made, and in milk, you have food which answers to this description. The former, on the whole, are probably the best. Milk is less easily digested, and does not always harmonize with other matters necessary to be given. Farinaceous matters may be introduced also in small quantities. A great secret of success in administering support to patients under these circumstances is this—to give it very frequently in small quantities—quantities so small, that the whole or greater part of one supply may be absorbed before the next supply is brought; and also not to give a variety of food. Keep to milk and beef-tea, or other broth, or to broth and farinaceous matter.

In the great majority of cases you must, I think, give stimulants, and give them early. They will often fail because begun too late. The best stimulants are brandy and port wine, with either of which chloric ether will go as well as a medical stimulant; any one of the three will often suffice alone. Port wine and brandy ought not to be given together, simply because in general the stomach does not digest well two kinds of stimulants. The same rule as to frequent administrations, and in small quantities, which I have already laid down for food, holds with equal if not greater force in giving stimulants.

In my opinion, the question in the treatment of fever is, not whether you shall give stimulants, but how much you shall give. In many you may give as much as half an ounce every half hour, or even an ounce of brandy, with advantage; but this is in bad cases. On this point you must be guided by the rapidity and compressibility of the pulse, and by the intensity of the heart's action. An important character of the pulse is found in the manner in which it strikes the finger; if vacillating, it is a decided indication for the use of stimulants. The strength of the heart's action, especially of the second sound, is also a good indication. If either sound be weak, but especially the second, you need not fear to give stimulants freely. An impulsive character of the heart's action with a feeble sound, also denotes the use of stimulants. Under such a plan of treatment, in which nutritious fluids and stimulants are given freely and from an early period, we find our mortality in fever to be small; we very seldom lose a case of fever. I do not allow myself to be deterred from giving stimulants by the state of the bowels; I know that many have a fear that much alcoholic stimulants irritates the bowels. If the alcohol be given in small quantities each time, it can not irritate it by direct contact, because it is absorbed before it reaches the intestines. Alcoholic stimulants, if not given too much at a time, are digested in the stomach, and the alcohol gets immediately absorbed and carried into the circulation. If it does harm, it does so from being in the blood; yet I must confess I have never seen satisfactory evidence of this.

We must also pay close attention to the bowels. If diarrhoea be present, it must be checked by those astringents which contain tannin; as the infusion or tincture of rhatany, catechu, of matico, of logwood, or you may give enemata with small quantities of laudanum. I find chalk often fails, and moreover it is liable to this objection, that as it does not dissolve, its particles may add to the irritation of the blood, by sticking in the ulcerated or inflamed patches. Counter-irritation over the abdomen by mustard, turpentine or blister, is also frequently of great use. If there is hemorrhage, you may give small doses of turpentine, five minims repeated every three or four

hours, and in such cases, turpentine must be used as an external counter-irritant to the belly.

Another feature in these cases is, the frequent occurrence of bronchitis or bronchial congestion, indicated by rhonchus and crepitation. The bronchial congestion and diarrhoea are frequently the most difficult symptoms we have to deal with in those cases in which we find maculae. The bronchitis may be relieved by the free application of turpentine stupes or blisters to different parts of the chest, at the same time or in succession; and though in such cases we must carefully watch the effects of our stimulants, we must not think of lowering our patient by bleeding, or by the application of any antiphlogistic remedies.—*Medical Gazette*, 1851.

Transactions Physico-Med. Society.—Binocular Microscope.—At a meeting of the Physico-Medical society, on Saturday evening, 2d October, Prof. J. L. Riddell called the attention of the society to an instrument of his own invention and manufacture, which promises to be of incalculable advantage in microscopic researches, especially in the prosecution of microscopic anatomy and physiology.

He remarked that he last year contrived, and had lately constructed and used, a combination of glass prisims, to render both eyes serviceable in microscopic observation. The plan is essentially as follows: Behind the objective, and as near thereto as practicable, the light is equally divided, and bent at right angles and made to travel in opposite directions, by means of two rectangular prisims, which are in contact by their edges, that are somewhat ground away. The reflected rays are received at a proper distance for binocular vision upon two other rectangular prisims, and again bent at right angles, being thus either completely inverted, for an inverted microscope, or restored to their original direction. These outer prisims may be cemented to the inner, by means of Canada balsam; or left free to admit of adjustment to suit different observers. Prisims of other form, with due arrangement, may be substituted.

This method proves, according to Prof. Riddell's testimony, equally applicable to every grade of good lenses, from Spencer's best sixteenth, to a common three-inch magnifier, with or without oculars or erecting eye-pieces, and with a great enhancement of penetrating and defining power. It gives the observer perfectly correct views, in length, breadth, and depth, whatever power he may employ; objects are seen holding their true relative positions, and wearing their real shapes. In looking at solid bodies, however, depressions sometimes appear as elevations, and *vice versa*, forming a curious illusion; for instance, a metal spherule may appear like a glass ball silvered on the under side, and the margin of a wafer may seem to ascend from the wafer into the air.

With this instrument the microscopic dissecting knife can be exactly guided. The watchmaker and artist can work under the binocular eyeglass with certainty and satisfaction. In looking at microscopic animal tissues, the single eye may perhaps behold a confused amorphous, or nebulous mass, which the pair of eyes instantly shape into delicate superimposed membranes, with intervening spaces, the thickness of which can be correctly estimated. Blood corpuscles, usually seen as flat disks, loom out as oblate spheroids.

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ART. I.—*Clinical Report on Chronic Pleurisy, based on an analysis of Forty-seven cases.* By AUSTIN FLINT, M. D.

(Concluded from page 418.)

PLEURISY CONSEQUENT ON PERFORATION OF LUNG.

In four cases pleurisy was developed in consequence of perforation of lung. In two of these cases the perforation occurred in the progress of tuberculous disease, and the pleurisy was, thus, a complication of phthisis. In the remaining two cases circumscribed gangrene of lung preceded the perforation, the latter taking place over the gangrenous part. A succinct account of these cases, severally, may possess some interest.

CASE 1. The patient, a male, was twenty-one years of age. There existed a family predisposition to tubercle, and he had evidently labored under the disease for more than a year. He was not under my observation prior to the perforation. He was able to be up and dressed when the perforation occurred. It took place during a violent fit of coughing, and was signalized by the sudden development of acute pain, and difficulty of breathing. I saw the patient on the third day after the perforation. The pain

and dyspnoea was then much diminished. He was unable to lie on the left side, which was the side in which was the perforation. The cough and expectoration were less after the perforation than before. The pulse was from 130 to 140.

The left side was visibly enlarged, and almost immovable in respiration. There was tympanitic resonance on this side except at the upper third posteriorly. Metallic tinkling was distinctly heard over the middle third; three or four tinkling sounds succeeding expiration, and occasionally accompanying inspiration. This sign was uniformly present at repeated acts of listening during the visit. It was absent at a visit on a subsequent day. At the next visit, a few days after the first (the precise number of days does not appear in the record) the left chest was still more largely distended, and the intercostal spaces elevated. He suffered much from dyspnoea.

He died shortly afterward, the precise date not being given. There was no autopsy.

CASE 2. The patient was a female, aged about thirty-five. I had examined the chest and determined the presence of tubercle months before the perforation occurred. At that time she was not under my charge. She came under my care in Sept., 1849, and at this time the evidences of perforation were present, and the time of its occurrence could not be ascertained, or, at all events, it does not appear from the notes that it was ascertained. The perforation occurred in the left side. There was tympanitic resonance at the top of the left shoulder and posteriorly at the summit of the chest. Below flatness. A loud cavernous respiration was present. A distinct tinkling sound was heard accompanying both inspiration and expiration, especially the latter; two, three, four and even five tinkling sounds with each act. This sign was not uniformly present. Lying down suddenly frequently occasioned a plashing noise, which had arrested her attention, and had also been frequently heard by her attendants. I was able distinctly to hear it as she lay on her right side and moved the body to and fro. I could not develop it by succussion while she was in a sitting posture.

She was quite feeble, but was accustomed to ride out on every pleasant day.

Pulse 120. Respiration accelerated. Some foetor in the expectoration was occasionally observed, but not in a marked degree.

Death occurred about two months after the case came under my charge. The patient suffered greatly from paroxysm of dyspnoea toward the close of life. There was no autopsy.

CASE 3. The patient, a male, was forty-five years of age. He had suffered from epilepsy for two years, which had considerably impaired the mental faculties. He had long been accustomed to use ardent spirits freely, and sometimes to excess.

I saw the patient, first, on the 28th Jan., 1852, in consultation. He had the day previous had a severe epileptic paroxysm. Prior to this date he had not had an attack for some time. At the time of my visit he appeared much prostrated, and had some cough with expectoration. On physical exploration I found only some bronchial râles, without any of the signs of solidification, and I supposed he was simply laboring under an attack of bronchitis.

I did not again visit him until the 5th of February. He then had a very fetid expectoration, of a gangrenous odor; and there were evidences of solidification at the inferior part of the chest on the right side anteriorly and posteriorly.

I saw him next on the 8th instant. The fetid expectoration had ceased. He now had little or no expectoration. His breath was occasionally fetid. He was greatly prostrated. The respirations were frequent and labored. Pulse frequent and tolerably developed. Some delirium. He had had a chill, and experienced acute pain in the right chest. He took food without reluctance and with some relish. On examination of the chest I found the physical evidences of perforation and pneumo-hydro-thorax as follows: tympanitic resonance over the upper and middle thirds of right chest anteriorly. Metallic tinkling at the inferior third just below the level of the nipple. The respiration over the middle third was blowing and low in pitch. At the upper third it was bronchial, i. e. tubular and high in pitch. Posteriorly at the upper third the respiration was bronchial. Over the middle third, posteriorly, it was blowing, feeble and low in pitch.

Death occurred on the night of the 10th of February, and the chest was examined, *post mortem*, on the 13th, by Prof. John C. Dalton, jr., who furnished the following report of the morbid appearances:*

"Autopsy of Mr. S—, February 13th, 1852. On opening the cavity of the right pleura, a considerable quantity of rather fetid gas escaped, and on raising the sternum the same cavity was found to contain about two pints of a dingy, yellowish-gray purulent fluid. The pleural surface at the anterior part of the chest was covered by a thin coating of recent lymph.

* This is one of the cases given in the Appendix to Prize Essay on Variations in Pitch, etc.

"The right lung, reduced to about one-fifth of its natural size, was compressed against the spine and the posterior wall of the chest. Before removing the fluid from the pleural cavity, a pipe was inserted into the trachea, and on inflating the lungs the air escaped freely in large bubbles, from a point situated toward the posterior part of the right lung, about the junction of its upper and middle thirds. The right lung, removed from the chest, was found to be completely carnified by pressure, except in those parts occupied by disease. No air-cells were visible anywhere, and the whole lung was destitute of crepitation. The lobes were adherent to each other by recent lymph. The upper third of the lower lobe was occupied by gangrene, which had reached an advanced stage, the pulmonary tissue forming a soft, shreddy, disintegrated mass of a fetid odor, and a dirty, grayish color, and infiltrated with purulent fluid. A gangrenous cavity of considerable size, had apparently existed at this spot before the compression of the lung. The opening by which it had communicated with the pleural cavity was about the size of a goose-quill, and situated posteriorly at the very uttermost portion of the lower lobe. The limits of the gangrenous portion of the lung, were well defined, but only a very little inflammation existed in its neighborhood, the solidification of the pulmonary tissue, except just outside the limits of this gangrenous cavity, being entirely of a passive character, and due to its compression by the pleuritic effusion. The left lung and pleura were healthy. There was no tubercle anywhere."

CASE 4. The patient was an Irish laborer, aged about thirty. He had complained of pain in the left side for two months, and had received medical treatment at the hands of Dr. Ring, of this city. The pain was not very severe, nor accompanied by sufficient embarrassment of respiration to prevent him from laboring. He was a hard-working man, not intemperate, so far as Dr. R. could learn, but lived in dirt and destitution, two families, each consisting of several members, occupying one small room, in an insalubrious part of the city.

About a week before his death the pain was suddenly increased; respiration labored; moderate febrile movement became developed, and he had a hard spasmodic cough. He was bled by Dr. R., took cathartics, with nauseating doses of ipecacuanha and antimony. The symptoms were not relieved. I saw him, with Dr. R., two days before his death, but owing to the filth of the place, and personal indisposition, the physical examination was very hurried. I found flatness on percussion over the lower part of the left chest, and, in front, absence of respiratory sound. Posteriorly I thought I

recognized a faint crepitation. The pulse was 120 and tolerably developed. He complained of pain in the left side on respiration. The chest on the left side was tender on percussion. He had had considerable expectoration, which Dr. Ring had repeatedly inspected with care. It had not been foetid.

On the day before his death he was transferred to the Hospital of the Sisters of Charity. Dr. Geo. N. Burwell, the attending physician, at that time on service, saw him once. He was then suffering from severe pain in the left side, with labored respiration. Dr. B. found flatness on percussion at the inferior part of the left chest, tympanitic resonance at the superior part, and well-marked metallic tinkling. The heart was displaced, so that its motions were visible to the right of the sternum. Exaggerated respiration existed on the right side. Fœtor of the expectoration was not observed while he was in the hospital. Death occurred, by apnoea.

At the autopsy, fourteen hours after death, at which I was present by invitation of Dr. B., the following morbid appearances were revealed:—The chest being opened, a quantity of foetid gas escaped. The left pleural cavity was filled with dirty ditch-water-like fluid, with shreds of fibrin. The fœtor was strong and characteristic. The quantity of liquid was estimated to be three or four quarts. Before removing all the liquid, the trachea was opened, and on introducing a blow-pipe, the perforation was demonstrated by large bubbles at the lower part of the chest. On removing the liquid, the pleura was found to be covered with layers of semi-organized fibrin. The pleurisy had evidently been of longer duration than the illness of the week immediately preceding his death, i. e. from the date of his giving up work and taking to the bed. It had doubtless existed while he continued at work, complaining of pain in the side. The lung on the left side was compressed to the back part of the chest, but extended to the lower part, having become attached at its inferior extremity to the diaphragm. It was free from tubercle. It was not solidified except by compression. At the lower part of the inferior lobe a perforation existed large enough to receive the end of the little finger. This opened into a gangrenous cavity of the size of a small English walnut. This cavity presented, all sides, a gangrenous layer about half an inch in thickness. The gangrene was distinctly circumscribed, and no solidification in its neighborhood. On closing the perforation the lung on this side was readily inflated.

In the case last given, the occurrence of gangrene with perforation as a complication of Chronic Pleurisy is an interesting fact. The absence of all fœtor in the expectoration in this case is worthy of note.

It will be perceived that in each of the four cases the existence of perforation was evidenced by the pathognomonic physical sign — metallic tinkling. So far as these few cases go, they show the value which this sign derives from its constancy, being one of the most characteristic of all the physical signs when present.

The cases illustrate the speedily fatal termination, by apnoea, which generally follows the occurrence of perforation; and, also, the fact that occasionally life may be prolonged under these circumstances for several weeks, and even months.

CASES OF EMPYEMA.

The liquid contents of the pleural cavity were ascertained to be purulent in three cases only. In one of these cases a collection of pus took place beneath the skin communicating with the cavity of the chest, which was opened, and the contents discharged. In the other cases death occurred without any operation. An abstract of the history in each of the cases will not occupy much space.

CASE 1. The patient was a male, aged thirty, a cabinet-maker, of intemperate habits. The case came under my observation in January 12, 1841. He was attended at that time by Dr. Samo, of this city. He was ill in a refectory, without conveniences or comforts. At the time I visited him under these circumstances the following were the symptoms:—Face ghastly, and expression anxious. Respiration labored. Anorexia. Increment hacking cough. Pulse at morning of normal frequency, at evening accelerated. Voice hollow. Great restlessness, frequently getting out of bed. Shortly afterward the pulse became very frequent and intermitting; the lower extremities cold; lividity at roots of nails, and a fatal termination was daily expected. He retained muscular strength to a surprising degree, getting out of bed, and insisting upon sitting up, while the foregoing symptoms were present.

After a few days, improvement took place. The chest, which had not before been examined with much care, was now found to be enlarged on the left side, and universal flatness on percussion, with absence of respiratory sound. He now had good appetite; obtained quiet sleep; respiration became less labored. He had occasionally pretty severe pain in the left chest. Some oedema of feet. Pain in feet, and some inability to control motions of lower extremities. The action of the heart occasioned a thrill or jarring

motion over the whole chest, the point of impulse being perceptible. The case ended fatally February 26, 1841.

The left pleural cavity was found to contain several quarts of laudable, inodorous pus. Pleural surfaces covered with layers of fibrin. Lung compressed to a mass of the breadth and thickness of the hand. No tubercle. Some adhesions in the right side; otherwise no morbid appearances. Moderate hypertrophy of the left ventricle of heart existed. Aortic and left auriculo-ventricular valves thickened.

CASE 2. The patient in this case was a female child, aged six years, delicate, but usually had good health. She was attacked December 14, 1845, with vomiting, accompanied by febrile movement. Stupor followed, leading to the supposition of congestion or effusion within the head. She emerged from this state, and appeared to be improving; when, December 22, there was a change for the worse. Cough, which had not before existed, was now a troublesome symptom. The respirations were accelerated and labored. Stupor again occurred, with great prostration, and frequent pulse, the respiration continuing accelerated and labored. Diarrhoea, with mucous and bloody stools, occurred. The mouth became aphthous. She had frequent paroxysms of distress accompanied by coughing. In these paroxysms she continued to throw herself about, and cry until exhausted. After this she gradually improved, consciousness returned, the mouth became well, and January 4 she seemed convalescing. She constantly lay on the right side. The pulse continued accelerated, being 130. She was free from pain, had good appetite, and there were no pulmonary symptoms to attract attention. January 22, the parents first discovered enlargement of the chest on the right side. The chest on this side was found to be immovable in respiration; the intercostal spaces elevated; flat on percussion, with no respiratory sound.

Death took place four days afterward.

At the autopsy, twenty-one hours after death, the right chest was found to be filled with laudable, inodorous pus. The lung was compressed into a mass of the size of the hand against the mediastinum. No tubercle. The pleural surfaces covered with layers of fibrin in different stages of organization. Lung on left side free from disease. Heart normal.

CASE 3. The patient was a female, twelve years of age. I have already referred to some of the circumstances of the history of this case, under the head of the causation of Chronic Pleurisy. She came under my observation July 16, 1845. She had been attacked suddenly three weeks before with

pain in the back, extending from the spine to the lateral portion of the right chest. A swelling on the right side posteriorly and laterally was observed. Dr. Josiah Trowbridge, of this city, saw her twice, and Dr. A. S. Sprague once. Both thought she was laboring under phlegmonous inflammation exterior to the walls of the chest, and looked for the formation of matter in a few days. Poultices were applied until the abscess should be ready to open. Dr. Trowbridge saw her, the first time, a week after the date of attack, and, the second time, a week after the first visit. She was seen by Dr. Sprague two or three days after the last visit by Dr. T. The swelling was tender to the touch, and Dr. Trowbridge thought he discovered fluctuation.

I saw her three weeks after the date of the attack. She was then obliged to maintain a sitting posture. The respirations were accelerated. She complained of pain in the right side. There was manifest enlargement of this side. The intercostal spaces were elevated. Tenderness existed over the right chest posteriorly and laterally. There was universal flatness on percussion over the right side. No respiratory sound save at the summit, before and behind, and in these situations bronchial.

July 19. Made farther record of symptoms as follows:—Has not had any cough, and has none now. No chills or rigors. Had much febrile movement at first. Skin now dry and temperature raised. Pulse 135. Occasionally slight perspiration. Tongue not dry nor coated. Some appetite. Not much thirst. Is more comfortable than on the 16th. Is able to lie down with more ease. She is greatly prostrated; speaks in a very feeble, almost inaudible voice, and the least exertion occasions panting. Intercostal spaces protruded to a level with the ribs, but, on measurement, both sides now equal. Other physical signs the same.

July 17. Much improved. Breathes comfortably, except after exertion. Pulse 100. The feet, since the date of the preceding record, have been very œdematous, but are now of natural size. *Dullness on percussion extends to left side beyond the sternum.* Right side almost immovable in respiration.

August 22. Tumor about three inches below the inferior angle of right scapula, evidently fluctuating. Dr. Trowbridge, who visited the patient with me on this date, stated that the former tumefaction occupied the same site, and that he was satisfied at his former visits that there was matter deeply seated.

August 24. Tumor opened by Prof. Hamilton, and several ounces of laudable, inodorous pus discharged. Flow of pus increased by coughing, and changes of position.

August 29. Free discharge of pus continues, quantity escaping in twenty

four hours, estimated at a pint. Expelled freely, and with force, by acts of coughing. General health improved. Appetite good. She is dressed and about the house. Pulse frequent.

September 14. Aspect improved. Appetite good. Has gained in weight and strength. Discharge diminished about one-half. Still laudable pus.

October 27. Much improved. Appetite good. Gained in weight and strength. Able to be out of doors, and to take exercise freely, but is put out of breath more readily than in health. No pain. Discharge continues, varying in quantity on different days. Sometimes a tablespoonful, and sometimes more in quantity. Still healthy pus. Little or no tenderness on pressure. No cough, and at no time has this symptom been present. Full inspiration occasions no pain. Is able to sing with ease and strength. Pulse accelerated. Tongue clean. Right chest contracted, and flat over lower half on all sides. Percussion sound relatively dull above posteriorly. Tympanic resonance at the upper third anteriorly. Absence of respiratory sound over the portions of chest flat on percussion. Tubular in interscapular space, and obscurely so at the upper part anteriorly. Bronchophony at summit before and behind, and also vocal fremitus.

She has taken no medicine for the last two months. She was bled by Dr. Trowbridge. After my visit she took mercury in combination with squill and digitalis, not carried to ptyalism. The chest was vesicated.

November 16. Still improving.

December 13. Discharge ceased a fortnight ago. Progressively improving in strength and general aspect. Tympanic resonance at the summit of the right chest anteriorly continues.

November 12, 1847. Patient has not been seen by me since the foregoing date, viz., Dec. 13, 1845, until the present time. She went to school during all last winter, and kept about during the past summer. Last spring pus began to discharge at a point anterior to the old orifice, and also at the latter point. The discharge continued through the summer until two or three weeks ago. Quantity discharged during the twenty-four hours, varied from a tablespoonful to half a pint. She has not been so well since the discharge ceased. She is still able to walk about, but is feeble and emaciated. Easily put out of breath on exertion. Anorexia. Troubled with diarrhoea. Slight cough. Pulse very frequent.

On the right side there is uniform flatness on percussion. No respiratory sound anteriorly or laterally; a faint tubular sound posteriorly at summit. On the left side flatness anteriorly except over an area from two to three inches in diameter at the upper part of the chest toward acromial extremity of the

clavicle. Respiratory murmur appreciable in this situation, and over no other part of the chest anteriorly. Percussion yields clear resonance over the left side laterally and posteriorly, and in these situations there is a loud puerile vesicular murmur. Sounds of heart loud. No point of impulse perceptible. Soft bellows murmur with first sound. In respiration the right side is almost quiescent, and on a front view the left shoulder is the only part in active motion.

The coexistence of pericarditis was suspected from the above signs and symptoms.

The patient was not seen afterward, nor the issue ascertained.

It is evident that the causes determining the formation of pus, so far as these few observations go, in pleurisy, are incident to the inflammation; that is to say, when the pleural cavity contains pus, instead of the turbid serum which is usually found, it is not owing to a longer duration of the disease, to a larger amount of effusion, or other circumstances by which it might be supposed ordinary pleurisy is so modified as to run into empyema. Empyema, in other words, would seem to be a form of the disease differing from ordinary pleurisy, *ab initio*, in the tendency to the formation of pus. In each of the three cases just given, pus was formed after the disease had existed but a short time; and, in several of the fatal cases in this collection, in which the disease ended fatally after a much longer duration, and in which the quantity of liquid effusion was equally great, the contents of the chest were found to consist only of serum and fibrin. A larger collection of observations might show that ordinary pleurisy is apt to be converted into empyema; but, in view of the facts contained in this collection of cases, the latter form of the affection involves an intrinsic difference so far as relates to the distinctive feature viz., the formation of pus.*

MORBID APPEARANCES AFTER DEATH.

The histories in the collection of cases under analysis, embrace eleven autopsies. Aside from those of which an account has been already introduced

* This accords with the view presented by Mr. Paget, in his admirable lectures on inflammation, delivered before the Royal College of Surgeons, of England, (which have fallen under perusal since this Report was written,) of the distinction between "adhesive inflammation," and "suppurative inflammation." Mr. Paget's explanation of the distinction is, that in the former, i. e. in adhesive inflammation, there is a preponderance of the fibrinous element in the effused inflammatory products; while in the latter, i. e. *suppurative inflammation*, the exudation-corpuscles are in excess.

into the Report, the morbid appearances were such as belong to ordinary Chronic Pleurisy with a greater or less amount of effusion of fibrin and serum, a corresponding compression of the lung, etc. The facts falling under this head are not considered to possess sufficient novelty to warrant a detailed description in the cases individually, which would require considerable space. They are accordingly dismissed with this brief reference.

TREATMENT.

The few remarks which I shall offer under this head, will be confined to the treatment of simple Chronic Pleurisy, i. e. general pleurisy, not involving perforation, or complicated with other affections. In many respects, however, the same principles of treatment are equally applicable under the latter circumstances. The number of cases in this collection in which the management was observed throughout, is too few, and the plan of treatment too little varied, to institute comparisons in order to determine the influence of different therapeutic measures by numerical results. Nevertheless, on an examination of the treatment pursued, (of which an abstract, as contained in the history of each case, is before me) I may be able to draw some practical conclusions. In doing this I shall avoid, as much as possible, details which the reader would find tedious.

Bleeding. General bleeding was practiced in six cases. In all but two of these cases, this remedy was employed before the patients came under my observation. The immediate effects, therefore, were not subjects of study for me, except in these two instances. In one of these the symptoms seemed to be relieved by bleeding; in the other, no alleviation ensued. In the former instance, the case ended in a slow recovery. The latter terminated fatally in seven weeks. In both cases bleeding was practiced twice, shortly after the date of the attack. Of the four remaining cases, recovery followed in two; in one, the patient was able to return to active duties in a month, the chest being, however, still moderately full of liquid effusion; in the other, the recovery was tedious. In the first of these two cases the patient was twice bled. In one of the remaining cases death occurred after five months. This patient was twice bled. The other case was that of the girl who had empyema, which discharged externally. The case, without doubt, ended fatally soon after she was last seen by me, which was more than two years after the date of the attack.

In all the cases in which bleeding was employed, the symptoms denoted acuteness of inflammation, or an approach thereto. The propriety of this

measure relates to cases of that description. Few practitioners, if any, at the present time, it is presumed, resort to bleeding in cases in which the disease is subacute at the commencement, excepting, perhaps, some instances in which manifest plethora exists. So far as any inferences are allowable from so few data, the facts which have been cited do not exemplify, in a striking degree, the efficacy of bleeding even in the cases in which the symptoms denote, at the commencement, acuteness of inflammation. The observations, however, are not sufficient in number to authorize any very decided conclusions respecting the value of this remedial measure in this disease.

Local bleeding, by leeching, was employed in two cases, and wet cupping in one. In each of these cases general bleeding was also practiced. Of the two patients leeches, one died, and the other recovered. The patient cupped, recovered. These facts are not mentioned because they are supposed to have much, if any value. They are obviously inadequate for any practical inferences.

Mercurials and Diuretics. I include these remedies in the same category because they were almost uniformly given in conjunction. Simple mercurialization, without diuretics, or other remedies, was not tried in any instance. In general, the plan of medication pursued was that advised by Dr. Hope, consisting of mercury, with diuretics, or cathartics, followed by, or associated with tonics, nutritious diet and vesication over the chest. With respect to the special agency of mercury, therefore, my observations do not furnish any definite results. The same remark will apply, measurably, to other remedies; but not to the same extent, because diuretics, and in some instances cathartics, etc., were continued after mercurials had been suspended. The object kept in view in directing these measures was chiefly the absorption of the liquid effusion. In several instances the effect of mercury and diuretics in this respect was marked. If the quantity of urine was increased in any considerable degree, the accumulation within the chest was sensibly diminished. This diminution, however, proceeded only to a certain extent. The liquid contents were in no instance speedily and entirely absorbed; but sufficient effect was produced to cause decided relief to the symptoms due to distension, and also to lead to an obvious contraction of the dimensions of the chest. Mercury was never carried beyond slight ptyalism, and not uniformly to that point. The diuretics used were squill, digitalis, the supertartrate, and the nitrate of potash. These were generally given more or less conjointly, or rather simultaneously. A dry diet was usually enjoined, especially while diuretic remedies were prescribed.

Cathartics. In some cases it was observed that the kidneys were ne

readily acted on to produce increased secretion; and, occasionally, watery evacuations from the bowels took place under the use of diuretic remedies. Under these circumstances, hydragogue cathartics were employed. The liquid effusion was sometimes apparently diminished by their operation. Gamboge, elaterium, the sulphate of magnesia, and, in one case, podophyllum were prescribed for this object.

Iodine. This remedy was given in a few cases, but without marked effect in promoting absorption of the liquid effusion.

External applications. These consisted of vesication, stimulating liniments, and the iodine ointment. Vesication was uniformly employed, to a greater or less extent, after the mode advised by Dr. Hope, viz., small blisters, removed early, and carried successively over different parts of the affected side. The impression derived from the constant use of this remedy was not unfavorable to their efficiency in favoring the process of absorption. It is true that the blisters were usually conjoined with other therapeutic measures, but in some instances they were continued with advantage after other remedies had been suspended. Stimulating liniments were employed simply to relieve pain. The iodine ointment was resorted to in a few instances without any special effect being observed.

Tonics, stimulants, diet, and regimen. After the use of the measures already referred to had been continued for some time, and sometimes before they were discontinued, tonic remedies, and diffusible stimulants generally constituted the medicinal treatment. They seemed to exert a good effect, and in some instances this was striking. Conjoined with their use, a generous diet, and moderate exercise in the open air were found highly useful. In several cases it appears from the histories that the patients after a certain measure of improvement, remained in a stationary condition until they began to go out of doors. In one case the patient had for several weeks been confined to the bed, and ulceration of the nates occurred in consequence. Under these circumstances, he was carried out, placed in an easy carriage, and rode a short distance. He returned much improved. The plan was systematically continued, with constant improvement, and from that date he steadily and rapidly advanced in his recovery. In a less striking degree the salutary influence of the same regimen was equally obvious in several instances.

In thus endeavoring briefly to state the results of my experience in the management of Chronic Pleurisy, the want of precision is not less obvious to the writer, than to the reader. Exactness in determining the relative agency of different measures of treatment is, however, seldom attainable; and especially when the treatment consists of several measures employed in

conjunction, or succession, the difficulty of ascertaining experimentally the influence which belongs to them separately is very great. Moreover, with respect to the disease under consideration, as also many other diseases, science has not yet acquired the true standard by which to measure the efficiency of remedies individually or collectively. This standard is the natural history of disease uninfluenced by medicinal management. A series of cases in which Chronic Pleurisy was allowed to run its course, under favorable hygienic conditions, without remedies, would furnish results of very great value as a point of departure for determining how far therapeutic agencies are really efficacious. The difficulties in the way of obtaining such a collection of facts, it is obvious, are of a nature to render it doubtful if they will ever become the property of science. As an approximation to an end so desirable, the amount of fatality under different modes of treatment is to be considered. The proportion of deaths, in the present collection, among the cases of simple uncomplicated pleurisy has been seen to be quite small. In the collection of seventy-eight cases analyzed by Dr. Blakiston, to which reference has before been made, recovery took place in every instance. Dr. B. quotes a statement by Louis, made to the Academy of Medicine at Paris, "that he had never met with a case that had terminated fatally when the disease was in its simple form." That the vast majority of cases end in recovery, certainly accords with the general experience of medical practitioners. It is, therefore, fair to presume that the natural tendency of the disease is toward a favorable termination. This consideration is to be taken into account in estimating the importance of remedies severally, or combined. When the disease proves fatal, it is by being complicated with other affections, which are but little under the control of art.

To sum up, in a few words, the practical views of the management of Chronic Pleurisy based on the personal experience of the writer, the first and chief object in the early part of the disease, is to effect absorption of the liquid effusion. It is an object, doubtless, to prevent or limit the effusion, if cases come under observation, and the diagnosis is made before this takes place to much extent. But in a large proportion of instances the effusion takes place so rapidly, and is accompanied by so little pain or other symptoms of prominence, that at the first examination the chest is found to be already filled. Mercury and diuretics exert a certain amount of efficiency in causing absorption to commence, and continue. If the kidneys are not readily acted on, hydragogue cathartics may exert a similar influence. Small blisters applied in succession, in a manner least liable to occasion constitutional irritation, are valuable means for the same end. The effect of iodine applied

locally, and administered internally, is less appreciable, but perhaps not altogether negative.

The beneficial results of these therapeutic measures have certain limits, both as respects the extent to which they promote absorption, and the duration of their employment. The complete removal of the liquid effusion by absorption, requires usually a long period, usually several months at least. The remedies mentioned should not be persisted in after they cease to exert an obvious effect. Fortunately the resources of physical exploration, in this disease, enable us to determine accurately, from day to day, the condition of the chest. If the accumulation within the cavity of the pleura does not sensibly diminish, but remains stationary for days or weeks, it is useless to continue measures which may do positive harm by compromising the powers of the constitution. Considering the natural tendency of the disease toward final recovery, or at all events the absence of a tendency to a fatal termination, we should not be justified in subjecting the system to the long continued operation of medicinal agencies which must of necessity prove hurtful, if they fail to accomplish the desired end.

After giving the measures just referred to a fair trial, reliance must be had on tonic remedies, diffusible stimulants, associated with a generous diet, and an invigorating regimen. To preserve the powers of the constitution in an affection requiring so long a period for the completion of recovery, is a prominent end to be kept in view in the management. My observations have furnished repeated illustrations of immediate and progressive improvement directly all debilitating medicines were suspended, and a sustaining plan adopted. So far as my experience goes, the effect of this change in the treatment has been more striking than the apparent efficiency of any medicinal remedies. The duty of guarding and developing the vital forces, in my estimation, ranks first in importance. It is almost needless to add, that due protection against exposure, over-exertion, etc., is by no means to be overlooked. Exercise in the open air should be restrained within proper limits, the surface of the body is to be kept at an uniform temperature by appropriate clothing, and dietetics duly regulated. The patient should not go out when the weather is inclement. Flannel, or raw silk, should be worn next the skin, and in winter vestments of buckskin or chamois leather may be super-added. The food should be substantial and plain, with a fair proportion of flesh, and a sparing supply of liquids. Excesses and irregularities of all kinds are of course to be interdicted.

In conclusion, a few remarks on the subject of *paracentesis thoracis* may not be out of place in this Report, although my observations contain no facts

relative to this operation. This subject has of late given rise to considerable discussion, and in this country the attention of the profession has been directed to the importance of the operation by Dr. H. I. Bowditch,* of Boston, Mass., who has reported several cases in which it was practiced. One object in Dr. Bowditch's communications has been to bring to the notice of practitioners a new method of performing the operation, devised by Dr. Morrill Wyman, of Cambridge, Mass. This method consists in using an exploring canula and trochar, which is attached by a flexible tube to a suction pump so constructed that the fluid may be removed from the chest through the canula, and discharged from the pump through another aperture.

Dr. Bowditch gives a report of eight cases in which this method was resorted to. Three of the cases ended in recovery, and the operation is considered by Dr. B. to have had an important influence in the favorable result. In three cases material improvement followed the operation as respects the rational and physical signs. In no instance were there any unpleasant consequences aside from the slight inconvenience from the punctures. The operation is shown to be made with ease, and apparently with entire impunity.

As regards the results, Dr. Bowditch was unfortunate in the selection of cases in which trial was made of this plan of treatment, inasmuch as the proportion of recoveries (3-8) is much less than would be expected to occur in cases coming indiscriminately under observation, and managed without resorting to paracentesis. It is probable that Dr. B. did not feel justified in making trial of the operation except in cases in which the symptoms were unusually severe. This is the impression received on reading the details of the histories; and it does not appear, as stated by Dr. B., that in any case the symptoms or progress of the disease were unfavorably affected by giving exit to a portion of the liquid contents of the chest. Moreover, several of the cases reported by Dr. B. were cases of empyema, and it is well known that, the absorption of pus being effected with much greater difficulty than when the liquid effusion consists of fibrin and serum, this variety of Chronic Pleurisy is much more likely to prove fatal than the simple form.

Rationally considered, the operation, performed after the mode suggested by Dr. Wyman, is an important improvement in the management of Chronic Pleurisy, and appears to be free from any serious objections. The end of medication is to effect removal of the liquid effusion, by means of absorption. If this end can be attained by a simple, trivial operation, devoid of all danger,

*Am. Med. Jour. of Med. Sciences, No. for April, 1852.

it would certainly seem that an advantage has been gained, inasmuch as the agency of remedies for the same end is uncertain, slow, often ineffectual, and exposes the patient to injury. If it be found by experience to be hurtful to remove the contents of the chest at once, or in a short space of time, and that, therefore, the gradual operation of the process of absorption is preferable, this difficulty may be easily avoided by repeating the operation at such intervals as observations show to be most judicious, and removing a small quantity at a time. The operation is so slight, and attended with so little inconvenience, that there is no objection to this course. In cases in which the accumulation is large, producing great distress, and perhaps threatening life, and especially when other remedies fail to procure relief, there can be no question as to the propriety and importance of paracentesis, even performed after the old method. In cases of empyema, also, it cannot be doubted that it is desirable to withdraw the purulent liquid artificially, and not trust to absorption. The difficulty here consists in determining the fact of the chest containing pus. As already remarked under another head, where this form of pleurisy is suspected, there seems no objection to the use of the exploring canula as an instrument of diagnosis.

Heretofore, the question of the propriety of resorting to paracentesis involved the liability of mistaking for pleurisy with effusion, other affections of the chest. The operation has frequently been made when the chest contained no liquid. At the present time, however, uncertainty in diagnosis is not a valid reason for omitting or delaying to puncture the chest. Physical exploration supplies demonstrative evidence of the presence of liquid in the cavity of the pleura. It remains by accumulated observations to determine whether in simple Chronic Pleurisy with effusion, the operation of paracentesis may be resorted to without diminishing the chances of recovery, and with the effect of relieving unpleasant symptoms, and expediting recovery.

ART. II. — *Scripta Diffusa*. By SMELFUNGUS.

"Non fumum ex fulgore, sed ex fumo dare lucem cogitat."—*Ars Poetica Horat.*

"Scribendi recte sapere est et principium et fons."—*Ibid.*

Whether, had he lived long enough since, my friend Smelfungus would have been a Broussaist or Brunonian, a Rushite or Tullyite, no man knoweth. My friend's qualities — good and bad — are all positive — noncommittalism was never his forte.

Therefore Smelfungus is no *expectant*. It is one of his peculiarities, that, sitting in his cobwebbed and smoke-stained office, he builds hypothetical castles in the air, and fights doughty battles with imaginary antagonists. Late in the lonely night he grows warm in argument, and waves his long right arm up and down after the manner of a pump-handle, in solitary dignity. Hear him now!

"This expectant theory is the offspring of scepticism! The medical mind is like any other mind. It would fain have certainty. As in intellectual philosophy; amid conflicting theories, where twenty rival truths stand up together to confound the page of inquiry; bewildered among deism and atheism, revelation and geology, spiritualism and materialism, idealism and eclecticism; but between Bossuet and Spinoza, Locke and Kant, the mind finds refuge in unbelief.

"Unable to reconcile all these, it turns to blind Chance as its only Creator; and in the very utmost stretch of pride of Reason, it bows itself to the disgrace of annihilation.

"Kindred causes make the expectant. To him two great and conflicting facts present themselves. One, the now acknowledged power of the *vis medicatrix naturae* in conducting disease to a safe termination: the other, the well-known influence of medicine. 'If,' he argues, 'twenty cases of pneumonia get well without treatment, and twenty get well with treatment, where has the physician the advantage over nature?'

"But, my dear Expectant," says Smelfungus, and up goes his pump-handle arm, as if to send the bucket to the lowest depth of the well of intellect; this proves nothing." It may be that every one of Dame Nature's cases would have been the better for antimony and blistering. Say nothing of the slovenly manner in which she performs her cures, and it is still evident that medical aid is efficient. Here you have an armament of cures; mighty weapons wrung from the bowels of the earth, the depths of the sea, or the surface of the dry land. With them you can change and modify the secretions, you can check and turn the current of the circulation, you can prostrate lusty health itself in death — are you then powerless in disease? Nature cures her patients — so do you. Do two affirmatives make a negative?

"Sir, if with all these powerful agents of the *materia medica* you have no control over disease, then are you a weak sister! You would be '*interpretes naturae et adjutor*;' be also her governor!

"It is said that 'incredulity is the first element of a philosophical mind.' It may be the first but there is a higher one — *Faith!* This expectant theory

savors of Pantheism. If it be true, Homœopathy is justifiable. It is a Pantheon, to which every idolatrous quack may bring his God!

“But I, Smelfungus, will yet believe that in mercury we have the mighty changer of secretions, in quinia the wonderful controller of disordered nervous action, and in opium the kind nepenthe for physical suffering.”

So much from Smelfungus on the expectant theory. I think were the truth known, that our friend intended in the beginning of his cogitations to think of something else. So let all this blast at the expectants go for nothing, while he resumes his broken chain of thought, and talks about blood poisons.

“I never was a solidist. All things solid are dry, and all things dry are dead. Juiciness is the *sine qua non* of vitality. But the most sanguine humoralist of former days would have stared, could he have known the extent to which humoralism is now carried. It does not stop with the contagious exanthemata, but runs on through the whole nosology. Not even nervous disease is excepted. I do not except it. The other day I treated a case of obstinate and persistent neuralgia with, what think you? It had previously resisted quinine, morphine, zinc, valerian, &c. Calomel, and that alone was the remedy, and was entirely successful in a few hours, and made a *permanent* cure. The rationale is simple. Clay-colored stools, and slight icterus accompanied the neuralgia. Now bile detained in the circulation is a blood poison, acting upon the nervous system, usually producing somnolency and dull headache. But these blood poisons act not always alike. Chloroform sometimes confines its effects to the motor tract of the spinal cord, giving no immunity from suffering. So bile may change the locality of its effect, and attacking the sensitive tract of the spinal cord, may cause neuralgic pain. Once upon a time, Smelfungus himself had neuralgia of the fifth pair. A certain distinguished professor traced a sinister relationship between it and an erysipelas then prevailing, and prescribed (*ex pede gnosce Herculem*) castor oil and turpentine as a ‘depurant cathartic.’ I have never forgiven him for that dose of unmitigated nastiness. It acted as an emetic merely. Another professor came; and when blue pill had done its perfect work, I slept a sleep the memory whereof is like a pleasant dream.

“In the last number of Braithwaite is an article ‘on the blood origin of a certain form of palsy, by Dr. Hamilton Kinglake.’ Here was a palsy, creeping gradually over the patient, until it was, to all appearance, likely to terminate fatally. But Dr. Kinglake followed out the indications presented to him, until he (*Dr. Kinglake* and not Dame Nature) effected a cure. I think his hypothesis is justifiable. He argues that the retention of urea in the blood caused the palsy, and the evident good effect of colchicum in the case

gives probability to the idea. It is well known, and actual experiment has proved, that colchicum does increase to a notable degree the quantity of urea eliminated by the kidneys. Carry out the suggestions afforded by this case and even you, oh Expectant! tossing upon the ocean of doubt and scepticism, may as you rise anon from the trough of the sea, catch a bright glimpse of the lighthouse of certainty in medicine.

“Feeble vision is not total darkness. The alembic of the chemist is solving many a weary problem, and shall yet place in our hands the divining rod which shall point us to the hidden springs of disease.”

As the most intimate friend of Dr. Smelfungus, it is my wish to make him shine in these papers. So I have urged him to take a text and stick to it. But he won't do it, and seems determined to prove Sidney Smith's assertion, that no man can, for two consecutive minutes, confine his thoughts to a single subject. If he does scatter his ideas like a watering-pot, let us hope that some feeble plant of wisdom may benefit by his aspersions—some “herb of grace” grow stronger. The urea has got into his brain now—for conscience sake let him urinate:

“I have got a patient who never had hysteria. She has palpitation—pain in left shoulder and left cheek-bone, numbness in the left arm, and flying pains in her wrists, ankles, and other locations. When these symptoms are worst, she makes many pints of limpid urine daily; this urine very deficient in urea. I believe nobody has ever attempted to account for the liberal outpouring of colorless urine which accompanies hysteria, epilepsy, and not unfrequently neuralgia. Would it be unphilosophical to suppose” (the reader must already have perceived that Smelfungus has a holy horror of hypothesis) “that this symptom may stand rather in the relation of a cause, than of an effect? and that colchicum may yet become an important remedy in nervous disease? And now I'll tell you something queer: this same patient was confined last spring at seven months, with a scrawny pair of twins. Her pains commenced in the left ankle, and there remained from 9, A. M. till 10, P. M., the os uteri meanwhile dilating as regularly as need be, without pain in the uterus. At 10 o'clock, P. M., the pains commenced in the uterus, the os being then well dilated, and delivery was completed in an hour. Furthermore, she had her afterpains in her ankle.

“What has this to do with blood poisons? It has a bearing; had you the eye to see it? If this patient had pure blood in her veins, think you she would have suffered those strange anomalies? That her blood was impure is proved by the existence of nursing sore mouth, for some time before and after confinement.

“Of late years sundry observers have noticed sudden deaths occurring in eruptive diseases, and attributable to subarachnoid effusion. The poison of scarlatina seems the more common cause of this, but it is also a sequel to other diseases. I have witnessed it in four cases. One was a child, convalescing from an apparently mild form of measles. In the other three cases neuralgia was a prodrome. In one of them I was unable to trace blood-poison, though as I only saw it *in articulo mortis* this may have no bearing. But in the two remaining cases — in one of them from nursing sore mouth — in the other from chlorotic anæmia — there was an unmistakable deterioration of the blood. In the latter case, by putting together this blood disease and a neuralgia of the back and side of the head, I was enabled, six weeks before death, to anticipate and warn the friends of the probability of subarachnoid effusion. Five minutes before her death, she complained of pain in the occiput and back of her neck, and requested her husband to rub her neck. While doing so, he found her insensible, with interrupted respiration, and she immediately died. It is not improbable that we might oftener be prepared for these events, were we to watch closer for faults in the rhythm of respiration, connected with neuralgic pain. And it is in blood diseases that this accident will most frequently occur.”

It is curious that Smelfungus has kept away from the subject of malaria so long in this discussion. But it is growing late, and the reader must be deprived of his argument thereon. But before we part he grows prophetic, and with his long nose elevated, he foretells the advent of a day, when by the aid of chemistry and an enlightened physiology, the physician shall prescribe, with an unerring certainty, for the elimination of morbid matter from the blood, or the supply of deficient healthy constituents.

However unwilling to cast a shade upon the enthusiasm of my worthy friend, I could not resist the temptation to quote to him the pithy lines of Hudibras:

“The wind that’s in the belly pent,
Is but a (*something unmentionable*) if downward sent,
But if perchance it upward rise,
It vents itself in prophecies!”

Smelfungus dashes his pipe upon the floor, and so breaks up our lengthy *sedesunt*.

ART. III. — *Suggestions as to the means of procuring an efficient law for the Registration of Births, Marriages, and Deaths, in the State of New York.*

The present law, all will agree, is good for nothing. It is equally true that were its provisions to be fully carried out, it would furnish a most valuable source of vital statistics. This point I do not intend to discuss as all medical men are agreed upon it.

What then are the reasons that a law in itself desirable does not work ?

The writer has for a year past held the office of town superintendent of Schools, and the details of his duties in that function have suggested to him a plan by which he believes the law can be carried out. The present provision is this: The school district clerk reports to the town clerk the number of births, marriages, and deaths; the town clerk reports to the county clerk, and the county clerk to the Department of State.

School district clerks are, upon a liberal average, a thick-headed race of mortals, having no interest in the law. The same may be said of town clerks.

District clerks have no other duty similar to this, and moreover have no official intercourse with the town clerk, save in this instance. In many cases it requires a special journey of six or eight miles, for the district clerk to make his report. It is, in every way, extremely inconvenient, and out of his accustomed line of action, for the district clerk to perform this duty; and the result is, it is left unperformed.

A parity of circumstance applies to the town clerk. He, at the season of the year in which his report is required, has no other official business with the county clerk, and caring nothing about the law, and receiving few or none of the district report, he fails to perform his duties.

Whether any penalties are attached to nonfulfillment I do not know. If there are any they are never exacted.

Now for my plan: A law of which self-interest guarantees the fulfillment, requires the *Trustees* of school districts annually to report the number of children in their district between the ages of four and twenty-one. This law is unfailingly complied with, as on it depends the school money of the district. Now, oblige trustees to *include in this report* the number of births, marriages, and deaths. Have the town superintendent, to whom the report is made, and who is generally a man of some education, and likely to feel interested in the law, withhold from all districts failing to report, their quota

of public money, and finally let the blank reports which are annually furnished to trustees contain the necessary forms.

The district reports once in the hands of the town superintendent, would unfailingly reach the State Department with his annual report made on the first of August, as self-interest again guarantees, for this report failing, no public money would be apportioned to the town. It will be seen that this plan takes the working of the law from the hands of ignorant and uninterested men, and gives it to men of education, with the strong guaranty of self-interest to insure its fulfillment. H.

ART. IV.—*Acute Laryngitis.* By WM. H. WHITE, M. D.

In the November number of your Journal I read a very interesting case of acute laryngitis, upon the subject of which, tracheotomy was successfully performed, while the patient was apparently in *articulo mortis*. I propose to give you the history and treatment of a case which occurred in my practice. I do not, however, expect it to be of equal interest as it was not necessary to perform tracheotomy, and therefore the *eclat* which hangs around *this*, as well as all successful surgical operations, is lost. Eminent surgeons have wisely said that it was more creditable to save a limb than to successfully amputate it. Viewed in this light my case may meet with some approbation, yet this is not my object in sending it to you. It is in the hope that it may be of benefit and interest to members of the profession, and call forth from them (as the above case did from me) cases and remarks of a similar nature.

Aug. 17, 1850. Mr. James McG——, an Irishman, between fifty-five and sixty years of age, called at my residence about 8 o'clock, A. M., (in a drizzling rain storm) complaining of a violent pain in the left cartilage of the pomum adami, which was increased upon pressure, with some considerable dyspnoea and difficulty of deglutition. He informed me that he had not been able to sleep the night previous, and could only lie on the right side: if he turned on the left side, or on the back, he smothered, and especially so while on the former. His voice was husky and much below its ordinary pitch, so much so, that had I trusted to it alone I should have presumed him to be laboring under an attack of acute bronchitis. The countenance was flushed, the pulse hard, the skin corresponded on touch to its appearance, it being red, dry and hot. On examining the fauces, uvula and velum, I found

them of a bright scarlet color, but no swelling either in them or the tonsils. I gave him calomel, grs. xxv, jalap, grs. x, to be taken as soon as he reached home (it being one mile distant) to be followed in two hours by sulph. magnesiae, ℥i. I saw him in half an hour or so after, and took xx℥ of blood and applied *immediately* a blister to the side and posterior part of the neck and a solution of N. silver, grs. xl, aqua pura, ℥i, I applied with a probang to the larynx and fauces, and in my absence he gargled his throat with a strong solution of acetum and chloride of sodium. I visited him six times in course of the day, and each time applied N. silver. At 11 o'clock, on visiting him. I found the cathartic had operated well; I then put him on digitalis, colchicum and calomel A. A. grs. i, every two hours. The blister having drawn, I dressed it with ungu. hydrarg.

Aug. 18, 4 o'clock in the morning, was called in haste to see Mr. McG. Found he had experienced great dyspnoea for the past three or four hours, was exceeding restless, and his countenance betokened the utmost imaginable fear and trouble of mind, and by the most excited actions urged to have the windows and door thrown open. I immediately applied the probang moistened with N. silver to the larynx when considerable — thickened mucus was expelled and relief to some extent was experienced. The fauces at this time did not appear so congested as the day previous. On inquiry I found that in the early part of the night the bad symptoms seemed to be mitigated. The blister was again dressed with ungu. hyd. and a wash for the throat and larynx made of the chlorate of potash ℥i, aqua pura, ℥ii, which was to be applied alternate every two hours with the N. S. The powder given the day before was continued.

10 o'clock. Patient somewhat easier save from pain at the neck of the bladder and in the testicle and cord corresponding to the cartilage of the larynx which was first complained of. For the new complication I gave a strong decoction of uva ursi and buchu, and had hops, soaked in hot water, placed over the bladder and testicle.

1 o'clock. Pain in bladder and testicle easier, otherwise no marked alteration. 4 o'clock. Pulse 86 and soft, not much fever, breathing easier, not as restless, but increased difficulty of swallowing, so much so that he thought it impossible to swallow the powders. I ordered injections of tinct. digitalis every four hours; if there should arise much febrile symptoms, every three hours. The application of the potash and silver continued every four hours.

Aug. 19th, 6 o'clock, A. M. Breathing better, voice quite clear, (which had been entirely lost,) can lie on either side; deglutition also better. **Treatment:** Injections of chicken broth with digitalis and colchicum every six

hours, and chlorate of potash to the throat continued. 12 o'clock. The mercury has had its specific effect. Calomel and ung. hydg. discontinued. The chicken broth, with the digitalis, continued every four hours.

Aug. 20th. Patient was quite comfortable.

Aug. 24th. Mr. McG. thinks he is well. Appetite good. Advised him not to take much food and no stimulating drinks. He has long been in the habit of taking intoxicating drink to excess.

Remarks. — I bled my patient but once, as I believe one general bleeding from 12 to 20℥, according to the habit and constitution of the patient, is all that is required in any inflammation. This class of diseases may often be temporarily relieved by bleeding at short intervals, as is by some recommended, but in my opinion it tends to leave the system in a state unable to perform a cure from a want of sufficient power in the active organs of life to rally when the main disease is subdued.

I blistered early. My experience has taught me its correctness. The counter-irritation and discharge from the denuded surface, has a direct and beneficial influence upon the seat of the disease, by causing the morbid excitement centered at the diseased point to be turned toward it, and thus a decided blow is at once struck. Most authors, and I presume most practitioners, judging from those I have met in council, &c., are opposed to this course in any inflammation, and especially directly over, or near the larynx, when it is inflamed; for fear of increasing the unfavorable symptoms by causing increased serous infiltration in the cartilage and glottis. Prof. Watson, however, says that "it is possible the œdema of the glottis might be produced or augmented in consequence of these topical remedies," (referring also to local bleeding.) From this there would seem to be a strong doubt in his mind as to its having this effect. It appears to me that the capillary vessels of the glottis are too *far off* to be materially affected by any external irritant.

I think the blister, N. silver and chlorate of potash, were the main remedies in effecting the cure.

ART V. — *Case of Tumor in the anterior wall of the Uterus, and extensive disease of the right Ovary.* By S. G. BAILEY, M. D., Buffalo, N. Y.

The following case may be interesting, in connection with others, of diseases of these organs; though, alone, it might not possess any particular interest. The subject of it was ill for the last ten years of her life. Her history

during that period is given as correctly as it could be gathered by careful inquiry—since January 8, 1852, during my attendance upon the patient,—up to that time when she first came under my observation. It was impossible to make a fully satisfactory diagnosis, which I believe is often true in these cases. The early life, and early married life, of the patient was healthy. She was a farmer's daughter in Canada — and says she has often spent whole days in the hay-field at hard work, as was the custom there in her young days.

Her parents were healthy. Does not know that her mother ever suffered in the least from uterine diseases or derangements. Patient was married at the age of seventeen years. Has had three children, the youngest now twelve years old, the eldest seventeen. After the second child, thinks she had an abortion, and was not well for near two years, when her health improved and she became pregnant with the third and last child. Does not recollect anything unusual in relation to her health during her last pregnancy. Enjoyed good health after the birth of her child for about two years, when she commenced to have the floodings that continued during the remainder of her life. From these floodings which occurred every month, and in increasing severity, she became so prostrated in about two years, that alarming faintings began to be experienced at these periods. The flow usually continued four or six days, by which time she would be compelled to keep her bed, which, aided probably by the exhaustion, would check the discharge, and another week in bed would suffice for her to regain sufficient strength to begin to get about again. A considerable strength would be regained between the periods, though subject to a constant slight discharge, a little colored, during the whole time. Never experienced much pain. Appetite always good; bowels regular. Has been troubled with a dry cough most of the time for the last five years, but it is exclusively sympathetic, and much increased by irritating the os uteri.

I first saw patient January 8, 1852. Within the last few months previous to that time, had been getting much worse. The discharge constant, profuse, and offensive, and much increased at the beginning of her monthly periods, but one day now of the flooding will completely prostrate her, and the discharge abates.

Has brown hair, blue eyes, blanched lips, and pearly skin. Is much emaciated. Thoracic organs are all good, organically and functionally. Examination by speculum presents the anterior lip of the os uteri much thickened and enlarged; the orifice and passage ulcerated as far in as could be examined. The ulceration I cauterized with Nit. Argt. There is now a dark-colored

and offensive discharge, which the speculum shows to come from, or through the uterus. Griffith's myrrh mixture and quinia were prescribed, for the system generally, and an injection of solution of Nit. Arg., locally. This treatment was continued for a month, occasionally omitting the quinia, in which time both strength and health had improved much. The flowing at the next period was less and more natural, and the discharge during the interval less and more healthy. She continued to improve till April 1st, though medicine was not constantly used, when, feeling so much better the medicine was omitted entirely, and I did not hear from her again until about the middle of May. The medicine was stopped contrary to my advice. May 1st, patient moved her residence, and thinks she over-worked. From this time she did not feel so well, and about the middle of the month the menses appeared, and the flow was again profuse. All the worst symptoms that she had ever had before, now appeared again, and her prostration was extreme. Large accumulations of coagula were discharged and the uterine organs were, from external appearances, increased in size with some tenderness manifest on pressure over them. About the 20th of May when the urgent symptoms had subsided a little, I discovered on more particular examination externally, a hard, round, movable tumor, which seemed to occupy exactly the position of the uterus. It was about the size and feel of the uterus, when contracted down, directly after delivery at childbirth. Was some tender upon pressure, but not remarkably so. In whatever position the patient placed herself the tumor would float upon the surface, or present itself at the most elevated point of the more yielding parts of the lower portion of the abdomen. Examination per vagina, afforded no assistance in the diagnosis, of the seat or character of this tumor.

It was discovered by the patient herself several days before I first noticed it, but it was, when first felt by patient, the full size that it ever attained. It appeared to her to have grown in a night. She did not seem to think it of consequence enough to call my attention to it, so accustomed had she become to afflictions, and changes from better to worse, and worse to better during the last ten years of her life. Of the diseased condition of the ovary we had no other indication than the discharge. There had been no hectic fever, or chills, or any other constitutional manifestations of so extensive a diseased condition as was revealed by post mortem examination. The local symptoms were fully sufficient to indicate extensive disease somewhere. The discharge per vagina had again become very offensive and irritating, of a dark color and thin in consistency. The treatment was resumed, as employed before, but with little satisfaction. In addition, some discutients were used.

over the tumor externally, though entirely ineffectual for good. About June 20th I asked counsel, and Dr. White saw the patient with me. We did not locate the tumor definitely in our diagnosis. In order to determine whether the tumor was within the cavity of the uterus or not, the os uteri was largely dilated by sponge tents. We found only some small warty excrescences, three or four attached near the fundus, and not larger than a large, long bean. They could be easily felt and distinguished by the exploration of the cavity of the uterus by the finger. These could not be the tumor felt externally, neither was it possible to tell now whether this tumor was made, or formed in the substance of the walls of the uterus, or one of the ovaries. Full examination, and exploration of the cavity of the uterus by the uterine sound, enabled us to determine that the tumor was not within its cavity.

From this examination, too, we were quite sure that it must be where the post mortem disclosed it, but that was not *clearly* evident.

Tonics, alteratives, and discutients, were from this time onward faithfully employed, which seemed the only means admissible for application in the case.

Anæsthetics and anodynes were attempted for the relief of pain, which at times of late had become very severe, but could not be tolerated.

She steadily declined, and died August 18, 1852.

Post mortem thirty-six hours after *death*. On opening the abdomen we found the anterior wall of the uterus entirely occupied by, and transformed into a hard, round, cartilaginous tumor, about $3\frac{1}{2}$ or 4 inches in diameter; the right ovary enlarged to about the same dimensions and excavated; so that its walls were quite thin, by an ulcerative action, presenting a ragged, rough, and black surface, internally, and secreting a thin, black matter.

The anterior lip of the os uteri was a little enlarged, though not so much so as upon my first examination in January, on first seeing the patient. The warty excrescences were found in the cavity of the uterus, as felt after the dilation of the os by the sponge tent. They were probably stimulated by the irritating discharge from the ovary, pathologically, possessing no further interest.

Several interesting questions arise in the consideration of this case:

First. The duration of the diseased action in the ovary; its origin and cause.

Second. Had this tumor grown entirely during the time from the last of February till the middle of May, or had it been a long time during the eight years forming, and growing slowly, and then thus suddenly developing itself in the space of three months? Nothing of the kind could be discovered upon my first seeing the patient.

Third. What influence had the tumor and the diseased ovary upon each other? The history of the case seems to indicate that the ulceration in the ovary must have been in existence from the first, producing those hæmorrhages from its surface indicated by the discharge of coagula at every menstrual period.

Fourth. Would it be possible for pregnancy to take place after the development of the disease in the ovary?

This question is worthy of consideration from the fact that the last pregnancy occurred after a supposed abortion, which, under the circumstances, might not have been such, the symptoms of which being similar to those often experienced afterward when no such condition could reasonably be supposed to exist. The opposite ovary, too, was perfectly healthy.

The diseased ovary, if distended, by filling its cavity with fluid, would be about the size of the hard tumor in the wall of the uterus, from $3\frac{1}{2}$ to 4 inches in diameter.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

The Liver and its Diseases. By W. B. HERRICK, M. D., Chicago, Illinois.

“The jaundiced thus see all things round them clad
 In yellow; every object as it flows
 Meeting new tides of yellow, from their forms
 Thrown forth incessant; and the lurid eye,
 Deep, too, imbued with its contagious hue,
 Painting each image that its orb assails.”

The above quotation from Lucretius, descriptive of a class of persons whose defective visual organs “see all things round them clad in yellow,” cannot fail to remind the reader of certain practitioners, the patients of whom are always *bilious*.

With them constipation or diarrhoea, dry skin or profuse perspiration, want of sensibility or extreme irritability, alike indicate that their patients are bilious, and require, therefore, in their treatment, blue pill, calomel, or some other mercurial.

This class of physicians, who thus make diseases so unlike in character and symptoms dependent upon the same cause, and, as a consequence, adopt the routine practice above indicated, must be deficient in judgment and mental capacity; or, what is worse, too indolent to obtain and appropriate to their

use the facts and information acquired by others, by which their mental vision might be extended, so as to embrace more than a single class of diseases and one mode of treatment.

In order to show that we are fully justified in making these strictures upon this class of practitioners, we will state briefly what is now known of the structure and functions of that organ, upon the abnormal condition of which these so-called bilious affections are supposed to be dependent.

The *liver*, as is well known, is a glandular organ, constituted of cells, excretory ducts, and blood-vessels. The cells are supplied by the *vena portarum* with the imperfectly elaborated and impure venous blood, directly from the absorbing mucous surfaces of the stomach and intestines; whilst the ducts, on the other hand, are surrounded by the terminal branches of the hepatic artery, containing pure blood from the great arterial current.

From recent physiological investigations, it appears highly probable that the hepatic cells abstract from the impure blood in the portal vein the starchy, and perhaps some other carbonaceous substances derived from food, and change them either into the fatty constituents of bile, or into sugar, to be reabsorbed by the hepatic veins.

That this change from starch granules to fat globules does in reality take place in the hepatic cells of the higher order of animals, is rendered almost certain by the observations made by Liedy upon the follicular liver of the crustacea.

"When," says he, "a *cæcum*, is viewed beneath the microscope, its lower half appears filled with a finely granular matter, and the anterior half with a mass of fat cells." That some of the carbonaceous substances contained in the blood are changed into sugar, during its passage through the liver, is made evident by the recent very conclusive and highly philosophical investigations of M. Bernard.

"He examined," says Donaldson, "the contents of all the principal venous trunks: the *vena porta*, the inferior and superior cava, the jugular, &c., and, singular to say, he could nowhere detect its presence, (sugar,) but in the hepatic veins, and in the ascending cava, and thence to the right auricle. There being no trace of it in the blood flowing into the liver, nor yet in the pulmonary veins, was not our experimenter justified in coming to the conclusion that it was fabricated in the liver and destroyed in the lungs?"

According to Liebig, the saccharine constituents of blood are, by two successive stages of oxidation, converted primarily into lactic acid, and finally into carbonic acid and water. Hence it would appear that sugar, whether absorbed directly as such, or formed in the liver, in the manner above indicated, supplies by its combustion the amount of animal heat required over and above that which would necessarily result from other and more important chemico-vital changes.

In view of these facts, it is rendered highly probable, if not absolutely certain, that the office of the hepatic cells is to take up the starchy materials, contained in the portal blood, and convert them either into fat or sugar, according as they are required or not to subserve the immediate purposes of respiration—into sugar when, from a deficiency of lactic acid and other organic compounds readily convertible into carbonic acid and water, there is a deficiency; and into fat, when an excess of these substances affords already an abundant supply of respiratory food.

The sugar thus formed is taken up by the hepatic veins, and passes immediately into the circulation, there to be changed by oxydation; first into lactic or some other organic acid, and finally into carbonic acid and water.

The fat, on the other hand, passes into the terminal branches of the hepatic ducts, where it finds, in the capillary net-work derived from the hepatic arteries by which they are surrounded, an abundant supply of arterial blood. This, doubtless, furnishes both the oxygen and the alkali, by which the fatty matter is rendered soluble, and made to pass readily and easily through the small hepatic ducts as a fatty acid combined with soda, in the form of bile.

These views of the physiological action of the liver are fully sustained by numerous facts, physiological, pathological, and chemical, which, however, cannot be presented in the short space allotted to this article; it being our object at this time, not to sustain our own peculiar physiological views, but to make such practical suggestions as may serve to direct the attention of our readers to the subject, and to show them the absurdity of the present indiscriminate mode of practice, adopted by many, in the so-called bilious affections, supposed to be dependent always upon some morbid condition or action of this much-abused organ.

From what has been said, it is evident that in warm latitudes, and in summer, when there is less oxygen, and, consequently, more lactic and other organic acids in the blood, the liver must change a larger proportion of the starchy constituents of food into fat. If the amount of oxygen and free soda in the blood is sufficient to combine with this fat, and render it soluble, it passes readily out of the liver into the intestines, in the form of bile, and is reabsorbed by the lacteals, like other fatty matter, and no indications of disease appear; or if in great excess, it passes off in the form of profuse bilious discharges, so common in the summer, especially in the South and West. A still greater deficiency of oxygen, and consequent accumulation of organic acids in the blood, to combine with its alkaline constituents, would diminish proportionally the amount of free soda, and thus prevent it from entering into the constitution of bile to a sufficient extent to make it perfectly soluble, and to neutralize its fatty acids, and thus give rise to acrid and vitiated bilious discharges, or to congestion, torpidity and enlargement of the liver, from an accumulation of imperfectly dissolved fatty matter in the hepatic ducts.

Admitting the correctness of the above views, it is evident that the proper treatment of the whole class of liver affections, above enumerated, would be the administration of alkalies, especially those which are among the natural constituents of blood, such as potash and soda.

Two years' experience in the use of potash and soda, in some of their forms, as remedies in the above named class of diseases, has convinced the writer that one or both may be used with confidence as substitutes for calomel in the treatment of such cases.

That the class of remedies under consideration was formerly used much more extensively than at present in liver affections, is evident from the following quotation from Good's Study of Medicine, published in 1829, in which, after discussing the merits of the dandelion as a remedy for jaundice, the author remarks, that "soap and alkalies seem to have much better pretensions to favor, and have been still more widely employed in this disease, and pretty generally regarded as general, and hence hepatic solvents.—*North-western Med. and Surg. Journal.*

Some Remarks upon the Functions of the Nervous System, in connection with the views of Sir Chas. Bell, Dr. Marshall Hall, and Dr. Bennett Dowler. By B. F. TAYLOR, M. D., La.

It is known to the student of physiology, that Sir Chas. Bell long ago announced his views in reference to the functions of the spinal cord, and endeavored to prove that it consisted of *four sets of fibres*—each of which was made to perform a separate function, though intimately associated with each other.

- 1st. A *sensory bundle*.
- 2d. A *motor set*.
- 3d. A set of *excitor* or centripetal fibres.
- 4th. A *motor* or centrifugal set.

The first and third are united in the posterior, the second and fourth in the anterior column of the spinal marrow. In other words, the *anterior for motion*, the *posterior for sensation*, and the *middle column for respiration*.

After the inception of this new theory, physiologists found it exceedingly difficult to trace the course of the fibres within the spinal cord; in consequence of which, Sir Charles Bell's views began to be distrusted. In the mean time, cases were constantly occurring, when a portion of one of the columns was found almost entirely destroyed by disease, with a corresponding loss of function.

Whilst the merits and demerits of this new discovery were being warmly discussed, by British and Continental Physiologists, a new star arises, in the person of Dr. Marshall Hall, who was destined to shed a brilliant, but an ephemeral light, upon the profession—but to give place to one far greater in magnitude and brilliancy, in the person of an eminent *savan*, whose genius and discoveries are destined to reflect additional lustre upon the American name and physical science wherever it is cultivated.

Dr. Marshall Hall contends, that the nerves of the spinal column have a *fourfold set of functions*—a *double set of excito-motor, and of sensori-volitional nerves*—and explains every phenomenon connected therewith, as purely reflex in its character. It cannot be denied but that this theory is surrounded with a great deal of mystery, since the editor of the London Lancet, in his rapturous support of the doctrine, was compelled to announce his conviction, that “not half a dozen of the members of the Royal College of Physicians could comprehend his peculiar views; and that he was an hundred years beyond his contemporaries,” &c. Looking through a *Hall-medium*, it is not to be marveled that Mr. Wakly should still continue to support those views, since it is shrewdly suspected that Marshall Hall himself controls that able and influential Journal.

Dr. Bennett Dowler, with a view of testing the truth of Sir Charles Bell's and Marshall Hall's views, has made a series of experiments upon the great Saurian, whose tenacity of life is greater than that of any known animal, and who exhibits the phenomena of reflex action upon a much greater scale than frogs—which demonstrate the fallacy of a “fourfold set of functions,” in opposition to which, very satisfactory and nicely conducted experiments are adduced.

In reviewing Dr. Dowler's paper, the editor of the British and Foreign Medico-Chirurgical Review, the most able reviewer in Europe, had the manliness and moral courage to come out boldly and renounce his adhesion to

the "fourfold system of nerves," "now generally admitted," says he, "amongst well informed physiologists, such having, *as we now believe, no real existence in nature.*" An admission, Dr. Hester judiciously remarks, which "forms an epoch in scientific progress, because with certain individuals it will weigh more than any amount of *demonstration, intuition, or possibly revelation* itself."

Dr. Dowler's discovery of a *diffused sensorium*, has furnished a key to the hidden recesses of the nervous system. The adaptation of those views to therapeutic medicine is most strikingly and beautifully illustrated in man's first ingress into this breathing world. The first respiratory effort of the new born infant is most vigorously performed when the cool air comes in contact with its general surface. Accoucheurs avail themselves of this important fact; hence the utility of slapping, frictions, and the application of cold water, in more effectually exciting the respiratory movements. In the treatment of asphyxia by pouring, hysteria, &c., the alternate application of heat and cold, is most powerfully manifest in restoring these movements. All of these phenomena are in harmony with nature's laws,—written upon the nervous system,—and the application of the doctrine of a *diffused sensorium*.

January, 1852.—*N. O. Med. and Surg. Journal.*

Extraordinary Precocity in the Development of the Male Sexual Organs and Muscular System in a Child Four Years Old. By ROBERT KING STONE, M. D., Professor of Physiological Anatomy in the National Medical College, and one of the Surgeons of the Washington Infirmary. (Read to the Pathological Society of the District of Columbia.)

I have the honor to present to the Society one of the most extraordinary cases of precocious development of the male sexual organs and general muscular system now on record:

Mr. Charles S——, of this district, brought his son, Theodore, to my house on the 14th of September, 1852, his birthday, for my inspection and opinion; stating that on that day he was four years old. I at once declared my incredulity, for his height and robust development seemed those of a child at least six years older than the age he mentioned. My astonishment was greatly increased, when, on stripping the boy, he offered to my view the well-developed sexual organs of a man, and the pubes covered with a luxuriant growth of hair.

I was perfectly incredulous that the boy was born on the 14th of September, 1848; but his father said he could produce his certificate of nativity, and that he, with his mother, the midwife who delivered him, and fifty other responsible persons, would swear that he had stated his age correctly.

The boy is remarkably handsome, and when stripped, presents a form of great beauty, which is, in fact, a miniature model of a perfectly developed *athletæ*.

The condition of his muscular and osseous system is extraordinary; the deltoids and other muscles of the arm, forearm, back, and thorax, have the same relations to his height that those of a hard-laboring man would have of the stature of six feet. The muscles of the thigh, gluteal region, and leg, are

perhaps better developed than those of the upper extremity, but in nearly the same ratio to the height.

If the child's face is concealed, the examiner would declare his figure to be that of a miniature man, perfectly developed, and at least twenty-one years of age.

There seems to be little adipose tissue about him, the muscular prominences being clear, and well defined, as if produced by constant exercise or hard labor.

The growth of hair is distinct in the axilla, but by no means so marked as that upon the pubes. As in very robust men, the lumbar and sacral regions are covered with a thick down of dark hair.

His height is now four feet one-quarter inch, and weight nearly seventy pounds; though his mother informs me he weighed seventy-five pounds in the spring, and attributes his diminution to the great number of lumbricoides which infest him.

His penis is that of a well-developed man, measuring in a semi-flaccid state four and a quarter inches in length, and in the state of perfect flaccidity three and a half inches. The prepuce is short, leaving exposed a perfectly formed glans penis. I might state, also, that the papillæ of the corona glandis are in a state of hypertrophy, being distinctly salient, and exquisitely sensitive. The pubes are covered with a luxuriant growth of crisp, curling, dark-brown hair, as found in the adult state. In the scrotum, presenting the appearance of the adult, are two firm, apparently well-developed testicles, perhaps rather under the average size of those organs in the adult. Independently of the penis, the development of these alone would have been decidedly remarkable at that tender age.

The spermatic cords are distinct, and under the finger give the impression of perfect organs.

Carefully examined from the neck down, the appearances are those of a *perfect man*, whilst the head and face were those of a child. On examining his mouth, it was found to contain only the twenty deciduous teeth of his age, with the exception of the middle incisors of the upper jaw, which were carious to the fangs.

The head was perfectly formed, and bears a proper proportion to the development of the body.

The breadth between the ears across the cerebellum was great; in fact, the anterior development of the cranium was less than the posterior; yet the relation could not be called bad at his early age.

The boy is lively and seems intelligent, though his speech is imperfect, but he pronounced with facility after his father. He seemed unwilling to talk of his own accord before strangers; his father informs me, however, that he is very talkative at home and quite intelligent. His temper is good, and he is almost always in good-humor, but when excited by anger, his father alone can manage him, which he does by an old-fashioned, knock-down blow.

His father observed last night, when he slept with him for the first time, a constant erection of the penis, accompanied by a nickering, like an excited stallion, and for these reasons consulted me.

The boy has almost always slept by himself, and on an hard pallet on the floor. His back and shoulders are covered with the *acne simplex* of puberty. He has never been known to attempt masturbation, nor is it known whether he has had sexual relations, although the organ has that appearance. The

slightest touch of the penis excites it, and the organ becomes tumid and of the average adult size, during the requisite examination.

The voice is that of puberty, and has been so for some time.

On the 15th of September, I visited him, accompanied by my friend and colleague, Professor John Frederick May, who verified the preceding examination and my measurements.

He is the seventh child and third son of his mother; weighed eleven and a half pounds at birth, and fifty-six pounds at three years.

At birth, the glans penis was perfectly uncovered, and the hair on the pubes half an inch long; at one year, things were just as they are now.

Around the thorax under axillæ, he measures 2 feet 1½ inches.

"	hips	"	"	2	"	2¾	"
"	thigh (middle)	"	"	1	"	2	"
Penis in semi-flaccid state		"	"			4½	" long.
" flaccid state		"	"			3¾	inches full in circumference.

Around the arm, below insertion of the deltoid muscle, he measures eight inches.

Around the neck he measures one foot.

Around the head (above ears and over hair) he measures one foot eight inches.

From meatus auditorius to meatus of opposite side across the occiput, he measures 9¾ inches.

Although his neck is full, there is no remarkable development of the laryngeal cartilages, Pomum Adami.

The next question is in regard to the power of the testicles to secrete. Since I first saw this man-boy, his father has made inquiry as to this fact, and states the following to me as the result:

On the 13th of September, he slept with a near relative, a married lady, the mother of several children. In the middle of the night, she was aroused by finding the boy closely clasped to her back, and her night dress saturated. She thought he had emptied his bladder upon her, but on carrying her hand to the part, she found that it was saturated with a *very different and glutinous material* from that she expected.

I regret that I could not obtain the ejected matter to submit it to a microscopical test. The boy is extremely fond of embracing the opposite sex, though nothing further has been ascertained. In no other of the seven children borne by the same mother, has the same condition been observed, and in comparing an elder sister of ten years, I found she was extremely delicate, and only half an inch taller than Theodore.

I have several times seen him during an attack of nickering, and am satisfied that it is produced by a tendency to epilepsy.

Since writing this account I have been furnished by Dr. W. A. Williams, with a certificate, stating that he has known the family of Charles S— for the last seven years; that he knows Theodore was born in September, 1848; that he saw Theodore when a very young infant, in the arms of his mother, and knows, from having seen him almost every week since that time, that he is the same child who was presented to the Pathological Society of the District of Columbia, by Dr. R. K. Stone, on Friday, Sept. 17, 1852.

Further, Dr. Williams states that he has attended the family professionally for about eighteen months, and was a student of medicine at the time

Theodore was born; and that he was first aware of his precocious sexual development in January, 1852, from actual inspection, though he had been informed of it at an earlier date.

In terminating this simple statement, I may observe that the father presented extreme precocity, having experienced his first sexual indulgence at the age of 8 years. He informed us that between the ages of 10 and 13 years he was a *better man* than he has ever been since. Delicacy forbids my detailing his prowess at that early age.

This extraordinary case will be perfectly under control for some time, and I will most willingly make any further observations which may be dictated by better heads.

It will be observed, that this is perhaps one of the most extraordinary cases on record, and I will now proceed to a hasty examination of a few of those to which reference has been made for me, in an exceedingly brief space of time, by my friend, Dr. R. D. Coolidge, U. S. A.

Precocious Puberty in Males.—1. An Account of a Child, 3 Years Old. By GILBERT BRESCHET, M.D. *Philadelphia Journ. of the Med. and Phys. Sciences*, 1821, vol. iii, pp. 417–18. James A. Sarin, born 20th October, 1817; 3 years and one month old at report; weighs fifty pounds, 3 feet 6½ inches high; penis when flaccid, 4 inches long, and 5½ when erect. Testicles *not enlarged* in proportion to penis.

2. *Good's Study of Medicine*, p. 73. N. Y. edition of 1829. BOISSET, in the *Journal des Savans*, gives a case of a boy 3 years. *Philosophical Transactions*, 1745, boy 2 years 11 months. These were cases of great salacity, and no description given. Mr. DANKEB, of St. Ives, near Huntingdon, reported the "Prodigium Willinghamense." The boy was buried at Willingham, and his epitaph was, "Born October 31, 1841, died September 3, 1747. At one year had signs of manhood, not 3 years was nearly 4 feet high; stupendous voice, and he died of premature old age."

3. PLINY (*Hist. Nat. Lib.* 8, c. 17,) reports a boy at Salamis 4 feet high, and attaining puberty at 3 years.

4. The case seen by CRATERUS, brother of Antigonus (*Phlegon, De Mirabilia*, c. 32,) who was infant, youth, adult, father, old man, and corpse, in 7 years.

5. *Milburger's Curiosities*, several cases.

6. Case reported by A. LOPEZ, M. D., of Mobile. *Amer. Journ. Med. Sciences*, vol. v, 1843, p. 500. Mulatto boy aged 3 years, 10 months, and 15 days; weight 82 pounds; height 4 feet half inch; width around chest 27½ inches; belly 27 inches; thigh 19 inches; arm 9½; circumference of head 22 inches; length of penis at rest 4; circumference 3½; his scrotum has a fair proportion to the other developments; but the *testes* have not descended; has whiskers; axillæ hairy; teeth 20, and *deciduous*; lifts a man of 140 pounds; covered with *acne simplex* (as Theodore S—— is) of puberty. Has spermatic odour, but not known whether he has venereal appetite; judge from stains on his shirt.

To these, I could add from *Beck's Med. Jurisp.*, p. 519, vol. i, 1838, several cases in both sexes.

Very many cases of precocious development in the female might be adduced: but to them I do not care to draw your attention; it is only necessary to recall the fact that the mother of a family on the banks of the Ganges need only be 9 years of age.

There has been no time to investigate this subject fully, but I think it will be difficult to find a case comparable to the one which I now present you. In most of the cases to which reference has been made, there is something wanting; and when we examine the totality of the appearances, there is not one, except the case described by Craterus, which approaches the proportion of Theodore S——.

WASHINGTON, D. C., Sept. 1852.—*Am. Jour. Med. Sciences.*

On the Anemia of Pregnant Females. By GEO. MARTIN, M. D., of Delaware County, Penn. (Read before the Delaware County Medical Society.)

Every obstetrical writer mentions plethora connected with pregnancy as the source of some of those diseases, which, when they occur, so often compromise the safety of both mother and child.

Much has been said about it, and so little about anemia, that it has been and is yet considered by some practitioners as the almost constant complication of pregnancy. The bare announcement of a poor woman being pregnant, has been to them clear evidence that she was plethoric, and a sufficient warrant for the use of the lancet; and to so great an extent has this been carried, that many females, even at the present day, think that they cannot be delivered of a healthy child, and themselves do well, if they have not been bled once or twice before the pains of parturition come on. Moreover, the physician who has been called upon to perform the operation, if he should chance to think differently, and have the hardihood to stand by his opinion, will find, in the event of a misfortune occurring either to the mother or child, that the blame will rest upon his shoulders; and there will be persons, and those not a few, who will not hesitate to tell him that the result would have been otherwise if he had not refused to bleed.

Now this is certainly wrong; for though no one will deny that plethora does exist in some instances, yet it does so much more rarely than anemia; and I shall here endeavor to show that most of these cases in which bleeding is resorted to, arise from an impoverished blood, and that the use of tonics and even of the chalybeates would be followed by the happiest results.

Are the nausea, vomiting, and depraved appetite that are so common, and occupy so large a portion of gestation, symptoms to lead to suspicion of danger from an over supply of nutriment? On the contrary, if such derangements of the digestive apparatus were met with at any other time, would we not immediately try to relieve them, for fear that debility, emaciation, and even death might ensue? All medical experience points to such a course; and why should not the result be the same in one case as well as the other. There surely is nothing mysterious hanging over the pregnant female that will reverse all our known laws of the animal economy.

It has been said that the stoppage of the menstrual secretion counteracts these influences, and produces the supposed plethora; but this can hardly be, for the amount of nutriment drawn from the blood in forming a highly-organized living being weighing from seven to eight pounds, with two or three more of appendages, must far exceed the amount required for nine catamenial periods.

The symptoms calling for depletion, as they are commonly described, are

headache, vertigo, flushes of heat, depression of spirits, a full, frequent pulse, a feeling of fullness and pain in the pelvic region, and a tendency to hæmorrhages in various parts of the body. Now it will be easy to show that many of these are often produced by anæmia; and though bleeding may relieve some of them for a time, they will be sure speedily to return, as the remedy only aggravated that condition of the circulating fluid in which they originated; and to this may be attributed that necessity of a frequent resort to the lancet which many practitioners will tell us they have found. Anæmia also has a strong tendency to derange the circulation; for when the blood is in this condition, it does not carry with it that stimulus which is necessary to excite the capillaries to do their part, and congestion will frequently ensue.

The nervous excitability, too, is greatly increased by this condition of the system, and this is the common cause of simple neuralgia, which it may produce in two ways: first, by its not being stimulating enough to the nervous centers to maintain a healthy action in them; and secondly, by its not affording the different organs a proper amount of good plasma to keep them in a healthy condition, and they demand through the nervous system a better supply. This also is the reason of that frequent, easily excitable, and sometimes full pulse which is so often met with in pregnancy and anæmia; for the heart here, in endeavoring to answer the demands made upon it, acts much more rapidly than it does in health, and as these wants are augmented in proportion to the amount of labor that is to be performed, we shall find that the least exertion will be followed by a great increase in the number of pulsations. During gestation, this excitability is manifested to a great degree, though in this case it is not to be attributed solely to the impoverished blood, for the nervous disturbances created by the changes wrought in the womb after conception tend considerably to increase it.

If we investigate the manner in which the above symptoms are produced, we shall find when they may be considered as evidences of plethora and when not. To begin we will take the pain in the head, which, when not sympathetic with some other disorder, often arises from neuralgia, sometimes from congestion at others from irritation, and occasionally from inflammation. Now, I have before stated that anæmia is the common cause of neuralgia, and I have shown that it strongly predisposes to congestion, which will be liable to take place whenever there is any excitement or irritation of the brain; and this may occur in any condition of the system. Yet, the result will be very different in the two cases; for when plethora exists, we shall have inflammation immediately following the congestion; whereas, in anæmia, it may exist for some time without it. And this is very true in pregnancy; for here we sometimes have it lasting for weeks; producing much suffering, and at times followed by the most disastrous results, without our being able in many cases to discover a symptom of inflammation during life, or a trace of it after death; and when it does occur, it will generally be found to have arisen from some direct injury which the overloaded vessels have inflicted upon the tissue of the organ. Vertigo generally arises from it, as does also the disposition to hæmorrhages, when they do not occur from the vitiated blood relaxing the vessels, and thereby obtaining a free exit; and the sense of pain and fullness in the pelvic region must be attributed to the same cause affecting the womb.

The flushes of heat and other nervous symptoms not unfrequently met with proceed from the excitability before treated of as arising from a deficient

plasma; and as this must necessarily have a great influence over the moral junctions, too, it will account for that depression of spirits so common in gestation.

From the foregoing facts, it will require no force to arrive at the conclusion that the symptoms of anæmia greatly preponderate in the above; and to them I shall add a few others which will serve to place the subject in a still clearer light. Thus, the face often appears somewhat bloated and discolored; the blood, when drawn, frequently presents the buffy coat without their being the least evidence of inflammation existing at the time; the carotids throb, and at times even a partial loss of vision ensues. Now, these are all known as symptoms of anæmia; and the latter has been frequently produced by an extensive hæmorrhage. In an analysis of the blood by Andral and Gavarret (for the account of which I am indebted to Caseaux's work on Midwifery,) thirty-two cases out of thirty-four were found in which the globules were below the healthy mean, in six of which they ranged from 120 to 125; and in twenty-six from ninety-five to 120; they also found for the first six months the fibrine in the thirty-four cases was uniformly below the natural quantity, varying from 1.9 to 2.9; while during the last three months it exceeded it, ranging from 2.9 to 4.8, and averaging nearly 4; this, therefore, gives the true reason for the buffy coat; and we here have thirty-two cases out of thirty-four in which anæmia was proved to exist.

The treatment of this affection should, of course, consist of tonics, the best of which are the ferruginous preparations, a nutritious diet, fresh air, and moderate exercise; the nervous symptoms, when excessive, should be allayed by the antispasmodics and opiates; and when congestion occurs, it will generally be speedily relieved by cold applications to the part, counter-irritants and rest, with a little aperient medicine if the bowels should be constipated; and perhaps it may not be out of place to mention here, that when the brain is the seat of engorgement, the best mode of applying the cold to it will be by pouring water of a low temperature in a steady stream upon the head, care being taken to watch the patient, that she be not too much prostrated thereby. It would be easy to cite numerous instances in which the above treatment has relieved headache, vertigo, and even convulsions, in some of which after depletion had been fairly tried and failed; but I trust enough has been said to show the real character of the complaint, and with a few words upon the injuries that a false diagnosis may inflict upon the child, I shall bring this subject to a close.

It is an acknowledged fact that tubercles and scrofula often originate in a depraved condition of the system, and as the vital organs of the fetus at the time of their formation are undoubtedly in their most delicate condition, and very susceptible of change, may not the imperfect plasma, which is all the parent can furnish out of her impoverished blood, lay the foundation for these diseases, which only wait for some exciting cause to call them into action? And if this be the case, how highly important it is, both to the mother and her offspring, that the true nature of the affection be clearly understood, and the proper remedies at once applied.—*Am. Jour. of Med. Sciences.*

Dr. Condie inquired of the Fellows present, whether they had any experience in reference to the effects of quinia in large doses in the treatment of

typhoid fever. He had himself employed it in five cases occurring in children, in all of which the characteristics of the disease were distinctly marked. The fever was in no instance cut short by the quinia; but it certainly appeared to him, that under its use, the duration of the disease was very materially shortened, and convalescence more promptly and fully established. The quinia was given in solution, in doses of from two to three or four grains, every three hours, according to the age of the patient, combined with one-third the quantity of tannic acid, by which the intense bitterness of the quinia was in a great measure removed, and one great difficulty in the administration of the remedy to young patients done away with.—*Quarterly Summary of Trans. of College of Physicians of Philadelphia.*

Treatment of Hooping Cough.—In hooping cough, Dr. Madison, of Virginia, recommends a blister to the nucha; Dr. Golding Bird uses *conium*, in the following form:

℞ Ext. Conii, grs. xii.
 Alum, grs. xxv.
 Syrup Papaver, ℥ijj.
 Aq. fœniculi, ℥ijj.

M.

A desert-spoonful every four or six hours.

Dr. Spengler, in a French journal, recommends *conia* in doses of one-fortieth of a grain for children three or four months old, and one-tenth of a grain for children of as many years. He has employed it in several cases successfully.—*Western Jour. of Med. and Surg.*

Solution Gutta Percha in Chloroform. By PROF. L. A. DUGAS.

This is made by dropping into a vial containing chloroform small fragments of pure gutta percha, until the solution acquires the consistence of thick mucilage. It is then applied with a camel-hair pencil, which should afterward be repeatedly dipped in pure chloroform and carefully wiped with paper or old linen, so as to prevent its becoming stiff and unfit for further use.

I will now relate the result of its application in two cases of cancerous affection.

Mr. L.— had been troubled with an epithelial-cancer of the lower lip which had resisted all applications for eighteen months. There existed, upon the right side of the median line and at the junction of the skin and mucous surface, a small and thin scale or scab, which would occasionally fall or be rubbed off, leaving a raw surface of exquisite sensibility exposed to irritation until another scab would be formed. Beneath this surface there was an induration about the size of a common pea, or rather a little larger, in which the patient felt a very annoying sense of burning, and sometimes darting pain.

At this stage of the case, as the patient was averse to the knife, he was advised to try the application of collodion, which he diligently persevered in for about six months, applying it three or four times daily. This arrested the farther growth of the disease, relieved its itching and burning, protected its surface from ordinary irritants, but did not heal the denuded surface. He then substituted the solution of gutta percha in chloroform in lieu of the collodion. In a letter to me he thus describes its effects: "In a few days I saw and felt a change in the color of the sore and in the irritation; in a week or ten days, the lump disappeared and the irritation subsided, and in three weeks it was almost entirely healed over; in less than a month it was well, leaving an indentation on the lip."

In a note dated the first of this month, (April,) my patient writes me: "I begin to fear a return of it in the same place the cure was made eighteen months ago. Recently a lump has appeared in the lip; it is *hard* and sometimes a little sore—it gives me no trouble yet, but I am afraid of it." I will advise him to use the gutta percha and chloroform again.

The next case was that of Maria, a negress, about fifty years of age, who was sent to me from the country on the 3d of November last, with a cancerous ulceration of the mamma of several months standing. Both mammary glands were very much atrophied, but the affected one was the smaller of the two, presented nothing but a mass of schirrous induration which seemed adherent to the thoracic walls, and in the depressed center of which the remains of a nipple were to be seen drawn back and ulcerated. The ulcer covered a surface equal to the areola. The axillary glands were much enlarged, and the patient a prey to continual pain, especially at night, which deprived her of sleep.

Feeling satisfied that the knife promised no relief under such circumstances, yet unwilling to send her off without trying something, I put her upon the use of the gutta percha and chloroform, thoroughly coating the whole breast daily with it. The discharge from the ulcers would at first cause the pellicle of gutta percha to become loosened in twenty-four hours, so that the surface had to be cleansed before the reapplication of the remedy. The suppuration, however, gradually lessened until the coating could remain a week—the painting still being made each morning. Under this treatment, the patient was gradually relieved of all pain about the breast and even in the axilla. She slept quietly at night, enjoyed her meals, and felt quite well. Her general health improved and she left at the end of one month, with instructions to continue the treatment perseveringly, and to get her master to inform me of the result. I have had no report from her since, but have learned incidentally that she never applied the remedy after she left here, and placed herself under the charge of some one who professed to be able to cure cancer—with what result, I know not.

These two cases are narrated with the simple purpose of directing attention to an application which may stay, if it does not cure, so formidable an affection as cancer.—*Trans. Med. Society State Georgia, 1852.*

Solution of Shellac.—The costliness of the solution of gun cotton and of gutta percha renders it desirable to have a cheaper article that may be used as a substitute in cases which require the consumption of a large quantity of

such plastic materials. A solution of shellac in alcohol has therefore been proposed for this purpose. This may be prepared by adding successively small bits of shellac to the alcohol of commerce until enough be dissolved to make a mucilaginous solution.

Some of the French practitioners having attributed to collodion extraordinary antiphlogistic properties when applied over affected joints and other inflammatory affections, even more deeply seated, I determined to try, during the last winter, the shellac solution in an old case of rheumatism, in which most of the joints of the extremities were being successively invaded. The toes, ankles, knees, fingers, wrist and elbows were nearly all alternately implicated—becoming very painful and rapidly swelling, so as to be almost doubled in size in a day or two. I furnished the patient a bottle of the shellac solution and ordered it be painted over and around the joint as soon as it would commence to be painful, and to repeat the application several times a day until a thick coating remained, after which it might be applied only once a day. Under this treatment I was gratified to find that the patient could, in a few hours, arrest the pain and prevent the swelling of the joints to which he made the application. He stated that he never had anything to give him such prompt and effectual relief, although he had been suffering such attacks every winter for the last ten years. One joint or another continued to annoy him for a month, during all of which time he resorted to the shellac with the same success.

This is the only case in which I have tried this solution.—*Ibid.*

The Radical Cure of Reducible Hernia, by Injection.

By JOHN WATSON, M. D.

I do not propose, just now, to go into the whole merits, or demerits, of this operation, a task which has been assigned to other hands, and which has already been in some degree achieved, by one of the committees of the American Medical Association (see their report in the Transactions for the session of May last.) My only object is to give the details of a single case treated in this way, in connection with such hearsay information as I have been able to collect concerning the operation in other quarters.

The procedure now under consideration, if I am not mistaken, was first brought into notice through an irregular channel, by a certain Industrialist, of New England. But the first notice I remember to have seen of it, was in Professor Pancoast's *Operative Surgery*. In July, 1848, my attention was for a second time called to this subject, by a gentleman under my care for the treatment of varicocele, who, a year or more previously, had undergone an operation for the cure of a reducible hernia, by what he described as a trifling process, and with complete success. He spoke of it as a simple puncture, which subjected him to very little uneasiness; and assured me that he was cognizant to the cure or relief of other individuals, who had been treated like himself, by a practitioner of Boston. I subsequently ascertained that the instrument with which this individual operates, had been prepared by a cutler of this city; but on inquiry I found that it had been patented by the operator, and was, consequently, to be used only by himself. As I had made inquiry for it, with a view of employing it on a case then in hand, I

could not but feel indignant that any practitioner claiming to belong to the regular profession, should have thus prostituted his noble calling to mercenary ends; but believing that no special form of patent instrument is essential for the making of a puncture, or for introducing an irritating fluid beneath the integuments through this, I attempted in my own way to get along without it, as in the following case:

Joseph A. Seavell, of Ohio, seaman, aged 31, was admitted into the New York Hospital, Nov. 24, 1851, with a large inguinal hernia, occupying the left side of the scrotum, which had been then protruding for several hours, and had resisted several well-directed efforts for reduction. The patient for the last four years had been occasionally troubled by the protrusion, but had never before been baffled in his efforts to reduce it; and by the use of a truss he had been able to follow his regular occupation. With some little trouble the tumor was reduced by taxis, soon after his admission; and on the 29th of November, having explained my object to the patient, and obtained his consent, I attempted to effect a radical cure of the hernia.

While the patient was lying on his back, with his scrotum and left spermatic cord drawn slightly toward the right side, and with the integuments over the left external abdominal ring slightly on the stretch, I introduced the point of a delicate bistoury through the integuments, directly down to the crest of the os pubis, the point of the instrument touching, without dividing, the lower termination of Poupart's ligament, and made to work freely in the loose tissue immediately in front of the ring, but without wounding the spermatic cord. Having made the puncture, and withdrawn the bistoury, the nozzle of a small syringe charged with tincture of cantharides, was introduced through the wound, and about a drachm of this fluid was injected into the bottom of the cut, the hand of an assistant, in the meanwhile, resting firmly over the inguinal canal to prevent any portion of the injected fluid from entering this, or passing through the sac into the abdomen.

The whole procedure was the work of a few seconds, and gave the patient little or no uneasiness. I next applied a compress and spica bandage, to keep the parietes of the inguinal canal in close apposition, and administered an anodyne, keeping the patient on his back, with directions to apply an evaporating lotion, should severe inflammatory symptoms supervene.

In a few minutes after the operation, he began to speak of pain from the injection. The sore became more troublesome, and extended for several inches in every direction, but was severest along the ascending track of the spermatic cord. He slept but little during the following night; but next morning the pain had subsided, a slight soreness, only, remaining in the part. The patient was at the same time suffering from chancres. I made the treatment of these the pretext for keeping him on his back with the compress and bandage applied as above, for several days. He spoke of no uneasiness from the operation after the second day. On the 12th of December he was walking about without his truss, and with no apparent tendency to a recurrence of the hernial protrusion. On the following day, being desirous to join his vessel, which was about to sail for South America, he requested his discharge, promising to write to me, and report the further progress of his case, should the swelling reappear—and, if possible, to report in person, at the close of his voyage. But, as yet, I have not heard of him.

The operation in this instance, had evidently a beneficial effect, and I am not certain that it may not have effected a permanent cure. I am not

disposed to believe that any portion of the injected fluid entered the hernial sac; but by exciting inflammation around this within the column of the external ring, and the subsequent condensation of tissues, which is so apt to follow acute inflammation, we can readily imagine that this procedure may, now and then, effect an object we have hitherto sought in vain to effect by other and severer measures.—*N. Y. Medical Times.*

On the External Use of the Nitrate of Lead. By DR. OGIER WARD, Kensington.

Dr. Ogier Ward was induced to make use of this preparation in cases accompanied by fetid discharges, observing its advantages as a disinfectant under other circumstances.

The first case in which he used it with this intention was that of a lady, whose lochia were so offensive as to scent the whole house, and nauseate even the nurse. It was used as an injection; and the third application effected a complete removal of the fœtor.

He has also used it with success as a lotion for sore legs when in a sloughy and indolent condition, and finds that it soon restores them to a healthy state, inducing a proper secretion of pus, with firm granulations on the surface of the ulcer.

He has not used the nitrate in acute gonorrhœa, but states that it acts admirably as an astringent in gleet discharges, as well as in those of cancer uteri, whether sanious or purulent. In short, as a lotion it is as extensively useful as the diacetate of lead, while it is superior to that preparation by its disinfecting property.

In chronic cutaneous diseases he has seen the most remarkable instance of its efficacy in a case of eruption, of a kind of rupia or impetigo of five years' standing. The complaint broke out on the vertex of a woman, æt. 50, and, leaving its original site, it has gradually crept down over the forehead, nose, and cheeks, to the level of the mouth. The primary form of the eruption consisted of inflamed flattened pustules, slightly elevated above the surrounding skin, which, discharging their contents, formed thick, rough, yellowish crusts, or scabs, fissured in all directions, like those of *crusta lactea*, which firmly adhering and growing from their base, like rupia, for a longer or shorter time, fell off at last, leaving cicatrices of various shapes, exactly like the seams and pits of small-pox. The patient came under Dr. Ward's care some months ago, when he tried many remedies, both internal and external, in vain, quinine being the only one that produced any good effect, and this not permanently. As the skin around the sores and where they had healed seemed in a state of hypertrophy, the papillæ projecting in many places as in elephantiasis, it occurred to him that the best way to check the progress of the disease would be to apply some penetrating astringent to the surface; and with this view he ordered a lotion of the nitrate of lead, with quinine internally. In the course of a few days the eruption ceased to make any progress, the crust began to fall off, and the skin to lose its redness and swelling; and in a fortnight every sore was healed, though the face and forehead remained still seamed with scars. There has been no fresh breaking out for

some weeks, though, as the nose is still red and swelled, he has ordered the continuance of the lotion.

The formula he generally uses is the following:

℞ Plumbi Carb., scr. j;
Acid. Nitr. dil., q. s. ad solvendum;
Aquæ dist., ℞j.

M.

Fiat Loticus bis terve in die assidue utenda.—*Prov. Med. and Surg. Jour.*

Ague Treated by a Terebinthinate Liniment along the Spine.—M. Aran mentions, in the *Bulletin de Therapeutique*, that he has succeeded in staying ague fits by the use of the following liniment: Essential oil of turpentine, three ounces and a half; chloroform, about one drachm. The patient was a young man, with whom quinine had failed, and the above liniment was used about two hours before the fit. The latter appeared at the usual hour, but was somewhat shorter than the preceding; the second was kept off for four hours; the third failed to appear altogether, and the patient was soon quite well, experiencing only for a few days a certain amount of discomfort at the accustomed hour of the fits. The liniment had several years ago been introduced by M. Belloncentre, laudanum being, however, used instead of the chloroform employed by M. Aran.—*London Lancet.*

Discoloration by topical use of Nitrate of Silver.—At a meeting of the New Hampshire State Medical Society, held in June last, a clergyman was introduced to the society, whose skin had been deeply stained by the topical application of nitrate of silver. The change of color did not appear until after the protracted and profuse application of it. The relief experienced from its use was so great that, though cautioned as to its effects, he continued to use it, and said, "if it is necessary for me to use it to enable me to speak, I shall do so, though it makes me as black as a hat." He is otherwise in good health. None of the means used have had any effect in removing the color.—*New York Medical Times.*

EDITORIAL DEPARTMENT.

Death ascribed to the use of Chloroform at the Massachusetts General Hospital.—In the New Hampshire Journal of Medicine, we find a paper copied from the Boston Traveler, read by Dr. J. C. Warren, before the medical class, in which death in one case, and serious symptoms in another case,

are attributed to the use of chloroform administered at the Mass. Gen. Hospital in connection with surgical operations.

It may, perhaps, be known to our readers, that Dr. Warren, early in the early history of chloroform, took ground against that anæsthetic agent, and recommended in its stead chloric ether. He has contended for the danger of the former, and the freedom from risk attending the employment of the latter. Under these circumstances, it is singular that chloroform should have been administered, *by mistake*, on the day on which the accidents just referred to occurred. "Chloroform," the Dr. states, "the popular favorite, is never used in this institution (Mass. Gen. Hospital) except as an external application; but on this occasion it was introduced from the fact that it had been poured into a bottle labeled with the title, 'concentrated chloric ether.' This bottle, then, marked as concentrated chloric ether, was placed on the table, and employed for these operations without suspicion on our part that it was not the article designated by the label on the surface. The error escaped the observation of those who administered it from the fact that there is a resemblance in the sensible qualities of the two articles."

We do not find in the Med. Journal referred to the evidence that the article was not chloric ether. This, we think, it would have been well to have introduced into the paper.

We give the account of the operations, etc., in full, and in the language of the operator:

I shall now proceed to give an account of three cases in which chloroform was accidentally administered in this hospital on last Saturday. In my account, I shall endeavor to avoid the use of names, as more delicate and proper; but I would say, that all who aided me on this occasion performed their duties with activity and with credit.

Three cases presented themselves for operation. The first was on a contracted hand. The patient was etherized with the supposed chloric ether. In two or three minutes, anæsthesia being produced, the operation was performed under the continued administration of the same article during from five to ten minutes. The patient escaped without any other inconvenience than a slight soreness of the throat—an effect of the inhalation of chloroform which I have myself experienced.

The second case was of a tumor on the right side of the face in the region of the parotid gland, supposed to lie in the substance of this gland. The attendants being arranged so as to give every assistance to the patient and the operator, the anæsthetic liquid was applied as usual with a sponge, and with the freedom employed in the use of ether, but not proper where chloroform is known to be used. As soon as the application was made, the patient began to struggle and throw his limbs about in so violent a manner that we were compelled for the moment to resign him into the hands of the assistants. But being soon exhausted by the excessive motion, he necessarily

inhaled more freely, thus filling his lungs with the vapor of chloroform, and in three or four minutes rendering him insensible. The operation was begun, the parotid gland laid bare, the tumor found to lie behind it, the parotid gland itself incised, and a round regular tumor, enclosed in a fibrous sac, was brought into view. At this moment those appointed to watch the patient gave a signal that the pulse was failing and respiration scarcely perceptible. Immediately cold water was dashed on his face, and this not reviving him, motions of the chest (in imitation of respiration,) by moving the ribs up and down, blowing of air into his face, and afterwards into one nostril, stopping the other, (pressing back the larynx, so as to prevent the air from going into the stomach,) frictions of the limbs, clearing the mouth of saliva and froth, and removing the mucus from the throat by the finger were resorted to. At this period, I called for ammonia. During the past year I have been in the habit when a patient was to be etherized, to direct a bottle of ammoniated alcohol to be placed on the table. This substance is a powerful stimulant, and has been employed from an ancient period to revive persons affected with many of the forms of suffocation, or asphyxia, and also for fainting. Its object is not to oxygenate the blood, which it cannot do, but to give a spur to the nervous system, and put in motion the dormant vital energy, for which purpose it is superior to any other stimulant. This is not the strongest preparation, but there was brought by mistake, and without my being aware of the fact, a bottle of aqua ammoniæ, which is three times stronger than the above named. It has the same color, and also the same odor, though the latter in a greater degree, with the ammoniated alcohol. The difference, however, would not be detected in a case of urgency, where a few seconds lost might be fatal to the patient. This article then was applied on a sponge to the nostrils, and not producing any effect, a small portion was insinuated into the mouth. At this moment some faint appearance of respiration was exhibited, and by the continued efforts of artificial respiration and frictions the pulse returned, the patient began to breathe freely, and in from five to ten minutes more he seemed quite out of danger. The operation was then concluded, and the patient carried to his bed.

After lying a few hours he recovered his usual state, at least so far as to speak and drink without difficulty; he was, however, much troubled with a secretion of mucus from the lungs, and a cough necessary to extricate it. He had also soreness of the throat in swallowing. The latter symptom has at this time disappeared, but the cough and expectoration continue. The patient wishes to leave the hospital this afternoon, but I shall previously take occasion to call him into your presence.

The third case is the most important. It was that of a young man, about twenty years old, a native of Ireland, who had his arm entangled in the machinery of a bark mill about five days before. The muscles and other organs were torn from the fore part of the arm, and some loss of blood took place. On his entrance to the hospital the hand was found cold and without sensation, showing that the nerves had been destroyed, and that his arm could not be restored. Amputation was proposed to him, but he rejected it, and notwithstanding the danger of mortification and lockjaw, and the ultimate uselessness of the limb were pointed out to him, he insisted that he would die with his arm on.

On Saturday, partial mortification having taken place in the meantime,

rendering the arm excessively painful and fetid, and being convinced that he could never recover the use of it, even if he lived, he agreed to have it amputated. The surgeons and assistants took their places around him, as in the last case, while I myself watched his pulse. Etherization was carefully made; immediately on the application of the anæsthetic fluid he became perfectly quiet; the operation proceeded and was accomplished in about two minutes. Just as it was finished, I perceived his pulse was rapidly failing. Word was given to suspend the dressing, and dash water on his face, which was immediately done. Notwithstanding this, the respiration and pulse went on diminishing, and soon ceased. He was, to all appearance, entirely dead. Artificial respiration was directly produced by moving the ribs; the limbs were rubbed, ammonia was momentarily applied to the nostrils and mouth, and when these things failed ammonia was introduced into the mouth as in the other case. Soon after this, to our great joy, a slight inspiration followed, and, the efforts being continued, his respiration improved, though he breathed with difficulty, owing to the quantity of mucus in the lungs. By great efforts on the part of the gentlemen standing around in lifting and turning him on his side, so as to drain out the mucus from his lungs, and by frequently sponging the back part of his mouth, he was from time to time relieved. At last, passing an empty spoon into his mouth, and pouring some brandy and water from another into it, he was made to swallow fully. A stimulating injection into the bowels was also administered.

After aiding in clearing his lungs for some length of time, it was thought he might be removed to his bed; there a little brandy and water was given occasionally, which he swallowed readily. He also spoke and answered all questions proposed to him until the last moment, showing that the organ of voice was not injured. When asked if he suffered, he said yes, and placed his hand on in the region of the heart. Mucus continued to fill his throat. There was no obstruction in the opening of the larynx, for mucus issued from it in a copious stream, showing that his whole lungs were affected. Having remained with him until the pulse had become pretty good, and the respiration apparently better, we adjourned to meet again in an hour and a half. Placing at the same time the house surgeon at his side with instruction to keep his throat clear of mucus, and support him by stimulants, with the strongest injunctions not to leave him till our return.

Shortly before the time fixed for the return of the surgeons, which was half-past three o'clock, the house surgeon perceiving his pulse to suddenly fail, and that his breathing was more hurried, uncovered the stump to see if it was bleeding, and found some effusion of venous blood, probably produced by the liquefaction of blood from the chloroform poison. He then cleared the mouth of mucus, which he hardly completed when the patient breathed his last without any effort or convulsion. Soon after, an opening was made in the trachea and air blown into the lungs for the purpose of inflation, but without effect. A proposal had been made to do this during life, but it was objected to, because air had already been thrown into the lungs through the nostril, because there was no obstruction in the larynx, because blood might escape through the aperture into the trachea and combine with congestion and mucus in the lungs to increase the difficulty.

On the following morning an examination of the body was proposed, but his friends arriving objected, and although we urged the importance of ascertaining the immediate cause of death, they continued to object decidedly.

Remarks. Immediately after the occurrence of alarming symptoms in this case, it was discovered that the substance which had been used was not chloric ether, but chloroform; and not till then did we understand the extraordinary phenomena which presented themselves in this and the preceding case. This patient died with the usual phenomena of chloroform poison.

If we consult the records of fatal cases of chloroform, published by me in 1849, we shall perceive that of fifteen cases there mentioned, the principal part took place in a very sudden manner, some of them occurring a minute or two after the application, and some of them in a period from ten minutes to fifty hours. In the latter cases the lungs were remarkably congested or filled with blood, owing to the poison applied to the air-cells of the lungs, or circulating with the sanguineous fluid, as in asphyxia. From various causes, asphyxia is of frequent occurrence; the phenomena are the same with those presented in these cases, and the remedies are the same—hence the great importance of being well acquainted with the treatment adopted in all such casualties.

The first class seem to have perished almost as if they had been struck by lightning, the powers of the nervous system appearing to be at once annihilated. In the second class the lungs exhibited a most remarkable state of congestion. In the death at ten minutes after the application, the lungs were “a good deal congested, and discharged, when cut, a large quantity of bloody serum.” In the death in three quarters of an hour, no examination was made, but “the respiration was infrequent and sighing,” showing that the function of the lungs was interrupted. In the third case, “the lungs were filled with blood and softened; bloody serum in pleura.” In some of the cases the heart was found disordered, in others the brain, but in the whole number, I believe, without exception, the lungs were charged with blood, or congested—the common, decided effect of chloroform.

The revival from anæsthetic symptoms and prolongation of life for ten minutes, three quarters of an hour and fifty hours, bring these cases into the same category with ours. The vital principle, after appearing to be extinguished, lights up, and gives the hope of recovery, but the blood continuing to accumulate in the lungs from the effect of the poison, and the weakness of the patient, its oxygenation is prevented, and, from want of the animating principle, life is suffocated and extinguished.

Is the fatal termination of the third case to be attributed to any cause other than that which exists in the preceding case? There is no reason to believe this to be the fact. The sinking after revival might lead to a suspicion that some other than the usual cause, congestion of the lungs, existed; and some one has suggested that it might have arisen from ammonia having entered the lungs. How could it be the cause of death in this case? By being introduced into the lungs and irritating and burning these organs? This was impossible in the given circumstances; the patient neither swallowed nor breathed after the ammonia had been employed until the whole had been washed out of the mouth by the abundant mucus. It may have been thought that the ammonia irritated the opening into the larynx and swelled it so that no air could pass through. The air did pass through freely till he died, and so did the mucus, a less volatile fluid than air. Further, when ammonia is introduced into the mouth, its entrance into the larynx is repelled by contraction of the laryngeal muscles, so that it cannot enter. Moreover, it was in excessively small quantity. A saturated solution of nitrate of silver,

it may be remarked, is frequently introduced into the mouth and even larynx for curative purposes. Most of the ammonia immediately ran out, and the rest was thrown off by the mucus of the lungs and throat. Had any disorganizing effect been produced, how could the patient have swallowed repeated draughts of brandy up to the period of his death? Finally, if the ammonia destroyed the last patient, how did the second escape the action of the same cause? He used the same quantity, so far as can be judged, he was able to swallow through the day and ever since, and to take even solid food; true, he was stronger than the other, but this difference of strength would have made no difference in the chemical or even vital action, more favorable to the one than to the other; yet the one is well, the other is dead. But I will insist no further on this point, and perhaps have already said more than was required.

We believe that the temporary resuscitation of one of these individuals, and permanent restoration of the other from apparent death must be considered as a triumph honorable to medical science and to this institution.

Several points relating to the foregoing report will be likely to arrest the reader's attention:

1. The occurrence of the mistake of labelling chloroform, "concentrated chloric ether."

2. The occurrence of serious accidents in *two* of *three* cases in which chloroform was administered, and a fatal result in *one* of the cases. That there is a liability to the occurrence of grave symptoms and death from the administration of chloroform is not to be doubted, but that *two* out of *three* patients should be seriously affected, and *one* die, would certainly not have been anticipated from the result of the employment of this anæsthetic agent, in a very extended series of cases in the hands of a great number of surgeons. The reader may fairly doubt if, assuming that the unfortunate events in these cases were due to the chloroform. The administration was conducted with that regard to safety which is proper to be observed with respect to the use of any anæsthetic agent.

3. The occurrence of the *two* mistakes is extraordinary. It seems that *aqua ammonia* was employed to revive the patients in lieu of *ammoniated alcohol*, and that the former substance was introduced into the mouth! In connection with this second mistake we are much mistaken if the reader is fully satisfied that the unpleasant after-symptoms in one case, and the death in the other case were not attributable to the inhalation of the vapor of the ammonia. We are free to say that the arguments against this supposition advanced by the Reporter are far from being satisfactory to our mind. It is in our view entirely inadequate to say that "most of the ammonia ran out, and the rest was thrown off by the mucus of the lungs and throat!" Let *Dr. W.* try the experiment, in his own person, of introducing a little aqua

ammonia into the mouth, and he will find, we reckon, that some time will elapse ere the mucus of the lungs and throat wash out the irritating, asphyxiating vapor! He says that the glottis could not have been obstructed because the air and mucus passed freely through it till the patient died. The poisonous action of the vapor of ammonia may have been on the lungs rather than the glottis; still, as no examination of the body was made, and *the mode of dying is not given in the report*, it is difficult to form an opinion as to how far the glottis may have been obstructed by oedema occurring shortly before death. In respect to the account of the symptoms preceding death, there is a remarkable deficiency of precision. That a saturated solution of nitrate of silver applied to the larynx does not cause fatal, or unpleasant results, we take to be no ground for arguing that the vapor of ammonia is equally innocuous! This mode of reasoning, it must be admitted, is peculiar. That one person escaped death by the ammonia, is assuredly no argument against a fatal result being due to the ammonia in the other case. Apply the same rule of logic to the chloroform, and it proves incontrovertibly that this agent did not kill the patient in Dr. Warren's case; for chloroform has been given in more than three thousand successive cases without any unpleasant consequences. This argument proves quite too much for Dr. Warren's purpose.

The only point which in the imperfect history of the symptoms is prominent, is the copious secretion of mucus. To quote the language of the report: "Mucus continued to fill his throat. There was no obstruction in the opening of the larynx, *for mucus issued from it in a constant stream.*" (Our italics.) "By great efforts on the part of the gentlemen standing around, in lifting and turning him on his side, *so as to drain out the mucus from the lungs,*"* etc. (Our italics.) Now, in view of the facts with respect to the secretion of mucus which are contained in the above quotations, one of two things is clear: either the patient, prior to the operation, was laboring under a pulmonary affection rendering the administration of any anæsthetic agent improper; or the bronchial mucous membrane was exposed to a cause of irritation far more prompt and potent in its operation, than the vapor of chloroform. We can readily enough conceive that the vapor of ammonia would prove an irritating cause adequate to the production of the phenomena.

4. Our contemporary of the *New Hampshire Med. Journ.*, comments on the fact that this report was communicated for the newspaper press. He

* The reader, we fancy, will be surprised to know that by turning a patient on the side, mucus can be drained away from the lungs!

says: "How does it happen that this account of the case appears in a newspaper, instead of a medical Journal, when the third section of the first article of the second chapter of the national code of ethics, says, "it is derogatory to the dignity of the profession to publish cases and operations in the daily prints, or suffer such publication to be made?" We concur fully in the above criticism, and the course pursued is the more censurable, as it seems to us, in view of peculiar circumstances attending the cases reported. Dr. Warren evidently wishes it to be believed that the death was owing to chloroform exclusively. He is excusable for wishing to arrive at this conclusion, and, if he so persuades himself, to desire that others should concur with him. But the question is an open one. He virtually admits that it is so, when he enters in his report upon the discussion of it. This being the case, Dr. Warren should have addressed his arguments and appeal to the medical profession through some appropriate channel, nor to the public, whom, it is to be presumed, are not generally competent to judge respecting matters so purely medical in their nature. We are entitled to *our* opinion on this point, and we do not hesitate to say that, in our judgment, the course pursued should be looked upon as highly improper.

Again, Dr. Warren has, from the first, repudiated in his surgical practice the use of chloroform, but is identified with the suggestion of chloric ether as the only proper and safe anæsthetic agent. In this position Dr. Warren is somewhat singular. At all events, very many, if not most distinguished surgeons, as well as accoucheurs, and general practitioners, are accustomed to use chloroform, and consider it not only more eligible, but a safe article, if properly managed. Now, what will be likely to be the effect of Dr. Warren's report on the public mind in the part of the country where his reputation gives to his opinions more or less influence? It is plainly calculated to create a distrust of chloroform, a fear of it, and a disposition to attach blame to those who employ it; and this, not as the consequence of a candid examination of facts, but as the result of Dr. Warren's personal influence. Regarded in this point of view, the publication of Dr. Warren's report in the newspapers, prior to any discussion, or action thereon by the medical profession, seems to us unworthy a high-minded practitioner.

As respects the subject of chloroform, we have personally no more interest than most practitioners of medicine. In its relations to medical science and to humanity, all have an interest in this subject; and the report which we have briefly criticised appears to us to be an attempt to create a popular prejudice against the use of chloroform unwarranted by the facts as presented by the reporter himself, and involving a procedure illegitimate and censurable.

Abortive Treatment of Continued Fever by the Sulphate of Quinia.—The attention of medical practitioners has lately been called, by communications emanating from regions widely separated from each other, to the efficacy of the sulphate of quinia, administered in large doses, with a view to arresting Continued Fever. The subject is one which appeals so directly and exclusively to experimental research as the mode of developing facts by which its merits are to be determined, that to enter upon an extended discussion of it, irrespective of the data to be furnished by observation, would be, to say the least, a waste of time. Our intention is simply to bring the subject before the minds of our readers, and to suggest some points to be considered in subjecting the plan proposed to the test of experience.

The first of the points just alluded to is, the importance of discriminating *Remitting* from *Continued* fever in making experimental observations of the abortive power of the remedy in question. The bearing of this point is obvious. Quinia does exert, it is now admitted by all, more or less potency in arresting remitting fever, and the question to be settled by the results of observation is, whether the remedy is equally, or measurably applicable to continued fever. That the two forms of fever, common continued or typhoid, and remitting, are often confounded in districts where both prevail, is undoubtedly true. Remitting fever may exhibit in its career many of the features which are more distinctive of typhoid, and hence the discrimination is not always to be made without careful examination, and a thorough knowledge of the various circumstances which are peculiar to the natural history of each. Moreover, the terms *continued* and *typhoid* are frequently employed very loosely, so as to embrace cases of periodical fever in which periodicity is not a prominent feature, and in which the typhoid condition is present.

The importance of this point has been lately impressed upon us by reading an elaborate and able article on the use of quinine in continued fever from the pen of our distinguished and much esteemed friend, Dr. Fenner, of New Orleans. The article referred to is contained in the last No. of the New Orleans Med. and Surg. Journal.

Dr. Fenner, in that article, quotes from a paper of Dr. Thomas Fearn, communicated for the second volume of the *Southern Medical Reports*, in which the efficacy of large doses of quinia in arresting continued fever is claimed to have been exemplified in a striking manner. To borrow Dr. Fenner's language in introducing the quotation: "In this paper Dr. Fearn gives a graphic account of a terrible *continued fever* that prevailed in the year 1831 among a few families residing in the vicinity of Huntsville, (Alabama,) and corresponding very accurately with the descriptions given of the *typhoid*

fever at this time prevailing through the Southern States. That fever proved intractable to all the various plans of treatment then in vogue; such as blood-letting, emetics, cathartics, mercurial ptyalism, stimulants, anodynes and the steam, a Thomsonian course. They *all failed*, and death was slowly but surely sweeping off every one attacked. In this painful and trying emergency, which appeared to defy all the known resources of our art, Dr. Fearn, with the boldness which ever characterises true genius, resolved to try a desperate experiment with the *great febrifuge the sulphate of quinine*. It was determined, in consultation with two other able and experienced practitioners, that in the case of a lady who had been sick three days with the prevailing fever, *when the most perceptible remission should occur*,* the sulphate of quinine should be given in doses of 20 grains, repeated every hour *pro re nata*. *As the remission was expected to occur about midnight*,† Dr. F. remained to superintend the prescription."

Dr. Fenner then proceeds to quote from Dr. Fearn's paper, as follows:—
 "When the fever was at its highest *there was slight delirium, great distress about the precordia, and tenderness on pressure, with rapid small pulse, and hot skin*. *When it began to abate*,‡ the dose agreed upon was administered. No very sensible effect was observed, and at the expiration of an hour the dose was repeated. Before the expiration of the second hour the pulse was reduced in frequency, and was softer than it had been since she was taken ill. The skin for the first time was moist, *and she was more composed*; nevertheless the third dose was given at the end of the second hour. Shortly after she became tranquil, and fell into a sweet sleep — perspired freely — *the pulse became reduced from 120 to 80 in the minute, and from that time she was convalescent*. Her brother was treated in the same manner, a few days after, and with the same success."

Dr. Fenner adds: "These are the only cases specially reported by Dr. Fearn," etc.

Now, in view of the data afforded by the foregoing very brief account, is the reader warranted in drawing the conclusion that the cases so successfully treated were cases of *typhoid fever*? In answering this question we are compelled to differ from the opinion entertained by our friend, Dr. Fenner. In the first place, there is entire absence of any *positive* evidences of the typhoid character of the disease. Dr. Fearn does not give us even one of the numerous distinctive traits which belong to the natural history of *Continued*, as distinguished from *Remitting fever*. That he is sincere in the

* We have italicized this passage. † Our italics. ‡ All the italics in this extract are ours.

opinion that he was dealing with the former of these two forms of fever we do not doubt, but as respects the grounds on which that opinion is based, we are left without information. Not only is there absence of positive proof, but the facts which are stated in the extract given seem to us to render the conclusion almost certain that the disease was *Remitting* fever. The passages which we have italicized are those which appear to us to sustain this conclusion. On referring to these passages the reader will find evidence of the following features which are highly distinctive of *Remitting* as contrasted with *Typhoid* fever: 1. An abrupt attack. The patient, already presenting great febrile movement, had been ill but three days. 2. Marked remissions, the characteristic giving the name to *Remitting* fever, and which very rarely occurs in the course of *Typhoid*. 3. An intensity of febrile movement seldom observed so early in the career of *Typhoid*, but often in *remitting* fever. 4. Delirium as early as the third day, which is common in *Remitting*, and comparatively rare in *Typhoid*. 5. Distress and tenderness in the precordia denoting gastric irritability — an element of *Remitting* fever not often observed in *Typhoid*. Couple the presence of these striking diagnostic criteria of *Remitting* fever with the absence of any of the criteria of *Typhoid*, and the evidence contained in the report is all in favor of the former.

If we are correct in this criticism, we have here an illustration of the point under remark, viz., the importance of being careful to discriminate between *Remitting* and *Continued* fever, in bringing the abortive efficacy of quinia to the experimental test.

In the article by Dr. Fenner, from which the foregoing extract from Dr. Fearn's report is taken, the writer comments very justly on the few observations made by us on the abortive treatment of *Continued* Fever by Quinia, contained in our *Clinical Reports*. We freely admit that the number of cases is too few, and the quantity of the remedy too small, to authorize any deductions as to the merits of the plan of treatment.

Another point to be borne in mind in conducting experimental observations, is the liability to deception by the apparent success of the treatment in cases in which the disease ceases from its own limitations. All who have studied the disease at the bedside are aware that cases not infrequently present themselves in which *Continued* fever appears to be formed, but ceases after a duration of two, three, or four days. Such cases are classed under the head of *Febriacula*. It is by no means always easy to foresee that cases will run this brief, imperfect career; in other words, to discriminate between *febriacula*, and true *Continued* fever, prior to the cessation of the fever. There are several circumstances which have an important bearing on this

discrimination, but they are not altogether reliable, and, hence, the most experienced and skillful often mistake a case of febricula for continued fever. Practitioners who do not appreciate that the fever in these cases is either a distinct type, or, possibly, instances in which an abortion takes place irrespective of art, not infrequently flatter themselves that the particular measures which they may have employed at the time the febrile movement ceases proved successful in cutting short the disease. Febricula is sometimes observed to prevail at particular seasons, or places, as the common form of fever. This is stated by Prof. Bennett to have been noticed at Edinburgh in the early part of the winter of 1841-2. It is obvious that the cessation of fever, after the administration of quinia, or any other remedy, in isolated, or a limited series of cases, does not establish the efficacy of the abortive treatment. Either the cases of apparent arrest must be numerous enough to outweigh the probabilities of their having been febricula; or, as suggested by Prof. Bennett, "it ought to be a *sine qua non* in all such trials not to commence the treatment until the seventh day." With respect to this suggestion by Prof. B., it may be said with propriety, that it is not improbable an earlier resort to the abortive treatment might be successful, in cases in which it proves inefficacious so late as the seventh day. A sufficiently large proportion of cases in which the treatment was tried earlier, and especially if the trials were repeated at different periods and places, would, therefore, be a fairer method, and equally satisfactory.

The success of quinia in arresting Continued fever has lately been advocated by Dr. Dundas, of Liverpool, Eng. Dr. Dundas' method is to give the remedy in doses of ten grains, repeated every two hours, until five or six doses are taken. Prescribed in this way he declares that the disease may be arrested as certainly as if it were an intermittent, and this statement he affirms to be based on abundant observations by himself and his friends at Liverpool. We have not seen Dr. Dundas' work, and we are not informed concerning the amount, or kind of data which he brings forward to sustain the practice recommended by him. It will not tend to diminish the scrutiny with which the reader should examine whatever facts Dr. D. may have contributed, to know that this author contends for the identity of Continued and periodical fevers.

The test, however, of the plan proposed by Dr. Dundas, (which is essentially the same as that advocated by Dr. Fenner,) is the results of clinical experience. Prof. Bennet was led by the views of Dr. D. to make trial of the practice in a series of cases at the Royal Infirmary of Edinburgh, during the winter session of 1851-2, but with a negative result, so far as concerns

the interruption of the febrile career, in every case. The cases in which the remedy was tried by Prof. B. were, (so far as our recollection serves,) about fifteen. The cases are reported in full in the *Monthly Jour. of Med. Science*, Nos. for April and May, 1852. We are not able, at the place where we are writing, to refer to these numbers, or we should give the facts with more precision, as well as more in detail.

Cases of Continued fever are numerous enough in certain districts to determine, without much delay, whether the disease be unaffected by quinia, or not; and it is to be hoped observations will be multiplied sufficiently to put the question at rest. The bare possibility that the advocates of this practice may have grounds for their enthusiasm, should induce observers to make trial of it, especially since no evil results need be anticipated provided the remedy be withheld so soon as any unpleasant symptoms follow a repetition of the doses.

We may remark, in conclusion, that the plan of endeavoring to arrest Continued fever by quinia, is not a novelty. It has been urged, years ago, by practitioners in this country and abroad. Without taking pains, at this time, to make bibliographical researches on the subject, we find in our *scrap book* the following notice of the conclusions of a French observer, M. Boucher, taken from Ranking's Abstract, 1846:

"M. Boucher has undertaken a series of investigations on the physiological and therapeutical action of the sulphate of quinine in large doses in typhoid fever, a subject which has recently attracted much attention among French practitioners. The following are the conclusions which he thinks himself justified in forming, as the result of his own personal observations:

' 1. The non-acid sulphate of quinine, in the dose of from two to four *grammes* (40 to 80 grains) in a mixture of 125 *grammes*, administered in spoonfulls by the mouth, every two hours, or more, does not produce any serious consequences.

' 2. It is generally taken with repugnance; often immediately after having been admitted into the stomach producing a temporary nausea, and sometimes vomiting.

' 3. The mucous membrane of the digestive passages does not experience from it any injurious influence; there is only some slight sensation of heat in the course of the oesophagus to the cardia.

' 4. The eruption of the lenticular spots of the skin and sudamina is not modified, and it appears to be the same also with the intestinal eruption.

' 5. Its administration is often followed by a remarkable amendment, which is sometimes only transitory.

'6. The apparent convalescence is generally rapid, but it is not the same with the confirmed convalescence.

'7. This apparent convalescence is owing to the modification of the general condition, the intestines not partaking in this modification.

'8. The nervous phenomena and the slowness of the circulation which are caused by the quinine soon cease when the administration of the medicine is suspended.

'9. It diminishes the headache and often causes it to disappear; the pain is then replaced by heaviness of the head.

'10. It often hastens the return of natural sleep.

'11. Finally, it does not appear that the sulphate of quinine should constitute a special method of treatment, but it may prove serviceable, combined with other means.'**

Arrest of the disease does not appear among the effects of the remedy, which was administered in large doses, and the results apparently studied with care in a large collection of observations.

Case of Aneurism of the Aorta, with the Physical Signs. Reported in a letter from AUSTIN W. NICHOLS, Med. Student.

Sept. 4th, 1852. Before the surgical clinic of a medical college in this city, there appeared a man, aged about 45, his neck enormously distended, and his breathing labored. He stated, that for several weeks he had been engaged in digging cellars, his feet being constantly in the wet. This swelling came on gradually, without any previous sickness. The swelling pitted on pressure and was due to serous effusion into the areolar tissue of the neck. He had no fever, there was no redness; and the only symptom was the quickened and forced respiration. The distinguished surgeon pronounced it, a case of cedematous erysipelas. He directed to be applied locally, a saturated solution of nitrate of silver; and internally, he gave diuretics as digitalis, squill, and calomel; and as a drink, he prescribed juniper-berry tea. On the 11th, he appeared at the same clinic much better, and was directed to continue the internal medicines. I did not see the patient again till my visit at the Pennsylvania Hospital, on the 10th of November. I was then surprised to see the man with his neck more distended and his breathing quicker and

* Credited by Ranking to *Gaz. Med. and Prov. Med. Journ.*, Jan. 1846.

more laborious than ever. Dr. Wood, the attending physician, uncovered the chest and arms of the patient; the superficial veins were much enlarged and could be seen distinctly; but the veins of the abdomen and lower extremities were healthy and not at all enlarged. There was no ascites nor anasarca. On percussion, flatness was found between the second and fourth ribs to the right and left and over the sternum. The left side was flat as far down as the fifth rib; and the right side was flat as far as the nipple. On auscultation, the sounds of the bronchi and lungs were found to be healthy or natural, though somewhat obscured by the following murmurs. On listening to the cardiac sounds, the first was replaced by a long continuous murmur occupying the time of the first natural sound and part of the time allotted to the second natural sound; the second sound was replaced by another continuous murmur occupying the time of the second natural sound and part of the time of the interval between the sounds. The first murmur was heard most distinctly over the mitral valves; the second, over the aortic or semilunar valves. These sounds were due to an imperfect closure of the valves on the left side of the heart. The sounds on the right side, though obscured, were not very unnatural. Particular attention, however, was not given to the state of this side of the organ. No aneurismal thrill was present in the course of the aorta or its large branches. I may here add, that when the patient stooped forward, his face became dark, his head swelled, and giddiness came on. On feeling the pulse of the right arm, it was found to be much feebler than that of the left arm. The diagnosis was easily arrived at; there was disease of the valves of the left side of the heart, and there was an aneurism of the aorta or of its large branches. This aneurismal tumor pressed upon the superior cava and impeded the return of the blood from the upper extremities. The treatment was but palliative; diuretics, as digitalis, squill, bitartrate of potassa, being given. On the 20th of November, I saw the pathological specimens from the above case; death had taken place slowly, not from rupture of the aneurism. The mitral and semilunar valves of the left side of the heart, were much thickened by a deposit semicartilaginous: the ventricle was dilated and thickened to a very great extent. The greater part of the ascending aorta and the entire arch, was an aneurismal sac; this was a mere dilatation of all the aortic coats, and not a dilatation of the outer one along with rupture of the inner coats; the branches of the aorta were given off from this enlarged aorta as usual. The sac would hold the two fists with ease; its inner surface was covered with an atheromatous deposition; on its external surface, the face toward the sternum had thrown out coagulable lymph and caused an adhesion to the sternal bone. The valves

on the right side of the heart, were slightly thickened; the cavities were of the natural size. But the superior vena cava, for near its whole length, was greatly thickened; its walls had been pressed upon and had adhered; the channel of the vein was reduced to about one line in diameter. Thus was the diagnosis of this singular case verified.

Jefferson Med. College, Nov. 22, 1852.

The American Journal of Science and Arts. Conducted by Professors B. SILLIMAN, B. SILLIMAN, Jr., and JAMES D. DANA. Aided in the Departments of Chemistry and Physics, by Dr. WOLCOTT GIBBS. Second Series, No. 42. Nov., 1852. With four plates. New Haven, Conn: Published by the Editors.

It were any thing but complimentary to the intelligence of our readers to commend to their knowledge, or regard, the above periodical. It has for a long period been the chief, and indeed almost the only serial, in this country, devoted to subjects of general science; and in its scientific character we suppose it to be well understood among those most competent to judge, that it compares most favorably with similar publications in any, and all countries. It is conducted by gentlemen holding rank among the foremost of those distinguished for scientific attainments; its collaborators are their compeers in this country and abroad, and no pains are spared to render the work worthy of science and our country. Among the scientific subjects treated of in its pages, are those in which most members of the medical profession feel an interest, and respecting which information, to say the least, becomes an accomplishment very appropriate to the physician. Hence it is that a considerable share of its support, both in the way of patronage and contributions, comes from medical men. We regret to learn that it does not enjoy a circulation due to its merits, and to its character as the representative of American scientific literature. The expenses of its publication are great, and are scarcely more than repaid by the annual avails of the subscription returns. This should not be; and we take the liberty of appealing to those of our readers who do not restrict their studies to subjects exclusively medical, to subscribe for the work, if their names are not already on the subscription list.

It is a bi-monthly publication, each No. containing about 150 pages.

The subscription price is \$5 *per annum*.

Orders, with remittances, may be addressed to Silliman & Dana, New Haven, Conn.; or to Phinney & Co., local agents at Buffalo.

Materia Medica, or Pharmacology and Therapeutics. By WILLIAM
TULLY, M. D. Vol. I, No. 1. Nov., 1852.

This is the first of a series of numbers (64 pages each No.) proposed to be issued monthly by Drs. Church and Seegar, Springfield, Mass., for an indefinite period, till the manuscript lectures of Dr. Tully are exhausted. A circular, setting forth the plan of the enterprise, and its importance, was published in a former number of this Journal.

By the warm recommendations which the announcement of the undertaking called forth from distinguished members of the profession, our anticipations had been highly raised, and we proceeded to examine the first No. of the series with eagerness. The paragraph, however, which first met our eye on opening the work, dissipated all our expectations. The paragraph alluded to was as follows: "As appears to me, there is sufficient evidence that those remedial agents which are taken into the alimentary canal, make all their medicinal impression upon the nerves of its inner parietes, whence it is propagated by some one of the modes of sympathy already explained, to the part or parts in which we perceive the first manifestations of their operations; — that no part or portion of one medicine in fifty, or a hundred, ever leaves the alimentary canal, except to pass off with the fæces; and that, when some part or portion of a medicine happens to be capable of being taken into the mass of the circulating fluids, it produces all its medicinal effects before it is so taken up; and no medicinal effect (at least as a very general rule) after it leaves the alimentary canal."

On looking over the No. we find that a greater part of it is taken up with a discussion of the proposition contained in the quotation just made.

The author, thus, adopts as the basis of the materia medica, the obsolete dogma of *exclusive solidism*. With this vicious principle pervading it, all the learning or thought which the work may contain, cannot save it from becoming a monument of the past errors of science, instead of an exponent of our present knowledge. Such a work, at this day, will only prove a failure, bringing pecuniary loss and mortification to those engaged in the undertaking. The pernicious doctrine upon which it rests is too thoroughly exploded, for its revival to be attended by any danger of mischief. It is greatly to be regretted that the friends of the venerable Dr. Tully have encouraged this project; and the most friendly wish, with regard to it, which can be expressed is, that the inadequate demand for the first number may lead to a speedy abandonment of the publication.

New Cupping Instrument and Breast Pump.

DEAR SIR:—In making some experiments with a broken syringe, I made the following discovery, which may be of service to the profession, and to the community at large, if it should come to their knowledge. It is this:

A glass tube, one inch in diameter, and six inches in length, with an open mouth of a proper shape, and a piston fitted to the caliber of the tube, thus constituting an air pump, on placing the mouth over the nipple, makes the very best instrument for elongating the nipple, or for emptying the breast of its contents that I have ever seen. It works like a charm.

Also tubes of various sizes, with proper mouths, make the best instruments for cupping of which I have any knowledge. The operation of cupping with this instrument is greatly facilitated.

You may make such use of these facts as to you may seem proper.

I have never ordered any of these instruments to be made at the glass manufactories, but have some tubes with which I have made tests that afford much satisfaction to me.

Yours, &c.,

ADDISON, N. Y., Dec. 7, 1852.

S. C. ROGERS, M. D.

Aromatic Schiedam Schnapps.—We have noticed in some of our contemporaries warm commendations of an article bearing the above prepossessing title, which, as we understand it, is but another name for *pure Holland gin*. The commendations to which we refer, appear to be redolent of said *schnapps*, and we doubt not that, after enjoying the same advantage for estimating the merits of this new medicinal article, we shall be equally *ardent* in its praise. We shall defer farther remarks till we have an opportunity of becoming more fully imbued with the *spirit* of the subject.

New Work by Dr. John Bell.—We perceive, by our exchanges, that Dr. John Bell, of Philadelphia, is engaged in preparing a work on Mineral Waters, designed as a companion to his treatise on Baths. Dr. B. invites communications from those residing at, or near any of the celebrated watering places of the United States. The N. Y. Med. Gazette states, that "Details are desired touching their accessibility, facilities, and accommodations, which those concerned will find their interest to furnish, obliging themselves rather than the author."

Biography of the late Dr. Drake.—Prof. Gross, of the University of Louisville, has consented to a request made by his colleagues, to prepare a biography of the late Dr. Drake. It will probably be publicly pronounced on some occasion during the winter, and will undoubtedly be published. The duty of preparing the biography of his deceased friend appropriately devolves on Prof. G. They were associated at the commencement of the career of the latter as a teacher, in the Medical College of Ohio, and as colleagues for many years in the University of Louisville, the most intimate relations subsisted between them. Prof. G. will do justice to the memory of the distinguished Drake.

We have lately purchased a complete set of "H. S. Day's Fracture Splints," made at Bennington, Vermont. We have also applied some of the splints to use: and we deem it only just to say that they are in most respects equal to any others in use, and in some respects superior. They were evidently made by a practical surgeon, possessing two requisites in good splints which are not always found, namely, *fitness* and *firmness*. They are admirably suited to the purposes for which they are severally designed, and they are substantial. The only objection which we can make to them is, that there are in the "complete set," a redundancy of double inclined planes, and not a fair compliment of straight, extending splints for the thigh. But to some surgeons, who are more in the practice of using double planes than ourselves, this might not be regarded as an objection.

They are for sale by A. I. Mathews, Druggist, at Buffalo. F. H. H.

Fusel Oil.—Our readers will recollect a communication from Dr. C. T. Jackson, in which the occasional fatal effects of chloroform were attributed to Fusel oil becoming commingled with it in its preparation. By the N. Y. Med. Times we perceive that at the monthly meeting of the American Academy of Arts and Sciences, held at Boston, September 14, experiments with Dr. Jackson's Fusel oil compounds on animals were reported, the conclusions from which are: 1. That the vapor of the Fusel oil compound alone is not poisonous; 2. That the view that this compound is the cause of the occasional fatal effects of chloroform is not sustained.

Dr. Smelfungus. — We tender our thanks to the editor of the Philadelphia Medical Journal, for his complimentary notice of the Buffalo Medical Journal, and the flattering compliment which he pays to our editorial labors. We cannot, however, assume the authorship of the Smelfungus papers. There are by a talented correspondent from whom we shall always be happy to receive communications, and for whom we predict a high reputation as a medical writer.

The annual meeting of the Erie County Medical Society, will be held on the second Tuesday in January, at the American Hall, Buffalo, at eleven o'clock, A. M.

The members will recollect that the meeting is expected to possess unusual interest from the arrangements made by a committee to provide a dinner at the close of the session.

The regular term of Lectures at the Buffalo Medical College, will commence on Wednesday, the 5th of January. The Introductory Lecture will be delivered by Prof. Moore, at 2, P. M.

Prof. Charles A. Lee. — Prof. Lee has been designated, as the substitute for Dr. Smith, to pronounce the annual oration before the Alumni of Williams' College, Massachusetts, at the next annual commencement.

Prof. Samuel G. Armour. — Prof. Armour, late of the University of Iowa, and editor of the Western Medico-Chirurgical Journal, has removed to Cleveland, Ohio.

Errata. — December No. page 452, 20th line, and several times in that article, for *ecrema* read *eczema*. Page 453, 15th line, for *grains*, read *grammes*. Page 453, 15th line, for *conscientiously* read *continuously*.

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NO. 9.

ORIGINAL COMMUNICATIONS.

ART. I. — *On Provisional Callus in Fractured Bones.* By FRANK H. HAMILTON, A. M., M. D., Professor of Surgery in the University of Buffalo, and one of the Surgeons to the Buffalo Hospital.

DR. FLINT:

You are aware that nearly a year since I called your attention to my views upon the subject of the union of broken bones, and that I then regarded these views as original and as entertained only by myself. In May last I prepared the following paper as a contribution to the semi-annual meeting of the State Medical Society, but learning soon after that I had been anticipated by Mr. Paget, of London, I withheld the paper from the Society, giving to its publication an indefinite, if not final postponement.

I have now, however, determined to publish it, and in the same form in which it was originally written, before I had seen or heard of the views of Mr. Paget.

My reasons for this determination you will permit me to state:

James Paget is the Professor of Anatomy and Surgery to the Royal College of Surgeons of England, and the exposition of his views of the union of broken bones constituted a portion of a series of lectures on the "Processes of Repair and Reproduction after Injuries," delivered by him before the College three years since. These lectures were published in the Medical Gazette for 1849, and subsequently in "Ranking's Abstract" for 1850.

It would seem that the position and rank of the author ought to have insured to his new doctrines general attention among surgeons and pathologists on both sides of the Atlantic. Yet I doubt whether they have obtained more than a casual and scarcely a respectful attention either here or elsewhere. I have seen no notice of them in American Journals, and among the many eminent surgeons with whom I have conversed, both teachers and practitioners, only one or two retained a vague impression of their existence. By a letter also from a friend in Paris, I learn that upon careful inquiry the writer could not ascertain that any one in that city had seen or heard of the doctrines announced by Paget; and the only evidence I have that these doctrines have attracted any more attention at home, in great Britain, than they have abroad, is furnished in the notice given of them by Pirriè, the Professor of Surgery in the University of Aberdeen, in his work on surgery published during the present year. After having explained the mode of union of bones, as taught by Dupuytren, he remarks:

“Such are the views of Dupuytren on this interesting subject; and until lately they were generally received as the correct explanation of the successive changes that take place, both in man, and in the lower animals, from the occurrence of fracture until the injury is completely repaired.

“Mr. Paget, in his ‘Lectures on Repair and Reproduction after Injuries,’ has brought forward different views from those which formerly prevailed regarding the repair of a fractured human bone, and has supported his opinions by most conclusive evidence. His views on this subject are in accordance with those of Mr. Stanley.”—Pirriè’s Surgery, p. 127.

[I think Mr. Pirriè is not entirely right in saying that Mr. Paget’s views “are in accordance with those of Mr. Stanley.” Mr. Stanley says, “in the human subject, no such cartilaginous and osseous deposit uniformly takes place around the fractured bone; here, therefore, it is not an essential part of the reparative process.” This passage implies that he regards provisional callus as a general but “not uniform” occurrence; useful, but not “essential.” These are by no means in accordance with Mr. Paget’s views; nor are they the views which I propose to advocate. If such were actually Mr. Paget’s views, then it will be seen that we are not agreed, and I should still claim originality for my doctrine.]

I thought it proper, therefore, that I should attempt to direct the attention of my professional brethren to Mr. Paget, by a reproduction of his and by a public statement of my own conclusions — conclusions nearly or quite identical, to which, without concert, we had almost simultaneously arrived, and yet by somewhat different roads.

Mr. Paget's first impressions of the fallacies of the doctrines of Dupuytren, and his subsequent full convictions were obtained solely from pathological specimens — from "the large collection of fractures in the museum of the college." While my first impressions were received from examinations of the progress of union in the living subject, and by similar examinations often repeated, have my early impressions grown into mature convictions.

I cannot but believe that an experience which, as will be seen by my "Fracture Tables," lately published by Mr. Boardman, now includes a personal examination upon the living subject, of nearly six hundred fractures, will possess some value as corroborative testimony.

In this way, mainly, have I arrived at my conclusions, yet I have not neglected the examination also of pathological specimens, such as I could find in my own or other private or public museums.

In 1844 I studied very critically the famous Dupuytren Museum, at Paris, which is peculiarly rich in fractures; and the results of my study and analysis were subsequently published in a tabular form in the Buffalo Medical Journal for 1849. And wherever else upon the continent, in Great Britain or in this country similar collections came within my reach, I have never failed to avail myself of them for the purposes of study. Thus from many and various sources the evidence has been derived, and the conviction has been gathering upon me that Dupuytren had greatly misapprehended the process of union of broken bones in the human subject: and this evidence was no less abundant in his own beautiful collection at Paris than elsewhere.

It is true that I was then especially directing my observations to the existence of deformity or perfection in the union of bones, and their relative frequency. But the frequent — I might almost say, constant absence of provisional callus in bones well united, and especially of the outer ring or "ferrule," very early attracted my notice and excited my surprise: and when afterward I had collected the undeniable proof that in a large majority of cases of fracture the union occurs by overlapping, there came also the very natural suggestion that very often, no doubt, the surgeon mistook this overlapping for provisional callus. That surgeons do often commit this error, I have since confirmed by my own observations in a multitude of cases.

Trusting that the care and attention which I have bestowed upon this subject entitle me to speak in a degree authoritatively, I have given you this paper containing my own views, for publication; and I have also to request that you will allow it to be accompanied with the able lecture of Mr. Paget, as delivered before the Royal College of London.

Yours truly,

FRANK H. HAMILTON.

Provisional Callus.

While prosecuting my investigations for the purpose of ascertaining the "average results in the treatment of fractures," several other points of interest have been suggested, some of which I have sought carefully to determine. That which arrested my attention earliest, and which I have most attentively noticed all along, until I have at length reached a satisfactory conclusion, is *the almost constant absence of provisional callus, both during the process of cure, and in the result, where the fractured ends have been kept in tolerable apposition, and free from undue excitement.*

I had never doubted before that provisional callus would be found in all cases of union of broken bones where the health of the patients and the condition of the fragments permitted the restoration to proceed in a natural manner. It was this which the experiments of Du Hamel and of Dupuytren seemed to have established.

The old surgeons spoke of an "exuberance" of callus, or of a "redundance," occurring as the result of displacement, frequent disturbance, inflammation, &c., but they never spoke of it as constituting any part of a healthy, normal process. Thus Mr. Pott writes:

"When a bone has been broken transversely, or nearly so, and its inequalities are therefore neither many nor great, when such broken parts have been happily and properly coapted, and proper methods have been used to keep them constantly and steadily in such state of coaptation, the divided parts unite by the intervention of the circulating juice, just as the softer parts do, allowing a difference of space of time for different texture and consistence. When the union of a broken bone under such circumstances has been procured, the place where such union has been made will be very little perceptible; it will be no deformity, nor will it occasion any inconvenience. It will indeed be discoverable, like a cicatrix of a wound in a softer part; but there will be no redundance of callus, because none will be wanted."—*Pott's Surg.; First Amer. Ed., vol. I, p. 234.*

It was believed that in refuting these erroneous doctrines of Mr. Pott, and of his cotemporaries, Dupuytren had performed an important service, and had made a most valuable contribution to surgical pathology. Mr. Samuel Cooper congratulating the art upon these modern discoveries speaks after this manner:

"A few years ago lecturers on surgery got over this subject very easily, and those teachers, whom I happened to attend, explained the matter in a concise and summary way, by stating, the only difference between the union of bone and that of soft parts, was that the coagulating lymph, effused

between the ends of fracture, gradually acquired the consistence of cartilage, earthy matter was deposited in it, and thus the bone was united, and acquired its former strength, the only particularity being in fact the deposit of the phosphate of lime in the uniting medium." — *Saml. Coop. First Lines; Fourth Amer. Ed., vol. I, p. 282.*

Although it has not escaped the observation of many shrewd writers that Dupuytren's experiments were all made upon brute animals, and they have therefore received with a prudent caution many of his conclusions, such as the period of time occupied in the several stages of reparation, the sources of the callus, &c., yet has it seldom if ever happened that they have called in question, or expressed a doubt of the accuracy of his conclusions as to the main point, viz., the existence of a provisional callus as a temporary bond of union in all cases where bones unite by a natural and undisturbed process.

I have looked carefully for such doubts or denials, but I find nothing of the kind clearly expressed; that is, nothing which can be construed into a substantial doubt or a denial that broken bones unite naturally through the interposition of provisional callus. Mr. Liston, in the following paragraph, speaks like one who sought to reconcile his own observations, not yet reduced to a system, with certain conflicting, but everywhere established doctrines, the correctness of whose maxims it would, perhaps, be scarcely respectable for a man of science to call in question:

"Union of divided bones, as of soft parts, is preceded by incited circulation in the part and effusion of matter. The extent of action is regulated by that of the injury, whether inflicted by accident or by operation. If the soft parts have not been much bruised, if the bone and its covering are merely separated and slightly displaced, and then speedily put in contact, *the incited action and the effusion are limited to the divided parts. There is no irregularity afterward at the point of fracture*, the new matter that is not required being absorbed soon after deposition; the bone is smooth and even as before. If, on the contrary, there is much displacement, and if that is not entirely removed, intense action ensues both in the soft and hard parts, there is great effusion of new matter or *callus*."

It is obvious, I think, that Mr. Liston had noticed the absence of provisional callus in simple and well-adjusted fractures, at least *soon* after the union was completed; a fact for which he offers the usual explanation, viz., that it is absorbed *soon after* deposition: yet he does not recognize its inconsistency with his preceding statement that in such simple fractures the "effusion is limited to the divided parts." But the fact that he had not noticed the presence of provisional callus even during the progress of restoration, seems probable from his account of what occurs in the opposite class of cases, where

"displacement," &c., exist, for there is now, he informs us, "great effusion of new matter or *callus*." The term *callus* or *new matter* being here first employed.

Perhaps I am disposed to infer too much from this somewhat ambiguous paragraph, but the language will certainly admit of the construction which I have given it, and I cannot dispossess myself of the belief that this great and eminently practical surgeon had seen and noticed much to conflict with the views of Dupuytren.

Mr. Stanley has made the nearest approach to a repudiation of the doctrines of Dupuytren of any man whose writings I have seen. In the preface to his excellent "Treatise on the Diseases of Bones," occurs the following passage:

"Experiments in animals have not accomplished so much for the elucidation of the reparative process of bone in man as might probably have been expected. The circumstances attendant on the fractured or necrosed bone, in man, are essentially different from those of the experiment of breaking, or causing the death of a bone in animals. Thus around the fractured bone of an animal, the deposit of cartilaginous and osseous substance, which has been designated provisional callus, is of uniform occurrence. But, in the human subject, no such cartilaginous and osseous deposit uniformly takes place around the fractured bone; here, therefore, it is not an essential part of the reparative process."

In this we have a plain declaration that provisional callus is not "uniformly" present in the union of divided bones, and that it is therefore "not an essential part of the reparative process."

But not in this admission of Mr. Stanley's, nor in the statements of any other writers do I find a complete expression of my own sentiments upon this subject.

I shall go much farther. I am now prepared confidently to affirm that the so-called provisional callus never constitutes any part of the reparative process in the union of divided bones, when all those circumstances of simplicity, apposition, quietude, health, just management, &c., obtain, which may properly be considered essential to a normal process — that bones unite most naturally by definitive callus, and that provisional callus is accidental and secondary — the result probably of undue excitement alone.

It may be, indeed, the rule that in the union of fractures some amount of provisional callus shall be found, but it will be because it is the rule rather than the exception that undue excitement exists. My fracture tables published a few years since, and farther observations lately made, will show that

broken bones are seldom kept in such complete apposition as will allow nature to proceed without interruption or disturbance.

Permit me to state my belief in another form :

Broken bones unite when submitted to the most favorable circumstances, by definitive callus, or by a process allied to adhesion — by *first intention*: but under less favorable circumstances by provisional callus, or by a process allied to granulation — by *second intention*.

The venerable and distinguished Dr. Mussy, of Cincinnati, to whom I stated my views, and by whom I believe their general correctness is admitted, said that he would express his notions of the reparation of fractured bones by saying, that nature, or the Great Author of nature, first sought to repair the injury in the **simplest** possible manner, by a direct union of the ends of the bones; but **being** defeated in this, she then chose the next best alternative, viz., to form a temporary callus, and this was the origin and object of nature's splint.

To these views, with certain qualifications, I assent. But then provisional callus is no longer the normal, but only a contingent or alternative process.

Such are the conclusions to which I have arrived, after having examined several hundred fractures, nearly one-half of which were sufficiently recent to have enabled me to have discovered the callus if any had ever existed.

There is generally no difficulty in determining the presence of provisional callus in fractures of such superficial bones as the inferior maxilla, clavicle, radius, ulna and tibia, of the metacarpal, metatarsal and phalangeal bones, and indeed very often in fractures of other bones. Frequently the swelling is so inconsiderable that the surface of the bones can be distinctly felt at any period of the process of union. I have seized all such opportunities as were afforded me, and without being able to state numerically the result I have no remaining doubt that provisional callus is not present in any stage of the reparation where the conditions of health, &c., &c., before stated, exist.

The accuracy of these conclusions can only be tested by similar examinations upon the dead or living human subject. It is not possible, I think, to put the limb of any brute animal into that condition of rest requisite to determine nature's first intention: and here is the source of the fallacy into which Dupuytren and his disciples have been lead.

While my convictions upon this subject have originated and been confirmed by my own observations, I find also many substantial collateral evidences which cannot properly be overlooked in the argument.

If Mr. Stanley is correct in supposing that provisional callus is not essential

to the process of union, and that it is not uniform in its occurrence, then it is reasonable to infer that this circumstance is not the first and established order of events: would bones, which are kept in exact apposition and undisturbed, unite by definitive callus alone, and that often in three or four weeks, if nature had established provisional callus as her chosen mode of union? If she is competent to unite bones by "first intention," why should she ever seek to unite by "second intention," unless driven to it as an alternative?

Nature is not so capricious. She never attempts to accomplish the same end, under the same circumstances, in different ways. To this law, I believe, there are no exceptions. It is clear, therefore, that Mr. Stanley admits too much or not enough.

I find another argument in support of my opinion in the fact that in the reparation of fractures occurring in certain bones, or in certain parts of bones, provisional callus, it is conceded, seldom or never occurs. Thus it is with the cranium, acromion process, coracoid, olecranon, patella, &c., and with all those portions of bones which are immediately invested with a synovial capsule.

If provisional callus is the established mode of reparation, why in these cases is it not furnished?

It was a very beautiful theory which referred the formation of provisional callus to an intelligent efficient cause, which in this manner sought to support the bones until a union of their divided ends was effected. Nor is the beauty of the conception marred by ascribing to it a more limited application, and invoking its interference only when the ordinary resources of nature have failed. We no longer hold that any such intelligent interposition is necessary in the *first* instance or in simple fractures, and if demanded at all, it is only for an exigency. But we have grave doubts whether nature ever allows any interference with her laws even in an exigency, unless by the substitution of a miracle. Provisional callus is just as much the inevitable result of natural laws, as is definitive callus, or any other reparative action. It is formed, because in that condition of the parts and of the general life, its formation was inevitable. Whether needed or not it will, under certain circumstances, exist.

It is affirmed, nevertheless, that in the fractures just named, this callus is not formed, because it is not required. While to me it seems that nowhere could it prove more useful, since, with the exception of the cranium, it is in these very cases that the obstacles to union are most numerous. In fractures of the patella, olecranon, &c., the action of the muscles tends constantly and *powerfully* to displace the fragments, and gladly would the surgeon avail

himself of the assistance of a temporary callus, but it is rarely present, and then in no useful degree.

So, also, in fractures of the neck of the femur within the capsule, and in other similar cases, we cannot say that temporary callus would not be advantageous in facilitating the retention of the fragments, yet the "intelligent efficient agent" neglects to furnish it.

The only satisfactory reason which, as we think, can be assigned for the absence of callus in these cases, is found in the doctrines which I now advocate; that is to say, it is usually absent because that amount of excitement and irritation are usually absent which alone determine its formation. In the case of the olecranon, patella, &c., the fragments being separated from each other by muscular action, so that no painful pinchings or chafings occur, and their rough surfaces, or sharp points being rather drawn away from than protruded into the flesh, no sufficient provocation exists for the production of inflammation and effusion. Hence the failure of *provisional* callus, but wherever the fracture occurs, and however moderate the action, *definitive* callus does not fail; still the broken surfaces of the patella and olecranon are softened, and smoothed, and covered over with a new matter, which, if contact could have been secured and preserved, would certainly have served to consolidate and repair the breach. The natural reparative process proceeds, but only the accidental process is omitted. This latter, however, is seen again even here, when from other and unusual causes a sur-excitement is established.

Temporary callus is not formed upon bones invested with synovial membranes, because here, too, as in the neck of the femur, there are not so many structures lacerated and irritated, and the supply of this effusion must be the less not only in proportion to the less intensity of the inflammation, but also to the less amount of structures implicated.

Possibly other and more satisfactory reasons may be assigned why *provisional* callus is not formed usually when the neck of the femur is broken within the capsule; but we certainly can never admit the common, and as here applied, the too palpably absurd explanation, that it is not wanted. It is wanted! and in no case so much as now.

The same argument applies to fractures of the cranium. With less soft parts to suffer excitement and to determine effusion, and with no motion of the fragments to provoke it, *provisional* callus is less apt to occur. But you need not be told, gentlemen, that here again, when the injury has been most severe and the consequent excitement most intense, the so-called "nature's splint," has been formed; although in this instance it could serve no possible purpose.

In short, provisional callus occurs still everywhere, when against and in the vicinity of the bone there is the requisite lesion and action; and it will occur as certainly when the fracture is incomplete, or when there is no fracture, but only a caries, a necrosis or a simple bony or even periosteal inflammation — and it becomes thus the basis of many tumors which grow from either the bone or the periosteum.

Recapitulation.

First. Broken bones unite directly, naturally and by preference through the interposition of definitive callus.

Second. Broken bones unite indirectly, and accidentally, through the intervention of provisional callus.

Third. The absence of provisional callus does not denote that it could serve no useful purpose.

Fourth. Its presence does not indicate its necessity or utility.

Fifth. It has, therefore, no final purpose, but is the unavoidable result of a certain abnormal condition: and while it is doubtless true that in fractures it frequently renders valuable assistance to the surgeon, it is also equally true that it often proves a source of hindrance.

Extract from Prof. Paget's Fifth Lecture on the "Processes of Reproduction after Injuries."

I shall not endeavor, in the present lecture, to treat fully of the Repair of Fractures. No one acquainted with the extent of the observations already made on this subject, and with the reputation of those who have been occupied with them, will blame me if I almost limit myself to the endeavor to explain only two or three points in the history of the repair of injured bones. The chief points that I have chosen are—first, the particulars in which the process of repair of fractures, observed in the human subject, deviates from that described from experiments upon lower animals; and secondly, the nature of the reparative material previous to its ossification.

On the first point, I must express my conviction that the description drawn by Dupuytren and others, from examinations of fractures in dogs, rabbits, birds, and other animals, cannot be applied without great deductions to the case of fractures in the human subject. True as the pictures are of the cases of the animals examined, they are exaggerations of the process in our own case. With a few exceptions, all that is written in these accounts of external and internal provisional and definitive callus, of the formations of cartilage and bone within the medullary tube and beneath the periosteum, can be traced only, as it were, in rudiment in the fractures of the human bones.

My impression of this was obtained while describing the large collection of fractures for the catalogue of the museum of the college.

With the concurrence of Mr. Stanley, who had long held a similar opinion,* I then wrote—"There is scarcely a specimen in the museum of such a provisional callus formed in the repair of a fractured human bone; in nearly every case of such fracture, the material of repair, whether cartilage or bone, is only inlaid between the broken surfaces, or between the adjacent parts of the fragments, and unites them by being fixed to both. In favorable conditions this appears to be the usual mode of repair, even though the fragments of the broken bone be very much displaced."

"But the formation of a provisional callus, completely encircling the broken ends and adjacent parts of the fragments, is usual in the repair of fractures of the bones of other mammalia, and of birds. * * * * * A similar but less perfect process is also shown in the accumulations of cartilage or bone which are often formed about fractures of the ribs, and of some other bones in the human subject, the fragments of which have not been held steady. It is probable, therefore, that the difference between the modes in which fractures are commonly united in man and other animals, respectively, depends in part on the movement to which the fragments are subjected in the latter; but probably in part, also, on the greater readiness with which, under all circumstances, bone is formed in the animals lower than man."†

Since that was written, I have examined many more specimens, and find the same rule true; namely, that in the ordinary repairs of simple fractures in the human subject, the reparative material, or callus, is merely inlaid between the several fragments; it fills up the interspaces between them and the angles at which one fragment overhangs another; but it does not encircle or ensheath them, in the manner implied in the description of provisional callus; nor is it in any considerable quantity, if at all, deposited either beneath the periosteum or within the medullary tube. In birds, dogs, and other ordinary subjects of experiments, the formation of a provisional, or as it may perhaps be better called, an *ensheathing* callus, is usual. It is illustrated by numerous specimens on the table; yet even in animals it is not constant. To obtain what would be called good specimens of provisional callus, the injuries must be inflicted upon young animals, and among these I cannot but suspect that particular instances have been selected for description—those in which less callus was formed having been put aside as imperfect instances of repair, though, in truth, they may have displayed the more natural process.

For fractures in the human subject, the evidence that union is accomplished by the reparative material being placed between, not within and around, the fragments—*i. e.*, as an intermediate, not an *ensheathing* callus—this evidence may be obtained by the examinations of such fractures even long after they are completely healed. In as many as you like to examine you will find the new bone formed exclusively between the fragments. Whether they were in apposition, or nearly so, or wide apart, still there is no appearance of new bone being formed on the outer side of any fragment—I mean on that side which is turned away from the other fragments. And this is the case even in those instances in which there is so much displacement of the fragments, and so much distortion, that we can hardly suppose the repair to have proceeded very quietly. Neither in any of these do you find new bone within

* Since the delivery of the lecture, Mr. Stanley has published his account of the Repair of Fractures in the descriptions of his beautiful "Illustrations of the Diseases of the Bones," p. 27.

† Pathological Catalogue of the Museum of the College of Surgeons, vol. ii, p. 37,

the medullary tube. It may be objected by some to these specimens, that the fragments were once ensheathed and blocked up with callus, and that it has been since absorbed. But this is not probable, seeing that in many cases there remain, on the outer surfaces of the fragments, certain marks of their original form and slight irregularities. In one of the specimens which I present, we have traces of the healing of a long fissure, which appears now as a sunken groove, making it nearly certain that no new bone was formed over it. In another, is a detached piece of the wall of a femur turned quite round so that its periosteal surface lies on the periosteal surface of the principal fragment; yet, on the outer surface of this piece (which was the inner surface of its wall) the thin plates forming the boundary of the medullary tube are still unchanged.

But if any deem these and the like characters insufficient to prove the absence of ensheathing callus, and of callus extending into the medullary tube, yet recent specimens are not open to such doubts. I add, therefore, that (with the exceptions presently to be mentioned) in all the specimens of fracture that I have been able to examine, in the human subject, within six months of the time of the injury, there has been the same absence of provisional or ensheathing callus. The specimens here present are—a radius, four weeks after the fracture; another, four or five weeks; a tibia, five weeks; a femur, six weeks; another of the same date; a third, I should think, about eight or nine weeks; a radius, of somewhat later date; a tibia, eight weeks; a fibula, eleven weeks; a tibia, twelve weeks; and a tibia, sixteen weeks after the injury. Here are, also, others of various but unknown dates, all in process of apparently natural repair. All these were cases of simple fractures, and they include (with a few exceptions presently to be mentioned) all the specimens of such recent fractures, in the human subject, as are in the museums of the College and of St. Bartholomew's Hospital. The displacements and other conditions following the injury have been manifestly various: but all agree in this—that the fragments are united by intermediately-placed reparative substance, and that this, whether soft or osseous, in no case surrounds or ensheaths the fragments, or does more than just close in the medullary canal. When present in the largest quantity, it is only enough to smooth off the chief irregularities, and to fill up the interspaces and the angles or corners, between the fragments.

Such, then, appears to be the natural mode in which the reparative material is deposited for the union of fractures of human bones. And, regarding the particular position which it may in each case occupy, I do not know that it can be more exactly described, than by saying, that it is deposited where it is most wanted for the strengthening of the bone—so that, whatever would be the weak part of the bone, if unhealed, there is the new material placed, in quantity as well as position just adapted to the exigencies of the case, and restoring, as much as may be, the original condition and capacities of the bone.

If now it be inquired why this difference should exist in the corresponding processes in man and other animals, I believe still that it must be ascribed principally to the two causes already quoted from the catalogue—namely, the quietude in which fractures in our bones are maintained, and the naturally greater tendency to the production of new bone which animals always manifest. Even independently of surgery, in the case of fractures of the lower extremity, the human mode of progression almost compels a patient to

take rest; and in fractures of the upper extremity, the circumstances of human life and society permit him to do so far more than other animals can. The whole process of repair is, therefore, more quietly conducted; and, as we may say, there is comparatively little need of the strength which the formation of provisional callus would give a broken limb.

The exception to the rule of difference in the repair of human bones and those of animals confirm it as thus explained; for the only bones in which, in the human subject, a provisional callus is generally or naturally formed, for the repair of fractures, are the ribs. In cases of fractured ribs one may see, indeed, a very close imitation of that which is described, from experiments on animals, as the ordinary mode of union. The provisional callus is well formed under the periosteum, and encircles, like a broad ring or ferrule, both the fragments, and may almost completely ossify before their union is accomplished, or even apparently begun.

Another bone for the repair of which, but more rarely, callus is formed around the ligaments, is the clavicle; and the best specimen in which I have here seen it is one in which the fracture was not detected, and the fragments were allowed to move on one another, till the patient died twelve weeks after the injury.

Except in such cases as these of fractures not kept at rest, I doubt whether a natural formation of callus beneath the periosteum, or within the medullary tube of a human bone, would ever occur. In disease, the occurrence is not so rare; for, when the natural process of union fails altogether, the loose ends of the bones may be inclosed within a case formed wholly or in part of bone; or an imitation of callus may be made by a gradual morbid accumulation of bone around a fracture, even after its natural union.

But I think the comparative restlessness of animals is not alone sufficient to account for all the difference in the processes. The remainder may be ascribed to their greater tendency, in all circumstances, to the formation of new bone. Not in fractures alone, but in necrosis, this is shown. It is very rarely that such quantities of new bone are formed in even children, as are commonly produced after necrosis of the shafts of bones in dogs or other animals; nor is there in the human subject any such filling up of the cavities from which superficial sequestra have been separated, as the experiments of Mr. Hunter showed, after such exfoliations from the metatarsal bones of asses.*

Other examples might be quoted; but these might suffice to show that, after injuries, new bone is formed more abundantly in animals than in man. And I hope enough has been said to prove that the generally-received account of provisional callus, and other parts of the healing of fractures, is an exaggeration of what occurs in man. It is to be asked what it is that is felt like a callus after fractures, I would say that, in such cases as I could examine after death, I have usually found that the overlapping ends of the bone, being both at once grasped, had been taken for the enlargement of callus. Sometimes, also, the thickening and induration of the parts around the fracture infiltrated with serous and bloody fluid, or with lymph, have been mistaken for it.

* Museum of the College, Nos. 641 to 653.

ART. II.—*On the Transformation of the Encysted Entozoa into Tape-Worms.* By JNO. C. DALTON, Jr., M. D.

The following report of some experiments by Prof. C. Th. von Siebold, will be found of considerable importance, not only from their general zoological interest, but more particularly from their bearing on the question of spontaneous generation. Notwithstanding that the idea of spontaneous or equivocal generation is repudiated by the great majority of physiologists at the present day, it seems to be still entertained in some quarters, even by men of considerable scientific attainments. The defences of this theory, however, have one after another given way before the advance of zoological science. It was demonstrated long ago by Redi and Valisnieri, that the worms, which appear in putrefying flesh, were not produced, as previously supposed, from the decomposing animal matter itself, but were the larvæ of winged insects, which had been led, by a curious instinct, to deposit their eggs in such places as would afford the necessary food to the young and imperfect progeny during the earliest stages of its existence. The Italian observers thus explained, in the most natural manner, what seemed to be a very puzzling circumstance, viz., that these worms, which always showed themselves in great numbers in decomposing animal substances, were not to be found in any other situations. The species seemed to be confined to putrefying substances; and as they often appeared under circumstances which precluded the idea of their having been transferred from other collections of decomposing matter, it was not easy to understand how they could have originated by the ordinary mode of generation. Redi and Valisnieri, however, demonstrated that they were the progeny of perfect insects; and that the *species* was not in reality confined to decomposing substances, but existed elsewhere, though in a different form.

The existence of the infusorial animalcula, again, seemed for a time explicable only on the supposition that they were produced spontaneously in the animal or vegetable infusions which they inhabited. But this supposition has been set entirely at rest since the experiments of Schultze, at Berlin, in 1837, proved that though the production of infusoria was almost invariable when the infusion was kept at the proper temperature and exposed to the access of atmospheric air,—yet that the animalcules were not generated if sufficient care were taken to preclude all possibility of living germs being introduced into the infusion from without. We take it, therefore, not even the most

determined advocate of the theory of equivocal generation will place much reliance on the celebrated experiment of the production of the *acarus Crossii* from the continued action of galvano-electricity on a chrystalizable saline solution; — particularly as the acari so produced possessed generative organs, and the females, soon after their appearance, provided for a continuance of the species by an abundant production of ova. The chances of the accidental introduction of living germs into the machine during the course of so long-continued an experiment are too great to allow any one to remain satisfied without a personal inspection of the apparatus.

The only remaining defence of the spontaneous-generation theory, is the existence of *entozoa*; and it is behind this last, and apparently most impregnable barrier, that its partisans have finally entrenched themselves. The entozoa, like the infusoria, are confined to certain situations. They are never detected out of the living body. Particular species of animals are even infested by particular species of parasites, and by no others. The *Taenia solium* inhabits the intestines of the human subject, the *T. serrata* those of the dog, the *T. crassicollis* those of the cat. In the same individual, even, different organs are occupied by different entozoa. We must look for the *Trichocephalus dispar* in the cœcum, for the *Strongylus gigas* in the kidney, and for the *Distoma hepaticum* in the liver. "These facts," it has been said,* "seem to show that some extremely local concurrence of circumstances is essential to the production of the several entozoa." But it is very easy to see that these strictly local conditions may be not at all necessary for the production, but only for the development of the entozoa. It is certainly no more surprising that one species of *Ascaris* should inhabit the large, and another the small intestine, than that the *Lobelia inflata* should grow only in dry pastures and the *Lobelia cardinalis* by the margin of meadow-brooks. The Lichens flourish on the exposed surfaces of rocks and stone walls; while the Fungi vegetate in darkness and moisture, on the decaying trunks of dead trees. Yet no one imagines these vegetables to be spontaneously generated from the soil which they inhabit. The fact is simply this: that if the animal or vegetable germ be deposited in a locality which affords the conditions necessary for its development, it becomes developed; otherwise not. The grains of wheat which had remained for centuries, wrapped up in the cerements of Egyptian mummies germinated freely when exposed, in an appropriate soil, to the influences of light, air, warmth and moisture. The circumstance, therefore, that particular parasites are confined to particular localities

* Stillé's Pathology, Philadelphia, 1848, p. 473.

presents no greater difficulty as to their mode of reproduction, than the same fact regarding other animal and vegetable organisms.

Every articulation of the *Taenia solium* contains, when in a state of maturity, many thousands of ova; all of which are necessarily expelled from the body when the articulation drops off. Now though the chances are enormous against any particular one of these ova being accidentally transported into the intestinal canal of another individual, it is easy to see that there are many causes in operation by which *some* of them might be so transported. By far the greater number undoubtedly perish, from not meeting with the conditions necessary to their development. One in a thousand, or, perhaps, one in a million, is accidentally introduced into the body of another individual, and consequently becomes developed into a perfect animal.

The greatest difficulty, however, was presented by the *encysted entozoa*, which are not only confined to particular organs, like other parasites, but which are also destitute of any generative apparatus. Now a species which is destitute of generative organs evidently cannot reproduce itself; and the encysted entozoa have therefore been regarded as presenting at least one undoubted instance of equivocal generation, i. e., a progeny without parents. The fact that no similar animals were found external to the body might have been got over; but that those existing as parasites in the parenchyma of living organs were themselves destitute of any generative apparatus, seemed to exclude the idea that they had been produced by the ordinary modes of propagation.

It is strange that those who advocated so strenuously the doctrine of equivocal generation could not see that this circumstance might be explained in the same way with the production of maggots in putrefying meat; a point which had been settled so long ago by Redi and Valisnieri. These maggots differ in structure from their parents because they are as yet incomplete. They may be considered, to some extent, as still in a fetal condition. For the same reason the generative organs are not yet developed. The larva is incapable of reproducing itself as a larva. But after it has passed through the natural transformations, and its organization is completed, a sexual apparatus appears, and the *species*, though not every individual belonging to it, is found to be perfect. Now, by watching the growth of any one individual, from the egg to the state of a complete insect, we get a history of its transformations, and comprehend that the animal may be destitute of sexual organs at one period of its existence, and provided with them at another. But if the conditions necessary to the later stages of development are wanting, the larva will not be transformed into an insect, but will remain a larva; and,

of course, so long as these conditions are wanting, so long will the sexual organs remain absent. And this is precisely the case with the encysted entozoa. They bear very much the same relation to the *Tænia* as the undeveloped larva to the perfect insect. It is, perhaps, unnecessary to remind our readers that the *Tænia* is not now considered as a single animal, but as a colony of animals; every articulation being a distinct individual. These articulations are multiplied by a process of budding, which takes place just behind the "head," or most anterior individual of the colony. As new ones appear, those which were previously produced are pushed farther and farther from the "head;" so that the oldest articulations are those situated at the posterior extremity of the chain. The young and imperfect individuals compose the "neck" of the *Tænia*. They are, as yet, without sexual organs; but as they increase in size, and are gradually removed farther from the head, they become provided with a sexual apparatus, each articulation containing both male and female organs; so that the posterior portion of the chain is composed of completely developed hermaphrodite individuals. As these individuals arrive successively at maturity, they become detached from the chain, and pass out of the intestine with the *fæces*, after which the ova are probably set free by the death and decomposition of the parts which enclose them. A portion, then, of every "tape-worm" is destitute of reproductive organs, and yet it has itself undoubtedly been produced from ova, and will hereafter, if circumstances are favorable, produce ova in its turn.

Now the following experiments, by Prof. Von Siebold, demonstrate the very important point that those parasites which have been regarded as incapable of reproduction as a species, are really incapable of it only as individuals, because they have been prevented from arriving at their mature condition; and that the sexless encysted entozoa are in reality only undeveloped or diseased *Tæniæ*. We have ascertained by personal inquiry, what might have been anticipated from the previous reputation of the observer, that both the experiments, and the results derived from them, are regarded by scientific men in Germany, as entitled to complete confidence. The following account of the experiments is translated from a report, in the "*Silesian Times*," of the Transactions of the "*Silesian Association for National Instruction*, Scientific Department, Session of July 7th, 1852:"

Report.

Professor Von Siebold made a report on the experiments which were undertaken some months previously, in the Physiological Institute, under his direction, for the purpose of showing the possibility of a transformation of
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the cystic parasites into tape-worms. He had already, in the year 1844, in the second volume of the *Encyclopædia of Physiology*, expressed the opinion that the parasitic cysticercus (*C. fasciolaris*), found in the liver of rats and mice, was nothing else than an abnormal, dropsical tape-worm; and that it was, in reality, identical with the tape-worm of the cat (*Tænia crassicollis*.) He maintained further that the *Cysticercus fasciolaris*, like all cystic worms, was invariably destitute of sexual organs, and could not multiply its species by generation, unless it were transferred to a favorable locality, where it might lose its dropsical condition and develop its sexual organs. These changes actually take place when a rat or a mouse, with a *Cysticercus fasciolaris* in its liver, is devoured by a cat. The parenchyma of the liver, according to Siebold, is digested in the stomach of the cat; but not so the entozoon. The parasitic animal loses only its dropsical appendage, and passes, with the digested food, from the stomach of the cat into the small intestine. It then finds itself in a favorable locality, and becomes developed into a perfect tape-worm, with articulations and sexual organs (*Tænia crassicollis*.) This idea had been first suggested to Prof. Siebold by the perfect resemblance between the cephalic extremity of the *Cysticercus fasciolaris* and that of the *Tænia crassicollis*; and by the fact that there are often found, in the intestine of the cat, several specimens of the *T. crassicollis*, in different stages of development. His opinion was adopted by many naturalists, but its correctness had also been called in question by others. Some years ago Dr. Kuchenmeister, of Zittau, had made use of the *Cysticercus pisiformis*, a species of encysted parasite very common in the peritoneum of hares and rabbits, for a series of experiments in which he caused these parasites to be swallowed by dogs and cats; in the expectation that they would become developed into tape-worms in the intestine of these animals. The trial succeeded perfectly with dogs; and the same thing that Prof. Siebold had previously inferred from a comparison of the *Cysticercus fasciolaris* of rats and mice with the *Tænia crassicollis* of the cat, would seem to have been definitely established by these experiments of Kuchenmeister. But Kuchenmeister's experiments, and the conclusions he drew from them, did not prove satisfactory either to naturalists or medical men. He had committed the error of publishing his investigations before they could properly be considered as terminated. He was consequently obliged, in the various communications which he published on the subject, one after another, in the medical journals of northern and southern Germany, to correct many of his former statements, and even to retract some of them; and he entangled himself, finally, in so many contradictions that it is to be feared the doctrine of a close relation between cysticerci and

tape-worms was rather retarded than advanced by his activity; particularly as he himself several times acknowledged that he was not sufficiently familiar with the study of intestinal worms to distinguish them with certainty. Such a confession certainly was not calculated to increase the confidence of naturalists in his experiments. He also exhibited his incapacity to distinguish the entozoa by giving, in succession, several different names to the newly-produced tape-worm, which he described at first as the "*Tænia crassiceps*" of the fox, afterward as the "*Tænia serrata*" of the dog, and finally as an entirely new species, under the name of the "*Tænia pisiformis*." Prof. Siebold then determined to undertake himself similar experiments. They were tried principally on young dogs, not only with the *Cysticercus pisiformis*, but also with the *C. cellulosa*, *C. tenuicollis*, *Cænurus cerebrialis*, and *Echinococcus veterinorum*.

The following results were obtained from the experiments with the *Cysticercus pisiformis*. These entozoa, which are usually about the size of a pea, were given to young dogs, mixed with milk, still enclosed in their peritoneal cysts, and in quantities varying from thirty to sixty. The dogs were afterward killed with chloroform, at various intervals of time, and the contents of the stomach and intestine being carefully examined, the entozoa were readily discovered in various stages of development. Two hours after being swallowed, they were almost all found still in the cavity of the stomach. The cysts, however, which had enclosed them, were generally digested and destroyed, and at the same time, the greater number were not only freed from their envelope, but had also lost the vesicular portion of their posterior extremity. This vesicle was either entirely digested or else hung in shreds attached to the end of the body. All the entozoa, which were found in the stomach, whether they had lost the vesicular appendage or not, invariably had the head and neck drawn back into the body.

When the dogs were killed after an interval of three hours, no entozoa were found remaining in the stomach. They had all passed, together with the digested food, into the small intestine. Their cysts and vesicular appendages had all been destroyed by the digestive processes in the stomach; but the head and neck were again, without exception, protruded, and the body, which had been before contracted, was stretched out longitudinally. In all of them were to be seen marks of injury at the posterior extremity of the body, where the vesicular appendage had been attached. When the dogs were suffered to live several days after the commencement of the experiment, the entozoa were found to have considerably increased in size. The largest had attained the length of three inches, the smallest that of one inch.

body, which had previously shown only transverse wrinkles, now exhibited very plainly articulations in its central portion. The posterior portion was still wrinkled transversely, and the lacerated spot at its extremity had assumed the appearance of a cicatrix.

After twenty to twenty-five days, the entozoa were already several inches long, and perfectly articulated quite to their posterior extremity, where the cicatrix was still evident; and on the posterior articulations were to be discovered traces of a sexual apparatus.

After eight weeks the cysticerci in the intestine of one of the dogs had attained a length of many inches. The largest were thirty-six to thirty nine inches in length, and their posterior articulations were provided with a perfectly developed sexual apparatus, and contained many mature ova. Several of those a yard long had already thrown off their posterior articulations, with their mature sexual products. Von Siebold was now able to recognise, in this tape-worm, developed out of the *Cysticercus pisiformis*, the *Tænia serrata* of the dog. The cephalic extremity, the form of the articulations, the structure of the generative organs, and particularly the mature ova of this tape-worm, corresponded, in the most perfect manner, with the same parts in the *Tænia serrata*. There was no longer any doubt that the *Cysticercus pisiformis* of the hare and rabbit bore the same relation to the *Tænia serrata* of the dog, as the *Cysticercus fasciolaris* of rats and mice to the *Tænia crassicolis* of the cat. Furthermore, the *T. serrata* is rarely met with in the intestines of parlor and house dogs, but is, on the contrary, very abundant in hunting dogs; no doubt because the latter are often allowed to devour the entrails of hares killed in the chase, swallow at the same time the *Cysticercus*, and so become infested with the *Tænia*; a circumstance which would naturally be less frequent with parlor and house dogs.

Although Siebold's experiments with the other species of encysted entozoa, mentioned above, were not entirely finished, he had yet carried them so far with the "*Cœnurus cerebralis*" as to convince himself that this worm, also, which is so much dreaded by sheep-breeders, becomes developed, in the alimentary canal of the dog, into a tape-worm. The tape-worm produced from this parasite had not yet, in Siebold's experiments, become developed to the stage of sexual maturity; so that he was still unable to determine with certainty its species. He hoped, however, by means of continued trial, to produce from the *Cœnurus cerebralis* perfectly mature *Tæniæ*; so that he might be able, after distinguishing their species, to determine what animal it is in whose intestine the sexless *Cœnurus cerebralis* becomes developed into a tape-worm with mature sexual apparatus. He will then, probably, have it in his

power to give agriculturalists some hints how to prevent the development of this parasite in the brain of the sheep. For he is convinced that the encysted entozoa do not originate by spontaneous generation, but are produced from the microscopic ova of the tape-worms of certain carnivorous animals, which are introduced by accident into the bodies of rodentia and ruminantia. Here they are not developed into tape-worms; but degenerate into encysted worms, which exert a more or less injurious influence on the life of the animal, according to the importance of the organ in which they have taken up their residence and at the expense of which they live.

The experiments which have been commenced with the *Echinococcus veterinorum* have already shown that this parasite is also to be considered as a tape-worm. The progeny of this destructive entozoon are produced, as is well known, in great numbers, by a process of budding from its inner surface. These were given in spoonfuls to young dogs, and in a few days afterward thousands of exceedingly small tape-worms were discovered, fastened by their four suckers to the mucous membrane of the small intestine. The bodies of these tape-worms consisted of only three divisions, viz., a head and neck for the first division, then a small articulation, and finally a longer one at the extremity. In both of these articulations the sexual organs had already begun to show themselves. They were not yet, however, so far developed that the worms could be considered as in a state of maturity, or their species accurately determined. It is Prof. Siebold's intention to continue these experiments; and he hopes, at some future time, to communicate their results to the Association.

ART. III. — *A Case of Metastatic Abscess, following Compound Fracture of the Thigh, Rupture of the Spleen.*

This patient was a boy of fourteen. Previous to his tenth year he had been generally healthy, but for the last four or five years he had grown pallid and slender, though without exhibiting any other very definite morbid symptom. On the afternoon of July 30th, as he was playing about the decks of a vessel, he fell through a scuttle, a distance of something over ten feet, striking on a pile of ring-bolts and other pieces of iron. He was stunned for a few minutes, but soon recovered. He made no complaint of pain except in his "stomach." He was at first somewhat collapsed, but grew warm soon after being put to bed.

When first seen by a medical man, about an hour and a half after the accident, he was lying quiet in bed, making no complaint; but when interrogated said he had pain in the left thigh and left hypogastrium. Countenance pale. Skin moderately warm. Pulse 60, of good force.

The left femur was found to be fractured a little below its middle; and one and a half to two inches shortened. The lower fragment was drawn inward, and, together with the leg and foot, considerably inverted, so that the foot rested on its inner edge. The upper fragment was directed outward. There was a wound on the outside of the thigh, one inch in length. The finger introduced into the wound, passed upward and inward for about one and a half inches, and came in contact with the lower extremity of the upper fragment of the femur, which was stripped bare of periosteum for one inch above the fracture. The lower fragment could not be felt. The wound was closed by adhesive plaster, the thigh done up in a many-tailed bandage, and the patient placed on a double-inclined plane.

He passed a tolerably quiet night. Next morning he was somewhat restless. The tongue and skin were well. Pulse 100, of good character. He had little or no pain in the thigh, but complained of tenderness over nearly the whole of the left chest. There was no bruise, however, or other injury discoverable at this part. The tumefaction of the thigh was moderate.

For two or three days succeeding he was much the same; quiet and comfortable, with some little heat of skin, and a pulse ranging from 92 to 104. His bowels were open spontaneously, and he had some appetite.

On the morning of August 3d, the fifth day of the injury, he had a well marked chill. It came on suddenly, without known cause, and continued for about ten minutes. It went off, however, after the use of a slight stimulant, and he passed the day as usual. That night he was delirious, according to the report of his nurse. Next morning (4th) he had a return of chilliness, but no distinct rigor. His countenance was then pale. Pulse 116. Manner rather flighty, but no delirium. Very talkative. Tongue natural. No appetite. A little pain in the fractured thigh.

The wound had not united at all, but otherwise there was no morbid appearance about it. The thigh was straight, and of good length and shape. The tumefaction was very moderate. He had a slight laxative of sulphate of magnesia.

Next morning (5th) he was more delirious, and the delirium had almost precisely the character of *mania a potu*. He was restless, talkative, agitated. Countenance pale. Some irregular, semi-involuntary movements of hands and fingers, approaching to tremor. He appeared sometimes to see objects

in the ~~room~~ which had no existence, as "mummies," &c. Pulse 104. Skin and tongue as the day previous. During this day he had the elixir of opium twice, in doses of ten drops, but it did not appear to quiet him in the least.

On the 6th August, (eighth day of the injury,) a swelling was first observed on the back of the right hand, which had increased by the next day (7th.) The general symptoms were also more unfavorable. The delirium continued. The countenance was more haggard than before. Teeth and lips dry, and covered with brown sordes. Tongue dryish, with a thin yellowish coat. Thirst constant. He had slept much at intervals since the preceding evening, but was easily roused. While awake he was anxious, querulous, restless. He cried out often, as if in pain or fear, but could not indicate distinctly the seat of his distress. He had taken five doses of elixir of opium since the morning of the fifth. The wound discharged no pus, but only a thin reddish fluid. The swelling on the back of the hand was moderately firm, situated chiefly over the back part of the carpus and meta-carpus. The integuments were cedematous, of a diffused dull red color, which vanished under the pressure of the finger and returned again immediately. There was much tenderness of the hand on firm pressure, but the patient did not complain of pain in this part except during examination. There was no fluctuation.

There was also a decided fullness, without any discoloration of the skin over the posterior part of the left shoulder joint, and the adjoining parts of the scapula. The patient complained bitterly when the left arm was moved, and talked much, in his delirium, of a "dog" having "bitten him there."

The elixir of opium was suspended, and he was ordered wine and water, aa ζ ss. every two hours.

He took the wine without difficulty, and the next day, as he had not lost ground at all, the dose of the stimulus was reduced to ζ ss every four hours; and he continued to take it in this quantity. The swelling of the right hand gradually increased, and extended up the fore-arm to the elbow, the redness of the skin continuing of the same character as at first, and confined altogether to the back of the hand. The swelling of the left shoulder increased but little, and though a slight redness appeared over the upper and outer part of the joint, it was never well-marked or extensive.

The delirium gradually altered in character and became more quiet and heavy, resembling that of typhoid fever. The skin was a little hot and rather moist, but there was at no time excessive heat or profuse perspiration. Pulse 112 to 120, moderately strong and full.

On the morning of the 9th, his bowels having been opened three times,

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the preceding day, without medicine, he had a fluid, light-colored, involuntary discharge; and soon after, having been left for a few moments by the attendant in charge of him, he fell from the bed on to the floor, a distance of about three feet. His fall was light, and he remained after it lying quietly on the floor, without making any outcry or other expression of pain. The thigh did not appear to be displaced, nor were there any bruises or other marks of injury consequent on the fall.

At this time his countenance was pallid and haggard as before; the skin moderately warm and moist; the tongue and lips dry. Pulse 112. He was sufficiently intelligent to put out his tongue when desired to do so, but talked irrationally and with an indistinct articulation. The pupils were equal, of natural size and sensible to light.

Copious fluid, involuntary, discharges from the bowels continued through the 9th and 10th. On the 11th there was no remarkable change, except that the countenance was more sunken. Skin warm. Pulse 136, and feebler than before. He took some beef tea, apparently with relief. He seemed to be free from pain. He got the following prescription:

R: Mist. Cretæ ℥ss.
Elixir Opii, gtt. x.

M.

On the evening of the 11th he appeared weaker. Pulse 140, and the skin inclined to be cool. He screamed out, from time to time, and talked unintelligibly. The articulation was very indistinct. He died on the morning of the 12th, without convulsion, stertor, or any other remarkable symptom.

He had no cough at any time during his illness. On the 10th his respiration was recorded as 48 per minute, but it was not at all laborious, and resembled simply the rapid respiration of constitutional irritation. He occasionally complained to those about him of pain in the abdomen, but this was not a prominent symptom, and never sufficient to attract much attention after the first two days of the accident.

Autopsy four hours after death. Cadaveric rigidity tolerably well established. Countenance natural. No remarkable discoloration of skin.

HEAD.

The longitudinal sinus filled with a dark coagulum. A drachm or two of reddish serum at the base of the brain. Brain and membranes everywhere

quite healthy. No softening, vascularity, discoloration, or other morbid appearance anywhere.

CHEST.

Heart, &c. Pericardium natural, containing the normal amount of clear serum. Right cavities of the heart considerably distended with dark fluid and coagulated blood, and fibrinous clots; the latter very firm in consistency. Venæ cavæ and pulmonary artery each contained dark fluid blood and some dark coagula. The left ventricle was firm, prominent, nearly empty. The left auricle contained a little dark-red and fibrinous clot. Endocardium and valves all healthy. Aorta, carotid arteries and jugular veins ditto. Weight of heart $5\frac{1}{2}$ ounces.

Lungs, &c. Anterior surface of both lungs of a light pinkish color. Both pleuræ healthy. No effusion. Upper and middle lobes of right lung very full of air. These parts did not collapse so much as usual after opening the chest, and were throughout of a light pink color. Pulmonary vesicles larger than usual.

Pneumonia. The lower lobe, excepting a very small portion of its upper and anterior part, was universally of a dark purple color, heavy and solid in consistency, and having a finely lobulated feel, like lung thickly filled with tubercle. The posterior surface showed numerous dark-red circular spots of ecchymosis, apparently in the substance of the pleura, or immediately beneath it. This lobe did not crepitate; and, when the remaining portions of the lung were distended by inflation, admitted the air very imperfectly, and retained nearly its original size and shape. Its section presented a dark-purple granulated surface, without pus, or much fluid exudation of any kind. Its texture was considerably softened. There was no alteration of the bronchi in the inflamed portion of the lung. The line of demarkation between the inflamed and healthy parts was very distinct. The left lung was for the most part quite healthy, only a small portion of the posterior edge of the lower lobe was inflamed, like the lower lobe of the right. The left lung weighed five ounces; the right eleven ounces.

There was a small flake of yellow lymph between the adjacent edges of the lower and middle lobes of the right lung; but this was the only alteration of the pleura discoverable anywhere.

ABDOMEN.

Blood in Peritoneum. The peritoneal cavity contained about an ounce and a half of dark fluid blood, most of which was collected in the lower part

of the pelvis, and the remainder smeared over the surface of the liver and intestines. The peritoneum itself was not altered. No lymph or unnatural adhesions anywhere. The exterior of the intestines was stained in some places of a dark-brown, almost black color. In the left iliac region, the mesocolon had some dark ~~colly~~ collymosis between its layers of peritoneum.

Alimentary Canal. The cesophagus was natural. The stomach empty. The gastric mucous membrane was not remarkable except for some cadaveric softening, with brownish discoloration of the blood vessels in the cardiac extremity. It was very much corrugated. The small intestine contained a little orange-yellow pasty matter. Peyer's patches moderately well-developed. Solitary glandulæ, especially near the termination of ileum, very much so. Large intestine natural. Cæcal glandulæ much developed. The liver was, in every respect, natural. It weighed two pounds, eight and a half ounces. The gall bladder contained about an ounce of dark brown bile.

Fracture of Spleen. The spleen was of the natural size and adhesions. It had evidently been fractured, at no very distant period, in a transverse direction. There was a line running across its surface, when the peritoneal covering and the substance of the spleen itself had been torn apart, and the interval filled up with lymph. The torn edges of the peritoneum were tolerably straight and regular, and were separated from each other by an interval of half an inch. The lymph that filled up the fissure, projected a little above the surrounding surface of the organ, and overlapped the torn edge of the peritoneum for a line or two. The fracture extended completely across the body of the spleen. The "filling up" of lymph was, at one end, yellow and quite hard, but, for the most part, was of a moderately firm consistency, of a dull reddish color, and of a somewhat granulated texture. There were two or three ~~small~~ small cavities in its substance, containing old, dark-colored fluid blood. The ~~remaining~~ remaining portion of the spleen was of healthy appearance and consistency. Its peritoneal covering was generally smooth and shining.

The pancreas was natural. Kidneys ditto. The bladder contained a few ounces of clear urine. Its mucous membrane and other coats were healthy.

The abdominal aorta, iliac arteries and veins, and the abdominal vena cava were all healthy.

LEFT SHOULDER.

There was a nearly uniform, smooth tumefaction of the left shoulder, as during life, with little or no discoloration of the skin. It was most prominent on its posterior aspect, just above the axilla. On cutting through the skin and subcutaneous cellular and fatty tissue at this part, an abscess

was opened, from which a quantity of thick, yellow, normal-looking pus flowed out. From this opening, situated just at the posterior edge of the deltoid muscle, the abscess extended backward on to the dorsum of the scapula, underneath the fascia covering the infra-spinatus muscle. The walls of the abscess were here rather ill-defined, — not at all indurated, — and appeared to consist of fibrous tissue, of which the connecting cellular membrane had been destroyed, and replaced by pus, or soft yellow lymph. The infra-spinatus, and other muscles in the immediate neighborhood, were quite natural in color and consistency. From this part the abscess extended forward, and communicated with the shoulder-joint, the capsule of which was open on its outer and posterior aspect, and which contained pus, of the same quality as that elsewhere. From the cavity of the joint the abscess extended in various directions; backward, among the muscles filling the supra-spinous fossa of the scapula; inward, beneath and in front of the clavicle, quite half way to the sterno-clavicular articulation; two or three inches downward upon the side of the chest, beneath the great pectoral muscle; and a little over half way down the posterior and inner part of the humerus. The walls of the abscess were everywhere nearly similar in appearance, and the muscles in its neighborhood were everywhere red and firm. The whole amount of pus was estimated at an ounce and a half. No bone was exposed anywhere. The articulating surfaces of the humerus and glenoid cavity were absolutely healthy in appearance. The cartilages smooth, hard, yellowish-white in color, and of natural adhesion to the bone.

RIGHT WRIST.

There was much oedema of the right hand and fore-arm, as during life. On cutting through the integuments and fascia in front of the wrist there was found to be a collection of thick, yellow pus among the deep-seated muscles and tendons of the fore-arm and hand. The tendons themselves were white and hard, with only a very little vascularity here and there; hardly enough to attract notice. The general appearance of the pus, cavity, and walls of the abscess were the same here as in the left shoulder. The abscess extended up the fore-arm, among and beneath the deep muscles, nearly to the elbow-joint; down the palmar and dorsal aspects of the hand, nearly to the extremity of the metacarpus; and, by several openings in the anterior ligament of the wrist, communicated with the cavity of the joint; and, finally, also with the cavity of the articulation between the first and second rows of carpal bones. The articulating surfaces were natural, except

for a small spot on the lower end of the radius, where the cartilage appeared thinned, so that the red color of the bony surface showed through it.

The brachial, radial, and ulnar arteries were natural. The basilic, cephalic, and brachial veins were also natural, externally and internally.

LEFT THIGH.

There was hardly any tumefaction of the thigh, or infiltration of the tissues. The fractured extremities of the femur were shot past each other about an inch. The lower two inches of the upper fragment was destitute of periosteum, white, and hard; and, on being struck with the back of the knife, returned a sharp clicking sound, like ordinary dead bone. The end of the lower fragment was in the same condition for the extent of one inch. The cavity of the wound was not very extensive. It contained no pus, but only a little thin, dirty, greyish-yellow fluid. Its internal surface was covered with yellowish and ashy-colored lymph. The muscles and other soft parts of the thigh were everywhere of a healthy appearance.

The femoral artery and vein were natural.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

Nature in Disease. An Address delivered before the Norfolk, (Mass.) District Medical Society, at the Annual Meeting, May 12th, 1852, and printed in accordance with a vote of the Society.* By B. E. CORRING, M. D., of Roxbury.

We give insertion in our pages to the following address, because it seems to us well calculated to direct the thoughts of the reader in a direction too

* In yielding, reluctantly, to the vote of the Society and the solicitations of friends, it is hardly necessary to remark that no one estimates more highly the value of a thoroughly rational, scientific treatment of disease, than the author of this essay. Such treatment is not only highly beneficial, but all-important. It is the routine, unscientific, reasonless and unnecessary medication, overlooking the real nature and tendencies of disease, which he deprecates.

often lost sight of by the medical practitioner. In connection with the subject matter, and tenor of the address, the note appended by the author is to be considered. While it is important in the pursuit and practice of medicine to study and appreciate the natural tendencies of disease toward recovery, it is equally important to study and appreciate the operation of remedies in arresting diseases, abridging their duration, divesting them of their intensity, and disposing to a favorable termination. In proportion as we advance in our knowledge of the natural history of diseases, it is true that the instances become more and more numerous in which the active interference of the physician is shown to be unnecessary, and, if so, of course injurious; but the duty of interference, under certain circumstances, is not on that account rendered less imperative. Discrimination and judgment in withholding as well as wielding potent remedial agencies, are invested with more and more importance in proportion as our knowledge advances. True boldness in medical practice is frequently evinced in the resolution and persistency with which a "masterly inactivity" is displayed; and, on the other hand, it is conspicuous in the prompt efficient application of powerful medication. The "expectant method" is not blindly and uniformly adopted, more than heroic remedies are indiscriminately resorted to by the judicious practitioner. We make these few remarks that we may not, more than the author, be misconstrued by the publication in our journal of the following address. Regarded in a proper light, with due qualifications in their general application to medical practice, we conceive that the views submitted by Dr. Cotting may be read with profit, as well as interest.—EDITOR BUFFALO MED. JOURNAL.

Notwithstanding the rapid progress of medical science in these latter days, and the great advances the present has made over past ages in freeing our profession from the mysticisms which have ever enveloped it, it is still to be feared that too many of our fraternity set out upon their professional career indelibly impressed with Mr. Bagges's notion, that "disease is a certain noxious something, to be destroyed by medicine as an acid by an alkali;" and when, like Dr. Labell, they have treated their patients to "leeches, blisters, antimony, opium, ether, ipecac., colchicum — lotions, fomentations, and liniments"—they, like him, take good care to impress upon the convalescent that these medicines have cured the disease by putting a stop to it! Believing this themselves, they indoctrinate their patrons, and through them the public, with the same idea. But it must have early struck the attentive student, as it may now-a-days even the superficial observer, that under various and conflicting methods of treatment many diseases come to about the same general results—about the same relative number of recoveries and failures. For a longer or shorter period, the most diverse theories, as of Cullen, and Brown, of Broussais, and Rasori, and others of a lesser note, have claimed and held pre-eminence. During its reign, each has not only been considered

superlatively successful, but boasted its unrivaled cures. Under each, patients recovered in sufficient numbers to enable its followers to predict its universal adoption. That many also died, though drugged in strict accordance with the prevailing and supposed infallible theories, as well as under other methods of treatment, is sufficiently evident from the fact that these systems lost the confidence they once obtained, and now only remain in the memories of our older practitioners, or serve to amuse those whose curiosity leads them to search the records of past hypotheses. No system has now such unquestioned sway, as those of Cullen and Brown with our fathers. We are now in an unsettled state—in transition from hypothetical to more rational methods. The doctrine of “nature curing diseases,” so full of baneful influences on the practice of physic in the opinion of Cullen and his followers, has been stripped of most of its supposed dangers, by the present generation, and is again in the ascendancy. The present period is remarkably favorable for more extended and more correct observations in this regard, and it is to be hoped that it will not pass unimproved by the profession.

The science of therapeutics, though freed of many of its absurdities, has not yet made great positive advances when compared with other branches of medical knowledge. Nevertheless, the recent results of a more exact pathological anatomy, registered and counted, have not been without their salutary effect upon the treatment of diseases. Sixteen years since, Dr. Bowditch's translation of that incomparable work of Louis on Typhoid Fever, was distributed to the members of the Massachusetts Medical Society. Many a doubting glance was cast over its pages, and grave and respected elders were then heard to remark to each other and to the bystanders, “that it would be a disgrace to any New England physician to treat fever as recorded in that work.” The vigorous—to call it by no harsher name—the vigorous treatment then and previously pursued in this neighborhood for typhoid fever, had *done so much* that the expectant method, therein alluded to, seemed doing nothing indeed. Venesection, emetics, cathartics, blisters and mercury, the remnants of English heurics, stood in strange contrast with the milder trifles, the barley-mixture and gum-syrup of the French hospitals.

The previous year, Dr. Bigelow delivered his admirable discourse on self-limited diseases, before the same society. The doctrines of that discourse fell like an exploding bomb-shell into the camp of those who had taught their patients, and probably themselves believed, that they had *broken up* unnumbered cases of fever by a master-stroke in the commencement, or had cut short their triumphal progress by some wonderful exploit of professional strategy. Many went away sorrowful at the doctrine—some at such heresies in high places, and some fearful perhaps that if disease had not suffered at their hands, the patient certainly had. The right spirit, however, was awakened. Accurate investigations were made and recorded. Autopsies, rigorous and general, were instituted anew; and the result has been that an entirely new view of the history and pathology of typhoid fever has since prevailed. And, whether the redness and ulceration of Peyer's patches stand in the relation of cause and effect, or neither—a constant coincidence of these phenomena with this fever, and the increasing belief of its self-limited nature, have been sufficient to remodel the plans for treatment. This has been done so effectually, that it may now be doubted whether it would not be a disgrace to any one of us not to recognize the principles established by Louis, in our treatment of this and similar diseases.

Valuable as these advances have been, the practical inquirer has other and equally important questions to ask of the observer. Disease has been noted, registered, and counted, under various forms of treatment—what would its history and course be, if left to itself, under no treatment at all, without the administration of any drugs, with a view to cut short or even to mitigate its progress? For this question must receive a distinct and definite answer, from the observation of a sufficient number of cases, before the real value of any method of treatment can be truly estimated.

It may be said, and with truth, that this is a difficult question to decide—that single cases vary greatly in character—that the constitution and state of the patient are not the same, for any two individuals—that in its tendency, severity and complications, each case differs from every other. But all this does not alter the proposition. From such cases we are constantly proclaiming the value of certain remedies, and deducing plausible theories of treatment. Aye, but the experiment—who will be bold enough to try it? The sin of omission in practice is the unpardonable of offences. To have *tried everything that could be thought of* is the impregnable retreat of the baffled practitioner, and a balmy sedative to the bereaved. Nevertheless, until the benefits of the prescription over its omission be known, the administration of a drug is as great and as hazardous an experiment as the withholding of it. Who can say with truth that it is not even more dangerous? The popular reasoning, that “it will do no harm if it does no good,” may be sufficiently satisfactory to ignorant and officious bystanders, who seem sometimes to literally revel in an opportunity to crowd a patient's stomach with multifarious mixtures, and to load his person with offensive masses; but it will hardly bear the test of ordinary common sense. The suffering individual may prefer the trial at any risk, under the irksomeness of debility or the pangs of disease; but a compliance with his wishes, followed by recovery, is not proof positive that he has been benefitted thereby.

A violent fever sets in—you bleed the patient, and administer powerful drastics. In a few days he is well. Has the disease been *broken up*? Might he not have recovered equally well and speedily had he never seen you, or your supposed remedies? Cases of recovery under similar circumstances, without interference, are not infrequent. And until the question can be decided on a large scale—until the degree of probability in a given case can be shown from multitudes of observations, the value of your interference, for good or for evil, must remain uncertain and problematical.

Now, hundreds of cases of typhus fever have been submitted to the most thorough expectant or let-alone treatment; and it has been found that so far as duration of the disease is concerned, the results were quite favorable. Cases commencing with most violent symptoms of inflammation, delirium, &c., &c., have subsided, after a day or two, and convalescence been fully established in less than a week. It has been found that the natural duration of the disease is from three to nearly or quite one hundred days—some of the longer cases having commenced or terminated so gradually as to render precision to a day impossible, and the shorter ones resembling, as far as they ment, those which proved of longer continuance and dangerous severity. By far the largest number were convalescent in less than twenty days. In severity of daily and progressive symptoms, these cases compare favorably with equal numbers of others under the various and ordinary treatment of competent practitioners. In general results, these cases presented a decided

amount of recoveries over those in which an active, or heroic, treatment was employed.

We may not be able or willing to adopt such a course for an individual in private practice; for, as has well been remarked, "such treatment may do for armies, where one man is as good as another; but does not answer for individuals, by nature prone to over-estimate their personal consideration." Still, until the principles be established, by which the individual may securely have just that degree of treatment suited to his distress and danger, better than they now are, the results of such investigations must have a beneficial influence. Let every opportunity of observing a case of fever, undisturbed by drugs, be improved by each one of us. It cannot fail to add to our knowledge of the real nature of our disease, and, perhaps, may save some of our patients from unnecessary suffering; for, although some of us may be wandering amongst infinitesimals, the most of us in medicine, even now, like the rich in their wealth in Hesiod's time, "do not know how much better a half is than a whole."

The truly expectant plan has also been tried in the treatment of scarlet fever—in fewer cases, but with very similar results. This disease is admitted on all hands to be self-limited, and no one pretends to break it up. Yet there are indications from all quarters, especially from such observations as those alluded to, that even in this day of small doses, professional overdosing is a great obstacle to the speedy and perfect recovery from this complaint. These cases of too much interfered happen the more frequently, where the great anxiety of influential friends, stimulating the too ready attendant, exacts a multitude of appliances and a legion of remedies—that there may be abundant evidence of "doing something" for the victimized patient. How much the probabilities of recovery have thus been diminished; how many, if not fatal, at least severer sequelæ have thus been entailed upon the sufferer; how many broken constitutions, what impaired vitality, and greater susceptibility to noxious influences; how many weaknesses in protean forms have thus originated; how many a fatal termination has thus been directly induced, we may never know. We may, however, taking heed to such suggestions, be less anxious to invent new prescriptions and appliances, than to dispense with many now usual and popular, lest perchance it some time turn out to our mortification that the disease, in our day and generation, is really less formidable, as nature forms and develops it, than as modified and complicated by the ordinary interferences of art.

The natural history, progress and tendency of dysentery, if carefully re-investigated, would form no mean addition to our professional acquisitions. That this disease tends to recovery, and is actually recovered from, in sufficient number of cases to inspire confidence in the treatment, under all varieties of practice, from the most heroic drastics to the most imaginary doses—the treatment by opium and astringents not having warmer supporters than that by repeated potions of castor oil; nor these than that by billionths of a grain of corrosive sublimate—and that we so often hear practitioners complaining that it is so very "obstinate" or unyielding to remedies, this or that season, are sufficient indications that it is self-limited, and defends itself, as best it may, against excessive medical interference. That, as in typhus, scarlatina, and other exanthems, a person having experienced one attack of this disease is thereby protected against a second, though not so certainly

proved, is not improbable from recent observations. The subject throughout possesses unusual interest, and is deserving of attentive revision.

In 1835-6, Dr. James Jackson caused to be translated and published, Louis's work on "Bloodletting in some Inflammatory Diseases, and on the Influence of Tartarized Antimony and Vesication in Pneumonitis," and added thereunto his valuable collections of cases from the records of the Massachusetts General Hospital. He was induced to publish this work, he says, by the deep impression which Louis's results, so little in accordance with the general opinion, had made on his own mind. And he candidly admits, after re-examining the cases referred to, that "it would seem to be of less importance whether our patients were bled or not, than whether they entered the hospital early or late." That is, comfortable apartments and attentive nursing exercise a greater influence over this disease, than all the boasted powers of bloodletting then so universally relied on. Well might he add, that such results "will, no doubt, surprise many, if not most medical men." They did surprise the profession; and the treatment of pneumonia now is quite a different thing from the treatment of the same disease fifteen years ago. Whether venesection is now sufficiently employed in pneumonia, or not, is a question I cannot answer; but certain it is that the average of fatal cases treated without it, in this vicinity, does not exceed, but rather falls short of, that stated by Dr. Jackson for the cases so treated in the Mass. General Hospital. At the time of the publication of the work alluded to, it was the practice, in this section of the country at least, to administer antimony in pneumonia to constant nausea—to tolerance, so called. This was a very happy expedient for the routine practitioner—so simple a thing was it to mingle the drug in the customary proportion of water, and so satisfactory a matter was it to nurses and friends to find sweet solace in the frequent administration of the mixture. But the poor patient—who that has once seen can ever forget the involuntary shudder, nay, the inexpressible horror, when the repulsive draught was again and again offered? Nor was the evil always confined to the administration of the supposed remedy. "Redness, soreness, and even pustules were produced in the fauces," admits Dr. Jackson. Yes, and autopsies revealed pustules throughout the intestinal canal, even where tolerance had not been exceeded. I well remember the subdued undertone in which such facts were whispered about among the profession; and the trembling hesitancy with which antimony was subsequently administered by those whose faith in it could not be shaken, though they were ready to admit an unaccountable irritability of the mucous membrane in some idiosyncrasies. How much the patients unnecessarily suffered by this and other equally harsh medicines for this disease, will probably never be accurately estimated—how many were relieved of their distress, or restored, in consequence of such practice, will remain equally a subject of conjecture. One thing is certain, that many distinguished practitioners thought and taught that they effected "remarkable cures" by such a course of treatment. And another thing is now not less certain, from the testimony of most respectable members of the profession, who have watched, expecting to prove the contrary—that pneumonia, even in the severer forms, may pass, with perhaps equal certainty, through all its stages to perfect recovery, under the administration of infinitesimal atomies.

Perhaps no disease, in this vicinity, is more dreaded by parents, and practitioners also, than membranous croup. Certainly none requires more

assiduous attention, and offers less prospect of ultimate success. We now speak of the membranous disease, and not of those so-called spasmodic or catarrhal affections generally classed with it. These latter, though often violent and alarming in the outset, are comparatively harmless, and ought no longer to be called by the terrific name of croup, with which they have little or no affinity.

Sixteen years since it was taught, from the lips of undoubted authority, that "croup is death." Its great fatality, its great frequency in certain localities, and the insidious nature of its attack, have made it the subject of observation by many anxious inquirers, who, of late, have added much to our knowledge of its nature and history. It has been found that exudatory inflammations (affecting chiefly, but not exclusively, the larynx, trachea and bronchiæ) spread invariably from above downward, and not in an opposite direction; that if it commence in the trachea it may descend into the bronchiæ, but will not mount to the larynx; that with nursing children false membranes are not infrequent in the fauces only, and that the liability to descend into the larynx increases in proportion to the age of the child; that in adults, on the contrary, false membranes are, except in rare cases, chiefly confined to the smaller bronchiæ. It has been found, also, that the membrane itself is of a peculiar nature—a tissue of elastic fibres, longitudinally arranged; the fibres smooth, and in no degree transversely striated. Great elasticity is one of its chief characteristics. It is inorganic in its nature, or so much so that it never tends to organic union with the subjacent tissues. In proportion and as soon as the inflammation begins to abate, it separates, and, by irritating, causes itself to be thrown off. It may be re-formed a second, or even a third time. Though generally considered the result of a peculiar species of inflammation, it certainly obtains in other parts of the system, and moreover (from which we may learn a lesson of caution in our treatment) fatal exudations, similar in many if not in all respects, have been known to take place in previously healthy larynges from the accidental inhalation of caustic vapors.

It is believed, from careful investigation, that death is not oftener due to the obstruction of the membrane than to the weakened or paralyzed action of the muscles which open the glottis—though spasm seems to be most dreaded by attendants generally. And further, observation has shown that cases of undoubted recovery, with expulsion of the membrane, have taken place under treatment by calomel to excessive salivation, emetics to cruel barbarity, caustics to distressing peril, more frequently under the milder process of anodynes and watery vapor, sometimes under imaginary doses; and, lastly, without any medical treatment, real or pretended—so that it must be set down among the self-limited diseases, with a natural tendency, though feeble it may be, toward recovery.

These few diseases have been adduced, among others that might be cited, to illustrate the position assumed, and to indicate the kind of observations we would urge. Such observations any one of us may make. They are easier, and will be more serviceable to ourselves and the profession, than attempts to solve the mysteries of disease by pathological dissections. These, though more generally insisted on, and certainly never to be neglected, often require most skilful hands and the most patient examinations of the practiced, and the numbers of cases which only large cities can supply; but the other is forced upon us at the bedside of every patient. No one can over-estimate

the importance of correct knowledge on this subject. Without it we shall ever be uncertain as to the *real* value of any therapeutic interference. The fear of not doing enough may deter us; but we have seen how much the best physicians have formerly erred in their implicit reliance on powerful medicines to shorten disease and to restore health. And we know that the natural tendency to recovery under simple nursing, or under imaginary doses, is at least as great as under the formidable heroics of former times. "When I came upon the stage," wrote a few days since a venerated friend, who last year entered on his second half-century of active practice—"when I came upon the stage, whatever might be the differences of opinion about the nature or origin of the disease, there was none at all about the treatment: the first day an emetic, the second a cathartic—just as regular as the first and second bells for meeting on Sundays. Over and over again, during my pupillage, I have heard the patient say to my teacher, "O doctor, I know I ought to have sent to you before, but I did *so dread* to take an emetic!" And this dread of seeing the doctor for fear of an emetic, was founded on woful experience—the one was as sure as the other. And such doses—Lord save us! Nothing short of the indomitable spirit and power of that strong race could have carried the Pilgrim Fathers through their trials, or their descendants through their struggles with such Herculean medical practice."

Thus saith my friend—and at the present day may it not be that we are standing in a similar position toward those who may come fifty years after us; and this the more likely, as it is an occasional remark of continental visitors, abundantly qualified to make correct observations, and after sufficient experience and intercourse in the country, "that our people are martyrs to drugs and medicines—and this, too, at the hands of the profession."

If we ourselves are not able or willing to make the trial where we feel that experience has given a power to alleviate or to arrest, many of us, if so disposed, may turn to account the cases of our neighbors who honestly deal in infinitesimals. It were better for ourselves, and the science to which we are devoted, to avail ourselves of such opportunities than to waste our time and temper in empty cavilings against their vaunted, but, as we believe, baseless theory. If we need not the instruction ourselves, it is time the public were instructed by us in more correct notions of the nature of disease. So long as physicians teach their patients, directly or indirectly, or allow themselves to suppose, that diseases cannot be removed unless broken up by some masterly exploit, or amazing mystery of art, so long will the profession stand in a false position—so long will it be subject, as in times past, to violent alternations from formidable heroics to mystified trifling—so long will practitioners be doomed to have some of their sickest patients taken from them and placed at the critical moment in the hands of reckless adventurers; perchance to recover under treatment wholly inappropriate or totally inefficacious—so long, also, will medicine be ranked among the uncertain sciences, and its results be classed by intelligent laymen as the offspring of blind chance. With more frequent reference to the natural history of disease, physicians will adopt a less assuming and presumptuous bearing, which, while it serves to make the vulgar stare, brings grief into the hearts of the discriminating. The most celebrated of our profession, ever remarkable for their little reliance on the specific powers of medicine, and always noted for administering the smallest quantities and the mildest forms, have ever been distinguished for modest demeanor and a willingness to admit that they have been merely

careful attendants and watchful assistants, nature guiding at the bedside of the sick. Thus we hear an illustrious example of medical lore, after skillfully carrying a patient through a protracted and almost hopeless disease, modestly remarking that he had "visited the lady and the Lord had cured her." And we are not the less impressed to admiration with the renowned skill of that glorious veteran of military surgery, after a successful attendance on a chieftain horribly mangled in battle,

"Who wrote from Suza's blood-stained field,
'I dressed the wound that God has healed.'"

Here, too, in our own day and circle—those of us who were privileged to listen to the teachings of

"The truest, noblest, wisest, kindest, best,"

of physicians and men, will bear witness to the earnestness with which he deprecated the use of the word *cure* as a result of medical treatment, and the decision with which he excluded it from the Hospital records, adding that in its legitimate sense (to cure meaning to take care of) all such patients had been *cured*, though only a part had *recovered*.

If we read aright the signs of the times, this spirit prevails to a greater extent than ever before in the history of the profession, and is on the increase. It is of good omen—let us bid it God-speed. We need not fear the loss of position and influence by instructing the community in the true nature of our science. The want of such information, and the belief that each disease or symptom has its appropriate and infallible remedy, if the practitioner could only hit upon it, has been the source of infinite mischief—the foundation of professional huckstering, and of vulgar empiricism. The only remedy for such evils, widely felt and sufficiently deplored, is to be found in an earnest and persevering application to investigations such as we have advocated. Such investigations will raise the medical attendant far above the mere prescriber of drugs or the dealer-out of nostrums. They will open his mind to a nobler view of his calling, and give a loftier purpose to his mission. To responsibilities, greater than fall to the lot of other mortals, they will add the necessity of augmenting professional acquisitions by an enlarged knowledge of collateral sciences. To watch carefully, to study thoroughly, to guide cautiously, will become only the more imperative. Individual labors may thus be increased; but as such investigations are successfully pursued, and the knowledge of the real nature of diseases better known and promulgated, the relations between physician and patient will rest on a more rational basis, the profession will reach a higher elevation and take a firmer hold on the confidence of the people, than it has ever yet attained; and its members will be saved from the reproach now sometimes cast upon them, that they have been "ever learning, but never able to come to the knowledge of the truth."

Castor Bean Epidemic.—One of the steamboats running between Cincinnati and St. Louis, on a recent trip, was crowded with German emigrants. As might be expected, their appetite for fruit and vegetables, after a long sea voyage, was most voracious. At Selma, a short distance below St. Louis,

the boat received some fifteen or twenty sacks of castor beans. Their appearance excited the cravings of the emigrants. Finally, curiosity and appetite triumphed; a bag was surreptitiously opened, a large panfull extracted, and a huge luncheon of soup prepared. In a short time, the passengers in the cabin and the officers of the boat were startled by the report that the cholera, in its worst form, had broken out on deck: the castor oil was doing its work. The bag lay exposed, and a large potfull was steaming hot on the table. The captain was ordered into quarantine.—*Boston Med. and Surg. Jour.*

Report on Variola and Vaccination.—The committee appointed at the last meeting of the Medical Society of the State of Pennsylvania, to investigate the accuracy of certain views relative to small-pox and vaccination, recently put forth by Drs. Gregory, of London, and Cazenave, of Paris, and referred to in a communication made to the society at its last session, report:

That considering the high authority heretofore attached to the names mentioned, the opinions in question, if erroneous, are calculated to unsettle the views of physicians, and shake the confidence of the public in regard to the protective powers of vaccination, more than any promulgated since its adoption. The committee think these grounds sufficient to justify them in treating the subject with particular attention.

The principal points and questions calling for consideration, are:

1. Whether persons vaccinated, lose, through lapse of time, any of the protective power once afforded against small-pox?
2. Whether the prophylactic powers of vaccination performed during infancy are restricted to the first fifteen years of life and of no avail afterward?
3. Whether the accumulated evidence of the present day is calculated to sustain Dr. Gregory in his belief, that the efficacy of cow-pox as a protection against small-pox has diminished, and a large increase of small-pox resulted from the extension of vaccination?
4. Whether, as asserted by Drs. Gregory and Cazenave, inoculation after the fifteenth year of age, of persons previously vaccinated, produces a specific papular eruptive disease of a non-contagious character, unattended with danger, and giving protection in after life against small-pox?
5. Whether circumstances exist which render it most advantageous to substitute inoculation for vaccination, after the fifteenth year of age, as proposed by Dr. Gregory?

II. The morbid miasm, or agent productive of small-pox, seemed for a long while kept in check by the prophylactic power of vaccination, which, indeed, at one time promised the complete extermination of variola. But it cannot be disputed, that of late years variolous attacks have been common among those hitherto considered as completely protected. A new form of disease has, in fact, become known, designated "*varioloïd*," from its resemblance to variola or small-pox, of which it is generally regarded a milder form, as if modified and rendered less formidable, through some remaining prophylactic influence. This, of course, long before Dr. Gregory promulgated his peculiar views, furnished grounds for believing that the protection once relied upon

from vaccination, was diminished by lapse of time, or that the potency of the small-pox miasm had increased.

Dr. Gregory's views, when first promulgated in England, were well calculated to rouse the attention of the medical profession, and elicit inquiry. The Epidemiological Society of London, appointed a special committee to investigate the important subjects of vaccination and small-pox, and this committee has recently collected and placed before the public some highly important facts, through its chairman, Mr. Grainger. As the information thus derived is so highly valuable, and directly calculated to meet the points started by Dr. Gregory, the committee think they cannot do better than give a short abstract from Mr. Grainger's statements.

In the evidence brought forward by the committee of the Epidemiological Society, we have the results of the experience of a large number of medical practitioners in different parts of England, and it is interesting to find, that out of 430 replies to questions issued by the society, one, only, expresses any doubt of the protective power of small-pox; and this one doubt simply amounts to this: that having been inoculated during infancy, this gentleman felt himself more secure than if he had been vaccinated!

With regard to opinions founded upon observations prosecuted in hospital practice, the committee would remark, that the results are so often influenced by the existence, here and there, of modifying circumstances, that an appeal to the experience of any single one would certainly afford most incorrect data, on which to found important conclusions, as these should always rest upon multiplied facts, and observations extended through long periods.

In a table presented by Dr. Gregory, and published in his paper given in the *London Medical Times*, for 1849, we find the following statement of the results exhibited in the Small-pox Hospital, over which he presided:

	Total.	Deaths.	Per centage of Deaths.
Unprotected cases,	254	103	40
Vaccinated, with cicatrices,	365	38	10
" without cicatrices,	63	25	39
Total vaccinated,	428	63	14
Previously inoculated,	3	1	33

Now the rate of mortality here presented, is so much greater than that generally met with, in other institutions or in common practice, as to leave little doubt that the patients had been subjected to some of those malign influences, such as defective ventilation, &c., which have so often operated most injuriously in rendering mild cases severe, and originally severe ones almost inevitably fatal. If we compare the results exhibited in Dr. Gregory's Hospital practice, with those presented in 30 returns received from medical practitioners, by the London Epidemiological Society, taken without selection, we shall find the contrast most striking:

	Total.	Deaths.	Per centage of Deaths.
Natural small-pox in the unprotected,	1751	361	20.85
Small-pox after vaccination,	927	32	3.44

Previous to the introduction of vaccination, the annual mortality from small-pox amounted to 40,000 per annum, in the British Islands alone, being about

1-10th of all the deaths from every source. The average number of deaths per annum in London from small-pox, a century ago, namely, during a decennial period ending in 1750, was 2036; which presents a proportion strongly contrasted with the annual average of a decennial period ending in 1850, which is 498. This shows a mortality four times greater during a period when the population was not a fourth of what it was at the time last named.

Dr. Casper, of Berlin, shows in his statistics that the deaths from small-pox, in Berlin, during the eight years from 1814 to 1822, were 535 out of a general mortality of 51,389, being only 1 death from small-pox in 1000 from all diseases. This exhibits either an almost total absence of epidemic influence, or a very general diffusion of protective means. It is stated in a publication containing the regulations for medical and other officers, issued in Berlin, in October, 1803, that small-pox caused, on an average, 40,000 deaths a year in Prussia, in a population of about 10,000,000, during a period when inoculation was the only protection relied upon. In 1849, when the population had increased to more than 16,000,000, the average mortality from small-pox was 1760; showing that, during the first period, when inoculation was the sole reliance, the proportional mortality from small-pox was 37 times greater than when vaccination became generally diffused. These striking facts are, we think, very far from sustaining Dr. Gregory's opinion that an extension of vaccination has resulted in an increase of small-pox; nor do they offer any encouragement to those who would restore the former practice of inoculation.

The frequent occurrence, of late years, of small-pox after vaccination, with instances of mortality, have been much commented on, and occasioned no small alarm. Hence, the great value of such accurate information as the following, furnished in 356 replies sent by physicians to the Epidemiological Society.

Of these, 182 state expressly that *they have never seen a death* from small-pox after vaccination.

44 state their experience in numbers, and give an aggregate of 70 deaths.
127 give no statements of their experience.

From the same source we gather the results of the experience of 30 physicians on the respective degrees of mortality of natural small-pox, small-pox after small-pox, and small-pox after vaccination.

	Cases.	Deaths.	Per centage of deaths.
Natural small-pox,	1731	361	20.85
Small-pox after small-pox, . .	58	22	37.92
Small-pox after vaccination, . .	929	32	3.44

It is remarked, in reference to the 32 deaths reported after vaccination, that in seven cases the evidences of vaccination was not satisfactory, whilst in six other cases the deaths were owing to superadded diseases. Deducting the 13 deaths, the ratio of fatal cases occurring after vaccination would be scarcely two per cent., whereas that of small-pox after small-pox is nearly 38 per cent.

To these statements of results of very extensive experience abroad, we are glad to have it in our power to subjoin evidence equally conclusive as to the protective power of vaccination, obtained among our own practitioners. In the report on varioloid, the protective power of vaccination, &c., presented to

the College of Physicians of Philadelphia, in Nov. 1846, replies to interrogatories of the committee were received from 51 practicing physicians of the city and districts, who reported 776 cases of varioloid as having occurred in their practice during the epidemic of that period. Forty of these cases occurred after inoculation or a previous attack of small-pox in the natural way, and the remaining 736 after a reputed vaccination. Of the whole number of 776, cases, but 12 deaths occurred, or less than two per cent., and of these cases several were attended with serious complications. These cases all occurred in private practice, except two, which took place at the Small-pox Hospital, at Bush Hill.

It is worth noticing, among the evidence from abroad upon this subject, that Mr. Marsden, resident physician of the London Small-pox Hospital, has, within the last sixteen years, vaccinated no less than 40,000 persons, not one of whom had returned to the hospital with small-pox. Had there been any considerable number of the vaccinated attacked subsequently with small-pox, there is reason to believe that very many would have found their way to the institution which receive multitudes of patients from the same ranks in which the vaccination took place.

Statements made by Dr. Grainger, prepared from official returns received from all parts of England to the Poor-law Board, show a greater neglect of vaccination than could be well imagined to exist among civilized people. In London, 13 unions, exhibiting 21,598 births, report the number vaccinated at only 4641, or 21 per cent; whilst 31 unions in the country give only 9.2 per cent. vaccinations under the first year of life. In many others, the proportion of infants vaccinated in the first year of life is much less, being occasionally as low as one per cent. Whilst such is the sad case in a country boasting a national vaccine institution, and acts of Parliament for the promotion of vaccination, things seem to be even worse in Ireland. In a very valuable report, made by Mr. Wilson, of Dublin, contained in the report of the census of Ireland for 1841, it is stated that, of the 56,000 deaths from small-pox which occurred in that country in the decennial period from 1831 to 1841, no fewer than 79 per cent., or 45,824, were of children under five years of age. Dr. Gregory gives results for England very nearly the same. He states that of 9762 persons who died of small-pox in that country during the years 1837-38, the deaths under five years were 7340, or about 75 per cent. of the whole. If, as Dr. Gregory asserts, in his valuable lectures on eruptive fevers, the protective power of cow-pox may, for all practical purposes, be considered as complete, at least till the eighth year of life, the frightful infantile mortality here exhibited from small-pox, proves a neglect of vaccination almost equal to that which prevails to such a lamentable extent in Ireland.

In Prussia, Sweden, and some other countries, legislative authority has been brought into play with considerable efficiency in promoting the general extension of vaccination. But still, in despite of every precaution and exertion yet made, it would seem there are everywhere to be found thousands of unprotected persons, among the improvident, ready to become victims to small-pox whenever this may be introduced through epidemic or contagious influences.

In estimating the protective powers of vaccination, the public mind often seizes upon individual and isolated cases of death occurring after vaccination performed in childhood, without forming, at the same time, a just estimate of

the vast number of individuals who are thereby enjoying immunity from the ravages of variola. Persons are not given to reflect that such deaths constitute the *exception* to the general law of exemption, and that they happen only among a very few individuals peculiarly susceptible to the variolous poison. It is also highly probable that the limited class upon whom vaccination appears to exert little or no protective power, are rendered no more safe by inoculation or an attack of small-pox, as we find occasional instances of death from a second attack of genuine small-pox, even in persons who have had the disease so severely as to be extensively pitted.

As to the new form of eruptive disease asserted by Drs. Gregory and Cazenave, to be developed by inoculation performed upon those vaccinated previous to the fifteenth year, the committee has been prevented from testing its verity by actual experiments, penal laws existing against inoculating within the city and adjoining districts, embraced within the limits of the Board of Health. A few experiments have, however, been made during the past year by Dr. D. F. Condie, of Philadelphia, on persons situated beyond the jurisdiction referred to, the results of which were by no means calculated to sustain the views of Drs. Gregory and Cazenave. Although such limited experience cannot be regarded as furnishing evidence sufficiently conclusive upon the subject, we think it proper to place the results before the society.

Ten cases were experimented on by inserting variolous matter in the arms of individuals, six of whom had been previously successfully vaccinated by Dr. Condie, and of the successful vaccination of the other four he had the most unquestionable evidence.

In *three* of the cases, between seven and eight years had elapsed since the period of the vaccination.

In *five*, between thirteen and fourteen years.

In *two*, between fifteen and sixteen years.

In *one* case, a local variolous pock appeared upon the arm at the place of inoculation—attended between the eighth and ninth days with a pretty smart fever. The scab separated on the twentieth day, leaving a decided cicatrix. The remaining portion of surface was entirely free from any form of eruption. This individual had undergone successful vaccination seven years and two months previously.

In *four* cases, the local disease was attended with a general eruption of acuminated pocks—with hard base and slight areola—sparsely disseminated over the surface. In different cases from twenty to one hundred pocks appeared. In these cases the pustules on the arm and over the body were attended with a very slight fever about the fifth day—after this period they desiccated very rapidly, forming small, light-brown conical scabs, which commenced falling off on the eighth day, leaving no cicatrix. The periods which had elapsed since vaccination in these cases were, thirteen years in two, fifteen in one, and fifteen years seven months in another.

In *five* cases, a local inflammation, but no pustule, occurred at the part where the matter was inserted, which disappeared within four or six days, leaving no cicatrix. These cases were unattended with fever, or any form of cutaneous eruption. These patients had undergone vaccination seven years and five months, seven years and nine months, thirteen years and six months, and in two between fourteen and fifteen years.

These experiments were performed without the jurisdiction of the Board of Health, of Philadelphia, with the consent of the parties and their friends,

and with due precautions to prevent the individuals operated on from becoming foci of contagion.

It certainly appears strange that the poison of small-pox, which, when taken the natural way by persons previously vaccinated, produces the disease in its regular, pustular and contagious form, should, when introduced by inoculation into the systems of persons similarly situated, develop an entirely different form of disease, such as that described by Dr. Gregory as a specific papular eruptive affection of a non-contagious character, unattended with danger, and giving the most perfect protection in after-life against small-pox. Even supposing the result to be as stated by Dr. Gregory, the production of such a mild and benignant train of symptoms as those he describes from the introduction of the small-pox virus, affords one of the strongest evidences of the inestimable protective power exerted by cow-pox.

III. In regard to the fifth and last point of inquiry, your committee have no hesitation in expressing it as their belief, that no circumstances exist to justify the general substitution of inoculation after the fifteenth year of age, as proposed by Dr. Gregory. And they regret that at the present time, whilst strenuous efforts are making through individual exertion, occasionally helped forward by judicious legislation, statements calculated to lessen confidence in the protecting power of vaccination, should have been promulgated. Happily, however, abundant evidence exists to show that although the hopes of complete exemption from small-pox, once fondly indulged, have not been fully realized, vaccination still offers the only dependence for protection against a disease, the fearful ravages of which have tended so much to darken the pages of history previous to the precious discovery made by Jenner.

As the neglect of vaccination, especially among the poor and improvident, may, we think, be regarded as the principal cause operating to promote the extension and mortality of small-pox, the committee would urge it upon the State Medical Society to continue their efforts to obtain from the legislature the passage of a law providing for the gratuitous vaccination of the poor, and calculated to secure, as far as practicable, the fullest extension of vaccination in every portion of the commonwealth.

G. EMERSON,
SAMUEL JACKSON,
JOSEPH WARRINGTON,
ISAAC PARRISH,
JOHN D. GRISCOM.

Trans. Med. Society of State of Penn., 1852.

Cholera in Rochester during the Summer of 1852. — From a "Report of the Board of Health on Cholera as it appeared in Rochester, New York, in 1852," we take the following extract, which gives an account of the successive occurrence of cases, the locations and their sanitary conditions, the number of cases during each month, fatality, etc. The circumstances connected with the rise and progress of this epidemic in different places should be placed on record with a view to the accumulation of facts which may

prove useful in future investigations of the causation of the disease. — Ed.
BUFFALO MED. JOURNAL.

Cholera—Its History and Progress.—Having described the sanitary condition of the city prior to its appearance, and during the first few cases of cholera, and the means provided for the relief of the sick, the Board proceeds to give as detailed and accurate an account of the epidemic from its commencement to its termination, as the data in its possession will permit—pre-mising, that many of the reports of physicians were very defective in the information communicated—and not a few of them, it is feared, not altogether accurate in their details.

On the 6th of June, Dr. Swinburne was called to prescribe for John Hart, an Irish laborer, residing in an old house on Factory street. The man had bilious vomiting and purging. About 9 o'clock in the evening of the following day, June 7th, his attention was called to the patient's daughter, Margaret Hart, aged 10 years—who had been seized with vomiting and purging some hours before—and on examining her, she was found pulseless, eyes sunken, extremities cold—skin of a leaden hue—discharges, watery; and the startling fact was apparent, that she had cholera, and was then in a state of hopeless collapse. She died at 4 o'clock on the following morning, June 8.

This case was reported to a member of this Board, who, in company with Dr. Swinburne, on the following day, June 9, examined the premises. The house was an old rookery—without a cellar, and presented inside the usual appearance of the residences of the poorer classes of laborers with the usual smell—but nothing very remarkable—the father still lying sick in a small illy-ventilated room adjoining the kitchen, which also answered the purpose of a dining-room, and probably of a dormitory also at night. The yard in rear of the house presented nothing objectionable—it was in good condition, but at the west end of the house a small pile of rubbish was discovered, and almost adjoining the house, a small pen, containing two small pigs. The condition of things outside the house would bear a very favorable comparison with that inside. In front and near the door, was an opening into the main sewer—into which the slops were thrown—but the water was seen running clear, with a free current—and no smell was discernable from the source. The premises were again inspected a few days afterward, but nothing new was discovered. The health inspectors were in the mean time put on the alert.

The next case that presented itself, was that of James Ward, aged 21 years, who sickened and died in the old "Factory Block," June 16; and three days afterward, his sister, Catharine Ward, aged 38 years, who occupied the same apartments, died also. On the following day, June 20, Patrick Enright, residing in the same block, sickened, but recovered, June 22. John Ferguson, aged 20 years, sickened and died on the 23d. Mary Dooley, in the same block, also, sickened the same day, and recovered. In the night of the 24th, William Cashmere, an English laborer, residing in the same block, sickened, and died on the 26th; and his son, aged five years, died on the following night. These were all the cases reported to the Board from the 8th to 26th June, inclusive: five in the Factory Block, and one in the old house, first named, and separated from the former only by a narrow court-yard.

The above deaths—the first on the 8th, and the others between the 16th and 26th June—occurring all in one locality, impressed the Board with a deep conviction, that there must be some great defect in the sanitary arrangements on the premises. Accordingly, a thorough search was made, but nothing was discovered to account for so great a mortality, except it were overcrowding; and on a careful inquiry made in the premises, on the 22d June, it was ascertained, that there were in the “Old Factory Block,” properly so called,

Adults,	31
Children,	25
	———56
And in the block adjoining, on Mill and Factory streets,	
Adults,	12
Children,	14—26
	———
Amounting in all, including old and young, to	82

individuals, living in close, illy-ventilated rooms, and as a necessary consequence, not kept in the best condition: but as a general thing, the premises outside the buildings, with the exception under Higgins' Block, already mentioned, were not in bad condition. But all the buildings were old and decayed, and as might well be supposed, even in a condition to produce the poison which proved so fatal in its effects.

On the 27th and 28th June, two children, Ellen and Alice Hickey, aged two and four years, died on Cayuga street. These make up eight cases in all, which occurred in the month of June—the same number mentioned by J. Moran, in his report to the Mayor, viz: June 15th, J. Ward, aged 24 years, State street; Mary Moran, aged 20 years, State street; John Ward, aged 24, State street; June 23, D. Ferguson, Frank street, aged 28; June 24, Patrick McGuire, aged 18, Fish street; June 28, J. O'Mera, aged 2 years, Cayuga street; and two children of Mr. Tuckey, on the same street, aged 4 and 6 years. It will be noticed, that the name of J. Ward is mentioned twice, while that of Margaret Hart, who died on the 8th, and those of William and Solomon Cashmere, are omitted altogether. D. is substituted for John Ferguson, whose residence is put down as Frank street; and the name of Mary Moran, was probably intended for that of Catharine Ward. It is singular enough, that so many mistakes should occur in recording the names of only eight persons—the ages of them being certainly wrong—the streets also wrong; and we have a right to infer, that the others are wrong also. The errors, which are known to be so, should at least put us on our guard, and should warn us not to place too high an estimate on the remainder of this and other similar reports—the propriety of which will be more clearly seen hereafter.

On the 29th of June, John Hickey, the father of Ellen and Alice Hickey, sickened, and died, 3d July. He was very intemperate; was absent from home from the 26th to 29th June; said he ate nothing while absent; slept out doors, and returned home, sick. It is reasonable to suppose, that his family was poorly provided for, and that the children were poorly fed. No other cause was known for the attack. The only thing discovered about the premises that could affect the health unfavorably, was an open spring, and a drain leading from it in one corner of the garden, a few feet from the house.

The ground, aside from this, was dry, loomy, and clean; cellar, dry; and the house in tolerable condition, for that of a drunken man. John Hoolihan, a boarder in the next house, said to be of good habits, died the same day; no cause for the attack being known.

No other cases are known to have occurred until the 9th and 10th of July, when two fatal cases took place on Furnace street; the mother, 35, and one child, 5 years old; the former leaving behind her an infant of three weeks, which still survives. Another child belonging to the same family, aged 3 years, sickened on the 13th, and recovered.

On the 18th, several fatal cases occurred on an alley opposite Gorham street, east side of the river; and also on Mill street, west side; and soon after, several fatal cases occurred in Taylor's Block, on Fish street—one in Lester's Block, on State street; three in Congress Hall, and one in the Jail. Fatal cases also occurred on State, Buffalo, Furnace, Mumford, Front, Water, Exchange, Court, Delevan, and Clay streets; and also on Brown, Pine, and Pindle alleys. During this time, until the 30th, inclusive, from one to two or three deaths occurred daily in the localities above named.

On the 31st July, about fifteen new cases occurred—nearly all of which proved fatal. Seven of these fatal cases occurred in Lester's Block, on State street; and on the following day, six other fatal cases occurred in the same block. The utmost consternation and alarm were produced by this sudden increase of the epidemic, not only in the block itself and its immediate neighborhood, but throughout the city. The block was very soon emptied of the principal part of its fear-stricken occupants, who fled to other parts of the city, and to the country, for safety; and several were overtaken in their flight, or after their arrival at their supposed places of safety, and fell the unhappy victims of their ruthless pursuer. In the course of one short week, twenty-five individuals, old and young, who had tenanted this fatal block, became the tenants, each, of a peaceful grave—two whole families having been nearly swept away; out of fourteen belonging to these families, only four survive—each family having lost five of its members, the parents of each being included.

In no other locality did the epidemic show itself with such perfect violence—but it was manifest, that it had assumed an increased degree of potency in other parts of the city—and the number of fatal cases increased from two or three, to ten or twelve each day. This, however, continued only a very few days—the number in the course of two weeks being reduced to less than one-half, and varying from 3 or 4 to 7; rarely after the 4th of August, exceeding the last number.

On the 2d of August, Moses B. Seward, Esq., a gentleman highly esteemed and respected, fell a victim to the scourge; and on the 6th was followed by Dr. J. J. Treat, whose zeal to relieve the sufferings of those who relied upon his skill, in the hour of their extremity, carried him beyond the just limits of prudence; and, heedless of his own safety, he sacrificed himself in endeavoring to relieve others.

On the 25th, Dr. William Bell, long known here as a physician, also fell a victim to the fell destroyer. His health had been long impaired, and for several days prior to the fatal attack, he had suffered from diarrhœa, which he had either neglected, or for the cure of which he had relied too explicitly on a supposed specific.

On the 15th of August, the epidemic broke out in Sherman's Block, on

Ford street; and in the course of twelve days, ten individuals became its victims.

During the first four days in August, 35 deaths were reported to the Board; and during the next four days, 28 more, or 63 deaths in eight days; averaging nine deaths per day. During the next eight days, 43 more deaths were reported—the whole number for the first sixteen days being 106. During the next fifteen days, 63 more deaths were reported—the whole number for the month being 169.

During the first fifteen days in September, 70; and during the remainder fifteen, 23 more deaths were reported; amounting to 93 during the month. They occurred on the streets already named, and also on Romain, Clinton, Jackson, St. Joseph, Riley, Union, Court, Adams, Jay, Martin, Strong, Bay, and other streets—showing very clearly, that the epidemic cause, or influence, or constitution of the air, had reached every part of the city; and the fact, that a considerable number of cases had appeared, not only in the remote parts of the city, but in the neighboring country around for several miles in extent, and among individuals, who had had no communication with the city, shows beyond all doubt, that it had become pretty generally diffused apparently without regard to any known local causes.

On the 22d of this month, Dr. D. C. Phelps, who resided on Buffalo street, and who had been a resident of the city for about three years, fell a victim to the epidemic. He was an active young man, a native of Ireland—belonged to the Eclectic School, and was thus early cut off, almost in the outset of his professional career.

During the early part, and up to the 18th of October, thirteen deaths took place; among which were those of George F. Martin, a native of England, a man of the most temperate and correct habits, who resided on King street—one of the most cleanly in the city—and his daughter, seven years of age—both of whom died on the 5th, after a few hours' illness. The daughter was first attacked, without any known cause. The attack of the father was apparently produced by fear and excitement, consequent upon seeing the hopeless condition of a beloved child. Neither of them had been guilty of any dietetic imprudence. The house and premises were clean, and in good condition; the cellar, dry and free from smell. Nothing could be discovered in or about the premises, likely to deteriorate the health, or cause the attack. But it was ascertained, on inquiry, that a drain from the cellar communicated with the sewer on the street—one of the tributaries to the Platt street sewer—and although no smell could be discovered coming through this drain, it is nevertheless possible that the poison may have found its way through this avenue. Five weeks previously, a fatal case, that of Mr. Osborne, had occurred in the next house, on the same street.

On the first of November, a person was brought to the jail, who had been unwell for several days—and shortly after died of cholera. A fellow-prisoner, in feeble health, 83 years of age, on being made acquainted with the fact, became panic-struck, and was immediately seized with cholera, and died also. These are the last deaths which are known to have taken place in the city—the last case being in singular contrast with that of the first—a child of ten years.

Owing to the causes already enumerated, it is impossible to state with entire accuracy, the number of deaths which have occurred from cholera. Much

pains have been taken and much time expended to ascertain the exact number; but an approximation to correctness is all that has been accomplished. To the list reported by physicians, have been added three cases which were well ascertained from reliable health officers, and from personal inquiries among surviving relatives and friends; and a careful comparison has been made with the reports made to the Mayor by the city sexton or undertakers.

The statistics, showing the number of deaths in each of the months, have already, in part, been anticipated. But those already shown were then only reported to the Board. That the entire subject—not the least interesting—may be the more easily and fully comprehended, the whole, although some repetition may become necessary, will be presented in a single view, and in different aspects.

During the month of June, that is, from the 8th to the 28th, eight deaths were reported, and one has since been ascertained—say,

From the 3d to the 30th of July, inclusive, the number of deaths, was	61
Ditto, on the 31st July,	17— 78
The number of deaths on the 1st August, was 12, and during 1st week,	66
Do. 2d do.,	33
Do. 3d do.,	32
And during the remaining ten days,	49—180
During the first half of Sept., the number of deaths was, .	112
“ 2d half do. do. do.	26—138
During the month of October, there were 13, and in Nov., 2 deaths,	15
The whole number of deaths being,	420

It will be seen that the greatest number of deaths, on any single day, occurred on the 31st July, viz., 17; and on the following day, 12; averaging $14\frac{1}{2}$ for each day.

The average number of deaths for July, was a fraction over $2\frac{1}{2}$; that for August, 5.8, or a portion over $5\frac{3}{4}$; and for September, 4.60, or a little over four and a half.

Veratrum Viride, or American Hellebore. By W. C. NORWOOD, M. D., of Cokesbury, South Carolina.

The past numbers of this Journal have contained several articles by Dr. Norwood, on the sedative influence of the *veratrum viride*, and its successful adaptation to the treatment of a variety of affections accompanied by febrile movement, or marked excitement of the circulation. In the January number of the Southern Med. and Surg. Journal, we find a long article on the subject, giving reports of numerous cases in which its remedial action appeared to be signally successful. We select a portion of this article, containing

an account of the properties of the drug, the mode of preparation for medicinal use, etc. The testimony made to the value of the remedy is amply sufficient to commend it to the attention of the profession.—EDITOR BUFFALO MEDICAL JOURNAL.

Veratrum viride, green hellebore, American hellebore, is not our common poke-root or *Phytolacca Decandra*, but is the poke-weed, veratrum viride, and is entirely different in its appearance and properties. Again—it is called white hellebore, by the Shakers, and those ordering the veratrum viride often get the white hellebore proper, or European, for it, by not being specific in the correction of the error in name. The properties and powers of veratrum viride are the following:

1st, acrid—This property is very limited and confined to the fauces.

2d. It is adanagic, deobstruent or alterative: this property it possesses in a marked and very high degree; not equalled by calomel or iodine in this particular, which will adapt it to the relief and cure of many diseases hitherto beyond the reach of any remedy. Of this class of diseases, those which we think will be much benefitted by it, are, cancer and consumption.

3d. It is actively and decidedly expectorant, so much so that we rarely add any other article.

4th. It is one of the most certain diaphoretics belonging to the materia medica: it often excites great coolness or coldness of the surface; in some cases the skin is rendered merely soft and moist; in other instances, the perspiration is free, and at other times it is most abundant; but, notwithstanding its profuseness, it does not reduce or exhaust the system, as many diaphoretics do when in excess, and therefore need not excite alarm nor be suspended on that account.

5th. It is nervine, not narcotic, under any circumstances; as since our first article, we have taken it more than twenty times to test its varied powers, and we have taken it in all quantities, from the production of free emesis down to the minimum dose. This property renders it of great value in the treatment of painful diseases and such as are accompanied with convulsions, morbid irritability and irritative mobility. For example—pneumonia, rheumatism, puerperal fever, convulsions generally, and palpitation of the heart, &c.

6th. It is one of the most certain and efficient emetics known, and is peculiarly adapted to meet that indication in hooping cough, asthma, croup, scarlet fever, and in all cases where there is much febrile and inflammatory action. It often excites severe nausea and frequent vomiting, which, taken in connection with great paleness, often alarms the patient and by-standers; but these effects, when in excess, are readily relieved by one or two full portions of morphine and tinct. of ginger, or of laudanum and brandy. One grand and leading feature is, that the exhaustion which follows it, is not excessive and permanent, but confined merely to the effort. Again, the matter first ejected, is a large quantity of thick, slimy mucus, and soon after, the liver is called on to pour forth its own fluid in abundance.

7th. The seventh property is its most valuable and interesting, and for which it stands unparalleled and unequalled as a therapeutic agent. So much has already been written on what we call the sedative—arterial sedative—properties of the agent, or the power it possesses of controlling and regulating arterial action, that we shall not again run over the amount of evidence on

this part of the subject. By virtue of this and other powers, the treatment of disease has been much simplified, and when the effects recorded in the case of Mr. G.'s negro woman, shall have been fully considered, we may bid adieu to much of the supposed necessity for stimulants in the treatment of atonic or asthenic cases. We challenge the medical world to produce its equal, as a therapeutic agent, for certainty of effect, for extent of effect, or for peculiarity of effect, and the ease and safety with which it may be administered to small and great. In small portions, we have found nothing to equal it in exciting and promoting appetite.

The formula we use is the following:

℞ Root of *veratrum viride*, dried, ℥viii.
Alcohol, of the shops, undiluted, ℥xvi.

Let it stand from ten days to two weeks. Medium dose for an adult male, eight drops, to be increased one or two drops every portion, until nausea or vomiting, or a reduction in the frequency of the pulse takes place; then reduce one-half in all cases. Females, and persons from fourteen to eighteen years of age, should commence with six drops, and increase as above. Children, from one to two years of age, to commence with one drop; from two to five years of age, two drops, and increase one drop. The usual interval with us is three hours between the portions. In ordinary cases of pneumonia, we usually continue it three days after the symptoms are subsided. In typhoid fever, and many other diseases, it requires to be continued much longer. For the satisfaction and information of the profession, we would state that it may be continued indefinitely, or any length of time, in moderate doses, or short of nausea, without the least inconvenience. The only objection that could be urged, is the increase of appetite, or desire for food. It is not cathartic—it is like all other remedial agents, subject to the same rules and regulations, making it out of the question for a person to lay down any but general directions for regulating the dose. We are better pleased with the method adopted for getting its first impression by Dr. Welburn, of Farmville, Alabama, than with our own. We allude to the short interval between the first three portions he administers: He gives "six drops; in ten minutes, seven drops; in ten minutes more, eight or ten drops; and then suspends the dose till vomiting occurs," which will be sure to take place in a large majority of cases. In the outset of many cases, we would recommend Dr. Welburn's manner of using it. In a male, twenty-five drops is the largest quantity we have known to be required to excite emesis, and sixteen drops in the female when given in the manner and at the intervals we have directed. There need be no danger apprehended of its exciting inflammation of the stomach—we have given special attention to that particular. It is peculiar and at the same time interesting in its effects. The fact of its acting as a sedative on almost every other portion of the system, diminishing the vascular and muscular action and motion of every other part, and increasing that of the stomach. We have seen it produce emesis in very susceptible persons, and the contractions of the stomach were so rapid as to be almost continuous and uninterrupted; but a strong alcoholic tincture of ginger and morphine would afford more prompt and immediate relief than any other article that we have ever used. We have never seen a case that failed to be relieved by the above remedies in thirty minutes. The great advantage of the remedy

is that it does not exhaust longer than the effort to vomit is concerned. A great many remedies leave the patient in an exhausted and enfeebled condition, aside from the effort or immediate action—not so with the *veratrum viride*. Again, tartar emetic should never be given with it, in any form or manner. The only cases in which we have seen the tincture of *veratrum viride* purge, were when given in combination with tartar emetic, or with Coxe's hive syrup. In most of these cases it excited a violent cholera-morbus. We would not think of giving the tincture of *veratrum viride* where tartar emetic had been used, without preceding it with a full dose of morphine or laudanum at least one hour. We have known many fall out with the *veratrum viride* when it was not at fault. Again, venesection, when a large quantity of blood is drawn, increases materially its effects, whereas opium and morphine lessens or diminishes them. If a patient had been bled freely, preceded or followed by a liberal use of tartar emetic, and then followed up with medium portions of the tincture of *veratrum viride*, we should anticipate and prepare for drastic, if not hazardous effects.

Blood Changes in Disease.— A recent elaborate memoir on diseases of the blood, presented to the Academy of Sciences, by MM. Becquerel and Rodier, winds up with the following conclusions:

1. In most chronic diseases, or in consequence of various hygienic influences, the three principal elements of the blood, viz., the red particles, the fibrin, and the albumen, may diminish or augment in quantity, separately or conjointly.

2. The red particles diminish in most prolonged chronic diseases, especially in organic diseases of the heart, in Bright's disease, chlorosis, paludal cachexia, hæmorrhages, the last period of tuberculosis, the cancerous diathesis; the globules diminish also when the individual has been insufficiently nourished or exposed to bad hygienic conditions, such as insufficiency of air, want of light, or to humidity.

3. The albumen diminishes in the last stage of heart diseases, great symptomatic anæmia, cancerous diathesis, and when the nourishment is insufficient.

4. The fibrin remains normal, or is sometimes augmented in acute scurvy. In chronic scurvy, and in the symptomatic scorbutic state, which attends some chronic maladies, especially in organic diseases of the heart, it diminishes.

5. In all the above cited cases the water of the blood increases in amount.

6. The diminution of globules is shown especially by decoloration of the skin, palpitations, dyspnoea, systolic murmur at the base of the heart, arterial and venal murmurs.

7. The diminution of albumen produces dropsy; speaking generally, dropsy is the symptom of diminution of albumen.

8. The diminution of fibrin shows itself by cutaneous and mucous hæmorrhages.

9. In the symptomatic anæmia of great hæmorrhages, of bad nourishment, and of too great catamenial flow, the specific gravity of the blood diminishes, the water augments, the blood globules diminish; the albumen remains normal, or slightly diminishes; the fibrin remains the same.

10. In chlorosis, which is an entirely different disease from anæmia, the

blood may be completely normal. When any changes occur, they are in the augmentation of the water, the diminution of the red particles, and the conservation of its normal figure or the augmentation of the fibrin.

11. In acute Bright's disease, the fibrin remains the same, the albumen diminishes; in chronic Bright's disease, the globules and albumen diminish; the fibrin remains normal or diminishes.

12. Most dropsies, called essential, are due to a diminution of the albumen.

13. In heart disease, the blood deteriorates more and more as the disease advances. The changes consist in simultaneous diminution of the red globules, albumen, and fibrin.

14. In acute scurvy, the blood is not altered; in chronic scurvy, the fibrin is diminished, and the globules augmented.

15. Quinine and generous diet are the best treatment for diminution of the fibrin; iron for the diminution of the red particles.—*Phil. Med. Jour.*

Additional Remarks upon the Treatment of Dysentery. By E. F. STARR, M. D., of Rome, Ga.

In a former number, I offered a few remarks upon dysentery, and the use of large doses of opium in its treatment. I now propose to direct attention to a plan of treatment which differs somewhat from that in general use. I do this, because I believe there is some room for improvement, and that the want of success is not to be charged altogether to a fault in the resources of the *materia medica*. It is a fact worthy of some notice, that, of late years, while medicine has been triumphing in so remarkable a manner over some forms of disease, dysentery has not been shorn of its terrors, and its treatment now is little more successful than it was many years ago. As we have certainly not arrived at a point beyond which we may not advance, we are justifiable in casting around us to see if there be not means as efficient for the relief of other diseases as quinine is for the cure of autumnal fevers. Is there no reliable remedy for dysentery?

If I am told there is a vast weight of authority in favor of the calomel treatment, and that it has been the established practice for scores of years, I appeal to the number of deaths by the disease at the present day. Comparatively but a few years ago, calomel was considered the great remedy for autumnal fevers; yet the physician who knows no better now is far behind the times. I esteem calomel very highly as a remedy; nor is my design so much to wage war against it, as to place it in the back ground, compared with opium, in the treatment of dysentery. I am disposed to "retain the mastodon in harness," and to make it useful by judicious and cautious administration whenever necessary, which will rarely be the case in dysentery. A patient with sound bowels may, perhaps, take a full purgative dose of calomel with impunity, but that one with inflamed intestines can do so with entire safety admits of much doubt. Unfortunately, from the word bile has been derived bilious, and from this, bilious attacks, and bilious fever, and bilious colic, and bilious dysentery, and bilious every thing else to which the term could be applied; and to this prevalence of the *bilious idea* is to be attributed the belief, that to cure these affections there is little more

necessity than to purge out the bile and "regulate the secretions," with calomel, of course.

Dysentery is a primary inflammation of the parts ostensibly affected, and it is not to be expected that it can be promptly and certainly cured, without directing attention to the condition of the diseased locality; nor need we hope to obtain success in the use of an irritating and motor-exciting treatment: for an inflamed organ needs rest, and to place it in the most favorable condition for a restoration to health, it must have rest. The hazard to which a little untimely exertion may subject a patient suffering with acute inflammation of any important organ is well known—how certainly then must undue action in an organ, itself inflamed, produce injurious effects. The value of opium, therefore, and the danger of a different class of agents, must be apparent under this view of the subject.

With regard to the use of cathartics, I would not be misunderstood: they are sometimes useful and necessary, but should, in such cases, be cautiously used, and, in general, be combined with opiates. I think there is often too much anxiety among practitioners upon the subject of *scybalæ*, and too great a passion for producing discharges of fecal matter; yet I would not produce the impression that the contents of the bowels are to be left entirely without attention, but, that they are of little importance compared with the *intrinsic features* of the disease. In confirmation of this view, I will quote a fact from Dr. Brandon's article in the March No. of this Journal. In describing some violent cases, which came under his notice, he says, that he "seldom observed fecal matter in the discharges until a change for the better had occurred." The improvement, then, could not have been produced by the feculent discharges, but these must have been the result of an amelioration of the disease—showing that the disease may be first subdued by opium and other remedies, and the bowels afterward emptied more safely.

In the treatment of dysentery, our object should be to effect, promptly, the cessation of spasm and pain, and the reduction of inflammatory and febrile action. These indications are all under the control of the therapeutic properties of opium, to a greater or less extent; and if these indications and these properties are properly weighed and considered, in connection with each other, the tendency will be toward the establishment of the treatment which I am endeavoring to urge upon the attention of those who may notice these suggestions. The reason why so little reliance has been placed in opium, is, that it has not been given in sufficient quantities—in doses large enough to overcome the force of disease, and to produce its legitimate and peculiar antiphlogistic and antifebrile effects. There is no confidence to be placed in an ordinary or medium dose of opium when the patient is suffering the effects of violent inflammatory action, the tortures of pain, or the depressing adynamic influence of malignant disease. The dose must be proportionate to the emergency of the case. I suggested from two to four grains, but this should not be considered the limit; this quantity is rather the minimum than the maximum—circumstances must determine the precise amount. In dysentery, if the pain, fever, and flux persist, they are sufficient evidence that enough has not been given—six grains are not too much, in such cases. The antiphlogistic virtues of opium seem generally to be imperfectly known or understood, or if known, not appropriated and applied. All agree in admitting its usefulness as an anodyne, as a soother of pain and promoter of sleep, *etc.*; but who administers it with a view of overcoming fever, or who looks

to it principally to subdue some severe forms of inflammation. Yet, what diaphoretic will produce such certain and general opening of the pores and genial moisture of the surface?—what will so equalize the circulation?—what so control the heart and arteries? and what afford such suspension of pain, thereby breaking the *chain* of the morbid actions of inflammation? Fever and inflammation cannot well persist under such circumstances—under the effects of full doses of opium.

To carry out more effectually the suggestion above made, in relation to the indications of treatment, it may be often proper to resort to one efficient bloodletting, in cases where there is much fever and no want of strength. This will render the system more susceptible to the favorable influence of opium, which now, if properly administered, will never fail to mitigate, and seldom to relieve entirely, the sufferings of the patient. When this is done, the use of opium is not to exclude other substances as auxiliaries; such, for instance as calomel or oil, when they are needed, or sugar of lead and other astringents, when, after the subsidence of the inflammatory symptoms, the discharges remain too frequent and watery. These, with fomentations, blisters, enemata of watery solution of opium and starch, &c., may be resorted to; but opium in large doses, given either by the mouth or rectum, in the early stage of the disease, should be the leading remedy and chief reliance.—*Southern Med. and Surg. Journal.*

Quinine in Typhus and Typhoid Fevers.—On this subject, Prof. Bennett read a paper to the Medico-Chirurgical Society, on Wednesday evening. It appeared, that about the month of November last, Professors Bennett and Christison had received communications from Dr. Dundas, of Liverpool, mentioning the beneficial results he had found to follow the exhibition of quinine in the continued fevers of warm climates. So strong were the representations of Dr. Dundas, that Prof. Bennett, whose period of clinical duty in the Infirmary had at that time just commenced, determined to give the remedy a fair trial. The doses Dr. Dundas recommended were ten grains every second hour. Dr. Bennett had given quinine in seven out of fourteen cases of typhus or typhoid fever admitted into the Royal Infirmary during the period from November to the end of February. In all seven, the physiological effects of the remedy had been produced. In most of the cases, the seventh was the day of the disease on which the treatment of quinine was began, in some it was later, one patient, however, had it on the sixth day, and none later than the tenth. In one case, 205 grains of quinine had been taken in eighteen doses. In five others, about 80 grains had been taken. Dr. Bennett had in none of these instances found quinine to cut short the disease, or in any way favorably to influence its progress. In one case, convalescence had been delayed till the forty-second day. At the same time, Dr. Bennett thought it proper, in bringing the subject before the society, to mention, that the experience of some other physicians had been more favorable. Dr. Graves, of Dublin, for example, had written to Dr. Dundas to that effect, and so had Dr. Kelly, of Drogheda, who had treated eight cases with the happiest results. Prof. Christison's experience had as yet been limited to one case, that of a girl who had contracted typhoid fever in the hospital when recovering from another disease. In it the employment of the remedy had certainly

produced no favorable effect—it was now the forty-second day, and no marked convalescence had commenced. He regarded Dr. Bennett's experiments as of additional value from having been made *coram publico*, in the presence of a large number of intelligent students, many of whom had been, from time to time, examined on the very cases themselves. Important as the inquiry was, he much feared that the discovery of quinine as an antidote in continued fever would prove of but little avail, unless the additional discovery of new cinchona forests was also made.

Dr. William Robertson had also administered the quinine in some cases, in none with benefit. He had in two cases observed very alarming symptoms to follow its exhibition, a state of almost complete coma having been induced.—*Med. Times, May 8.*

EDITORIAL DEPARTMENT.

Encouragement of American Scientific Labors.—We have repeatedly taken occasion to comment on the tendency, in this country, to undervalue the labors of our own countrymen in behalf of medical science. Foreign works and communications in the Medical Journals of other countries, are reprinted, copied, circulated and absorb the attention, to the neglect of native productions which, to say the least, are equally meritorious, and which therefore, it would be supposed, should claim at least a fair relative consideration on the score of nationality. This disposition to depreciate everything of American origin, and to magnify everything emanating abroad, is by no means confined to medical literature. It pervades all the operations of mind, from the inventive genius which produces the never-ending variations in the fashion of dress, to the most important matters connected with scientific truth. A coat or boot made after the latest mode at the metropolis of the new Empire, is the *beau ideal* of excellence, and so the latest lucubrations of some unknown German, French, or English writer, is almost sure to be greeted with the utmost cordiality, while the truly creditable efforts of our fellow countrymen are but too often unnoticed. We do not find fault with any stretch of courtesy, or generosity, toward the labors of our brethren in other lands, all being fellow-artizans in the great republic of science which should, be limited by no territorial or sectional boundaries; still less do we desire to

see a degree of national assumption, or pride of patriotism, which might conflict with a recognition of superior claims whenever they are justly due to developments, scientific or otherwise, which come to us across the Atlantic. What we object to is, that undue importance should be attached to what is foreign simply because it is so, and on the other too little estimation placed upon native productions in consequence of their domestic origin. We complain, too, of the habit of thinking that the ore from our own scientific mines, must pass through a foreign mint before it is entitled to currency at home. It has happened more than once that practical improvements have been communicated on this side of the water which have been scarcely noticed till they attracted the attention of a journalist or author in Europe, and thus endorsed, they succeeded in obtaining a recognition in our own country. How often is it said with emphasis that such or such an one has acquired an "*European reputation*." This, we admit, is legitimately enough a matter of congratulation, or pride; but it is far from being complimentary to ourselves to present the fact as a reason why a person should enjoy with us a better character than before. It is customary to grumble at the disparaging remarks sometimes made by foreign writers respecting American contributions to the general stock of intellectual acquirement. We smart under a sense of injustice conveyed in the sneers in which it pleases some illnatured scribbler of another nation, occasionally to indulge. But this we fairly deserve so long as we virtually admit the charge of inferiority by a want of proper appreciation and encouragement of the labors of our countrymen. If we do not evince self-respect, we have no right to expect that marked consideration will be accorded to us by others.

Our thoughts have been directed to this subject by reading an editorial article by our friend, the editor of the Southern Medical and Surgical Journal. At the conclusion of a volume of his excellent periodical, he says: "Thirty-two writers, residing in nine different states, have furnished us fifty-two original articles for the last volume, which will, we think, compare favorably with the original department of any of our contemporaries. The great majority of these papers are essentially practical, very few of them being theoretical disquisitions. This we regard as an important feature in this publication, and have always borne it in mind in making up the eclectic department. Among our selections, we count sixty-six original *American* articles, derived from our respected exchanges, thus making no less than, one hundred and eighteen contributions to medical knowledge by our countrymen, in the volume for 1852! We are not of those who will transfer to their columns every idle lucubration or hasty suggestion found in foreign prints, while they

- pass without notice the writings, however valuable, of their fellow-citizens. This indifference, manifested by some of our contemporaries, to the dissemination of American contributions, has long been felt and complained of by the profession — and while we know that we have not republished as many of them as we could have wished, we have the satisfaction to reflect that we have done as much as our limits would permit. There are Abstracts, Retrospects, and Periscopes published in Europe, republished in the United States, and bought extensively by our professional brethren, and which yet scarcely contain an allusion to any thing written or done in this country. Why so? Can it be that nothing is done or written by the American medical profession worthy of a place in such compilations? Dr. Norwood, and others, have now for two years been publishing, from time to time, in this Journal, the discovery and usefulness of an agent capable of controlling with almost mathematical certainty, the action of the heart in disease. There are upward of thirty medical periodicals in the United States, with all of which, save one, we exchange. Of these, we doubt whether more than five or six have even noticed the discovery, directly or indirectly. How different would the case have been if the newly ascertained property of *veratrum viride* had been first announced in England, in France, or even in the depths of Germany! We might cite similar instances of disregard of valuable contributions published in the Journals of almost every section of our country.”

After the remarks introductory to the quotation just made, it is needless to say that we honor the views and conduct of the writer, and they must commend themselves to the approbation of every truly *American* reader.

Since the foregoing was written, we have happened to meet, in another of our contemporaries, statements very similar to those which we have expressed. In noticing the late treatise on Operative Surgery, by Henry H. Smith, M.D., contained in the *Phil. Med. Examiner*, for Jan., we are glad to observe that the editor comments, with proper severity, on the custom of deriving materials for systematic works from foreign authors, to the neglect of the abundant resources existing at home. We quote the following: “The materials for a genuine *cis-Atlantic* work may yet be had abundantly within the reach of those amongst us — and there should be many such — who are competent and not afraid to use them. The task, however discouraging it may now appear, must sooner or later be fulfilled, without this resort to the enervating draughts from foreign sources, which of late years have been the cause of so many of our publications. No man need go to Paris or London now to learn how to amputate a limb or extirpate a tumor, reset a bone, extract a calculus — in short, to effect the greatest triumph which surgical art

and science, under Providence, have ever yet achieved. Why, then, this never-ending slavery to the parasitic feebleness which will scarcely allow us, without resorting to an unknown tongue, to show our pupils how to perform the simplest operation."

Politically, the United States of America hold an independent position, and are fast becoming the most conspicuous and powerful of the nations of the world. Let the American mind be fully emancipated, and it is destined to accomplish for science, literature and art, results commensurate with those which pertain to civil and religious freedom!

Reduction of Dislocations of the Femur without the aid of Pulleys.—In an article by Dr. John Watson, in a late number of the New York Medical Times, we find the following remarks on the reduction of dislocation of the femur without the aid of pulleys. While we are glad to see that Dr. Reid's plan has been successfully applied in the New York Hospital, we regret to notice that the tenor of the remarks by Dr. W. does injustice to Dr. Reid. The writer refers to several German surgeons in such a way as to leave the impression that the subject had very little novelty in this country at the time Dr. Reid's article appeared, and that Dr. R. is not entitled to any credit on the score of originality. There seems to be an indefinite traditional notion that Dr. Nathan Smith recommended a plan somewhat similar to that described so perspicuously, and demonstrated so satisfactorily by Dr. Reid. The description of Dr. Smith's method, if we are correctly informed, by his son, in the memoirs prepared by the latter, is not very clear, and the method as described is not very practicable. At all events, it never was adopted to any extent in this country. Dr. Reid, however, was cognizant of the tradition, and is careful to say so in his essay. As respects the German authors who have "carefully studied and systematised" the subject, we wish Dr. Watson had been more explicit. It strikes us as rather remarkable that the fruits of the thorough investigation of the matter in Germany were not better known among us prior to the present time, if they have been in progress of development since 1823! Will not some one conversant with German medical literature, give the American public an analysis of what has been accomplished in Germany relative to the subject during the last thirty years, of which we have been so strangely ignorant, subjecting patients, in the mean time, to the barbarous treatment by pulleys, which are now ascertained to be useless!

The following is the quotation referred to :

Surgery, as well as medicine, has its fashions; and while we are ready to apply the extension pulleys to new uses, we are learning to dispense with them in some of the very accidents for the cure of which they were originally introduced. Our celebrated countryman, Nathan Smith, about the commencement of the present century, as I learn from one of his early pupils, Dr. Batchelder, of this city, as well as from the memoirs of him prepared by his son, Dr. N. R. Smith—was in the habit of pointing out to his students a method of reducing luxations of the femur on the dorsum of the ilium by merely flexing, adducting or abducting, and rotating, the thigh upon the pelvis. Others, both in this country and Europe, have, since the time of Nathan Smith, been accidentally successful in a few similar cases. In Germany, since 1823, the subject has been more carefully studied and systematized by Colombat, Wattmann, Kluge and Rust; and in our own state, within the past year, by Dr. W. W. Reid, of Rochester, whose process, as described in the last volume of the Transactions of the New York State Medical Society, is nearly identical with that of the latter writer. I am happy to say that the attention which Dr. Reid has anew attracted to this subject, is likely to make the flexing, adducting, abducting and rotating process, the general mode of treatment in all recent luxations of the head of the femur backward; and that, at the New York Hospital, two, if not three cases have already been reduced, without the aid of pulleys, by this method.

Hoping that these suggestions may be further tested, I remain, with becoming respect,

Yours, &c.,

New York, December 18, 1852.

JOHN WATSON.

We find in the Jan. No. of the American Journal of Med. Sciences, the following notice of reduction of a luxated femur, at Boston, Mass.:

"Dr. Parkman reduced a recent dislocation of the hip very easily with his hands alone, and unassisted, the patient being thoroughly etherized. The head of the bone was in the ischiatic notch. Taking the foot in his hand, Dr. P. bent the leg on the thigh, and the thigh on the abdomen, and, with slight outward rotation of the foot, drew the limb downward, with immediate reduction of the displaced bone. The dislocation was caused by the fall of some bags of coffee upon the patient who was aiding in raising them from a vessel's hold."

Ranking's Half-Yearly Abstract of the Medical Sciences.—This very complete abstract of the improvements and discoveries in practical medicine, continues to appear with its accustomed regularity. The American reprint, by Lindsay & Blakiston, is furnished to mail subscribers, free of postage, at \$2.00 per annum.

A Practical Treatise of Dental Medicine, &c. By THOS. E. BOND, A. M., M. D., Prof. of Special Pathology and Therapeutics in the Baltimore College of Dental Surgery. Second Edition. Philadelphia: Lindsay & Blakiston.

This is a well printed octavo of 360 pages. It treats somewhat extensively of matters connected with the practice of dentistry, and notices some subjects not strictly belonging to that department. In his chapter on the use of anæsthetic agents, the author very properly comes to the conclusion that the inconvenience and danger attendant on the use of these substances are too great to justify the dentist in their habitual use. The risk of producing two or three days' discomfort, and even of destroying life, should not be incurred to avoid the comparatively slight and transient pain of dental operations.

The book is for sale in Buffalo by T. Butler.

Remarks on the Treatment of Typhoid Fever by Disulphate of Quinia.
By J. W. HAYWARD, Esq., of Liverpool, Eng.

In our last issue we offered some remarks on the treatment of typhoid fever by large doses of quinia — a subject which, at the present time, appears to be occupying the attention of observers and writers both in this country and in Europe. The efficacy of this mode of treatment is, of course, a matter to be settled by observations made with proper care, reported with candor, and accumulated in sufficient number at different times and places to obviate the liability of attributing to the influence of the remedy, results due to the inherent tendencies of the disease. Thus far, facts pertaining to this subject seem to conflict with each other. We referred in our last number to observations which appeared to show that the quinia does not possess the power to cut short or abridge the duration of typhoid fever. In the Southern Med. and Surg. Journal, we find the following paper, copied from the London Lancet, in which the remedy is stated to have proved apparently highly successful in a large collection of cases. We reproduce the article in our columns in order that it may aid in inciting to further experimental trials of the plan of treatment. It is to be regretted that the report is so concise. More precise statements respecting the symptoms, the duration of cases, &c.,

would have rendered the paper more satisfactory. The author being unknown, the reliability of the statements must be based on the internal evidence of the article, and of this we leave the reader to judge, without offering any opinion of our own. The article is as follows:

In a practice, private and parochial, of which I have had charge for some time, at the south end of this town, I made particular observations on eighty successive cases of fever of the typhoid type; and I found the first symptom, in twenty of them, was diarrhoea; in twelve, diarrhoea and vomiting; in seven, vomiting alone; the rest began with pain in the head. All had pain in the head afterward; sixty-six describing it as "lightness," fourteen as "heaviness." All had tenderness of abdomen — nine to a great extent. Seventy-one complained of soreness of the flesh over the whole body—some to such an extent that their impression was that they had rheumatism. Seventeen had considerable inflammation of the sub-maxillary glands. Seventy-three had delirium, twenty-one of which were very severe. In all, the tongue became very dry, brown, hard, and cracked; the first crack was generally a deep, longitudinal one down the centre of the tongue, (even whilst it was clammy and velvety in appearance, and of a milk-and water-color,) extending from the base to nearly the apex; then many transverse and oblique ones. In twenty-four, the skin became rough and brown, with petechiæ observable. In all the thirst was intense; and the other symptoms of fever were not less evident; therefore suffice it to say they were well-marked cases of fever of the typhoid character.

In twenty-seven the treatment was commenced in the "first stage;" in fifty-three in the "second stage." Three were fatal. All the rest recovered more or less quickly.

The principal treatment in all except one was the use of disulphate of quinia, — so much recommended by Dr. Dundas, of this town, — of course modified according to the predominating symptoms. Thus, if I found the pulse quick, weak and thready; the tongue cracked, brown, and dry, (rough or smooth;) great thirst and delirium; no appetite; tenderness of abdomen; soreness of flesh, &c., I put the patient upon disulphate of quinia, in solution, at once; four or five grains every two hours. If great restlessness, and no sleep, I added three or four minims of tincture of opium to each dose. If general sinking of vital powers, some wine or brandy, with beef tea. If the solution of quinia were vomited, it was given in an equal quantity of wine, or wine and water. If the patient continued sinking, I increased the quantity of quinia, but never had occasion to extend beyond seven grains per dose. When ringing in the ears occurred, the quantity was decreased, but still kept up till there was a good appetite. If the delirium was intense, the pain in the head described as heavy, (which were always strong subjects, whose bowels were confined,) with strong pulse, a dose of chloride of mercury, and sometimes a blister to the nape of the neck. If tenderness of abdomen was great, a few leeches or sinapism. If vomiting, a sinapism over the stomach. If diarrhoea, a little calomel and opium, or diacetate of lead and opium; though this symptom sometimes required nothing more than the tincture of opium given with the quinine.

In seventy-nine cases, marked improvement was observable in the course of twelve hours: the pulse was the first to improve, then the delirium to give

way, then the pain in the head, the thirst, the soreness of flesh, tenderness of abdomen, dryness of tongue, and the appetite to improve. A pulse of 120, small and thready, would become 90, softer and fuller; and in the majority of the cases the pain in the head and delirium ceased entirely in the same period, and a rapid improvement followed; with the small exception of two cases of children ten and eleven months old, neither of whom had the medicine regularly, and both were complicated with bronchitis. The eightieth case was that of a woman, in which quinia was not used. These three died, evidently from typhus. If the typhoid symptoms were allowed to become well-marked, and the patient to sink much, his recovery was slow, and he required wine or brandy. I feel confident, from personal observation, that were the disulphate of quinia to be used promptly, and to cinchonism, the mortality even of typhus itself would be very small.

P. S. The practice alluded to is that of Dr. Whittle, who wishes me to make this communication to you.

Effects of an accidental administration of Ammonia.—The following case, (the report of which is highly creditable to the author's candor and regard for science) will be read with particular interest from its bearing on the question respecting the effect of the ammonia in the cases of supposed death from chloroform reported by Dr. Warren.—EDITOR BUF. MED. JOUR.

Dr. A. Flint,

DEAR SIR: The circumstances of a double mistake which occurred recently in the surgical clinic of Prof. J. C. Warren, concerning which strictures have appeared from several able sources, have induced me to transmit to you a report of an unfortunate mistake of my own, which may throw, by analogy of symptoms, some light upon the untoward event of Dr. W.'s cases, especially as the first case to which ammonia was not administered, suffered much less than the two with whom it was used.

Mr. C. B., a cooper by trade, aged about twenty-five years, applied to me in April, 1851, having general indisposition with neuralgic symptoms, etc. He said he had felt similarly several times before, and that these symptoms had usually been precursors of severe and protracted illness. After a few prescriptions, the relief from which was only temporary, I suspected verminous irritation, and was strongly supported in the opinion by the patient himself. Accordingly, on the 23d of April, he took at bedtime a full dose of *Dolichos Pruriens*, and called on me on the morning of the 24th. The bowels had not moved, and I gave him a vial of (as I supposed) *Ol. Terebinth*, directing him to mix with one tea-spoonful of it, a table-spoonful of castor oil, and take as soon as he reached home. I was summoned hastily

in a few minutes and informed that "what I had given him was choking him to death." On my arrival I was shown a half mixed fluid, viz., castor oil and *aqua ammonia*, which I had carelessly given for ol. tere. I might, perhaps, exculpate myself, in part, by farther particulars, but the bare fact is only necessary. The suffocation at first had been extreme. The patient had called for milk as soon as he swallowed the portion, and drank freely. He then irritated the fauces with his finger and emptied the stomach thoroughly. The tongue and tonsils were exceedingly red, the former having been thickly furred before. Respiration was loud and labored. Extreme anxiety of countenance, frequent efforts at deglutition, a short cough evidently restrained as much as possible, and a profuse discharge of ropy mucus from the lungs and throat. The voice was croaking, but soon became more natural. His complaint was of "distress through the chest." The mouth and larynx swelled but slightly. The distress through the lungs was severe, the cough considerable, and the discharge of mucus copious for about thirty-six hours. Both general and local antiphlogistic remedies were promptly used as soon as the system rallied, and on the morning of the 28th, (i. e., the fourth day,) he seemed to have recovered from the effects of the unlucky dose.

About noon of the 23th, the skin was observed to be turning yellow. At 4 o'clock, P. M., the icteric hue was deep, but he was walking about, and said he did not feel very bad. At 10, P. M., I was called, and found him in severe general distress, with high fever, which in about forty-eight hours assumed a low type, during the course of which pneumonia of both sides and pleuritis in the lower portion of the right thorax were severe complications; and it was not until May 11th, that he was fairly convalescent from a disease, the severity of which I have seldom seen outlived. Pneumonitis was very prevalent in this vicinity at the time.

Private interest, and personal pride, would forbid this exposure; but if in your judgment the facts can be of service, I have no right, or wish, to suppress them. I do not believe the *aqua ammonia* was the cause of the icterode, or of any part of the disease after the fourth day, (except, perhaps, to increase the pulmonary irritation,) but it was severe enough during the first twenty-four hours to cause fearful forebodings on my part, and would undoubtedly have been much worse had the patient been under the full influence of the safest anæsthetic.

Very respectfully, yours, &c.,

J. H. BEECH, M. D.

COLDWATER, Mich., Jan 18, 1853.

The Esculapean.—Dr. C. D. Griswold has commenced a monthly publication called the *Esculapean*, the object of which is to diffuse information relative to medicine, and the medical profession, among the people. Of course it is designed for popular readers. The following list of headings of the several articles contained in the first No., will convey an idea of the scope of the work: "Sketches from the everyday experience of a Young Doctor;" "How benevolent persons may be led to do evil;" "Water-cure waxes mighty;" "Familiar Letters on the Structure and Functions of the Human System" (with illustrations); "The Chemistry of the Atmosphere;" "January, its joys and sorrows;" "Habit becomes imperative;" "What has become of the Bloomers;" "What every one should know;" "Book notices."

It is published in a folio form with a tripartite division of columns.

As the work costs but a dollar *per annum*, we would advise our readers to subscribe for a year, and if they find it calculated to do good, their inclinations will of course prompt them to use exertions to promote its circulation among the people.

"*Ye are all Physicians of no value.*"—The editor of the Philadelphia Med. and Surg. Journal states that "Among the sermons delivered under the auspices of the Medico-Chirurgical College, on Sunday evenings, to medical students, was one by the Rev. W. Bacon Stevens, M. D., on the above text." The editor adds, "It was one of the most ingenious, learned, and eloquent of the season."

We trust the text has no special significance as applied to the physicians of the city in which the sermon was delivered! Possibly the preacher limited its application to irregular practitioners. The auditory must have been taken aback somewhat at the information contained in the announcement of the text.

New Medical Journal.—A new Medical Journal has been commenced at Nashville, Tenn. It is entitled the *Southern Journal of the Medical and Physical Sciences*, and is edited by Drs. Jno. W. Sting, Wm. P. Jones, R. O. Cuney, and B. Wood. It is to embrace subjects pertaining to dentistry. It is a bi-monthly of seventy-two pages. The typographical appearance is good, and as it has four editors, it is to be presumed there will be no lack of original matter. We wish the enterprize success.

Spiritual Rappings. — The New York Medical Times states, that in the State Lunatic Asylum, of Ohio, at Columbus, are twenty persons whose insanity is clearly traceable to spiritual rappings; and in the State Asylum, at Utica, there are nine victims of the same delusion.

We happen, every now and then, to hear of the members of the Fox family pursuing their knee-and-toe-knocking vocation in different parts of the country. If there should be a reader of our Journal in any place in which they may undertake to practice their deception, and he will take the trouble to test their spiritual functions in the simple way that was adopted in Buffalo, which is described in our exposition, (vol. vi, page 628,) he will very shortly put a stop to the delusion *in that place*.

The Smelfungus Papers. — Considerable curiosity is manifested to know the authorship of the Smelfungus papers. The name of the writer will not be disclosed so long as he chooses to remain *incognito*. We will say this much — he does not reside in Buffalo. We hope to be frequently favored with his very clever productions.

Editorial Changes. — Dr. Geo. Mendenhall has withdrawn from the co-editorship of the Western Lancet, leaving Dr. L. M. Lawson sole editor. The Western Lancet has entered on the fourteenth year of its existence. Dr. Lawson has been connected with it from the commencement.

Cincinnati Retreat for the Insane. — Dr. Edward Mead, formerly of Chicago, Illinois, has opened a private retreat for the insane, near Cincinnati, Ohio.

Notice. — On, and after the first day of March next, the address of the editor of this Journal will be *Buffalo, N. Y.*

BUFFALO MEDICAL JOURNAL

AND

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NO. 10.

ORIGINAL COMMUNICATIONS.

ART. I.—*Case of Acute Atrophy of the Liver.* By CHAS. A. LEE, M. D.

Mary Berry, Irish, aged 22, servant, generally robust and healthy, entered the Hospital of the Sisters of Charity, of Buffalo, Jan. 21st, 1853. Has been somewhat unwell for several weeks; troubled with nausea and vomiting, and occasionally diarrhœa; seems inclined to coma; but little power over her limbs; skin slightly icterode, and temperature of skin about natural; face considerably bloated; limbs somewhat œdematous; pulse slow and feeble; tongue furred; disposed to lie on her back. A mustard plaster was prescribed, to be applied to epigastrium, and a teaspoonful of a mixture composed of three drops of creasote and ℥ij water, every hour, to allay the sickness and vomiting. In addition, a pill of $\frac{1}{2}$ gr. opium, and ij grs. blue mass, was added every six hours.

Jan. 24th. Pulse 76. Tongue thickly furred. The skin yellow, bordering, in some places, on a dusky-brown; heat of skin about natural. The sickness of stomach had been entirely relieved by the creasote and mustard; bowels had not been moved since her entrance. Inclined to sleep constantly, but **says** she feels better.

Jan. 25th. Pulse 68. Tongue continues furred; countenance sullen; very stupid. Temperature of skin natural. No pain. No movement of

bowels. Increased yellowness of skin. Prescribed *hyd. sub. mur.* gr. x, *rhei*, gr. x, *ol. menth.* gtt. iij, followed by *Rochelle salts*, *senna* and *anise seed*, a wine glass of a strong solution, every hour till it should operate.

Jan. 26th. The stomach had rejected most of the medicine. No operation. Pulse 88. Heat of skin natural. Tongue furred and dry. Much thirst; inclines to coma, but can be roused. No urine secreted, nor has she passed any since her admittance, as can be ascertained. Skin deep-yellow, or bronze color, over whole body. The right hypochondriac region was directed to be rubbed with an ointment, consisting of equal parts of *compound iodine* and *mercurial ointment*, and a stimulating enema administered.

Jan. 27th. About the same. Still comatose. Directed warm *brandy toddy*, with grs. v, *carb. ammonia*, every two hours, and a drop of *Croton oil* every two hours, between same, till it should operate.

Jan. 28th. The coma increased; one or two involuntary evacuations had taken place from the bowels; the pulse became feeble and more frequent; and she expired toward morning.

Autopsy ten hours after death.

All the tissues were deeply tinged of a bright yellow hue. Nothing abnormal was noticed in any of the organs, except the liver, which was placed in the hands of Prof. John C. Dalton, who has kindly favored me with the following account of its appearance:

JANUARY 28, 1853.

This liver was reduced to one half its natural size, and very flabby and loose in consistency. The exterior was in parts pale, but mostly of a dusky hue, considerably darker than the normal color of the organ. There was no opacity or other inflammatory alteration of its peritoneal covering. The organ presented the appearances designated by Rokitansky, under the name of "Acute Atrophy of the Liver." The cut surface showed a considerable portion in a state of partial hyperæmia, the capillaries in the centre of each acinus being gorged with blood, while the intervening spaces remained of a yellowish color; giving the marbled appearance to the cut surface, known as "nutmeg liver." There were, also, large portions in which the whole tissue was so infiltrated with bright yellow biliary matter, that though the form of the acini was preserved, all distinction between these component parts was lost; and the hepatic tissue presented the appearance of a congeries of vessels injected with yellow paint. The portions in which this alteration had taken place were mostly globular in shape, and varied from the size of a hazel-nut to that of half an egg. They were considerably more friable than the rest of the hepatic tissue. When situated near the exterior, these portions projected

somewhat above the surface of the organ, presenting an appearance very similar to that of a mass of varicose veins, except for the strong yellow color, which showed distinctly through the peritoneal coat.

The bile contained in the gall bladder, was very black and tarry in consistency.

J. C. D.

Remarks. — This affection appears to have been first described by Rokitsansky, and is still unnoticed in our standard works on practical medicine. He described it as “Yellow Atrophy of the Liver,” an acute affection, “the expression of a constitutional malady,” and having its immediate origin in anomalies of the blood. It is characterized by the saturated yellow color, owing to a diffusion of bile throughout the tissue, by extreme flabbiness and pulpiness, loss of the granular texture, extreme rapidity in the reduction of size, which chiefly affects the vertical diameter and consequently induces a flattening of the liver. He remarks that it occurs chiefly in the early years of life, during puberty and middle age, and is remarkable for the rapid course it runs, for extreme tenderness of the liver, nervous attacks and jaundice, and that it terminates fatally with febrile symptoms of a disorganized state of the blood, irritation of the brain and its membranes, hydrocephalic softening of the former, and with symptoms of exudation and suppuration generally, and especially of the mucous membrane, pneumonia, &c.

He also calls attention to the fact that the blood contained in the large vessels of the liver and in the trunk of the vena porta, is reduced in consistence, and of a dirty reddish-brown color, and that the coats of the latter vessel are tinged with bile; and this, he remarks, points to the fact that the portal blood itself contains such an excess of biliary constituents, that they are separated here, and still more in the capillaries, and thus fill the entire vascular and biliary system; the coats of the vessels and the cellular strata thus absorb bile by exosmosis, the true glandular tissue fuses, is lost in the biliary colligation and disappears. The immediate consequences of this condition are, that the blood in the vena cava is infected and overcharged with bile, causing intense jaundice, and when this has reached a certain point the above symptoms terminate in a rapid consumption of the blood and in exhaustion, and it is not unusual to find in these cases, biliary matter of a deep-yellow color, or a black tarry substance in the intestines, owing to an exudation of the disorganized blood through the mucous membrane. This affection is readily distinguished from Lænnec’s *Cirrhosis*, which is a chronic affection, though the color of the liver resembles that of acute yellow atrophy, but instead of being soft and flabby, the liver is abnormally firm and tough

There is a similar affection described by Rokitansky, called "*Red Atrophy of the Liver*," which is distinguished from the yellow by its dark-brown, or bluish-red color; the liver is gorged with blood, and presents a spongy, elastic consistency; there is an absence of granulation, and the texture is perfectly homogeneous when cut into; the organ is reduced in size, though its thickness preponderates over the other dimensions.

It is, moreover, a chronic affection, and accompanied by torpor of the abdominal ganglia, venous plethora of the abdominal viscera, and by the formation of brownish-black, or greenish-black, tarry bile, and fæces of a similar constitution. Rokitansky thinks that death generally results in this latter disease, from the congestion of the portal system.

We have only met with one well-marked case of this disease before, and that was many years since in dispensary practice, in New York. The symptoms were very similar to those above described in Mrs. Berry. That it is not very uncommon is very probable, as Prof. Dalton informs me that he saw two or three cases of it the last summer, while attending the autopsies of Rokitansky, in the General Hospital, at Vienna. The symptoms, as it will have been noticed, were, with the exception of jaundice, very similar to those produced by the retention of urea in the blood; and as I could not ascertain the state of the urinary secretion, it is very possible that the comatose symptoms may have been partly due to that cause. The coma and jaundice, however, increased pari-passu, until death took place. Rokitansky, it seems, attributes death in this disease to a "rapid consumption of the blood, and exhaustion, consequent on overcharging the blood with bile."

We submit this case to the profession, with the hope that the attention of others may be directed to this disease, who have better opportunities of observation. There is no class of diseases involved in more obscurity, at this present time, than those of the liver; and none need more the patient investigation of practiced and enlightened pathologists. Our country opens a wide scope for such study; but to be successful, the investigation is not to be limited to the liver only, but extended to the constitution of the blood, and the various modifications, consequent on the different kinds of hepatic derangement.

BUFFALO, February 10, 1853.

ART. II. — *Two Cases of Irritant Poisoning, in one of which death occurred on the third day, from purely nervous symptoms, and in the other on the first day, with signs of violent inflammation of the alimentary canal.* By JNO. C. DALTON, Jr., M. D.

No. 1. A Case of poisoning with Bichloride of Mercury, inducing delirium tremens. Death on the third day.

L. W., 42 years of age, took poison under the following circumstances: He had been quite intemperate for several years, and had had delirium tremens some time previously. For several days preceding the 12th of August, he had been unable to work on account of debility and a kind of "sick headache." On the forenoon of that day, he had hard words with his wife, who reproached him with lounging about the house, drinking and making himself a burden to her. In consequence of this he procured, at a neighboring apothecary's shop, a little over one ounce of a solution of Bichloride of Mercury, in dilute alcohol, in the proportion of thirty grains to the ounce; giving, as an excuse, that his wife wanted it for "bed-bug poison." The bottle was labeled, in large letters, "ARSENIC.—POISON."

Immediately after obtaining the poison he went home, and at half-past eleven o'clock began pouring it out into a tumbler, in presence of his wife. She endeavored to strike the bottle out of his hand, and in the attempt spilt, as she thinks, about one third of it. He then went down into the cellar and there drank the poison, having (as he said) mixed sugar with it, came up again into the kitchen, placed the bottle and tumbler, both empty, upon the table, and went up stairs to bed. Immediately after swallowing the poison, he experienced, as he said, a burning sensation, which lasted for two hours. He vomited, spontaneously, in about ten minutes. After this, his wife sent for the apothecary, who came immediately and administered an egg, beat up with flour and water, and about fifty grains of powdered ipecac, which operated freely as an emetic. He was first seen by the attending physician, at 12, M. At that time the skin was very cool and the countenance collapsed. The pulse was imperceptible at the wrist. At the carotids it was feeble, and 166 per minute. He took six or seven more eggs, and plenty of flour and water. The vomiting continued. He had, also, frequent dark fluid stools.

He was again seen at 3, P. M., on the same day. He was then lying on his right side, partly prone; but soon changed his position, and often tossed about restlessly. The skin of the face and extremities was cool, damp, and sublivid. The countenance collapsed and anxious. Pulse at the wrist 140

very small, and irregular in force; sometimes strong enough to be counted, sometimes not. The tongue somewhat furred, cool and sticky. Altogether he had much the aspect of a cholera patient, in an early stage of the disease. He did not complain of pain in the abdomen except slightly, just at the time of having a stool; but he complained much of thirst and of "sick headache." The eyes were bright, but he said he was unable to see distinctly. He heard well, however, and answered questions properly. He complained some of occasional cramps in the legs. He was a little inclined to be talkative, reminding one of a patient with delirium tremens. The abdomen was rather sunken, moderately resonant, *not at all tender to pressure anywhere*. The fauces were a little reddened. The patient vomited freely during the visit, nausea being excited by examining the fauces with the blade of a case-knife. He had, also, during the visit, a discharge from the bowels, amounting to about four ounces, and consisting of a thin, watery, dark-reddish fluid, entirely without fæcal odor, and having a quantity of soft, yellowish-brown and whitish flakes floating in it, and also a few minute bloody coagula.

9, P. M., (same day.) Has continued about the same, vomiting frequently after drinking, and having dysenteric discharges, like that during the last visit, about once an hour, without pain. At 6 o'clock he was ordered to take half a grain of opium every hour. He is still restless. Has *no pain*, but complains much of thirst, and insists on drinking, though taking fluid always excites vomiting. His "headache" is much abated, and he feels tolerably easy when lying still, but when he raises his head from the pillow says he has ringing in the ears and some dizziness. Skin about the same, or a little warmer. Pulse stronger, so that it can readily be counted at the wrist, 120. Abdomen as before, absolutely without tenderness on pressure. Patient's manner rather more talkative and flighty than at the last visit, but no positive delirium.

R̄. Pulv. Opii. gr. i;

and repeat in one hour, unless quiet. Let him have no drink but bits of ice to hold in mouth.

13th, 8, P. M. Patient slept some after two o'clock this morning. In latter part of forenoon the vomiting and purging ceased, but commenced again about 2, P. M. Since then he has vomited often, and been purged twice. He is now lying on the back, and rolling often restlessly from side to side. He has still more the aspect and manner of delirium tremens, though his mind is quite clear. He talks much, and in a quick, hurried manner. He turns his head frequently from one part of the room to another. When

questioned, says he does this on account of tendency to vertigo, which is relieved by frequently turning the eyes from one object to another. Has had double vision, occasionally, this P. M. Sight constantly a little dim. Has no pain; but, on raising his head from the pillow, suffers increased dizziness and dimness of vision. Pupils natural, sensible to light. Great coolness of skin on hands and forearms, but less of face. Pulse 108, a little improved in character. Tongue clean, warm, not sticky. Thirst continues, but is less urgent. Patient vomits always after drinking, even a very little. Has also vomited a green, bilious-looking fluid this P. M. Evacuations of the same character as before. No pain with either vomiting or purging, except from straining with the former. Abdomen as before, not all tender. Has passed urine, he says, but once since morning, and then only "a few spoonfuls." He has some soreness of the throat on swallowing, but none along the course of the œsophagus. Has had, to-day, enemata of rice-water and morphine, (gr. $\frac{1}{4}$), which he refuses to take any longer.

14th, 8, A. M. Delirious. Talks incoherently at times; though, when questioned, answers generally coherently, but in a hurried, irregular manner. Little or no tremor. Has vomited much during the night, and been twice purged. No urine. Has taken about one grain of sulphate of morphine, in divided doses, after which he has shown a disposition to doze for a short time. Coolness of skin about the same. Pulse 100, in character as before. Tongue clean, a little red, dryish. Thirst continues. Abdomen rigid, not tender. *Pupils contracted, insensible to light.* Says he has still double vision at times. No pain. Insists upon having drink as before. Matters vomited of a bilious appearance. Breath very fetid. Gums swollen and dark-red. No salivation to be observed. Refuses the enemata.

R. Morphiæ Sulph. gr. j;

Repeat in one hour unless quiet. Let him have a soda-biscuit, and a piece of salt fish.

Patient became somewhat more quiet in the forenoon, but vomited again afterward. Delirium increased. Between 1 and 2, P. M., his wife gave him a piece of soda-biscuit and salt fish, which were not vomited, at least immediately. A little after 4, P. M., he got out of bed and came down the attic stairway on to the first floor. His wife urged him to go back again, telling him "the doctor was coming." He remounted the stairs hastily, and on reaching the top fell down in a fit. During the fit he was convulsed generally and unconscious. He regained consciousness twice, but immediately

relapsed into his previous condition. He died in about ten minutes after the second relapse.

Autopsy twenty-four hours after death.

Cadaveric rigidity moderately well established. Countenance natural. No remarkable discoloration about the body.

HEAD.

Dura mater natural. Some serous effusion, $\frac{3}{4}$ j. or more, in arachnoid cavity, but the arachnoid membrane itself has a natural appearance. Subarachnoid fluid considerably more abundant than usual.

Mulformation of Falx major. Falx major of the dura mater is of natural breadth at its commencement at the crista galli, but immediately behind this point is entirely wanting for the anterior quarter of the longitudinal fissure of brain; its place being supplied only by a slightly raised whitish ridge on the lower surface of the dura mater, and a few fibres which run forward and lose themselves in the cellular tissue between the anterior lobes. At the part where the falx is deficient the anterior lobes of the brain are adherent to each other by their plane surfaces, and the arachnoid passes across from right to left, with only a slight depression at the situation of the fissure. No morbid appearances in the neighborhood. The whole of the encephalon is of firm consistency and natural in color, or a little paler than usual. No effusion of fluid into ventricles.

CHEST.

A very small quantity of fluid in the pericardium. Internal surface of the pericardium natural. Right cavities of heart filled with dark-red and fibrinous coagula, and a little fluid blood. The left ventricle well contracted, nearly empty. Left auricle contained a moderate amount of dark fluid blood and coagula. Substance of heart and endocardium natural.

Some old, cellular, pleuritic adhesions of right lung: none of left. Both lungs present a remarkably healthy, well developed appearance, full, rounded, crepitating freely everywhere, and without the slightest induration or atrophy in any part. A moderate post-mortem engorgement of posterior portions.

ABDOMEN.

The peritoneal cavity contains $\frac{3}{4}$ jss. to $\frac{3}{4}$ j. of thin, watery fluid, slightly tinged with red. Peritoneum itself natural. Intestines of a healthy appearance externally, smooth and shining, of a dull-yellowish color, varied by spots where they are mottled dull-red and purplish. Stomach white externally,

with a circular constriction at its central part, by which it is divided into a cardiac and a pyloric portion.

The stomach contained about ℥ij. of a thick, yellowish, gruelly fluid. *The only alteration of the mucous membrane is an increased vascularity*; and this is nothing more than would be produced by the process of digestion. It consists in numerous small, tufted bundles of bright-red vessels, contained in the substance of the mucous membrane. They are most abundant in the great pouch, particularly on its posterior wall, and along the small curvature, where they are numerous for about three-quarters the entire length of the organ. There is a very moderate cadaveric softening and thinning of the mucous membrane in the great pouch; — less than is often seen in cases where no affection of the stomach has been suspected. Toward the pyloric extremity the mucous membrane is entirely natural in color, vascularity, thickness and consistency. The mucous membrane is also stained with bile in some spots in the great pouch and small curvature. There are two small mucous polypi growing from the anterior wall of the organ. Nothing else remarkable. Internal surface of oesophagus natural throughout.

The small intestine contains a little dark-brown and greenish, and rather thick fluid. There is considerable dark-red, vascular congestion in a few spots; and elsewhere the mucous membrane is stained by the contents of the intestine. But there is no ulceration, thickening, nor well-marked softening anywhere. The intestinal glandulæ not at all remarkable. Internal surface of the large intestine also healthy.

The liver was quite pale, but otherwise natural. The gall-bladder contained about ℥ij. of very dark bile. The spleen was very small and quite firm in texture; otherwise natural.

The other abdominal organs were healthy.

This case was somewhat remarkable for the small quantity of the Bichloride (less than thirty grains) which produced death, notwithstanding the appropriate antidotes were immediately administered, and free vomiting took place very soon after the poison was swallowed. The poison seemed to act, also, very much as a mechanical injury would have done, in bringing on delirium tremens in a patient predisposed by his bad habits to this form of disease; and the inflammatory effects of the mercurial salt were, at the same time, very slightly developed or altogether absent, except so far as the intestinal mucous membrane itself was concerned. For although there was frequent vomiting, and abundant dysenteric evacuations, there was no pain of the abdomen, nor tenderness to pressure at any period of the illness. Notwithstanding the late period at which death took place (the third day after

swallowing the poison,) the fatal result in this case seems owing to the impression on the nervous system, and not to any local effects of the irritating substance. In fact, the post-mortem appearances would not have given rise to any suspicion of irritant poisoning, if the previous history of the case had been unknown. It should, therefore, be remembered, in judicial investigations, that a patient may die from the effects of a poisonous dose of corrosive sublimate, and yet the intestinal mucous membrane show no inflammatory appearances sufficient to account for death. It is already well understood that this may be the case where a very large dose of the poison has been taken, and death has followed suddenly, at the end of a few hours, in the stage of collapse; and the above case shows that the same thing may happen, under some circumstances, even when death is delayed till the third day. It forms a marked contrast to the following case, in which the whole course and termination of the malady were different.

No. 2. A Case of irritant poisoning. Death on the first day, from collapse. Violent inflammation of the stomach, and some of small intestine, larynx and trachea.

A. G., a married woman, 26 years of age, had been somewhat depressed in spirits owing to the loss of an infant, and other domestic troubles. On the 20th September, she suddenly left home without giving any notice to her friends, and made her way alone to the railroad station of a neighboring city, where she purchased tickets to continue her journey in a direction away from home. A little after 3 o'clock the same afternoon, the station-master observed her to walk quickly through the station-house and enter the ladies' dressing-room. After she had remained there some ten minutes, he sent a female after her to see what she wanted, and learning that she was ill, went in himself, and found her sitting in the water-closet, vomiting freely a kind of "phlegm mixed with white substances." Her lips were very white and swollen. He spoke to her, but she would answer only in monosyllables. She was put into a carriage and conveyed to more comfortable quarters, where she arrived at half-past four. A physician was immediately in attendance.

She was at first able to stand up, and even attempted to get out of the carriage without assistance. Although unable to speak, she evidently understood what was said to her. The skin was cold and damp. Countenance livid. Lips swollen. Pulse imperceptible at the wrists; at the heart, 160. Respiration 40. Eyes suffused. Pupils insensible to light. She was making ineffectual attempts to vomit.

An emetic was prepared for her, but she was unable to swallow it. She

rapidly grew more feeble, and was soon unable to stand. At 5 o'clock she vomited about four ounces of a thick, slimy fluid, mixed with reddish flakes; and half an hour afterward had an evacuation from the bowels, of a similar character. Her position was at first bending forward, and indicated distress in the abdomen. She had no marked convulsions, but only once or twice a prolonged *shuddering* motion. The respirations grew gradually slower and more laborious, and the countenance more livid. She died at fifteen minutes past six, P. M.

Autopsy — sixteen hours after death.

Cadaveric rigidity very strong. Much purple discoloration of the dependent parts of the body and limbs, and a little of forehead. Excessive lividity of the finger nails. A little frothy fluid, tinged with red, protruding from between the lips. Abdomen natural in appearance externally.

HEAD.

Blood in the Arachnoid Cavity. There was a very small quantity of dark fluid blood in the cavity of the arachnoid, smeared over the posterior part of the right cerebral hemisphere, near the longitudinal sinus. No similar appearance elsewhere. The arachnoid membrane itself, after the blood was wiped off, was natural in appearance. Brain and cerebellum in other respects everywhere healthy.

CHEST.

There was considerable dryness of the pleura, on both sides of the chest, between the lungs and the pericardium, and between the lungs and the diaphragm. The pericardium contained its ordinary amount of clear fluid.

Old Pericarditis. There was a white opaque fringe of old lymph, three-eighths of an inch in length, attached to the anterior surface of the left ventricle, occupying a spot, about a quarter of an inch, or more, in diameter, and having its fringed extremity floating loose in the cavity of the pericardium. Pericardium otherwise healthy.

The right cavities of the heart, and the veins in its immediate neighborhood, contained a very large quantity of dark fluid blood, but no coagula. The left ventricle was firmly contracted.

Ecchymosis in Heart. On its internal surface were many spots of dark ecchymosis, situated immediately beneath the endocardium, some of which extended an appreciable distance into the muscular substance of the heart. Heart and endocardium otherwise natural.

Infiltration and Ecchymosis of Larynx and Trachea. Some old pleuritic adhesions on both sides of the chest. Much purple cedema about the pharynx and the superior border of the larynx, with spots of ecchymosis immediately beneath the mucous membrane. Some abrasions, also, of the mucous membrane, but no ulcerations. The larynx contained a little semi-transparent and opaque mucus. There was much purplish infiltration of the submucous cellular tissue about the aryteno-epiglottidean ligaments, and the upper part of the larynx generally; none of the vocal chords. The posterior surface of the epiglottis was of a clear red color. There was but little infiltration here, but many small circular spots of dark-red ecchymosis, beneath the mucous membrane, particularly toward the right side. The internal surface of the ventricles of the larynx presented the same appearances of infiltration and ecchymosis.

Immediately below the vocal chords the redness and dark purple, submucous ecchymosis became intense, but there was little or no serous infiltration. This discoloration and ecchymosis continued nearly down to the bifurcation of the trachea on the left side, and a little beyond it on the right, increasing in intensity from above downward. It was everywhere most intense at the anterior part of the trachea, being nearly or quite absent in its posterior, membranous portion. The primary and secondary bronchi were a little redder than usual, but there was no other alteration of their mucous membrane, and no ecchymosis. The bronchi contained a small quantity of frothy, viscid, transparent mucus, slightly tinged with red.

The lungs were everywhere natural;—only a very moderate bloody engorgement of their posterior portion.

ABDOMEN.

Sub-peritoneal Infiltration. The peritoneum contained two ounces of a clear, thin, claret-colored fluid. The membrane was everywhere natural, except at one spot on the anterior surface of the great pouch of the stomach, where there was a cluster of bright-red streaks of injection or ecchymosis in the peritoneum. There was much reddish infiltration of the cellular and adipose tissue between the layers of the peritoneum along the small curvature of the stomach; and much actual *bloody* infiltration along the large curvature.

The small and large intestines were of a light pinkish color externally; perfectly natural in appearance.

Inflammation of the Stomach. The stomach was somewhat contracted and collapsed. It contained about six ounces of a dark-red, bloody-looking

fluid, without remarkable odor, which was thickened by holding in suspension a large quantity of light-reddish flakes, like those seen in the discharges of dysentery. There were, also, a few soft masses, like the remains of half-digested food. The parietes of the stomach were generally excessively thickened; so that the anterior wall of the organ, near the pyloric extremity, measured three-eighths of an inch in thickness, and near the cardiac extremity, nearly a quarter of an inch. This thickening was entirely owing to infiltration of the submucous cellular tissue with a light-red, serous fluid. This infiltration was nearly uniform throughout the organ. The mucous, muscular and peritoneal coats were not changed in thickness.

The internal surface of the stomach presented an intense purple redness throughout the middle third of the organ, where the mucous membrane was thrown into numerous transverse and longitudinal folds. The redness here existed in the substance of the mucous membrane; and in some spots was so dark and so intense as to constitute a true stain or ecchymosis.

In the great pouch the mucous membrane was darker, but not so red; being of a deep slate color on its free surface, and having a finely *granulated* appearance in consequence of having been strongly corrugated and thrown into minute folds; which had, at the same time, an opake gray color. There was here, also, much dark-purple or black ecchymosis in the substance of the mucous membrane, and beneath it.

In the pyloric portion of the stomach, the mucous membrane was of a dull slaty color, not much reddened, but "mamellonated" and finely granulated, as in the great pouch. (Those portions of the mucous membrane which presented this *grayish* color, had the appearance of being reduced to an eschar, in consequence of uniting chemically with the poisonous substance.)

There was no ulceration, or other morbid appearance beside the above. The œsophagus was of a dull-purple color, internally. There was no alteration of its mucous membrane proper, but considerable purplish-red infiltration of the submucous cellular tissue. Surface of the mucous membrane smeared with a little whitish opake mucus.

Redness of Intestine. The upper part of the small intestine contained a fluid similar to that in the stomach; its middle portion a smaller quantity of the same, but lighter colored; the lower part, a much thicker, semi-solid substance, nearly white.

The small intestine had a reddish, slaty color, internally, at the upper part of the duodenum. Below this, the mucous membrane was of a strong brick-red, gradually diminishing in intensity, from above downward, so that there was little or no abnormal redness below the upper third of the intestine.

The intestinal glandulæ were natural.

The large intestine contained three ounces of a fluid, similar to that in the stomach; but there was no marked alteration of its mucous membrane.

Mesenteric glands generally unaltered.

The liver was rather pale and flabby; otherwise natural. The gall-bladder contained a couple of ounces of thinnish, yellowish-brown bile.

The pancreas was natural. The spleen rather large, weighing eight ounces.

Both kidneys were considerably engorged with blood. Other abnormal organs natural.

The blood was everywhere fluid.

Although it was not directly proved, in this case, that the patient took poison, the post-mortem appearances were such as to leave no reasonable doubt of the fact, and at the coroner's inquest it was not thought necessary to order a chemical examination of the contents of the stomach. The woman was known to have corrosive sublimate in her possession, and it was probably this substance which she took for the purpose of self-destruction. The inflammatory appearances in the larynx and trachea are interesting as a somewhat unusual occurrence in these cases. They were probably produced by the contents of the stomach partially finding their way into the air passage during the spasmodic attacks of vomiting.

ART. III. — *Opium*. The opinions thereon of SCHRAPPENKUTTEL
SMELFUNGUS, M. D.

“I acknowledge no Procrustean creed, decapitating nonconformity!”

All is blue in the office of Smelfungus. He is blowing a cloud, and the room is filled with the aroma of tobacco, about the paternity of which there can be no doubt — it is genuine Connecticut, and no mistake.

“Longevity,” he says, “cannot be considered a characteristic of the *materia medica*. From the earliest days of medicine, *ephemeræ* have arisen; fluttered out their existence, and disappeared from the view. But other articles there are, which possess inherent vitality, which have been for ages the main pillars of therapeutics; and which still stand firm in their stately proportions. And stateliest, with firmest shaft of these, is opium.

“Opium! mysterious drug, whose potency is felt not only in the body,

but in the recesses of the mind: where is the link that connects the sleepy poppy with the grandest powers of our nature? By what deep-hidden agency does it lull the racked nerve to quiet, and steal upon the mind with gorgeous dreams, extending time and space beyond conception? No man has yet returned from these close penetralia with power to tell their secrets. Used daily and hourly for many centuries, it is still unknown, misunderstood, abused, and underrated. De Quincey, wrapped in Elysian dreams by its still influence, quaffing by imperial pints his 'happiness in bottles,' with powers of utterance never equaled, 'speaking as never man spake,' has lifted the silver veil, only to reveal behind it the blackness of darkness. He has called from out the depths; but his voice is choked by sorrow, and we hear sighs only, — *suspira de profundis*. Oh mighty agent! instrument by which the soul dips down to Acheron, and gazes through the portals of Tartarus; no power can interpret or destroy thee!"

"Now, Smelfungus," I venture to insinuate, "you are on stilts as long as that euphonious baptismal name of yours, and allow me to suggest, you are gyrating rather awkwardly. How are you going to get down?"

Smelfungus looks grieved, holds silence for a moment, and then abruptly changes his style of address.

"Have you noticed, my friend, how all things work together for good to them that love physic? Have you seen how, out of this expectant humbug, have come goodly things, figs from thistles, grapes from thorns? Our over-wise Expectant must needs do something. His lazy theory was to wait for positive indications; and very naturally, one eternally recurring indication was the relief of pain. So our friend Expectant follows it out, and when you come to examine his treatment, you find here, there, everywhere, opium, and opium alone, prescribed in all the ills that flesh is heir to, from Abscess to Unknown. Of course this was nonsense and error. Better and more scientific was old Dr. G.'s prescription for a 'singing in the head,' viz., a poultice of old music books applied to the coccygeal region, with the luminous idea of 'drawing the music down!' There's revulsive treatment for you! But after all, good cometh from every new thing. Some of Expectant's patients get well, and that without regard to old time depurative theories. And some of these were cases whereof Dame Nature had not the handling. Dame Nature is a botch! To relieve a pleuritic inflammation, she fills one side of the chest with water; a remedy from which the patient might well pray to be delivered. But Expectant coming to a case of incipient pleuritis, makes a homœopathic diagnosis, calls it 'pain in the side,' and gives three or four grains of opium therefor. Patient gets well instanter. Well, Expectant

comes to another patient, finds her with low typhoid symptoms, and abdominal tenderness, but not much else to complain of. In the absence of any decided indications, Expectant prescribes his eternal opium. By-and-by this patient, too, recovers, and Mr. Observer, standing by, beholds a case of puerperal peritonitis cured by opium.

“From all this, I, sir, sitting in philosophic judgment, derive the great fact that we know nothing about opium. Tell us, old Monument, who have for forty years dealt out your opium, what know you about it? Did you ever dream that it was a *curative* you were handling, and not a palliative merely? Man of the microscope and testing tube, read for us this deep, this Sphynx-like riddle. Are we yet — we staunch old regulars — to yield the field in this matter? Is opium the curative means, the efficient drug in our mixed prescriptions of calomel et opii. \bar{aa} grs. \bar{ij} ? For look you, country practitioner dealing Schieffelin’s best powdered from the blunt point of your old jack-knife, you old Clysterpipe, who have not weighed a prescription these half-score years, your small doses of opium weigh three stout grains!

“*Rest!* That is the word. Here, in rest and sleep, ‘tired nature’s sweet restorer’ lies the secret. One sensible thing that Dame Nature does, is to put her patients to bed — to so prostrate their lusty muscles, that the sturdy knees give way, and the recumbent posture becomes necessary. Here, then, is the great curative, and opium is a means thereto. ‘Old Functional Harmony’ used to tell us with extremest unction, that ‘uterine contraction was the remedy for uterine hæmorrhage.’ Not lead, not ergot, not cold water, not compression, not the tampon, but any one, or all of these, which would bring on uterine contraction. So here in inflammation. Rest is our remedy — not calomel, not antimony, not bloodletting, not opium, but any one, or all, or none of these, so that you secure rest.

“There is a vague idea, derived from some exploded theories, that calomel has, *jure divino*, a certain control over inflammation — that the presence of calomel in the stomach, simultaneously with inflammation in the viscera, is incompatible. Now, good Sir Hunker! you know that I — Smelfungus — have the highest respect for you of the conservative school. You know that on every occasion I have ranked myself among you — have bowed to the existence of a liver — and scouted at the pretensions of these new comers, who bear that flaunting ‘banner with the strange device,’ *Young Physic*. But, my dear sir, we must compromise or surrender. Let us come down a peg or two and we shall still be men of note. Let us use a little Twigg’s hair dye, and rejuvenate ourselves — gray hairs are at a discount now-a-days. Look you! only a day or two since a certain divine declared a decided

preference for young physicians, over old. He had the hardihood to intimate that one good theory, sound and well established, was worth ten years' experience. He was a Scotchman, and I said to him, that 'it behoveth a Scotchman to be right, for if he be wrong he be forever and eternally wrong.' Between you and I, my aged friend, it is about time to cave. Now I have a talent for compromises, and I can propose a satisfactory arrangement which shall govern this vexed question of Calomel *vs.* Opium. Let us hereafter say nothing about being 'bilious.' That's all well enough at the bedside, but it is ruled out of professional intercourse. Let us give our calomel as we did of old to all patients of firm fibrinous habits, whose blood has a tendency to plastic exudation. So much we claim for our side of the house. Now we may as well, (needs must when the devil drives,) concede to Young Physic, that calomel shall be withheld in cases where this condition does not obtain, viz., in those manifold diseases where the blood has a tendency to fluidity. I fear that this will narrow down the amount of the drug used, but we must come to it. Do n't you recollect feeding calomel, for a fortnight on a stretch, to that strumous little girl with dysentery, last year? How fast she did gain, did n't she? And how nicely you could trace the curative influence of the calomel, could n't you?" And Smelfungus puts his tongue in his cheek, and makes a mysterious gesture with his thumb over the left shoul'er. And I can imagine Editor Flint perusing this backsliding confession of Smelfungus with a quiet smile and a chuckle, which means "I told you so!" But Smelfungus loves freedom of action; he cannot bear to be fettered by the green withes of tradition. Witness the motto at the head of this article.

But touch him gently, Dr. Flint! or you may yet see Smelfungus astride his old bilious Rosinante, charging the windmills of natural medicinae with the stern voiced war cry, "*Floret Calomelas — ruat coelum!*"

"Salts, sir, in all his steps — manna in his eye —
In every gesture, calomel and rhubarb."

Smelfungus loves fun, and from no recent occurrence has he derived so many good horse-laughes as from the developments at Bellevue *en re* of opium in puerperal peritonitis.

"Dr. Clark would have made no great stir with his interesting experiments in peritonitis, had he not been so severely criticised. Not but that Dr. Clark's treatment was a goodly instance of a *priori* reasoning applied to therapeutics — brilliant in its conception, and triumphant in its success. But the fun of the matter lies in the criticism of the New York Medical Gazette. 'This,' he says, 'comes of making hospital doctors of mere theorists.' He

tells us that we must look in the dead-house records for the results of such treatment. *Of course* the patients are dead. He stops not to inquire about it — they are dead *de necessitate*; and he sheds his tears over them as freely as a child in the measles. Smelfungus can see him; leading a lachrymose group of anxious inquirers beside the green shores and still waters of the East River. With solemn step and slow, he conducts them down the gravel walks of the garden at Bellevue, 'twixt cabbages and onion beds, and sadly points to the little dead-house, as filled with the sad mementoes of Dr. Clark's recklessness.

"But they look in vain — these women are not yet defunct, but still live to bend as best they may, with abdomen probulgent over their wash tubs, the spared monuments of human folly."

"Oh that mine enemy had written a book!"

If I were to tell you, gentle reader, all that Smelfungus says about this matter, I should detract from that solemnity which becomes the pages of a medical journal. A pompous dignity is the main-stay of our profession, and by a parity of reasoning a medical monthly should indulge in no unseemly cachinnations. *Vale.*

ART. IV.—*Two Cases of Peritonitis from Perforation occurring in Typhoid Fever.* Reported by AUSTIN W. NICHOLS.

PHILADELPHIA, Jan. 20, 1853.

DEAR DOCTOR:

The two following cases I have thought of sufficient interest to send you. If important enough to warrant an insertion in your Journal, they are at your disposal. They present the symptoms following perforation of the intestines during an attack of typhoid fever. In one of these, recovery took place; in the other, death terminated the disease.

I hope the class in the Buffalo University meets the expectations of its well-wishers. The Hospital presents advantages that I have not seen equaled in this city. I, for one, will return satisfied with the benefits to be derived from the teachings in the *Hospital of the Sisters of Charity*. With my best wishes for your prosperity, I remain,

Yours respectfully,

AUSTIN W. NICHOLS,
Jeff. Med. College.

CASE I. Typhoid fever. Peritonitis following perforation. Recovery.

A boy, aged 13 years, entered the Pennsylvania Hospital, Dr. Wood being in attendance. He had no aberration of mind; his countenance was dusky; tongue moist; skin hot and dry; pulse frequent, yet under 100; had diarrhoea; tympanites; gurgling; pain in right iliac region on pressure; no eruption; had epistaxis before his entrance; this was the first day he had taken to bed. The diagnosis was a mild attack of typhoid fever. The disease progressed favorably for ten days; there had been no necessity for medication with the exception of slight alcoholic stimulation; his diet was almost entirely of milk and farinaceous substances.

On the tenth day he was seized very suddenly with intolerable pain in the abdomen, shooting from the right side; his knees were drawn up and the cover was thrown off; excessive tenderness was found over the abdomen; his pulse was quick and small; his breathing costal and hurried; his countenance wore an anxious expression; his mind began to wander; these phenomena were sufficient to characterize a sudden attack of peritonitis.

The abdomen was immediately covered with a large blister, and this dressed with an emollient poultice. Morph. sulphat. gr. $\frac{1}{4}$ to $\frac{1}{2}$ was given every two hours, for two or three days; so that from three to four grains were administered in the twenty-four hours. At the same time, Mass. Pilul. Hyd. gr. j, was given every hour till some sensible improvement had taken place. His diet was restricted to arrow-root and milk. On the second day he began to grow better; and the inflammation subsided rapidly toward evening. For two or three days, he continued to amend, but his tongue was dry and his skin hot. Ol. Terebinth. gtts. x, ter die, was prescribed. From this time, he had no unfavorable symptoms; his appetite returned and gradually his strength. In a few weeks, he was about the ward; and now is perfectly well.

CASE II. Typhoid fever. Perforation. Death.

Girl, aged 9 years; began to complain on Tuesday of feeling ill. On the day previous, she was visited by her friends, who brought her cake and candy, of which she partook freely; some mild cathartic was administered, and the same medicine given on the next day, as the indisposition continued. The child took to bed on Saturday; and a tendency to sleep, and inattention to objects around her, was shown on this day. On Sunday evening, Dr. West called; the somnolency was prominent, sudamina on the neck were apparent, but there were no other symptoms to lead to a perfect diagnosis; there was a little fever, but no epistaxis, no diarrhoea, no tympanites, and no

signs of an eruption. The Dr. supposed it to be an incipient case of typhoid fever. Some slight medication was employed, and the diet was ordered to be of milk and farinaceous substances. On Thursday there was a decided improvement in all the symptoms excepting the continuance of the somnolency. On Friday, the characteristic eruption of typhoid fever presented itself. On Sunday, sudamina were again observed; the somnolency still continued; the child had but little medicine, some mild stimulant was given, and the above diet was continued. In the afternoon, delirium of a most violent nature set in; the patient had to be held in bed, and restrained from injuring herself. She continued in this condition till early on Monday morning, when she expired. During this state, leeches were applied to the temples, blister to nape of the neck, and large doses of morphia were given internally. The post-mortem examination revealed ulceration, very extensive, of Peyer's glands, enlargement of many high up in the ileum; and in one patch near the ileo-colic valve, a perforation was discovered, the peritoneum in the vicinity alone was inflamed; the ventricles of the brain contained a little excess of fluid, otherwise the organ was healthy; there were no lesions in any other of the viscera.

ART. V.—*Treatment of Acute Rheumatism.* By EDWIN R. MAXSON, M. D.,
Adams' Centre, Jefferson County, N. Y.

As acute rheumatism is a disease, the pathology of which has been a matter of doubt, and the treatment consequently various, I here offer a plan of treatment which I have found very satisfactory.

In a severe case, in which the inflammatory fever runs high, if the arms are suffering, I draw from two to four ounces of blood, by cups, from each side of the spine, near the origin of the brachial nerves, between the shoulders. If the lower limbs are suffering, I draw the same quantity, by cups, from each side of the spine, in the lumbar region, near the origin of the crural nerves. This generally relieves the pain in the limbs immediately, and checks the progress of the disease. I procure an evacuation of the bowels by mag. sulph. ℥ss., repeated, if necessary, and then give potassa nit. ℥i. dissolved in a tea-cup full of warm gruel, every three hours, and continue this till the fever and inflammatory symptoms subside, which will generally occur in from four to six days.

At this stage there is generally left slight swelling and some stiffness of

the joints. I then discontinue the nitrate and give potassa iodide, gr. x, with vin. colch. gtt. xx, every six hours. This should be continued till the slight swelling and stiffness of the joints subside, which may take place from four to six days. The appetite will, in the mean time, generally become good. The patient then need only be directed to take potassa iodide, gr. v, three times per day, for a few days more, to prevent a relapse and render the cure permanent.

Adams' Centre, N. Y., Jan. 20, 1853.

ART. VI.—*A Case of Pneumonia, accompanied by transient albuminuria.*

W. M., a laboring man, about 50 years of age, of a robust constitution, applied for medical advice on the 28th of January. He had been in his usual health till about ten or twelve days previously, when he was exposed to wet and cold during a hard day's work. From that time he had a slight cough, and the appetite was somewhat diminished. Three days afterward he again worked out of doors in the rain, became chilled and wet through, came home with a severe headache, and took to bed. The next day his headache continued severe, with chills, cough, and a sense of oppression in the chest. His appetite disappeared altogether and did not return. He had not much pain in the chest except occasional stitches on the right side, but had much pain in the back, and a general soreness of the limbs. He kept about somewhat, however, every day. On the 28th, as it was pleasant weather, he walked from his house up to the office of his family physician, a distance of three-quarters of a mile; and not finding the doctor at home, returned. Since then he remained in bed. He took two doses of cathartic medicine, which operated freely, and applied a "poor man's plaster" to the right side of the chest; but had no other remedy. He had already lost flesh considerably since the commencement of his illness.

At the time of the first visit, on the 28th, the patient was in bed, but immediately rose, and left his bedroom. He stood up during the examination of his chest, which lasted some twenty minutes, without appearing remarkably fatigued. He showed some emaciation. The countenance was rather pale. The skin, generally, was of a moderate warmth, not moist. A little coolness of skin about the face. Pulse 128, tolerably large and soft. Respiration 38, slightly labored. The tongue was moist, with a thin, whitish coat. There was considerable thirst, but no appetite. He had had some

vomiting, after drinking one or two cups of tea. Had had no headache since the first two days of his illness, but complained of feeling "weak." The voice was strong and clear; the cough loose, not very troublesome, not frequent nor painful. Had been rather restless the night previous, though he slept well part of the time. Had taken nothing since morning except tea. Had had one full evacuation from the bowels.

Examination of Chest. Has a moderate lateral curvature of the spine, in the dorsal region, toward the right side, and the whole of the right side of the chest is more prominent than the left. Posteriorly the left chest is everywhere resonant on percussion. It rises well in inspiration. Respiratory murmur pure, and the voice of natural resonance throughout, except for a little bronchophony at the root of the lung between the scapula and spine. No râles. The right chest, however, rises during inspiration much less than the left, and the fremitus of the voice, as felt by hand, much greater than on the opposite side. Resonance, on percussion, very slightly, if at all, diminished at any part, but throughout the middle third of the lung there is strong bronchophony and bronchial respiration, extending round quite to the side of the chest. At the lower and lateral parts of this portion of the lung there is a very fine crepitus on full inspiration and often cough; not otherwise. At the lower part of the right back, respiratory murmur pure, and voice natural. Anteriorly,—at the right apex the respiration is somewhat bronchial, but without crepitus. On the left side, respiration pure. On both sides, resonance, on percussion, natural.

He was ordered to keep in bed, and to take cold water for drink, and gruel for food.

℞. Antim. tart. gr. $\frac{1}{2}$.

in solution, every two hours.

29th. Reports better. Has less oppression in the chest, and no pain. Took four doses of the tartarized antimony, with only slight nausea and no vomiting. Had one copious loose discharge from the bowels after the third dose, and three more after the fourth;—when the medicine was discontinued. No discharges since. Says he "feels stronger" than yesterday P. M. Is now in bed, lying on back. Countenance as yesterday, except that it is a little less dull. Skin of moderate temperature, not moist. No coolness about face. Pulse 96, same in character as before. Respirations 32, not labored. Tongue as before. Expectoration since yesterday \mathfrak{z} ij. in amount, somewhat viscid, but not excessively so, containing bubbles of air, and of a uniform light-yellowish rusty color.

Examination of the chest gives the same results as yesterday, except that now there is distinct, though moderate, dullness on percussion in the middle third of the right back. The crepitus is very fine, but still has a moist sound. Respiration between spine and scapula tubal. Bronchophony most distinctly pronounced about lower angle of scapula, where the dullness is also most marked. No bronchial sound to-day in respiration in front of right chest. No r le. Resonance, on percussion, also, perfectly good on this point.

The urine since last visit, (17 hours,) is about \ominus j. ξ xij. in amount. It is of a natural acid reaction, specific gravity, 1023, and thickly turbid with yellowish-red urate of ammonia; looking like a mixture of weak coffee and milk. On being heated, the urate of ammonia is dissolved, and the urine becomes quite clear, but afterward, on boiling, is again clouded with a deposit of albumen, so as to become nearly as thick as at first. The filtered urine is also clouded by heat and coagulated by nitric acid.

Resume antim. tart. as before, but the medicine to be omitted if purging returns.

30th. Reports rather better than yesterday. Oppression less. No pain, except some on right side of chest, on full inspiration. Has taken medicine regularly, without vomiting, and with but little nausea. One loose f cal discharge, rather less than a pint in amount. Perspiration abundant this morning. Cough same. Pulse 88, of good character. Respirations 32. Expectoration since yesterday, about ξ ij. mostly similar in appearance, but lighter colored, and mixed with some bronchitic sputa. Countenance good, skin of natural temperature.

Examination of the chest gives much the same results as yesterday, except that the dullness on percussion, crepitation, and slight bronchial resonance of the voice have extended to the upper part of the lower third of the right lung posteriorly, and the crepitus in the middle third is rather coarser than before.

The urine, since yesterday, (24 hours,) is a little over \ominus ij. in amount, rather less turbid, with urate of ammonia. It is still albuminous, but the albumen less abundant.

Increase antim. tart. to gr. $\frac{1}{2}$, every two hours.

31st. Reports continued improvement. Has taken his medicine regularly. No discharges from bowels. No nausea. Much perspiration. Feels

comfortable. Wishes to sit up. Skin warm and moist. Pulse 80. Respiration 26. Tongue the same. Expectoration about ξ iii., not at all colored, but moderately viscid, and quite frothy. Urine, since yesterday, \ominus ii. ξ xiv.; still turbid with light yellowish-red urate of ammonia. Albumen continues, also, but is considerably less than yesterday.

Reduce medicine to gr. $\frac{1}{4}$, every three hours.

Feb. 1. As well. Countenance a little flushed. No diarrhoea. No nausea. Feels comfortable; only has some uneasiness on right side of chest on cough or long inspiration. The signs, on examination of the chest, were the same, except that the crepitus is rather less abundant, and bronchial respiration and bronchophony less strongly marked. Urine about \ominus iii.; still turbid, but deposit lighter colored and less abundant, though it is still copious. Albumen still perceptible, but very slight in amount.

Omit medicine.

2d. Has continued to improve. Slept well most of night. One discharge from bowels, nearly natural. Cough very moderate. Sat up half an hour without fatigue. Aspect good. Skin comfortable. Pulse and respiration as yesterday. Tongue a little cleaner. Some appetite. No thirst. Expectoration, ξ iii., nearly all watery and frothy; with only one or two faint streaks of blood.

Examination of chest shows the dullness on percussion, diminished. Crepitus much less abundant and bronchial respiration and bronchophony less pronounced than yesterday, though still rather strong. Respiratory murmur at the *bottom* of the right back has continued pure throughout. Urine a little less than \ominus ij, urate of ammonia about the same. No albumen perceptible.

May sit up an hour.

‘ Rice, soda biscuit, and chicken broth for food.

4th. As well, or better. One discharge from bowels yesterday. Now countenance natural. Skin in good condition. Pulse 80. Respirations 20. Tongue natural. Sat up two hours yesterday without fatigue. Appetite moderate. No thirst. Cough, and pain in right side, trifling. Expectoration since yesterday, about ξ iss. to ξ ij., perfectly colorless and frothy.

Examination of chest shows the signs of solidification still diminished in extent and intensity. They now occupy a space in the middle third of the

posterior part of the lung, about six inches by three. A little crepitus, on forced inspiration, just outside of this space; none elsewhere. Urate of ammonia in urine of yesterday, is diminished in amount, but there is, instead, a considerable quantity of red, crystalline uric acid as a deposit. Urine of to-day, contains some white urate of ammonia, but no uric acid. Very faint indications of albumen still remain.

5th. As yesterday. Pulse and respiration the same. Expectoration hardly ζi . in amount; in appearance as before. Appetite quite moderate. No thirst. Says he has had a disagreeable sensation at the stomach, approaching to nausea, till within a day or two, but none now. Some dullness and bronchophony remain in the posterior middle third of the right chest, but they have diminished since yesterday. No crepitus. Little or no bronchial respiration. Urine about ζiij ., still with a moderate deposit of white urate of ammonia. Albumen none, or doubtful.

7th. Up and dressed. Pulse 84. Feels every way well, except that appetite is not completely restored. Still feels weak. Is somewhat emaciated. Expectoration hardly ζss ., quite fluid. No physical signs of induration of the lung remain in the right chest, except a slight bronchial tone to voice, at about the centre of the middle third. No crepitus. Respiratory murmur pure. Urine has continued, yesterday and to-day, moderately loaded with the same light-colored deposit as before. No albumen.

9th. Has continued improving. Strength increased. Ate a piece of chicken to-day with relish, but the appetite is still only moderate, not craving. No pain. Has coughed only two or three times since yesterday. Countenance well. Pulse 88, soft and full. Patient thinks he has improved more since yesterday than any day yet. Urine since yesterday abundant, rather over \div iv. by estimate, *perfectly clear and light-colored, without any deposit of urate of ammonia.* No albumen.

11th. Better and better. Strength and appetite improving. No morbid symptom. Urine, since yesterday, moderate in amount, clear, and natural in appearance. No deposit or abnormal ingredient. After this time there was no recurrence of the difficulty.

The appearance of albuminuria during the course of acute inflammatory diseases, seems to be no unusual occurrence. It has been observed in pleurisy, pneumonia, pericarditis, bronchitis, inflammation of the brain and spinal cord, hepatitis, &c. In the preceding case its quantity diminished in proportion as the intensity of the pulmonary inflammation subsided. The same, also, was the case with the urate of ammonia deposit, which first became light-colored, and then diminished in quantity. It disappeared, however,

more slowly than the albumen, remaining after most of the other morbid symptoms had subsided. At the same time, the patient's appetite returned more slowly than is usual after cases of pneumonia. It will be noticed that the patient experienced the greatest improvement in his sensations on the same day that the morbid deposit disappeared altogether from the urine.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

The following case is one of great interest and value, provided the reader can feel assured of the credibility of the narrative. We do not, of course, doubt the honesty of the reporter, but the account is so extraordinary, that a due regard to caution cannot but suggest the possibility, if not probability that the medical attendant was deceived by the patient as respects complete abstinence for the space of three months. Instances of a perverseness of human nature leading to similar acts of deception, are unfortunately but too numerous. We copy the account, and our readers will judge for themselves whether to regard it as containing facts having most important bearings on physiology and medical practice, or an illustration of human weakness and depravity.—EDITOR BUFFALO MED. JOUR.

Case of Abstinence for three months, no Food, during that period, being taken into the stomach. By J. L. PIERCE, M. D., of Fallsington, Bucks County, Pennsylvania.

On the 13th of January, 1836, my friend, Dr. May, was about leaving Philadelphia for the East, and requested me to take charge of a number of cases of considerable interest, which he then had under his care. Among these was a lady, who had for many months been severely affected with disease of the stomach. As the exact character of the disease may admit of dispute, and as it is not this to which I wish to draw attention, but a peculiarity in the mode of treating it, I have headed the article in the manner it stands.

I called, with Dr. May, to see this patient on the 13th of January, 1836. She was about 26 years of age, sallow complexion, emaciated, pulse 120, feeble, but regular. A spot over the left epigastric region, of an inch and

a half in diameter, was very tender on pressure, and considerable tenderness existed for some distance around it. Her appetite, though not craving, was sufficiently good to enable her to eat almost any food, though she had been restricted to a very simple diet. Her bowels were open every two or three days, passages natural. She was in the habit of vomiting two or three times a day, and her food was generally rejected within a few minutes after it was taken. She had been sick about three months, and had been confined to the bed a large portion of this time.

I was informed that her case had been considered as carcinoma of the stomach. Her treatment had consisted of such articles as were calculated to allay the irritability of the stomach and the vomiting, and to quiet the severe pains of the epigastric region. A constant sore had been kept up over the affected spot by means of blisters, which were dressed with anodyne ointments, and occasionally leeches were applied to the spot.

On taking charge of the case, I watched it for some days very closely. I found that neither food nor medicine appeared to remain upon the stomach, but that as often as either was administered, vomiting ensued; and with the food ejected, there was from a tea-spoonful to a table-spoonful of matter, such as is usually discharged from an abscess. Should nothing be taken into the stomach for some hours, emesis would take place of this kind of substance by itself. During the first week, I pursued the same mode of treatment as had been made use of formerly, but I soon became satisfied that it was altogether useless, and that the patient must sink unless some more effectual plan was adopted. The simplest medicine taken into the stomach was rejected as soon as the severer kind, and even gum-arabic water or barley water was as irritating as oysters or roast beef could have been.

Toward the close of January, I asked her if she was willing to submit for one month to an entire abstinence of all kinds of food by the mouth, and be nourished solely by enemata. The idea was a novel one to myself, but my view was that the stomach must rest, and let the character of the disease be what it might, it would thereby stand a much better chance of healing. That there was a suppurating sore in the stomach appeared evident, and rest was absolutely essential. She was now exceedingly emaciated, and so feeble that she was entirely confined to the recumbent posture.

On the 1st day of February we entered upon our new mode of treatment. I directed lamb or mutton broth, of good quality, to be kept constantly on hand, and a half a pint to be used as an enema every three hours. During the first week, I allowed her to take not exceeding a tea-spoonful at a time of gum-arabic water or pure water, several times a day, with the request that she would lessen the frequency of it toward the latter part of the week, so as to be able to refrain from it entirely on the commencement of the second week. She very readily acceded to my wishes in every respect, and I have not the least doubt of her acting with perfect honesty toward me. I kept up a running sore over the epigastric region, which I dressed with simple cerate or basilicon ointment, sprinkled with morphia.

During the first week my patient vomited from three to six times daily, discharging pus, and a considerable quantity of matter resembling tubercles. Her sensation of hunger was at times great, but she bore it with remarkable fortitude. At the expiration of the week she was evidently more comfortable, and became increasingly so during each succeeding week of the month, at the close of which her condition was as follows: Vomiting occurs on an

average about twice a day; substance discharged the same as before, tinged with a little blood; tongue clean; pulse fuller than formerly, and less frequent; countenance more healthy. I now asked her if she were willing to continue the same treatment for another month. She did not hesitate in giving a prompt response; and during this month, not a drop or morsel of any thing passed her lips.

April 1. Condition in every way improved. The vomiting occurs once in two or three days; quantity less; appearance of discharge white, with but very few of those particles resembling tubercles; bowels regular; pulse 90; very little pain; strength improving.

I now requested her to continue the treatment one month longer. She consented to do so, and it was astonishing how rapidly the improvement progressed. By the middle of the month I considered the cure to have been nearly completed, but thought it best not to depart from the course we had been pursuing, lest irritation of the stomach should renew the vomiting, and other unpleasant symptoms. About the 24th of the month, I allowed her a tea-spoonful of gum-arabic water three or four times a day, with directions to increase it in frequency each successive day, so that the stomach might be gradually prepared for its usual nourishment. By the close of the month I felt satisfied that the disease of the stomach had ceased, that the sore had healed, and that my patient, by a forbearance and perseverance seldom equaled, had been rescued from an untimely grave. She was now able to sit up a considerable portion of the day, a part of the time attended to some needle work or knitting; could walk some distance without assistance; complexion good; pulse 80; bowels regular, and much fleshier than when I commenced my attendance upon her.

I continued calling on the patient occasionally, until the 18th of May, when I took my leave of her, with many blessings showered upon my head.

In consequence of her removal to Southwark, I lost sight of this patient for nearly two years, when I accidentally met her in the street. Her appearance had so much improved that I scarcely recognized her, and she assured me that she was in the enjoyment of uninterrupted health.—*Amer. Jour. Med. Science.*

Obstruction of the Bowels. Two Cases reported by E. D. FENNER, M. D.

CASE 1. A few weeks since, I was requested by Dr. Moss, of this city, to assist him in the post-mortem examination of a mulatto man aged about 35 years, who, after suffering repeated attacks of obstruction of the bowels, accompanied by great pain and stercoraceous vomiting, finally sank and died. He had suffered three attacks within the month previous to death. Dr. M. had attended him in several of them, and only succeeded in relieving him with great difficulty by means of free cupping, the warm bath, and Croton oil. These means succeeded in opening his bowels in his last attack, but he did not recuperate afterward. On examination after death, we found a *small piece of bone* lodged in the lower portion of the ilium. It had excited inflammation and *thickening of the intestinal walls* to such extent, as to cause an almost impermeable stricture of the canal. Here was the cause of death. The piece of bone was only about *three-fourths of an inch in length*, and

rather flat. The ends were not sharp, and the only wonder is, that it had not passed without difficulty.

Now let us see how much larger an amount of foreign substance did pass the entire extent of the alimentary canal, till it reached the anus, where it was impeded by the sphincter, and had to be removed mechanically.

CASE 2. On the 11th September, 1852, I was called to see a white female child, aged about two and a half years. I was told that she had diarrhœa with prolapsus of the rectum. No assignable cause was mentioned at the time. About three months previously, I had attended this child for an obstinate attack of diarrhœa, and relieved her entirely.

On this morning, the child did not appear to be much sick, I advised a little Hydrarg. C. Creta, to be followed by a dose of castor oil. A few hours afterward I was sent for, and informed that a piece of cork had been discovered in the child's anus. Upon reaching the patient, I found this to be the case; a large piece of cork was plainly visible. I readily succeeded in removing it with my finger; but this was not all. I continued to take away piece after piece, until I removed *nearly a handful*. The operation gave considerable pain, and caused slight hæmorrhage, but I removed all I could reach. I then prescribed a dose of castor oil, which produced a copious operation, and gave complete relief. A considerable quantity of cork came away some days afterward. We were then informed by a larger sister of this child, that she had often observed her with cork in her mouth, but did not know that she had swallowed it. Thus it is evident that this large amount of cork, some of the pieces as big as the end of my thumb, had been swallowed, and traversed the alimentary canal as low as the anus. There were, perhaps, a dozen pieces, twice as large as the piece of bone that caused the death of the man first mentioned. The quantity of cork passed completely filled a common match box.—*New Orleans Monthly Med. Register.*

Fatal Poisoning from Sugar Plums.—At the meeting, Saturday evening 20th November, Professor J. L. Riddell stated that, at the request of Dr. Geo. E. Harral, he had examined a handful of variously painted sugar plums. They were globular, about half an inch in diameter, and with a verrucose surface, somewhat resembling strawberries. About one-fourth of them wore upon the surface a deep orange yellow color, which was found to be due to *chromate of lead*.

The case of poisoning occurred as follows:

At 11, A. M., on the 18th instant, the slave girl, æt. 7, in perfect health, belonging to Mrs. Connellin, 50 Carondelet street, accompanied her mistress to the Poydras Market. A confectioner of that neighborhood, of whom Mrs. C. made some purchase, gave the girl a liberal handful of the sugar plums alluded to, which she ate. In the afternoon the child complained of headache, and had slight attacks of vomiting. Toward evening, the mistress becoming alarmed, sent for Dr. Harral, who responded promptly to the call, but arrived a minute or two after the child had ceased to breathe. Another handful of variegated sugar plums, was, without explanation, procured from the same jar, and submitted for examination, as alluded to.

Professor R. suggested for consideration, whether the Society should not

recommend the enactment of a city ordinance, aiming to prevent the sale of poisonous confectionery.

Professor Hunt stated that he had often observed the pernicious effects of poison candy in young children. He had seen distressing symptoms arise from candy colored with gamboge. He thought the attention of the city authorities should be called to the subject.

Dr. Fenner thought it entirely useless for the Society to trouble itself, for he felt assured, from past experience, that the city authorities could not be induced to notice the matter.—*Trans. of the Physico-Medical Society.—New Orleans Monthly Med. Register.*

Adhesive Plaster as a means of making Extension in the Compound and other Fractures of the Lower Extremity. By S. D. GROSS, M. D., Professor of Surgery in the University of Louisville.

UNIVERSITY OF LOUISVILLE, Sept. 27, 1852.

Gentlemen :—Will you be so kind as to grant me a little space in your valuable Journal for a few remarks on the subject of Adhesive Plaster, as a means of making extension in compound and other fractures of the inferior extremity? I am induced to ask this favor, because the origin of this mode of treatment, which has lately attracted considerable attention, and which has been adopted with great advantage, in several sections of our country, has been ascribed to a gentleman who is in nowise entitled to the credit of it, if credit it deserves.

In my work on the "Anatomy, Physiology, and Diseases of the Bones and Joints," composed within a few months after I was invested with the honors of the Doctorate, and published in Philadelphia in the summer of 1830, is the following passage:

"In complicated fractures of the leg, it not unfrequently happens that the soft parts about the ankle are so much contused, or otherwise injured, as to render it impossible to employ the usual extending bands. When this is found to be the case, the difficulty may usually be remedied by applying along each side of the leg, as high up as the seat of the fracture will admit, a piece of strong muslin, about two feet and a half in length, two inches and a half in width, and spread at one of its extremities with adhesive plaster. The part which is applied upon the limb should be confined by three or four circular strips, so as to keep it firmly in its place, and equalize the extending power. The free extremities of the extending hands should then be tied under the sole of the foot, and be secured to the block or bar which connects the lower ends of the splints. This mode of making extension, for which we are indebted to the ingenuity of my friend and preceptor, Dr. Swift, of this place, will, I am fully persuaded, be found highly useful in practice, and satisfactorily obviate the inconveniences to which I have alluded."

At the time of writing the work here quoted, I was spending a few months at Easton, Pennsylvania, where I had an opportunity of witnessing the excellent effects of this mode of management. Since that period I have omitted no opportunity of employing it in my own practice; and I have never failed, during the last thirteen years, to speak of it prominently before my classes in the University of Louisville.

Dr. Sargent, in his excellent little work on "Bandaging and other Operations of Minor Surgery," ascribes the credit of this method of extension to Dr. E. Wallace, of Philadelphia; and the same statement is reiterated in that gentleman's edition of Mr. Druitt's Surgery, published at Philadelphia in 1848.

Within the last two years, Dr. Josiah Crosby, of New Hampshire, has published a short account of this mode of treatment, in the *New Hampshire Journal of Medicine*, illustrated by several cases, in which it appears to have been adopted with the happiest effect. In one of them, a compound fracture of the tibia and fibula, the counter-extending band was applied to the upper part of the leg, and the extending band to the lower part of the leg and foot; the plan answered most admirably, and caused not the slightest inconvenience to the patient. Dr. Crosby states that he has healed two cases of fracture of the clavicle in children two years of age, with nothing but adhesive strips, with as good success as he had ever with the old methods, and with half the trouble. The same mode of treatment has been lately employed with great success in the New York Hospital, in fractures of the inferior extremities.

My conviction is that this plan of making extension deserves to be much more extensively employed than it has hitherto been by my professional brethren. It is particularly applicable to compound and complicated fractures of the leg, but it may also be advantageously resorted to in all cases of fracture of the leg and thigh, in which, on account of injury, excoriation, disease, or excessive morbid sensibility of the ankle, heel, or instep, it is impossible to use the ordinary extending means.

The limb should always, as a matter of course, be shaved before the bands are applied; and the substance of which these bands are composed should be of the most pliant and unyielding character. The adhesive plaster should also be of a very superior quality. The circular strips should not completely encircle the limb, lest they impede the return of the venous blood, and the leg should be carefully bandaged from the toes up, as in the ordinary mode of treatment.

I am, gentlemen, very respectfully,
Your friend and obedient servant,

—*Phil. Med. Examiner.*

S. D. GROSS.

Royal Medical and Chirurgical Society. On the Pathology of Rheumatic and Non-Rheumatic Pericarditis. By Dr. ORMEROD.

The author commenced by a reference to the researches of the late Dr. Taylor, who had satisfactorily shown that acute rheumatism was not exclusively the cause of pericarditis, and who had also called attention to the importance of granular disease of the kidney in reference to this morbid condition. The author desired to limit the use of the word pericarditis to present inflammation of the pericardium; and this analysis referred exclusively to classes of this nature. The means of investigation comprehended complete records of 1410 cases observed under nearly similar circumstances; that is, in the wards of different hospitals. Of these, 1249=88.59 per cent. were not cases of rheumatism; 161=11.41 per cent. were admitted on account of rheumatism, or suffered from it while under observation. Of the whole number,

85—6 per cent. had recent pericarditis, observed during life, or discovered after death, and were thus distributed:

24—1.92 per cent. occurred among 1249 non-rheumatic cases.	
61—37.88 per cent. " " 161 rheumatic cases.	
<hr/>	<hr/>
85—6 per cent.	1410

The mean age of 61 subjects of rheumatic pericarditis was about 21; the mean age of 24 subjects of non-rheumatic pericarditis was 42; the extremes being 7 and 63 years. As to the different causes of the pericarditis:

Rheumatic, -----	61 cases coincided with acute rheumatism.
Non-rheumatic of local origin,	7 ensued on inflammation of lungs or pleura.
	2 ensued on malignant disease of the pericardium.
	1 ensued on old cardiac disease.
Non-rheumatic of constitutional origin,	6 coincided with granular disease of kidney.
	4 coincided with hæmorrhage or exhaustion.
	2 coincided with scarlatina or erysipelas respectively.
	2 were inexplicable.

85

The date of the accession of pericarditis was determined in 33 of the rheumatic cases. The mean of these observations gave the 105th day of the rheumatic attack as that on which the pericardial complication most commonly supervened. The question, whether a first or second attack of rheumatism was more likely to be accompanied by pericarditis, was beyond the reach of hospital statistics. This source of information was silent, also, on the question, whether pericarditis be more likely to occur in severe or in the slighter cases of rheumatic fever. It might, however, be safely inferred, that the severity of the articular and pericardial affections bore no very close relationship to each other. It was certain that the most severe, even fatal pericarditis, might occur where there was but faint evidence of articular affection, and this latter condition might exist in the most aggravated and intense form without involving the addition of pericarditis to the other sources of distress. The author then entered upon the consideration of the subject of non-rheumatic pericarditis of local origin; and a question of importance here presented itself—what was the influence of pre-existent cardiac or pulmonary affections in inducing inflammation of the pericardium? The question was of equal importance in relation to acute rheumatism. The relation of pulmonary inflammation to pericarditis was thus illustrated: In the 1410 cases, the basis of this inquiry, some form of pulmonary inflammation—that is, pneumonia, pleuritis, or pleuro-pneumonia—was ascertained to exist, either by auscultation or dissection, in 265 cases. Of these:

117 had pneumonia,	of which 19 had recent pericarditis.
86 had pleurisy,	“ 6 “
62 had pleuro-pneumonia,	“ 8 “
<hr/>	<hr/>
265	33 — 12.4 per cent.

In the rheumatic class, pericardial inflammation commonly preceded, yet sometimes, though rarely, followed pulmonary inflammation. The non-rheumatic class told quite a different story; here pulmonary inflammation had apparently a distinct influence in inducing pericarditis, and this influence was most evident in cases of pleurisy; and clinical observation bore out the conclusion, that the pericarditis was subsequent to, and probably contingent on, the pulmonary inflammation. The author then referred to the comparative fatality of non-rheumatic compared with rheumatic pericarditis, and also to the desirableness of instituting an exact comparison between Bright's disease of the kidney and acute rheumatism, in respect to their tendencies to induce inflammation of the pericardium. In conclusion, the author desired to ascertain how far the results obtained by his present analysis agreed with those of the published cases of Dr. Taylor, who had made the subject of non-rheumatic pericarditis so peculiarly his own. The deductions seemed identical, and one rose from the perusal of those elaborate clinical reports with a conviction that non-rheumatic pericarditis was more within the province of the anatomist than of the physician. It was a disease with few or no symptoms, its physical signs recognized more often by a chance discovery than on the suggestions of the disease, and its morbid changes small in amount and apparently inactive; and where opportunity had occurred of watching the disease some time previous to death, it had been apparently without effect on the general symptoms, its presence or absence being determined by the ear alone; and still, in these, its connection with the fatal termination had appeared to be that of a coincidence rather than of a cause.—*Dublin Medical Press.*

Rapping Mania.—Our readers will bear us witness that from the modern inception of this terrible iniquity at Rochester, and the attempted mercenary speculation of transferring the "weird sisters" to this city to exhibit themselves to visitors at a dollar a head, this paper fully and fearlessly denounced all parties who gave the foul imposture its first impulse in New York, whether Clerical, Judicial, Legal or Medical, as the dupes of designing men and women conspired to deceive. We then took the early responsibility, not merely to deny anything supernatural or spiritual in the "mysterious knockings," but we boldly alledged a systematic scheme of villainy upon the women who were used, and the men who used them, to make money by deception, fraud, and imposture; and insisted that for obtaining money under false pretenses, all parties were open to conviction under our statute law, and we called on the District Attorney, Grand Jury and Courts, to do their duty; naming the Penitentiary as the due of the knaves, and predicting the Lunatic Asylum as the refuge of their dupes.

But finding our efforts fruitless, and our warning unheeded, and discovering that a portion of the public press had been subsidized to silence, and still another portion hired to echo the mystery, and give currency to the pretended revelations from the spirit-world, we renewed our exertions to arrest the unutterable mischiefs of the delusion. Foreseeing the certainty that multitudes would be led away by these deceivers, if they were not unmasked, and their ^{sed,} we sent forth a warning, not merely against the insanity
 it upon weak minds, but urging that the dethronement of

reason which would follow would be suicidal and homicidal, and hence predicted deeds of "darkness, infamy and blood," as the legitimate and unavoidable results, of a belief in these spiritual and ghostly revelations.

It was but a few weeks before the knavery of the weird sisters, Fish and Foxes, was proved and promulgated by Professors Flint and Lee, of Buffalo, two members of our profession, who nobly threw themselves into the breach in the vain hope of stopping the plague; and their exposure of these prime movers of the rapping mania, has since been verified by the Cincinnati Ladies' Committee so as to annihilate every doubt. Meanwhile insanity has gone on multiplying, and filling up our Asylums all over the country with its victims, and thousands more are tottering on its verge, bewildered and confounded by morbid imaginings of ghostly messages from the spirit-world. The deeds of darkness, infamy and blood, justly apprehended as the fruits of this form of insanity, have repeatedly been published in the suicides and bloody murders of both men and women, leaving widows and orphans to bewail their hapless fate. And all this because summary punishment has not been inflicted upon the guilty originators and propagators of this abominable imposture.

So far from visiting these offenders with the penalties of the law, there are clergymen, physicians, judges, lawyers, and even editors, who have marveled and doubted, and gravely said, there is something strange, mysterious, unaccountable in some of these revelations, and speculated on their causes; thus admitting the reality of the so-called phenomena, and conceding something supernatural, if not ghostly in them; and thus betraying all around them who do their thinking by proxy, into the belief of all the miracle-mongering, which the rappers and their victims alledge.

Hence, they have gone on from one imposture to another, from rapping and alphabets when these become stale, to bell-ringing, table-moving, singing, dancing, writing, discerning spirits, healing diseases, revealing truths and denouncing errors in religion, morals, science and philosophy, and all professedly from the ghosts of the departed. And the public press has done, and is doing, much to perpetuate the iniquity, by recording as facts the most absurd of these stories. In several of these presses, the movement of tables has been alledged as capable of being effected by a circle sitting around it, and touching it with their hands; thus giving color to the wildest of the ghostly stories, while disclaiming these and alledging electricity as the cause. But they forgot to tell, that the tables only move in the *dark*, and that there will be found under each moving table a stout negro, white or black, whose muscles furnish the locomotion. If any body alledges the contrary, we have a small table in our office on which we write, and we offer one hundred dollars to any ghost or medium, from this world or the other, who will move it an inch in daylight, by any supernatural, spiritual, magnetic or electrical influence, which shall be invisible and intangible to our own optics; and they may sit in a circle around it for a month, and "call spirits from the vasty deep, but will they come?"

Surely, sense has "fled to brutish beasts, and men have lost their reason," who can be so easily gulled to sacrifice their intelligence, their philosophy, their religion, and even their Bibles, to the ghostly rappings of these impostors, and even "pay for the poker." But we have done our duty, and will only again express our gratification at the fact that no regular physician within our knowledge has been seduced from his propriety by this stupendous

folly, and that Homœopaths are the only medical victims of the mania to which the honest among them have an intellectual proclivity, as they have ever had to the kindred impostures of animal magnetism. Thus far they have only been found among the knaves, and make money by the operation. We believe no one of them has yet become a dupe.—*New York Med. Gaz.*

Intermittent Fever in Chelsea.—General Summary of the Facts contained in the Report, and the Inferences therefrom.

First.—In 1789 a dam was erected at Chelsea, whereby the salt water was cut off from two hundred and seventy-five acres of land adjacent to the village. From this period, until 1816, various ineffectual endeavors were made to reduce this tract to cultivation. Finally, this plan was given up in despair, and the water again allowed to overflow the land; but of course it could not do so as easily and effectually as before, owing to the fact that there was only a narrow sluice-gate through which the tide was compelled to ebb and flow. In 1845 the gate was again closed, and has remained so ever since, whereby the old bed of the river and a large space of porous earth were left exposed to the rays of the sun and with a southern exposure.

Second.—It has been proved that certainly three, and probably four, cases of genuine fever and ague have arisen either on, or very near, the edge of the partially dried up marsh, and this since 1845, when the dam was finally closed. As these are the only well-authenticated cases* of indigenous fever and ague in this vicinity, and as similar results from similar causes have occurred elsewhere in Massachusetts, we infer that the dam has been the exciting cause of their appearance.

Third.—Chelsea has been more liable to dysentery than other places, as far back as any statistics will carry us, and since 1845, this tendency has been augmented. From these statistics we learn that Chelsea is about three times as subject to the malady in question as Boston, and twice as much as the state at large, Lynn and Gloucester.

Fourth.—Notwithstanding Chelsea is thus subject to the influence of fever and ague and dysentery, it is not a more unhealthy place of residence than the other towns with which it has been compared. The statistics of table 2 prove this.

Fifth.—May not the curious fact that Chelsea is more liable to fatal dysentery than other towns, while, at the same time, its annual percentage of deaths to the population is only equal to, and in some respects less than, the annual percentages of these same towns; may not this fact be an illustration of the great law of compensation which governs the liability of communities and nations to disease and death? Does it not point to the inference, that while certain influences tend to excite certain diseases, these same influences may check the development of others?

Sixth.—One final inference, of a very evident and practical character, the committee would make in concluding their report. It is as follows: Whenever any patient in Chelsea seems to have a tendency to diseases of an intermittent type or to dysentery, it becomes the duty of the medical attendant

* See exceptions to this. [See Appendix.]

to think of the propriety of removing him from the deleterious influences exerted on him.

HENRY I. BOWDITCH,
JOHN WARE,
EPHRAIM BUCK.

February, 1852.

Appendix.—After this report was presented to the Society, the committee were informed of the following very important case. By the kindness of Dr. W. H. Thayer, the committee are enabled to present it, as drawn up by him from his original notes:

Sept. 30, 1850.—John Mason, *æt.* 12. School-boy. Eliot-st. Has lived with his present guardian since six months old. Birthplace unknown. Has not been out of city over night since 2 years old. Always lived where now does, between Washington and Tremont streets.

Rather puny. Witnessed display of fireworks on 18th inst., on the common, sitting on the ground. The evening was too cool to sit in the house with open window, and there was a dense fog. The next day, having been previously well, had a chill in the afternoon, followed by heat, and that again by sweating. The next day (20th) he visited the Mechanics' Exhibition, was much fatigued, and had a more severe exacerbation of the same character than the day before.

The same attack has been repeated every day since, the chill being from 5 to 6½, P. M., followed by great heat, which ends by profuse sweating at 11, P. M. With the paroxysm comes headache, pain in neck and across small of back. At the first attack he had nausea—none since. He has lost strength and appetite, is very thirsty during heat; is up all day.

Now—Sept. 30th, 1, P. M. Up and dressed. Pale; cool; p. 70; tongue nearly clean; has no pain. During paroxysm, has some cough, which gives him a little pain in chest. None now. Nothing abnormal discovered on auscultation. Has taken a cathartic since first attack. No other treatment. One dejection daily.

℞. Quinæ sulphatis, gr. jss.;
Acidi tartarici, gr. ʒ. M.

Three times daily, avoiding paroxysm. Cold sponging during heat.

Paroxysms occurred as usual on the 30th of Sept. and 1st of October. On the first, without any pain in neck and loins.

On the 2d October, chill from 9¼ to 10, P. M., followed by heat till 2, A. M. Pain in neck and back severe. On the 3d October, no chill—but heat from 1 to 5, A. M., of October 4th. Quinæ s., gr. ij. four times daily.

Oct. 5th.—No chill nor heat. Sweating began at 2, A. M., and was profuse.

6th.—No chill, heat nor sweat. Feels well. Spleen cannot be felt; no tenderness over it. There was some tenderness across upper abdomen at the first visit, but spleen was not searched for. One dej. daily. Pill three times a day.

9th.—No chill nor heat since last visit. Sweats freely every night, but this is his habit. Is at school. Mother reports him very well.

Omit medicine.

By the following still more curious document, brought to the notice of the

committee by Dr. Z. B. Adams, we have undoubted proof that genuine intermittent fever formerly prevailed, and was quite fatal on some parts of the eastern shores of Massachusetts.

These data are extracted from a diary kept by the Rev. Noadiah Russell, in 1682 and 3, while a tutor at Harvard College. By the College Catalogue this gentleman appears to have received his degree of A. B. in 1681. He died in 1713. When writing his diary of everyday events, he scarcely could have hoped to have been the means of illustrating the history of disease in New England. The original diary, fragments of which alone remain, is now in the possession of his descendants, and has been recently published. (See Drake's New England Historical and Genealogical Register, &c., January, 1853, page 53.)

"16th day 6 month (Aug. 16, 1682). * * The next day being Fry-day I went to wait on some company to Lynspring where for company's sake drinking too much cold water I set myself in an ague wch came again on Sabbath day and Tuesday.

* * * * *
 "10 6 (Aug. 10, 1683) Samuel Gardner a student of ye College, of 2 years standing prompt for learning exemplary for piety and Sobriety died at Salem of ye feaver at which time many were visited with ye feaver and ague which was very mortal."—*Boston Med. and Surg. Journal*.

Case of Hermaphrodisism, involving the Operation of Castration, and illustrating a new principle in Juridical Medicine. By S. D. GROSS, M. D., Professor of Surgery in the Medical Department of the University of Louisville.

THE following case, which came under my observation in 1849, will, if I mistake not, prove both novel and interesting to my professional brethren. So far as my information extends, there is no account of any operation for a similar object upon record.

The subject of the case, at the time I first saw her, was three years of age, having been born the 10th of July, 1846. She had always, up to this period, been regarded as a girl, and had been so pronounced at her birth by the accoucheur. At the age of two, however, she began to evince the tastes, disposition, and feelings of the other sex; she rejected dolls and similar articles of amusement, and became fond of boyish sports. She was well-grown, perfectly healthy, and quite fleshy. Her hair was dark and long, the eyes black, and the whole expression most agreeable. A careful examination of the external genitals disclosed the following circumstances: There was neither a penis nor a vagina; but, instead of the former, there was a small clitoris, and, instead of the latter, a superficial depression, or *cul-de-sac*, covered with mucous membrane, and devoid of every thing like an aperture, or inlet. The urethra occupied the usual situation, and appeared to be entirely natural; the nymphæ were remarkably diminutive; but the labia were well developed, and contained each a well-formed testis, quite as large and consistent as this organ generally is at the same age in boys. Her hips and chest, thighs and superior extremities, were perfect.

It being apparent, from the facts of the case, that it was one of malformation of the genital organs usually denominated hermaphroditism, the question occurred whether any thing could or ought to be done to deprive the poor child of that portion of the genital apparatus which, if permitted to remain until the age of puberty, would be sure to be followed by sexual desire, and which might thus conduce to the establishment of a matrimonial connection. Such an alliance, it was evident, could eventuate only in chagrin and disappointment, if not in disgrace, ruin of character, or even loss of life. Certainly, impregnation could never occur, and even copulation could not be performed, except in the most imperfect manner.

I need not say that I gave the subject all the consideration and reflection that I was capable of bestowing upon it. I was deeply sensible of the responsibility of my position. A new question, involving the rights and happiness of my little patient, and the dearest interests of her parents, was presented to me. I examined the case in all its bearings and relations—moral, physiological, and juridical; I appealed to the records of my profession for a precedent, and I sought the counsel of medical friends. The parents were anxious for an operation; they were intelligent, kind, and tender-hearted, and were willing to sacrifice every thing for the welfare of their child. Their only object was to save it from future suffering and misfortune. My own mind was made up; but, before I proceeded to take any further steps, I determined to consult my excellent friend and colleague, Professor Miller, in whose judgment and integrity every one who knows him has the utmost confidence. He saw the child and examined her. He viewed the case, as I had previously, in every possible aspect, and his conclusion was, that excision of the testes was not only justifiable, but eminently proper under the circumstances; that it would be an act of kindness and of humanity to the poor child, standing as she did toward society in the relation not of a boy or a girl, but of a neuter, to deprive her of an appendage of so useless a nature; one which might, if allowed to proceed in its development, ultimately lead to the ruin of her character and peace of mind.

Backed by such authority, I no longer hesitated what course to pursue. I performed the operation of castration on the 20th of July, 1849, aided by my pupils, Dr. D. D. Thompson, of this city, Dr. Greenburg R. Henry, of Burlington, Iowa, and Dr. William H. Cobb, formerly of Louisville, now of Cincinnati. The little patient being put under the influence of chloroform, I made a perpendicular incision, about two inches in length, into each labium down to the testis, which was then carefully separated from the surrounding structures, and detached by dividing the lower part of the spermatic cord. The arteries of the cord being secured with ligatures, the edges of the wound were brought together with twisted sutures, and the child put to bed. Hardly any blood was lost during the operation. About two hours after, the left labium became greatly distended and discolored; and, upon removing the sutures, the source of the mischief was found to be a small artery, which was immediately drawn out and tied. No unpleasant symptom of any kind ensued after this, and in a week the little patient was able to be up, being quite well and happy.

The testes were carefully examined after removal, and were found to be perfectly formed in every respect. The spermatic cords were natural.

I have seen this child repeatedly since the operation, as her parents live only a few squares from my office, and have carefully watched her mental and

physical development. Her disposition and habits have materially changed, and are now those of a girl; she takes great delight in sewing and housework, and she no longer indulges in riding sticks, and other boyish exercises. Her person is well developed, and her mind uncommonly active for a child of her years.

I would fain present this example as a precedent in similar cases. The reasons which induced me to recommend and perform this operation, in the instance before me, have been already mentioned, and now, after a lapse of three years, I have no cause to regret the undertaking, or to think that I acted harshly and inconsiderately. If the records of surgery and medical jurisprudence are silent upon the subject; if the learned doctors of the Sorbonne, the fathers of the Royal Academy of Paris, and the Fellows of the Royal College of London have left us no precepts; and if the experience of the present day furnishes no examples; all this, and much more, does not prove that the practice here recommended is not perfectly just and proper, and vindicated upon every principle of science and humanity.

A defective organization of the external genitals is one of the most dreadful misfortunes that can possibly befall any human being. There is nothing that exerts so baneful an influence over his moral and social feelings, which carries with it such a sense of self-abasement and mental degradation, or which so thoroughly "maketh the heart sick," as the conviction of such an individual that he is forever debarred from the joys and pleasures of married life, an outcast from society, hated and despised, and reviled and persecuted by the world. Nothing but the most perfect resignation, and a well-founded confidence in the mercy and justice of the Creator, can render the lot of such a being at all supportable.

The subject of doubtful sex is one which has always, in all ages, and in all civilized countries, excited the warmest attention of the physiologist, the philosopher, and the medical jurist. Under the vague and ill-chosen name of hermaphrodisism, invented at an early period of the world, was described every imaginable form of malformation of the genital and urinary organs, most dissimilar in character; and, consequently, were calculated to mystify and mislead the public mind. A class of beings was imagined, combining, it was said, the qualities of the male and female in the same individual, and capable of performing, within itself, the generative functions. The idea that such a union might exist, had its origin, no doubt, in fable. The reader of mythology need not be reminded here of the story of Hermaphroditus and the nymph Salmacis; how the former so ungallantly resisted the charms and entreaties of the latter, and how, finally, through the interposition of the gods, their bodies were united into one. The ignorance of medical men, the conceit and folly of legislators, and the mercenary conduct of many of the subjects of this variety of congenital malformation, served afterward, in no small degree, to perpetuate the error thus engendered, and to transmit it, in nearly all its ancient force, down to a comparatively recent period. Modern researches had done much to dissipate these absurdities, when the publication, in 1836, of the great work of Mons. Isidore St. Hilaire, entitled *Histoire des Anomalies de l' Organization*, set the long agitated question forever at rest, by demonstrating, in the most undeniable and conclusive manner, that there is no such thing as hermaphrodisism, in the vulgar acceptance of the term; or, in other and more philosophical language, that the union of perfect male and

female organs in one and the same individual, is an anatomical and physiological impossibility.

Much prejudice, leading often to the most cruel persecution, existed against this class of individuals among some of the nations of antiquity. The Athenians had a law, providing that all hermaphroditic children should be consigned to the flame; while the Romans ordained that they should be boxed up and thrown into the sea. In more recent times, all individuals of this description were excluded from holy orders, and from the office of judges, "because they were ranked with infamous persons, to whom the gates of dignity should not be opened."* Much of this prejudice has, fortunately, disappeared, under the benign influences of Christianity and civilization; but much still remains, and must continue in operation, as long as the human mind retains its present organization. If hermaphrodites are no longer burnt and drowned, stoned and persecuted, and mocked and reviled, they are universally regarded with a degree of prejudice, amounting, generally, to positive aversion; and as unfit for any offices of dignity, divine, legal, or political. If such be the fact, and no one can doubt it, every suggestion, calculated to ameliorate the condition of this unfortunate class of beings, by depriving them of their only incentives to matrimony, and thereby dooming them to everlasting celibacy, should be hailed as a valuable contribution to the science and humanity of the present age.—*Am. Jour. Med. Sciences.*

Should the use of White Lead as a Paint be forbidden by Public Authority?—This question is exciting considerable interest in France, one of the few countries in Europe where a due regard to the public health is part of the business of government. In England, and this country, we are too jealous of individual rights, too independent, if you please, to allow our rulers to watch over the well-being of the community.

We shall therefore merely present facts, without comment, as given to us in a memoir of Dr. H. DE CASTELNAU:

"In his remarkable memoir on *Painting with White Zinc*, Dr. Bouchon advised the government, if it had due regard for the health of workmen, to forbid the use of white lead as a paint on all the public buildings, and that an example should be presented for imitation by the substitution of an article less deleterious. The favorable manner in which this proposition was received by the Academy of Medicine, at its session on the 4th of November, 1837, indicates their full accordance in the idea, although they were necessarily restrained from entering into the merits of the question of economics, and we derive a similar indication of opinion in the large premium bestowed by the Institute, in 1849, on M. Le Claire, for his essay on the means of rendering occupations less unhealthy.

All these circumstances have doubtless tended to aid in diffusing a report that government is about suppressing the manufacture of white lead. To aid such a measure, a few details on the point of sickness and mortality will be of use.

In accordance with a requisition from the prefect of Police, the administration of hospitals demanded an annual return of all cases admitted into

* Beck's Medical Jurisprudence, vol. i. p. 106, fifth ed. 1835.

them of diseases from lead. It thus appears, that during ten years, (1838 to 1847,) 3142 were admitted, and that 112 of these died, being an annual mean of 314 sick and 11 dying. There can, however, be scarcely a doubt that the first number is too low. There is very frequently a doubt as to the nature of the complaint on admission—indeed, lead affections take some time to develop themselves, and thus cases are frequently referred to other diseases. It is highly probable that at least 400 cases are annually admitted, and that fifteen deaths occur.

Of the gross number, (3142,) three-fifths (1898) were cases of workmen engaged in the manufacture of white or red lead, and the remaining two-fifths were persons employed in using these products, as painters, grinders, makers of porcelain cards, (so called,) &c.

Then again, there are many cases treated at their own dwellings, but unfortunately we have no data exactly to estimate their number. It is quite probable that they are at least equal to those treated in hospitals, and if this be conceded, we have annually 400 cases of lead disease in those who are strictly manufacturers of the preparations of lead, and of which 14 die. It would be too extravagant to carry this proportion throughout France. Reducing it nine-tenths, and with a due regard to the statistics of provincial hospitals, we are certainly safe in stating the total annual result at 2000 cases of disease and 80 deaths. These would be at an end with the suppression of the manufacture.

But there is another matter to be also considered. The average sojourn of a patient with saturnine disease in a hospital is sixteen days. Add to this the illness and loss of time that precedes, and the debility, broken health, and loss of business that follows so many of the workmen. Even if we do not estimate this last, still, the hospitals will be relieved annually of sixteen or seventeen thousand days of sick persons, not to take into account the permanent residence of many incurables.

Can there, then, be a doubt that the public health will be greatly improved by the suppression of these manufactories? Still, however powerful may be the arguments in favor, it would hardly answer to attempt their suppression, unless we could find a proper substitute, both in the healthiness of its manufacture, and its value in the arts. Can both of these objects be accomplished by the employment of the white oxide of zinc? (*le blanc de zinc?*)

As to the first, Dr. Bouchut, just at the time of concluding his memoir, in July, 1851, received the following return from the company manufacturing zinc at Asnieres. Up to the date named, they had employed 151 workmen, who together had performed labor during 31,585 days, and had been in the factory 36,156 days. In other words, the average was 209 labor days, and 344 days of residence for each person.

It is scarcely possible to present a more favorable bill of health. Whoever heard of a manufacturer of white lead remaining in its manufacture during 344 successive days? Besides, most of the above workmen still remain, and are able to count upward of 1000 labor days.

Dr. Bouchut has carefully studied what should be called the *effects* rather than the *diseases* caused by this species of manufacture. They are as follows:

1. Pains in the throat and slight cough occur during the first days of labor, until the mucous membrane becomes accustomed to the exhalations from the white zinc. But they disappear very soon, and the workmen there

are no more subject to cough and throat affections than the same given number of any other persons.

2. Many of the workmen are at various periods subject to a curious species of innervation, shown by febrile or non-febrile restlessness at night. But this does not affect the general health, and they return in the morning to their labor. Occasionally, there is a species of excitement, temporary, such as Delaroche and Barbier ascribe to the oxide of zinc, but with most it is the short feverish feeling just described. It is always of short duration, never dangerous, and disappears after the system has become accustomed to the employment.

3. Occasionally eruptions appear on the skin, of reddish papulæ, which readily disappear with proper treatment.

Having thus noticed the effects, Dr. Bouchut proceeds to mention three cases of slight disease, ascribed to this cause. But a careful analysis proves that they were not owing to it.

Here, then, we have results which are frequently produced by emanations from the *most harmless substances*, when inhaled in the form of powder. The difficulty only extends thus far. But while white lead, as powder, causes its severe results also, we must recollect that it is equally noxious when manipulated in the humid form. From this, however, white zinc is totally free. It is only the powder of it that affects the workmen.

We should also remember the large doses that have for many years been administered of the white oxide as a medicine, without causing any accident. M. Orfila, the highest authority in toxicology, gave 20 grammes () to small and feeble dogs, with only the result of gentle vomiting, and a subsequent perfect recovery. How very different are the consequences of administering white lead.

As to the economic value of white zinc: it can be manufactured for exactly the same price as white lead, and being much lighter, a larger quantity can be sold for the same sum of money. It cannot be adulterated. This, indeed, has been made a formidable objection to it. White lead is very commonly mixed with sulphate of barytes, and not unfrequently with chalk. White zinc can be used with equal facility as a paint. It does not dry as readily as white lead, but the difference in time is small. It has been objected that it does not set well as a paint, but this is altogether a mistake. Two coats cover wood *very nearly* as well as white lead, and there is this further advantage, the vapors of sulphuretted hydrogen do not affect it, while all the preparations of lead turn black from them.

M. Leclair, an eminent house-painter, and others, have verified its use, on more than two thousand buildings, some of them public ones, to the satisfaction of the community.

The results, then, of suppressing the use of white lead by public authority will be—to save annually the lives of eighty workmen—to prevent 2000 cases of disease, some of them, indeed, incurable—and to enable active industry to continue its labors uninterrupted.—*Abridged from La Lancette Francaise (Gazette des Hopitaux) American Jour. Med. Sciences.*

Binocular Microscope.—At a meeting of the Physico-Medical Society, on Saturday evening, 2d October, Prof. J. L. Riddell called the attention of

the society to an instrument of his own invention and manufacture, which promises to be of incalculable advantage in microscopic researches, especially in the prosecution of microscopic anatomy and physiology.

He remarked that he last year contrived, and had lately constructed and used, a combination of glass prisms, to render both eyes serviceable in microscopic observation. The plan is essentially as follows: Behind the objective, and as near thereto as practicable, the light is equally divided, and bent at right angles, and made to travel in opposite directions, by means of two rectangular prisms, which are in contact by their edges, that are somewhat ground away. The reflected rays are received, at a proper distance for binocular vision, upon two other rectangular prisms, and again bent at right angles, being thus either completely inverted, for an inverted microscope, or restored to their original direction. These outer prisms may be cemented to the inner by means of Canada Balsam; or left free to admit of adjustment to suit different observers. Prisms of other form, with due arrangement, may be substituted.

This method proves, according to Prof. Riddell's testimony, equally applicable to every grade of good lenses, from Spencer's best sixteenth, to a common three-inch magnifier, with or without oculars or erecting eye-pieces, and with a great enhancement of penetrating and defining power. It gives the observer perfectly correct views, in length, breadth and *depth*, whatever power he may employ; objects are seen holding their true relative positions, and wearing their real shapes. In looking at solid bodies, however, depressions sometimes appear as elevations, and *vice versa*, forming a curious illusion; for instance, a metal spherule may appear like a glass ball silvered on the under side, and the margin of a wafer may seem to ascend from the wafer into the air.

With this instrument, the microscopic dissecting knife can be exactly guided. The watchmaker and artist can work under the binocular eye-glass with certainty and satisfaction. In looking at microscopic animal tissues, the single eye may perhaps behold a confused amorphous or nebulous mass, which the pair of eyes instantly shape into delicate superimposed membranes, with intervening spaces, the thickness of which can be correctly estimated. Blood corpuscles, usually seen as flat disks, loom out as oblate spheroids. Prof. R. asserted, in short, that the whole microscopic world could thus be exhibited in a new light, acquiring a tenfold greater interest, displaying, in every phase, a perfection of beauty and symmetry indescribable.—*N. Orleans Monthly Med. Register.*

Autopsy of the Hon. Daniel Webster.—[From an article in the American Journal of Medical Sciences, we extract the following description of the post-mortem appearances, &c., by his physician, Dr. John Jeffries.]

The autopsy was made by Dr. J. B. S. Jackson, who furnishes the following report:

Autopsy thirty-two hours after death; present Drs. Jeffries, Porter, J. Mason Warren, Wyman, Parkman, and Jackson.

The emaciation was very marked, as shown by the state of the integuments and muscles; the latter being wasted, pale, and flabby.

Abdomen.—The peritoneal cavity contained eleven pints of serum. There were also old and strong adhesions about the spleen, the gall-bladder, the

cæcum, and to a small extent between the left extremities of the arch of the colon and the parietes of the abdomen.

The stomach was distended, and contained half a pint of very dark blood, about one half of which was in the state of a soft coagulum; and this was the only appearance that was found of coagulum in any part of the body. The mucous membrane was deeply stained by the contents, generally rather soft, and in the pyloric portion somewhat mamellonated. The intestines were opened throughout, washed, and fully examined with reference to the diarrhoea that had so long existed. Blood was found throughout in very considerable quantity as far as the descending colon, below which there was no trace of it; in the large intestine it was altered as usual in color. Mucous membrane stained by the contents so far as blood extended. In the large intestine were numerous herniæ of the mucous membrane, so common in this situation; from many of these, small masses of feces or of mucus could be forced out, and these were the only traces of feces that could be found. Otherwise the mucous membrane of the intestines appeared quite healthy; there being nowhere any ulceration to explain the diarrhoea, nor ecchymosis connected with the hemorrhage.

The liver was throughout very markedly granulated; dense, and contracted in size; the color externally was greenish or bronzed, but internally everywhere of a pale red; showing, as we may not very unfrequently observe, the inappropriateness of the term "cirrhosis," which would generally have been applied to the present case. Weight of the organ, three pounds and one-third, avoirdupois. Bile in the gall-bladder nearly black, and of a tarry consistence.

Spleen small, pale, and shriveled. Investing membrane to some extent opaque, white, thickened, and condensed; this change being probably due to the old peritoneal affection.

Kidneys and pelvic organs healthy.

Thorax.—Old pleural adhesions over nearly the whole of the right side; none on the left. Lower lobe of the left lung and the two lower lobes of the right much congested, and very dark; a change that undoubtedly occurred toward the close of life, being simply passive.

Heart flaccid; very little blood in cavities, and this was quite liquid. Slight disease of aortal valves, but organ otherwise healthy. Foramen ovale; a small valvular opening existed. Aorta not ossified except to a small extent in the abdomen.

Head.—The membranes of the brain were most remarkably diseased. In the cavity of the arachnoid was a layer of fibrine which covered almost entirely, and about equally, the convexity of both hemispheres; It did not extend, however, beneath nor between them, nor about the cerebellum. In the recent state, it had a rather dull, yellowish, infiltrated, oedematous appearance; being one-fourth of an inch in thickness over the upper surface, but becoming gradually more thin on the sides, where it terminated in a thin edge. The adhesion to the dura mater was in some parts quite close; but it was generally very readily stripped off, and left the arachnoid with its usual polish. It was more adherent to the subjacent membrane; this last being irregular, and having generally a clouded and slightly opaque appearance, with many milk-white spots, but without any appreciable thickening. The quantity of serous effusion into the membranes was altogether large. The subarachnoid tissue, corresponding to the layer of fibrin above described, was

infiltrated with a straw-colored serum in some places, separating the convolutions from each other; this separation was quite remarkable at the posterior part of the right cerebral hemisphere on its upper surface and near the median line, there being also a slight depression at this part. The dura mater adhered firmly to the calvaria, but was healthy in structure, as were the membranes otherwise; there was, however, a serous infiltration into each plexus choroides; though no more, if not less than usual, into the lateral ventricles. No appearance of recent meningitis; and no effused blood or cysts in or about the false membrane. The brain itself was perfectly healthy; and the arteries at the base very nearly so. Cranium healthy. Over the right frontal region a scar existed, the result of the injury that occurred last May; integuments not otherwise remarkable.

A portion of the fibrine from the arachnoid cavity having been removed for microscopical examination, it was found, some hours afterward, and when the serum with which it had been infiltrated was absorbed, to have almost the consistence of one of the natural tissues of the body; being strong enough to bear considerable traction; it also appeared then to have somewhat of a laminated structure, and blood-vessels were distinctly seen in it, even with the naked eye. Dr. Wyman found it "organized, and, in some places, vascular. Under the microscope, the lymph was resolved into minute fibers, like those forming the white fibrous element of areolar tissue, and including in their meshes large numbers of minute granules."—*N. York Med. Gaz.*

Quinine and Opium in the Cold Stage of Paroxysmal Fevers.—We have long thought it a desideratum in practice to find out some mode of treatment by which the practitioner may be enabled, when summoned to a case of remittent fever, *during the cold stage*, to cut short the paroxysm—to extinguish the intense febrile reaction, which usually succeeds the cold stage of our summer and autumnal diseases. When called upon to visit a patient, and we find him shivering with a chill—with blue lips and fingers—goose-flesh, shrunken surface—quick, small, feeble pulse—more or less nausea—great thirst—and in a word, laboring under the usual distressing symptoms of the cold stage of fever, we usually content ourselves with warm foot-baths, tepid drinks, etc.; but these domestic remedies serve only to hasten and augment the reaction; they do not even abridge the febrile paroxysm which must succeed the chill.

We are about to recommend measures which will meet the indications so much desired in these cases, such as we have frequently tested in hospital, and sometimes in private practice. Macintosh practiced blood-letting in the cold stage of fever; but if this treatment proved beneficial in some cases, it operated injuriously in others; and thus, by common consent, venesection in the cold stage of fever has been abandoned in the United States, as far as we know.

Full doses of quinine and opium, given in the midst of the cold or shivering stage of fever, will be found both safe and efficacious in the large majority of cases. It puts a stop to the cold or chilly stage, increases the fullness, while it diminishes the frequency of the pulse, allays, as by enchantment, the violent neuralgic pains with which the head, back, limbs, etc., of the patient are tortured, equalizes the circulation, promotes free perspiration,

and rarely fails to extinguish the intense febrile reaction which, without the interposition of our art, rarely fails to succeed the cold stage of our autumnal fevers. Opium and quinine, thus administered, seem to restrain the violent action of the heart and arteries, the former of which is curbed, so to speak, by the combination, and held within its normal force and frequency. The patient passes at once from the chill or chilly, into the sweating stage—characterized by a full, soft and regular pulse, a warm, moist, and relaxed surface, absence of thirst, headache, restlessness, and the usual concomitants of intense febricity. He exchanges great suffering, pains and uneasiness, for sweet and refreshing slumbers, for the most part, from which he awakes at the end of a few hours, perfectly delighted with himself and his physician.

By this treatment, we arrest the paroxysm more certainly than if we had administered the quinine during the apyrexia; the series of morbid phenomena by which a paroxysm of intermittent fever is characterized, is broken up; and the quinine and opium appear to arrest the disease definitively, by extinguishing the germ of morbid action. The dose, in the instances recommended, should be from 20 to 25 grains of the sulphate of quinine, and from 2 to 4 grains of opium, in combination; it may be repeated in severe cases, but in diminished doses, after the first three or four hours.

Quinine and opium, given at the moment advised, are less likely to disturb the encephalon than might be suspected by those who have never ventured to try it in the cold stage of fever, to adopt a misnomer. Treated after the method above recommended, the paroxysm is broken up, and will rarely recur the second time, although we may withhold the further use of remedies. Under our plan, convalescence becomes speedily established, the patient rapidly recovering his appetite and strength. Many, and we include ourselves, venture to administer the quinine and opium during the height of the febrile excitement, with the most beneficial effects; but it must strike any reflecting mind, that if opium and quinine, given during the stage of exacerbation, exercises a sedative influence over the heart and arteries, how much more rational and easy to keep down such febrile excitement, by interposing our remedies before all the links in the chain of morbid causes which constitute a febrile paroxysm, shall become firmly united!—*N. Orleans Med. and Surg. Journal.*

Museums of London.—The following account of the Museums of London, is interesting in its reference to the lives and labors of the distinguished men originating them. The account is written by a correspondent of the *Dublin Medical Press*, author of a series of letters in that able print, entitled *Medical Life in London*—letters, as severe as racy, from which we have heretofore copied some extracts.—EDITOR BUFFALO MED. JOURNAL.

“Our greatest museum in London was founded by an Irishman, Sir Hans Sloane; the great College of Surgeons’ collection by a Scotchman, John Hunter; while at the College of Physicians, poor Harvey was nine years illustrating his doctrine of the circulation, with preparations, all in vain ‘He fell mightily in practice,’ we are told; and in London it was believed

'that he was crack-brained, and all the physicians were against him.' His 'therapeutic way' was not admired, and he was left on the high road to starvation but for King Charles. These facts, at least, should make us a little modest. It is good for us occasionally to think over the lives of such men. Hunter dying in debt, and his magnificent collection going begging, refused by this College of Physicians, and grudgingly received into its present situation. Sir Hans Sloane giving away thirty years' income as charity, but he himself now half forgotten.

"Sir Hans Sloane was born, according to some documents in the Library of his splendid collection at the British Museum, in the County Down, Ireland, at a place called Killeagh, April 16, 1660. He stated in his will that the collection he was bequeathing the nation was richly worth £80,000; it contained 200 volumes of dried plants in the form of a *hortus siccus*, 30,000 minerals and other specimens of great interest in natural history, with a library of fifty thousand volumes, and 3566 very rare manuscripts. There are two pictures of this great and talented Irishman in the museum. One would like to see them more generally known. It appears from the little written of Sir Hans Sloane, that it was in Ulster, in Ireland, he first imbibed that love of scientific pursuits that have since rendered his name, and we fear only his name, illustrious; in France and Ireland, in fact, was laid the foundation of his great museum. Like Hunter, Mozart, Goldsmith, Hadyn, Johnson, and a legion of other great men, we find Sloane, in early years, struggling a good deal with adversity. Before he was of age he had several severe attacks of hæmoptysis, which threatened him with an early grave, like Laennec, Bichât, and others; death, however, spared him till he had done his work. Stahl, Ray, and another great countryman, Robert Boyle, were then in the ascendant, all of whom were known to Sloane, and helped to form his mind. And yet who thinks, amid the winged bulls from Nineveh, and the magnificent collection crowded at present in the huge building of the British Museum, of Sir Hans Sloane or Boyle. We think of Watt whenever we see a steam engine, because, with one happy thought, he has made it the last wonder of the world in a utilitarian point of view: the great workers in the mine of abstract science and philosophy, we disregard. In 1683, young Sloane set off for France, and there seems to have been delighted with the botanical collections and lectures of Tournefort, and spent a year collecting plants for the museum. We find him next going out to the West Indies, a young man, under 30, physician to the Duke of Albemarle, and in spite of many crosses and disappointments, still further adding to his specimens 800 species of rare and valuable tropical plants. These two collections form the first nucleus of the British Museum. It seems all this time he never forgot his native country, Ireland, and we should in all probability have those specimens now in Trinity College, Dublin, and the British Museum (which would be a great blessing) quite a different institution, but that Sloane got married to a very rich wife, and settled permanently in London. In 1693, he became secretary of the Royal Society, and in 1727 was appointed physician to the king, and succeeded the great Newton as president of the Royal Society. George I. made him a baronet, and he died, at the age of 93, in 1753.

"Of the life of the great founder of the Hunterian Museum we need say nothing; the facts of the eventful biography of Hunter are among the household words of the profession. He, too, was looked upon as an innovator, like Harvey, and fought his way to his position among the greatest men that

ever lived. Hunter or Harvey have no monuments in brass or marble in London; but they want none: their memories and great acts are enshrined in every good man's soul, without the empty nonsense of colleges. Our American and other friends walking through the galleries of the Museum, should recollect that it was Sir J. Banks and Lord Auckland rescued it from destruction; that poor Hunter's worldly chattels were all sold for debt—for debts incurred for putting the museum together; that the College of Physicians refused the museum as a gift, and that under certain favor, it was offered a domicile by the College of Surgeons, whose income, from getting the hard-earned work of Hunter's life, was doubled after a little, so that in 1833, it was said to be over £10,000 a year, and £66,000 in hands; the government gave £30,000 to build the museum, and £15,000 for the preparations. With all this money, or a tithe of it, in Dublin, with another Sir Hans Sloane, Macartney, or Carmichael, what miracles might not Ireland perform. Poor Hunter's life seems to have been one battle. He set out early, under the guidance of his brother, with whom he soon fell out; in 1753 he goes to Oxford, laughs at Latin and Greek, and a little after we find him battling with the Monros and his brother. His next encounter was with Pott—a sort of Syme of those days, that every one thought it correct to have a tilt with. Our intention, however, at present, is to say something of the museum, and wish peace to the troubled shade of its great founder.

EDITORIAL DEPARTMENT.

Die Bright'sche Nierenkrankheit und deren Behandlung. Eine Monographie von Dr. FRIED. TH. FRERICHS, ordentlich. Professor der Medicin und Vorstand der Medicinischen Klinik in Kiel. Braunschweig. 1851.

Bright's Disease and its Treatment; a Monograph. By Dr. FREDERICK TH. FRERICHS, Professor of Medicine in Ordinary, and Chief of the Medical Clinic at Kiel. Brunswick. 1851.

The history of Bright's disease illustrates, in a very remarkable manner the slow and difficult process by which we attain complete knowledge of a new disease. At first, only the grosser points in its natural history and symptomatology are pointed out; and even these cost the earlier observers much time and labor. Afterward their results are corroborated, or modified, by the labors of others; and the gaps, which were necessarily left in some parts of the structure, gradually filled up. A knowledge of the intimate nature of the malady, and its relation to other diseases, is usually of still more difficult attainment; and many erroneous conclusions must be adopted, many false opinions embraced and rejected, before any one succeeds in establishing a theory of the disease, sufficiently consistent and natural to receive the general assent of the medical public.

Before the publication of Dr. Bright's treatise on diseased kidney, in 1827, the amount of knowledge possessed on this subject was so insignificant, that he must certainly be considered as having explored a new region of pathology; and the disease will probably always be known by his name. Still, the history of the malady was far from being completed by his labors. Even during his time it was already observed that the urine sometimes contained albumen in cases where the autopsies did not show any distinct appearances similar to those of the "granulated kidney;" and some writers attempted to point out the means of distinguishing cases of albuminuria dependent on Bright's disease, from those which had their origin in some other affection. Within a very few years, it was difficult, or impossible, to decide, from the loose and contradictory statements of authors, whether anasarca were necessarily accompanied by albuminous urine, or whether albuminuria always produced anasarca. After the microscope came into vogue, as a means of

investigating the minute anatomical alterations of diseased organs, the matter, instead of being simplified, was still farther complicated. Different appearances were seen, not only by different observers, but even by the same observer in different cases; so that some writers, of the highest authority, were led to assume the existence of several different forms of Bright's disease; or rather of several essentially distinct morbid processes, all accompanied by albuminuria. These different affections received names corresponding to the notions entertained by authors as to their nature; fatty degeneration, cirrhosis of the kidney, desquamative nephritis, &c. The opinion, however, has been recently gaining ground that these differences were not owing to any variation in the essential nature of the disease, but depended simply on differences in the *stage*, *intensity*, and *extent* of the morbid process in the diseased organs. It is now well understood that Bright's disease, like many other maladies, may run an acute or a chronic course; that it may attack healthy or cachectic individuals; that it may be moderate or excessive in the degree of its intensity; and that it may invade different portions of the kidney at different periods of time. All these circumstances will, of course, give rise to variations both in the morbid appearances and in the accompanying symptoms, without changing necessarily their essential character and connections. These variations are of so much importance that one of the latest and best writers on the subject, has been led to make use of the expression that "Bright's disease, as a unique and distinctly defined malady, has no existence."

This expression, however, is much too strong. Other diseases present quite as striking variations as this. No one, for instance, will call in question the individuality of Pulmonary Phthisis; and yet the tuberculous deposit may occupy a larger or smaller quantity of the pulmonary tissue: it may be developed slowly or rapidly; it may remain confined to its original limits or extend to neighboring parts; and the whole process, as every observer knows, is subject to constant remittances and exacerbations, and may be permanently arrested, when the deposit is not too abundant, at any one of its different stages. Very much the same thing may be said of Bright's disease; and when these circumstances are taken into consideration, the different appearances, seen by different observers, may be reconciled without difficulty.

The most complete and satisfactory accounts of the views entertained at the present moment, in Europe, on this subject, are to be found in the monograph, by Dr. Frerichs, the title of which is quoted above, and in an article by Dr. Reinhardt, in the annals of the Berlin Charity Hospital, for 1850.

Both writers agree in acknowledging the individuality of the disease, and also as to all the important points of its morbid anatomy, and its relation to other affections. The same opinions are entertained at Vienna by those who are particularly engaged in the pursuit of morbid anatomy in that city.

According to Frerichs, the morbid process which goes on in the kidney may be divided into three stages, of which the first is characterized by congestion and exudation, the second by a metamorphosis of the exuded matter, and the third by its absorption or elimination and the atrophy of the renal tissue. It would, perhaps, be more accurate, strictly speaking, to describe only two stages, *viz.*, the stage of development, in which the congestion and exudation take place, and the stage of retrogression, in which the exuded product becomes altered and finally absorbed. The other division, however, is more practically useful in enabling us to understand the morbid appearances.

In the first stage the kidney is slightly enlarged, and shows, throughout the cortical substance, a strongly marked, diffuse hyperæmia. In consequence of the congestion, an exudation of the fluid parts of the blood takes place everywhere; but more particularly from the contorted capillary vessels of the Malpighian tufts, which project, without any other support than their own walls, into the cavity of the Malpighian capsules. The plasma of the blood is, then, exuded directly into the capsules and the tubuli uriniferi. The albumen and salts pass off with the urine, while the fibrin coagulates in the tubuli under the form of solid, transparent, homogeneous cylinders. If the congestion is very intense, the vessels of the Malpighian tufts (as elsewhere, under similar circumstances) may be ruptured; and then the blood-globules escape also, and may be found suspended in the urine, or entangled in the fibrinous coagula. The coagula themselves, either with or without inclosed blood-globules, are swept out of the tubuli, from time to time, by the fluid which accumulates behind them, and appear as a sediment in the urine. The external surface of the kidney, in this stage, is smooth; and the fibrous capsule is more easily stripped off than usual, owing to a thin sheet of serous fluid which has been exuded beneath it.

From this condition there is a gradual transition to the second stage. The hyperæmia diminishes with the increasing exudation. Not only the tubuli, but the Malpighian capsules themselves, may become filled with fibrinous coagula, and the vascular tufts compressed against their walls. When the exudation is very active, the fibrinous plasma may be deposited, not only in the tubuli and capsules, but also in their interstices; so that the whole organ

is considerably swollen, and has an unusually homogeneous aspect on its cut surface.

The second stage is characterized by a fatty degeneration of the epithelium of the tubuli uriniferi, and of the exuded fibrin. The epithelium cells show every grade of transformation, from the deposit of a few fatty granules in their interior, to the stage in which the entire cell is converted into a mass of oily drops and molecules. The cylinders of coagulated fibrin, which were before transparent, or inclosed only healthy epithelium or a few blood-globules, become granulated and finally completely metamorphosed into fatty matter. This transformation is accompanied by corresponding changes in the gross appearances of the kidney. The organ is almost always larger than natural. The dark-red color of the congestive stage has entirely disappeared, and the tissue of the kidney has assumed instead the pale-yellow hue, characteristic of fatty degeneration. When the exudation has been very general, and has taken place into the interstices between the tubuli, as well as into their cavities, it undergoes here, also, the same transformation; and the organ presents on its cut surface a uniform, pale, waxy appearance, which has been often mistaken for an entirely distinct form of disease. Usually, the pyramids retain in a great measure their natural ruddy color, and contrast strongly with the pallid hue of the cortical substance. The fatty matter accumulates in the tubuli and distends them, producing minute roundish *granulations*, which project slightly from the external surface. The texture of the kidney is brittle.

With the third stage commences absorption of the fatty matter. Where the preceding changes have not been so violent or so prolonged as to destroy permanently the functional activity of the tissues, the natural processes recommence as the fat disappears. But where the tubuli have been long obstructed by the fibrinous exudation, where the vessels have been compressed and obliterated, or the blood coagulated in their interior, these parts, also, shrivel and become atrophied, and a general diminution in the size of the kidney results. At the same time the investing capsule becomes closely adherent, and cannot be stripped off without bringing away with it, here and there, small flakes of the cortical substance. As the processes, however, seldom or never go on with the same rapidity in all parts of the organ, those portions which are already atrophied appear as grayish, depressed, fibrous, cicatrix-like furrows, between which are prominences formed by those portions which are still sound, or only in the second stage of the disease. In this way is produced the granulated aspect of "Bright's Kidney;" an appearance altogether different from the more minute granulation of the second stage, which

has already been spoken of, and one that can readily be distinguished from it by careful examination. As successive portions of the renal substance pass out of the second stage into the third, the kidney becomes more and more reduced in size, and firmer in consistency; until the cortical substance is reduced in many parts to a condensed, grayish, fibrous layer, not more than a line or two in thickness, and in which no trace of the natural structure of the organ can be distinguished.

From the above description it is easy to see what variations may be met with, both in the gross and microscopic appearances of a kidney affected with Bright's disease. In the first stage we may have every gradation between a moderate hyperæmia, in which the color, consistency and size of the kidney differ but slightly from the natural, and a state in which the organ is turgid, and infiltrated throughout with a dark, bloody fluid. The fibrinous coagula may be abundant or rare, colorless or filled with blood-globules. In the second stage, the organ may show, in different portions, the fatty degeneration confined to the contents of the tubuli, pale-yellow striæ or spots, minute granulations, or the smooth, waxy luster of general fatty infiltration. At all times fresh exacerbations may take place, and active congestion, or even bloody extravasation show itself anew in different localities. Usually the atrophy of the tissues in the third stage will be more or less complete, according to the amount of the previous infiltration.

In the chapter on Special Symptomatology, the nature and origin of *dropsical effusions*, in Bright's disease, are discussed at some length. This symptom is, of course, regarded as one of the most common accompaniments of the disease; but, at the same time, the author is very positive that it is not by any means a constant one. On the contrary he asserts, as Bright had previously done, that the malady "may become perfectly developed, and may even terminate fatally by either acute or chronic processes, without the effusion of a single drop of fluid into the cellular tissue at any period of its course." The dropsy is also liable to the greatest fluctuations in quantity, and changes in position; and may disappear entirely for a time, to return again on the occurrence of any slight exposure. From an examination of 430 cases, reported by himself and others, the author finds that the proportion of cases with dropsical effusion to those without, was about 7 to 1. If the comparison, however, was confined to those cases in which autopsies were made, the proportion was different, viz., about 4 to 1;—a proof, as the author intimates, that the existence of the disease was often overlooked during life. The difference, however, might be as easily explained by supposing the

disease to be more generally fatal when unaccompanied by dropsy than when this symptom is present.

As to the origin of dropsy in Bright's disease, the author is clearly of opinion that the common explanation of its occurrence is not satisfactory. It is usually taken for granted that the watery condition of the blood, owing to its loss of albumen, is a sufficient cause of its transudation through the walls of the vessels. It should be remembered, however, that such a state of "hydræmia" may be produced by abundant hæmorrhages, by profuse suppurations, &c., without any dropsical effusion following in consequence. In acute cases of Bright's disease, also, the dropsy often appears simultaneously with the congestion of the kidney, long before the blood has suffered any considerable diminution in the proportion of its solid ingredients. In these cases the dropsy has a common origin with the local affection of the kidney, and appears as its accompaniment, not as its consequence. It is evident, therefore, that although a watery condition of the blood, no doubt, predisposes to dropsical effusion, there is still another cause necessary. This cause is of a very obscure nature, but one which certainly has none the less an actual existence. It is designated by Dr. Frerichs to be a "paralysis of the capillary blood-vessels;"—an expression which is evidently nothing more than the sign of an unknown quantity.

Among the most novel and original ideas in the book, are the opinions advanced by the author on the *poisoning of the blood by urea*. All the older and more recent writers, with only one or two exceptions, agree in considering the peculiar nervous symptoms which so often show themselves in the course of Bright's disease, viz., convulsions, blindness, delirium, coma, &c. as owing to the accumulation in the blood of the excrementitious ingredients of the urine. When the kidney is overloaded with the products of congestion, or when large portions of its substance have been atrophied by a long continuance of the morbid process, its functions must necessarily cease more or less completely. It is well known that entire suppression of the urine invariably produces these nervous disorders in the human subject, and that extirpation of the kidneys in animals is soon followed by a train of similar phenomena. Finally, urea has been repeatedly discovered in the blood of individuals laboring under Bright's disease. Frerichs, however, maintains that the nervous symptoms, in these cases, are not produced by a simple accumulation of urea in the blood, but by the presence of *carbonate of ammonia resulting from its decomposition*.

The grounds on which this opinion is based, may be expressed by the three following propositions:

First. — Urea, artificially introduced into the blood, does not produce the symptoms of narcotic poisoning.

Second. — Carbonate of ammonia exists in the blood and secretions of animals dying from extirpation of the kidneys.

Third. — Carbonate of ammonia, injected into the blood, produces symptoms not to be distinguished from those following suppression of the urine.

The author vouches personally for all these facts. The first, indeed, had been previously asserted by other experimenters. Pure urea, injected into the blood of animals, produced no result beyond a temporary increase in the quantity of urine; and when the kidneys had been previously extirpated, the death of the animal was not perceptibly hastened by the injection. The two latter statements are, so far as we know, peculiar to the author. They were established by the two following series of experiments. In the first series "a solution of from thirty to thirty-five grains of urea was injected into the veins of animals, whose kidneys had been previously extirpated. During the first hours after the operation the animals invariably remained free from all morbid symptoms. After a longer or shorter time (an hour and a quarter to eight hours,) they began to show signs of uneasiness, and vomited the contents of the stomach, which consisted, sometimes, of an acid chyme, sometimes of a yellowish, mucous, alkalescent mass. At the same time the vapor of ammonia was discernible in the breath, and the animals were seized with convulsions, which were alternately suspended and renewed, and gradually gave place to increasing coma with stertorous respiration. Sometimes the convulsions were absent, and the animals remained in a soporose or comatose state from the beginning. After death, which took place from two and a quarter to ten hours after the injection, ammonia was in every instance found in the blood in considerable quantity. The contents of the stomach almost always smelt strongly of ammonia and contained abundance of its carbonate; and ammonia was also distinguishable in the bile and other secretions." "The animals whose veins had been injected with urea remained quiet and comfortable so long as the expired air was free from ammonia. As soon, however, as the breath produced visible fumes with muriatic acid, there was an immediate appearance of those disturbances of the nervous system which are characteristic of a poisoning of the blood by urea."

In the second series of experiments the veins of the animals were injected with a solution of carbonate of ammonia. "Immediately afterward they were seized with convulsions, sometimes of great severity, soon followed by unconsciousness. The respiration was difficult, and the expired air loaded with ammonia. There was choking, and vomiting of bilious matter. The insens,

ibility lasted several hours, and so long as it continued, ammonia was expired from the lungs. Gradually these appearances subsided, and the animals slowly returned to consciousness. If a fresh injection of carbonate of ammonia were made during the state of insensibility, the convulsions and vomiting were renewed, and the urine and feces were discharged involuntarily."

The author, therefore, concludes that a simple accumulation of urea in the circulation is not sufficient to produce symptoms of narcotic poisoning; but that another condition is also necessary, viz., the presence of a peculiar "ferment" in the blood, which induces the decomposition of the urea and the production of carbonate of ammonia. Precisely what this "ferment" is he does not undertake to say, nor does he pretend to demonstrate its presence directly. He considers, however, that its presence may fairly be inferred from the above facts. Consequently he regards a patient laboring under Bright's disease, whose blood is loaded with urea, as in a condition analogous to that of an animal whose veins have been injected with a quantity of *amygdalin*. Notwithstanding the presence of the foreign substance, the animal may remain unharmed for an indefinite length of time; but the ingestion of a single sweet almond is sufficient, by acting as a ferment, to cause the production of an abundance of hydrocyanic acid and oil of bitter almonds, and destroys life instantaneously.

By this hypothesis we are enabled to explain some facts in relation to the narcotic poisoning in Bright's disease, which have never been clearly understood. In cases of latent or mild character, for example, in which the nervous symptoms declare themselves suddenly, this ferment is supposed to be produced by some occasional disturbance of the system, and to decompose suddenly the urea which had been previously retained in the circulation. The explanation of the manner in which the ferment originates will be best given in the somewhat vague and indefinite language of the author. According to Frerichs, "the complicated series of chemical metamorphoses, which are constantly going on in the blood, require apparently only a slight modification to produce the decomposition of so unstable a compound" as urea. Acute diseases which are accompanied by an abnormal metamorphosis of the blood, readily become the occasion for such a decomposition; so that congestion of the kidneys during scarlatina, or after cholera, typhus, &c., is particularly liable to result in narcotic poisoning. It is well known that the albuminuria and anasarca of pregnant women, symptoms of a true Bright's disease in the kidney, may exist for months during gestation, and yet the convulsions and insensibility, which so often follow, ordinarily take place only at the time of delivery (eclampsia parturientium). Prolonged renal conges-

tion and an accumulation of urea in the circulation may remain without influence on the nervous system until the process of parturition induces a change in the blood, and the decomposition of the urea. Then come the symptoms of narcotic poisoning.

There certainly seems to be good ground for the opinion that these affections of the nervous system are not simply dependent on the presence of pure urea in the blood; and that some other condition, very possibly its decomposition into carbonate of ammonia, is also necessary. As for Dr. F.'s "ferment," however, that must be regarded for the present as having altogether a hypothetical existence. Such a body may readily be supposed to exist in the blood during the acute exanthemata. Indeed, there are many circumstances which render it highly probable that such is really the case with regard to this class of diseases. Still it must be remembered that complete suppression of the urine (from excessive renal congestion after exposure to cold or from extirpation of the kidneys in animals) is invariably fatal in the same way; and it is not easy to understand why the "ferment" should always happen to be so suddenly produced in the healthy body under these circumstances.

As to the frequency of Bright's disease, Dr. Frerichs agrees with most of the later writers that it is much more common than has been previously supposed; and that no disease is so frequently overlooked as this, particularly when it is unaccompanied by dropsical effusion. He establishes completely the identity of the disease under its different forms; acute or chronic, simple or complicated with tuberculosis or cardiac disease, occurring during or after scarlatina, after cholera, typhus, small-pox, and measles, and during pregnancy. He estimates the proportion of recoveries to deaths, in the acute form, as two to one; in the chronic form, as one to eight. J. C. D.

Hats and Baldness.—The subject of baldness is one in which many within the social circle of every medical practitioner are more or less interested; and it is one which probably comes nearer home than this to not a few of our professional readers. On this subject our observations and reflections (Providence be praised! not as yet our *experience*) have led to an abiding conviction. In fact, with the assistance of a scientific friend, we are persuaded that we have made a discovery of considerable practical consequence to those to whom it particularly concerns. The subject is one which may provoke mirth, but a bald pate is often no joke to the possessor, *capital*

though it be in one sense. He may receive a jocosse allusion thereto with an amiable spirit, but there is sadness at the bottom, and his recognition of the jest is usually with a sardonic grin. We state this as a matter of observation, and we feel sure of the correctness of our *diagnosis* in such cases.

What occasions baldness, is a question by no means beneath the dignity of the medical editor. We ask the question with reference to the *male sex*. We do not, as a point of gallantry, presume that women never grow bald; but as a matter of fact they do not nearly as often as do men. This is a fact having an important bearing on our theory. We ask our readers, is it not true that a much smaller proportion of women, than of men, are affected with alopecia? Another *fundamental* (if the reader will excuse the antithesis) fact, viz., *men grow bald almost invariably on the top of the head (alopecia calvities) while the loss of hair, in women, is uniform over the head, or as often laterally, as on the crown.* We put this latter fact in italics, because it is the main spoke in our argument. Now we revert to the first question, what occasions baldness in men? We answer, in imitation of beau Brummel, *Hats is the man.* Seriously, and in the most scientific frame of mind which we are capable of assuming, we believe that male baldness is generally owing to the hat which is commonly worn.

Our train of argument is as follows: Men are affected with baldness much more frequently than women. The baldness in men affects the crown of the head, while in women it is not confined to that situation. There must be adequate reasons for these differences in the two sexes. These reasons, it is rational to conclude, pertain to the articles of dress worn on the head. The hat commonly worn by men differs in some striking peculiarities from the bonnets worn by females. It is logical to infer that the causes of the differences just mentioned, if they relate to the hat, concern the peculiarities in which the hat differs from the bonnet. The legitimacy of this conclusion is sustained, if, on examination of the peculiar features of the hat, it be found that such an effect might *a priori* be expected to occur.

In this discovery we are not entirely original. Such an idea appears lately to have been entertained to some extent, and the hatters, in their latest constructions, have recognized the truth of this idea by making a small ventilating aperture in the crown. This assumes that the objection to the hat lies in the confinement of the air and cutaneous exhalations in the vacant space between the crown of the hat and the crown of the head. There may be something in that supposition. This is one of the peculiarities of the *hat* contrasted with ladies' *bonnets*. The latter permit free ventilation. We do not look upon this feature, however, as the most important in its relation to

baldness. The most characteristic trait of the hat is the tightness with which it encircles the head. Herein consists, in our opinion, its agency in the loss of hair. The stove-pipe hat must needs encircle the head tightly, in order to be secure in its position in spite of wind and other disturbing forces. To appreciate the degree of compression, one has only to note the indentation left on the forehead after a tightly fitting hat has been worn for some time. Every body knows how commonly this is to be observed. The head is in fact pretty firmly ligated while the hat is worn. Now what must be the effect of this on the circulation? Plainly, the effect is to interrupt the circulation in the scalp above the circle on which the compression is made. It is precisely like tying a cord around the head sufficiently to diminish, if not stop, for the time, the flow of blood through the temporal and other arteries by which the blood is distributed to the superior portion of the scalp. The hair follicles, as is well known, are very vascular. Their functions require this vascularity, and an adequate, constant supply of oxygenated blood. If this supply be diminished, the growth and nutrition of the hairs are proportionably affected; and, finally, the *pulp* inclosed in the follicle withers and dies, as does any other part when deprived of the *pabulum vite*. This effect occurs on the crown because interruption of the circulation in arteries is always felt most in the parts to which the terminal branches are distributed.

Such is our explanation of the fact that baldness is so frequently observed in the young and middle aged men of the present generation. The remedy is to repudiate the present fashion of hats. Let some inventive genius devise a substitute for the unseemly, as well as hair-destructive article which is now the *mode*, and we are firmly convinced that toupees will become objects of curiosity rather than utility, and the bald pate will again be venerated as the distinguishing trait of old age.

We have discharged our duty, in this important matter, by sounding the *tocsin*. It remains for others whose personal interest in the subject is not so remote as ours, to put their shoulders to the wheel, and urge the reform onward.

American Medical Journals.—The Western Journal of Medicine and Surgery, (No. for Feb. 1853,) contains an interesting editorial article on medical journalizing, suggested by the occasion of noticing a new candidate for favor in that line. Our friend, the writer of the article, will, we know, excuse the liberty which we take in appropriating portions of his remarks for the benefit of our readers, and also as texts for a few original comments.

Speaking of the great number of medical serials at present issued in this country, he asks, "Is it possible that all the journals in existence and contemplated, can live? Are they and will they be supported? We are no prophets, and shall not attempt to foretell what will be the fate of many of our respected contemporaries; but we are sure we are fully warranted in saying, that at the present time their support is most inadequate. Hardly one comes to us in which there are not complaints that subscribers do not pay well. The cover of our own journal bears too frequent testimony to the tardiness with which the demands of our publishers are met. As a body, we take it that the medical editors of the United States labor for a smaller pecuniary remuneration than any other class of men in the community. The hope of such reward cannot be a motive to these enterprises."

It was remarked by Adam Smith, that almost every one, in commencing an undertaking in life, evinces an overweening reliance on his own good fortune. Although multitudes in a similar undertaking have failed, *he* expects to succeed. Perhaps this law of the mind may serve to explain the reiterated efforts to establish medical journals. Were the editorial neophyte governed by the results of previous undertakings of the same sort, he would hardly feel desirous to incur the chances of failure, but he is far from subjecting *his* expectations to the calculation of probabilities. It is fortunate that this egotism, or hopefulness (whichever of these names we choose to give it) is so powerful an element of human character. Without it undoubtedly there would be still fewer of the relatively small number of undertakings in the different fields of effort which are crowned with success.

Another explanation of the frequent accessions to the list of medical editors is, that the editorial functions are considered to be rather a kind of recreation than otherwise. To write articles, make selections, remodel contributions, and perhaps correct proof, are thought to be duties filling up agreeably the small gaps of time which are at the disposal of the physician. To have such a resource is considered an advantage on the score of relaxation. In this respect disappointment is sure to follow. Novelty may for a brief period give a charm to editorial labors, but ere long they become pleasant only as other labors are so, by habit, and satisfying that want of occupation which Providence has made the condition of happiness in this world.

We find in the article to which we have referred, the following account of the origin of medical journalism in this country, and of the number of medical periodicals now in existence:

"It was near the close of the last century when our first periodical devoted

to medicine was established, and at that time there were few such journals in existence anywhere. The *Medical Repository* was begun at New York in 1698, and for fifteen years had no ally or rival in our country. From first to last, in fact, it may be said to have occupied the field alone, for while it went on to the completion of its twenty-third volume, its few contemporaries, after struggling on for a year or two, were discontinued for want of patronage. The *Medical Recorder* succeeded to the popularity of the *Medical Repository*, and thirty years ago, if not the only one in existence, was the only one that was widely circulated, or had any considerable duration. Another was at length established by its side in Philadelphia, and this still goes on under a new name, the largest, the most elaborate, and the best supported of all our professional serials. The *Boston Medical and Surgical Journal* in age ranks next to the *American Journal of the Medical Sciences*, and its accomplished and amiable editor, we believe, has been longer on the tripod than any of his brethren, and is therefore the patriarch of the fraternity.

"Looking over our exchange list, we find that the number of our medical journals has now swelled to twenty-eight, not to speak of those devoted to Pharmacy, Insanity, Dentistry, and the Sciences allied to medicine, or of that mongrel brood which defiles while professing to reform and advance our noble profession. Of these New York furnishes five, Pennsylvania four, Tennessee four, Ohio two, Louisiana two, Kentucky two, and Massachusetts, New Hampshire, New Jersey, Virginia, South Carolina, Georgia, Missouri, Iowa, and Illinois, each one. All France and Great Britain, together, scarcely do more than this."

On the subject of the literary character of medical journals, our *confrere* makes the following remarks, which, it must be admitted, are founded in fact:

"We have remarked, that most of these publications afford but too indubitable proof of insufficient pecuniary support. Most of them barely pay expenses, if, indeed, they are not a tax upon their conductors; and in this connection another question naturally suggests itself—*is their literary support any better?* We believe that no one, after a careful inspection of the weekly and monthly issues of our medical press, will answer this question in the affirmative. The pages of our journals testify to a sad deficiency of practiced, competent contributors. Any one who will be at the trouble to estimate the proportion of "original" matter in them, one month with another, will find that it is small, and, what is worse, that the articles forming this department have too probably obtained admission, in many cases, for no better reason than the straitness of the editor's supplies. A journal once launched forth must be continued to the end of the year, whether it has subscribers or not, and its pages must be filled with or without the aid of contributors. Publishing day comes round and the editor is not prepared for it; he racks his brains for matter, but discovers, at length, that his powers of production have their limits. His Hippocrene has run dry. In despair he seizes upon whatever may chance to lie within his reach, retouches, or remodels it, vamps it up, and sends it fourth among his "original" essays and cases."

The following extract lets out a secret of editorial experience which, since our contemporary has *peached*, we may confess is no novelty to us. Our

readers will be amused with the letter of the repudiating subscriber, and, under the circumstances, they will find no fault with the editor for reproducing the correspondence, *verbatim et literatim*, not attempting, as in other instances, to renovate the composition, so that the author will hardly recognize himself in print. *M. Jeffries, M. D.*, will find his animal the identical one sent to pasture. Inasmuch, however, as the owner himself has *gone to grass*, it is very doubtful if he ever knows that he has been made conspicuous as a writer. But to the extract, which must be read before what has just been written will be intelligible:

"We once heard of a correspondent of a western journal who, on the appearance of his first article in print, wrote a kind letter to the editor, in which he related the following anecdote: He had once a friend, he said, who concluded to buy a little horse at auction because he was going cheap—an ill-formed, starved, sorry looking little creature. He sent him to the country to be recruited. After a few weeks his pony was brought back to him, but so metamorphosed that he did not recognize him. He could hardly be persuaded that the plump, sleek animal before him was the shabby thing that he had a little while before dispatched to the country. We may add that this correspondent was not discouraged by the liberties which the editor took with his manuscript, but has continued to write, and now when his papers return to him he has no difficulty in recognizing them. We have no doubt but the experience of our brethren of the quill would furnish many similar instances. But all correspondents are not so reasonable, nor so improvable. What, for example, could be made of a man who wrote thus?

'Dear Sir I wish you to Stop sending the Medical Journal to me for I will leave for oregon this Spring So close up for it is the no a countest thing I ever read it is at least 50 years behind the times you had better stop grinding or turn Eclectic then you will be a benefit to mankind.

M. JEFFERIES M. D.'

"We think this rare literary specimen is worth preserving, and doubt much whether any of our brethren can furnish from their correspondence anything to match it. *Dr. Jefferies*, it will be remarked, does not deign to make a stop in his letter. It is to be hoped that he did not stop till he got to Oregon."

Portraits of living Physicians.—The zealous editor of the New Jersey Medical Reporter, proposes to introduce a new feature into medical Journalism in this country. He promises his patrons, occasionally, a portrait of some distinguished member of the profession, with a biographical sketch, selecting those who are natives of the state of New Jersey. *Dr. George B. Wood*, of Philadelphia, formerly of New Jersey, is to head the list.

We presume this effort to render the Reporter more attractive will be duly appreciated.

New York Preparatory School of Medicine.—The *New York Medical Times* contains an announcement of an association in the city of New York, under the above title. The design of the association is to “furnish to medical students the *elements* of a complete and scientific education, by means of *daily recitations* and familiar *demonstrations*.”

“The plan of instruction,” it is stated, “is so arranged that the whole groundwork of practical medicine and surgery, with anatomy, physiology, and chemistry, may be advantageously gone over in one year, by means of *Systematic Text-books*; and at the same time a course of *specialties* will be followed with the aid of standard *monographs*, which will require three years for its completion.”

The instructors are Prof. Ehrick Parmlee, M. D., Descriptive Anatomy; Prof. C. Dalton, M. D., Physiology and General Anatomy; J. Outram, jr., Chemistry and *Materia Medica*; A. K. Gardner, M. D., Midwifery and Diseases of Women; Henry Weeks Brown, M. D., Principles and Practice of Surgery; C. F. Heywood, M. D., General and Special Pathology, etc.

Most of the foregoing are new candidates for favor as medical teachers. Of one of the members of the school we may be permitted to speak from personal knowledge and association. The departments of physiology and general anatomy could hardly be intrusted to one better qualified to do them justice.

New Jersey Medical Institute, at Burlington, N. J.—A faculty has been organized at Burlington, N. J., with a view to a systematic course of medical instruction during the summer months. The plan embraces familiar didactic, and clinical lectures, but not the conferring of degrees. Dr. Joseph Parrish, editor of the Reporter, will instruct in the theory and practice of medicine. The other departments of medical science are assigned to competent teachers.

Amende.—The address by Dr. Cotting, entitled *Nature in Disease*, contained in the February No. of this Journal, should have been credited to the *Boston Medical and Surgical Journal*. This credit was inadvertently omitted.

The short article in the same No. entitled “The Castor Bean Epidemic,” was incorrectly credited to the *Boston Med. and Surg. Journal*. It should have been credited to the *North-Western Med. and Surg. Journal*.

Ratio of deaths, insanity, blindness, etc., in different parts of the United States.— For the following statistics from the last national census, we are indebted to the New York Medical Times:

	†	Annual deaths per cent.	Ratio to the number living.
New England States,		1.55	1 to 64
Middle States, with Ohio,		1.39	1 to 72
Central Slave States,		1.38	1 to 73
Coast Planting States,		1.37	1 to 73
North Western States,		1.24	1 to 80
United States, total,		1.38	1 to 73

From the same source are derived the following statistics:

There are 9,091 white *mutes* in the United States, and 632 colored, of whom 489 are slaves. Among the white population, there is one deaf mute to each 2,151; of the free colored, one to each 3,005, and among the slaves, one to each 6,552. There are 9,702 *blind* persons, of whom 7,000 are white, 1,211 slaves, and 494 free colored persons. Among the whites, there is one blind person to each 2,445; among the free colored, one to each 870; among the slaves, one to each 2,345. Of *insane* persons the whole number is 15,768, of whom 15,156 are whites, 321 free colored, and 291 slaves. Of *idiots*, there are whites, 14,230; free colored, 436; slaves, 1,040. With the white population in the United States, there exists one insane person for each 1,280 individuals; among the free colored, one to each 1,338; and among the slaves, one to each 11,010. With respect to idiocy, the white population presents one to each 1,374 persons; the free colored, one to each 985; and among the slaves one to each 3,080.

Inoculation for Syphilis.— A plan has lately been proposed by M. Auzias Turenne, of Paris, to inoculate with the syphilitic virus till the system becomes proof against the disease! Messrs. Malgaigne and Depaul, Parisian surgeons of prominence, have advocated the plan. We know not what evidence has been advanced by these gentlemen of the efficacy of this method of protection; but we think they should at the same time suggest a safeguard against the moral taint of licentiousness! The proposition shows that the subject has, by them, been regarded as one of importance only in its physical relations. It is stated that the subject excited an animated discussion before the Academy of Medicine, of Paris, and that, to the credit of the Academy, the practice was repudiated by all the members excepting the gentlemen named.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Contributions to the Chemical and Physiological History of Diabetes Mellitus.* By J. BRYANT SMITH, M. D., of New York City.

This disease has been recognized for a long time as a distinct malady, mention of it being made by Galen, whose attention seems to have been called to it by the extraordinary flow of urine, and not by any peculiarity in the secretion itself, a matter of surprise to us since the sense of taste seems to have been used extensively in diagnosis by the old physicians. The sweet taste of diabetic urine was first noticed by Thomas Willis, and published in 1677, and confirmed by innumerable observers from that time to the present. Ambrosioni obtained sugar from the blood of diabetic patients in large quantities. The identity between this sugar and glucose or grape sugar, was first recognized by Cherreuil, in 1815. Dupuytren and Thenard recognized two species of sugar in the urine, to one of which, having a sweet taste, and possessing the ordinary properties of sugar, they gave the name "sapid," and to the other, wanting these characteristics, they gave the name "insipid." This latter variety has since been shown to be a compound of grape sugar and common salt, a substance obtained by Calloud, in 1825, by evaporating a syrup of diabetic sugar with common salt. Reynoso states that sugar is

present in the urine of dogs treated with arsenic, lead, sulphate and carbonate of iron, and that he has detected it also in the urine of patients laboring under tuberculosis, hysteria, chronic bronchitis, pleurisy, asthma and epilepsy.* Michea also detected the same peculiarity in four cases of hysteria, two of epilepsy, and seven of delirium tremens.† The former of these authors also stated that sulphate of quinine, when administered in disease, determines sugar in the urine.

Diabetic urine is light-colored and transparent; of a pale, straw or greenish tint. Its odor is very much like that of an apple chamber, its taste is more or less sweet, and its specific gravity unnaturally high, varying from 1021 to 1050, the healthy average being about 1020. The quantity voided during twenty-four hours, is stated at almost incredible amounts, 40, 70, and even 200 pints, having been reported in this disease. Taking 1040 as the average specific gravity, and ten pints as the average quantity a diabetic patient loses in a day, 15 oz. 7 drams, or more than $1\frac{1}{2}$ pounds of solid matter per diem. Crystallized diabetic sugar has the composition $C_{24}H_{34}O_{24} + 2HO$. It may be easily detected in the urine by mixing with it a little sulphate of copper and caustic potash, and boiling the solution, which soon deposits the red suboxide of copper. This test enables us to detect the $\frac{1}{10000}$ part of grape sugar. (Silliman.)

Messrs. Robin and Verdeil, two French chemists and physiologists, in a new work entitled "Chimie Anatomique et Physiologique," 3 vols., and an atlas published in Paris during the present year, give to this variety of sugar occurring in the disease diabetic mellitus, the name of "Sucre de Foie," or sugar of the liver, a preferable term to diabetic sugar. According to these authors this principle exists in the normal state in the parenchyma of the liver, in the blood of the hepatic veins, in that portion of the vena cava above them, in the blood of the right breast and the pulmonary arteries.

In the young animal we find a trace of sugar in the blood of the pulmonary veins, in the blood of the left breast, in the aorta and its branches, but not in the general veins. During digestion we find it everywhere where it is in the young animal, but in larger quantities, a little being even detected in the nervous system. It is never a constituent of normal bile. It is found from the fourth and fifth month of intra-uterine life to advanced age; it is found in the urine of the fetus of the cow from four to seven months, and in that of the sheep from six weeks to two months, and also in the liquors aminon and allantoid of these animals.

* Comptes Rendus, 1851, vol. 33. † Ibid.

There are some morbid conditions in which we find sugar in those parts of the body which do not normally contain it, and others in which we do not find it where it exists normally.

The disease known as diabetes, is "anatomy-pathologically" characterized by the presence of a great quantity of grape sugar or glucose in the urine. We find it in the saliva, kidneys, serosity of the pericardium and spermatic fluid, as has been shown by Bernard in a dog rendered diabetic. It is not found in the nervous system, nor in the pancreas or spleen. It has been found in the serous fluid of a blister, and also in vomited matter and in the sweat of a patient treated for glucosuria.

There are a number of diseases in which sugar disappears from the liver and is not found there after death, and in diabetic persons during life sugar oftentimes disappears from the urine and even the liver. It diminishes very considerably, or disappears altogether, from animals who become diseased or die from long continued abstinence.

It seems, from these facts, that the duration of sugar in the economy is very great, commencing to appear in the early part of intra-uterine life, and persisting even to the close or near the close of life, although it is liable to be interrupted or entirely absent under certain circumstances.

The quantity of sugar in the liver and blood has never been determined by figures, but we can determine approximately the quantity of tartrate of copper and potassa reduced by a given quantity of the liquid. It is thus that we state the proportion of sugar to be very considerable in man, birds, dogs, horses, rabbits, but much less in reptiles, and entirely absent in the eel and ray. It is also ascertained in this manner that the blood after it leaves the hepatic veins, contains less sugar, and that this decrease gradually continues until it is lost in the pulmonary veins.

In the urine we find the following quantities given by different experimenters:

Urine, (Rayer,)	100 parts in 1000.
“ (Mialhe,)	62
“ (Bouchardat,)	33 to 134.
“ (Simon,)	from 25 to 39.80.
“ (Mialhe of Contour,)	45 grammes to the litre.
“ (Reich,)	43 “ in 1000.
“ (Fouberg,)	32.
Blood, (Dr. —,)025 grammes in 72 gra.

Morin, in the French Journal of Pharmacy and Chemistry, 1843, vol. iii,

page 354, compares the quantity of urea to that of sugar in 1000 parts of urine with the following results:

	Sp. gr.	Sugar grammes.	Urea grammes.
November, } 1st Case.		21 to 24	5.4
December, }	1025	17	8.7
June, }	1026	00	10.6
December, }	1034	26	0.5
November, }	1040	37	1.27
January, }	1027	13.5	3.64
March, }	1038	trace.	3.32
August, }	1022	0.16	7.07
July, }	1039	5.75	13.86
August, }	1037	47.00	19.31
February, } 4th Case.	1027	45.00	0.85

Glucose exists in the blood in the liquid state, being dissolved directly in the water. It preserves in the economy the optical properties which it possesses externally; that is, in the urine it turns the polarizing plane to the right; a character which enables us to recognize sugar when it exists in a liquid in the system. "Sugar of the liver" presents in the economy in contact with chemical agents the reactions which are proper to it. Organic substances act on it in the blood, in the liver, in the urine, in the serosity of the pericardium, &c., as out of the system, causing it rapidly to undergo catalysis. In twenty-four hours sugar, which had existed, disappears, and the liquid which had been neutral or alkaline becomes acid. If to urine or blood containing sugar we add a ferment, there will be a decomposition of the grape sugar into alcohol, carbonic acid and water. Sugar combines in the system, as without, with salts; the most of these compounds is that with common salt, having the constitution $C_{12}H_{22}O_{11} \cdot N$ or $O \cdot HO$. or one equivalent of water in ordinary grape sugar is replaced by one equivalent of chloride of sodium. It is this which we believe to be the compound existing in the disease termed "Diabetes Insipidus." The sugar which exists in the liver is in sufficient quantity to give that organ a little sweetish taste. What are the particular substances or principles which furnish the materials for the formation of sugar in the economy we are unable to say, but M. Bernard has shown that dogs and cats nourished for four, five, or six months entirely on flesh, bones and greasy matters, do not present sugar in the blood which enters into the liver by the vena porta, although the blood in the hepatic veins contains it. Cane sugar ($C_{12}H_{22}O_{11}$) which penetrates by endosmosis into the portal vein, disappears as such in the liver, and passes into grape sugar, gaining four equivalents of water. It is, perhaps, the same

with milk sugar, which gains two equivalents of water ($C_{24} H_{20} O_{30}$) + 2 HO.
+ 2 HO. = ($C_{24} H_{24} O_{32}$)

It is in the liver that the principal constituents for the formation of sugar are found, and all the causes which influence the circulation in this organ, influence, also, the production of sugar. When the circulation is active here, the quantity of sugar is increased, and vice versa. During digestion the vena porta system is turgescient, and there is present more sugar in the blood whatever be the nature of the food.

Bernard has shown that the formation of sugar is influenced by the nervous system. Not that this system can have any influence on the chemical fact of the combination of the elements hydrogen, carbon, and oxygen in the proportion which constitutes sugar; but the nervous system influences the circulation in the liver, and then follows certain conditions which put into juxtaposition the materials on which depend the formation of glucose.

When we divide the lingual nerve and irritate the periphery of the extremity, we produce no effect; but if we irritate the center we produce effects analogous to those which result from the action of sapid substances on the tongue, that is to say, we have a flow of saliva from the ducts of Warthon and Steno. Thus we have an action transmitted to the nervous centers, and reaction from those on the salivary glands. The pneumogastric nerve is to the lung what the lingual nerve is to the tongue. When we divide this nerve and irritate the inferior extremity, we produce no effect; but if we irritate the superior extremity, there is impression on the nervous center and reflex action on the great sympathetic nerve, which, in its turn, influences the vessels of the liver and there is sugar produced.

This increased production of sugar in the liver which we thus obtain artificially, by irritating the lung, by the administration of chlorine or ether, or by the division of the pneumogastric nerve and irritation of the superior extremity of the nerve, or increasing the reflex action of the medulla oblongata by a wound, is observed in men under some morbid conditions. We find, then, sugar in all the parts of the body of which we have spoken; it is this which characterizes physiologically the disease called "Diabetic Sucrie," a preferable term to "Glucosuria," since we find sugar in all the humors of the body as well as in the urine. The conditions for the formation of glucose from cane sugar, or dextrine in the liver, are only the contact of these substances with the anatomical elements of the liver. In effect many authors have said that the contact of the hepatic parenchyma of a dead animal with cane sugar in solution, was sufficient to cause the sugar to undergo glucose catalysis. Cane sugar which, as such, exists in the vena porta, is passed into

glucose in the hepatic veins, and the nervous system acts only to produce the change in the system in a few minutes which requires some hours without this influence.

The crude fecula which is introduced with the intestines comes in contact with nitrogenized bodies and a temperature of about 98° F., there results dextrine catalysis and, perhaps, a small quantity undergoes glucose catalysis, and in these conditions it is carried into the vena porta and thence to the liver, where the glucose catalysis is completed, and the hepatic veins contain sugar.

Finally, we have in the blood the conditions for the destruction of the glucose formed in the normal condition of the system.

Under the influence of fermenting substances, present in the blood, the sugar becomes converted into lactic acid, losing thereof its two equivalents of water and becoming $C_{24}H_{24}O_{24}$; this, reacting on the carbonate, forms lactates of the alkalies present, and as such passes out of the system. If the production of sugar is increased beyond the normal standard, the lactic acid catalysis is no longer able to convert all the sugar into this state, and we find it in the secretions as such.

These views which we have given, alone are founded on the experiments of M. Robin and Verdeil, or M. A. Bernard, of Paris, and are contained in the work of the former authors above cited. M. Bernard's experiments have been published for the most part in the *Comptes Rendus de Soc Biol*, during the past few years, and will be found highly interesting to those who take an interest in physiological and chemical science as relating to medicine.

LABORATORY OF THE UNIVERSITY OF LOUISVILLE, Ky.

ART. II. — *Pneumonia*. By SMELFUNGUS.

"During the angry controversy between the Nominalists and the Realists, certain offensive books were ordered to be chained in the libraries. It were well if, by a similar decree, nineteen-twentieths of the materia medica were locked up in the cabinets of the curious."— PROF. ABNER H. BROWN.

Smelfungus, standing a few days since on the icy bank by the little watch-house beside the railroad bridge at Portageville, upraised his hat in reverence to the mind that planned it. There stood the bridge, a structure, firm, sure, and steadfast, yet light, graceful, and symmetrical. Down hundreds of feet in the abyam flowed the dark river, fretting about the solid piers, where,

"Far, far beneath the vast incumbent pile
Slept the broad rock!"

"Here," he exclaimed, "here at last is the true emblem of medical science. Every stick in that vast congeries of trestle-work is isolated, and capable, in event of decay, of removal and replacement by another. On no one timber rests any special importance. Any worthless piece may be cast aside without detriment to the unity of the whole. And so in medicine. Our temple is not built of impoverishable materials, neither does the safety of our art rest upon any one fact, medicine, or theory. In the progress of discovery our facts may become worthless, our medicines inert or hurtful, our theories 'the baseless fabric of a vision,' and still the goodly structure stands; for a new and truer fact is substituted; by a better interpretation of nature we have some better treatment of disease, and for our withered theories we gain some surer basis. No mortise or tenon is in the edifice, and the casting out of an erroneous idea does not involve the destruction of its neighbor, or endanger the stability of the whole.

"Therefore, oh watchman! scan closely all the parts — reject fearlessly all decayed and broken elements. Go on, ye builders, taking from and adding to, until the temple of Esculapius stands a perfect whole of solid and enduring material!"

Having had an easy delivery of these pregnant remarks, Smelfungus retired to the Lauman House to warm his feet and get a cigar. Once thoroughly warmed and under the influence of a specially good cigar, he went into a discussion of medical matters — things new and old — with all that homely enthusiasm for the good, and peppery indignation for the bad that characterize him.

"*In medio tutissimus ibis*, said the olden poet, and ever since, like wine long kept, the pithy adage has grown richer and more truthful. In all the little eddies and whirlpools of medical inquiry, (with little great men floating like chips upon the seething waters) we recognize two mighty currents, deep flowing and rapid in their diverse ways, uplifting huge contentious waves of difference. The one of these sweeps on with gathering force against old theories, and already it threatens the gorgeous though composite pile of our old materia medica with destruction. And this is the tide, or the gulf-stream, or if you please the all-enveloping maelstrom of natural medicine."

Here Smelfungus sticks in his allegory, and like Mr. Micawber, descending abruptly from his lofty periods, he "docks the tail of sentiment."

"In short," (growing red in the face,) "here are two separate packs of

fools annoying sober men with their nonsense. Here is one class, who tell us that medicine is an invention of the enemy — who call aloud for another Hercules to turn another Alpheus through those Augean stables the apothecary shops, and sweep from the learned shelves the latin-labeled drugs as things of no account or value. Oh ye cold blooded animals who look upon a patient as an interesting specimen of natural history, and turn a deaf ear to his earnest cry for pills and potions — *the world loves medicine!* Man, quoth Cuvier, (?) is a pill-taking animal, and you who deny this first want of his nature he will not call upon.

“And yet woe to the poor patient, if in shunning the natural historian he falls upon the other extreme of unlimited control over diseased action by medicines! He will surely find himself *in extremis!* These men of blood and guts, who remove a pint of disease from a hole in the brachial vein, and a half a gallon more *per anum*, are worse than their do-nothing antagonists. They are no rare birds either. I have a dozen in my circle of acquaintance whose latest author is good old Dr. Thomas. Why sir! when my poor friend Dr. T. was coming down with consumption, we met in consultation; a half a dozen brother chips, when, as the youngest man, I was first called on for an opinion, I proposed such a mild febrifuge, anodyne, and counter-irritant course as should allay the pressing inflammatory symptoms then present, and restore the tone of the stomach, old Dr. W.’s gray hairs stood up in mingled wonder and disdain. ‘Give him an emetic!’ thundered the doughty old Hunker. ‘He’s got the consumption, *that’s* the treatment for consumption, and *that* will do him good!’ The good old gentleman evidently fancied that poor T. was going to puke up a phthisis pulmonalis! And every day, with younger men, we see this same insane notion of a routine of pills, powders, and emetics, for a disease of assimilation.

“I would to God,” (and Smelfungus crushes his cigar in his righteous indignation,) “that I had the power to make these men read, learn, and inwardly digest the whole of Martyn Paine’s abstrusely learned volumes!® After such a course as that I fancy they would turn to our lighter periodical literature with a relish which they do not manifest at present.

“*Quo me vehis!*”

‘Prone to wander, Lord I feel it!’”

sings Smelfungus in answer to a gentle hint as to the subject matter of con-

®“Man’s inhumanity to man
Makes countless thousands mourn.”

versation, "and if my text be true I have been wandering in dangerous paths. Now let us get back to this middle ground if we can find it.

"The revelations constantly making in the natural history of disease have taught all seekers after truth, that medicines are not the actual necessities we have deemed them. Yet it does not hence follow that they are *per se*, useless or injurious. One great general principle may be laid down, viz., that when we can with safety omit a medicine, it is our duty so to do. But this word safety should imply not only immunity from death, but from unnecessary suffering. I like much, (always excepting a certain timidity in its tone,) the article on pneumonia, in the last 'Braithwaite,' by Dr. R. B. Todd. Dr. Todd proposes to strike out from the list of medicines in this disease, all the weighty items, such as tartar emetic, calomel, bloodletting, etc., and to substitute for them in increased doses a medicine long used in pneumonia, but considered merely as an adjunct, viz., the acetate of ammonia given in six dram doses. Externally he makes frequent use of the turpentine stupe. Thus Dr. Todd relieves us from the use of drugs frequently hurtful and unmanageable. By some process of reasoning not very lucid, he connects this change of treatment with the idea of blood poison.

"Smelfungus will help Dr. Todd through in this matter. Perhaps this blood poison may be an excess of albumen in the fluids. We have been in the habit of calling the albuminous sputa of pneumonia the result of inflammation, but the occurrence of critical albuminuria in this disease would seem to indicate that albumen was in excess throughout the system, independent of phlogistic action. Dr. Smelfungus stands ready to receive the thanks of Dr. Todd for this explanation. But, seriously, the presence of a poison in the blood has something to do with pneumonia typhoides, and who can tell us the pathological differences between that and the acute form.

"The self-limitation of pneumonia is another idea advanced by Dr. Todd. Now I have had the good fortune to see several cases of this disease, which, from the stupidity of the friends, had no medical treatment until the occurrence of bloody expectoration in the second stage alarmed them. In all these cases, the disease had reached its acme, and was on the decline, having involved only a limited portion of the lower lobe. Now, my good Sir Hunker! these cases (three or four in number) are, so far as they go, positive contradictions to your pet notion, that an inflammation once lighted in the lung, will spread like wildfire through its whole parenchyma. It may do so, mind you, but you may safely draw the conclusion that the almost uniform departure of the inflammation at a certain point in the lung, is not *all* owing to your own skill. For if you have such unlimited control over

inflammation, why can you not bring it to bear on an erysipelas, or a synovitis? As Allapod says, 'hence we view,' that you do not in every case cut short the disease with your routine of bloodletting, calomel, antimony, and blistering. *It cuts itself short*, and therein only manifests its natural tendency.

"Shall we, then, abandon bloodletting in pneumonia? Even so; for it is generally unnecessary. Not so; for cases there be when the delirious mind grows rational, the swollen countenance natural, and the choked and labored pulse grows soft and easy, from the lancet.

"Shall we abandon calomel and antimony? Again yes; and again no; for cases will occur, when every means that science can prompt or art direct, are necessary to guide and govern the lava tide of inflammation, to prevent effusion and abscess, and the whole dark array of *sequelæ*.

"And blistering;" and Smelfungus speaks tenderly as a lover of his mistress, "blisters are always good, and never disappoint us." If in all the nauseous scented armamentaria of therapeutics there is one thing that Smelfungus is willing (metaphorically speaking) to take to his heart, it is Emplastrum Catharidis! He loves his blisters as fervently as old Dr. Clysterpipe his syringes, for the old man based his claim to Christian character *on the love he bore his enemies*. Pardon the pun!

"Sir Hunker! from premises like these we predict the dawning of a milder day. Pneumonia is still, and ever will be, a disease eminently requiring the guiding hand of the physician. The old (and we may still call it the usual and authorized) treatment of pneumonia, is a club in the hands of Hercules wherewith we may deal mighty blows. But a pounding less severe will answer in a majority of cases, and we shall yet learn that a rat-trap is no better than a smaller tool for catching the 'small deer' that infest our crania.

"Listen, then, to the truthful lesson! Pneumonia is, in a majority of cases, a self-limited and little dangerous disease, but it should be closely watched, lest, as sometimes happens, the diseased action may not stop at the usual point in the lower lobe, but rage on unchecked throughout its utmost borders. And mark you, man of the lancet! He who cures a pneumonia predestined to occupy a whole lung, does a goodly thing and may congratulate himself. Here come in your whole catalogue of remedies. The God Antiphlogos alone is mighty to save!"

ART. III. — *Four Cases of Glossitis.* By T. D. STRONG, M. D.,
Westfield, N. Y.

The infrequency of Glossitis, its severity and danger, as well as my personal appreciation of the intense suffering incident to the disease, prompt me to report the following four cases, with the results of treatment:

CASE I. Thomas Doherty, Irish, aged 22, laborer.

Sept. 18th, 1851. He used some bad tobacco, and tongue commenced swelling.

20th. First saw him this P. M. Tongue nearly double its natural size, swelling mostly confined to one side of raphe. Red and glossy. Complains of great pain in it. Saliva flowing freely. Articulation indistinct. No general febrile movement.

℞. Sulph. magnea. et sennæ, freely.

21st, 9, A. M. Rather more swollen and painful. Scarified it superficially, but only little blood flowed. No fever.

℞. Hyd. sub. mur. grs. v, at four hours.

8, P. M. Much more swollen and painful, and heavy yellow coat upon it. Cannot articulate at all. Respiration somewhat obstructed. Skin hot. Pulse 96, and bounding.

℞. Venesection ℥xx.

and soon followed by four large Swedish leeches under tongue. Sulph. magnes.

Leeches drew well, and the bites bled freely. He expressed himself "better," and could articulate indistinctly.

Resolution went on rapidly during night, and in the morning he sent me word that he was almost well. Two days after came to office with scarcely a trace of the disease remaining.

CASE II. This was one of personal interest, being myself.

Oct. 10th and 11th. Felt some soreness of fauces, with swelling of left tonsil, and a slight aphthous exudation. Gargled with infusion of capsicum.

12th, morn. Tongue commenced swelling $1\frac{1}{2}$ inches from apex, mostly confined to one side. Soreness of fauces subsided.

℞. Sulph. mag.

It advanced through the day, felt rigid, and became painful. Could swallow fluids only.

13th. Dr. Henn called. Did not advise active antiphlogistic treatment. Feared gangrene.

℞. Gargle of chlorid. sod.

Grew worse. At eve applied four leeches under tongue with temporary relief to pain.

14th. During latter part of night had grown rapidly worse. Could not talk a word. Saliva flowed profusely. Pain intense, a deep throbbing, and a superficial, like inserting hot needles. System but little affected. Pulse 80, and soft. No heat of skin. Drs. Henn and Spencer called. Dr. S. bled me ζ xvj, without much effect. Disease advanced till the

17th. Tongue protruded. Teeth more imbedded in it. Was covered with a most thick dense yellow coat.

2, P. M. At my request Dr. H. gave me chloroform and laid freely open. Some pus and considerable blood escaped. Pain was entirely relieved, and in a few minutes I slept the sleep of the weary. Convalescence proceeded rapidly, and in five days the incision had healed.

Some induration remained at seat of abscess.

CASE III. Continuation of Case II.

In February, the induration increased slowly for a week, then inflammation of tongue commenced. Its course was more rapid, attended with less pain and swelling, and in three days fluctuation was evident. Opened abscess, and it discharged pus for several days. Took hydriod. pot. for some weeks, and induration entirely disappeared in March. No indications of a return to this time.

CASE IV. Patrick O'Connell. Irish laborer. Aged 30.

Oct. 30th 1852. Attack commenced the 29th. could assign no cause.

30th. 2, P. M. Tongue somewhat swollen most at root and advancing toward apex, both sides alike. Smooth and shining. Increased flow of saliva. No constitutional disturbance. Sublingual muscles involved.

℞. Hyd. sub. mur., grs. v,
 P. Doveri, at 4 hours, give 3 times, and follow by sulph. mag.,
 Gargle. Vinegar, ℥iv,
 Mur. ammon., ℥ij,
 Tr. opii, ℥j.

31st, 9, A. M. Swelling increased rapidly. Tense, shining appearance, except in center, where thick yellow coat is collecting. Tongue protrudes. Cannot articulate at all. Pulse 100, full and hard. Skin hot.

℞. Venesection ℥xxx to syncope,
 Sulph. mag., ℥iiss. and irritation externally to throat.

Nov. 1st. Resolution rapidly taking place. Talks indistinctly. Pulse 80, and soft. Tongue cleaning.

℞. Light diet.

Discharged him next day.

From my own observation, and the reported cases within my reach, I believe too much reliance is placed on the "expectant" treatment in this disease. Its course is too rapid, and an unfavorable result too certain, to trust to palliatives, or the "vis medicatrix naturæ." During the five months that the induration remained in my tongue, my mind was constantly haunted with fears of fresh attacks, or (what is more to be dreaded) of malignant disease, and an occasional twinging pain at the seat of induration was not well suited to allay those fears. The determination then became fixed that no one under my care should ever suffer the same, either in mind or body, if active measures would produce resolution and avoid abscess. In neither case did I have reason to regret an *over activity*, but in my own, I regretted, with tears, my "masterly *inactivity*" in the early stage.

In Case I, the effect of the leeches, following venesection, was so satisfactory, that I relied upon them in my own case, but a previous venesection was lacking to prevent reaction. In Case IV I had no leeches, and so was more severe in the general treatment.

In the Journal of April, 1852, page 651, is a reference to Cases I and II, and I was surprised at both the facts and inferences drawn from the partial notes given in my letter. In my own case the symptoms were not "highly aggravated." The next day no trace remained of three of the bites. The

other one was inflamed, but it was the one farthest from the most highly inflamed part of the tongue, and that inflammation subsided very soon, before the glossitis reached its climax, so I did not attribute the final result to any bad effects of leech bites.

The leeches were applied on the 13th, and the "free incision" was not made till the 17th, after pus had formed. In Case I, no "free incision" was made. My notes say that I "scarified it superficially, and but little blood flowed," and it had no effect in arresting the disease, as shown by the farther history of the case.

February 26, 1853.

ART. IV. — *Report of the Obstetric Committee on Anæsthesia in Midwifery, and the Speculum Uteri.* By HENRY MILLER, M. D., of Louisville, Ky. Reprinted, for private distribution, from the Transactions of the Kentucky State Medical Society, 1852.

We have referred, in a previous No. (Dec. 1852) to the very interesting and able report by Dr. Miller, made to the Kentucky State Med. Society in Oct. 1852. The author has kindly placed his friends under additional obligations, by causing it to be reprinted for private distribution. The sound judgment for which Dr. Miller is so eminently distinguished, together with his learning, and large experience in the department with which he has long been identified as a teacher and author, as well as practitioner, will commend his views on the two practical topics to which his report is restricted, to the consideration and confidence of the profession; and inasmuch as the circulation of the Transactions of the Kentucky State Med. Society, and of this reprint of Dr. Miller's report, will be limited, we conceive that we shall confer a favor on many of our readers by reproducing those portions of the report in which the views of the writer are concisely set forth.

On the subject of *Anæsthesia*, the "benefits of anæsthetics" are considered under two heads, viz, *Firstly*, in ordinary Labor, as an anodyne; and, *Secondly*, in extraordinary Labor.

We copy his remarks under both these heads:

First: In ordinary Labor, as an Anodyne.

- Whatever doubts may have existed formerly, the experience of our own times has demonstrated that pain is not an essential component of parturition in the human species, any more than in the inferior animals. There are,

truly, mechanical impediments peculiar to woman, incident to her erect position, and imposed as a tribute for her superior rank in the scale of being. But to surmount these, she is benevolently provided with a muscular apparatus, superior in point of strength to what belongs to any of the inferior creatures; and, in obeying the requisition made upon it, when the fœtus is to be expelled, this apparatus may be exerted, to the utmost of its capabilities, without the accompaniment of pain. There is no more curious phenomenon (curious, because we are so accustomed to witness suffering in travail) than that which we now often see in the lying-in chamber, a woman in the strong throes of childbirth, straining every auxiliary muscle in aid of the powerfully contracting uterus, while the breath is held and the big drops of sweat stand on her brow, and yet she feels no pain, even though consciousness may remain. The motor nervous power is unimpaired, while the sensory is temporarily paralyzed.

“If, then, pain be not essential, we are strongly tempted to suspect that it is *unnatural*, and when we consider that among many families of our race, there is almost a complete exemption from it, our suspicion is converted into assurance. When it may be truly predicted of any physiological function, that it is unnaturally performed, it must be concluded that the organs concerned in it are in a morbid condition, and this was a doctrine held and promulgated by Dr. Rush, which anæsthesia has contributed not a little to confirm. “The philosophers,” wrote Dr. Rush, “in describing the humble origin of man, say that he is formed ‘*inter stercus et urinam.*’ The divines say that he is ‘conceived in sin, and shapen in iniquity.’ I believe it to be equally true, and alike humiliating, that he is conceived and brought forth in disease.” This disease, according to the wise and benevolent author, appears in pregnancy and parturition. In pregnancy, there is inflammation of the uterus, evinced by all the usual phenomena of that morbid state, in other parts of the body, such as swelling and enlargement, hæmorrhage, a full, quick, and tense or frequent pulse, sily blood, and the formation of a membrane upon the internal surface of the uterus, similar to that which is formed upon other inflamed mucous surfaces. “Parturition,” he continues, “is a higher grade of disease than that which takes place in pregnancy,” consisting of “convulsive or clonic spasms in the uterus, supervening its inflammation, and accompanied with chills, heat, thirst, a quick, full, tense, or a frequent and depressed pulse, and great pain.”

“For this disease of pregnancy and parturition, the great remedy of Dr. Rush was bloodletting, and perhaps his partiality for the lancet gave too much depth and intensity to the coloring of the picture which he has drawn

of the pregnant and parturient states. After all due abatement, however, it must be allowed, by the candid inquirer, that in the actual condition of civilized society, much more suffering and danger await women, at such junctures, than in other conditions more simple and natural. We are not going to sing the praises of barbarism,—for we do not believe that mankind were intended by nature to live by the chase, and sleep in wigwams,—but it cannot be doubted that the habits of females, in highly civilized countries,—their luxurious living, their modes of dress, their neglect of exercise, and erroneous mental and moral culture,—tend to unduly exalt the nervous system, while the muscular is depressed and enervated. Hence, with not a few of them, pregnancy is but an interminable train of anomalous maladies, and parturition an enfeebled struggle, in which sensibility displays its usurped ascendancy over muscular contraction.

“To allay this morbid sensibility, — to assuage the pain and anguish which are its product, — is surely a task that the obstetrician cannot deem unworthy of his best exertions. If it be esteemed, upon the authority of Lord Bacon, “the office of a physician, not only to restore health, but to mitigate pain and dolors,” he will find no fairer field for the exercise of this enviable prerogative, for there are no “dolors” that are at all comparable to those of morbid parturition; and, in the qualified sense which has been explained, nearly every case with which we are concerned is of this nature.

“But the benefits resulting from anæsthetics in midwifery are not confined to the alleviation or annulment of pain. There is nothing established by more indubitable evidence than the greater freedom from the aches and ills of childbed, enjoyed by those who have been subjected to their influence. They complain of little or no weariness or soreness, and make better and more rapid recoveries. The writer was early struck, in a remarkable manner, with this fact, as he thinks every observant practitioner must be who uses chloroform, before his attention was particularly drawn to the testimony of others. This testimony it is needless to quote in detail; it may be found, in great abundance, in nearly all the narratives of cases, and in all the communications that have been written on the subject. What adds greatly to its credibility is the further fact that, in most instances, it is the spontaneous declaration of the patients, — their unprompted and grateful tribute, — who, if they have borne children before, under the old régime of strong cries and groans, wonder to find themselves so much more comfortable — so much more like themselves in their best estate.

Secondly: The benefits of Anæsthetics in extraordinary Labor.

“If it be thus safe and salutary to administer chloroform in ordinary labor, we should expect still greater benefits from its use in extraordinary labor, involving, as it does, increased suffering and danger. The pain attendant upon natural parturition is perhaps as great as that which is endured in most surgical operations, while there can be no doubt that in some cases of preternatural labor the agony is both greater and more protracted than in any of the operations of surgery. We refer more especially to delivery by turning in shoulder presentations, when the uterus is emptied of its waters and is molded to the fetus, — leaving but little space for the hand, and disputing every inch of ground, by the indomitable contraction of its fibers. In such a condition, the patient is not only exposed to excruciating pain, but to the risk of laceration of the womb, or destruction of the child. The imprisonment of the placenta by the hour-glass contraction, offers another formidable difficulty, the surmounting of which subjects the woman to cruel torture and no slight danger. We see not how a humane practitioner can justify himself in withholding chloroform in such cases, even if it were not otherwise beneficial than as an anodyne and oblivious antidote of the pain which his ministrations must otherwise inflict. The appeal is, however, not to his sympathy alone. There are valid grounds for the belief that chloroform, in such cases, is not only a pain-annulling but a life-saving medicament. Dr. Channing has collected the statistics of fifty-one cases of instrumental, preternatural and complicated labor, treated with ether or chloroform, among which were nine cases of presentation of the shoulder, — all the mothers recovered, and six of the children were born alive, — which is more favorable, we think, than the general results of such cases. There were twenty-four cases of instrumental delivery, viz., twenty forceps and four craniotomy; all the mothers recovered, and fifteen of the children were born alive, which is also greater success, we think, than generally attends the employment of instruments in obstetric surgery.

“We do not alledge that the statistics of Dr. Channing prove absolutely that chloroform has diminished the mortality of obstetric operations; but they serve, at least, to strengthen the faith, which reasoning is calculated to beget. What is there in manual and instrumental delivery, that enhances its danger more than the additional pain and shock which must be endured? And to neutralize this pain, to foil this shock, must increase the chances of recovery, by counteracting one of the elements of destruction. Pain is

depressing in its influence upon the vital energies, and in its greatest intensity may even extinguish life.

"We do not doubt that when the subject shall be fairly investigated by treating a sufficiently large number of difficult cases in midwifery, similar in kind and circumstances, with and without etherization, a large balance of maternal and foetal life will be found in favor of etherization. This prediction is justified by the statistical evidence collected by Dr. Simpson, which proves undeniably that the mortality of one of the most formidable operations in surgery, viz., amputation of the thigh, has been remarkably diminished by etherization. He gives, in tabular form, the results of this operation in several extensive hospitals, from which it appears that the lowest mortality was thirty-six per cent., or forty-six deaths in 127 cases, in the Glasgow Hospital, wherein ether was not employed; while the same operation upon 145 patients, in an etherized state, but in other respects, under similar circumstances, was fatal in only thirty-seven cases, or twenty-five per cent. — being a saving of eleven lives in every 100 amputations! This furnishes a strong argument from analogy in favor of the saving efficacy of etherization in midwifery operations; for between these and surgical there is no difference.

"We may advance a step further, and venture to predict that chloroform administered in natural parturition, may sensibly curtail it of the mortality incident to it independently of any operative procedures on the part of the accoucheur; so that of an equal number of cases of natural labor, treated with and without chloroform, fewer of the former will die in childbirth than of the latter. The more fortunate issue of the etherized cases will be mainly attributable to the annulment of pain, which is a pernicious ingredient in the process of parturition; but we would not perhaps greatly err if we were to consider every parturition to be a natural surgical operation, and, therefore, as likely to be benefited by chloroform as the surgical operations of man's device. In the natural operation the dislodgment of the foetus is effected by the enormous and forced dilatation of the os uteri, vagina and vulva, and the application of a powerful *vis a tergo*, instead of a tractor, which an accoucheur would employ. After the operation is over, the uterus is in a state, which has been compared by Cruveilhier, (*Anat. Path. du corps hum., livr. XIII.*) to a wound or the stump of a limb after amputation. Where the placenta and membranes had been attached, the mucous membrane is no longer found, or is only seen in the cervix, and about the orifices of the tubes; everywhere else, the muscular tissue appears naked. Upon that portion of the uterine surface, which corresponded to the placenta, may be seen large venous orifices, plugged by coagula, resembling the orifices of the veins of

the stump after amputation, — the simile is Cruveilhier's, not ours. Here, then, is an extensive solution of continuity, which needs to be repaired, and the process instituted by nature for that purpose, is accompanied by more or less fever, having many points of resemblance to that which follows other wounds, and hence denominated by Cruveilhier traumatic fever."

On the subject of the "*Speculum* as a means of diagnosis," the reporter's views are particularly valuable. There are probably few, if any, practitioners in this country, more competent to speak of the value of the speculum from practical experience than Dr. M.

We quote his remarks at length on this topic:

The Speculum as a means of Diagnosis.

"In a paper, on the use of the speculum, read before the *Royal Medical and Chirurgical Society*, May 28, 1850, Dr. Robert Lee makes the assertion, that in the two great classes of organic diseases of the uterus — malignant and non-malignant — and in all the displacements of the uterus, he has derived little or no aid from the speculum, in their diagnosis and treatment. The writer confesses his unfeigned surprise when this assertion, by an author of Dr. Lee's standing in the obstetric department of the profession, first arrested his attention, in perusing the report of his paper in the *London Lancet*. In the discussion which ensued, none of the distinguished gentlemen present appear to have noticed it or animadverted upon it in such terms as it deserves. Let us, then, inquire whether the speculum is indeed superfluous, first, in organic diseases, and secondly, in displacements of the uterus. It will be conceded, we presume, that inflammation is an organic disease, and that it is, moreover, the architect of numerous other diseases of the same class. Now, Dr. Lee virtually affirms that the speculum is not needed to discover the existence of inflammation of the cervix uteri, and upon this we join issue with him, being willing to stake the fortune of the speculum on its trial by a jury of our peers.

"If the speculum be discarded, we cannot discover inflammation in this, its favorite lurking-place, except by the symptoms that accompany it, or by the touch, in the usual mode of examination. Will the symptoms reveal it? Their uncertainty and the dimness of the light they shed, are proverbial. There may be pain or a sense of heat in one of the iliac regions, together with back-ache and neuralgia of the musculo-cutaneous nerves of one or both thighs. There may be frequent and painful micturition or tenesmic irritation of the rectum. The menstrual function may be deranged, and there

may be leucorrhœal discharge. But any or all of these symptoms may be present, and yet inflammation may not exist, while there may be inflammation, and few or none of these symptoms be complained of. Of the truth of these remarks no practitioner can be ignorant, who is much conversant with the diseases of females, and is familiar with the use of the speculum. The writer well remembers the case of a lady, the mother of two children, who miscarried in her third pregnancy, and suffered severely with her head for more than a year afterward. She complained of fullness of the head, with more or less pain continually, and occasionally with very acute pain. On the part of the uterine system there was no evidence of any thing amiss, except that she did not conceive again, and menstruation, though regular, was scanty, seldom lasting more than a day, and amounting to a mere show. There was not, at any time, leucorrhœal discharge, nor did she complain of pelvic pains, and yet when examined with the speculum, chronic inflammation, with hypertrophy of the uterine neck, was discovered. This was cured by the usual treatment: menstruation returned to its healthy type, and the cephalic symptoms gradually abated.

“Can the touch detect inflammation of the cervix? This question might be answered by another; could a blind surgeon detect cutaneous inflammation by the touch? The truth is (and every accoucheur well knows it) none of our senses is more deceptive than the touch, or more frequently leads to mistakes. The only discovery which can be made by it, in the matter under consideration, might be made as well by any other instrument as by the finger, viz., the existence of morbid sensibility in the cervix uteri. When the inflamed cervix is pressed upon by the finger, the patient usually winces, and so she would were it pressed upon by a stick. Morbid sensibility may, however, exist independently of inflammation, and cannot, therefore, be regarded as furnishing conclusive evidence in such an investigation.

“Upon the whole, then, the practitioner who relies on the symptoms and touch only, for his diagnosis in these cases, can never know of a surety that inflammation exists: he may surmise it, but cannot possibly have any greater certitude than could a blind oculist concerning the existence and nature of inflammation of the eyes.

“Ulceration belongs also to the class of diseases, in which, according to Dr. Lee's assertion, little or no aid is to be derived from the speculum,—howbeit he is incredulous as to the occurrence of this morbid state, in the female sexual organs, except to a very limited extent. He says explicitly that he has never seen ulceration of the os and cervix uteri, which was not of a specific character, especially scrofulous and cancerous. To fortify

himself in this position, seems to have been the main object of his paper; for could it be proved that ulceration is a rare disease in these parts, the speculum might the more readily be driven from the field. Dr. Lee's clique, who rallied around him in the debate, felt equally with himself the necessity of expunging ulceration from the list of female sexual maladies. To accomplish this, they were forced to maintain that ulceration necessarily involves a palpable loss of substance. It is readily admitted that, in this sense, ulceration is a rare form of disease of the os uteri; we are not sure, indeed, that we have ever once met with it, nor have we a right to look for deeply excavated ulcers in such a situation. The mucous membrane alone is commonly implicated, and this is here of such exceeding tenuity that it cannot be dissected from the subjacent tissue. The nearest approximation to a dissection, which can be made by the most skillful anatomist, is to lift it up, in delicate patches, upon the point of a sharp lancet. Supposing the membrane to be destroyed, in its whole thickness, by the ulcerative process, there would not, therefore, be palpable loss of substance or any thing like an ordinary ulcer upon the skin, or even upon the mucous membrane of the intestines. But there is, nevertheless, what fulfills the definition of ulceration, namely, a solution of continuity, in a soft part, accompanied by a purulent discharge, for it may be brought to light by the speculum, and when wiped with a sponge, a raw and often a bleeding surface is exposed. What matters it, if Dr. Lee and his partisans choose to call it 'abrasion,' 'excoriation,' or by any other name. Such a surface, produced by morbid action, were only the epithelium destroyed, is ulceration; for there is solution of continuity and there is purulent secretion.

"Ulceration of the os uteri is usually accompanied by inflammation, and the symptoms to which it gives rise are nearly the same, only there is more constantly purulent leucorrhœa. But this discharge does not always attend it; for the secretion may be so slight as to be absorbed, and there may be purulent discharge without ulceration. Ulceration cannot, therefore, be predicated of any case from the symptoms only. It may be discovered by the touch, when the roughness of the affected surface is well marked, but in the very great majority of instances, nothing can be positively affirmed until the parts are brought under ocular inspection. Of this, every day's experience convinces the writer more and more firmly. While inditing this report, he had occasion to examine a lady, from a distance, whom one of the most distinguished surgeons in this country, after examination by the touch alone, pronounced to be laboring under displacement of the womb, the organ being, as he assured her, perfectly free from disease: the writer was soon satisfied,

by a specular, as well as tactual examination, that there was chronic ulceration of the os uteri, but no displacement of any kind!

"The committee will next attempt to estimate the claims of the speculum, as a means of diagnosis, in displacement of the uterus, the other class of cases, in which Dr. Lee says it is of no value. None of these displacements is clearly indicated by the symptoms alone, except retroversio uteri occurring in the pregnant state, in which the sudden and total suppression of urine, together with the severe sufferings of the patient, points plainly enough to its existence. But in the non-gravid state, neither retroversion, nor anteversion, nor prolapsus (the most common of all the displacements) is accompanied by such symptoms as throw any satisfactory light on the subject. To the touch, at least, an appeal must be made and through it we may learn that the organ is displaced, and the manner of its displacement; but we cannot learn its pathological condition, a capital hiatus in the information we are in quest of, for the speculum has taught us the frequent, nay, the almost constant coexistence of inflammation or ulceration of the cervix uteri. So true is this, that the writer can conscientiously declare that, since he has used the speculum freely in his practice, he has seldom seen an instance of prolapsus or retroversio uteri, uncomplicated with inflammation or ulceration of the cervix; and he is becoming more and more skeptical as to the existence of simple displacement of the uterus. His own view of the pathology of such cases, is that inflammation is the primary and essential disease, while the displacement is merely a sequence. Such is the doctrine advocated by Dr. James Henry Bennet, in his valuable practical work on 'Inflammation of the Uterus,' who attempts to explain the occurrence of prolapsus on the principle of the increased gravity of the uterus, acquired by inflammation. Dr. Meigs rejects the doctrine, and thinks he has most triumphantly refuted it by showing, as we think he has very conclusively, the insufficiency of the explanation. (*Females and their Diseases*, p. 137.)

"But it does not seem to have occurred to Dr. Meigs that the doctrine may be true, while the explanation may be false. Grant the existence of inflammation of the cervix as the antecedent, and it may be that the irritation, established in the part and propagated to the neck of the bladder and to the rectum, will eventually cause prolapsus by the bearing-down efforts which it provokes, and this, we suspect, is the true etiology.

"Be this as it may, and whether inflammation is the antecedent or the consequent of the prolapsus, the writer reaffirms, without the fear of successful contradiction, that inflammation or ulceration exists in nearly every case

of displacement of the womb, and that it can be detected only by the speculum.

"But Dr. Lee, as we have seen, not only renounces the speculum in the diagnosis, but also in the treatment of the whole class of diseases we have been considering. It is difficult to imagine the grounds of this renunciation. Can it be that the treatment of these diseases, by other means, has been so successful in his hands as to preclude the hope of improvement? If so, we sincerely congratulate him on his good fortune, in a field where all other practitioners, from time immemorial, have met with little else than discomfiture. For our own part, we are not ashamed to confess that, until we called the speculum to our aid, we were defeated on every hand, or, at best, victory so seldom perched upon our standard, that we were bound to regard our success as fortuitous, rather than merited. We never cured a case of prolapsus by the pessary, or of long-standing leucorrhœa, connected with inflammatory or ulcerative disease of the cervix, by constitutional treatment and the ordinary local appliances.

"Such *fillibustering* may succeed in recent and trivial cases, but when the disease is more strongly intrenched, it can only be dislodged by a superior force operating directly and systematically upon it.

"These uterine affections are essentially local in their nature: they owe their origin to local causes, and are most successfully treated by local remedies. But the remedies must be sufficiently potent to make an impression upon the disease. The sprinkling of an inflamed or ulcerated os uteri, with simple or medicated water, by means of a syringe (the only local remedies resorted to by the *fillibusters*) cannot be more efficacious than such piddling ablutions upon other parts of the body. What would be thought of a surgeon who should attempt to cure an external chronic inflammation by squirting a little water or solution of lead or zinc upon it, two or three times a day?

"The more potent remedies which are addressed to the affected part through the speculum are, chiefly, the local abstraction of blood by scarification or leeching, and superficial or deep cauterization, according to the circumstances of the case. It is not the design of the writer to enter into details on this part of the subject; he begs to refer the society to practical works, particularly to Dr. Bennet's treatise, already alluded to. He will, nevertheless, submit a few annotations, suggested by his own experience in this branch of practice, which has been pretty extensive.

"*First.* Local depletion may be effected as well by scarification as by leeching, when the inflammatory congestion occupies the superficies of the

a uteri, and ought to be preferred, because it may be done more expeditiously, and is far less revolting to the patient. When the inflammation is deep-seated, and there is little or no discoloration upon the surface, leeches should be employed, and half a dozen are commonly sufficient to procure as free bleeding as is desirable. Local bloodletting is a valuable part of the treatment of these cases, and ought always to be premised, whenever there is any considerable degree of inflammation. It is a good preparation for cauterization, and may be advantageously repeated, in conjunction with cauterization, until the inflammatory congestion is subdued.

“Secondly. With the same view, cold mucilaginous injections — infusion of flaxseed or slippery elm — should be thrown into the vagina, by the patient, three times a day. But these will accomplish nothing unless a good syringe is provided, and the patient properly instructed in its use. The injections should be taken in a recumbent posture; the syringe ought to hold several ounces and have a pipe, with a bulbous end, long enough to reach the superior portion of the vagina.

“Thirdly. When the inflammation or ulceration is confined to the mucous membrane, with only slight enlargement, and no induration of the cervix, cauterization with the nitrate of silver in substance, is the only application which will be found necessary in most cases. This ought not to be repeated too frequently — an error, which the writer has reason to believe, is committed by some — not oftener than once a week. Six or eight of these hebdominal cauterizations may suffice to cure the disease; but in some cases, a longer perseverance may be necessary, and in a few, the inflammation may prove altogether refractory. In such instances, the writer’s practice is to cauterize once superficially with the potassa cum calce, and afterward, with nitrate of silver as at first.

“Fourthly. Should the inflammation have extended to the proper tissue of the cervix, and resulted in induration, deep cauterization with the potassa cum calce will be indispensable to restore the part to its normal state, and heal any ulceration which may exist. It is quite useless to treat such a condition with the nitrate of silver; the ulceration will seldom be cured by it, and it can make no impression upon the deeper-seated disease. The writer has practiced deep cauterization, in many cases; in several, he has used the actual cautery, and he has never known any serious accidents to follow. He is always careful, however, to apply the caustic through a tubular speculum, and to sponge off the part, so as to guard against any of the caustic remaining and spreading to the sound parts, after the withdrawal of the speculum. With this precaution, he considers it to be as safe to apply caustic to the

cervix uteri as to the skin. Much obloquy has been cast upon the speculum on account of alleged abuses of cauterization, and the writer doubts not that there is some foundation for it; for he can easily conceive that the careless or inexpert use of such a potent agent, may produce extensive inflammation and sloughing, followed by unnatural adhesions and contraction of the genital passage. But such consequences are attributable to the awkwardness or ignorance of the operator, and are no more chargeable to the speculum than is the transfixion of the vein in phlebotomy to the lancet. The writer can truly say that no such consequences have ever happened to him or need happen to any one, fit to be trusted with the speculum.

"*Fifthly.* Rest in a recumbent posture, more or less strictly guarded, according to the degree of inflammatory action that exists, is a material adjuvant in the treatment of these cases: and where this cannot be enforced, the disease is greatly prolonged, and may prove altogether ungovernable.

"Exercise, or even the erect or semi-erect position tends, in a direct manner, to increase the uterine congestion and aggravate the sufferings of the patient. The writer cannot doubt, from what he has seen, that much mischief is often done by urging the patient to take exercise, under the fallacious idea that weakness is the sum total of her ailments, and that if she can only be strengthened by air and exercise, all will be well with her.

"So strongly is the imagination of some physicians haunted with the bugbear, weakness, that they will persist in keeping the patient in motion, notwithstanding that every step is a dagger to her. When shall more rational views obtain currency in the profession? How long shall a mere effect engross the attention, while the cause is overlooked?

"The writer was recently consulted in the case of a lady, who suffered greatly from pelvic pains after her second confinement, increased by exercise or the erect position. She had hæmorrhagic discharges from the uterus for several weeks after parturition, with almost daily febrile excitement, intense thirst, loss of appetite, and general debility. The debility, unfortunately, absorbed the attention of her medical attendant, and to remedy this, exercise in a carriage was commenced on the eleventh day after her accouchement, and persisted in daily, in spite of her remonstrances, extorted by the increase of her suffering, and finally, she was sent away on an excursion in pursuit of the *ignis fatuus*, 'strength.' When she returned home, a specular examination was made, and a high degree of inflammatory engorgement of the uterine neck and upper portion of the vagina, with ulceration around the os, was discovered, which had existed doubtless since her delivery.

"*Sixthly.* Although the local treatment is paramount to every thing else,

the state of the general system must not be overlooked or neglected. If constitutional irritation exist, it must be subdued by appropriate remedies, or if any of the functions are sympathetically deranged, they must be restored to a healthy condition by suitable treatment. In recent cases, some degree of febrile excitement not unfrequently exists, and to allay this, it may be proper to put the patient upon an abstemious regimen, to purge actively every day or every other day, and if there be hardness as well as acceleration of the pulse, general bloodletting may be necessary.

“Dr. Dewees was well aware, though he had not the ocular proof, of the existence of uterine and vaginal inflammation, in many instances of leucorrhœa, which is only another name for the disease we have been considering, and the success of his treatment was doubtless attributable to the bleeding and purging he prescribed, rather than to cantharides, which he regarded as a kind of specific. This is fairly to be inferred, from the fact that none of his cotemporaries or successors have been as fortunate in the use of cantharides as himself, which can be accounted for only by supposing that they have relied principally upon the specific, to which the multitude are always prone, to the neglect of due attention to the state of the system. It is not intended to be asserted that cantharides is devoid of all remedial virtues in these cases. By its action upon a contiguous and associated viscus, it may exert some beneficial influence upon the genital organs; nevertheless we are persuaded that the antiphlogistics, so vigorously employed by Dr. Dewees, had a larger share in extinguishing the disease than had the cantharides, pushed ever so often *usque ad stranguriam*.

“In more protracted cases, the general state is characterized by veritable debility, a languid circulation, coldness of the extremities, and impaired digestion and assimilation. Under such circumstances, it will be proper to administer tonics, especially some of the preparations of iron, and to regulate the secretions and excretions by the use of alteratives and purgatives. The selection of these will be governed by the indications of each particular case. As to purgatives, it is necessary to observe that only such of them are admissible as may be required to procure one full alvine evacuation daily, to effect which a pill or two of rhubarb and extract of colocynth, or of rhubarb, aloes and soap, may be taken every night.

“Mercury, iodine, arsenic and antimony, are among the most powerful alteratives, and the indications for the use of remedies of this class may be fulfilled by the various preparations and combinations of these agents.

“As to sarsaparilla, which is so often prescribed, we do not know that we have ever obtained any good from it, even when furnished by the regular

apothecary; while sure we are, that the quackish preparations of it, which find their way by the hogshhead into the stomachs of our nostrum-loving population, are utterly worthless."

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS.

The Country Doctor.—Some of our contemporaries pride themselves upon never quoting anything from "the secular press;" but we have always preferred to adopt the wiser course suggested by the motto of the Southern Medical Journal—"je prends le bien où je le trouve"—and in so doing do not feel that any one's professional dignity has been injured, or any one's professional robe sullied by contact with the common herd. Acting on these principles, we give below an article from the Knickerbocker, entitled "The Country Doctor; a faithful Autobiography. By Glauber Saultz, M. D." Perhaps all our readers can narrate facts which surpass these; we are sure almost all can equal them; but then it is easier to laugh at another's misfortunes than our own, and "Doctor Saultz's" feelings will not be hurt if every one laughs as hard as it will answer for a dignified M. D.; neither will he make a charge for this visit:

I had stumped about the country for a dozen years or so, in the same equipage, having wonderful success in curing "cases," but half the time cheated out of the credit of it by catnip tea. I took a notion to cast up my books to see how rich I was, and what could be made of outstanding accounts. It cost a great many evenings of hard work to arrive at the knowledge that, all debts being paid I was not worth a "brass farthing"—not a red cent. Notwithstanding all the lucrative cases of typhus which I had managed, I remained poor. I believe that people in the city pay their fees with alacrity, because the charges are exorbitant. When a bill for a hundred dollars, for looking two or three times at a sick child, is presented to one who lives in a well-furnished house in the upper part of the town, the very largeness of the demand is a delicate compliment upon his ability to pay. The man of the house sits down at a handsome secretary, and draws out a clean check for the full amount, saying, "Doctor, you are very moderate: now that Jacky is out of the woods, come in, in a sociable way."

As soon as the messenger is gone, the *pater-familias* exclaims, "What an outrageous bill! It is an expensive luxury to be sick." However, it has its advantages to be attended by a fashionable doctor, as it has to worship in a fashionable church. On one occasion I was called in midsummer to attend a sick man on the sea-shore. After several days, his family physician, the

renowned Doctor Jallaps, arrived from the city, and the patient was soon after on his legs, no thanks to me, and ready for the surf.

"How much are you going to charge him?" said Doctor Jallaps.

"Twenty-five dollars," said I.

"Pohl!" said he, "make it a hundred. He expects it."

"If he expects it," said I, "it would give me great pain to disappoint his expectations;" whereupon I acted advisedly, and received an honored check for a round C. on the Phoenix Bank.

On another occasion, when attending one of my own patients in the same vicinity, while crossing the "big bridge," when the tide was up, I came near being drowned. My sulky was soon afloat, but the horse, being a good swimmer, reached the opposite bank. Now, besides risking my own life, I fairly dragged the patient from the very gates of death. I got him out of a bilious-remittent, drove the jaundice out of his skin, and when I came to ask him for ten dollars, he blackguarded me like a chicken-stealer, and would never employ me again. The fact is, that people in the country abhor taxes and a doctor is the worst of publicans. To be sick they think is a dead loss, which they unchristianly grumble at; but to have to pay for being cured irritates them beyond measure. Oh! how meek they are when they lie prostrate in a burning fever—when their teeth chatter, and the whole house jars with their shaking ague! Oh! how welcome the latch is lifted up to admit you when life seems to hang upon a hair! But get them on their legs, and the first thing which they forget will be that they were ever on their backs. If many of them do pay you, it is under protest, procrastinating the settlement to a time when the account might be outlawed, clipping down the fair proportion of a just bill, and giving you the most ragged representative of money.

I say that when I came to overhaul my accounts, I was not worth anything, and therefore arrived at the conclusion that it was high time to marry a wife who would take care of my money. I did so, and found my condition better, but for some years had a hard time of it. My children were extremely pettish and peevish, and what with nocturnal calls, I had not a night's rest for five years. If anything ailed them, they were sure to cry the night long; but if they were well, they woke up long before the crowing of the cock, climbing over me at the very moment when I had composed my head for a short morning nap. But paternal philosophy can well be reconciled to the sweet music of "crying babes," some thousands of which have been imported into New York during the present year. But the number of people taken sick in the day-time, who send for the doctor at night, produced a compound fracture of my time, which seldom gave me a comatose state. It is the sweetest of all consolations to lay a weary head upon the pillow, with the thought that rest awaits you until the dawning light. Whatever carking cares have vexed you, that is a long season of immunity which stretches through the dark hours of the night. Then do the strained muscles lapse into the most easy attitudes in the yielding couch, and the taxed intellect is still, and you bolt the door upon ingratitude and strife.

But to lie down without security from disturbance is enough to frighten away sleep. Such is the lot of a country doctor. I could relate innumerable instances of the utter disregard with which he is routed from his bed, without occasion, at all hours. Here is one in point:

I arrived late one winter evening at my own door, after a hard day's toil.

With what a feeling of relaxation did I divest my feet of heavy boots, set them smoking at the fire, and then regale them in easy slippers! Then, wrapping about me a soft padded gown, with what luxury did I fall back in my arm-chair, peruse the daily paper, and sip a cup of tea! "Now," said I, "the labors of the day are over. A storm is brewing out of doors. I hope that no body will come here to-night. If they do, I won't go. Let them go after Bogardus. I won't immolate myself for any body. It is unreasonable." With that I pulled down my ledger and made a note of the day's visits, one half of which were to poor houses, negro huts, and Irish shanties. As to this class, they loved me like a brother, and their confidence in me was unbounded. They sent for me if their bones ached, or if their corns hurt them, and I went with all speed, though I sometimes had occasion to scold them. Before retiring for the night, I opened the outer door, as was my custom, to see the state of the weather. It was a tremendous night. The moon shone palely, but the wind blew a hurricane. It rained, it hailed, it snowed, it blowed. I thought again of the poor mariners on the coast, and with a silent prayer for them, and all houseless, unprotected ones, I closed the door, and went to bed. I had just recovered from the shivering sensation of cold sheets, and become conscious of grateful warmth, while that delightful drowsiness which borders upon sound sleep stole over me, when there came a knocking, impatiently repeated, enough to wake the dead. "Bless me!" I groaned out, crawling out of bed, and lifting the sash, "what do you want?"

"Doctor, want you to come right straight away off to Banks's. His child's dead."

"Then why do you come?"

"He's p'isoned. They gin him laud'num for paregoricky."

"How much have they given him?"

"Dono. A great deal. Think he won't get over it."

"When did they give it to him?"

"This arfternoon."

"Why did n't you come sooner? How do you think I am to go two miles on such a night? Have you brought a wagon?"

"No."

"Then I won't go. Tell them to ——," and having prescribed hastily out of the window, I closed the sash, and went back to bed. But the howling wind and rattling sleet against the panes had not that soothing effect which they have to one who lies snug and warm and irresponsible in his couch. "What," said I, "if that child should die through my neglect! Will it absolve me from criminality because the parents are poor? I will go: I must." With that I leaped out again, kindled a match, and went down into my office. Not choosing to wake my man Flummery, or to disturb my old horse, who was craunching his oats, and housed for the night, I took my stick, and set out to walk. The snow water went through my shoes like a sieve; my neck and bosom were instantly covered with sleet. Nevertheless, I had some humorous thoughts while breasting the storm, and composed a Latin distich by the way. I had just got the last foot of the pentameter correct, when my own foot struck against something which looked like a black log. On scrutiny, by the light of the moon, I found it to be my old patient, Timmy Timmons, apparently sound asleep, with his beloved rum-jug by his side. In vain I shook him, to make him aware of his situation, and see if

the spirit had left his body. I shook the rum-jug, but there was no spirit there, not a drop. "Timmy," said I, "wake up." No answer. I then kicked him, but he bore it as if he had been used to kicks. "He is dead," said I, and passed on to the next house. There, while opening the gate, I was fiercely attacked by a stout bull-dog; and while keeping him off, and fighting my way up to the house, the master came out in his shirt-tail with a loaded gun. "Do n't you breechee me?" said I, as he examined the priming; "it is the doctor."

"Souls alive!" responded he; "I thought it was a thief! I'm glad you spoke when you did. In a minute more I should have popped you over, Doc'. Sorry to do that. My son John's got the fever-aig. Here, Bull, Bull, Bull!—g' home, sir!"

"Timmy Timmons," said I, "is lying out in the lane, drunk or dead, I do n't know which; dead drunk at any rate. He must be looked after."

"Wait till I put on my breechee. What a wunnerful night! Won't you come in and get warm?"

"No; get on your breeches and make haste."

"Guy! when I first heered you, I thought it was Lawrence comin' to break house. He's a desput fellow. So I gets up and looks out of the window, and then I went into the corner to find my gun, and if I did n't——"

"Come, come; do you want——"

"To get the rheumatiz? No, I do n't. Hold on, Doctor; be down in one minute."

We returned to the congealed Timmons. My coadjutor took up the jug, shook it, and said, "Not a drop." He then smelt it.

"It is rum," said I, "the cause of all this misery."

"No, Doctor, not *all* rum; there's been a little *molasses* into the jug, by the smell of it."

"Lift him up," said I. He did so, and carried his burden home, where I brought Timmy to life.

I now trudged on upon my original errand, hoping to save another life more valuable than that of Timmons. Arrived at the house, I perceived it shut up as if hermetically sealed. Not a light was to be seen. I knocked furiously, and at last a night-cap appeared from the chamber window, and a woman's voice squeaked out, "Who's there?"

"The doctor, to be sure," said I; "you sent for him. What the dogs is the matter?"

"Oh, it's *no* matter, Doctor. Ephraim's better. We got a little *skeered*, kind of. Gin him laud'num, and he slept kind o' sound, but he's woke up now."

"How much laudanum did he swallow?"

"Only two drops," said she. "'T as n't hurt him none. Wunnerful bad storm to-night!"

I buttoned my coat up to my throat, turned upon my heel, and tried to whistle.

"Doctor, Doctor."

"What do you want?"

"You won't charge nothin' for this visit, will you?"

Now, as I traveled back on foot, the moon became obscured, the driving sleet blinded the eyes, I heard the Atlantic breakers booming and beating upon the coast; and with head down, like a bulrush, I arrived at my own

door, wet and disconsolate, saying to myself, "THAT LITTLE PLANT CALLED PATIENCE DOES NOT GROW IN EVERY GARDEN!"—*New Hampshire Journ. of Medicine.*

Case of Portal Phlebitis. Reported by C. E. ISAACS, M. D.

On the 4th of January, 1853, I was requested by Dr. Blakeman, to make a post-mortem examination of the body of a man aged 25, who, for some five or six weeks previously, had complained of "dyspeptic symptoms," and occasional attacks of colic. On the first of December, while working in his store, he was seized with very severe and griping pains in his bowels; these were relieved, at the time, by brandy. He took, the next day, some of Lee's pills, followed by castor oil, which, however, did not operate until three days afterward. Dr. Blakeman was called on the 8th, and found him complaining of some slight derangement of the stomach, want of appetite, etc., etc. Calomel and rhubarb were given, and operated easily and freely, with much relief. The bowels remained unusually open for several days after. On the 12th, the patient felt so well that he went out to visit some of his friends, but the pain in his bowels returned, and he sent for his physician. He found him free from his pains, which he described as having been very severe and spasmodic, with a feeling of constriction around the abdomen. These symptoms continued, with intermissions, for several days. There was no swelling, nor distension, nor any tenderness on pressure, although repeated examinations were made with reference to these points. He could not lie on his right side, and severe dragging pain was felt whenever he attempted to do so. It should be mentioned, that when first taken sick, he experienced a slight *chill*, that he had four chills during the progress of his disease, and that they were about one week apart. The first was not followed by sweating, but the others were so. The last *chill* was on the 25th, lasted one hour, and was followed by the most profuse sweating. Previously to this, the patient had sat up, and walked about; but after this occurrence, he was entirely confined to his bed. He could sleep only by taking large doses of morphia, to relieve pain. The pulse, during the course of the disease, was about 80, but after the last chill, rose to 120, and upward, and so continued until his death.

With the griping pains, there was occasionally nausea, and sometimes vomiting, during the whole continuance of the disease; but, generally, the appetite was tolerably good. The skin was usually moist and cool. He had, at times during his illness, passed bloody urine; occasionally it was loaded with lithic acid, would suddenly become clear, and again bloody, etc., etc. Pyæmia was diagnosed by Drs. Blakeman and Post, some days previously to death.

Post-mortem. Present, Dr. Blakeman, Professor A. C. Post, and Dr. Eastman, of Owego. The omentum was very thin, and spread out over the surface of the intestines. On raising it up, a large drop of purulent matter was perceived, which induced me to make the examination with great care and caution; and it was at length ascertained that the upper part of the ilium had passed through a rupture, or opening, in the omentum; and had been *partially* strangulated by the latter. Above the constriction, the jejunum was very much distended, being nearly as large as the colon, while below, the

ilium was of the usual size; abscesses existed throughout the mesentery, and purulent matter in its veins. On removing the liver, and laying open the *vena porta*, this vein was found to contain purulent matter. A mass of coagulable lymph and partially discolored blood was tightly adherent to the internal serous lining of that vessel. On cutting across the liver, in various directions, pus issued from the cut orifices of the branches of the portal vein. The hepatic veins were healthy. The spleen, where cut across, showed various points of purulent matter throughout its substance. The matter from the portal veins, and also that from the spleen, was examined microscopically by Dr. Clark, and exhibited very clearly globules of pus. Above the constriction, jejunum was more vascular than natural, and there were some flakes of coagulable lymph on its outer surface. The kidneys were not remarkable, but on pressing on the cut surface of the pyramidal portions, semi-purulent matter could be pressed out. Blood was extravasated in small spots under the mucous membrane of the bladder. The other organs were in a healthy condition. The constriction had been partial,—not enough to entirely interrupt the passage of substances through the small intestines, but sufficient to cause obstruction to the intestinal circulation, and consequent inflammation of the portal veins with abscess of the mesentery.—*N. Y. Med. Times.*

Observations on the Treatment of some forms of Obstruction of the Bowels. By THOMAS AICKIN, M. D.

The fatal termination of the affection which has been termed "Ileus," or the "Iliac Passions," seems to be owing in many instances to the prostrating influence of the symptoms usually attendant on the local visceral lesion, independent of any progressive change of a necessarily fatal character occurring in the latter. Abercrombie's statement, that "ileus may be fatal without obstruction," and that "it does not appear to be necessarily connected with obstruction in any part of the canal," is calculated to excite some doubt as to the propriety of directing treatment almost exclusively to the removal of an obstruction, the nature, and possibly the existence of which continue to be matter of surmise and not of positive diagnosis.

I have had opportunity of observing the progress of some cases of obstinate constipation, in which the symptoms of ileus set in with great intensity. I have in some instances found it possible to subdue those symptoms many days before the removal of their assumed cause and the reappearance of feculent discharges; and I have remarked that the prolonged retention of the latter was not productive of dangerous consequences when the abdominal pain, vomiting, and general distress, were in the first instance controlled by appropriate treatment.

The first case which served to convince me of the injurious effects resulting from the repeated administration of purgatives, to the exclusion of other remedies, happened to be one of those in which purgatives seemed to be specially indicated. The patient, a young lady, had been subject to torpid bowels and pain, chiefly in the cœcal region; vomiting and severe abdominal pain supervened on protracted constipation. She was harassed for three days with these symptoms; the bowels remaining obstinately confined, notwithstanding the repeated exhibition of purgative medicine both by the mouth

and per rectum; the stomach retained nothing, she had no sleep, and scarcely an interval of ease, up to the morning of the fourth day from the invasion of the acute symptoms, when I saw her for the first time. I found her much exhausted, the pulse very small, the extremities cold, and the nausea and straining to vomit most distressing; the bowels had not been moved for many days. On examination, a large and resistant mass was plainly perceptible in the cœcal region; it was about the size of a large orange; there was some surrounding tenderness; a blister had been applied over this region, and large quantities of purgative medicine administered before I saw her. Finding that the treatment previously adopted had been ineffectual, and that her strength was rapidly failing, I at once put her on calomel and opium—two grains of the former and one grain of the latter, in pill, were given, warmth applied to the extremities, hot stupes to the abdomen, and abstinence from all fluids, except cold water in small quantities, enjoined; the pill was retained, and the stomach became quiet. The calomel and opium were repeated every fourth hour. On the following morning—namely, the fifth day of her illness, I found that she had obtained upward of six hours of sound and refreshing sleep, the vomiting had been arrested, but the abdominal tenderness had increased. General reaction was now fully established; the pulse, which on the preceding day was almost imperceptible, had become full and strong, and the countenance flushed. Twelve ounces of blood were taken from the arm, and in a few hours after leeches were placed over the cœcal region, and followed by a large poultice. The calomel and opium were persisted in. On the sixth and following days, she was almost free from general or local suffering; the gums became tender. Croton oil was now given in drop doses, which caused some return of the tormina; a blister was applied over the cœcal tumor; the blistered surface was dressed with mercurial ointment. On the tenth day, there were some small fragments of hardened fœces discharged after the administration of an enema; on the following days, gentle frictions with mercurial ointment were repeatedly made over the mass in the cœcum, which ultimately broke up and was discharged piecemeal. Its fragments were of a dark color, and strikingly resembled shoemaker's wax blended with some fibrous matter, both in their color and amazing tenacity. This patient was sustained by the administration of enemata of beef-tea, thrown up by the tube. Three pints were thus given every day from the subsidence of the acute symptoms, until the stomach became able to tolerate food. Her recovery was perfect.

In another case of obstruction, accompanied in its early stage by the usual distressing symptoms, the sensation of pain and distress was chiefly referred to the umbilical region, several attempts had been made to open the bowels by the administration of oily purgatives, but without effect; the incessant retching proved so distressing that in the course of four or five hours from its commencement the patient became much exhausted. Calomel and opium were administered in pills, repeated every fourth hour. Leeches were applied over the umbilical region, and iced water was allowed in small quantities. Under this treatment the vomiting became less troublesome, and the abdominal pain and tenderness were much diminished; large linseed-meal poultices were applied to the abdomen, and afforded much relief. Eight days elapsed from the commencement of the treatment of this case before fœcal evacuations were procured; no distressing symptoms were manifested after she was brought under the influence of opium. On the fourth day, the gums became

tender; the calomel and opium were now given at longer intervals. Attempts were again made to free the bowels by repeated doses of castor oil, and by large enemata, but up to the eighth day without effect; on and after which liquid fæces were discharged. Blistering the abdomen and mercurial dressings were also in this case attended with manifest advantage. Her recovery was unchecked by any untoward symptoms.

In another case of obstinate constipation, occurring in a lady who had been constantly obliged to resort to purgatives, the symptoms of obstruction set in with great violence. I found her on my first visit in a state of great exhaustion; she was straining at vomiting, and brought up frothy mucus in large quantity. She was so much sunken as to be unable to reply to the queries put to her; the pulse were scarcely perceptible, and the extremities cold. As soon as the cessation of the retching allowed a momentary pause, I gave her four drops of black-drop in a teaspoonful of water, had warmth applied to the extremities, and enjoined as perfect repose as the nature of her suffering would admit of. After the lapse of an hour, I found that the opiate had quieted the stomach, and that reaction was beginning to set in. Hot turpentine stupes were applied to the abdomen, and ten grains of calomel and a drop of croton administered. Finding, after the lapse of some hours, that pain followed the purgative, and that the vomiting returned, I repeated the opiate, and directed the administration of a large enema containing turpentine and oil. The bowels continued obstinate, and the paroxysms of pain, which were evidently owing to the stimulus of the purgative, became very distressing. Black-drop was again given; she passed a bad night, occasionally straining very much at vomiting, and brought up dark-brownish matter in small quantities. On the day following, finding that the bowels had resisted the purgative, and the abdomen continuing very much swollen and tender, I prescribed pills of calomel and opium; these were regularly persisted in for upward of three days, until the gums became tender. After she had taken three or four pills, the symptoms were much improved; large enemata were occasionally exhibited, and her strength was supported by fluid nourishment given in small quantities. About the tenth day, the bowels were slightly moved, and on the two following days dark-colored fetid discharges were freely passed. It may be remarked, concerning this case, that the first decided relief was obtained by opium, while the removal of the obstruction, whatever its immediate cause may have been, was obviously aided by the mercurial action, inasmuch as purgatives produced no effect previous to the manifestation of the latter; a few doses of castor oil were then sufficient to procure stools, after which nothing occurred to interfere with her recovery.

The causes of ileus are various, and their differential diagnosis is, in many cases, impossible. The affection may occur, as noticed by Abercrombie, in "the simple form, without any previous disease," or "with previous disease which deranges the muscular power without mechanical obstruction," or it may occur "with mechanical obstruction." Now, it is sufficiently obvious, that when distressing or dangerous symptoms arise as consequences of an undiscovered or merely surmised lesion, our treatment should in the first instance be directed to their alleviation or removal. The possibility of effecting this, even when their exciting cause cannot be directly interfered with, is illustrated by everyday practice. Thus, the symptoms following obstruction of the bowels which are developed in virtue of an exalted reflex action, are controlled by sedatives which diminish the excitability of the nervous centers,

and render them less capable of propagating morbid impressions originating in local disturbance of either function or structure. Hence the necessity of resorting to this class of remedies in order to check the diffusion of excited or perverted action, arising from any cause whatsoever. Hence the necessity of exhibiting opium in ileus occurring in any form, "with or without mechanical obstruction," when once the effects of an over-excited or perverted innervation are strongly developed. According to this view, it is applicable to every form of the affection accompanied by urgent symptoms, whether arising from intussusception or strangulation, from peritoneal adhesions or bands of lymph, internal or external irreducible hernia, impacted feces, concretions, stricture, musculo-enteritis, and simple distension, with loss of power, &c., &c. Abercrombie states, that "a modification of the disease yields to a full opiate more readily than to any other mode of treatment." Mackintosh observes, that in ileus "opium seems to increase rather than diminish the laxative effects of medicines." The profound sagacity of Sydenham enabled him to discover a mode of treatment scarcely improved on by subsequent practice. For the treatment of the iliac passion, he directs that "blood be taken from the arm; after a few hours, twelve grains of scammony and a scruple of calomel are to be given; if rejected by vomiting, twenty-five drops of the liquid laudanum are given in half an ounce of strong cinnamon water. Should the vomiting cease, the purge is to be repeated; but should it return along with pain, and the cathartic be retained, the same dose of laudanum is to be again given, and to be repeated every fourth or sixth hour until the motion of the intestine be perfectly quieted; after the action of the purge, the opiate is to be repeated twice, thrice, or oftener, during the day, until the pain and vomiting have wholly ceased." Had Sydenham been aware of the remarkable effects of mercury pushed to salivation when it failed to purge in such cases, there would have been nothing to add to this method of treatment.

Amongst the many remedies employed in this affection, tobacco and belladonna enemata have been occasionally successful; the former is a remedy of much power, and its depressing effects are well known; the latter has been extolled by some modern practitioners, but is confessedly a dangerous remedy. Hanius, Wagner, and some others, have seen it followed by good results; the former has given the infusion of the root of belladonna, made by infusing a dram in as much boiling water as would yield one ounce on straining; this, with an equal quantity of infusion of chamomile, is administered as an enema. (Canstat, *Med. Clin.*) Messrs. Trosseau and Reveil direct dried belladonna leaves (gr. xv, to ʒss.) to be infused in eight ounces of boiling water for one hour, and strained; this quantity is to be administered as a glyster in cases of ileus from strangulated hernia. Amongst other remedies occasionally successful, are enemata of large quantities of warm or cold water, thrown up by O'Beirne's tube. Dr. R. H. Townsend, of Philadelphia, succeeded in overcoming an obstruction which threatened speedy death, by first throwing a quart of ice-cold water into the rectum, then suspending the patient by his feet to the ceiling of the chamber, and kneading the abdomen with considerable force; the signs of obstruction immediately ceased, and in fifteen minutes the injected water with feculent matter were evacuated. (*Wood's Practice of Physic.*)

I have succeeded in freeing the bowels in a case which had resisted powerful purgatives for a week by pumping up large quantities of strong soap-suds.

by means of the tube and syringe. Rokitsansky thinks that injections of air are applicable to cases of invagination. To be of any use, however, they must be employed before adhesions take place between the opposed surfaces of the invaginated bowel.

A host of remedies, familiar to most persons, have been from time to time resorted to for the relief of ileus; some of them are undoubtedly productive of dangerous effects. I have known shot to be swallowed, and strange to say the relief was immediate; but such remedies are rarely successful. In every case, it is indispensable that a minute examination of the abdomen should precede the use of remedies. There are instances recorded of death from strangulated hernia unnoticed during life; stricture of the rectum, or obstruction occurring in this bowel, should be sought for before resorting to active remedies. I have known the sudden occurrence of retroversio-uteri give rise to very severe symptoms of obstruction; in another case, attended with much pain, whenever the patient endeavored to force a motion, a hair-pin was found to be firmly fixed by one of its arms, which had penetrated the recto-vaginal septum, whilst the other, remaining free, was pressed upon by the solid contents of the bowel, and inflicted so much pain, as to cause the patient to refrain altogether from straining at stool; the consequence of which was a large accumulation of hardened feces, which were dislodged by enemata after the foreign body had been removed by the aid of forceps.—*Dublin Medical Press.*

On Bright's Disease, and its connection with Puerperal Convulsions.
By Dr. LITZMANN, (Deutsche Klinik.)

Professor Litzmann, of Kiel, has published an elaborate memoir on renal disorder in pregnant and puerperal females. He commences by describing twelve cases in which albuminuria and cedema were present; some of the women had convulsions. Three of the patients were pregnant with twins; and nine (including the twin cases) were primiparæ.

In several of the cases, especially where convulsions were present, or were threatened, or where there were symptoms of toxæmia, as amaurosis, etc., carbonate of ammonia was discovered in the blood and in the air expired from the lungs, and in the blood of the children who were born dead. This was in accordance with the view of Dr. Frerichs, that urea, when accumulated in the blood, becomes converted into carbonate of ammonia, and then produces toxæmic symptoms.

Including the twelve cases described, Dr. Litzmann has examined the urine of one hundred and thirty-one persons: seventy-nine during pregnancy, eighty during labor, and eighty after parturition. In these he has found albumen present in thirty-seven, and absent in ninety-five. The examinations began, with few exceptions, in the last three months of pregnancy; in almost all cases they were repeated several times, and, in several, daily; yet it is possible that small quantities of albumen may have escaped notice. In order to avoid the admixture of foreign matters from the vagina, the catheter was used, always with parturient women and those who had been delivered, and frequently in pregnant females. In the latter, as well as in those who

had been confined, the urine passed in the morning was employed. Heat, and acetic, or nitric acid, were the tests used.

Of the ninety-five females, whose urine contained no albumen, fifty-three were primiparæ and forty-three multiparæ. Of the thirty-seven who had albuminuria, twenty-six were primiparæ and eleven multiparæ; among the latter was one who had albuminuria in her first pregnancy, and two who were pregnant with twins.

Among the thirty-seven females, the urine of sixteen was discovered to be albuminous during pregnancy. In ten of these the albuminuria continued during labor, and for some days afterward; in four, where it was less intense, it disappeared before confinement; and in two cases, circumstances prevented the examination from being continued.

In four women who had been confined, and whose urine contained albumen, no examination had been made during pregnancy. There can be no doubt, however, from the quantity of albumen found, and the other symptoms that it had been present.

In four females, in whose urine no albumen had been found during pregnancy, albuminuria appeared during labor — in two, evidently for the first time.

In ten persons, in whose urine no albumen had been present during pregnancy, there was considerable albuminuria after delivery. In eight of these, the urine, examined during labor, was found to contain no albumen.

In three females who had been confined, and in whose urine albumen was found, no previous examination had been made.

There is a form of albuminuria unconnected with renal disease, but arising from catarrhal irritation or blennorrhœa of the bladder. Dr. Litzmann has observed it twice during pregnancy, several times during labor, but most frequently after delivery. It generally appears on the second or third day, and goes on increasing till the sixth or seventh. The quantity of albumen is not great, and is connected with the presence of purulent mucus. At first, the urine after delivery is high colored, of acid reaction, and contains a large quantity of urates and uric acid, with epithelium and a number of pus or mucous globules. It afterward becomes lighter and yellow in color, slightly turbid, contains less urates, and is less acid; on long standing, it deposits a sediment of pus corpuscles with more or less epithelium. Fibrinous casts of the uriniferous tubes are never present. In only two cases was the quantity of pus in the urine sufficient to be detected by the naked eye: these were more chronic than most of the others, recovery not being effected till the eighth week.

In both these cases, which Dr. Litzmann relates, there had been, during labor, long continued pressure on the urethra and neck of the bladder; and this may have acted as the proximate cause of the disease. Yet cases very often occur, in which the same causes do not produce such effects. In most of the women affected with vesical catarrh after delivery, the disease could not be attributed to the above cause, the labors having been regular, and even easy. Dr. Litzmann is inclined to believe that slight vesical catarrh, which may not cause albumen to be present in the urine, but which may furnish a sufficient quantity of pus or mucous corpuscles to be detected with the microscope, is of not unfrequent occurrence after delivery. During and before labor, the disorder is much more rare. When it occurred during labor,

this had always been tedious, without, however, in general, producing retention of urine. One of the pregnant females who were affected had rigors, and was at the same time seized with rheumatic swelling of the hands. In another, the vesical catarrh was brought on in the fourth month of pregnancy, by retroversion of the uterus. The local symptoms were generally very slight; on being questioned, the patients would acknowledge a little uneasiness or sense of heat in the bladder; but the state of the urine was generally the only diagnostic symptom. Of the thirty-seven cases of albuminuria which Dr. Litzmann observed, he believed that half were referrible to catarrh of the bladder; ~~this~~ he ascertained, in nine of the cases, by microscopical examination.

Simple albuminuria, and Bright's disease, (albuminuria accompanied by fibrinous exudation into the uriniferous tubes,) pass gradually into each other in pregnant females. In most of the cases in which albuminuria had reached a high degree, fibrinous casts were found toward the end of pregnancy, or during labor and the early part of the subsequent period. Of the thirty-seven cases of albuminuria, Dr. Litzmann found thirteen to be connected with Bright's disease; in seven, fibrinous casts were found; in one, the urine could only be examined once, and, though not then found, they were probably present; and in five, there could be little doubt from the quantity of albumen and the concomitant symptoms—eclampsia in three cases—that they were present, although the urine was not examined microscopically.

There can be no doubt that, as has already been pointed out by Rayer, the albuminuria of pregnant females arises from a mechanical obstruction of the renal circulation. From the experiments of Frerichs, it appears that the retardation of the venous circulation on the kidneys easily produces exudation of albumen and fibrin, and finally, even of blood. On the other hand, increased arterial impulse, as from ligature of the aorta below the renal arteries, only rarely gives rise to slight albuminuria; and it is when one kidney is extirpated at the same time with the ligature of the aorta, that any notable quantity of albumen appears in the urine. In favor of the mechanical view of the explanation of albuminuria in pregnant women, may be adduced its predominance in primiparæ—a fact recognized by all observers. The tight and unyielding abdominal wall must naturally cause the uterus to press more strongly on the organs lying behind and above it. It is moreover probable, that when albuminuria has occurred in multiparæ, it has been but a repetition of the same affection which they had as primiparæ.

In many of the cases, there were also other causes which tended to increase the pressure. Five of Dr. Litzmann's patients who had albuminuria, were pregnant with twins; in others, there was a large quantity of liquor amnii, or a large child, or both; in one case, there were periodical contractions of the muscles, especially of the recti, pressing the uterus against the spinal column; in four cases, the pelvis was narrow. Three of the patients had chronic pulmonary catarrh. It cannot, however, be denied, that cases are often met with; in which all these circumstances are present without producing albuminuria; and, on the other hand, that albuminuria sometimes occurs, even in a high degree, without more than ordinary pressure being apparently exerted. An additional ground for assuming the dependence of albuminuria in pregnant women on mechanical impediment to the circulation, is found in the rapidity with which it disappears as soon as, by emptying the uterus, the free circulation of the blood has been re-established.

Besides the mechanical cause we must take into account the state of the blood in pregnant women (increase of water and fibrin, decrease of albumen, diminution of the red, and increase of the colorless particles.) Most of the pregnant women with albuminuria, observed by Dr. Litzmann, had a more or less chlorotic appearance; while others appeared fresh and healthy. The state of the blood did not appear sufficient to cause albuminuria, without an impediment to the circulation through the kidneys. In common with Lever, Devilliers and Regnault, etc., Dr. Litzmann has not been able to recognize cold or the abuse of spirituous liquors, etc., as the cause of albuminuria in pregnant females.—*London Med. Jour.*

Dr. Wood's Treatment of Scarlet Fever. By Prof. J. H. BENNETT,
Edinburgh.

The most recent system of treatment which has been brought forward is that recommended by Dr. Andrew Wood; and I notice it in deference to the great experience that gentleman has acquired from his position as physician to Heriot's Hospital and other educational establishments in the city, which have been attacked by numerous epidemics of the disease. He considers that the most efficient and safe method of treatment consists in acting powerfully on the skin, with a view of thereby assisting nature to eliminate the scarlatinal poison from the system. As ordinary diaphoretics frequently fail, he has recourse to the following method: Several common beer bottles, containing very hot water, are placed in long worsted stockings, or long narrow flannel bags, wrung out of water as hot as it can be borne. These are to be laid alongside the patient, but not in contact with the skin. One on each side, and one between the legs, will generally be sufficient; but more may be used if deemed necessary. The patient is to lie between the blankets (the head of course being outside) during the application of the bottles, and for several hours afterward. In the course of from ten minutes to a half an hour, the patient is thrown into a most profuse perspiration, when the stockings may be removed. In mild cases, the effect is easily kept up by means of draughts of cold water, and, if necessary, by the use of two dram doses of *sp. mindereri*, every two hours. In severe cases, where the pulse is very rapid—the beats running into each other—where the eruption is either absent or only partial, or of a dusky purplish hue—where the surface is cold—where there is sickness or tendency to diarrhoea—where the throat is aphthous or ulcerated, and the cervical glands swollen, then he follows up the use of the vapor-bath by four or five grain doses of carbonate of ammonia, repeated every three or four hours. Should this be vomited, then brandy may be given in doses proportioned to the age of the patients. Carbonate of ammonia he considers to act beneficially: 1st, by supporting the powers of life; 2d, by assisting the development of the eruption; and, 3d, by acting on the skin and kidneys. Where the vapor-bath was used early in the disease, and its use continued daily, or twice or thrice a day, according to circumstances, he has found that the chance of severe sore throat was greatly obviated. In regard to supervening dropsy, he considers that, by the use of the vapor-bath, with the other necessary precautions as to exposure, diet, etc.,

its recurrence is rendered much more rare. In the treatment of the dropsical cases, it was also very useful, and even might be trusted to entirely in some cases. Dr. Wood also condemns all depleting treatment, and even purgatives during the first ten days, as not only not required, but positively dangerous, as tending to interfere with the development of the eruption. In the latter stages, as well as in the dropsy, however, he thinks purgatives are often beneficial.

The general plan of this treatment appears to be so far rational that its object is to hurry forward the disease by applying damp heat to the skin, and by thus assisting nature to make her operations more perfect than they might otherwise be. In other words, by rendering the febrile eruption more complete, diminish the risk of its leaving behind it a tendency to subsequent disease. Whether this plan as a whole will, in practice, prove more extensively beneficial than any other, can only be determined by an extensive trial and careful comparison of the results.—*Monthly Journal of Med. Science.*

Prospectus of Massie's Eclectic Southern Practice.—We are gratified to perceive, from the "Prospectus" before us, that we are about to be favored with a work on the Diseases of the South, by a gentleman every way competent for the undertaking. Dr. Massie, of Texas, has been solicited by a large number of his brother practitioners, who are well acquainted with his high qualifications, to write a work on the Theory and Practice of Medicine—a work "presenting the various modifications which diseases assume in Texas" (and the South generally.—*Ed.*) The material for such a work is varied, abundant, and needs but some skillful hand to gather the rich harvest which is now ripe, to digest and arrange it for our common profession. As yet little has been done, save through the pages of a few medical periodicals, to advance, in the estimation of our cotemporaries, the science of Medicine in the South. For works, whether in Surgery or the Practice of Medicine, we have heretofore looked to friends of the North and of Europe; when we should begin to put forth our own efforts, and build up a system of practice adapted to our climate, the peculiar nature of our diseases, and the wants of the profession. Facts in abundance are at hand, as rich as they are diversified—diseases unknown to other latitudes and other climes daily fall under our observation, and demand our care, yet we have but few records, and no standard work, to which we can turn for instruction and advice, treating of our peculiar diseases, in times of doubt and difficulty. To possess a work embodying the history, the peculiar features of our epidemic and endemic diseases, and also their pathology and therapeutics, is indeed a desideratum which has long been felt by the profession throughout the South. We are rejoiced, therefore, to learn that a physician—a man of, and belonging to us—one of extensive experience, enlarged and liberal views—of much reading and reflection—a scholar, in short, is about to undertake to supply this desideratum to the profession.

The work will be brought out in the course of twelve months, at least, the price of which will be fixed at five dollars per copy.

We look forward to the appearance of Dr. Massie's new work with unusual interest, and shall hail its publication as the establishment of a new epoch in Southern Medicine.—*N. Orleans Med. Journal, (Editorial.)*

On the Use of Purgatives in the Treatment of Bilious Fevers, and other Bilious Affections of the South and West. By SAM'L G. ARMOR, M. D.

In withholding active purgation in the treatment of a class of diseases which prevails during the hot summer and the fall months, especially in the Southern and Western States, I am aware that I come in conflict with high authority; and I would not presume to question such authority, but for the conviction, strongly impressed upon my mind, that as a class of remedies, they are dangerous in the treatment of what are commonly called the Bilious, or Bilious Remittent Fevers of the South and West. It appears to be a common impression with many, that purgatives are the *only* remedies necessary in the treatment of this class of fevers.

It is not my purpose, at present, to inquire into the pathological relations of morbid hepatic secretions, further than as connected with diseased action of the gastro-intestinal mucous membrane. The general principles of pathology and practice, however, apply to all derangements of the hepatic functions.

Although lesions of secretions are generally classified by writers on pathology as *primary* elements of disease, yet a close examination of the subject must satisfy every reflecting mind that they are mere *symptoms*, or sustain secondary relations in the order of pathological manifestations. Before the lesion of secretion takes place, must there not be either a lesion of the blood, of the circulation, of structure or of innervation? A clear conception of this fact would, I think, throw light on a class of diseases associated with derangement of the hepatic function, and banish from our Nosology those numerous primary and idiopathic affections which are attributed to the liver.

It must be acknowledged, however, that as an excretory and depurating organ, the liver performs an important function in the animal economy; and the rationale of its increased action, and consequently increased stimulation, during the hot summer and fall months, must be apparent to every one who is familiar with the relation it sustains to the respiratory function. And the very importance of its office is a sufficient reason to induce us to investigate more closely its varied pathological conditions, that we may strike out, if possible, the first link in the chain of morbid action, and thus annul a train of secondary affections resulting from the forward action of a morbid secretion.

The peculiar tendency of Duodenitis to produce functional derangement of the liver has long been recognized by observers. The distinguished physiologist, Broussais, was the first, I believe, to call attention to this subject, and although he carried his views to great extremes, yet everlasting honor is due his memory for the clearness of his expositions of diseases of the gastro-intestinal mucous membrane. It is true, that so far as relates to the duodenal mucous membrane, different explanations have been given of the jaundice that so frequently follows. It has been supposed that a swollen condition of mucous membrane extending into the ductus communis choledochus, gives rise to mechanical obstruction to the flow of bile from the gall duct; and although in many instances this explanation may be the true one, yet the fact that we may have jaundice without closure of the common duct, is adverse to the universality of this explanation. We are led to infer, therefore, that the elements of the coloring matter of bile exist in the blood in health, and that other causes may impair or entirely suspend the secretory function of the liver; thus permitting the coloring matter to accumulate in the blood.

In cases of this kind, with the usual manifestations of an icterode appearance of the eyes and skin, and white or clay colored fecal evacuations, we do not often have very marked tenderness over the region of the duodenum.

The question may arise, then—what is the morbid agency which gives rise to increased, suspended, or perverted action of the liver? The answer to this would show that the causes are various, although all agreeing, perhaps, in many essential particulars.

First, congestion from intropulsion of blood, whether from the cold stage of an intermittent fever, or from protracted cold to the surface, will give rise to it. The result of the congestion from any cause, whether active or passive, is the lowering of the vital properties of the gland, and a consequent suspension or perversion of secretion.

Again, perverted secretion may result from a primary diseased condition of the blood itself.

Or lastly, we may adopt the explanation of Bichat, "that between a secreting organ and the surface upon which its excreting duct opens, there is a sympathy by which a stimulus applied to the latter is communicated to the former." As applied to the liver, I should have enumerated this as first in the order of causes, because most important. The illustration of this law is very manifest. We have a familiar one in the effects of food, tobacco, or other stimulating substances taken into the mouth. A copious secretion from the salivary glands is the result. We have no explanation of this but that based on the influence of the sympathetic system of nerves over organic functions, and as applied to secretory organs, we have abundant evidence of this influence. Mental emotions also, such as anger, anxiety, fear and terror, very sensibly affect the secretion of glands. And so great is this perverted nervous influence, that it frequently affects, in a very marked degree, the quality as well as quantity of the secretion. Instances are on record, apparently well authenticated, of the secretions of the liver being rendered so acrid by violent emotions of anger, that at the moment of ejection it irritated the mouth, fauces and orifice of the anus. And the instance related in Carpenter's Physiology, of the violent combat between the soldier and the carpenter, whose wife was nursing a young infant, very forcibly illustrates the effect of passion in changing the secretion of the mammary gland. In our pathological reasonings we do not, perhaps, duly appreciate the influence which the great sympathetic system of nerves exercises over secreting structures.

These remarks are introduced for the purpose of showing that disordered hepatic secretion is a secondary condition, to be removed only by removing the cause. Hence, in a great practical point of view, the very important inquiry as to the nature of the cause.

But if from any cause, general or local, inflammatory or irritative, there is an interruption of the accustomed actions of a secreting organ, congestion of its vessels follows. As applied to the liver, diminished secretion of the bile gives rise to a congestive state of the vena portarum and its branches, and in some cases, to a similar state in those organs whose venous system is associated with that of the liver.

This organ, like all others, may be the seat of congestion, of inflammation, or of both. In speaking of congestion of the liver, I allude to a condition essentially different from inflammation of that organ. In acute inflammation, it is mainly the arterial action of that organ that is excited, and the congestion

is arterial; whilst in venous congestion of the liver, consequent upon an interruption in its secretory action, the arterial system of the liver is necessarily but little, if at all affected; the congestive state of that organ being, in all probability, limited to the vena portarum and its branches. But from the peculiar vascular structure of the liver, while arterial determination must necessarily be followed by venous congestion, it can in no instance, as in other structures, contribute toward the relief of that condition.

If my premises then be correct, why do we administer cathartics for the relief of biliary derangement? I am aware that it is argued, theoretically, that the serous exhalation from the intestinal canal, caused by the action of a cathartic, unloads the vessels of the liver, and thereby restores its healthy circulation; and this argument might have weight, were it not for the counter-acting influence of irritation, caused by the operation of the remedy; but this element of evil, I doubt not, more than overbalances all the benefit derived from the depletion. In many instances, the manifestations of biliary derangement are produced by irritation and phlegmasia of the mucous membrane; and it is very evident that this condition would be only exasperated by purgatives. An increased irritation is communicated to the parenchyma of the liver, and whatever be the intensity of the phenomena attributed to the bile, calmness, is generally re-established as soon as there is a cessation of the local phlegmasia. I regard this as an established fact in pathology of the highest importance.

In our ordinary bilious fevers, therefore, accompanied as they generally are with irritation of the stomach and bowels, I would abstain from the use of cathartics as calculated to aggravate the symptoms of biliary derangement and increase all the phenomena of the disease. I would not be understood, however, as entirely excluding alvine evacuants in the treatment of these fevers. Their operation is sometimes, doubtless, attended with benefit. The acrid secretion may be a greater source of irritation, forward upon the mucous membrane, and backward upon the gland secreting it, than would be the effect of a laxative to remove it; but it would be with this view, mainly, that I would administer them. The other fact, namely, that the tendency of cathartics is to increase the phlogosis of the mucous membrane, and that this condition is, through sympathy and direct continuity of structure, communicated to the liver, should ever be borne in mind.

If their effect be to indirectly at least stimulate the liver, our deduction may be regarded as illogical. The question may be asked, is not this the great object to be effected? Grant that it is, to say the least of it, a desirable object, and still it by no means follows that the enteric and hepatic excitement will be promotive of biliary secretion. Is not indeed the converse of it true? Yet there may be a possibility that the secretory action of the liver is suspended from the want of the normal sensibility of the duodenal mucous surface.

The chyle, which is the natural stimulus of this surface, may, as a consequence, fail to communicate its stimulatory impression to the liver, and a sort of torpor or paralysis may be the result. This condition is generally manifested by the clay-colored or white discharges from the bowels, unaccompanied by hyperæmia and tenderness. If we are able to diagnose this condition, then purgatives, especially the mercurial ones, may be admissible; although even in this case, broken doses of calomel, short of purgation, would be better practice.

Is it true, that in the class of cases under consideration these are the manifestations? Is not indeed the very opposite condition generally present, such as local tenderness, irritability of the stomach, and dark discharges, indicating morbid sensibility and hyperæmia of the mucous membrane to the point of effusion of the morbid and fluid elements of the blood?

Shall we, then, in this condition, administer cathartics? Many reasons forbid; I will be content with enumerating a few:

1st. As a general and valuable therapeutical principle we should never resort to medicinal agents when nature is doing her proper work.

2d. Cathartics will, in all probability, increase the very difficulty which nature is endeavoring to overcome, by adding irritation and determination to the congestion already existing.

3d. Protracted congestion of the liver, by damming back the venous circulation of the abdomen, may give rise to formidable disease of the intestinal mucous membrane.

And lastly, there is no indication, as a general proposition, for their use, as evinced by the color and character of the discharges characterizing hyperæmia and effusion, positively contra-indicating their use.

I might add, that experience abundantly demonstrates not only the inutility, but the positive injury following the use of active and repeated purgation in the treatment of the miasmatic fevers of the Mississippi Valley. I doubt not but that hundreds have fallen victims to erroneous views on this subject, propagated by Hamilton in his work on Purgatives.

I have alluded more especially to the use of cathartics in the treatment of ordinary bilious fevers, as they are generally termed, and have called attention to but one pathological feature of the disease. In so doing I would not be understood as referring all the phenomena of bilious fever to derangement of the biliary organs; nor to enteritis or gastro-enteritis as the cause; notwithstanding this is undoubtedly a frequent and formidable super-addition to the general fever.

The effect of cathartics is also bad on the gastric mucous membrane, and consequently on the functions of the stomach; and it is only necessary to reflect on the importance of the perfect action of the digestive apparatus to a maintenance of a healthy condition of the entire system, to be convinced of the multiplied variety of secondary disturbances which may result from derangement of the primary action of the series of animal functions. It is indeed the "golden bowl at the fountain," the "wheel at the cistern," and if its functions be perverted, disturbance is, of a physical necessity, propagated remotely through the system. Strike upon the first link of the chain of sympathies, and vibration runs through its whole extension. Hence the varied course which the derangement of function may pursue, and hence the difference of character which disease may ultimately assume. If this thought were more rigorously pursued in all investigations at the bedside, the result would doubtless be a more rational and simple practice.

Medicine has too often and long been engaged, and too often worsted, in the contest with affections of an idiopathic and independent character, which werethe secondary, or perhaps more remote result of pathological derangement; and in no instance, perhaps, have we a more striking illustration of this than in diseases of the gastro-intestinal mucous membrane.—*Ohio Med. and Surg. Journal, from Western Med. Chir. Journal.*

Wry neck cured without cutting. By GURDON BUCK, M. D., Surgeon to New York Hospital.

The success obtained in the following cases of Distortion of the Head, commonly known as Wry Neck, induces the undersigned to make them known to the profession, in order that the treatment employed, which it is believed has not hitherto been applied in such cases, may be fully subjected to the test of experience.

Case First. Hester Higgins, a native of Ireland, aged 25 years, unmarried, was admitted to the New York Hospital on the 6th of Nov. 1848, at which time she had suffered from rheumatism already about seven months; all the larger joints of the body having been successively affected. About four months prior to her admission, she suffered a relapse, after having nearly recovered, and since then she has experienced but little alleviation of her ailments. Her neck, as well as most of her larger joints are painful, though not much swollen. Her tongue is slightly furred, her pulse is 85, and soft; her skin moist, and bowels regular.

On the 19th of January following, she had nearly recovered from her rheumatism, under treatment, except rigidity and contraction of the muscles of the right side of the neck, by which the head was drawn downward, and toward the right shoulder. To relieve this distortion, frictions, with stimulating and oleaginous liniments were diligently employed, and subsequently, sulphuric ether was applied to the neck. Some slight improvement resulted from the use of these means. On the 18th of April, however, the condition of the neck had for some time been stationary, and all hope of further benefit was abandoned. The motions of the head were very much restricted, and any attempt to overcome the resistance of the rigid muscles by stretching them, occasioned severe pain. The rigidity did not appear to reside in the sterno-mastoid muscle, inasmuch as this muscle did not grow hard and stiff when efforts were made to elevate the head; the resistance was evidently seated in the deeper muscular and tendinous parts.

At the request of my colleague, Dr. Swett, of the Medical Division, I saw this patient, and proposed to make cautious attempts to overcome the resistance by force, the patient being first subjected to the influence of sulphuric ether. Considering the resistance to depend on contracted muscular and tendinous fibres, my object was either to stretch or rupture them; and in doing this no danger was apprehended to the important nerves and blood-vessels of the neck, since the forced movement necessary to accomplish this object would fall far short of the extensive motions in every direction to which these parts are accustomed naturally to accommodate themselves.

Dr. Swett assenting to my proposal, the patient was laid upon her back in bed, with her head resting high up on a pillow, so as to be easily got at from the head of the bed. Taking the head between my hands, placed one on either side, I cautiously stretched it with a very moderate degree of force, in the direction opposite to that in which it was distorted, that is, upward and to the left side. Almost immediately, every one standing round the bed (of whom there were at least eight or ten pupils and medical men,) was startled by a

loud snapping sound of something rupturing, and at the same time I perceived that the head yielded, and could be brought almost to its natural position. It was thought prudent to proceed no further at this operation. The patient on recovering her consciousness was not sensible of any new soreness in the parts, and could bear the head to be moved much easier than before the operation. She was directed to lie as much as possible on her left side. On the following day, there was considerable soreness on the right side of the neck. On the 25th of April, one week after the first operation, the soreness of the neck having very much diminished, the operation was repeated a second time.

The proceedings were the same as in the first operation, only the stretching was carried to a much greater extent, and with a much less timid hand. Several times resisting fibres were felt to yield with a rupturing sensation, till, at length, no further resistance was encountered, and the head could be carried to the full extent in every direction. After the effects of the ether had passed off, the head was bandaged down toward the left shoulder. On the 1st of May, the bandage being dispensed with, the head showed no disposition to resume its distorted attitude. On the tenth of May, (1849,) the head could maintain, unaided, its erect natural position, though rotation and flexion were still limited in extent, and performed awkwardly; the patient, however, was sensible of progressive improvement in these respects. She took her discharge from the Hospital for the purpose of returning to her friends in Ireland. About one year afterward, she was heard from as continuing well, and free from any distortion or rigidity of the neck.

Case Second. In January, 1852, Maria P——, of Guilford, Connecticut, aged 12 years, and of a healthy constitution, came under my care, with the head very much distorted from being drawn down toward the chest, with the face turned to the left side. The motions of the head were also very much restricted. In the month of July preceding, she had been attacked with sore throat and stiff neck, that left her ever since in the condition just described. She had never suffered from rheumatism in any other part of her body, and had generally enjoyed good health. I at once decided to employ the treatment which had been so successful in the preceding case; and on the 15th of January, having first etherized my patient, I performed the first operation. In order to carry the extension of the head to the requisite degree, it became necessary to have her supported in the sitting posture in a chair, and to place myself in front of her. Grasping the head between my hands, I acted on it in the various directions in which resistance was encountered, but felt no sensation of rupturing fibres, in this or in any of the subsequent operations. The resisting parts, however, yielded in some measure, and allowed the head to be brought more nearly into its natural position. No pain was experienced from the operation on recovering her consciousness.

On the 19th, no effect was observable from the first operation; it was therefore repeated a second time, with the aid of ether. On the 24th, 26th, and 30th of January, and on the 4th and 7th February, it was also repeated, each time with the aid of ether. Though a gradual improvement was perceptible from these repeated operations, it became evident that a complete cure could only be achieved by a patient and persevering repetition of them for a long time; it was therefore judged most prudent to continue the operations

without the aid of ether. The patient's courage and endurance, though put to a very severe test, proved adequate to the trial. Once every day she submitted to the stretching process, for about ten minutes each time. This was continued up to the 1st of March, after which it was repeated twice every day. The manner of manipulating was as follows: The patient was seated in a chair, and her body steadied by an assistant standing behind, and holding her shoulders firmly with both hands. Placing myself in front of her, I grasped her head with my hands, in such a way as to perform most efficiently the different movements I wished to execute. These movements were varied in every direction in which resistance was encountered, my object being to stretch to the utmost the contracted muscles, and to maintain them on a stretch for a certain length of time. The process was painful only during its actual performance, and ceased to be so the moment it was discontinued. On the 24th of March, the operations were suspended, while the patient made a visit to her family, and were resumed again on the 8th of April. During this interval no relapse took place. The same course of treatment was continued till the 10th of May, when she returned home, highly gratified at being able to maintain her head by her own efforts, in its natural erect position, and to turn it in different directions almost as well as ever she could. She was advised to continue for a long time the daily practice of performing the various motions of the head as extensively as possible. On the 13th of January, 1853, I conversed with an aunt of my patient, who had recently visited her, and who reports that she holds her head in a very natural manner, and can move it at pleasure, freely in every direction. In a word, she considers herself quite well again, and without any disposition to relapse.—*N. Y. Med. Times.*

121 Tenth st. January 20, 1853.

Bite of a Copperhead—"Trigonocephalus Contortix"—treated with Whisky. By N. HARRIS MORANGE, M.D., of Abbeville, S. C.

On the 21st of June last, I was called to see a negro man belonging to Capt. P—, of Abbeville district. Found him partially delirious; skin hot and dry; pulse very much excited, ranging from 100 to 120; left leg and ankle swollen to a great degree. Upon making inquiry into the history of this case, I learned that the patient had been bitten about 12 hours previously by a "trigonocephalus," or, as it is frequently styled in this part of the country, copperhead or highland mockeson. This very poisonous reptile was concealed beneath the step of a *meat-house*, and inflicted a wound upon the inside of the foot, near the ankle joint. I immediately applied a ligature above the seat of affection—prescribed poultices over the wound; and olive oil, ammonia, &c., internally.

22d. The patient is *in statu quo*—no abatement of the swelling, delirious: ordered whisky, *ad libitum*.

23d. No decided improvement—still anxious, restless, and uneasy; skin hot and dry. Continued the whisky, combined with capsicum: it was administered, until the patient was fully under its influence, without regard to quantity. Left opium to be given if necessary.

24th. Had passed the "crisis." A profuse perspiration was over his entire system; the tumefaction was subsiding; the delirium had ceased; he spoke rationally, and speedily convalesced.

Gibson says—"Of the numerous American serpents, two species only are known to be poisonous—the crotalus or rattlesnake and copperhead." If he includes under the common name of copperhead, both the highland and the water-mockeson, then we concur with him in the assertion. The two latter are of the same family, but not of the same species, which is abundantly manifest by their mode of living.

The same writer says: "These reptiles are more lively, and their venom more active during the very warm weather. Upon the approach of the cold season, they become languid, and then strike reluctantly, and frequently without any ill consequence."

The interesting case which I witnessed whilst a student in the University of New York, furnishes a striking proof of the speedy operation of the poison even in the dead of winter.

Dr. W., of that city, was bitten on the hand, by a rattlesnake, sent to him by a friend from the State of Alabama. The hand soon began to swell, and in a few hours the whole arm was very much tumified, presenting a mottled appearance, even to the shoulder and axilla. He had the best medical advice the city afforded; yet, after intense suffering for two or three days, he died.

I have no doubt, that if the *alcoholic* treatment had been instituted in this case, with the application of the ligature above the seat of affection, (which *last* was timely suggested by some of the Southern students present,) Dr. W. would have recovered.—*Southern Med. and Surg. Journal.*

The Medical profession and American Slavery.—A certain BENJAMIN W. RICHARDSON, in a letter to the editor of the *Lancet*, (Dec. 4, 1852,) writes as follows:

"I see in the *Times* of to-day, that a meeting of ladies has lately been held at Strafford House, for the purpose of addressing a memorial from the women of England to the women of the United States, in favor of the abolition of slavery.

"While I earnestly admire the philanthropic motives which called these ladies together, and anticipate great good from their labors, I cannot but think that they have set before the English members of the profession of medicine, an example well worthy of being followed."

He then inquires: "Now might not we, the medical men of this country, do much good by transmitting a fitting memorial to our Transatlantic brethren, entreating them to lend their aid to every endeavor that shall be made to abolish slavery?" and he makes a long appeal, characterized by the usual cant and twaddle, that they should do so, very thriftily observing "that the memorial might be prepared at the merest outlay of labor and at little pecuniary cost."

We might suppose, from the grievous complaints continually made in the British Journals, of the evils which afflict the profession in that country, and which have driven, we are told, some members of it to enroll themselves as policemen, and others to advertise for menial situations, even that of gentleman's valet, that the profession had enough to gratify their philanthropy in

effecting reforms at home without adding to their labors by intermeddling with the concerns of those at a distance. When they shall have succeeded in accomplishing a reform in their own condition, in ameliorating that of their laboring population, and in liberating their vast East Indian population from their worse than negro bondage, their example will effect more in stimulating the profession in this country to engage in similar philanthropic enterprises than any memorial they can now send us.—*Med. News and Lib.*

EDITORIAL DEPARTMENT.

New York State Medical Society.—The New York Medical Times, No. for March, 1853, contains a full account of the proceedings of the New York State Medical Society at the annual session in February last. The society remained in session for three days, and, in addition to topics of importance pertaining to the future usefulness of the organization, several subjects of scientific interest were introduced and discussed by the members present. We propose to notice the latter first, embracing the occasion to offer some remarks of our own thereupon. Afterward we will chronicle such other matters entering into the proceedings as will be likely to interest those of our readers residing in this state.

Croup.

On this subject we quote as follows:

“Dr. Snyder, chairman of the committee, read a paper on Diphtheritic Croup—he gave the detail of thirty cases that had come under his observation—a disease that occurs in children between the ages of one and twelve years. It occurs more frequently in variable and damp weather, in the autumn and spring most often, never as an epidemic. This disease is not identical with the croup of adults. In the treatment of it, drastic cathartics and antimonials are usually injurious. Small and repeated doses of calomel; anodynes, to allay the irritant spasmodic action of the laryngeal muscles; the application of strong solution of nitrate of silver to the diseased part, and emetics of ipecac, are the best reliable means. In the success of tracheotomy he had no experience.

“Dr. Clark inquired the number of cases that recovered after the membrane had formed on the tonsils?

"Dr. Snyder said that in the fourteen who had died, the membrane had formed on the tonsils. None of these cases which recovered expectorated false membrane, though the other symptoms were similar to those where the membrane had formed. He thought the lymph was first exuded and formed on the palate or tonsils, and then extended downward upon the air passages.

"Dr. Clark gave an account of the microscopic structure of the exudation in cases which he had examined. The exudation is of a fibrous structure, and, he had observed, thicker at the upper part of the trachea in which it formed than at its lower part, where the formation was so thin and delicate that it could scarcely be seen by the naked eye. The examination he had made had been upon membranes expectorated, and also upon those found after death.

"Dr. Burwell thought the expectoration of the membrane in croup was exceedingly rare, and usually imperceptible.

"Dr. Clark said the membrane must be expectorated when it had formed, or the patient could not recover. This he considered as a truism.

"Dr. Shipman inquired for the experience of any person present, regarding the success of tracheotomy.

"Dr. Wood said the subject brought before the society by Dr. Snyder was one of great interest to the profession. With the profession it was looked upon as a fatal disease. Diphtheritic croup is different from spasmodic croup; and the child with the former may be playing with his fellow a few hours before, yet death in nine cases out of ten was then upon him. He had seen many cases of diphtheritic croup, but few of them recovered. He had seen none of them recover from the application of nitrate of silver. In New York, at the present time, it was treated with cinnabar fumigations, thus rapidly bringing the patient under slight mercurial influence. The great majority of operations (tracheotomy) were unsuccessful. In the majority of cases following scarlatina and erysipelas, it is cedema of the glottis which kills the patient, instead of the diphtheritic croup.

"Dr. Guiteau said he thought it useless to perform the operation of tracheotomy at a late period in the course of the disease. It was almost always unsuccessful. He had resolved that in his next well-marked case he would perform it at an early period, and give the child almost the only chance of recovery.

"Dr. Wood said that where the membrane had commenced its formation on the tonsils, it would go down on the larynx into the bronchia and into the air cells, and the patient die. With regard to the time of the operation, there appeared to be but little difference; the result was almost alike successful.

"Dr. Snyder said he thought the only hope of success was in the early diagnosis—the discovery of the membranous formation before it had entered the larynx, and then arresting its farther progress. He did not mean to be understood that in his sixteen cases of recovery, this membrane had extended into the trachea and bronchia.

"Dr. Van Buren related a case which he has at present under treatment.

"Dr. A. W. Marsh said he had operated twice for membranous croup, both of which proved fatal, and he thought that in all cases where the inflammation had extended to the trachea, death is a natural consequence. The membrane is so rapidly formed that an operation does not afford permanent relief, and though the passage be opened, it is impossible to keep it so.

‘ Dr. Barker thought the operation more successful than had been reported. He hoped the observations of physicians would be continued through the year, and considered at the next annual meeting.

“ Dr. Ayers related a case that came under his observation, in which the membrane was of an hour-glass form, and the blood-vessels visible in it.

“ Dr. Barker moved that the subject be laid on the table, to be brought up for discussion at the next annual meeting. Adopted.”

The subject of croup is one of very great practical interest and importance. Perhaps there is no subject in practical medicine concerning which loose and incorrect pathological notions are so largely in vogue. We are surprised that in the remarks offered on the subject by the members present, no reference is made to the most valuable contributions by Dr. John Ware, of Boston. These contributions appear most unaccountably to be overlooked, to a great extent, by the medical profession of this country. An explanation of this fact may in part be derived from the small efforts that have been made by Dr. Ware and his friends to give his communications general circulation. They have been, however, neglected in quarters where this excuse cannot be considered to apply. The contributions to which we refer were published, some years ago, in this Journal, copied from the Boston Medical and Surgical Journal, (See No. for September, 1850.) The first and most valuable of several successful papers appeared originally in the New England Quarterly Journal of Medicine, more than ten years ago. In that paper Dr. Ware gave the results of an analysis of 131 cases of the several affections usually included under the name of croup, the records of which he had collected during twenty-five years of private practice. The results thus developed have established several most important inductions.

They established, *firstly*, that there are several pathological conditions with which are associated the croupy cough, viz: 1. Pure spasm of the laryngeal muscles; 2. Spasm accompanied by catarrh of the laryngeal mucous membrane; 3. Spasm with simple enythematic inflammation; and 4. A fibrinous exudation in the larynx, extending to the trachea, and frequently more or less over the bronchial tree.

They established, *secondly*, that the last named pathological condition is relatively infrequent in occurrence, the vast majority of cases of so-called croup involving one or more of the three conditions first mentioned.

They established, *thirdly*, that cases of croup involving pure spasm, or with catarrh, or with simple inflammation almost invariably end in recovery, and require very little in the way of treatment, while cases involving fibrinous exudation, (diphtheritic or true croup,) generally end fatally.

They established, *fourthly*, that exudative or true croup almost invariably is attended with a fibrinous exudation in the pharynx, while this exudation, in that situation, is absent in the several varieties of false croup; and, hence, the value of the presence or absence of this exudation in the pharynx as diagnostic criteria.

They established, *fifthly*, that cases of spasmodic, catarrhal, or simple inflammatory croup are not converted into true or exudative croup; but that whenever the latter form of disease exists, its distinctive character is intrinsic. Hence the error of the idea generally entertained, that all cases of croup, unless vigorously treated, will be likely to eventuate in the only serious or fatal form.

They established, *sixthly*, that the fibrinous exudation in true croup, occurs early in the disease, and already exists in the great proportion of cases when patients first come under treatment. Hence, the idea of preventing the formation of the false membrane is gratuitous.

They established, *seventhly*, that when cases of true croup end in recovery, the process by which a favorable issue obtains, is the separation of the false membrane by a puriform fluid produced beneath it, detaching it, and allowing its removal by expectoration; and that this process requires time — several days at least. Hence the notion that true croup can exist, and the false membrane dissolve in an imperceptible manner, or undergo absorption, is absurd. A case of croup in which no patches of false membrane are ever discoverable in the matter of expectoration, is therefore not a case of true croup.

In view of the practical bearing of the foregoing conclusions, it seems to us that Dr. Ware's contributions on this subject are entitled to be ranked among the most striking and valuable contained in the history of medicine for the past quarter of a century; and although there seems to be a reluctance, as yet, to attribute to them due importance, probably, in a great measure, because the profession, in general, are not acquainted with them, it may safely be predicted that Dr. Ware's name will, in all future time, be identified with the literature of this subject. For the correctness of the digest which we have thus hastily penned, we would respectfully refer our readers to the papers contained in the No. of this Journal already referred to.

In several communications made by Dr. Ware subsequent to the first paper published in 1842, he submits a plan of management of true croup different from that still pursued by the majority of practitioners. In this point of view the inductions presented deserve the most careful consideration. It would be altogether out of place to treat of this branch of the subject in the

present connection. We have no intention of so doing, but we would commend it to the reflections of our readers; and as suggestives for thought we will briefly add a few considerations. The several varieties of croup which are devoid of any fatal tendency, there is reason to think, are frequently, if not generally treated by the vigorous employment of emetics, local and general bleeding, etc., either under the erroneous idea that they are cases of true croup, or that they will become so, unless the progress of the disease be arrested.

Dr. Ware may well ask "if this treatment does not do good, does it do no harm?" There can hardly be a doubt as to the proper answer to this question. We cannot resist the conviction that many children are thus sacrificed to the nimia diligentia. We beg practitioners who may read these remarks to ponder on this point; and in connection therewith we would take the liberty of recommending the little treatise by the late Prof. John B. Beck, entitled "Infant Therapeutics."

But suppose that we have clearly the responsibility of the management of a case of true or fibrinous croup, what are the ends to be kept in view in the treatment; in other words, what are the rational indications? Not to prevent the fibrinous exudation, for this has already taken place. Not to arrest inflammatory action, for this cannot be done here, more than in other situations. Not necessarily to abate the intensity of the inflammatory action, for it is not the intensity of the inflammation which places the patient in danger, but the product of inflammation — the fibrinous exudation. And here it may be remarked that the degree of inflammation is often, if not generally less in true croup, than in simple laryngitis. It is the *kind*, not the *degree* of inflammation which renders true croup so fatal.

The rational indications plainly relate to the prolongation of life till the separation and expectoration of the false membrane take place; to measures calculated to facilitate and hasten this process, and to the relief of urgent symptoms. Taking this view of the subject, Dr. Ware, and others have abandoned, in the treatment of croup, perturbatory and debilitating measures. These measures are as useless in *true* croup, as they are unnecessary in the several varieties of *false* croup. In both there is reason to think that they are destructive in their effects.

Rapid mercurialization, in order to secure the antiplastic effect attributed to this measure, fomentations, and the inhalation of watery vapor, are mainly relied upon, conjoined with anodynes to arrest, or hold in abeyance the spasmodic element which enters into the pathology of *true*, as of *false* croup, accounting for the paroxysmal occurrence of difficult respiration in the former,

as well as the latter, and contributing not only to the suffering, but to the danger.

By reference to the late papers communicated by Dr. Ware, this new plan of treatment, pursued in a small number of cases, has been found more successful than the mode heretofore followed.

In this connection the subjects of tracheotomy, and the topical application of the nitrate of silver as proposed and practiced by Dr. Horace Green, are important. The latter measure, from several reported cases, seems to possess more efficacy than would appear from the observations of Dr. Wood referred to in the foregoing extract from the proceedings of the New York State Society. We have ourselves witnessed the apparent efficacy of this measure in a case which seemed to be desperate, occurring in the practice of Dr. Strong, of this city, and reported by him in a former No. of this Journal.

The views presented by Dr. Snyder as respects the treatment of croup, accord with those just presented; nor does it appear that exceptions were taken thereto by any of the members present.

Dr. S. is in error in stating that croup never occurs as an epidemic. Valleix gives instances of epidemic croup. We were recently informed by Prof. B. F. Palmer, of Woodstock, Vt., that an epidemic prevailed a few years ago in a small village in his neighborhood, several cases occurring in rapid succession and proving fatal.

Puerperal Peritonitis.

We find the following relating to the above subject:

"Dr. Van Dyck called upon Dr. Clark to continue his remarks, made to the Society last year, on Puerperal Peritonitis.

"Dr. Clark said: After the meeting of the Society, during the month of February, he had four cases in private practice. With these cases he had better success. They were seen by Drs. Rockwell, Smith, Gilman and Higgins. In none of these cases did he make the diagnosis first. Two or three drachms per hour of Magendie's solution of morphia, was the amount taken by some patients. Strange to say, when he got out of the hospital, such doses were not required. In no subsequent case was more than one grain or three-fourths of a grain of morphia required to keep the patient free from pain.

"Dr. Gilman has reported three cases treated in this way: one was unsuccessful. In the successful case he did not have the full charge of the patient.

"Dr. Williams, who has charge of the Lying-in Hospital, was unsuccessful, and thought he lost more patients by it than Dr. Clark had saved. The point is—the marked diminution of the quantity of opium to keep the patient narcotized until the pain and tenderness of the bowels subside, and the

bowels are moved, and the pulse comes down from 160 to 140, or even from 180, as some of you have seen it, until any remaining symptom that would give special anxiety terminates. Cases occur in which the alarming symptoms continue ten or fifteen days, but because the narcotism was not carried to the point desired. The respiration in some cases is reduced to *seven per minute*. Unless the practice is brought to this point, it is not considered a fair trial. Whenever opium was used under my direction in the hospital, no pills that had been rolled more than six hours were administered, so that their hardness should not resist the action of the gastric juice. There is no particular choice between opium and morphia. He believed the disease did not prevail as it did last year; he had heard of none for several months. There had been a few cases in Williamsburgh. He commenced the treatment of the disease in its early stages with the doses he had named."

The method of treating peritonitis by the free exhibition of opium, first suggested, if we are correctly informed, by Dr. Armstrong, and practiced recently, with marked success, at the Bellevue Hospital, by Prof. Clark, is, in our view, one of the grand improvements in modern practical medicine. We do not express this opinion without due deliberation. We are cognizant of facts which, in our estimation, warrant such a conclusion. Observations bearing on this subject have not yet, to much extent, been submitted to the profession. We hope this will be done ere long. A full, authentic report of the cases treated by Dr. Clark is much to be desired. We hope he will soon favor the profession with such a report. We presume to remark that, after what has been said on the subject in various journals, this is due to himself, as well as to medical science.

It is a singular fact that cases in private practice did not require the large doses of opium administered to hospital patients. That all cases do not call for very large doses is shown by the case reported by the editor of this Journal in a former No.

We shall avail ourselves of another opportunity to enlarge on this subject.

Chloroform in Midwifery.

"Dr. Burwell then read a paper on the use of chloroform in obstetrical practice. He had administered it in one hundred and eighty cases—one hundred and twenty-two of which were greatly relieved, fifty-five partially, and three got no relief from its use. In seventeen cases, labor was terminated by the use of the forceps,—in one case by craniotomy,—in one by turning. Eighty-eight of these cases were patients in labor for the first time. And in all these cases there had been not a single accident resulting from its use. He considered at length its effects upon the mental faculties and upon the muscular system—upon the pulse and upon the respiration. He then gave the general rules by which to be governed in its use, and particular directions about the manner in which it should be administered, and the quantity to be given at each dose.

"Prof. Barker confirmed the observations of Dr. Burwell in nearly every respect. He said that those patients who had full confidence in the good effect of chloroform rarely suffered any inconvenience from its use; while those who doubt its utility and are agitated before taking it, are not unfrequently affected with slight hysterical delirium, and require larger doses to bring them under its influence." * * * * *

"Dr. Spencer said he opposed the use of chloroform in obstetrical practice, more from theory than from practice; for in the latter he had had but little experience. He then stated the ground of his theory in the action of that anæsthetic, in its chemical influence upon the circulatory and respiratory system.

"Dr. Clark said the question is: Is it most serviceable for lying-in women to take chloroform or not to take it? He alluded to the success of Dr. Burwell's one hundred and eighty cases, of as many more of Dr. Barker, and of the success of other physicians. He thought where their experiences confirmed so much that is at variance with Dr. Spencer's theory, that gentleman might question the correctness of that theory. He thought the rules for its administration had been so definitely laid down by Dr. Burwell, that an intelligent man would not be likely to err.

"Dr. Shipman said he had used chloroform in a good many hundred cases for surgical operations, and without any serious result. He considered it a safe remedy. He had used it in only eight cases of midwifery. It did not, in any of these cases, prolong labor. He had administered it repeatedly to the inferior animals—birds, rats, mice, and turtles. He thought it more destructive to them than to man.

"Dr. Burwell stated that he thought the danger consisted in the manner in which it was administered. If given in large quantities at first, it was dangerous. It should be inhaled in small quantities. The patient should be allowed to inhale the air with it.

"Dr. Cock said it was quite too late to question its utility; and related some of the cases which had come under his observation, where most decided benefit attended its use."

We have had some experience with chloroform in midwifery. Given with proper care we conceive it is unattended by much, if any risk of accident. We have generally, however, preferred that the responsibility of its use should rest with the patient, or friends. We are satisfied that it sometimes retards labor, but this objection is more than compensated for by the comfort it affords, and the refreshing influence derived from it when not carried to the point of insensibility. We do not hesitate to employ it, and are always glad when patients or friends solicit it.

Visitation of Medical Colleges.

"Dr. Rockwell made a verbal report in behalf of the censors of the Southern District. In accordance with a resolution passed at the last session, they had attended some of the examinations of the medical colleges in the city of New York. The committee found it impossible to attend all the examinations, which continue through nearly two weeks. He was pleased to say that

those students whom he had marked as unfit to receive diplomas were rejected. So far as the committee had opportunity to observe, the examinations were honorably conducted."

Increasing number of Delegates to the Society.

"Dr. Blatchford read a paper on the propriety of increasing the number of delegates to this Society, making a ratio of one to every ten from the county societies, and each delegate to hold his office for two years, and offered a resolution that a committee be appointed to confer with a committee of the Legislature for an amendment of the statute law on this subject.

"Dr. Clark said it was an important matter to alter the fundamental organization of the Society. The changes proposed would entitle New York to ninety members, which is almost equal to this Society's present members.

"Dr. Ely suggested that it would be well for each county society to be entitled to delegates corresponding to the number of members of Assembly from such county; and that the resolution should be so amended.

"Dr. Wood said, this body was intended for an executive body—to it the people and the Legislature look for a healthful medical influence; and that this Society should carefully examine the condition of subordinate societies before a larger representation is admitted from them.

"Dr. Blatchford withdrew his resolution, when the subject was referred to a committee consisting of Drs. Coventry, Rockwell, Cock, Blatchford and Thompson."

Subsequently Dr. Coventry, chairman of committee, reported as follows:

"*Resolved*, That a special committee of three members, residing in this city, be appointed to confer with the medical gentlemen of the legislature, as to the propriety and practicability of the passage at the present session of the legislature, of an act having in view the more equal representation of the medical talent of the state in this society."

Delegates to the American Medical Association.

"Leave was granted to Dr. Bradford to substitute the following preamble and resolutions for the paper offered by him yesterday:

"Whereas, it is essential to the interests of the whole medical profession of the United States, that the system of representation by delegates in the American Medical Association, should be established upon a free, equal, and liberal basis, so as to secure the attendance of a large number of respectable physicians from every state in the Union; and

"Whereas, the percentage of representation, as established by the present constitution of the association, is deemed to be liberal and just, in so far as it applies to State, County, and Voluntary Medical Societies; therefore

"*Resolved*, That the Medical Society of the State of New York deprecates, and considers unadvisable, any change in the constitution of the association, which would diminish their number, or exclude delegates from State, County, and Voluntary Medical Associations.

"*Resolved*, That the Medical Society of the State of New York approves of the system of representation to the association, embraced in the amendments proposed and adopted at the last meeting of the American Medical Association, and hopes that the said amendments may be favorably received by the next meeting, and be adopted.

"Resolved, That a certified copy of the above preamble and resolutions be transmitted by the secretary to Beverley R. Wellford, M. D., president of the American Medical Association, with the request that it will present the same to the association at its next meeting.

"The preamble and resolutions were adopted."

Petition for a Room in the Capitol.

"Dr. Van Buren offered a resolution to appoint a committee to petition the legislature for a room in the capitol for the exclusive use of the Medical Society. The resolution was adopted."

We see no reference, in the proceedings, to a semi-annual meeting of the society. This plan is, therefore, we infer, abandoned.

*Anatomical Bill.** — A bill is now before the legislature of this state, legalizing dissections, and making provisions, with proper restrictions, for supplying medical schools with anatomical material. We observe in one of the reports of the proceedings of the Assembly, that a Mr. Kennedy, when this bill came up, moved "that all doctors who die may be delivered to any other doctor applying for the same within twenty-four hours after, unless requesting burial before death." Now we have no doubt, in Mr. Kennedy's opinion, this motion was wonderfully witty. It is to be presumed he imagined the assembly, and his worthy constituents would be electrified by so admirable a jest. He is evidently a man of no little *smartness* in his own estimation; and one can fancy the self-complacency with which this funny legislator rose and threw off so brilliant a scintillation. We have no disposition to disturb his good opinion of himself as a humorist. Indeed, we are inclined to congratulate him that so nice an opportunity presented itself for the display of his facetious ability. He had an excellent chance to give the doctors a slap, and it would be altogether too much to expect him to resist the temptation. Besides, it is more than probable that in all the graver matters of legislation he is but poorly qualified to cut a conspicuous figure. It would be ungenerous, therefore, to complain of his availing himself of the only field suited to his capacity.

In perpetrating the *jeu d'esprit*, however, Mr. Kennedy gives utterance to an idea in which he is not alone. The idea involved in his elegant sally is, that legalized facilities for dissection are petitioned for by the doctors with reference to their own exclusive advantage, not for the benefit of the public.

* Since this article was written we perceive that the bill referred to has passed the Assembly, and been ordered to a third reading in the Senate. We shall endeavor to present its details in our next.

Mr. K. is one of that very intelligent and liberal-minded class who suppose that doctors dissect dead bodies for the love of the thing. They regard a medical man as a species of *ghoul*, and think that in asking the legislature to make provision for the knowledge of practical anatomy, the object is purely that they may enjoy larger opportunities for the gratification of a singular taste for an occupation which other persons view with repugnance, if not with horror. Such a sentiment, we regret to say, is not altogether confined to the facetious Mr. Kennedy.

We venture to *guess* that Mr. K. is one of those who would be sufficiently requiring of his medical attendant in the event of an accident befalling himself which called for accurate anatomical knowledge. We opine that if he got a crooked leg after a bad fracture, the unfortunate doctor employed in the case would think himself well off if he escaped only the worse by a jest or two at his expense. We doubt if Mr. K. would rank himself among the advocates of a repeal of the law rendering a surgeon liable for damages for mal-practice.

We would respectfully submit an argument on the inconsistency of demanding, on the one hand, knowledge and skill which can alone be acquired by dissections, and, on the other hand, imposing the necessity of prosecuting the latter under fearful penalties — but for our professional readers this would be out of place because the matter is to them so palpable, and we cannot flatter ourselves that any remarks we might pen on this subject would affect the judgment of so profound a legislator as the author of the caustic witticism which has occasioned what we have already written.

We cannot refrain from expressing surprise that with Mr. Kennedy's notions of the propensities of doctors, he did not feel some apprehensions lest in their utter inability to cope with his satirical genius, they might be induced to take post-mortem revenge. He could hardly be blind to the danger of coming at last under their scalpels. This shows that he is a man of mettle, as well as wit and wisdom. *Au revoir* Mr. K.

Medical Department of the University of Buffalo. — The annual commencement in this institution will be on the 27th inst. The commencement exercises will consist of the conferring of degrees upon the successful candidates, accompanied by addresses, on the part of the Council of the University, by His Honor, George W. Clinton, President of the Council, and, on the part of the Medical Faculty, by Prof. Frank H. Hamilton. These exercises will take place in the evening of the 27th instant. Due notice of the place and hour will be given in the daily city papers.

The examination of candidates before the curators of the medical department of the University, will take place at the College building, on the 27th inst., to commence at ten o'clock, A. M. The curators will please notice this announcement, and they are respectfully invited to attend without more formal invitation.

The attendance during the course of lectures now drawing to a close has been, in numbers, respectable, and in the character of the class, aside from magnitude, more than respectable. For attention, zeal, and orderly deportment, the students assembled at this institution during the past winter, have been particularly distinguished.

The connection of Dr. E. M. Moore, of Rochester, N. Y., with this school, having commenced with the present session, we may with propriety advert to the impression which he has produced as a teacher of anatomy. His success has in every respect justified the expectations of his friends, and realized all that could be desired by the friends of the school. Thoroughly versed in his department of instruction, and especially familiar with its important practical relations, he possesses, in an admirable degree, the sterling qualities of a sound and instructive lecturer, viz., method, clearness, fluency, a chaste style, and an agreeable manner. The institution may well be congratulated that this important chair is so satisfactorily filled.

The permanent appointment of Prof. Dalton dated with the beginning of the present collegiate year. Our columns have borne testimony to the residence of Dr. D. in Europe during the last summer and autumn, for the purpose of prosecuting, under the most favorable circumstances, studies in physiology and morbid anatomy; and his lectures during the present session, especially in the former of these branches, have afforded abundant evidence of the extent and accuracy of his attainments in the recent developments due to organic chemistry, the microscope, and experimental researches. It is but simple justice to say that the lectures on physiology, with the accompanying microscopical demonstrations, and illustrations by experiments on living animals, have excited among the class a degree of enthusiasm which will become the basis of a lasting interest in that most attractive, progressive, and important province of medical science.

Of the two members of the Faculty just referred to, it does not seem to us indecorous thus to speak, inasmuch as they have recently become connected with the University. This fact, while it suffices for an explanation (if any be required) will also divest our remarks of any invidiousness which might otherwise attach to them.

A Discourse on the Life, Character and Services of Daniel Drake, M. D., delivered, by request, before the Faculty and Medical Students of the University of Louisville, January 27, 1853. By S. D. GROSS, M. D., Louisville. 1853. 8vo. pp. 92.

This discourse is as honorable to the heart of the distinguished author, as it is creditable to his ability as a writer. It is a tribute worthy of the character and services of the late Dr. Drake, whose decease has occasioned a void in the profession of this country, felt more sensibly in the great interior valley with which he was particularly identified by his life and labors.

The discourse is eminently a panegyric. But to this no just exceptions can be taken even by those (if there are such) whose estimate of the deceased is less exalted than that which is cherished by his enthusiastic and eloquent eulogist. Dr. Gross was long associated with Dr. Drake as a colleague and intimate friend; and while the partiality of affection would commend itself to respect rather than criticism, it is to be considered that these relations involve the best opportunities for judging correctly of character and attainments. But the evidences of the talents and industry of Dr. Drake, are before the public in his several works, and especially in the last and most elaborate publication to which he had devoted many years, and in the completion of which he was engaged at the time of his death.* An ardent and indefatigable laborer in the cause of medical education, the name of Dr. Drake will live in the recollections of the members of the numerous classes who have listened to his prelections, and in the histories of the institutions with which he was connected. True to the highest moral instincts of our nature, and to the tendencies of medical pursuits, he was not unmindful of objects of philanthropy. Aside from constant exertions, both by precept and example, in inculcating religious truth, temperance, and other virtues, we are informed by the author of this discourse that the asylum for the blind now in successful operation at Louisville, was mainly indebted to his efforts for that public interest which led to its erection and liberal endowment by the state.

In the domestic and social relations he not only was without reproach, but exemplified the devoted husband, the kind parent, the constant friend, the courteous gentleman, and the earnest Christian.

Such were the life, character and services of Dr. Drake, as portrayed in

* We are glad to see by a notice published by a son of the late Dr. Drake, that the materials for the second volume of his great work on the Diseases, &c., of the Interior Valley, are left in such a condition that the volume will be published.

the discourse by Dr. Gross. The subjects were worthy the pen of the writer of the discourse. It will be read with interest, not only by the numerous friends of the deceased, but by all who are fond of enjoying, by means of biography, intercourse with the great and good among the dead, as well as cultivating the society of the excellent among the living.

Marshall Hall, M. D.—It is stated that this distinguished physician is about to visit this country.

How rare is it that we have an opportunity of seeing the distinguished members of the medical profession of other countries on our own soil! Cannot some plan be devised by which they may be attracted to our shores, and, thus, a fair reciprocity, in this respect, be established?

Since the foregoing was penned, we find the following in the Boston Med. and Surg. Journal:

Dr. Marshall Hall.—Of course every medical reader is familiar with the name of this distinguished English author. He is now at Washington, accompanied by his lady and son. After visiting the South, he proposes to return and travel over the West extensively, and next season visit the Eastern States. Although considerably advanced in years, Dr. Hall is a laborious student, a close observer, and may justly be called one of the most celebrated medical writers of the age. We bespeak for him the attentions of the professional brotherhood wherever his steps may be directed.

Meeting of the American Medical Association.—We would remind our readers of the annual meeting of the American Medical Association, on the first Tuesday in May, at the city of New York. The attendance will probably be larger than at any previous meeting owing to the facilities of traveling in every direction to the great American metropolis, and the many attractions which the city of New York present, aside from those pertaining to the association. We do not presume too much (although it is quite unnecessary) in giving assurances of a hospitable reception by our brethren in New York.

Western Lancet.—Dr. Thomas Wood has lately been associated with Dr. L. M. Lawson in the editorial management of the above journal. The surgical department of the work is assigned to Dr. Wood.

Posthumous volume of Dr. Drake's treatise on the diseases, etc., of the interior valley. — We have alluded, in another article, to an announcement by the relatives of the late Dr. Drake, that the materials for the second vol. of the work on the diseases, etc., of the interior valley, were left by the author in a state to admit of its publication. Since that article was written, we have been informed that the labor of preparing the volume has been entrusted to S. Hanbury Smith, M. D., late professor of the principles and practice of medicine in the Starling Medical College, and more recently superintendent of the Ohio State Lunatic Asylum. Dr. Smith will bring to this duty a willing heart, and the ability as well as disposition to do justice to the unfinished undertaking of his deceased friend.

Decease of Prof. Horner. — We are pained to announce the death of the distinguished anatomist, Dr. Wm. E. Horner. He died of disease of heart. Dr. Horner held the chair of anatomy in the University of Pennsylvania, for nearly, if not quite a quarter of a century. His works on anatomy have long been recognized as standard text books in that department of science. In his early professional life he was connected with the army, and served, during the last war with Great Britain, on the Niagara frontier. Some interesting reminiscences of this period of army service, from his pen, have lately appeared in the Philadelphia Medical Examiner. In his decease science loses one of its most distinguished votaries; but his justly earned reputation, and many excellencies, still remain to confer honor on the American medical profession.

Obituary. — We regret to announce the death of Benjamin F. Wendel, M. D., of yellow fever, contracted in the discharge of his professional duty at Harbor Island, in the Bahamas, which occurred December 18, 1852. Dr. Wendel was one of our diligent and valued Assistant Physicians at Bellevue Hospital, in 1847 and 1848, when he nearly sacrificed his life in that service, having suffered an attack of ship fever of great severity and danger. He was a graduate, we believe, of the University of Louisville, Kentucky, and had just attained the age of twenty-six years. He was a clear headed and sound hearted man, and promised to take a high position in his profession. We loved him for his truthfulness, fidelity and honor, and sorrow most of all that we shall see his face no more on earth. Peace to his memory.—*N. Y. Med. Gazette.*

Concours in France. — It is stated that Napoleon III. has abolished the system of *concours*, and that medical professors and hospital physicians are hereafter to be appointed by himself.

Death of Dr. Pereira. — Died at London, Jan. 20th, Jonathan Pereira M. D., aged 49.

The name of Dr. Pereira is familiar to the profession of this country by his most valuable works on food and diet, and the *materia medica*. Science has lost an able and industrious laborer.

Chrono-thermal Medical School. — The Philadelphia Med. and Surg. Journal states, that the Legislature of Pennsylvania have granted a charter for a medical college denominated *Chrono-thermal*, and that a spring course of lectures is announced. Quackery appears to be particularly rampant in the ancient seat of medical science.

Transactions of the American Medical Association. — We perceive by our more favored exchanges, that the volume of the *Transactions* for 1852, has been issued. A copy has not yet reached us. We must offer this as our apology for not offering some notice of its contents.

Transactions of the Kentucky State Medical Society. — This publication has been received, but too late to be noticed in the present issue. It will receive attention in our next No.

Owing to an overplus of Editorial matter prepared for this No., some articles are omitted. Among these are notices of numerous introductory lectures received during the last few months. A letter from a correspondent on *hats and baldness*, is in type and will appear in our next issue. Several new publications have been received which, will be noticed hereafter.

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NO. 12.

ORIGINAL COMMUNICATIONS.

ART. I. — *On the use of Tobacco in producing Sciatica.* By HARVEY
JEWETT, M. D., Canandaigua, N. Y.

“The cold *Sciatica* cripples
Our senators, that their limbs may halt as lamely as their manners.”

The positive and latent influence of tobacco in the production of disease, has never received its merited share of attention from investigating medical writers.

Scarcely a page in our standard authors, and only occasionally a floating paragraph in medical Journals, has called public attention to this most fruitful, and all-pervading source of human misery.

The universality of the practice of chewing and smoking tobacco, and our familiarity with its use, may in a measure explain the fact, that we have hitherto overlooked its undeniable influence in producing that class of affections denominated neuralgic.

The effects of tobacco upon the uninitiated, are so unmistakable, so real, and positive, that we need not stop to explain them.

If any doubt exists on this point, let the novice smoke, chew, or apply tobacco to the surface of the body for a short time, and the pallid face, the

disgorged stomach, and palsied energies, will bear ample testimony to the truthfulness of our position.

So prompt and powerful are the effects upon the nervous system that it is not only unreasonable, but it is little short of madness to suppose that the habitual use of the narcotic for years, will cause it to be tolerated with impunity.

And strange as it may seem in view of the universally acknowledged effects of this poison, many medical men have not only recommended it to others for fancied or real diseases, but contrary to their own animal instincts, have vitiated their natural appetites, instilled into their persons a deleterious narcotic influence, and fallen into one of the most filthy habits that ever perverted the taste of a refined and cultivated people.

It is not our purpose, at the present time, to speak of the general, moral, and physical depravity, growing out of the use of tobacco, or even to allude to the numberless diseases which it engenders, but to confine our investigation to its effects upon the lower part of the spinal column.

That tobacco is instrumental, with certain constitutions and temperaments, in producing a large share of the limping, halting, cane-depending, crutch-using necessities so common among us, can be attested by a little observation on the part of the physician, and the result of his inquiries will essentially aid in relieving or radically curing those who suffer under these painful difficulties.

Diseases of the nervous system are universally acknowledged to be the most recondite and obscure of any class of affections to which the human system is liable.

The remote and proximate causes, and the unexplained mystery that hangs over the intense neuralgic suffering without any visible or appreciable change in the structure of the nerve itself, or any of the adjoining tissue, have been the theme of much fruitless speculation and inquiry.

The most intense agony is endured for years without the slightest morbid appearances; neither inflammation, thickening, adhesion of structure, nor the slightest deviation of any kind from the healthy condition of the part being apparent.

There is no part of the body which may not be the seat of neuralgic pains, yet some parts are more liable to them than others.

They are less frequently met with in the viscera supplied by the great sympathetic nerve than elsewhere.

Nervous pains are more common and severe in those parts which receive their nerves from the fifth pair, as the face and adjoining region.

In ascribing a large share of one class of neuralgic diseases to the influence of tobacco, it is not to be supposed that this is a tithe of the suffering and misery that tobacco entails upon its victims. A large proportion of those who use it keep themselves constantly under its narcotic influence. It pervades the whole animal economy, from the cerebral mass and spinal column, to the minutest nervous filament. All the organic functions are changed or modified by it, every drop of blood, every muscular fiber, every vitalizing breath, is to some extent brought under its modifying and pernicious influence.

In short, the head, the heart and the lungs, are made to feel directly the potential agency, and enduring consequences of this poison.

The brain is befogged, the blood is poisoned, the nervous system enervated, and the elements of disease scattered unconsciously throughout the delicate mechanism of the human frame.

There is great difference in the constitution and temperament of men, consequently the effects of tobacco will be varied, and more speedily manifested in some than in others.

Sciatica or lumbago, is not *necessarily the result* of excessive tobacco using. Neither does trembling of the hands, indigestion of food, palpitation of the heart, or indecision of purpose, follow in all cases as a consequence of this indulgence; but some, and perhaps many of the physical illa charged over to tobacco, will assuredly develop themselves in some form.

If in prosecuting our investigation, we find a large share of those addicted to the habit of smoking, subject to palpitation of the heart, disease of the throat, or sciatica, we are most naturally led to conclude that some connection, dependence or relation, exists between the habit and its almost unvarying consequences. And because we are unable to understand, or explain the phenomenon, it may be none the less true. If the attention of medical men should be turned to this subject, we believe the position might be established that nine-tenths of that form of neuralgic disease denominated sciatica or pain in the hip and leg, are the direct and legitimate effect of smoking or chewing tobacco.

It is true that only a small proportion of those who smoke are afflicted in this way, but our experience goes to prove, that disease of the nerves of the lower part of the spine and hip seldom exist except in connection with tobacco-using in some form, and generally in that of smoking. "A leading practitioner in England, gives a list of sixteen cases of *paralysis* produced by smoking, which came under his own knowledge within the last six months."

This is a significant fact, and goes to show the varied influence tobacco exerts upon different temperaments and constitutions.

Without going into a discussion of the subtle intricacies of nervous affections, and the manner in which external influences and vicious indulgences aid in the development of Sciatica or other neuralgic pains, I submit some statistics and facts taken from my note book, showing the connection between habitual tobacco-smoking and Sciatica and other anomalous ailments of the loins, hips, and lower extremities.

CASE I.—Mr. N——, aged 48, a farmer, of good constitution, and apparently robust health, of sallow, dingy complexion so common to smokers, applied for relief in the summer of 1852, stating that he suffered the most intense agony in his right hip and leg.

On investigating his case, I learned he had been complaining of rheumatism, as he expressed it, in his back, at different times, for several years; but had attended to his usual business up to within a few weeks of my seeing him.

His health was good aside from the pain in the hip, which was severe and uninterrupted, except when lying upon his back, when he was comparatively easy.

The usual remedies had been resorted to with no effect. On instituting a critical inquiry into his habits, I ascertained he had smoked from five to fifteen pipes-full daily for eighteen or twenty years.

Upon indicating to him the probable cause of his suffering, he promptly decided to make any sacrifice that would afford him relief. The pipe was laid aside, he gradually improved, and in two months was entirely free from pain. He called upon me a few days since, and expressed his gratification that his attention had been directed to the subject of smoking, that he was unwilling to believe himself so enslaved to the practice until he attempted to abstain from its use, and that he felt a degree of vivacity of spirit, and elasticity of body he had not known for many years.

CASE II.—Mr. G——, aged 68, a farmer, of bilious temperament, and swarthy, dusky complexion; had been accustomed to labor in the earlier part of his life, but was prematurely infirm and disabled on account of pain in his hip, which he had suffered, with more or less intensity, for twenty years.

He also had vertigo, palpitation, indigestion, sore mouth and tongue, together with great physical prostration and debility. The latter symptoms had been gradually increasing upon him for several years.

At the time I saw him in consultation with his attending physician, he was confined to his bed, dejected in spirit, and appeared like a man eighty years old. His pulse was faltering and intermittent, faculties lethargic, sleep disturbed and dreamy, epithelial investment of the mouth and tongue abraded. In short, the energies of the system and vital powers, were evidently prematurely waning, without apparent or obvious cause; with no organic disease, and no fever. The only satisfactory explanation of his case was to be found in the fact, that the patient had been for forty years one of the most inveterate tobacco-smokers in that region. His habit was to smoke from morning until night, except when taking his meals, and during divine service on Sunday, when he had a brief interval, but he seldom failed to light his pipe before leaving the sanctuary to return home.

The habit with him was so confirmed, his system so constantly narcotized, that he was restless and wretched, except when in the enjoyment of his pipe.

Upon stating to him my opinion as to the cause of his disease and disability, he frankly acknowledged his conviction that tobacco-smoking had insidiously and gradually brought him to his present condition. That for years, when he had allowed himself to reflect upon the subject, he had known that it was destroying him, yet he had never been able to resist the almost insatiable demand of his perverted appetite. With this acknowledgment upon his lips, and death in full view before him, he would call for his pipe and take a whiff or two at a time, which practice he continued up to the period of his death.

In this case, which is an extreme one, we have not only the pain of the back and hip so common to smokers, but we have a clear and full development of its pernicious influence throughout the entire system. The hips, tongue, mouth and fauces, were as red as scarlet, which, together with the palpitation and vertigo, he had suffered to some extent, many years past.

With this train of physical ills constantly pressing upon him; with an appreciating sense, and in view of the ultimate consequences, he persisted in the habit of smoking to the last, and died a victim to his indulgence.

CASE III. — Mr. B——, aged 36 years, a farmer, of sanguino-bilious temperament, was seized suddenly, during the winter of 1851, with an excruciating pain in the hip, extending down the leg, unattended with redness or swelling, which disabled him from attending to his ordinary duties. The only relief he could obtain, was by lying upon his back. His general health was good, save the local pain, which continued during the winter and most

of the succeeding summer. The ordinary remedies and nostrums were resorted to, with but partial and temporary benefit.

On inquiry Mr. B. stated that he had been in the habit of chewing twelve pounds of tobacco annually, for several years, and smoked a pipe occasionally when it was convenient. He was never without his quid, except when asleep, and then he sometimes neglected or forgot to lay it aside.

Upon being apprised of the possible relation between his tobacco-using and his suffering, he decided to abandon his pipe altogether, and chew much less.

The pain gradually abated as his system was partially relieved from the influence of tobacco, and he is at present comparatively free from it, although he continues to chew moderately.

CASE IV. — Mr. W——, aged 67, a laborer, had smoked a pipe thirty-five years. He had suffered intense pain in the right hip and leg many years, which disabled him for active labor. About three years since his attention was directed to a small tumor upon the lower lip, which gradually increased in size, attended with a stinging, creeping sensation. When I saw him in consultation with his attending physician, the sublingual and maxillary glands were enlarged, indurated and painful, with a well-developed epithelial cancerous ulceration of the under lip, of which he died in a few months.

This man not only smoked habitually, but he made use of the same pipe for months, without burning out or purifying with fire. The lips, salivary glands, and lining membrane of the mouth, being constantly subjected to the acrid influence of an old pipe, are not unfrequently the seat of cancerous disease.

CASE V. — Mr. R——, aged 47, a farmer, of active habits, good constitution, and unusually robust appearance, was seized with severe pain in the loins and hip while attending court as a juror in May, 1851. The pain continued with great severity during the spring and summer, despite the nostrums and pathys that were resorted to for a cure. At times the pain was periodical, returning regularly every twenty-four hours with greater intensity and attended with spasmodic action of the muscles, so as to draw the leg suddenly up to the body. Large and repeated doses of morphine gave only partial relief from the suffering.

Much obscurity rested over the pathological character of the case. He was treated alternately during his illness for lumbago, psoas-abcass, and

sciatica. Early in October there was observed on the left side of the spine, about the fourth lumbar vertebra, a slight prominence which gave evidence on pressure of something soft, doughy, or yielding, beneath the fascia — of indistinct, or obscure fluctuation. When standing upon his feet he inclined to lean backward to relieve himself of pain and tension produced by standing upright or bending forward. Upon consultation it was deemed advisable to puncture at the point of the elevation. A double-edged bistoury was introduced one inch and a half, when the resistance to the progress of the knife ceased as though it entered a cavity; but no discharge save florid blood could be obtained. The patient continued to suffer the most excruciating pain for a few weeks and died.

In making a post-mortem examination we found the center of the longissimus dorsi muscle filled with a whitish, cheesy, brain-like substance, which extended along the course of the spinal column and sacrum, to the gluteal muscles where it was found in globular masses from the size of a pea to that of an acorn. The center of the longissimus dorsi contained nearly a pint of the encapholoid deposit, while the exterior and adjacent structure was unchanged in appearance.

The transverse processes of the lower lumbar-vertebra were extensively diseased, involving the bodies of the bone.

A specimen of the diseased mass was submitted to microscopic test, by Prof. Dalton, which presented the characteristic evidence of tubercular formation.

Familiarity with the patient's habits for fifteen years, enables me to speak of them from personal observation. He was in the practice of smoking a pipe, most of his waking hours during that period of time. It was his constant companion when at his ordinary labor up to the time of the attack.

The question very naturally arises in the mind of the reader, had the habit of smoking any thing to do with the suffering and disease which caused his death. I have no hesitation in deciding affirmatively — and that he would not have been afflicted in that way had he never used tobacco.

The abruptness of the attack, is only one, among many instances that have fallen under my observation, when the subject had been in the habitual use of tobacco for a long period of time, with scarcely any perceptible manifestation of its untoward influence, until its insidious, silent, pent-up virulence, suddenly bursts upon its victim, revealing the occult treachery and deceitfulness of this pernicious practice.

CASE VI. — Mrs. S —, aged 70, of bilious temperament and fine

constitution; has had pain in the hip upward of twenty years, and is now unable to walk except by the aid of a crutch. She has smoked a pipe forty-five years.

CASE VII.—Mr. B——, aged 38, a farmer, athletic frame and unimpaired health, except the annoyance of pain in the hip and leg along the course of the ischiatic nerve. Mr. R. smoked a pipe most of the time when at his usual avocation, and all his leisure hours are occupied in that way.

CASE VIII.—Mr. A——, aged 72, has smoked a pipe 40 years; has had pain and lameness of the hip most of the time for thirty years past.

CASE IX.—Mr. H——, aged 35, has chewed tobacco fifteen years; has had pain in the hip and thigh, accompanied with lameness ten years.

CASE X.—Mr. B——, aged 49, a farmer; has smoked tobacco immoderately, twenty-six years; has had pain in one or both hips in the direction of the ischiatic nerve, fifteen years.

CASE XI.—Mrs. S——, aged 38, has been afflicted three years with pain along the track of the ischiatic nerve. Mrs. S. *never used tobacco.*

CASE XII.—Mr. L——, aged 60, a farmer, of large frame and apparent robust health; has smoked a pipe and tobacco thirty years; has had pain in his back and hip twenty years. And within the last five years has had two attacks of paralysis, from which he has partially recovered, although he continues to smoke occasionally.

CASE XIII.—Mr. J——, aged 45, has been in the habit of smoking a pipe twenty years, and has had pain in the hip eight years.

CASE XIV.—Mr. S——, aged 63, has smoked a pipe thirty-five years; pain in his back and hip twenty-two years.

CASE XV.—Mr. C——, aged 72, a farmer, had smoked a pipe forty-five years; has had pain in the hip twelve years, and is unable to walk without a cane.

CASE XVI.—Mr. J——, aged 28, had smoked alternately a pipe and

segar nine years; had a severe attack of sciatica, followed by inflammation of the articular structure of the joint, with displacement of the head of the os femoris from the acetabulum.

CASE XVII.— Mr. D——, aged 60 years, has smoked a pipe thirty-five years; had pain in hip and leg ten years.

CASE XVIII.— Miss S——, a servant girl, aged 27, was suddenly seized with violent pain in the hip, in the latter part of the summer of 1852, after a hard day's work, and exposure to the evening air. She was sent to the poor-house of the county for treatment, where she still remains. She suffers severe pain in the direction of the ischiatic nerve, and is unable to walk without assistance. She has recently complained of pain in the chest, accompanied with cough and other symptoms of phthisis. The physical signs give undoubted evidence of tubercular formation in the summit of both lungs. *This patient never used tobacco.*

CASE XIX.— Mr. P——, a farmer, aged 40, has had pain in the hip, running down the leg, four years; has smoked a pipe moderately twenty years.

CASE XX.— Mr. N——, a laborer, aged 42, has smoked moderately, and chewed constantly, for twenty years. Has had pain in the right hip and leg three years.

CASE XXI.— Mr. P——, aged 50, a painter by trade; has smoked twenty-five years; had pain in the hip four years. Still pursues his avocation and continues to smoke.

CASE XXII.— Mr. A——, aged 48, of active business habits, and nervous temperament; has chewed tobacco twenty-five years. At one period he used three small papers of fine cut in a day; but for the last year or two he has chewed only two or three papers a week. He has been subject to attacks of what he calls neuralgic rheumatism; sometimes attacking the shoulders, arms and back, but the pain is most intense and continued in the direction of the ischiatic nerve. This case differs from most of the foregoing, inasmuch as the paroxysms of pain last but three or four weeks, and entirely disappear under treatment, and again recur at longer or shorter intervals.

The above twenty-two cases of this particular affection of the nerve of the

hip, constitute all that have fallen under the writer's observation in a practice of twenty years.

Some of the subjects were advanced in life, and it may be said, were only suffering the rheumatic pains, and decrepitude incident to age. But in almost every instance, the suffering and lameness commenced in the prime of life, and before they had reached threescore, and ten were not only suffering, but disabled and dependent; unlike their cotemporaries in life, who had performed more labor without the use of tobacco, and were comparatively erect in person, and still active in habit. Two of the above cases, who still suffer pain along the course of the ischiatic nerve, never used tobacco in any form.

Many of the cases referred to are still living in the delightful enjoyment of the luxury of tobacco, notwithstanding the writhing agony and decrepitude they endure as the penalty of the indulgence.

Some of them, doubtless, ignorantly shortened their lives and greatly increased their suffering, by persistence in the practice of this irrational and fancied pleasure.

Three of the more recent cases under medical advice, were induced to abstain from the use of tobacco, and were relieved after a few months from the suffering in the hip, and restored to the enjoyment of health, which was unknown to them while under the influence of the narcotic.

That there is an incalculable amount of human wretchedness growing out of the enormous consumption of tobacco, no one of common observation will pretend to deny.

The profligate waste of health, time and money, the abridgment of precious lives, and many other considerations not within the legitimate scope of this paper, all combine to make it a subject worthy the most grave consideration by medical men.

CANANDAIGUA, April, 1853.

ART. II. — *A Case of Poisoning by Arsenious Acid.* Reported by J. BRYANT SMITH, M. D., of Louisville, Ky.

During the month of February last, a negro woman and her child residing in this city, were suddenly attacked with symptoms of poisoning after drinking some tea which had been prepared by some members of the family. The physician who was called in attendance, not suspecting poison, administered some laxative oil, which produced no effect. The child and

mother were vomiting continually, complained of burning pain in the epigastric region, the pulse feeble and fluttering, the extremities cold, face anxious and debilitated.

During the day following the child died, and a post-mortem was made by the coroner of the city, and the stomach of the child removed. The case having come under the observation of Dr. Lyle, of this city, he obtained the coffee-pot, with its contents, from which the patients had drunk, and brought it to the laboratory of the University of Louisville, for analysis. The vessel contained about a quart of tea, with a quantity of white sediment, nearly half an ounce in weight, which, when placed on hot coals, emitted the peculiar alliaceous odor of arsenic. On examining the tea by Marsh's test, it was found to give unmistakable evidence of the presence of arsenic, and appeared to be nearly, if not quite, a saturated solution of arsenious acid.

The coroner having brought to Prof. Silliman the stomach removed from the child, it was subjected to the process which, as it is interesting to the physician and easy of application in similar cases, may prove useful to the readers of your Journal.

This portion of the viscera was tied before removal from the body, at both the œsophageal and pyloric orifices, and the contents thereby secured. On removing these ligatures, there flowed about four fluid ounces of a dark coffee-colored fluid, having the flavor of some essential oil. This fluid gave with the tests to be described, no evidence of arsenic. The mucous lining of the stomach was, in some spots, slightly ecchymosed, but otherwise presented no abnormal appearance. A portion of it was cut into small pieces, placed in a porcelain evaporating dish, and treated with a very few drops of sulphuric acid and a drop of nitric acid, and heated. By this means the organic matter, which, when present, makes the detection of arsenic difficult, is destroyed, and the insoluble arsenious acid, if present, is converted into soluble arsenic acid; the material soon becomes reduced to a coaly mass, it is heated then until the sulphuric acid is driven off, allowed to cool, and then treated with water to remove the arsenic acid if present. This liquid thus obtained is filtered and treated with a current of sulphuretted hydrogen gas obtained in the usual way from sulphuret of iron and sulphuric acid.

By this process, conducted in the case of the stomach of the child, we obtained, after a time, a small yellow precipitate; this, if arsenic had been present in the solution, would have been the "yellow sulphuret of arsenic" or orpiment. But in acid solution, sulphuretted hydrogen may precipitate sulphur, which may be mistaken for orpiment. If the precipitate is very small, as it was in the present case, it is boiled to facilitate its being separate

filtered from the liquid, and then treated with "aqua ammonia," which dissolves it if it be orpiment, but has no action on sulphur. The solution in ammonia is now evaporated to dryness, and the residue redissolved in hydrochloric acid. We have now a solution containing all the arsenic which may have been present in the portion of stomach or other viscera under examination, and which we can subject to the appropriate tests for the detection of arsenic. The one which in this case we employed first was one known as "Reinsch's," which consists in boiling in the hydrochloric acid solution a strip or two of perfectly clean bright copper. If arsenic be present in the solution these strips are soon coated with a gray deposit of metallic arsenic. In the case under examination, on the slips of copper, in the course of a few minutes, there was an evident blackening, sufficient to render the presence of arsenic in the solution certain. To corroborate this test recourse was next had to "Marsh's test," which consists in adding the solution suspected to contain arsenic to a flask or bottle in which are placed the materials for generating hydrogen gas, namely, zinc and sulphuric acid. Pure hydrogen gas, when burning, is entirely converted into water, and hence there is deposited on any cold surface held over it, only this compound; but the presence of arsenic in the generating flask, gives rise to the production of "arsenuretted hydrogen," which produces a peculiar change in the flame, and an evolution of white clouds of arsenious acid. If over this flame, and about its center, is held a piece of cold porcelain or any white dish, there will be deposited a brownish-black "tache" or spot, with a metallic luster. There is only one other metal with which this is liable to be confounded, and that is antimony, which produces like results under similar circumstances. It is, however, more black and has less luster. The two are also easily distinguishable. Exposed to the vapor of iodine in a small crucible, the antimony spots turn reddish-orange, while arsenical spots appear orange-yellow, and soon entirely vanish.

Exposed for a moment to the vapor of chlorine gas obtained from ordinary bleaching-powder, (hydrochlorite of lime,) in a capsule, the spots of both metals disappear. If a drop of nitrate of silver be then allowed to fall on the surface of the vessel on which the spots were, if arsenic be present, (in the form of chloride now,) there will be a brick-red stain visible, amounting to a precipitate if the metal existed in any quantity, while antimony produces no such results. These distinctions are conclusive. In the case which was examined in this way, we detected in the stomach and liver of the child, perfectly decisive evidence of arsenic by both of these tests.

During the progress of this investigation, an old man, who had also drank of the tea, had died, and as poisoning was suspected, he was disinterred and

the body opened in presence of Dr. Silliman and myself, by Dr Lyle. The liver and vessels were very much engorged; the blood not coagulated; the stomach and liver were removed and taken to the laboratory, and subjected to the same process of investigation before described. In both, the evidence of arsenic were as conclusive as in the case of the child. The mother, who at the time was nursing, recovered, but complained of her infant being sickened by her milk and refusing to nurse. A portion of the milk was sent us for analysis; it was very dark-colored, and gave, by the same test, evidence of the presence of arsenic. I am not aware that this poison has ever been detected before in this secretion, and it may be the happy accident of lactation which saved the life of the mother, in opening thus a channel through which the poison was discharged from the system.

The criminal part of this case rests in a negro man who had for a long time professed friendship for the family, and under such profession had become quite intimate. The morning on which the poison was administered he made some base offers to the woman and her sisters, which were repulsed, when, in revenge, he infused arsenious acid into the tea. The evidence which was given in this case was deemed sufficient to remand the prisoner for trial in the following term of the Circuit Court of Kentucky.

It seems to me that there ought to be in every state some legislation whereby the sale of arsenic may be restricted to druggists properly authorized, and that such druggists should be compelled to keep a register, in which every sale of arsenic should be entered, with the name, residence, and occupation of the buyer and the purposes for which obtained. Under such vigilant restrictions no person would dare to purchase arsenical poisoning except for lawful purposes.

ART. III. — *Extensive Fracture of the Cranium, with Extravasation of Blood, Death occurring in fifteen hours.* Read before the Buffalo Medical Association. By JAMES M. NEWMAN, M. D.

On Sunday, the 23d of January, 1852, about 8 o'clock, A. M., a man by the name of John Malony, was received into the Erie County Penitentiary, in a state of complete insensibility. He had been committed by a Watch-House Justice, for twenty-five days, on the charge of drunkenness, though he was completely insensible at the time of his sentence, and unable to give any account of himself. The constable accompanying him, pronounced him "dead drunk."

He had been conveyed nearly a mile in an open wagon, upon a chilly morning, and was carried into the Penitentiary apparently more dead than alive. He was cold and nearly pulseless; breathed feebly; face congested; speechless, and unconscious of every thing.

He was placed upon a bed near the fire, warmth applied to the extremities, frictions used, and stimulants administered. No benefit resulted from these applications, other than a return of warmth to the surface, an increase of the pulse, and of the strength of respiration.

I saw him about 2½ o'clock, P. M. I found him unconscious; pulse about 125 per minute; breathing stertorous; pupils dilated and insensible to light; eyelids drooped; the conjunctiva injected; the lips of a dark-blue color, but the face blanched and pale. I could obtain no other information as to the cause of this train of symptoms than that already given. His pulse evidently failed while I was examining him. I did nothing, and he died in about half an hour after I first saw him. There was not the least mark or sign upon the face or head, to indicate that he had received injury.

From a prisoner who had also been committed the same day, we learned that he had been placed in the same cell, in the watch-house, with Malony, about 12 o'clock the previous night, and that he found him then insensible, and lying upon the floor, and that he had vomited. He endeavored to arouse him, but without success, and he continued equally insensible during the night.

The wife of the dead man asserted that her husband was well the previous evening, but admitted that he had been drinking some during the day. His brother stated that he was at his house near the Niagara Falls car-house, until between nine and ten o'clock the previous evening, when he left for his own home, and he insisted that he was sober at the time.

A coroner's jury was summoned, when the following facts were elicited:

Post-mortem examination; about 10 o'clock Monday morning, some eighteen hours after death, assisted by Dr. Eastman. Attention was directed to the head. Externally it presented no mark of contusion, or injury, except that the right ear was deeply colored. About an inch above, and a little to the right of the right eye-brow, a point *seemed* more depressed than the parts surrounding, and suggested the possibility of this being the point of a fracture.

Upon dissecting off the scalp, the parts over the right temporal muscle, and the muscle itself, together with the parts over the occiput, were deeply infiltrated with blood.

Upon removal of the calvaria, a fracture was found commencing about

three-quarters of an inch above, and a little to the right of the superciliary ridge, involving both plates of the frontal, temporal, parietal and occipital bones of that side, extending to the base of the cranium. The internal portion of the *os temporis* was driven in upon the brain.

A second fracture, through the inner plate only, existed in the parietal bone about an inch above the first fracture, extending in length about one half of the distance of the first.

Blood had been largely effused upon the brain, external to the dura mater, forming a large, firmly coagulated mass, weighing, as was estimated, some four ounces, and compressing the brain so firmly, as to have reduced the right hemisphere to nearly one half of its normal size. Numerous dark coagulated masses were to be seen beneath the dura mater, studding the entire surface of the brain, giving it a dark, mottled appearance.

The brain was not cut into, or removed, or the examination further prosecuted, as we had found sufficient cause of death, and the jury were in waiting, and the friends of the deceased were becoming clamorous for the removal of the body.

Before the coroner's jury, the testimony disclosed the facts that Malony was seen about 10 o'clock the evening previous to his death, intoxicated; that at near midnight he went to the front door of a dwelling-house on Franklin street, and rang the bell; and being asked, from an upper window, what he wanted, he looked up to reply, and in doing so fell over backward from the stoop, upon the edge of which he was standing, falling upon the pavement beneath, a distance of some four or five feet. Not moving from the position in which he fell, a watchman was summoned, and the man conveyed, in an insensible state, to the watch-house; and in the morning, without any attention being given to, or examination made of his case, he was convicted and punished for the crime of drunkenness.

The points of interest in the case are, the absence of any mark or injury; the extent of the fractures; and the amount of the effusion of blood.*

* We would add, as another point of interest in the case, the detention in a watch-house cell all night, no medical examination being requested, and the trial, conviction and sentence the following morning, of a person all this time in profound coma! In this aspect the case presents a striking commentary on the summary administration of justice! We trust, for the credit of our city, as well as for the sake of humanity, such truly horrible instances are of infrequent occurrence.—EDITOR BUFFALO MED. JOUR.

ART. IV. — *Necrosis of Femur resulting in a malignant degeneration of the soft parts requiring amputation.* By C. D. Robinson, M. D., of Almond, Allegany county, N. Y.

PROF. F. H. HAMILTON:

Dear Sir, — Some medical friends consider the case of Necrosis, in part detailed to you, to be worthy of publication, as symptoms of malignancy are extremely rare, or altogether wanting in such cases. With this view, I have thought best to give you a more extended history of the case.

The following is the account as given by the patient:

A. S. S., aged 41 years, a merchant by occupation; of good constitution, of nervo-bilious and sanguine temperament, was attacked with fever when about 11 years of age, which continued some three or four weeks; during convalescence he was taken with pain and swelling of the lower part of the right thigh, which terminated in suppuration, and after some three or four months, pieces of bone made their appearance at the sinus. However, he regained his health so as to engage in the active pursuits of life, occasionally suffering from attacks of inflammation of the thigh, which were frequently followed by the discharge of small spicula of bone. About eight or nine years ago, a large piece made its appearance, which was attached to the shaft of the femur. The patient was often importuned to have this extracted, but refused, as he did not think it necessary, the pain and inconvenience from it being inconsiderable, evidently dreading the pain of extraction.

His general health remained unaffected by the local disease, notwithstanding the profuse discharge, which became very offensive, and the irritation to which he was continually subjected, until about the end of July last. I was now requested to see him. He was affected with violent neuralgic pains in the lumbar and umbilical regions. The limb was swollen and tender to the touch, and the granulations around the old sinus were loose and spongy, like fungus in appearance. Considerable excitement of the circulatory system, loss of appetite and derangement of the liver were present. I attributed the foregoing symptoms to the irritation produced by the loose piece of bone, and urged its removal, to which he consented.

He was put upon the use of conium, quinine, iron, alkalies, and such other remedies as appeared from time to time to be indicated. A slight improvement took place in his general health, though the local symptoms remained nearly the same, except the limb was not as painful.

In Oct. I was obliged to be absent for two weeks, during which he put himself under the care of another practitioner, who induced him to visit New

York, for the purpose of consulting Dr. Mott. Dr. M., together with Dr. Post, pronounced the disease of the soft parts, of a cancrroid nature, and recommended amputation. To this Mr. S. did not wish to submit, and soon fell into the hands of one of those irregular practitioners who *boast* of great skill in the cure of old sores. He continued to grow worse, appetite failed, emaciation, loss of sleep from severe pain, colliquative sweats, and irritative fever appeared to be rapidly hurrying him to the grave.

On the 6th of February last, I was again requested to take charge of him. The limb now presented a frightful appearance, being much swollen, and discharging matter from several openings. The different tissues of the thigh were condensed or agglutinated.

I recommended amputation as the only means of saving life, and on the next day, assisted by Drs. Case, Olin, and my partner, Dr. Jas. W. Black, I removed the limb above the junction of the upper with the middle surgical third of the femur. Our patient was put under the influence of chloroform, and bore the operation well, and at the time of writing, has almost entirely recovered; the stump being nearly or quite healed. Constitutional symptoms have all improved since the operation. His appetite is good, and he has nearly regained his ordinary flesh. Countenance looks healthy, and has lost that peculiar sallow expression so characteristic of malignant affections. He is able to go from his dwelling to the store upon crutches.

The bone was found, on examination, carious, for some three inches above the condyles, and so softened that the handle of a scalpel could be pushed, with ease, into its medullary structure.

There were two large openings, one of which extended quite through the bone. I infer from the history of this case, that had the patient suffered the surgeon to have removed the cast-off portion when nature failed, an entire cure would have been effected, and the subsequent malignancy of the soft parts, which extended to the bone itself, was the legitimate effect of long-continued irritation, acting on a susceptible constitution.

Before closing, I will briefly advert to a plan of treatment in necrosis, to which I have resorted in a few cases. It consists in making early and free incisions down to the diseased bone. The great object in the treatment of this disease is to subdue the inflammation before the bone is destroyed by its severity. The usual treatment, as you well know, consists in the exhibition of purgatives and febrifuge medicines, anodynes, with leeches, blisters, fomentations, poultices, &c., at first, and as soon as fluctuation can be detected, an incision, to give free exit to the matter. In the method of treating necrosed bone, to which I have referred, the incision, to be effectual, should be made

in the onset of the disease, as the rational end is to relieve the inflamed periosteum and bone before supuration takes place.

Yours,

C. D. ROBINSON.

ART. V. — *Case of Ununited Fracture, cured by Subcutaneous Perforation.* By HARVEY JEWETT, M. D., Canandaigua, N. Y.

The discrepancy of opinion that exists among surgeons on the subject of treating ununited fracture of bones, and the failures that have attended the various plans proposed for their consolidation, have induced me to add the testimony of another case of subcutaneous perforation of the ends of the bone, proposed by Prof. Brainard, of Rush Med. College.

Dr. B. reports a case in the May No. for 1852, of the Buffalo Med. Jour., successfully treated in this way after a failure with the seton, which is the only case we have on record treated in this manner.

Prof. Sandford, of Davenport, Iowa, gives two cases of ununited fracture, which were cured by subcutaneous scarification of the ends of the fragments, which more nearly resembles the mode proposed by Prof. Brainard, than any other recorded case that has come under my observation.

The simplicity, safety, and ease, with which this operation is performed compared with other modes proposed for the relief of such conditions, if it be equally effectual, certainly commends itself to our consideration and regard.

The pain of the operation is comparatively slight; it is attended with no hæmorrhage, and the suppurative process, which tends to convert the effused blastema into pus, is wholly obviated.

The result of this simple experiment thus far, has been highly satisfactory, and I trust will be faithfully tested whenever such cases present themselves to the surgeon.

Mr. P., aged 30, on the 31st Sept., had both bones of the leg broken by being caught under a large stone; the tibia, one-third distant from the upper, and the fibula, one-third from the lower extremity. The ordinary dressings were applied at the time of the accident, and the limb put upon the inclined plane. About the twelfth day after the accident the dressings were loosened by the patient, and were allowed to remain so for three days, when they were again secured by the attending physician.

At the end of the twelfth week the dressings were removed. The ankle and foot were œdematous, the fibula was firm, but not the slightest evidence

of bony union in the tibial fracture. I procured a long straight awl, (the ordinary pegging awl being too short for the purpose,) and introduced it between the fragments, and made six or seven perforations in the lower, and without removing it from the skin, bored the end of the upper fragment in the same way. The limb was secured, the patient put upon a generous diet, with an occasional glass of porter. At the end of two weeks, a second perforation was made in the same manner, and the leg firmly secured with splints as before. After ten weeks the dressings were removed and the union was complete.

CANANDAIGUA, March, 1853.

ART. VI. — *Hats and Baldness.*

The following letter has been received since our last issue. We heartily sympathize with our correspondent, and regret, on his account, as well as in behalf of many others, that the promulgation of our discovery comes too late for their benefit. They must comfort themselves by the reflection that if the needed reform be accomplished, a valuable boon will be conferred on future generations!

ALMOND, *March 21, 1853.*

SIR,—Having just seen your article on baldness in the Journal, and being unfortunately one of the many to whom it addresses itself with peculiar force, I acknowledge the justice of your remark, that any allusion to one's bald pate "leaves a sadness at the bottom," as it reminds him of the loss of that appendage of manly beauty, and strength, of which Delilah bereft Sampson, the loss of which was followed by such sad consequences to the Jewish champion.

I well remember the first symptoms of baldness which I felt. Before I became aware that my hair was falling—a soreness on the crown of my head, in the morning when brushing my hair—and had I then been aware of the cause, I might not now be obliged to deplore its loss. I am now forty-four years of age, generally well, hale and hearty, and though time has laid his hand on me but slightly in other respects, yet this depilous condition of my vertex, and certain crow-foot appearances visible in the corner of my eyes, admonish me, as I in vain attempt to cover the denuded portion of my caput from the temporal regions,—thank God! I am not gray—that I must soon pass off to make room for those who are to come after. With my

present experience, aided by your views, I would eschew hats forever, and wear turbans "a la Turk," or anything else rather than lose my fair locks.

"What in the whole work of art," says a late writer, "is more unnatural, stiff and uncomfortable than a modern hat—a mass of glue, paper, and wool, formed into a cone or circle, with hard lines and stiff unyielding corners presented to view on all sides, without beauty, or grace, or comfort, or even use," &c., &c.

As the ladies *have donned* some portions of the male attire, I would suggest the propriety of gentlemen adopting the head-dress of the fair sex, which it appears is so efficacious in preventing baldness.

I go for a change—for the bonnet—perhaps; altered a little, so as to give it more of the air of the male costume—if for nothing else than to *be revenged* on the ladies—*provoking creatures*—for adopting the pants. It might be so constructed as to appear graceful, to fit easily, to set lightly on the head, and be surmounted by a plume, after the fashion of the Kossuth. But cannot something *be done* to do away the tyranny of fashion in this article of our attire, which is not only worthless for comfort or beauty, but positively injurious, as whatever interrupts the circulation in the scalp, may, through the vascular connection existing between it and the brain, induce disease of that organ!

A SUBSCRIBER.

ECLECTIC DEPARTMENT,

AND SPIRIT OF THE MEDICAL PERIODICAL PRESS

Notes taken from Lectures of M. Trousseau, on Croup and Diphtherite, at L'Hopital Des Enfants Malades. By WALTER ATLEE, M. D., of Philadelphia.

PARIS, 1852.

In the Hospital, gentlemen, it is but rarely that you can follow a case of croup from one end to the other; generally you see but the last scene. The croup is the most usually curable when the physician is called in time. There is an inflammation of the mucous membrane of the larynx, sudden, acute, considerable, without false membrane, as the inflammation of the nasal mucous

membrane. Let us now pass from this croup that gets well to the croup that kills. The *ulcus Egyptiacum* of the ancients is the same affection. The Spaniards of the sixteenth century have described it marvelously. I am going to describe an epidemic: After the fall of Napoleon, the Bourbons made what might be called provincial armies; each province furnished its legion. The legion of La Vendée was at Tours, in 1818. M. Bretonneau is there. It was observed that all the men of this legion had an affection of the gums; they were swollen, and with a white border; in some cases the cheeks and even the pharynx were covered with false membranes, white and fetid. A certain number died, soon after, in the neighborhood of the barracks, the children died of croup. M. Bretonneau, struck by this, examined what he was calling gangrenous angina, and found it to be the same affection.

I will describe to you a case of croup: An adult complains of sore throat; this year I have seen four or five cases in adults. There is a slight febrile movement and a swelling of the glands at the angle of the jaw. If you look at the tonsils you will see a piece of false membrane; an hour after it is larger, for it spreads as oil upon cloth. If you remove it, you see that it has a thickness of three or five millimetres; you see false membrane on the other tonsil, on the palate, &c. By forcibly pushing down the tongue, you can almost always see the limit of the disease; but the false membranes commence to thicken; they form a kind of stratifications, fetid, gray, brown, yellowish, gangrenous, but there is no gangrene, and it will yield in a few days to a proper medicine. Cough commences, the respiration begins to be difficult and hissing. Sometimes they get rid by the cough of false membranes, which gives an immense relief, but in twelve hours all comes back again. Let us take, now, a child of five years; he cannot tell us that he has sore throat, &c., at the commencement. He dies two or three days after the commencement of the laryngeal affection, but eight days after the affection of the pharynx. Bring up your children in the fear of the croup, and teach them to tell you when they have sore throat, and to show their throats when asked. The parents only have alarm when the cough commences. In the child the disease marches more rapidly than with the adult; with them it is very rare that the false membrane has a gangrenous appearance. This is because they do not acquire sufficient thickness. They remain white. This is the reason why in the child this same disease has one name, and in the adult, another.

In the former, the formation of false membrane may begin in the pharynx: it is, however, rarely limited to this spot, but extends with the greatest facility into the air passages, when it is called croup. In the latter, it is commonly limited to the pharynx and is then termed diphtherite.

Now I will describe the true croup: After three or four days of malaise, the child has a slight cold: moreover, he coughs a little, but the cough has no extraordinary character. After twenty-four hours the cough assumes another character; it is *hoarse* and *frequent*. The hoarseness increases with the frequency; it is very sonorous, very noisy. The voice is almost extinguished; after some days the cough becomes less hoarse, less sonorous, and much less frequent. The voice is extinguished, and the respiration assumes a peculiar character. In an individual in good health the respiration is performed without noise, but when there is a narrowing of the larynx, the air is forced to traverse it with a hissing sound. At this period the respiration is constantly difficult, and what are called paroxysms of suffocation occur; all

of a sudden the difficulty of breathing becoming greater. These paroxysms at first take place every two hours, afterward every hour, then every half hour, &c. The child is so agitated in these paroxysms that sometimes there is a kind of madness. When they are over, he falls into an extreme prostration. He dies, generally, half an hour after a paroxysm that has left him totally prostrated, cyanosed. It is very rare that he dies in a paroxysm.

This disease has then a very slow march; but, nevertheless, in some very rare cases, from the commencement of the alteration of the voice to the death, but twenty-four hours elapse. Ordinarily there is an interval of five, six, or seven days.

Meanwhile this cough, that had been hoarse and frequent, from the moment that the oppression begins to be felt, loses its hoarseness and its frequency. Ordinarily it is extinguished and hissing, very rarely hoarse, never frequent.

They speak of premonitory symptoms of this disease, but the fever, the swelling of the glands at the angle of the jaws, are not premonitory symptoms of the disease, but of the affection of the larynx.

Before these false membranes are formed, something is necessary,—a phlegmasia. When this phlegmasia commences it has no particular anatomical character. Do not then be surprised that you have the same characters in the cough and in the voice, as in a common catarrh. When the ligaments of the glottis are invaded, and after the phlegmasia has lasted for twenty-four hours, they are covered with false membranes. Then, in place of the ligaments being smooth and tense, they are covered by these false membranes. The voice and cough are necessarily extinguished, and the cough becomes rare. Why? Because, the ligaments being inflamed, the cold air irritated them; but when there is an interposition of false membrane, there is no irritation, and he coughs no more.

It is a disease slow in its evolution, but in some very rare cases it lasts but two or three days. The slower the disease in its evolution, the more certain is the operation of tracheotomy.

There are some cases, *exceedingly* uncommon, where the disease commences in the larynx, but for some physicians, they are not uncommon. Let me explain to you why. In the greater number of cases the physician is not called at the beginning of the affection of the pharynx, but one or two days after the affection of the larynx, and then he does not occupy himself with that. And, moreover, there are many physicians who cannot examine the throat of a child. A physician should know how—he must learn. The first thing to do is to hold firmly the head; when that is done the child begins to cry; pass quickly the spoon, and always a very large one, into the mouth, and push it into the pharynx; the child has a desire to vomit and then opens its mouth widely. You must push the spoon beyond the isthmus of the throat. When he closes firmly his teeth, hold his nose, pinch his lips, and when you loosen the lips and he opens his mouth, in order to breathe, pass quickly the spoon and force it back into the pharynx. Sometimes a child who had false membranes of the pharynx, has them no longer; but in these cases a swelling of the glands at the angle of the jaws remains.

Some events, I must tell you of, that occur and are of a nature to deceive you. After a paroxysm, a tube of false membrane is thrown off, and after that the child breathes as he does naturally. You cry victory, and in twenty-four hours he is dead. Do not be in a hurry to give a favorable prognosis,

for of a hundred cases, where false membranes are thus thrown off, ninety-nine die. After he has thrown them off three or four times, give a more favorable prognosis.

Let us now commence the history of diphtheritis. It is in a village, in a hamlet, that you can study an epidemic—in this Babylon you cannot. I have studied several epidemics, and have recognized the diphtheritis to be a specific disease, having peculiar characters in all the tissues.

When the diphtheritic virus is placed in contact with the skin, it causes a false membrane. Ammonia produces the same effect, and the measles can do it. You insert under the skin some syphilitic matter, and after some days you destroy it; nothing is produced—which proves that the syphilis is at first a local disease. Let us regard some affections more acute, that are at first local. If you insert under the skin some vaccine matter, and at the end of four days you destroy the pustule with some caustic, in eight days the child will take the vaccine. The diphtheritis holds the middle ground in regard to duration. The syphilis and the vaccine are at the two ends of the ladder. It is a long time local. When I was in S——, in 1828, to study the epidemic, I saw a child who had false membranes on an abraded surface. The skin had been rubbed off, while another child who had false membranes behind the ears, was drawing him on the barrow. The one had had the false membranes a month, the other a longer time. They called the disease then the *mal blanc*, and also chancre. I saw there one woman who had taken the disease by the genital organs, one girl who had taken it by a blister, another by a scratch on the breast.

It is a disease that is not a disease of the throat, and this is why the name croup is a bad name. You say that there is a scarlatinous anasarca, a syphilitic caries. You do not say that there is an exostosis, but you say that it is the syphilis with a manifestation in the bones. You must resume the names of the disease by synthesis. The term diphtheritis, signifies a disease with production of false membranes; but false membranes having a specific character, that can occupy all the parts of the body.

The diphtheritis remains fixed, if it occur upon the skin, as in a wound or in the vulva. It causes gangrene of the skin, but more especially is this the case when the vulva is attacked. It is rare to see gangrene when the gums are attacked, except sometimes in the hospitals.

There are several maladies that are accompanied with sore throat—for instance, the scarlatina. The sore throat, then, has great resemblance to the croup, for the tonsils are covered with a white coating, that you can well mistake for some false membranes; but that does not deceive you if you take into consideration the symptoms of the disease. In the scarlatina there is always fever, the tongue is swollen, and its papillæ become very manifest, giving it the appearance of a strawberry. In the scarlatina, the cervical glands are seized; the sore throat remains always limited to the tonsils, and does not attack the larynx and the trachea, as the croup does. In the diphtheritis the disease invariably advances, a white spot is seen on the tonsils, which increases as a drop of oil on paper.

The analysis of the false membranes of croup and those of other anginas, teaches us nothing; there is no difference. How, then, can we recognize the disease? You must see and study the premonitory symptoms and progress of the affection.

The acute laryngitis of pseudo croup is, without doubt, only a simple acute

laryngitis, with a greater degree of intensity. The latter attacks young children very suddenly—very rarely adults. It is a disease sudden and rapid in its progress, without premonitory symptoms. The child goes to bed well, and perhaps at midnight wakens up with suffocation and cough. The voice is veiled; there is fever. With the day all the symptoms disappear to return the night following; and, moreover, the cough in the daytime diminishes, becomes hoarse, and it is terminated by a simple catarrh. Then there have been no false membranes. This is a malady of the kind which succeeds very well with the homœopathics, for it invariably has a happy termination.

In the true croup, the child has a malaise of three or four days; he does not eat, or very little, for it makes him suffer. There is an alteration of the voice; there is croupal cough, fetid breath, and great difficulty of deglutition. The voice remains veiled. I have seen several cases where the patients have died by disease of the pharynx, the diphtheritis having taken that road, and having limited itself exclusively to that part, without affecting, in the smallest degree, the larynx.

Treatment.—Every time that you see appear on the mouth or throat, whitish concretions, the first indication is the local treatment. Epidemic diphtheritis is very mortal. I have seen fifteen die in a family of sixteen. The treatment by emetics, succeeds sometimes quite well, as a mechanical means, in order to displace the false membranes. When you are called to a patient with croup, examine the mouth, and you will almost always find that the disease is limited to the pharynx. The first thing to do, is to cauterize, thoroughly, the throat with nitrate of silver. Repeat the cauterization several times a day. You must not be satisfied, until you see the tonsils red. During the cauterizations with the nitrate of silver, you must also make insufflations of alum into the throat. This insufflation of alum should be repeated several times every day. You must blow hard, without pity, for the alum can do no harm, even in great quantity. The hydrochloric acid and nitrate of silver are the best caustics; never use the sulphuric acid. If you prefer the solution of nitrate of silver, put one part of it to five of water. When the disease has invaded the larynx, the treatment becomes much more difficult. In order to cauterize the larynx well, you must have a piece of whalebone, that you curve as you wish, by placing it in hot water. To its extremity, you fasten a bit of sponge, in order to dip it into the caustic solution, and you introduce it into the larynx itself. Make the child swallow, every hour, a teaspoonful of honey, with a little calomel and alum.

In 1822, M. Bretonneau tried to perform tracheotomy; all the patients died: but they would have died any how. In the month of August, 1825, he operated upon a young girl, and was successful. In this case, he made use of a larger canula. In 1830, I had a successful case. In 1834, I operated thirty-four times, and some of the cases got well. In fine, now-a-days, the operation has passed into medical usage.

Indications for this operation.—By what signs do you recognize its necessity? When the affection has commenced in the pharynx, and has extended slowly, in such cases the recovery is so rare, without tracheotomy, that you must operate. You must not wait for the last moment of asphyxia. When the cough is rare, generally extinguished; when the respiration is hissing; when there are orthopnea and the paroxysms of suffocation, then you must operate.

In what circumstances will the operation succeed the best? I believe that

there is a better chance of success when the operation is performed too soon. They ask you: "But if you do perform the operation, will my child get well?" I answer: "I do n't know, at all." "But what chance?" "One chance in five." Of one hundred cases there are twenty-five successful. I have not hesitated to perform the operation upon children dead—that is to say, no longer breathing, and they got well. This extreme degree of asphyxia is an inconvenience, but it is not a formal contra-indication.

When the disease has progressed slowly, hope much. When suddenly, do not hope. When there are false membranes on blistered surfaces—the nose, &c.—it rarely if ever succeeds. As to the age, it is from three to five years that you count the greatest number of cases. With adults, you are almost always unsuccessful. Under two years of age, the operation is quite useless; they die, almost always, of convulsions.

For the operation.—You must have a mattress, on a hard and flat table. No pillow, but a block of wood, under the neck. The instruments you require are, a common bistoury, a blunt-pointed one, some hooks, a dilator, and some canulas—the canula to be double, and its diameter as great as possible, consistently with the age of the patient; a proper number of aids; and you must have a *tallow* candle—never use one of wax—to perform an operation. The block must be placed under the first dorsal vertebra, and, perhaps, under the last cervical. The child has always the time to be operated upon slowly. trace a line with a burnt cork. Then open the skin, *perfectly at your ease*. You cut, carefully, between the muscles, until, at last, you perceive the thyroid gland, lying across the trachea. You cut the gland, and a small jet of arterial blood spouts; but do not trouble yourself with that. At last, you see the rings of the trachea at the bottom of the wound. Now, suppose a case in which the veins trouble you; if you cannot avoid them, cut them. The jet is violent, but soon ceases. Cut a millimetre each stroke, and use the sponge after each stroke. Ordinarily, you come upon the first ring, or the cricoid cartilage. You lay bare three rings; with the bistoury you make a small hole, the size of a pin's head; then you introduce the blunt-pointed bistoury, and finish. You make use of the dilator, and at the end of a minute you introduce the canula.

Dangers of the Operation.—What are the dangers that frighten the young physician, and that ought to do so? It is rather the operation of the physician, than of the surgeon; one of those little operations that a physician ought to perform. I repeat it again, that you have enough time to perform it. You must go to work, then, slowly. Of all the accidents, the gravest is the cutting of the carotid. It happens, in some very rare cases, that the carotid takes an anomalous course; but if you perform the operation as you should perform it, there is no danger even then. Twice in my life I have encountered anomalies, and I have drawn aside the vessel with a hook. The venous hæmorrhages, are they dangerous? They exhaust the child, and again they make the physician lose his presence of mind. When you have cut some veins, do not tie them—it is useless; but let me advise you to wait. Put your finger on the bottom part of the wound, sponge, and *wait*. Hold the child up; quiet him a little; *you have time enough*. You may attack the trachea too violently. It is not as large as the finger, and flexible. Be gentle, cut by very slight strokes, and then use the blunt-pointed bistoury; with it you can cut nothing else. If the child has an attack of syncope, wait. When the trachea is opened, the blood enters. This is by no means a great

inconvenience; open the trachea with the dilator; the child breathes well, and the flow of blood ceases. After the operation, you often see an attack of syncope; but it is nothing. The operation is over, and the canula is introduced. Immediately after, the canula becomes a source of irritation; but after some time that passes off. The febrile excitement, &c., diminish. This state lasts seven to twelve hours, and there are false membranes in the bronchial tubes. In some cases, in twenty-five or forty hours, there is a change. In these two circumstances there is not the same cause of death. If, at the end of a quarter of an hour, there is a respiration analogous to the sound of a saw, sawing a stone—the serratic respiration—death is certain. If the pulse, at the end of twenty-four hours, becomes extremely frequent, the children die. When the child is seized with convulsions, he dies. I speak here of the signs that I have observed myself. After twenty-four hours, the wound generally swells a great deal. It is a favorable sign; if it does not swell, the children die. There is a complication, an enormous swelling of the neck. You must ward it off, by cauterizing the edges of the wound, at the end of twelve hours. You must repeat the cauterization three or four times. If this enormous swelling takes place, the children invariably die.

By a provision of nature, the air enters the nose, and is warmed, and becomes moist before entering into the lungs. On the contrary, in these cases, the air enters cold and dry. A cravat should be placed around the neck of the child, extending over the chin and the upper part of the chest. Thus, the air entering the lungs will be warm and moist. These things are so important, that I do not hesitate to say that the great number of successful cases in three years ought to be attributed to two things—the cauterization and the cravat. I was obliged to make use of the cravat, because I had to inject water to moisten the hardened mucus, and that tormented the child, and irritated the bronchial tubes.

Presentation of false membranes.—At the moment of finishing the operation, a gush of mucus issues, and sometimes a false membrane, occasionally bifurcated; but in a certain number of cases, on introducing your dilator, you see a white membrane, and you hear a noise when he breathes—the noise of a valve. In these cases, you should not introduce the canula. Try to excite the cough, by the introduction of some drops of water, and when the false membrane is expelled, you introduce the canula, and wait for another. When you have waited for three or four days, without any other accident occurring, you can believe the child cured. After that time, the danger is not of extension into the bronchial tubes, but in the lungs; for when there is an extension into the bronchial tubes, they die in twelve or fifteen hours. The danger is of lobular pneumonia. These accidents that I have just described, can be called accidents inherent in the operation.

Other accidents.—Sometimes, at the end of sixteen days, the child can no longer swallow; everything enters into the trachea and comes out by the canula, or enters the lungs. For this, there is but one remedy—to make them swallow solid substances, as some long vermicelli. If you have the slightest sign of diphtheritic cachexia, give your patients coffee with quinine.

When must you think of removing the canula? At the end of four or five days, you remove the canula softly, in order to see. Here, in the hospital, after its removal, they leave the wound open, covered with a little *linge fenetre*, the child being left to breathe by both openings, until the wound is closed. I think I shall adopt this plan.—*Charleston Med. Jour. and Rev.*

Dislocation of the Os Humeri, upon the Dorsum Scapulæ, reduced after the expiration of five weeks. By PAUL F. EVE, M. D., Prof. of Surgery in the Nashville University.

Mrs. A. was thrown from a carriage while the horses were running away with it, and in the fall was struck by a wheel upon the left shoulder. This occurred just five weeks, lacking a day, before the dislocation, (the result of this accident,) was reduced. Owing to the great tumefaction which immediately ensued, the peculiar nature of the injury was not detected. When this had subsided, her physicians recognized a dislocation, which was so unusual that she was advised to visit Nashville. Drs. Kelly and Porter examined the case with me on the 21st of November, and we confirmed the opinion already expressed by our professional brethren who had seen it, that there was a dislocation backward of the humerus at the left shoulder joint. This was further strengthened the next day by Drs. Jennings and D. W. Yandell concurring with us.

The symptoms present were a loss of contour in the articulation affected, motion backward and upward of the left arm; flatness of the shoulder, great projection of the coracoid process, prominence of the acromion, hollow under it; a distinct tumor on the dorsum scapulæ behind, and a little below the glenoid cavity; the spinus process of this bone was obscured; the tumor on its dorsum was much nearer its posterior edge than was the head of the humerus on the sound side to the corresponding point of that side; the longitudinal axis of the os humeri was directed behind the glenoid cavity; the left fore-arm was pronated. The inferior extremity of the dislocated limb was longer than the one on the other side. There was no tumor in the axilla, and the elbow of the affected side could be made to approach the chest.

The patient did not now suffer much, but could only use the fore-arm to a limited extent, and the function of the arm was nearly lost.

The peculiar symptoms in the case were the altered direction of the long axis of the arm, the impossibility to carry the elbow backward, the projection of the coracoid process, and the head of the os humeri on the dorsum scapulæ.

Kindly assisted by the gentlemen above mentioned, while one maintained counter-extension by means of a folded sheet in the axilla, (the patient being seated in a chair,) two others extended the limb horizontally outward and forward, with directions to carry it suddenly backward, the head of the os humeri was pressed toward the glenoid cavity, when the reduction was easily effected, without resorting to chloroform or the pulleys. Upon the second trial, probably in three minutes, the bone slipped into its socket with distinct recognition to all present. In a week the patient returned home a distance of about thirty miles.

That the backward dislocation at the shoulder joint is a very rare one, a mere glance into the records of surgery will satisfactorily prove. Its *bibliography* does not extend beyond the present century.

Cases, no doubt, have occurred earlier than this period, but nearly all available in the profession have been derived from modern Surgeons.

In Prof. Pirrie's Principles and Practice of Surgery, 1852, on dislocations *backward*, at the shoulder-joint, he says that "of the head of the humerus

on the dorsum of the scapulæ is so rare an accident that Desault had never seen an instance of it; Baron Boyer met with it once in the living body; only two cases occurred at Guy's Hospital in thirty-eight years; in the same number of years Sir Astley Cooper met with two cases, and not more than four cases occurred during his whole professional career; and Mr. Lawrence, in his lectures delivered at St. Bartholomew's Hospital, in 1830, states that at that time he had never seen the humerus dislocated *backward*. After alluding to three or four other cases, and two examples he had met with, he concludes the paragraph by stating that there are on record a few others.

Mr. Bransby Cooper, in his lectures on the Principles and Practice of Surgery, published last year, writes that Boyer, speaking of this accident, says, "there is no well attested instance of dislocation of the humerus outward and backward." He states, however, that he himself had seen several cases, alluding clearly to some of his uncle's Astley; and reminding an American of a similar connecting of E. Holme to the celebrated John Hunter. But he, too, referring to his illustrious relative, remarks, that it was singular that two instances of so rare an accident should occur so closely together in the practice of one individual. In Sir Astley Cooper's great work on dislocations, we find these very cases detailed. In the other Cooper's writings, (Samuel,) he states distinctly a few cases have been recorded. Ferguson has seen one instance; Liston,* Miller and Skey mention none.

During the visit of my colleague, Dr. Buchanan, last year, to St. Bartholomew's Hospital, the first case of dislocation of the head of the humerus on the dorsum of the scapulæ was brought into that institution. Mr. Stanley said it was the first of the kind he had ever seen, and he had been connected with it thirty years. Mr. Lawrence stated that he had met with but one other case in fifty years practice.

In our own country, Dr. Physick, if we recollect right, met with two such dislocations. One was produced by the patient falling into a hatchway and striking the arm near the shoulder-joint upon its edge as the body descended into it. In this instance, the blow or force causing the luxation was applied directly opposite to that which resulted in a similar case here recorded. In my example, the wheel struck the scapulæ posteriorly, carrying it suddenly and forcibly forward, while the arm, fore-arm and hand having no such movement communicated to them, by their dead weight, overcame the slight comparative resistance of the atmosphere, ruptured the scapulo-humeral articulation, and were thrown backward.

In 1831, Dr. George Snyder, of Jackson, Tenn., communicated a case of backward dislocation at the shoulder-joint, to Prof. Gibson, of Philadelphia, in which not succeeding in effecting its reduction as recommended by Sir Astley Cooper, he afterward replaced it by the ordinary means applied to luxation of the os humeri in the axilla. Dr. S. made the very sensible remark, that producing a secondary, or consecutive displacement of the humerus downward, which some authors recommend, cannot facilitate the reduction. To be reduced from its second position, it must necessarily increase the rupture in the ligaments or soft parts, or describe a curve to enter again the glenoid cavity.

The case now recorded we believe is the first of the kind occurring in, or about Nashville.—*N. Y. Med. Gazette.*

* See Elements.

Treatment of Epilepsy. By HORACE GREEN, M. D., Prof. of the Theory and Practice of Medicine in the New York Medical College. (New York Medical Gazette.)

Viewing Epilepsy, when unconnected with organic lesion, to depend upon laryngismus, Dr. Green was induced to try the effects of cauterization of the larynx with the nitrate of silver, in a case which presented itself some time since.

The patient was thirty-seven years old, and had epileptic fits during a period of twenty-seven years. It was supposed, in its commencement, to depend on the presence of worms. At first the spasms occurred once in two weeks. In some instances he would escape for a month, or even two months, but ordinarily the attacks were about once a fortnight. This continued until he was nineteen years old, unrelieved, apparently, by any of the many remedies employed. At this period, the patient, at the advice of his physician, commenced the internal use of the nitrate of silver. Beginning with the eighth of a grain, twice a day, the amount was gradually increased to half a grain twice daily, and, subsequently, thrice daily, and so continued for a period of *two years*. The attacks of epilepsy diminished in frequency, until, at the end of two years from the time of commencing the use of the remedy, the disease ceased altogether. But long before the disease disappeared, the nitrate made its appearance on the skin, which has continued to deepen up to the present time through a period of fifteen years.

After two years the disease returned, gradually becoming more violent, and the attacks occurring at increasing shorter intervals. For the four years preceding September, 1853, they came on, not only daily, but generally several would occur every day, and not unfrequently as many as six or eight attacks would take place every twenty-four hours.

In reflecting on the case, in connection with the theory advanced by Marshall Hall, I recollected the fact, that spasm of the glottis occurring in my practice, in laryngeal disease, had, in many instances, yielded to the topical applications of nitrate of silver to the larynx; and I proposed to try the effects of *cauterization of the larynx*. With a sponge-probang I applied a strong solution of the nitrate of silver to the interior of the larynx. At this first application of the remedy to the larynx, I remarked the unusual insensibility of the parts to the local irritant; for although the ordinary caution, that of cauterizing the pharynx and opening the glottis, before passing the probang into the larynx, was not adopted, yet not the slightest cough or apparent irritation was induced, notwithstanding the sponge, charged with a strong solution of the salt, (45 grains to the ounce of water,) continued daily for five or six consecutive days; and from the first application the spasms ceased, and the patient *passed over a period of ten days without having a single epileptic attack*.

This intermission continued until the *tenth* day, when, after having been exposed to considerable excitement, he had a slight epileptic attack. Again were the applications of the nitrate renewed, and in order to produce as much effect as possible on the larynx, the strength of the solution was increased to eighty grains to the ounce of water, and the sponge-probang, nearly straight, being used, the applications were carried through the larynx and trachea to the bronchial bifurcation.

As in each instance before, the spasms ceased after continuing the application a few days, and passed the critical period; but as the patient approached

the termination of this second *decade*, he was directed to take ten grains of quinine daily, for several days. This being done, the applications were continued, and the patient passed over the critical period, to the *nineteenth* day, without experiencing the slightest attack. But near the close of the *twentieth*, his disease again returned. There was no difficulty, however, in arresting the spasms by the topical treatment as soon as this period was passed; that is, no difficulty occurred in arresting entirely the attacks of epilepsy, for a period of from ten to twenty days, during which interval the patient appeared in excellent health, in good spirits, and in the possession of his ordinary mental powers.

As the patient has in only one instance, as yet, employed freely the anti-periodic remedy, it has been determined to enter again on its use, with the hope that by its full administration the intermittent character of the disease may be controlled, and the patient restored to permanent health.

The last number of "Braithwaite's Retrospect" contains an article "on the relation of Laryngismus to Epilepsy," by Dr. E. Watson, of Glasgow, in which the same treatment was practiced as that pursued by Dr. Green, without either physician knowing of the application of the same treatment by the other.—*Charleston Med. Jour. and Rev.*

On the Employment of Iodide of Potassium as a Remedy for the Affections caused by Lead and Mercury. By M. MELSENS. (Translated from the paper in the "Annales de Chimie et de Physique," June, 1849.) By WILLIAM BUDD, M. D., Physician to the Bristol Royal Infirmary.

In the January No., 1853, of the British and Foreign Medico-Chirurgical Review, an article under the above caption appears, prefaced by some valuable remarks from the pen of Dr. William Budd, in which he sets forth the highly interesting character of the evidence by which the facts brought forward by M. Melsens are authenticated.

If, as M. Melsens alleges, and his numerous experiments seem conclusively to show, iodide of potassium is not only a safe, certain and radical cure for the common forms of saturnine and mercurial poisoning, but an equally sure preventive of the injurious effects so frequently produced by emanations from lead and mercury, the fact is one which cannot be made too widely known.

In all cases of mercurial and saturnine poisoning, he assumes that the metallic substance is in actual union with the affected parts, and is retained there in the form of some insoluble compound.

According to this view, the iodide of potassium, after its absorption into the blood, combines with the metallic poison, and forms with it a new and soluble salt—liberates the poison from its union with the injured part—dissolves it out, so to speak, from the damaged fiber, and sets it once more afloat in the circulation.

The new compound thus set at liberty, (under the form, it is presumed, of a double iodide of mercury and potassium,) he supposes to be eliminated through the kidneys almost as soon as formed, in combination with any excess of iodide of potassium that may happen to be present. So that both poison and remedy being cast out together, the cure may be said, in a peculiar sense, to be radical and complete.

Dr. Budd relates the case of a man who was admitted to the Bristol Royal Infirmary, about three years ago, for the cure of secondary syphilis, affecting his throat, joints and bones. He was also the subject of partial paralysis of the lower extremities, which had come on concurrently with the other symptoms. He had had chancre for some months before, for which, from his statement, he took mercury largely. From the time when he had chancre, (five months previously,) up to the day of his admission to the Infirmary, he had not taken any mercurial compound. The day after he came in he was put under treatment by iodide of potassium in pretty free doses. A few days afterward profuse salivation took place. The swollen face, the peculiar state of the tongue, the loosened teeth, the ulcerated mouth, and the characteristic fœtor, were all present; and yet this man, as was specially ascertained, had not taken a grain of mercury since his admission.

Under these circumstances, Dr. Budd concludes that a portion of the mercury, administered some months before, had become fixed in the body, and that the liberation of this mercury, under the solvent influence of the iodide of potassium, was the cause of the severe salivation under which the patient was now laboring. No other theory of the case, in fact, seems admissible. It may not be uninteresting to mention, that under the continuance of the iodide not only the syphilitic affection soon got better, but the paralysis, also, was almost entirely cured.

Since the occurrence of the case, Dr. Budd has seen many others of the same kind, in which mercurial ptyalism came on during the use of the iodide of potassium, in persons who had taken mercury some weeks or months before, but none within a more recent period.

M. Mulsens begins his memoir by expressing a hope that medical men will pardon him for having treated the question of the cure of the disorders produced by metallic compounds in a purely chemical point of view. "From the very outset of my researches," says he, "I have always looked at the subject in its simplest aspect. I have never, in fact, kept in view more than two definite things—the disease from the presence of poison in the system, and the cure by the expulsion of this poison out of the system."

It appears from the result of many experiments, that the iodide of potassium does not tend indifferently to every part of the body, but that marked differences occur in the quantity found in different organs. Thus blood from the heart and the liver do not appear to contain similar quantities of the iodides; iodides are found in the liver when they are absolutely wanting in the liquid of the gall bladder; the serum which bathes the intestinal canal, the pleura, etc., contains compounds of iodine, but the matters found in the intestine itself contained ordinarily none beyond the first half of the gut.

The principle of the treatment by the iodide of potassium, is to render soluble any metallic compounds which have become fixed in the living body, and to facilitate their elimination by uniting them with a substance most readily cast out of the system. In a chemical point of view, this plan of treatment differs essentially from any hitherto proposed. * * * * It has been generally supposed that mercury and lead are present in the body in these cases, in the form of soluble salts, (a supposition which, for lead especially, is very doubtful,) and many remedies founded on chemical views had for their object to form insoluble compounds with the poisonous substance, such compounds being thought to have no action on the living economy. The sulphate of lead, which, so far from being without action on the

living body, inevitably causes death within a limited period—a fact which leads to a distinction between slow and rapid poisoning.

According to the old chemical view, therefore, the object was to render insoluble a poison which was supposed to be previously soluble. The object of the treatment by the iodide of potassium, on the contrary, is to render soluble metallic compounds that have become fixed in the body, to the end that, being again in a state to be taken up by the blood, they may be cast out of the system.

The iodide may be given in doses of from 30 to 93 grains a day for an adult, without producing any unpleasant consequences. It traverses the system with great rapidity, being found in the urine a few minutes after its administration by the mouth. The kidneys are the principal outlet of the iodide of potassium. It is even with extreme difficulty that this salt can be made to pass through the bowels into the stools. Every mercurial compound which can possibly occur in the living economy, even metallic mercury itself, is soluble in iodide of potassium; the presence of the organic substances of the body does not hinder these reactions.

Let us examine now the facts, on the strength of which I would induce medical men to employ the iodide of potassium in chronic disorders produced by poisonous metallic compounds.

1. M. Boucher, house-painter. Saturnine pains in the spine, (Rachialgie,) incomplete paralysis of the arms. Lead colic treated with success.

From the 10th December to the 13th March, he took 3087 grains of the iodide of potassium, when he was perfectly cured.

2. A type-founder complained of termina, and of weakness of the legs. 2778 grains of the iodide entirely restored him.

3. A workman of forty or fifty years of age, who had worked in a white lead manufactory, had been in hospital more than six weeks. When first subjected to the iodide of potassium treatment he was weak and quite broken up.

It is almost impossible to form an idea of the rapidity of the amendment which ensued in this man. He grew better, so to speak, as you stood by. At the end of three weeks he left the hospital completely cured. The dose of the iodide had been raised pretty rapidly; when he left the hospital he was taking 77 grains a day.

4. A man about fifty years old had been subjected to different modes of treatment without benefit. When he began his treatment he could scarcely hold himself up; all his limbs were more or less palsied; he was pale and emaciated. After five or six weeks' treatment he was perfectly well, and left the hospital at his own request.

5. Ordinarily, dogs and cats die within a short time in the greater number of establishments where lead and its compounds are worked: rats and mice do not harbor there. All animals exhibit symptoms analogous to those which are observed in man, and are benefited by the same treatment.

Neither sulphuric acid, nor sulphates, can serve as antidotes to slow poisoning by the salts or compounds of lead: the sulphate of lead being itself a slow, but sure poison, capable of killing vigorous dogs in 20 or 30 days.

The evidence commonly adduced is not sufficient to prove that sulphuric acid is an antidote to slow lead poisoning, although sulphate of magnesia may be properly given in cases of poisoning by a soluble salt of lead, to act on the portion yet unabsorbed.

Sulphate of lead is, probably, less readily absorbed than other compounds, but that sulphuric acid will reach lead already deposited in the system is quite unproved. Moreover, sulphate of lead is itself a poison, as can be shown by experiments on dogs, some of which are detailed. One dog was paralyzed on the eleventh day, after 108 grains of the pure sulphate was given; he then refused food, and died epileptiform, emaciated, and in a state approaching scurvy, on the 22d. Another dog took 293 grains, and died on the 28th day, and with similar symptoms.

When a great excess of sulphate of lead is administered, the phenomena of lead poisoning are not in ratio to the quantity of poison administered. Experiments on dogs prove this. It appears that only a certain portion can be absorbed—the rest passes with the excrement.

If, to a dog that has been for some time under the poisonous influence of sulphate of lead, iodide of potassium be administered suddenly, in pretty large doses, death will ensue. If, on the contrary, the two drugs be given concurrently, the dog will suffer no harm. Iodide of potassium may be employed, therefore, as a prophylactic.

Experiments on dogs, very carefully made, prove this assertion, and show that the minute quantity of lead which traverses the body disappears, without causing much harm if it meets with a sufficient quantity of iodide of potassium to favor its expulsion. When, on the contrary, lead compounds accumulate in the body of the animal, death is inevitable. For even if the iodide be given, dangerous phenomena ensue more rapidly in consequence of the larger quantity of the poison which is thus rendered active and thrown into the current of the circulation.—*Ibid.*

Iodide of Potassium a remedy for the affections caused by Lead and Mercury.—We find in the January number of the London edition of the British and Foreign Medico-Chirurgical Review, a very valuable paper under the above head, first published by M. Melsens, in the *Annales de Chimie et de Physique*. By a series of experiments on man and dogs, conducted with great care, with the results accurately noted, M. Melsens has conclusively shown, that the iodide of potassium is not only a safe, certain and radical cure for the common forms of *saturnine* and *mercurial* poisoning, but an equally sure preventive of the injurious effects so frequently produced by emanations from lead and mercury. M. M. assumes, that in all cases of lead and mercurial poisoning the metallic substance is in actual union with the affected part or parts, and is there retained in the form of some *insoluble* compound. He believes that in these instances the iodide of potassium, after it reaches the circulation, combines with the metallic poison, and forms with it a new and *soluble* salt; liberates the poison from its union with the part or parts, dissolves it, and thus suffers it to again enter the circulation. Once in the circulating fluids, (in the form of a double iodide of mercury and potassium,) M. Melsens supposes these metals to be eliminated from the blood and tissues through the kidneys, nearly as soon as formed, in combination with any excess of the iodide that may chance to be present.

M. Melsens details a number of cases of mercurial poisoning, in which the iodide produced a rapid and permanent cure; and in some instances, the elimination of the mercury by the kidneys, was positively ascertained by reagents.

Most of us have had evidences of the anti-mercurial influence of the iodide of potassium upon the human subject; we have seen persons who had been the victims of mercurial poisoning, after taking the potash a short time, become profusely salivated. Does not this fact demonstrate that the potash liberates the mercury from the tissues, throws it into the circulation, and leaves it free to act on the glandular system?

M. Melsens has proven, by repeated experiment, that if to a dog that has been for some time under the poisonous influence of sulphate of lead, iodide of potassium be administered suddenly in pretty large doses, death will ensue. If, on the contrary, the two drugs be given concurrently, the dog will suffer no harm. Iodide of potassium may, therefore, be employed as a prophylactic in lead-poisoning.—*Condensed by Ed. N. O. Med. and Surg. Journal.*

Arsenical Confectionery.—At an inquest held last week at Ashford, upon two brothers killed by eating the painted ornaments of a twelfth cake, Professor Taylor, who analyzed the stomachs of the deceased children, said that he detected arsenic, which the system had completely absorbed. In his opinion, the yellow color of the ornaments that contained the poison was produced by orpiment, or sulphate of arsenic. The quantity that he discovered did not exceed a quarter of a grain. The green color of these ornaments was imparted by the arsenic of copper, which poisoned in small doses. During the two last years he had met with ten fatal cases from children eating these ornaments.—*Ibid.*

“An exchange paper states that sixty students in the medical department of the University of Louisville, Ky., are to be seen every Sabbath morning in the Sabbath School of the Chestnut St. Presbyterian Church, diligently engaged in the study of the Holy Scriptures, under the instruction of Drs. Yandell and Silliman, two of the University Professors.”

To our mind, the above simple announcement speaks volumes in favor of the young gentlemen referred to. We shall fear nothing for the science of medicine when it is in the hands of competent men, of established christian principles. As a rule, it is not they who are “carried about by every wind” of medical doctrine.—*New Jersey Med. Reporter.*

MR. EDITOR,—On the 21st of February last, I delivered a lady of a male child which weighed eighteen pounds and a fraction. The child had been dead about three days. The labor was instrumental. I merely mention this case for its novelty. The lady is very large and has borne several children.

Yours, J. M. MEYER, M. D.,

—*Transylvania Med. Journal.*

Dansville, Ky.

Cholera.—At last we are about to be rid of that scourge of the four quarters of the globe—the cholera; it has, we would fain believe, accomplished its work of death; fulfilled its mission, under Providence, and departed from among us. In no part of the United States does it prevail to any extent; it has spent its force—dried up the fountains of life in millions of subjects—swelled the bosom of the earth with its victims—made desolate the hearts and households of thousands of families, and gone forth to other climes—to other regions, to renew the contest for life or death with the sons and daughters of mortality. Cholera! its mere name sends a thrill of horror to the heart of the brave as well as the timid; to many it will convey sad recollections and heart-rending reminiscences, of friends, relatives and kindred, who were cut off from earth by the Great Destroyer. But why pursue the subject? Let the past be forgotten.

*Quis talia fando, * * * temperat à lachrymis?
* * * * animus meminisse horret, luctuque refugit!*

We trust we have seen the last of this scourge; let us hope that it has departed from us forever!—*New York Med. Gazette.*

Treatment of Dysentery.—Dr. J. B. Evans, of Frankfort, Ohio, in a communication for the North Western Med. and Surg. Journal, writes as follows:

After all is said, opium is the boon. I say *Opium*. It cannot be pressed too strongly. Sulphate of morphia seemed to be the best form. Without it the suffering of the patient was insupportable; he would die of actual pain. Providence is kind in giving us such a cordial for our sufferings. In many cases the amount of morphia used was very great. In my own case my attending friends gave me within a pittance of a dram in four weeks; the nervous irritability was so great that the strictest quiet, and the constantly soothing effect of opium was the only alternative. In these extremely nervous cases all treatment was useless, unless the strictest quiet was observed. Too many persons are allowed in the sick room. We should not let delicacy toward the friends of our patients interfere with our duty in this matter; for we are accountable for his life, so far as he is within the reach of means in our power.

Nourishment. The duration of an epidemic attack is usually beyond the starving point, and on this account, if no other, would arise the necessity for food. The starving plan of many of our professional brethren, I hope will be abandoned. Our diseases are becoming more grave and protracted, as the country becomes older and more densely inhabited. I have seen nothing more marked than the good effect derived from food in our scourge of dysentery. Nourishment was necessary from the commencement to the close of an attack. Even when the patient had no relish, and his feelings forbade him eating, he would be much better and suffer less after taking suitable nourishment. In many of the worst cases, the nervous system was completely unstrung, and having a constant griping, the horror produced is indescribable, except to the sufferer, who has felt it in his own person. The suffering was little short of a *fundamental* fire. In short, opium, quiet, and a judicious nourishment, were the great essentials in our epidemic. It was

necessary to persist in a well-selected and oft-repeated nourishment, without which the fabric would fall, and I am satisfied that by its neglect many died.

Treatment of Sprains by "Firing." By JAMES DICKINSON, Esq.

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

Sprained backs are cases which give the surgeon much trouble and annoyance, appearing in many instances to resist every remedy. Many cases have come under my notice, and finding that blisters, cupping, stimulating liniments, &c., failed, I tried "firing," and the results have been most successful; patients who for many weeks have evinced the greatest agony, have, after the first or second application, been perfectly cured. The plan to be adopted is as follows: Heat a metal button, the shank of which is fixed into a wooden handle, to such a temperature as can be borne with slight pain. Pass it lightly over the affected part, without inducing vesication, which is unnecessary. The pain produced is severe, but is transient. In long-standing cases two or three applications are required; in recent ones one will be found sufficient.*—*Prov. Med. and Surg. Journal.*

* This remedy sometimes affords relief in cephalalgia, and other forms of neuralgia.—
EDITOR BUFFALO MEDICAL JOURNAL.

EDITORIAL DEPARTMENT.

The Transactions of the American Medical Association, for 1852.—A copy of this work, which had not reached us at the time of our last issue, was duly forwarded to our address, but had not been delivered. It makes a volume of between nine and ten hundred pages. Its appearance has been greatly delayed owing to circumstances which the publishing committee could not control. The preface states that the greater portion of the causes retarding the publication might have been avoided had all the parties interested been as assiduous in securing its appearance as were the committee of publication.

The first seventy-two pages of the work are occupied by the minutes of the fifth annual meeting of the association.

The prize essay next follows, covering fifty-four pages.

Then follow Reports of Committees on Special Scientific Subjects. These reports consist of — 1. An essay "on the blending and conversion of types in fever." By Samuel Henry Dickson, M. D., of Charleston, S. C. This is an ingenious and interesting disquisition, but it may, perhaps, be questioned whether it will tend to settle doubtful questions of pathology pertaining to the subject of which it treats. 2. "On the action of water on lead pipes and the diseases proceeding from it." By Horatio Adams, M. D., of Waltham, Mass. We have read this report with much interest. The facts collected are of great practical consequence, and will serve to call the attention of the profession to the poisonous effects of lead, which are often not recognized, as such, in practice, and consequently persist in default of the appropriate management, and from the continued operation of the cause. We quote the last paragraph of the report: "In conclusion, your committee will only add that the settled conviction to which their labors, in preparing this report, have conducted them, is this: that it is never safe to use water drawn through lead pipes, or stored in the leaden cisterns for domestic purposes, and that any article of food or drink is dangerous to health which by any possibility can be impregnated with saturnine matter. It may possibly be done in some cases with impunity, but it is *impossible to predetermine* the cases of safety where so many are fraught with danger." 3. "On permanent cure of Reducible Hernia." By George Hayward, M. D., of Boston, Mass. This, also, is an interesting report. The subject is treated practically and ably, as would be expected from the character of the distinguished surgeon by whom it was prepared, as well as that of each of the gentlemen (Drs. J. Mason Warren and S. Parkman) associated on the committee. The following are the opinions at which the committee arrived after a careful examination of the subject, having obtained the views of several other eminent surgeons: "I. That there is no surgical operation at present known which can be relied on with confidence, to produce, in all instances, or even in a large proportion of cases, a radical cure of reducible hernia." "II. That they regard the operation of injection by the subcutaneous method as the safest and best. This will probably, in some cases, produce a permanent cure, and in many others will afford great relief." "III. That compression, when properly employed, is, in the present state of our knowledge, the most likely means of effecting a radical cure in the greatest number of cases." 4. "On Water; its topical uses in

Surgery." By Charles A. Pope, M. D., of St. Louis, Mo. We have not bestowed on this report, as yet, the careful attention which it claims. On a superficial examination it appears to us to be a timely contribution of judicious views on a topic of not a little practical interest.

The foregoing reports extend over one hundred and fifty-eight pages. They will be read, we doubt not, with satisfaction, and generally considered valuable contributions to our medical literature. For ourselves, however, we must confess that we should have preferred the reports on the progress of the several departments of medical science, after the plan originally adopted by the association. The change from these reports, to those on special subjects, seems to us to have been injudicious. This was the view taken by us at its first announcement, and the examination of the present volume of Transactions has served to strengthen this opinion. We sincerely hope the association will go back to the old system, which we are convinced would be more acceptable and useful, than the present, to the great majority of medical readers.

More than four hundred pages of the work are devoted to reports on Epidemics in different States. Of the value of these reports we are not prepared to speak. That they possess some, and it may be considerable value is possible, but by most medical readers they will never be read. We doubt the policy of filling so much of the volume of the Transactions with merely historical facts of this description. They are important we concede, but what concerns the profession is, not the details, for which few have any taste, but the conclusions to be deduced therefrom. The practical reader naturally inquires, on looking over so large a collection of facts, "well, what does it all amount to; what shall I gain if I take the pains to read all this matter?" A candid consideration of this question will, we fear, seldom lead to the resolution to enter on the task of a perusal.

The last two hundred and more pages are occupied by a "Report on Medical Botany." By A. Clapp, M. D., chairman of committee. This will, undoubtedly, be useful for reference, and is evidently the result of much labor on the part of the author.

As a whole, we continue to look on the annual volume of Transactions with pleasure and pride, and we earnestly hope that the evidence which is thus afforded of the utility of the association, will, for a long time to come, be reproduced with each returning year.

Transactions of the Kentucky State Medical Society, 1853.—The Transactions of the Kentucky State Medical Society at the meeting held at Louisville, in October, 1852, have recently been published in a neat volume of three hundred and thirty-three pages. First, is an interesting address by the president, Dr. W. L. Sutton. Next comes the report of the standing committee on vital statistics, Dr. W. S. Chipley, chairman. In illustration of the advantages of a system of registration of deaths, the statistics of mortality for the city of Lexington during the last twenty-one years are given. On comparison, these show a progressively increasing ratio of deaths, a fact which proves the existence of causes of disease to a greater extent than heretofore. The practical consideration is, that these causes are, if possible, to be discovered and obviated — but these would probably have remained unsuspected save for the statistics which developed the knowledge of their existence.

The importance of the subject with reference to sanitary improvements, is earnestly and ably presented in this report.

Next follows a report on medical ethics, by Drs. Ashbury Evans and Wm. R. Chew.

The report on obstetrics is the next paper. This is from the pen of Prof. Miller, and a considerable portion of it appeared in our last issue.

A committee on registration report an act which, mainly through the efforts of its members, has been passed by the legislature of the state.

The report on surgery, by Prof. Gross, is the most elaborate paper contained in the Transactions. We have already taken occasion to notice this production, having had the pleasure of being present when it was read by its accomplished author. It is an historical account of the surgery of Kentucky. It is a comprehensive and complete history. Prof. Gross spared no pains in collecting information from all available sources, and he has rendered a most valuable service to the profession of the state. It is a work for the future, as well as the present; and it will remain, and be referred to in all time, as authority for the historical details which it embraces. We have already referred to the biographical sketch of Dr. McDowell, the author of the operation of ovariectomy. This portion of the report will be read with great interest.

Succeeding the paper by Prof. Gross, is a report on indigenous botany, accompanied by a catalogue of the plants of Kentucky, from the pen of Dr. C. H. Spilman.

A brief report on epidemics follows, by Drs. Darby and D. P. Drake.

In conclusion, a committee, consisting of Drs. T. S. Bell, E. D. Foree, and

H. S. Chipley, report a plan for a case, book which, under the auspices of the society, has since been published.

Judging from such a beginning, this being the first volume of Transactions issued, the society has entered on a career of honor and usefulness.

Obituary Notice of the late William Power, M. D., of Baltimore.—The following eloquent tribute to the memory of the late Dr. Power, is contained in the April No. of the American Journal of the Medical Sciences. The initials show it to be from the pen of Dr. Alfred Stille, of Philadelphia. It is written in the chaste, elegant style for which Dr. S. is distinguished. But its merit, and the interest with which it will be read, involve something higher than merely literary excellence. It inspires admiration for the character of the deceased, whose example is so worthy of imitation, and, at the same time, a profound respect for the qualities of the heart which have prompted so touching an eulogy:

Died, in Baltimore, on the 15th of August, 1852, and in the 39th year of his age, William Power, M. D., late Professor of the Theory and Practice of Medicine in the University of Maryland.

When the young die, we grieve for the hopes that have faded like the flowers that wither on their bier; when the places of the old know them no more forever, we mourn that the tree which had long sheltered us, and been perchance a landmark to the wandering, has fallen from its station; but when death smites the vine that was still giving us nourishment with its fruit and refreshment with its shade, how sadly tender is the memory of its fragrant blossoms, how mournful and desolate seems the future of whose riches it had offered so liberal a pledge. Such was our departed friend. His nature, his culture, his earlier, and his maturer fruits, combined to make his life a blessing to those who partook of his gifts or dwelt within his shadow; but, even whilst supplying others most largely, a worm was at his own core; so one by one his branches withered and he fell away from among the living.

His life was a life of earnest study, and of noble ambition. He came into the world of professional action thoroughly furnished with all that that could make him a wise and accomplished physician. Just twenty years before his death he took the degree of A. B. at Yale College, and in the following year commenced in the office of Dr. Backler, a course of study, which, until the last year of his life, he never ceased zealously to pursue. In 1834, he entered, as a Resident Student, the Baltimore Almshouse Hospital, the institution of which he was destined to become, in later years, the most distinguished ornament. After graduating in medicine, and performing his hospital duties with ability and zeal, he determined to increase his knowledge by a course of study and observation in Europe, a step of which at that time there were but few examples in his native city.

The writer of this sketch first knew him in the midst of the scientific turmoil of which Paris was then the seat, and can testify to the ardent and steadfast enthusiasm which animated him; to his constant and unwearied attendance at the hospitals, the laborious records of his observations, his intelligent and discriminating discussions of the daily lessons which, in common with a crowd of American listeners, he was learning from the lips of Louis and Chomel, of Andrel and Rostan, of Grisolle and Barth. In the rigid school of the Medical Society of Observation, and of its illustrious president, the astute and penetrating genius of Dr. Power found a severe but salutary discipline, and he always referred to the evenings spent in its exercises as among the most delightful and profitable of his professional life. Its training developed his capacity for the systematic study of disease, and fitted him to become an example and a teacher to the medical students of his native city.

Enthusiastic in his pursuit of knowledge, he immediately, on his return home, sought and obtained anew the post of Resident Physician in the Almshouse Hospital, and only relinquished it when, after nine months' service, he was honored with the appointment of Visiting Physician to the same institution. As a consequence of the disgraceful custom of allowing political partizanship to interfere with the administration of public charities, Dr. Power was removed from his office on a change in the direction of the almshouse; but subsequently, in 1841, was reinstated. In this and in the following year he delivered two courses of lectures on the Physical Exploration of the Chest, at the Baltimore Infirmary, and under the auspices of the Faculty of the University. They were well attended, and gave him considerable reputation for attainments and skill in lecturing. His health now became precarious, and he felt obliged to quit his post, both as clinical teacher and lecturer, nor did he resume its duties until 1844. In the following year, he was appointed Professor of the Theory and Practice of Physic, in the University of Maryland.

The influence of Dr. Power over the young men, who, as students or as assistant physicians, followed his daily instructions, was unbounded. One and all caught somewhat of the enthusiasm which inspired their teacher and stayed him against the crushing weight of his disease. Before his time, we are assured, it was difficult to find candidates for the place of Resident Student in the Hospital; but owing to the interest with which he invested the duties of the office, and the reputation which the institution thereby acquired as a school of medicine, it was now only by an application of a year or more that candidates could secure their election. Even at this period, the physical sufferings of Dr. Power were such that he could hardly go up stairs without panting; yet his love of knowledge and his devotion to duty made him cling resolutely to his post, and point the way to others, although he did not hope long to tread in it himself. It is a sad, and at the same time a singular fact, that of the eight Resident Students who, in 1844-45, were his assistants, all but one preceded him to the grave! How painful a commentary is this on the deadly influences of our professional pursuits on life and health.

The career of Dr. Power, as professor, was a distinguished one. That it was singularly successful is proved by the large number of students whom he drew to the University; by the position he at once assumed among his professional brethren, and by the reputation he soon acquired as a clinical teacher in the College Infirmary. His thorough, systematic, and intelligent study of disease, his accurate diagnosis, and his mode of illustrating one case by another, interested his audience deeply, and accustomed them to observe

for themselves. In a word, his whole system of analytical study, as he had learned it from the ablest teachers of Europe, attracted and delighted all of the maturer and best-furnished minds among his hearers, and gradually raised up a class of ardent cultivators of medical science, which has already given to the world some of the fruits of his instruction, and some earnest of their own future attainments. As a lecturer, we are informed, he did not aim at originality, but rested satisfied if what he taught, from whatever source obtained, was *true*. The confidence he inspired was boundless; for did he make a wrong diagnosis, or misstate a fact, he was the first to admit it when discovered, and to confess his error. His lectures were less remarkable for excellence of style and originality of thought, than for their presenting a calm and unbiassed arrangement of the opinions of others, corrected and modified by his own experience. In this, as in all else, he held truth to be more valuable than ingenuity, and a positive result than the most elaborate and cunningly devised fiction. Yet, in dealing with the errors and follies of medical systems, he was careful not to forget the charity which is due to the weakness of human nature, nor the distinction, too often lost sight of, between the doctrines he combated and the persons who propounded or adopted them.

Dr. Power's health had never been robust. Emphysema of the lungs, and a consequent derangement of the heart's action, had, for a long time, made active exertion fatiguing to him. As long ago as 1837, a pedestrian tour in Switzerland cost him several paroxysms of severe dyspnoea and palpitation of the heart with hæmoptysis; but he did not become seriously alarmed about his situation until nearly six years afterward. In the autumn of 1843, he was obliged to seek relaxation in Cuba, whence he returned in the following spring, with his health materially improved. He continued better, in the main, although far from being vigorous, and from time to time he showed the effects of too close an application to his duties as hospital physician. His marriage in 1847, and his appointment in the University, which he had received two years before, exerted, doubtless, a very favorable influence upon his health, for he continued to perform with animation and a good degree of vigor, his professional duties, to draw around him a crowd of earnest and attached pupils, and in the social circle to win by his amenity, and interest by his varied talents, a large number of sincere and valuable friends. But the disease which had so long been baffled by skill and sedulous attention, at length gave signs that it was obtaining the mastery. During the winter of 1851-2, Dr. P. was unable to perform his public duties, and, in February of the latter year, resigned his professorship. In the month of May, the writer of this notice had the satisfaction of an interview with his old companion. He was wasted and wan indeed, for consumption had nearly worn away his earthly dwelling; but his soul shone all the clearer, and brighter, and purer for its freedom from the clogs of flesh. One might well have expected to hear the devotee of science, and the ambitious teacher, mourn over the unfinished career he had run, and resent the defeat of his wishes. But his spirit had been chastened. Contented with his share of worldly success, without a syllable of complaint at the disappointment of his hopes, he could still take interest in the science which he loved, and find in conversation upon its progress a consolation for his own withdrawal from a share in its rewards. Resigned and cheerful, he awaited with patience, yet with strong desire, the moment of his departure; for, while he longed for a release from suffering, he did not disdain the world in which he had been permitted to fit himself for a better.

For three months after this interview, he waxed feebler and feebler, but his soul was more and more brightened by a light from beyond the grave, and displayed to the last moment the calm hopefulness of one whose heart is fixed on Heaven.

As a student, Dr. Power was intelligent and industrious; as an instructor, he was faithful in duty, thorough in teaching, earnest in manner, clear in exposition, copious in illustration, convincing in argument; as a friend, he was sincere and constant; and, as a man, noble in his aims, pure in his means, a scorner of the false and hollow, and a lover of the real and the true. Thus, true to Heaven and to human duty, he lived respected even by those who did not share his love, and died as they should die who see beyond the grave an immortality of knowledge and of blessedness. A. S.

Death of Dr. Graves.—The death of Dr. Graves, of Dublin, will be deplored by not a few of the medical profession of this country, who are familiar with his contributions to medical knowledge. The writings of Dr. Graves have been most highly esteemed by American practitioners. From their practical character, and the internal evidence which they afford of sound judgment and sterling good sense, as well as learning and acuteness, they have exerted not a small degree of influence upon medical practice—an influence which, we believe, has generally, if not uniformly, been in the right direction.

In the following article, which we copy from the Dublin Medical Press, the editor of that able work evidently aims to present a candid notice of the professional character of the deceased. The praise bestowed is enhanced by the determination of the writer to avoid an indiscriminating and excessive strain of panegyric. The reader will not fail to observe that the superior professional ability and attainments of Dr. Graves did not secure him against injustice and neglect:

The late Dr. Graves.—It is not merely as a tribute of respect for the memory of Dr. Graves that we have to offer a few observations with regard to his character; the interests of our profession require that such a man should not pass from amongst us without special notice. Anxious as we are to give expression to our feelings of regret for his loss, we are doubly so in order to redeem his reputation from any stain it may have received from a fulsome eulogy furnished to a morning paper by one of the members of a fraternity which has brought discredit on our body by its unwarranted assumptions. The career of our lamented brother must not be depicted by the puffing parasite, its success must be explained by a simple statement of facts. Dr. Graves was the son of the late Dean Graves, a Fellow of Trinity College, well known to theologian, his mother having belonged to the highly

respectable family of Drought, in the King's County; he therefore enjoyed the advantage of intercourse during childhood with persons, of station and independence; an advantage which, in his case at least, contributed in no small degree to his subsequent position in society. He passed through college with more than the ordinary *eclat* which academic honors secure, and commenced his career as a physician some time about the year 1820: precise dates are of little importance in such a notice as this, and we therefore write from personal recollection. A residence at a German University for a sufficient time to become familiar with German habits and pursuits gave a tone to his mind which favored originality of thought and independence of action; and other causes operating in the same way at the same time, served to afford him a very fair start indeed in professional life. The Stethoscope led to the commencement of a new era in medicine, and in the hands of Dr. Wm. Stokes became a most formidable weapon in the great struggle for public patronage. Working in the same hospital the two young aspirants after medical celebrity seem to have cordially co-operated, and the result was a speedy advancement of both in reputation and practice. All kinds of extravagant nonsense have been from time to time written respecting their labors at this period; but praiseworthy as they were, we must not allow such false impressions to remain undisturbed. If we were to credit the parasite press, we should assume that, previous to this period, Dublin had never enjoyed the advantage of a physician of character. Perceval, Harvey, Cheyne, John Crampton, Barker, and a host of others, were consigned to oblivion, and no name was held up to public applause but the imitators of French and German practitioners. Even the junior members of our profession, to their subsequent cost, favored the delusion, and joined in the wild halloo against the sober conclusions of experience. It has been absurdly asserted, over and over again, by the same parasite press, that instruction by clinical lecture, was for the first time at this period introduced by Dr. Graves. Nothing could be conceived more contrary to truth; and giving the writers of such stuff ample credit for ignorance, we cannot help saying that we believe they knew and now know that the statement was a fiction. All the world knows that clinical instruction, in its most ample form, was fully employed in Dublin, Edinburgh, and London, long previous to this period. Be all this, however, as it may, out of this chaos rose the gifted object of our present notice; to our personal knowledge laughing heartily at much of what was in progress. His reputation became speedily established, and a lucrative practice followed, while his writings won for him the good opinion of the medical world. What need to enter on details. He succeeded to his heart's content, and he deserved to succeed. His loss to our profession, at the present moment, must be severely felt. A gentleman and scholar, an honest, out-spoken member of society, and a man capable of sustaining the high character he had earned, is not to be met with every day. The history of his life teaches a lesson, however; it tells us that success is not without its alloy. We believe that the subject of our remarks discovered, as others have, that the employers of medical men in this city are not what they once were. To use the political cant of the day, the "consumer" was supplanted in society by the "producer;" the affluent gentleman by the hard-fisted man of business, or the ostentatious *parvenu*. A man of Dr. Graves' bearing was not suited to such a change; the low cunning and base servility, so necessary now to secure success, he could not adopt, and the consequence we believe was, that

his practice for some years had not been so extensive as formerly. He had, however, "made hay while the sun shone," and any falling off in this way gave him no trouble. Another page in his history is also instructive, and let the neglected man of ability study it. Wealth, connection, and high character, will sometimes no more secure its reward than the humble, patient, pursuit of knowledge. If ever man had fairly won a Professorship, Dr. Graves had won that called the Regius Professorship of Physic in Trinity College, yet it was denied him. We cannot, perhaps, correctly say that "an eagle towering in his pride of place was by a mousing owl hawked at and killed," but it was something like it. Premiums, certificates, gold medals, and all other academic honors, were forgotten, as if to show the estimation in which they were held by those who dispensed them; and the first medical prize in the University was awarded to one who, in this respect at least, had no claim whatever to any such distinction. Our limits do not permit us to offer to our readers more than this very inadequate sketch of the life of this distinguished man, but the majority of them can supply from their own sources of information much of what we have been obliged to pass over.

Introductory and other Addresses. — So large a number of publications falling under the above class have accumulated on our table, that we shall be obliged to content ourselves with but little more than their enumeration with their respective titles, and a general acknowledgment of the courtesy of their authors in favoring us with copies.

1. *A Discourse on the Times, Character and Writings of Hippocrates.* By ELISHA BARTLETT, M. D., Prof. of Materia Medica and Therapeutics in the College of Physicians and Surgeons, N.Y. Published by the class.

This is an elaborate discourse, and as a rhetorical performance highly finished.

2. *Inaugural Address of Worthington Hooker, M. D., as Professor of the Theory and Practice of Medicine in Yale College.*

The subject of this address is "The present mental attitude and tendencies of the medical profession."

3. *A Lecture on the Treatment of Disease.* By JACOB BIGELOW, M. D., Prof. of Materia Medica and Clinical Medicine in Harvard University.

A sound, sensible discourse, which every practitioner may read with advantage.

4. *Certainty in Medicine.* By JAMES BRYAN, M. D., Prof. of Surgery.

A well written address.

5. *Sketches of Military Surgery: an Introductory Discourse delivered to the Kentucky School of Medicine.* By Prof. J. B. FLINT.

In this discourse Prof. Flint proposes a new method of making medical appointments for the army and navy. The method consists in the delegation of power to the faculty of every medical school of good standing, in the United States, annually to confer upon one, two, three or more, as need may be, of the most meritorious and accomplished of their graduates, certificates of qualification for army and navy preferment, the delivery of these certificates to make a part of the public exercises of the commencement. He urges the merits of this plan with cogent arguments.

6. *The incentives, means and rewards of Study: an Introductory Address, delivered at the opening of the 23d Annual Course of Lectures in the Med. College of Ohio.* By L. M. LAWSON, M. D.

A learned and philosophical discourse.

7. *An Introductory Lecture read before the class in Starling Medical College.* By ROBERT H. PADDOCK, M. D., Prof. of Anatomy and Physiology.

8. *Professor Baché's Introductory Lecture, delivered in Jefferson Medical College of Philadelphia.*

9. *Professor Baché's Valedictory Address to the Graduates of Jefferson Medical College.*

10. *Lecture Introductory to the Second Course in the Medical Department of the University of Nashville.* By W. K. BOWLING, M. D., Prof. of the Institutes and Practice of Medicine.

In this discourse the author, very naturally, discusses the prospects of the institution recently established at Nashville, and augurs future prosperity from the success of the two first sessions. Believing that honorable competition is not of disadvantage in medical education, more than in other departments of human effort, we say God speed! The field is open and broad enough. *Ferat qui meruit palmam.*

11. *Biographical Sketch of J. Kearny Rogers, M. D.* By EDWARD DELA-FIELD, M. D.

In this excellent biographical sketch we find a passage which provokes criticism:

Prior to describing the operation for tying the subclavian as performed by Dr. Rogers, the author says: "He did at last arrive at one great object of every surgeon's ambition, the successful performance of a great and difficult operation which had been attempted before by the greatest masters of his art, but without success." An account of the tying the subclavian follows, which the writer characterizes as remarkable and successful, albeit the patient died the 15th day after the operation, of arterial hæmorrhage! We claim that the sentiment conveyed in the foregoing quotation is a libel on the true surgeon, and we are at a loss to perceive how the shortening of a poor patient's life by tying the subclavian is to be viewed as the "crowning act of the surgical life" of the operator.

12. *History of the Medical Department of the University of Louisville; an Introductory Lecture.* By LUNSFORD P. YANDELL, M. D., Professor of Physiology and Pathological Anatomy.

As the title purports, this is a historical sketch of the University of Louisville by one connected with the institution from the beginning, and to whose efforts it is much indebted for its past and present prosperity. The address is written in the easy, graceful style for which the author is distinguished.

13. *Introductory Address delivered by Professor J. C. Hughes, before the class of medical students at the College of Physicians and Surgeons of the Iowa University.*

This address contains many interesting facts culled from the history of medical science.

Foundation of a Museum and Prizes, by Orfila.—We copy the following account of the foundation of the museum and prizes by the eminent and now venerable Orfila,* in the Dublin Medical Press. It will be read with interest, and may, perchance, suggest to some of the wealthy members of the profession in this country, the advantage which would accrue to American Medicine, as well as the honorable perpetuation of their names by similar acts of munificence.

"M. Orfila has just read the following letter before the Academy of Paris :
"I do not wait, as is generally the rule, till death has removed me from

* Since this paragraph was penned the papers have announced the death of this distinguished member of the profession.

among you, to assign the sum of £4800 to different public establishments. I have two reasons for acting thus: first, because it is of some importance that the institutions to which I refer should as soon as possible reap the benefit of the donations which I am offering; and secondly, because I thought my presence would be of some use to overcome any difficulties which might arise during the carrying out of my plan; or perhaps in order to modify the latter, if the necessity of doing so were clearly demonstrated. I shall not attempt to enter into the reasons which have induced me to give the preference to certain institutions over others; it will be sufficient for me to state, that by giving £2400 to government for the completion of the museum which bears my name it is my intention to endow France with a scientific collection which will be unparalleled, and also to afford students in medicine a new proof of the sympathy and good-will with which I have always regarded them. I am also anxious to show them how grateful I feel for the very flattering attention they invariably have given to my lectures for the last thirty-four years. I am anxious for this reason, that no misapprehension should exist regarding my motives, and have directed the following inscription to be placed over the principal entrance to the museum:—

“*To Students in Medicine.*—I founded this museum, in 1845, for promoting medical studies, and solely to be useful to yourselves.—ORFILA.”

“I have thought it right to found a small annuity in favor of the keeper, who has always rightly attended to his duties. I also institute two prizes, one to be given by the Academy of Medicine (£80), the other by the School of Pharmacy, (£40), on subjects which have fixed my attention all through life. I have thus no other ambition but that of serving a science to which I have always remained faithful, and without allowing myself to be led astray by politics. I give to the Preparatory Schools of Bordeaux and Angers, £40 to the former, and £88 to the latter, to show how I approve of this kind of schools, which were organized upon a proposal of mine. To the Benevolent Medical Association of the Department of Seine, I give £16 a year, in proof of the high estimation in which I hold this society, which I am proud of having founded in 1833.”

M. Orfila mentioned various other acts of kindness and benevolence of smaller importance, and received at the end of his discourse the hearty and unanimous applause of the members present. The Academy have decided that thanks should be tendered to M. Orfila by a deputation; and the medical press are calling a meeting for the same purpose.”

Dr. Bennet Dowler.—The friends of this distinguished experimentalist and writer, will be gratified to learn of a flattering distinction which his labors have procured. The Royal Society of Northern Antiquarians, of Copenhagen, of which the King of Denmark is president, have signified their intention of electing him a fellow. We congratulate him most cordially on this new honor.

The Obstetric Catechism; containing two thousand three hundred and forty-seven questions and answers on Obstetrics proper. By JOSEPH WARRINGTON, M. D. One hundred and fifty illustrations. Philadelphia: Edmond Barrington and Geo. D. Haswell. 1853. 12mo., pp. 445.

The author of this work disclaims for it any title to the character of a *text* book. He calls it a *test* book. "It is," he says, "your catechist or inquisitor, not to tell you any thing new, but enable you to determine what you do, or what you do not already know." Used for this end such books are undoubtedly convenient and useful, and we have no doubt that this work is well adapted to fulfill the object for which it was designed. The reader's attention cannot but be arrested by the title page. There are 2347 interrogatories to be answered before the medical student is master of all the important facts pertaining to obstetrics! The practitioner may gauge his knowledge of this branch of medicine by ascertaining how many of these questions he is already prepared to answer; and in prosecuting this inquiry he may possibly discover that on some points his stock of information is defective. If not, the satisfaction of thus proving the amplitude of his knowledge will sufficiently repay the labor of perusal.

As respects anæsthesia in labor, Dr. W. is a conservatist, conforming to the sentiment on the subject which, it is generally understood, prevails at Philadelphia. The flippant manner in which he disposes of the matter in the preface, is unworthy of the author. We quote: "I have thoughtfully refrained from alluding to the subject of anæsthesia in obstetric practice, having not much to say from my own experience in its use, and after stating my strong objection to making women transiently *drunk*, whenever any substitute may be successfully available, I have still preferred not to attempt in the text to prejudice the mind of the student against any preceptorial or professional bias he may have received."

Spiritual Rappers.— We meet frequently with notices of insanity produced by belief in spiritual rapping. In the April No. of the New York Medical Times, it is stated that during the past year eighteen persons were admitted into the Indiana Lunatic Asylum, whose insanity was ascribed to this belief. If the delusion continue to prevail, the subject may perhaps assume sufficient importance, by and by, not to be beneath the notice of even so dignified a body as the American Medical Association!

The American Psychological Journal, devoted chiefly to the elucidation of Mental Pathology and the Medical Jurisprudence of Insanity. Conducted by EDWARD MEAD, M. D., Physician to the Cincinnati Retreat for the Insane, etc., etc.

This is the title of a new periodical, the first No. of which has lately appeared, to be issued at Cincinnati, Ohio. It is a bi-monthly publication, of thirty-two pages, the subscription price being but one dollar *per annum*. The editor, Dr. Mead, is a working man, and an excellent writer. The work will be worth more than the subscription price to all who feel an interest in the subjects to which it will be devoted.

The Lights and Shadows of Medical Life.—We fully concur with our correspondent below, as respects the propriety of contemplating the joys, as well as sorrows pertaining to the life of the medical practitioner. And, in our opinion, it is the fault of the physician himself if the former do not preponderate. The grateful appreciation of professional services, the social relations founded thereon, and the inward satisfaction accompanying the successful management of diseases, more than overbalance the peculiar annoyances incident to medical life, not a few of which proceed from the jealousies and malignity of the small proportion of unworthy members who disgrace the medical profession:

April 19th, 1853.

MR. EDITOR,—Perhaps you have read some of the "Sketches from the every-day experience of a Young Doctor." I have, and also the life-like narrative of Dr. G. Saultz; and both brought reminiscences of so gloomy a character, that I was glad to turn to the other side of the picture where Doctors are in luck, to prevent a fit of the blues. Do not the following, which have come to my lot, furnish a spice to the life of a physician so agreeable as to remove that disgust which frequent abuse often develops.

Had you not better mix in something of the kind to keep the readers of the Journal from pouting.

Dr. ———,—Please to accept the accompanying trifle, (a valuable diamond brooch,) as a token of that gratitude which your kindness and fidelity in my recent sickness, has inspired.

This pin was long worn by a most valued relative, and since his decease I have found no friend whom I would so well like to see wear it.

Respectfully yours, &c.,

M. W.

Dr. ———,—Dear Sir: Please accept this book, (a ten dollar Bible,) as

a token of friendship from one who feels herself very much indebted for your kind services while in affliction.

L. H.
BIEN HENREUX, M. D.

Medical College of Buffalo. — Annual Commencement. — The commencement exercises of this institution took place on the 27th ultimo, at American Hall. The degree of Doctor in Medicine, was conferred upon eighteen candidates in course; and the honorary degree on Dr. Comfort Hamilton, of Ashtabula county, Ohio.

Prior to conferring the degrees, the President of the Council of the University, Hon. George W. Clinton, delivered an address replete with scholarship, and pronounced in the felicitous style which characterizes all the public efforts of this gentleman.

He was followed by Prof. Hamilton, to whom was assigned the customary charge to the graduates. This duty was performed by Prof. H. in an admirable manner.

Mr. Clinton's address, we are informed, will appear in one of the daily papers of this city.* We hope, also, that Dr. Hamilton's charge will be published. It will be read with interest, albeit medical addresses are so abundant, that, to borrow a mercantile phrase, the market is somewhat overstocked.

A number of the curators of the medical department of the University were present at the examination of the candidates for graduation, and expressed themselves highly gratified with the evidences of proficiency exhibited on that occasion.

The following is a list of the graduates:

Charles Ap Arthur Bowen, Hamilton, C. W.
Alfred Payson Crafts, Sodus, N. Y.
Joseph Rowe Smith, A. M., Buffalo, N. Y.
Albert Chaddock, Pavilion Centre, N. Y.
John Augustus Yeyte, Buffalo, N. Y.
Edwin Evans Kendall, Phillipston, Mass.
Thomas David Powell, Eden, N. Y.
Edwin Amsden, Gainsville, N. Y.
David Burnet McKay, Eddyville, N. Y.
George Edenmuller, Germany.
John Hart, Oswego, N. Y.

* It is contained in the Buffalo Daily Commercial Advertiser, April 28.

James Burley Rounds, Burford, C. W.
 Owen Prouty Marsh, Springville, N. Y.
 George E. Smith, Brighton, Michigan.
 Albert Levant Cotes, Batavia, N. Y.
 James Henry Moore, Davenport, N. Y.
 Charles Lemon Dayton, Black Rock, N. Y.
 Wm. Picket Bowers, Sinclairville, N. Y.

The medical faculty of the University, through the President of the Council, made public announcement, that, of the several graduates, Dr. Joseph B. Smith deserved especial distinction for a meritorious thesis, and a high order of attainment in the several branches of medical science.

Prof. Hamilton's new views on Provisional Callus.—Prof. Hamilton's paper on the provisional callus has excited much attention. His observations, with those of Mr. Paget, will probably effect a radical change in the current doctrine relative to the union of fractured bones. So in every branch of medical science, old opinions based on authority alone are being subjected to the scrutiny of rigorous analytical investigation, which, while it confirms many truths, also discovers a multitude of errors.

We copy, with pleasure, the following extract from a review of Prof. Hamilton's memoir, by the editor of the New Hampshire Journal of Medicine:

“We cannot deny ourself the pleasure of expressing our warm admiration of the manner in which Prof. Hamilton has asserted his claim to equal originality with Prof. Paget, in proving the incorrectness of the notions usually entertained on this subject. With a manly straight-forwardness he states the facts concerning the previous publication on the part of Mr. Paget, and while he does not attempt to take a leaf from his equal's laurels, asserts on his own word, which is sufficient, that he is entitled to the same honor, and claims his reward. He accompanies his article by a large portion of Paget's lecture, that that gentleman's views may be distinctly understood. We confess we do not know which to admire most, the originality of mind displayed in making these investigations, or the gentlemanly manliness with which the apparent rival is treated.”

Delinquent Subscribers.—The editor of the New Jersey Med. Reporter, in commenting on an advertisement of a “Physo-Magnetic doctor” proposing to prescribe for distant patients, after examining a *lock of their hair*, remarks that a similar testimony of the existence of some of his subscribers would afford him some gratification!

Proceedings of the Medical Association of the State of Alabama, at its Sixth Annual Meeting, held in the city of Selma. 1852.

We presume that our readers are not indifferent to the efforts made in different parts of our country to advance medical knowledge, and improve the character of the profession. They are doubtless glad to be informed of what others are doing for these ends, as a mere matter of medical intelligence. But the advantage, it may be hoped, will extend farther than this. Such information is well calculated to afford encouragement, and develop a spirit of emulation in undertaking, or carrying out more zealously enterprises of a similar character. In proportion as these effects follow, good will be accomplished.

These reflections have arisen on taking up for notice the work with the above title. It is a volume of one hundred and sixty-eight pages. We have before had occasion to notice former volumes of Transactions emanating from this society. The present publication shows that the society continues to flourish. As a whole the papers contained in the present volume are highly creditable. We have only space to give the table of contents:

“Report of the Diseases of Mobile, by W. H. Anderson, M. D., and G. A. Ketchum, M. D.; Report on Indigenous Botany of Clarke, by A. Denny; Report on the Diseases of Centreville and vicinity, by J. W. Crawford, M.D.; Report on Surgery, by L. H. Anderson, M. D.; Cases reported by J. T. Gee, M. D., Burnsville; Report on the Article Gelsemium Sempervirens, by J. C. Batchelor, M. D., of Dallas county; Report on Diseases of Cahaba, by J. A. English, M. D.; Report on Indigenous Botany of Perry, by F. A. Bates, M. D.; Changeability of Disease, by W. Taylor, M. D., of Talladega; Case of Dislocation of Spleen, by J. C. Batchelor, M. D.; Paper on the Unity of Disease, by H. Backus, M. D., Selma; Prize Essay on the Summer and Autumnal Fevers of South Alabama, to which is appended, some remarks on the Diagnosis and Treatment of Typhoid Fever, by L. H. Anderson, M. D., Sumter county; Valedictory Address, by F. A. Bates, M. D., Marion; Reports on the Number and Character, &c., of Practitioners of Medicine, by J. P. Barnes, M. D., Mobile, A. Denny, M. D., Clarke, F. E. Gordon, M. D., Marion, and R. H. Erwin, M. D., Camden; Names of Members of the Association.”

What is Man?—We find in one of our exchanges the following answer to this question: “Chemically speaking, a man is forty-five pounds of carbon and nitrogen, diffused through five and a half pails full of water!” This is more in the spirit of modern science than the ancient definition of Plato, or the modern description by Carlyle.

A Clinical Phrasebook, in English and German, containing the usual questions and answers employed in examining and prescribing for patients; questions in asking for and buying medicines, etc., etc., etc. By MONTGOMERY JOHNS, M. D. Philadelphia: Lindsay & Blakiston. 1853. 16mo., pp. 308.

This little work is designed to aid physicians and surgeons in hospitals, almshouses, and private practice; also druggists and pharmacutists in dispensing their prescriptions. The necessity for it grows out of the large proportion of Germans in the population of many parts of our country. It will prove very convenient and useful to those who are so situated as to have occasion to hold intercourse with Germans, and who are imperfectly acquainted with the German language.

The Virginia Medical and Surgical Journal. Edited by GEORGE A. OTIS, M. D., and HOWELL L. THOMAS, M. D.

This is the title of a new medical journal to be issued at Richmond, Va., the first No. of which appeared in April. The typographical appearance of the work is excellent, and the editors have entered on their duties apparently with zeal and confidence. As another medical journal, the *Stethoscope*, is already established at Richmond, we fear the speedy success of the work may not meet the sanguine expectations of the authors of this new enterprise. Judging from the first No., however, this will not be the case for lack of ability on the part of the editors.

Anatomical Bill. — We regret to learn that this bill, after having passed both branches of the legislature, was reconsidered, and finally killed in the senate. A senator who voted for it was induced to move a reconsideration, and to offer an amendment, which would cause it to be returned to the assembly, rendering its passage, owing to the late period in the session, impossible. Under these circumstances, the senate refused to adopt the amendment, and the senator referred to voted against the bill. The name of the senator has escaped us at the time we are writing, or we would give him the benefit of being known to our readers as the man who is responsible for the loss of the bill. The name shall appear in our columns hereafter.

Vicarious examination for a Medical Degree.— A correspondent of the Western Lancet, writing from Philadelphia, mentions a method of securing a medical degree as singular as it is novel. He says: "A pretty good joke occurred the other day in the faculty of one of our most crowded schools. One of her weaker candidates obtained a student of a neighboring college to *stand the examination* for him, who, it is said, actually run through the duty without detection, until another boy told on him. I have heard of the facility of getting through *in a crowd*; but this is the last instance. No such game, however, need be played here, for *all* may be satisfied with very little trouble."

New York University.— Dr. Meredith Clymer has resigned the chair of the Theory and Practice of Medicine in the University of New York, and Dr. John A. Swett has been elected his successor. The election of Dr. Swett to this chair will be highly acceptable to the profession. His written productions, and clinical lectures at the New York Hospital, are an earnest of his success in the discharge of the duties of his present responsible position.

Appointment.— Dr. S. B. Hunt, formerly of Mendon, Monroe Co., N. Y., has been appointed Demonstrator of Anatomy, in the Medical Department of the University of Buffalo, in place of Dr. Hugh B. Vandeventer, resigned. It is due to Dr. Vandeventer to state that his services have been most acceptable to all parties. He leaves the institution with the view of seeking a residence in a southern climate. The warmest wishes of those with whom he has been associated, will go with him. His successor, Dr. Hunt, is an accomplished medical scholar, and we doubt not will prove a valuable acquisition to the institution.

Of the Use of Chloroform in Midwifery. By GEO. N. BURWELL, M. D., one of the Physicians of the Buffalo Hospital.

The paper on the use of Chloroform in Midwifery, read by Dr. Burwell at the annual meeting of the State Medical Society, has been issued in a pamphlet form, making thirty-eight octavo pages. It gives the results in

one hundred and eighty cases, with the practical rules for its administration deduced therefrom, and is a valuable contribution to our knowledge bearing on the vexed question of the propriety of anæsthesia in labor.

Meteorological Phenomenon. — Many of our readers may have met with newspaper notices of a curious meteorological phenomenon lately observed at Louisville, Ky. The Western Medical and Surgical Journal gives the following account of it:

“During a storm of wind and rain, on the night of the 25th of March, a yellow, impalpable powder fell in this city and neighborhood, in such quantities as in some places to cover the ground. Being light, it floated upon the water, and formed a thick film along the sides of the gutters. The phenomenon gains interest from the circumstance that a similar precipitate occurred in Tennessee ten years ago, (March, 1843,) after a cold, backward spring, much like the present. Then, as recently, it was accompanied by a rain which succeeded a day of high southerly winds. The powder had very much the appearance of *flowers of sulphur*. It was combustible, and burnt with the odor of vegetable matter. The explanation generally given of it is, that it is the pollen of flowers wafted into our region by the winds from the South.”

Buffalo Hospital of the Sisters of Charity. — This hospital has been enlarged by the addition of a wing, which will increase its capacity by nearly one-third. The new wards are now ready for occupancy. Vacant lots in front and rear have been purchased, securing for the institution grounds sufficiently ample for quiet, pure air, and any future additions which may be required. The bounty of the state has enabled the trustees to make these important improvements, placing the institution on a firmer basis, and augmenting its charitable resources.

Close of Volume. — Our readers will take notice, that with the present No. closes the eighth volume of the Buffalo Medical Journal. The index to the volume accompanies this No.

We reserve an announcement of some new arrangements for the next volume until the issue for June.

We shall commence, in our next No. the publication of a valuable monograph on diaphragmatic hernia, by H. I. Bowditch, M. D., of Boston, Mass.

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