
Dr. Dugas—In the July No. of your Journal is found the following extract:

"We have had an eruptive fever here for a month or two, and I understand you have had the same, or a similar disease, in Augusta. There is diversity of opinion among the 'faculty' here, as to its character. I think the majority of the cases are Roseola, and a few cases of Scarlatina; the other physicians say, it is all Scarlatina. If you have had the same epidemic in Augusta that we have had, I, for one, would like to have your views on it through the Journal."

The disease here spoken of, or a similar one, prevails to a considerable extent in this section. The majority of the cases were Roseola—none Scarlatina. I have notes of the cases attended, which were taken at the bed-side. The first case within my neighborhood I did not see, but I noted its details from the description of it by the patient, who is quite an intelligent lady.

Case 1. Miss M. W., æt. 22—plethoric—had visited a family some distance from her residence, and remained some four or five days. About a week after her return, she felt unwell. Several children of the family visited had what was supposed ed to be scarlatina. She expected to be similarly affected.

Symptoms.—Fever, sore throat, head-ache, cough; tongue,
gums and teeth were sore; some swelling of the throat; red spots first appeared upon the face and spread over the entire surface, irregular in form, producing a burning, smarting sensation. No sloughing, no scales, no roughness of the skin. The efflorescence continued nearly a week. Felt relieved after the disappearance of the spots. She did not take anything, and got well in a week.

Such are the symptoms as related by the patient, and I do not think that any intelligent physician would select this as a case of scarlatina. It is to my mind a case of roseola aestiva.

Case 2. Miss E. A., æt. 10: Sanguine temperament; fine figure, robust, and of good constitution; complains of giddiness and head-ache; considerable thirst; distressed with heat, and aching of her limbs; tongue slightly furred; costive; restless. No efflorescence discovered on first visit. Considering it, however, a case of roseola, I gave an emetic of ipecacuanha. On the second visit, found the rose-colored efflorescence over the entire surface, and variously figured—some very small, others as large as a half dollar. Gave no medicine; she recovered in four or five days.

This case differs slightly from the first, though I think the disease the same.

Case 3. Miss D. P., æt. 28: Nervous temperament; not very healthy. Complains of constant and acute pain in the head; aching of the eyes; general weakness; loss of appetite; flushing of heat; the pulse full; head hot; bowels costive; periods irregular—no efflorescence discovered. Considering the attack due to costiveness and irregular menstruation, I bled freely; applied a cataplasm to the spine, gave a cathartic, ordered mustard foot-bath, and hip-bath at night. On second visit, discovered spots about the face and neck, arms and hands. The efflorescence was not general and soon disappeared, leaving the patient in more distress than at first. Every attempt failed to reproduce the eruption. The headache continues in despite of every remedy.

Of the nature of this case I am not satisfied. That it was partly due to the arrest of the catamenia cannot be denied; but that this arrest was the cause of the efflorescence is another question. I leave this to the savans.
Case 4. Miss S. W., æt. 6: Nervous temperament; generally healthy. Complains of pain in the right side; breathing short and quick; pulse full; some fever. Considering it an attack of pleurisy, cupped and scarified the affected side; ordered warm poultices; gave ipecac and veratrum; ordered the asclepias. On second visit, after removing the poultice, discovered small red spots. She was greatly relieved. Attempted, by warm baths and an emetic, to produce general efflorescence. Did not succeed. Continued the asclepias—the efflorescence of the side continued five days, and the case gradually convalesced.

Many more cases have occurred within the limits of my practice; but these are a fair selection of the varieties which have been observed. Many patients I have not treated at all, and they recovered in about the same time as those who were treated. I am firmly of the belief that these cases are neither scarlatina nor rubeola.

Bateman, in his synopsis, gives the following as the symptoms of roseola. The roseola is a rose-colored efflorescence, variously figured, without wheals, or papulae, and not contagious. He then mentions seven varieties: R. aestiva, R. autumnalis, R. annulata, R. infantalis, R. variolosa, R. vaccini, R. miliaris. Of Roseola aestiva, he says: "Roseola aestiva is sometimes preceded for a few days by slight febrile indisposition. It appears first on the face and neck, and, in the course of a day or two, is distributed over the rest of the body, producing a considerable degree of itching and tingling. The mode of distribution is into separate small patches, of various figure, but larger and of more irregular forms than in the measles, with numerous interstices of the natural skin. It is at first red, but soon assumes the deep roseate hue peculiar to it. The fauces are tinged with the same color, and a slight roughness of the tonsils is felt in swallowing. The rash continues vivid through the second day, after which it declines in brightness, slight specks only, of a dark red hue, remaining on the fourth day, which, together with the constitutional affection, wholly disappear on the fifth."

The first case related appears to give the disease a contagious nature. She had visited a family where the disease was
prevalent, and in seven or eight days after she is attacked in like manner. But I should have added, whilst stating that case, that none of her sisters or brothers contracted the disease, though she was constantly with them. This case, therefore, cannot be considered as a violation of Bateman's and Willan's rule—that the disease is not contagious.

I believe all writers upon the Exanthemata freely acknowledge, that the efflorescence of Roseola is frequently partial, and that its retrocession is accompanied by headache, giddiness, disorder of stomach, &c. This remark may serve to reconcile the anomalous cases which I have related, and to sustain the opinion that all are but varieties of Roseola.

These thoughts I have hastily grouped together, and, in your own language, if what we have observed here does not accord with the experience of other Southern practitioners, we would like to be apprized of the fact.

**ARTICLE XXXIII.**

*Case of Stabbing—Reported by W. C. Musgrove, M. D., of Burke county, Ga.*

**Dear Doctor—In** the August No. of the Journal I notice a communication from an old chum, which reminds me that we physicians should be true to our profession, by aiding and promoting the advancement of the science. How may this be done? By drawing upon the *note-books* of Southern physicians, many interesting cases may be brought to light, and valuable points of practice deduced therefrom.

In sending you the within case, I must acknowledge that it was in some respects unsatisfactory, as, owing to the nature of the wound, no opportunity was afforded me for a correct anatomical examination.

J. A. S., (white,) æt. 25; robust, weighs 180 lbs.; intemperate—was stabbed, on the 28th December, 1852, at 4 P. M., in the right side of the neck, with a knife, the blade 3½ inches long, ⅝ inches wide, ⅜th thick on the back; the wound was diagonally across the external jugular vein, which was divided ⅔. The direction of the wound could not be ascertained, owing to extreme hemorrhage. The wound was, as before stated, on
the right side of the neck, about midway, and 1\(\frac{1}{4}\) inches above the clavicle. The thrust was an over-thrust, and the only resistance met with was a very thin silk cravat, which was cut through in its upper edge; the violence of the blow was such that he was thrown back several paces. He was not aware where he was struck—"crying out that his hand was cut all to pieces." The hemorrhage was excessive, spitting from him some three feet. Being present, and from the alarm of those about him not having any aid, I applied my fingers to the orifice, and in ten seconds he fell. Compression, in this manner, was applied for two hours, when a coagulum having formed, the edges of the wound were brought as near together as possible, compresses and bandages applied, morph. sulph., in \(\frac{1}{4}\) gr. doses, was given every two hours until 2 grs. were taken. (He had been under alcoholic influence for several days previously.) He slept well until 3 o'clock, A. M., when he became agitated, the pulse quick, small and tremulous; owing to jactitation the bandages became displaced, and hemorrhage recurred, which, however, was soon arrested. Brandy was now given; his pulse soon reacted, became full and soft—remained quiet during the night.

Dec. 29. Assisted by my friend, Dr. Hainey, the compresses were removed, and the wound was found to be filled by a strong coagulum; the lips of the wound were brought together by two sutures, and light compresses and bandages applied. At 3 o'clock, P. M., patient was removed home, (in an ox-cart, filled with fodder, upon which a mattress had been placed,) a distance of three miles; he bore the trip well—better than I anticipated; a slight oozing from the wound. Ordered 1 oz. sal. eps. He was removed because the affray occurred at a grocery, where no accommodation could be afforded.

Dec. 30. There is some febrile action; complains of numbness of the fingers and fugitive pains in the right side; no cough; great thirst; no movement of the bowels. B. sal. eps. 1 oz.; acidulated drinks in small quantities.

Dec. 31. Cathartic has acted finely; the febrile symptoms have passed away; the pain in the chest removed; arm much less painful. The wound examined; healing finely, and the coagulum nearly absorbed.
Jan. 3. Patient has been doing well, coagulum nearly gone, size of an ordinary bullet; previously as large as a guinea egg; some numbness of the arm. Consider him out of danger.

Aug. 27, 1854. Patient convalesced, and has been pursuing his ordinary avocations, but an unfortunate sequent has been the result; the fingers of the right hand have become very much contracted, particularly the 2d, 3d and 4th; there is no wasting or loss of muscular substance.

The blade of knife presented to the scapula, and if my anatomical views are correct, the knife after dividing the external jugular \( \frac{3}{4} \) must have touched the subclavian without penetrating, the back of the knife passing across the artery and penetrating the tissues beneath the scapula and wounding some portion of the brachial plexus, perhaps the flexor and ulnar nerve.

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**ARTICLE XXXIV.**

**Cases of Dysentery.** Reported by B. R. Rives, M. D., of Milford, Baker county, Ga.

**Dear Doctor**—I have for some time wanted to write to you on the subject of Dysentery, as it has been, and is now prevailing in various portions of the State; but owing to the short time that I have been engaged in the business of the profession, together with the limited number of cases that have been confided to my care, I have hesitated doing so until now. However, since noticing your article on the subject, in the August No, of the Journal, I feel relieved, to some extent, of the embarrassment I would have labored under in giving my views, with the treatment I have found to be invariably successful, so far.

The disease was said to be raging to a frightful extent in the adjoining counties, and hard by our own neighborhood, for some time before it reached the bounds of my practice. The following cases are related from memory, as I took no notes at the time. They will, however, suffice to the end in view.

**Case 1.** Mr. N., on the evening of the 23d of July, 1853, called on me to prescribe for his child, aged 18 months: he
stated that it had the fever and bowel complaint. I prepared
him some paregoric and chalk, to control the bowels, and order-
ed quinine next morning to meet the fever, requesting him to
let me know if it got no better.
On the evening of the 25th, I found it with high fever—pulse
150; tongue rather dry and coated, edges and tip very red;
stoach distended; would rest for a few minutes on one side,
and then turn rapidly over on the other; countenance marked
the degree of suffering; bowels acting every five or ten min-
utes; character of stools would change alternately from bloody
mucus, with or without fecal matter, to bloody serum, with but
little or no faeces.
Treatment.—Calomel and opium; laudanum, camphor, pare-
goric, and chalk; tannin; chalk and gum-water as a common
drink; starch and laudanum injection; warm bath and warm
fomentations to the bowels, were all used in their turn, up to the
night of the 26th, when the child died.
Case 2. July 25th. Mrs. B.'s child, aged 2 years, was taken
in the same way as case 1st. Treatment, about the same; ter-
mination the same.
Case 3. August 1st. In this case the writer had himself to
treat. I was just convalescing from an attack of intermittent
fever, when taken with dysentery. Knowing that I had been
unsuccessful in the cases above reported, and that my condition
was such as demanded speedy relief, I determined upon trying
the saline treatment, and accordingly got a tumbler half full of
cream of tartar, added water to it, and swallowed the whole at
one draught. The result was, that in about two hours I had a
copious serous discharge, unaccompanied with pain or tenes-
mus, freed from all griping—in a word. I was relieved of all the
distressing feelings I had previous to taking the medicine. Af-
ter suffering it to act three or four times, I found no difficulty
in checking it with laudanum. I was annoyed but very little
the next day, and, without taking anything else, was soon well.
After this, I treated seven other cases, in rapid succession,
the same way, and with like success. It would be well,
perhaps, to remark, that these cases were seen at their very
onset, and that they were accompanied with neither nausea nor
bilious vomiting.
Case 4. August 12th, 1854. Mr. E., aged 44 years, constitution much impaired, commenced throwing up at 9 o’clock A. M. 3 P. M. Still throwing up, and passing blood from the bowels, with much pain and tenesmus; frequent desire to go to stool; griping of the bowels; thirst urgent; tongue rather dry and red.

Treatment.—Attempted to allay the vomiting, which I partially succeeded in doing with internal remedies and mustard externally. Then gave a large dose of cream of tartar, which soon caused serous discharges and a manifest improvement in the symptoms. Ordered morphia to be given, so as to allay all pain and to keep the stomach quiet, and if his bowels should again become active, to give laudanum after each stool.

13th. All the symptoms had greatly improved; bowels had not been disturbed; passed a good night, and thought he would soon be up.

17th. In the same condition as the 12th, except that he had considerable fever. Ordered a large dose of cream of tartar, with instructions to give laudanum after it acted three times, in order to check it. Morphine and sinapism, if necessary, to quiet the stomach; and quinine (15 grains) to be taken in three portions, at intervals of three hours, commencing at sunrise tomorrow. Under this treatment he soon convalesced.

Case 5. August 20. Was requested to see the Rev. T. C., aged 55: found him having alvine evacuations, every ten or fifteen minutes; character of discharge the same as in first case reported; some nausea; tongue heavily coated, with point and edges red; pain in bowels; tenesmus great; some fever; had been sick for several days. Ordered cream of tartar in full dose; in four hours had serous discharge, but was not entirely free from griping. After three or four serous actions the bowels were checked, as in the other cases, with laudanum. Elm water was used as a common drink. As he had great aversion to quinine, I did not prescribe any for him the next day. The case went on gradually improving; but was rather tedious, and he had on the 24th to take another dose of cream of tartar. Morphine was used all the time to allay pain. He soon convalesced after the second dose of the saline, and is now well.

In conclusion, I must say, that I firmly believe that quinine, cream of tartar, laudanum, morphine, and elm and chalk water,
are all the remedies needed in the treatment of dysentery. Mustard may be used over the stomach, and cups applied to the back of the neck when the head is affected, &c., &c.; but in most of the cases cream of tartar and quinine will do alone.

**On Headache and its Varieties.** By Patrick J. Murphy, M.D.

(Concluded from page 554.)

**Active Congestive Headache.**—By active congestion is meant *plethora*, or too much blood within the cranium. It differs, however, widely from passive congestion. In the former, the arterial system is in fault; in the latter, the venous. In the one, there is too much blood sent to the brain; in the other, the blood moves too slowly in the veins. Arterial fullness shows power; venous distention is an evidence of obstruction or weakness.

**Causes.**—A general plethora, in which the brain shares; hypertrophy of the left ventricle; adhesion of the pericardium, when menstruation is ceasing.

**Diagnosis.**—This form of headache is easily diagnosed, either from the organic causes or the symptoms of constitutional plethora, as the full habit, the florid complexion, the incompressible pulse, the resistance to cold, the giddiness attendant on stooping the head, and the obtuseness of hearing. This habit of body is oftentimes the precursor of gout. The heart should always be examined, especially if the person has passed his fortieth year; and before the twentieth year, adhesion of the pericardium from rheumatism is more frequent than is usually believed. The pain of the head is trifling, unless when the heart is in fault.

Treatment is almost obvious, but it is very difficult to change a plethoric constitution. We should recommend moderation in diet, especially in animal food; abstinence from alcoholic drinks, and from any other liquid; the lessening the hours of sleep; to lie with the head high; to avoid much stooping, for a temporary apoplexy is not rare in those who stoop and exert themselves to pull on their boots; not to wear any thing tight round the neck, and to exercise as much as possible in the open air.

If there be a persistent giddiness, ten or twelve ounces of blood may be taken from the nape of the neck by cupping, or from the arm. If an issue or seton be deemed advisable it will be better to insert it in the left arm than in that most inconvenient part the nape of the neck. The tinctures of digitalis or
Headache and its Varieties. [November,

or hyoscyamus will be found useful, also pills of ipecacuanha, or nauseating doses of tartar emetic. Dr. Cheyne's favorite remedy was the pulvis antimonials.

The sick headache, to which females are such martyrs when menstruation is ceasing, comes under this denomination of headache. It comes on at their usual period, but the menses either cease flowing, or escape very scantily; it is very distressing, and attended with an inclination to vomit, hence the expressive term sick headache. The face is flushed, the appetite lost, and the temper disturbed. It will be found both a preventive and cure to take blood, for several periods, either by lancet or otherwise, some days previous to the attack; to keep the bowels very lax, especially with the acetous extract of colchicum and saline purgatives; setons and issues are very useful when inserted in the lower extremities; the diet must be moderate; the feet kept warm, and exercise prescribed. During the attack, the feet placed in very hot water, and sinapisms to the lumbar region give great relief. Vomiting, if spontaneous, sometimes relieves, but an emetic often fails. In treating females, no matter what the disease may be, it should never be forgotten that it is exasperated, and a headache sometimes added a few days previous to menstruation.

The Neuralgic Headache.—Neuralgic headache is synonymous with those headaches described by some old authors as hemicrania, by others as clavus hystericus, and by Dr. Graves as hysterical congestion. It is peculiar to females, and to females during a certain period of their existence only—from puberty until the final cessation of the menstrual secretion. Dr. Graves gives a graphic description of the symptoms and of the injurious effects of the usual routine treatment. He calls it hysterical congestion; but he seems not to have understood its true pathology. There is no doubt of its being hysterical, but there is no congestion, for the seat of the pain is in one of the nerves of the scalp, which can be easily proved by a slight examination, and it is therefore an external headache. The error may have arisen from his having met with cases where this headache was in combination with the anaemic headache. The proper name which should be bestowed on this headache, in order to facilitate the diagnosis, is spinal irritation of the sub-occipital nerve. Spinal irritation is beginning to be well understood in this country; we are indebted to a French physician, M. Valleix, for the discovery. Since then many other disorders, such as irritable mammae, pleurodyne, and neuralgic headache, are discovered to originate in functional derangement of the spinal cord; and I believe whoever
will carefully compare these disorders with cases related by Dr. Tilt must come to the conclusion that they are nothing more than symptoms of subacute ovaritis. They are hysterical disorders, and *hysteria is subacute ovaritis*, which displays its phenomena on the sensitive and motive nerves of the spinal column.

On comparing the neuralgic headache with the phenomena of spinal irritation in other parts, we find how exactly they coincide. Like spinal irritation, it is a form of hysteria, and therefore peculiar to females. It is not only peculiar to females, but attacks them only during the menstruating period of their existence—that is, from about the thirteenth to the fiftieth year. It is exacerbated just previous to menstruation, makes its first attack on the left side, and rarely passes over to the right side.

**Cause.**—As this form of headache is peculiar to the female sex, it must therefore have its origin in some organ peculiar to them; and as it is felt during a certain period of existence only, the organ must have the performance of its functions limited to that period. As there is no organ by which these two facts are explicable, unless the ovarium, it is not unphilosophical to conclude that the disorder proceeds from the ovarium. There is certainly also the uterus, but the functions of this viscus cease on the removal of the ovaria. We daily meet with the uterus inflamed, ulcerated from cancer or cauliflower excrescences, distended by hydatids or pregnancy, producing moles and polypi, but none of the phenomena of spinal irritation are present. In the married female who bears children regularly it is scarcely ever known. Before the commencement of menstruation, or after its termination, it is equally rare.*

What is the state of the ovarium I do not pretend to affirm. If inflammation, yet it has often yielded to tonics; it may depend on moral causes, but such explanation has never satisfied me. An accumulation of faeces in the rectum has appeared to me as occasionally the source of irritation; in a few cases I think it was traceable to ascarides in the rectum. We witness the action of cold in paralysing the trunk of a motor nerve, the portio dura, as it escapes from its cranial foramen, but cold cannot be a cause of this headache, otherwise why should not the male sex equally suffer?

Occasionally spinal irritation, in other parts, has been observed earlier in life, but I have not met with the headache;

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*While writing the above, I referred to Dr. Tilt's work on Diseases of Females, first edition, and in page 58, he gives the valuable fact, that he found the right ovary affected in only five out of seventeen cases. Now, it might be worth inquiry to ascertain whether the left had not been previously affected, but that the irritation was transferred to the right, as we see in ophthalmia occasionally.
and, as the headache has occurred some years before the appearance of the menses, so I believe it possible it may arise a few years after their total cessation. The headache resembles spinal irritation, also, in a curious and hitherto unexplained phenomenon: *commencing* on the left half of the body, we occasionally meet with it also on the other side; but I have never discovered that it began there, nor is it ever restricted solely to that side. When both sides are attacked they are unequally so, the left being by far the more painful. As another proof of its being spinal irritation, if further proof be necessary, we find it under two distinct forms, and these forms are easily distinguished by the nature and extent of the pain. In the one it is confined to the exact track of the sub-occipital nerve, it is lancinating or shooting, intermitting, and chiefly felt at its determination in the integuments of the temporal region; when severe in this spot it is the *clavus hystericus*. When the whole course of the nerve and its branches are implicated the entire left side of the scalp is very tender, sometimes exquisitely so; this is the hemicrania. It is singular how much this disease is confined to the left side of the head; we find such to be the fact in ninety cases out of a hundred. It seldom reaches the aggravated form of clavus hystericus without being accompanied with other well-known hysterical symptoms which, of course, facilitate the diagnosis.

**Diagnosis.**—This headache attacks females exclusively. I have never heard or read of men suffering from this kind of headache. It is only during the menstruating period of life that even females are liable. The pain is referred to the left side of the head; it is worse on the approach of the menstrual flow; it is found on the track of the sub-occipital nerve. The course of this nerve is well known; it accompanies the sub-occipital artery, emerging from the spinal canal; it passes along the back of the head, midway between the mastoid process and the mesial line, sending branches to the integuments which cover the parietal protuberance, and terminating in the temporal region. Its course from its exit to its termination, can oftentimes be accurately ascertained, from the pain induced by pressing upon it. Although the head suffers, pressure may not always produce the pain, for it is intermitting. In general, however, pain may be thus detected in one of three places; on the left side of the neck, where the head and vertebrae join, at the parietal protuberance, or in the temporal region; when concentrated in the last spot it is the well-known clavus. It is sometimes painful in all three, and, sometimes in its whole track. It is, however, rare that the tenderness is absent in the occipital region. The parts suffer more when
pinched than when pressed. When the branches as well as trunk suffer, we then have hemicrania—a most painful form, less intermitting than the other, and preventing the unfortunate girl from lying on the affected side. It is more commonly met in the unmarried female, from the twenty-second to the thirty-fifth year, but the married females who are childless do not escape.

This headache is chronic, intermitting, may continue for days, weeks, or months, then subside, and return after the lapse of months, or even years. A first attack is seldom felt before the twentieth year, nor after the thirty-fifth. The pain is generally of a shooting kind, darting from the neck towards the temple, and never towards the neck, by which it is easily distinguished from odontalgic pain. Neuralgia of the left mamma, (irritable breast,) or of the seventh or eighth intercostal, (pleuroodyne,) frequently co-exists. It is sometimes found in combination with the anaemic, but more rarely with the congestive headache. From caries of the body of a vertebra it is easily distinguished by the pain being superficial, being confined to the left side of the spine, by its not becoming worse when the head is flexed on the chest, nor by jumping, nor by pressing the head against the spinal column.

This neuralgic pain sometimes accompanies the rotated spine. It is singular how often toothache is mistaken for headache, especially for this form. In both, the pain is described as shooting in the course of the nerves, but in toothache the pain shoots towards the neck and ear, leaves no tenderness of scalp, never goes so high as the parietal protuberance, and is more correctly discovered by learning that a paroxysm is brought on by food, sometimes when warm, at other times when cold.

Treatment.—If the disease be not complicated, we can promise relief. The bowels should be kept open by regulated diet, and by aperients, such as castor oil, olive oil, lenitive electuary, powdered rhubarb, soluble tartar, or the compound rhubarb pill. If the bowels are obstinate, an enema of a pint of cold water daily answers the double purpose of removing the contents which may irritate the ovary, and as a local application to the organ chiefly in fault. The cold hip-bath is a valuable remedy when the constitution is vigorous, but all these things are inferior to sea-bathing. Stimulants should be abstained from, employment should be found for mind or body, but physical efforts are preferable. The sedentary position required by the needle, especially in solitude, is very injurious. A sinapism over the exit of the nerve gives great temporary relief; a vesicating plaster of cantharides is better, but it oftentimes
leaves a mark, and therefore, on account of sex, age and position in life, may be objectionable. A croton-oil liniment, made with one drachm of oil and one ounce of camphorated tincture of opium, and rubbed until pustules appear, is preferable, as it leaves no permanent blemish. The belladonna plaster, mixed with powdered opium, or a liniment of extract of belladonna, rubbed with mucilage, are useful and unobjectionable remedies. Speedy relief is occasionally afforded by veratrine or aconitine ointment, made with from four to six grains to half an ounce of spermaceti ointment. The finger used in rubbing should have a piece of bladder interposed.

One ounce of tincture of aconite, with seven ounces of rose-water, is a safer remedy to trust to inexperienced hands than the veratria. The internal medicines are not so easily chosen. Tonics are frequently required, and they may be combined with anti-hysteric remedies. The disulphate of quinine may be exhibited in a strong infusion of valerian, compound iron pill, with assafetida in large doses, is very beneficial. If there be irritability of the stomach co-existing with profuse menstruation and leucorrhœa, pills of valerianate of zinc, half a grain three times a day, with one drop of creosote, answer many intentions. If there be much debility, the sulphate of iron may be combined with infusion of valerian and ammonia, or the ammoniated tincture of valerian may be prescribed. The pain is sometimes so acute that some relief is quickly demanded, and half a grain of morphine will lessen the pain for a while until other remedies have time to act. For the leucorrhœa one drachm of acetate of zinc to one pound of distilled water is useful as a lotion. But we are sometimes perplexed, for the tonic treatment is not the best for a full plethoric female; leeching or even general bleeding is required, but the cases are rare which require general bleeding.

If the patient be not very weak, and there is much leucorrhœa and menorrhagia, the treatment laid down by Dr. Tilt for sub-acute ovarianitis should be adopted. He leeches in the menstrual interval, and then blisters the iliac regions; but as his work is universally read, the treatment is well known. Sea-bathing, when practicable, should never be omitted.—[London Lancet.

On the Treatment of Cancer by Congelation; and Local Anaesthesia from Cold. By James Arnott, M.D.

(Continued from page 546.)

It has unfortunately resulted from the circumstance of extreme cold having been recommended as a mode of producing insensibility in surgical operations, as well as a remedy of great
antiphlogistic and anodyne powers, that the sphere of its action in the latter capacity has been reckoned much more limited than it ought to have been. It is true, that complete insensibility cannot be produced by the common frigorific mixture, and without the aid of pressure, or the arrest of the circulation for a time, beyond a very small depth from the surface; but its remedial efficacy will extend as deep, at least, as the depression of temperature produced, and this may be much more extensive than the insensibility. Consequently, when in a recent criticism of my publication on the impropriety of using the dangerous expedient of chloroform in cases where cold might be safely substituted, and with other advantages in addition to its anaesthetic power, the reviewer would limit the remedial action of congelation to erythema, boils, and other cutaneous inflammations, he shows that he has not reflected on this circumstance; and that he is not better acquainted with the effects connected with the remedial use of severe cold than he is with the important fact in connexion with its anaesthetic use, that in at least three-fourths of the reported 50 cases of sudden death from chloroform, (and probably in as large a number of the 150 unreported cases,) complete anaesthesia might have been safely produced by it.* If cold 60° or 70° above zero Fahr. is employed with well-merited confidence in diseases of the joints, in gun-shot and other severe wounds, in inflammation of the brain or its membranes, in ophthalmia, in phlegmonous erysipelas, and, in fact, in all external inflammations, surely it is not unphilosophical to suppose (to say nothing of the results of ac-

*Medico-Chirurgical and British and Foreign Review, for April, 1854. There are other statements and opinions in this notice of my pamphlet equally objectionable which it would be irrelevant to mention. But if the reviewer's admission, that congelation is "a very valuable remedial measure" in the disease he mentions, and that the pointing it out as a substitute for chloroform in certain minor operations is "confering a great benefit on operative surgery"—if these admissions are generally acted upon, I cannot doubt that the sphere of its utility will soon be extended; that many dangerous diseases will be promptly cured by it; many formidable inflammations prevented, and many lives saved that would be otherwise sacrificed by chloroform. Before concluding these remarks on cold as a local anaesthetic, I may be permitted to refer to some experiments connected with this subject lately made in the Hotel Dieu, of Paris, and recorded in the Gazette des Hopitaux of the 18th ult. Cold in these was produced by the evaporation of ether. Although it matters little how the principle is carried into operation, if carried out effectually, I think frigorific mixtures, appropriately modified, will be found the best plan, and certainly the least troublesome. Amongst other devices that might be adopted, it has occurred to me that a continued jet of air, cooled to the requisite degree, might be projected on the part from a gasometer; or the air might be cooled by passing through small tubes of sufficient length immersed in a frigorific, or, through a tube containing solid carbonic acid, connected with a gasometer of French bellows. The probable benefit from intense cold, in certain important cases where the ordinary means of applying it cannot be employed, as in inflammation of the larynx, would in the opinion of a conscientious practitioner, render the trouble and expense of such a device no legitimate objection.
tual experience) that a cold of zero would produce a greater amount of benefit; and, to return to the subject of this paper, if much benefit in cases of cancer has been produced by common refrigerating lotions, much more might have been expected from pushing farther this therapeutical principle. The effect of congelation is merely the fulfilment of the surgeon’s intention when he applies cold in the common modes, but he has hitherto never attained his object because he has always (ever since the origin of the medical art) been under the erroneous notion that a severe degree of cold applied, for however short a period, would endanger the vitality of the parts subjected to it. The mistake has originated from making no distinction between short and long applications of extreme cold.

Every one knows that long-continued congelation will destroy the vitality of a part, and M. Velpeau has used such congelation with this view in certain cases of cancer. In his recently published work on Diseases of the Breast, he speaks approvingly of this practice;* but though whatever is approved by a surgeon of his great and justly acquired celebrity is entitled to every consideration, my own experience convinces me that the minor effects of short congelation, produced without the slightest hazard to the vitality of the skin, would, under ordinary circumstances, and if properly carried out, be perfectly sufficient. It is true that in certain cases, where the only alternative would be the use of the knife or caustic, such a proceeding might be preferable to either; but cases of this description must be of rare occurrence.

It is almost unnecessary to mention that M. Velpeau makes a marked distinction between long and short continued congelation. In enumerating the various operations in which he has employed the latter as a local anaesthetic, he does not deem it necessary even to allude to the perfect safety, as respects the vitality of the skin, with which it may be employed for such purposes.

In the following case of scirrhous cancer of the breast, the tumour was not only larger, but it was more deeply seated in the breast than in that related in the former number of the journal, and consequently there has been more difficulty in causing the remedial influence of congelation to pervade it. It constitutes, however, a better example of the mere arrest of the disease, for I have little doubt that had the other case continued under my own immediate care, all trace of cancer would have soon disappeared; and the case now to be related would, I think, have made quicker progress to recovery had the treatment been modified.

It was early in May of last year that I was consulted on this case. The patient had previously left her residence in Kent to ask the opinion of Mr. Lawrence, who not only agreed with her usual medical attendant that the tumour in the breast was cancer, but said that unless she immediately submitted to its excision, it might prove fatal within six months. She preferred the treatment by congelation. On examining the breast, I found a hard, flattened, hemispherical swelling, of about three inches diameter, knotted on its surface, contiguous with, but not adhering to the skin, excepting at the nipple, which was retracted and slightly ulcerated. There was at times a lancinating pain. The disease had existed more than two years, and although the usual routine had been had recourse to, no kind of treatment had appeared to be of any service. The disease gradually but steadily progressed.

The frigorific mixture of ice and salt was applied for about four minutes, the usual precautions being taken to prevent the smarting that would otherwise take place on the return of sensibility to the parts which had been congealed. A similar application was repeated about every month by her medical attendant in the country; and after about six such applications I again received a visit. The tumour appeared to be smaller than when I first saw it, the decrease being chiefly in its thickness; and in other respects there was great improvement. She continued the same plan of treatment, and the principal results are recorded in the following extracts from a letter which I have lately received from her, dated April 6th:

The substance of this letter is, that the tumour continues of nearly the same dimensions; though it appears to be a little longer, it is less thick. She has not "for the last four months known what a bad night is, being always free from pain," though during the day there is, "at times, three or four, or perhaps more, transient pains, while, at other times, she passes some days without any pain." Her "general health is very good, and is kept good by regular exercise in the open air." As I had expressed the opinion that she should make longer intervals between the applications than a month, in order to ascertain whether the tumour was not now merely a lifeless mass, like a bullet in the flesh, which might give occasional uneasiness, particularly when the mind was intent on the subject, she states, in reply—"Five weeks have intervened between the last applications; I have these renewed, because, while I feel there is life in the tumour, I think they are necessary." She concludes a letter written a month previously (March 4th) by the expression of a wish "that every sufferer
from the same disease were as happily delivered from the effects of a cancer as she has been by this remedy.”

As in almost every case which I have treated by congelation a certain degree of hardness and swelling remained after the disappearance of other symptoms, it is important to investigate the cause of this. On the supposition that cancer is essentially a congeries of living cells, we may reasonably think that the absorption of these, after their vitality has been destroyed, must be slow, if it takes place at all; and perhaps the irritation that has induced patients to apply for the frigoric application afresh, may have proceeded from the presence of this inert mass of dead cancer-cells. I should be sorry, however, to think that the absorption of these never takes place, because in a voluminous congeries of cells it were difficult to understand how the cold could reach the inner surface of the mass without the absorption of the more superficial layers having previously taken place after the extinction of the life of the cells constituting them; unless, indeed, the layers of dead cells were to form so good a conductor as scarcely to resist its passage. But in their living state, the tumour in which they are interspersed is so dense and so little intermixed with blood-vessels as to form a substance easily permeable by cold—as easily, perhaps, as cystic tumours are, the fluid contents of which I have congealed in applying cold to them, as an anaesthetic, previous to their excision.

If it be objected to the cases which I have now related that they do not furnish satisfactory evidence that cancer is completely curable by cold, they show, at least, that it will produce much more benefit in this disease than any other remedy. If an unequivocal cancerous tumour subjected to this treatment, instead of increasing in size, gradually decreases, while the patient becomes free from pain (excepting such uneasiness as a mass of dead cancer-cells might excite), and the general health improves,—if no glandular enlargement takes place in the armpit, or extension of the disease to any other part appears, and if this condition continues without change for a long period, it is surely a very satisfactory change in the patient’s condition. I question much if it be possible entirely to remove the hardness in the majority of such cases, but this hardness is no proof of the presence of malignant affection, and no source of inconvenience to the patient. What other remedy yet employed in cancer will effect as much? Too many of the measures employed as remedies only precipitate the unfortunate patient’s fate.

Before concluding this paper, I must advert to a statement which appeared some time ago in a medical journal, that a pa-
tient had been seriously injured by a prolonged application of ice to her cancerous breast of upwards of twenty-four hours.

As every one has not yet learned the essential difference between the application of ice and congelation in disease, it may be advisable now briefly to point it out; and as the subduing of the inflammation usually accompanying cancer is one of the objects in using either of these methods of treatment, it may simplify the matter to speak of this difference as respects their use in preventing or removing the inflammation after wounds or other mechanical injuries. Ice, or a temperature of 82° Fahr., acts as minor degrees of cold do, in lessening the increased vascular action which accompanies (if it does not constitute the chief element of) the morbid condition called inflammation; but congelation, by causing a temporary suspension of the circulation and sensibility of a part, does this and a great deal more. It effects such a change in the vital actions of the part, perhaps by removing for a time the tonicity of the small arteries, as not only immediately to arrest inflammation, but to render the part unsusceptible of this condition for some time afterwards. If this suspension of vitality be of short duration, there is not the least hazard of the part being permanently injured, and when congelation is purposely effected by a powerful frigorific, its duration and extent can be exactly limited. But if it should happen that owing to a debilitated state of the body, or other circumstances, a prolonged application of ice should at length congeal the part, as the surgeon may not be aware of this unintended effect until after it has lasted for a long time, (as has happened in applying ice to gunshot wounds and in strangulated hernia,) much injury may be the consequence. Fortunately, the aching usually caused by a continued application of ice is a preventive of such mischief, and another reason why it has been so little employed in surgery.

P. S.—Since the above article was forwarded to the office of The Lancet, on the 10th ult., Dr. Hardy has published a proposal similar to that contained in the note to the first paragraph, in the Dublin Medical Press of the 19th; but a stream of artificially cooled air was suggested as a substitute for other refrigerating means in the introduction to my collected essays on Congelation, published last year. Within this short period, likewise, Dr. Snow, who I believe is reckoned an authority on the subject of general anaesthesia, and who is much employed as a chloroformer, has, naturally enough, thought fit to attack its rival—local anaesthesia from cold—in a paper read before the Medical Society of London. Notwithstanding the fact, not unknown to his hearers, that intense cold has already been
extensively employed for this purpose by some of the most eminent surgeons of the day, he indulges them with the recital of some hypothetical notions upon its applicability! When Dr. Snow spoke of the great pain produced by a frigorific mixture applied to his hand, he must have forgotten that both hands and feet often become benumbed in very cold weather without any preceding pain. Complete anesthesia to the small extent he speaks of, may be produced by cold, properly applied, with scarcely the slightest uneasy sensation; and when the circulation is quite arrested by cold, the smarting or tingling does not exceed that produced by mustard, and is never complained of by the patient. It is much less disagreeable to him than the sense of suffocation which accompanies, and the headache and sickness that follow, the exhibition of chloroform; to say nothing of his dread of losing consciousness and life.

I may probably on some future occasion, notice certain other statements in Dr. Snow's paper; but I cannot advert to them at all without expressing a feeling of surprise, that a practitioner, who has himself experienced all the horrors attendant on causing death by chloroform, should still talk of employing it on the most trifling occasions. The case of death from chloroform which has just occurred in France, and in the hands of so careful a surgeon as M. Richard, will, it may be hoped, be amongst the last; for surely a recourse to chloroform in preference to cold must very soon be confined to the comparatively small number of operations which involve the incision of deep-seated and sensitive parts; and even in these (as I have explained in my late pamphlet on the subject) a combination of cold with chloroform would render the exhibition of a large dose of the latter unnecessary, while it would very much promote the healing of the wound.—[Ibid.]


Every practitioner is in the habit of looking at the tongue in all cases of internal, and in most cases of external, disease. From a glance at its general appearance, he forms an idea of the amount of irritation which may exist in the digestive apparatus, large or small intestines. In a surgical case, also, the tongue informs him what may be the amount of constitutional irritation present. I believe, however, that the tongue is an organ which requires looking into much more minutely than is commonly done, that certain portions of it may be allotted to particular diseases, and that our remedies may consequently be given with greater precision. In making this assertion,
I am advancing no new theory, but merely supporting an old one, as well as a neglected one, proposed by Dr. Ridge. I read his work, tested it to the best of my ability, and have arrived at the following conclusions:

1. The tongue points out to us the particular organ affected in all chylo-poietic derangements.

2. It indicates the seat of disease in the respiratory apparatus.

3. The tongue gives unmistakeable appearances in affections of the circulation and media thereof.

4. It is a most valuable guide in fevers.

Many of the readers of the Lancet may not have seen the book I allude to, and therefore, without some little explanation, will scarcely understand some of the terms used in this communication. Dr. Ridge divides the tongue longitudinally into three parts: central laterals, small portions on either side the raphe; laterals, to the outer side of the foregoing; and edges. He then divides these, by means of transverse lines, into—

\[
\begin{align*}
\text{Anterior,} & \\
\text{Second,} & \\
\text{Central,} & \\
\text{Posterior,} & \\
\end{align*}
\]

fourths,

giving a part of the anterior fourth of the central laterals to the "tip" and the junction of the anterior and second fourth, to the "oval."

The central laterals show the respiratory organs.
The laterals, the chylopoietic viscera.
Edges, the brain.
The tip shows the state of the large intestines.
The oval is given to the pleura.
The heart seems to influence the whole of the tongue.

In support of my first conclusion, I would wish to call attention to any ordinary case of dyspepsia, with sluggish liver. Here the tongue may almost always be found covered with a thin, white film, the whole length of the laterals, these portions being thrown prominently forward, and most clearly defined; and in nearly all those cases where sickness and pyrosis have been present, the posterior and central fourths have been most thickly coated. It sometimes occurs in this affection that the tongue is slightly coated over its whole surface; but on desiring the patient to protrude it forcibly, the posterior fourth of the laterals is seen thickly furred. As the affection extends to the liver and duodenum, the laterals become coated their whole length; and on the application of remedies, embracing the stomach and liver, the disease quickly yields. If the posterior and central fourths are alone affected, our treatment must be
confined to the stomach alone, and, I believe, with a certainty of success. In one instance of stricture of the oesophagus, near the cardiac orifice of the stomach, I observed the posterior fourth of the laterals to be thickly coated, and as the disease advanced, doubtless affecting the extremity of the stomach, the furring progressed anteriorly. Again: in a case of malignant disease of the pylorus, which rapidly extended to the duodenum, and formed a mass of disease easily felt externally, the laterals were the only parts of the tongue coated, the other portions of the organ being perfectly clean and glassy. I could produce many other instances of derangement of the prime viæ which my note-book informs me invariably presented the same appearances, but I will not occupy the pages of the Lancet with "twice-told tales."

Secondly.—The appearance of the tongue in cases of phthisis (which have come under my observation) has been varied, not in the position, but in the degree of coating. We are to remember that the central laterals are the portions of the tongue given to all thoracic disorders, as well as to those of the larynx and trachea, and it is to these parts we must look for our indications of disease. Accordingly, we shall find them covered with fur thrown up, as it were, by injection of the vessels, and clearly defined. I am not now referring to pneumonia and pleuritis, (neither of which diseases I have seen during the last two years,) but to cases of tubercular deposit, and ultimate softening, with all its attendant symptoms. With regard to the degree of coating, I have found, that although well-marked as to position, it has been in some of the recent cases less than in more advanced ones. I will quote a case, illustrating in a remarkable manner that the diseases of the air-passages, as well as of the substance of the lungs, are indicated by the state of the central laterals.

"A patient of broken-down constitution was afflicted with secondary symptoms—ulceration of the fauces and soft palate among the number. After the usual remedies had been applied, and persisted in for some time, the parts became sound, and the case progressed favorably, when suddenly great difficulty of breathing came on, approaching to strangulation, and rendering it a matter of doubt whether the operation of tracheotomy should not be performed. The symptoms were relieved by anti-spasmodics and sedatives in powerful doses. The attacks recurred at intervals of fourteen hours during three days and were subdued by the same treatment. On the sixth day, the disease was apparently checked, but only for a short time, for within a few days unmistakable symptoms of gangrene of the lungs made their appearance, and rapidly carried off the
patient. I watched this case narrowly, and I found that immediately the difficulty of breathing came on then did the posterior and middle fourths of the central laterals become coated, which coating disappeared on the cessation of the urgent symptoms; but after the lungs had been attacked, a thick creamy deposit covered the whole of the central laterals as far forward as the tip."

Thirdly.—In all cases of chlorosis, where the system is deprived of proper nutrition, the tongue assumed a flabby aspect; it is tremulous, and its papillae, particularly those at the tip and sides of the tongue, assume a fringe-like appearance, but are pale, and almost oedematous. In these cases there is a loud aortic bruit, with the bruit de diable. Upon the application of the proper remedies in such cases, the papillae first became very slightly coloured, then contract into their normal dimensions, and, lastly, the whole tongue assumes a healthier aspect some time before the struggle of the patient returns.

In some instances of hypertrophy, with and without dilatation the tongue becomes covered with sulci, more particularly on either side of the raphé. It is also drier than usual, thickened, and appearing as if it laboured under some congestive disease. I have had no opportunity of seeing a case of aneurism lately, but I have frequently searched for and found disease of the heart after observing these deep fissures of the tongue.

Fourthly.—A large number of cases of fever occurred in this city last autumn. It was always a simple type at the onset presenting all the usual symptoms of derangement of the primæ væ with rapid pulse, hot skin, &c. After the initiative measure of purging and an emetic, I found the tongue remaining, if possible more coated than before; and in some cases repeated the purge and emetic given, at the same time, saline aperients and diaphoretics. After four or five days the coating of the tongue became dry, but did not disappear, and I was compelled to have resourse to stimulants, which performed their task most satisfactorily, for the tongue became moist, and in twenty-four hours, was nearly clean. In all my future cases I pursue this course. I gave, first, an emetic, then a purge, and waited until the second day; I then examined the tongue, and if I found the tip (which is the part given to the large intestines) in a clean state, in ever so small an extent, I gave ammonia and bark, and had the pleasure of seeing my patients recover more rapidly than I ever remember under any other plan of treatment.

In these cases I ought perhaps to have waited until the tongue gave evidence of the small intestines having recovered themselves. I did not do so, and never lost a case afterwards.
It is not only in fevers that we must study the appearance of the tip of the tongue. It is a most valuable guide in constipation of the bowels. It is then slightly furred, and covered with papillae, which are so injected as to appear some distance above the fur. In these cases violent purges only render matters worse, and it is only by gentle and long-continued medicines that we shall restore our patient.

In affections of the small intestines we all are accustomed to the appearance of the tongue; but on looking at the anterior fourth of the laterals, by its injection or non-injection, by its ulceration or non-ulceration, shall we detect the true state of the mucous membrane of the small intestines as far as the cecum.

In conclusion, I beg to say that with respect to the brain my observations have been so limited that I have not ventured to deduce any inferences, but that whatever I have now advanced I have repeatedly proved. I have not given any anatomical proofs of the intimate connexion of the several divisions of the tongue with the parts they represent, as I consider that would be trespassing on Dr. Ridge's province, and I trust that my professional brethren will derive as much pleasure and profit from his book as I have done; or, rather, that it may encourage abler heads than mine to investigate his theories for themselves; and, in the end, to impart them to the profession as facts, as both he and I believe them to be.—[Ib.

Source of Hemorrhage in partial separation of the Placenta.

Dr. Mackenzie, in a paper read before the Medical Society of London, Dec. 17th, 1853, pointed out that three different opinions prevailed at the present day respecting the anatomical source of hemorrhage in cases of partial separation of the placenta; the first affirming that it was principally or wholly uterine; the second, that it was principally or wholly placental; the third, that it was both uterine and placental. He further directed attention to the fact, that puerperal uterine hemorrhage, whether occurring in connection with partial or entire separation of the placenta, was generally considered to be principally venous, and he quoted passages from the writings of Drs. Simpson, Radford, Murphy, and Lee, in support of this statement. On reflecting upon these circumstances, he was led to believe that some light might be thrown upon the question by ascertaining experimentally the source of hemorrhage in an animal whose placenta, like that of the human female, was both decidual and fetal. A pregnant bitch was accordingly
hemorrhage in separation of the placenta.

obtained, which had nearly completed the full period of gestation; the uterus was opened, several placentæ were detached, and the following observations made: 1. On separating each placenta, it was found that blood flowed freely and continuously from the denuded uterine surface, increasing with the detachment, while none escaped from the detached portion of the placenta. 2. That the blood which escaped from the uterus was distinctly arterial. 3. On rupturing a placenta while still partially adherent to the uterus, that a small quantity of dark venous blood escaped from the torn part. Thus it would appear that in the canine species, the source of hemorrhage in cases in which the placenta is partially detached is exclusively the denuded uterine surface so long as the placenta is entire, that the hemorrhage is of an arterial character, and that a small quantity of dark venous blood escapes from the placenta on being lacerated while still partially adherent to the uterus. The results of this experiment were not, however, deemed conclusive as to the source of hemorrhage in cases of partial separation of the placenta in the human female, on account of the different distribution of the veins in the maternal portion of the canine and human placenta respectively. Their anatomical peculiarities were briefly pointed out; and in the early part of April, 1853, the author had an opportunity of performing a more decisive experiment with the assistance of Dr. Sharpey. In this, the hypogastric arteries of the uterus of a woman who had died of internal hemorrhage during labour, and in whom the placenta was partially adherent, were injected with defibrinated blood, and the organ, as well as the vessels from whence the blood escaped, were carefully noted. It appeared, on injecting the hypogastric artery, that blood escaped freely from the torn utero-placental arteries on the surface of the uterus; that none escaped from the torn uterine veins, or from the detached portion of the placenta; and it was ascertained, that the blood was not injected with greater force than that of the heart acting under ordinary circumstances. The opposite hypogastric artery was next injected, with the following results: The blood escaped freely from the torn utero-placental arteries on the surface of the uterus; none escaped from the torn utero-placental veins; while, in this case, a small quantity escaped from the detached portion of the placenta contiguous to that which was still adherent. Repeated injections led to no other results; while it was particularly remarked, that the torn utero-placental arteries on the surface of the uterus were free from any plugging previously to being injected. Two things were thus clearly shown from this experiment: 1st. The readiness with which blood escaped from the torn utero-pla-
and, 2dly. That these arteries had not been plugged by any coagula during life. Such facts, coupled with the results of the previously related experiment, and taken in connection with various clinical circumstances, appeared to the author to afford strong grounds for the belief that the principal source of hemorrhage in cases of partial separation of the placenta was arterial rather than venous, and uterine rather than placental; and he proceeded to consider the data upon which the opposite opinion had been affirmed. In doing so he quoted the following passage from Dr. Simpson’s writings, as containing a reference to the several grounds upon which the occurrence of arterial hemorrhage has been denied in cases of partial separation of the placenta. “Uterine hemorrhage, after separation of the placenta,” says Dr. Simpson, “in any of the stages of labour is not arterial in its character, because the uteroplacental arteries are so long and slender as to become readily closed; 1st, by the tonicity of their coats; 2d, by contraction of the uterine fibres upon them; and 3d, principally by the changes in their tissues produced by the mechanical rupture of their coats, torn arteries being little, if at all, liable to bleed, and the placenta being separated by a true process of avulsion.” With reference to the first statement, that uterine hemorrhage, after separation of the placenta in any of the stages of labor, is not arterial in its character, the author observed that, so far as he was aware, it was one which was not only unsupported by any evidence, but directly at variance with many observations which he and other medical men had made. He referred to cases in which he had distinctly observed that hemorrhage occurring between the birth of the child and the complete separation of the placenta was of an arterial character; and he referred to the fact, that the blood which escaped from the uterus of the bitch when the placenta was detached was of a bright arterial colour. With regard to the second point affirmed, that arterial hemorrhage from the uterus is prevented by the tonicity of the utero-placental arteries, he observed, that, while he believed this to be generally the case in a state of health and tranquility of the circulation, that, under other circumstances, it might be doubted whether such was the fact. The third doctrine affirmed, that hemorrhage from the utero-placental arteries is prevented by contraction of the uterine fibres upon these vessels as they pass through and amid the uterine structure, was in the author’s opinion, completely invalidated by the well-known fact, that there is often no direct relation between the degree of uterine contraction and the degree or tendency to uterine hemorrhage; and he further
appealed to the two following series of facts as being opposed to its correctness: 1st. That in several instances the placenta has been spontaneously or artificially separated from the uterus before the birth of the child, and, consequently, under circumstances in which contraction of the uterus could not take place without any hemorrhage supervening; and 2dly, that when it has been attached to the os and cervix uteri its separation has been effected, in many cases, without any particular hemorrhage resulting, although it is affirmed by some anatomists that there are few or no contracting fibres in the structure of the os and cervix uteri. The last proposition affirmed, that hemorrhage from the utero-placental arteries is prevented by the changes in their tissues produced by the mechanical rupture of their coats, torn arteries being little or at all liable to bleed; and the placenta being separated by a true process of avulsion, was completely negatived by the author's experiments upon the pregnant bitch, for on detaching the placenta from the uterus, and thereby lacerating or tearing through the utero-placental arteries, arterial hemorrhage was actually observed to follow; that is to say, having separated the placenta by a true process of avulsion, and thereby having torn across the utero-placental arteries, it was demonstrated that such proceeding was not productive of those changes in their torn coats which are assumed to follow such operation, and by which it is alleged, arterial hemorrhage is prevented. Upon the whole, it appeared that two things were certain; first, that no necessary relation existed between the degree of hemorrhage, and the degree of separation of the placenta; or, secondly, between the degree of hemorrhage, and the degree of contraction of the uterus: uterine hemorrhage having been variously moderate or excessive under similar degrees of separation of the placenta, and similarly moderate or excessive under the opposite conditions of relaxation and contraction of the uterus. Could it, then, be doubted, that the absence or disposition to uterine hemorrhage depended, in many cases, upon other causes than the anatomical connection of the placenta with the uterus on the one hand, or the contractile mechanism of this organ on the other? Or, further, that these were to be sought for in the occurrence of arterial hemorrhage, and the varying conditions of the utero-placental arteries, as modified by the general condition of the arterial system. Bearing in mind this view of the case, the author maintained, that we could best account for the phenomena of puerperal hemorrhages. We could understand how it might happen, that the tonicity of the arterial system being great, uterine hemorrhage would be prevented when the uterus was most relaxed, and when, consequently,
venous hemorrhage would be most liable to occur—that, under the influence of morbid excitement of the heart and arteries, it might be profuse when the uterus was contracted, and when venous hemorrhage would be most effectually prevented; and that it might vary, in different cases, with the same amount of separation of the placenta.

Dr. Crisp was of opinion, that, in cases of partial separation of the placenta, the hemorrhage is not from the denuded uterus, but from the free portion of the placenta. He had been requested to see a case of Mr. Howell's in which serious hemorrhage had followed delivery, and, on examination, observed a portion of partially adherent placenta projecting from the os uteri, and was assured, from the touch, that the blood flowed from that structure. The placenta was entirely removed, and the hemorrhage ceased. He thought that abnormal position of the placenta frequently led to hemorrhage, and that the latter is quite independent of an enfeebled state of system. He attached but little importance to the experiments of Dr. MacKenzie.

Dr. Winn, on the other hand, believed that similar experiments performed on animals would ultimately set the question at rest, and, while agreeing in the author's results so far as to affirm that hemorrhage proceeds from the uterus, he differed with him in believing that the immediate source is the uterine sinuses, and not the uterine arteries. He thought that the distinction between the colour of arterial and venous blood, is sometimes liable to fallacy, since it is possible that the state of extreme nervous excitement of a patient in labour might render the venous blood of a more florid colour. A friend of his had observed, that on drawing blood from the arm of a patient labouring under pneumonia the colour was quite florid.

Dr. Murphy was quite confident, from his own experience, that, whatever may be the immediate source of hemorrhage in these cases, the flow of blood ceases on the entire removal of the placenta. He thought it important to separate these practical facts from any theories. He then referred to the two classes of opinions; the one, held by a section whom he termed the Protectionists, that it proceeds from the uterus, and the other, supported by the Reformers, that it flows from the placenta; and believe both to have built their theories upon hypothesis, and not upon experiment. He congratulated the author on having taken a first step in the required direction, and advised him to repeat his experiments again and again. He, however, cautioned him to bear in mind the great difference which exists between vital and dead structures, since, after death, the parts lose their tonicity, and any plugs which may
have once been formed remain, and prevent the passage of the injection; but, in the living structures, any new flow of blood may detach the plugs, and permit the vessels to pour out blood anew. The author had referred to Dr. Gooch’s case, in which no hemorrhage occurred, although the uterus remained of large size; and thought that so exceptional a circumstance could not support the deduction—that, therefore, the contraction of the uterus exercised no influence over the flow of blood.

Mr. Clark had assisted the author in his experiments, and bore testimony to the truthfulness of his descriptions, and the fairness of his deductions. He illustrated the latter by referring to those cases of post-partum hemorrhage in which the flow of blood begins long after the placenta has been expelled; and also by stating that, as the direction of the current is from the placenta to the uterine sinuses, the latter can supply but little, if any, blood in such cases.

Dr. Snow Beck was prepared to defend each of the author’s opinions seriatim; but stated, that the author’s facts might be admitted and referred to apart from his deductions. He did not think that the uterine veins could be the sources of hemorrhage, and had met with several cases of partially detached placenta, in which the hemorrhage did not cease on perfect separation of that organ. He also had met with a case so far resembling one mentioned by the author, that the colour of the blood was distinctly florid, and the source, as he believed, arterial, and the hemorrhage in that case diminished or increased with the state of contractility of the organ. He would make a distinction between the contractility of the uterus as a whole, and that of the walls of the organ, and believed that the flow of blood through the vessels is rather due to the latter circumstance. This state is induced when the uterus is manipulated either within or without; and in those instances in which the hemorrhage was arrested on detachment of the placenta, the arrest might have been due solely to the contraction induced by the manipulation.—[Med. Times and Gazette.

On Trismus Nascentium. By Nicholas Meriwether, M.D., of Montgomery, Ala.

This disease generally appears sporadically, but sometimes endemically, rarely attacking white children; which is to be accounted for by the superior cleanliness of the white race. To show the great prevalence of this disease in some portions of the Southern States, I will quote the following from a paper in the May number of the New Orleans Medical and Surgical
"The first form of disease which assails the negro race among us, is trismus. The mortality from this disease alone is very great. No statistical record, we suppose, has ever been attempted, but from our individual experience we are almost willing to affirm that it decimates the African race upon our plantations within the first week of independent existence. We have known more than one instance in which, of the births for one year, one half became the victims of this disease, and that too in Despite of the utmost watchfulness and care on the part of both planter and physician. Other places are more fortunate, but all suffer more or less; and the planter who escapes a year without having to record a case of trismus nascentium, may congratulate himself on being more favoured than his neighbours, and prepare himself for his own allotment, which is surely and speedily to arrive."

When this disease appears endemically on a plantation, it may be arrested by having the negro-houses whitewashed with lime inside and out; by raising the floors above the ground; by removing all filth from under and about the houses; by paying particular attention to cleanliness in the bedding and clothes of the mother, and in the dressing of the child so as to prevent any of the matter from the umbilicus lying long in contact with the skin of the latter. To effect this last, I usually slit a small piece of old linen, and, after greasing it, pass it between the abdomen of the child and the dressing usually applied to the umbilicus. This is to be renewed every day. The planter or overseer should be requested to examine and see whether there is any disturbance of the bowels for the first ten days after birth, so that the physician may have early notification. So much for prevention.

From the similarity of trismus to traumatic tetanus, it has been supposed that the disease is caused by absorption of pus by the umbilical vessels. I am inclined to that opinion, and base my treatment accordingly. In all the cases I have seen there was an unhealthy appearance of the navel, and disturbance of the bowels; the passages were generally greenish and ill-looking. When called early to a case (that is, as soon as there is spasmodic action in the muscles of the extremities and back, which appears usually before the affection which characterizes the disease as infant lock-jaw), I commence the treatment by giving the following mixture: B. 30 gtt. paregoric; 2 gtt. oil turpentine; 4 grs. gum kino; 1 teaspoonful prepared chalk, to be mixed in 8 teaspoonfuls of water; a teaspoonful to be taken every hour or two, taking care not to narcotize the child. If the disturbance of the bowels continues, or if the spasm of the muscles does not cease, I apply a blister immedi-
Strangulated Hernia. 675

ately over the navel; the blister should be circular, and larger than a dollar. With this treatment, I have rarely failed to arrest the convulsions, and save the patient.—[American Jour. of the Med. Sciences.

On the propriety of opening the Bowels soon after the operation for Strangulated Hernia. By J. S. Gamgee, Esq.

The various conditions in which we find cases of hernia at the time of operation, admit of their arrangements under four heads. 1. Those cases in which the intestine is in good condition, and inflammation has not yet manifested itself; 2. Those in which, though the bowel is in very fair condition, there are local and general signs of a moderate amount of peritonitis; 3. Those in which the bowels are notably discolored, but of good consistence; peritonitis being intense; 4. Those in which there is threatening gangrene of the gut.

Let us consider the first class of cases. Since it is reasonable to suppose that in a case of strangulated hernia that has been operated upon, a part at least, of the uneasiness dependent upon the intestinal function is immediately due to its restoration, we should in this class of cases theoretically be disposed to promote the action of the intestines, when it does not occur spontaneously, a short time after the constriction has been removed. Such practice seems the more reasonable, in that there can be no fear of aggravating inflammation which does not exist, and in that there is reason to believe that the expulsion of the accumulated feces is tantamount to the exclusion of a possible, if not probable, cause of inflammation.

In the second class of cases, the first part of the argument used above applies, but not the second; for inflammation exists, and the question arises, whether the administration of purgatives, theoretically indicated by the necessity of restoring the intestinal functions, may not be productive of evil by aggravating the inflammation. From what I have seen, however, these fears would not disquiet me. I think there is more chance of the progress of inflammation being checked by the expulsion of irritating feces, and restoration of the gut to its function, than of its being aggravated by the stimulant action of the purgative.

In the third class of cases, in which the bowel is in moderate condition, but the peritoneum intensely inflamed, it is reasonable to believe that the constipation, although in great measure dependent upon the atony which has resulted from long inactivity, is likewise due to the disturbance of innervation incident upon the inflammation. It seems hence prudent to respect the
objections of those who allege that the inflammation may be aggravated by purgatives; but while antiphlogistics are being actively employed, there is no reason for objecting, if the bowels do not act, to enemata; the probabilities of their doing good are much greater than those of their possible perniciousness.

In the fourth class of cases, (threatening gangrene of the intestine,) inasmuch as there is more to fear from the action of the intestines, though it be but moderate, than from their activity, though it be extreme, enema and purgatives appear contra-indicated so long as there is reason to fear disorganization of the gut.

From the foregoing considerations, flow three rules for practice, in cases in which the operation of peritonitis is not followed by spontaneous action of the bowels.

1. When the condition of the gut is good and there is little or no peritonitis, an oleaginous enema should be given an hour or two after the operation, and repeated after three or four hours in case of failure, or a purgative exhibited by the mouth.

2. When the peritoneal inflammation is intense, even though the bowels be in fair condition, antiphlogistics must be perseveringly employed; and though a simple enema may be given in the first six hours, it is inadvisable to excite the action of the bowels until the next day, either by more active enema or purgatives by the mouth.

3. In the case of mortification threatening the gut, the bowels should be kept quiet by opium, and purgatives and enemata abstained from until the danger of perforation has passed.


On Hydrophobia as it occurred in France in 1852. By M. Ambrose Tardieu.

In the year 1850, the minister of agriculture and commerce, on the recommendation of the committee of public health, sent a circular to every prefect in France, requesting him to give information regarding any cases of hydrophobia which might occur in his department. A number of reports were in consequence sent in, but as these were in some respects incomplete, a fresh circular was issued, detailing more particularly the manner in which the cases should be recorded. From the information so obtained, M. Tardieu drew up a report regarding the cases which occurred in the years 1850–51, as well as in 1852. As the report for the year 1852 is much more complete than the others, we subjoin an abstract of it.

1. The number of cases of hydrophobia which occurred in France during the year 1852, was 48. These were observed
in 14 departments: the department in which the greatest number occurred was that of the Hautes Alpes, (in the south-east of France, latitude between 44° and 45°); while the department of Lozère (also in the south, and having the same latitude as the other) came next.

2. With regard to the sex; 36 of the 48 cases were males, 12 females; the proportion in the two preceding years was almost the same.

3. The following table exhibits the ages of the subjects affected with hydrophobia:

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<th>Below 5 years, in 1852</th>
<th>3 in two former years</th>
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<td>Above 70</td>
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| 48                   | 88 = 136             |

This table shows the incorrectness of the opinion which ascribes the disease to the effects of terror, for it shows that 7 children under five years of age have been attacked.

4. All the cases which occurred in 1852 originated in the bites of dogs, except one, where the bite of a cat was the cause of the disease.

5. The situation of the wounds inflicted by the rabid animals was as follows in 48 cases:—On the face, 13 times; on the upper extremities, 15; on the lower extremities, 12; not mentioned, 8. In two of the cases the disease was communicated by pet dogs which were accustomed to lick their master’s faces, and where excoriated lips were the seat of the inoculation.

6. In 40 out of the 48 cases the date of the inoculation has been observed. It occurred in March, April and May, in 10 cases; in June, July, and August, 16; in September, October, and November, 4; in December, January, and February, 10.

7. It seems a considerable number of individuals who are bitten by rabid animals escape the disease. During 1852 some observations were made on this point, and it appeared that out of 44 persons bitten, about the same time 23 only were attacked.

8. The period of inoculation of the disease was exactly noted in 20 cases. It was as follows: Less than a month in 8 cases: from 1 to 3 months 10; from 3 to 6 months 1; 11 months, 1.

9. The duration of the disease in 20 cases was 2 days in 6 cases; 3 days in 8; 4 days in 5; 6 days in 1.
10. The termination of confirmed cases of hydrophobia was constantly fatal. Of the 48 cases, it appears that only 27 came under this category; in the others the effect was merely local. In 12 of these 27 cases no precaution was taken, in 4 no mention is made of this circumstance. In 8 of the remaining 11 cases cauterization was resorted to immediately, in 3 at a late period. Of the 21 individuals who escaped (see § 7) cauterization was energetically performed in 12 cases; the details of the other 9 have been omitted.

11. As to the mode of cauterization employed, the actual cautery was used in all cases but 5, and these were treated by protonitrate of mercury, nitric acid, ammonia or butter of antimony. In Germany it has been proposed to excise the bitten parts and then to wash the wounds with a solution of caustic potash.—[Annales d'Hygiene. Peninsular Journ.

Placenta Prævia. By O. H. Taylor, M.D., of Camden, N. J.

In examining the recent medical report from Gloucester County, N. J., I observe that Dr. Sickler has presented, with some interesting remarks, two cases of presentation of the placenta, in each of which the placenta was spontaneously delivered before the birth of the child.

The question, whether the result so happily effected by nature in these instances, should ever be promoted by art, is one that has been canvassed of late, by obstetricians, with deep interest and anxiety. Nor is this surprising, when we consider the extreme urgency of the dangers threatening the mother from hemorrhage, and the vital importance of the function of the placenta, even during labor, if at all protracted.

From my own observation and reflection on several cases of attachment of the after-birth over the os uteri, coupled with severe hemorrhage, which have occurred in my practice within the last twenty-eight years, I am induced to coincide with Dr. Sickler in opinion, not only as to the propriety and safety, but the absolute necessity, in certain circumstances, of removing the placenta before the delivery of the child. That the established method of turning the infant in cases of severe hemorrhage, during parturition, may be impracticable, without great delay, when the os uteri is but slightly dilated, and presents rigid and unyielding edges, every experienced practitioner is aware. The strength of the mother may be fatally exhausted before the hand can be introduced. Even in the most favorable cases, the condition of the placenta during the delivery, with much of its surface still adherent, and preventing the contraction of the bleeding vessels beneath the detached
portions, is incalculably more favorable for an arrest of hemorrhage, than if it were entirely detached; and the question of danger to the child, from the removal of its external lungs before its mouth reaches the atmosphere, is one of time only. If there be a possibility of a rapid delivery, we should not hastily allow this danger to prevent us from giving the greatest possible security to the patient.

Three cases of placenta implanted over the os uteri, occurred to me in the early part of my practice, and their history proves how fatal may be the results of uterine hemorrhage, even during the act of turning and delivering the child. In each of these cases, the operation of perforating the after-birth was performed in the presence and by the counsel of the late distinguished professor James, of the University of Pennsylvania; Dr. Charles D. Meigs, now Professor of Obstetrics in Jefferson Medical College, being also present in consultation in one of these cases. All these cases terminated fatally in a very short time after the delivery of the infants, which were still-born. Neither of the mothers survived more than three hours, and all died from the exhaustion caused by excessive hemorrhage.

That the operation of removing the placenta immediately, when found to present itself at the os uteri, is capable of being performed with safety to both mother and child, under favorable circumstances, I am fully convinced; but as Dr. Sickler very pertinently remarks, "the advisability and determination of the conditions in which it should be practiced, must be decided by an induction from a larger number of cases than have as yet been submitted to the profession."

My own experience leads me to the conclusion, that when the placenta is fairly implanted over the os uteri, and is firmly attached throughout its borders, it can be removed before delivery, with more safety both to the mother and child, than can be secured by perforating or elevating the edges, and bringing the child down by the feet.

In nearly all the cases that I have witnessed, expulsive and efficient pains have been brought on soon after the placenta has been completely detached; and these pains have continued so as to produce the prompt expulsion of the child whenever the presentation has been natural.

Even when the complication is coupled with a preternatural presentation, it does not appear to me that the immediate delivery of the placenta is necessarily contra-indicated. It would be folly to dwell upon the imminent danger to the child in such cases; and if the hemorrhage be permitted to continue unchecked during the protracted delivery by the feet (supposing
this to be possible in the case) what will be the fate of the mother? Should we not give her the advantage of that arrest of hemorrhage which appears so generally to follow the entire detachment of the placenta, even before its positive expulsion? Certainly we should, at least whenever the presentation or the condition of the patient can be ascertained to be such, that very prompt delivery by the feet is impracticable; for then the death of the child is insured at all events; and as has been already hinted, it is precisely when such prompt delivery is most easily effected, that the danger to the child from the previous detachment is least, and the advantage to the parent, I think, indisputably the greatest.

I throw out these few suggestions to the profession, in the hope of inducing the report of every fact which may tend to decide one of the most important practical questions which has been mooted by obstetrical practitioners for many years.—Uterine hemorrhage has long been the terror of both patient and physician, and any thing which tends in the least degree to lessen its dangers, is worthy of the most profound respect and serious consideration.

Permit me, then, to offer, in concluding this note, the abstract of a recent case, in which the effect upon uterine hemorrhage produced by the expulsion of the placenta before the delivery of the child, is happily illustrated.

On the 27th of April, 1853, I was requested to visit Mrs. B., on account of a profuse and unnatural flow of blood from the uterus. She considered herself as being eight months advanced in pregnancy. At the time of my first visit, she was not complaining of much pain, though the hemorrhage was very considerable in amount.

A digital examination proved that the os uteri was but slightly dilated. I directed pulv. acetat. plumbi et opii, but found it necessary, also, to apply the tampon, by means of which the bleeding was restrained, and I was enabled to leave my patient in an hour.

After the lapse of eight or ten days, the flooding recurred, and the same treatment was repeated, with a similar result.

On the 15th of May following, being the 19th day after the first attack, I was again summoned to the case, and found the patient laboring under a hemorrhage quite as profuse as at the time of the first visit, or even more so. By examination per vaginam, I found the os uteri dilated about two inches. The placenta was evidently implanted immediately over it. By pressing the index finger firmly towards the right ilium, I detached a portion of the adherent placenta, and was able distinctly to recognize the presenting portion of the foetus, which proved to be the head.
Periodic pains of the regularly expulsive character, were now established, but each was accompanied with an excessive discharge of blood.

The imminency of the danger to the mother from hemorrhage, induced me to decide that nothing would be forfeited by following any plan of action calculated promptly to arrest it. I therefore proceeded to separate the attachments of the placenta to the uterus in the hope that possibly I might be able to thrust the after-birth back from the orifice, and thus enable the head to engage itself in the superior strait. On making the attempt to push the placenta beyond the presenting part of the head, however, I found myself opposed and thwarted by the descent of a large portion of the mass into the vagina. The result accorded with the experience adduced from the history of other and parallel cases; for two or three more pains completely expelled the placenta into the vagina, and the hemorrhage then instantly ceased. In five or six minutes more the child was born, and although reasonably a little languid at first, it soon began to cry, and has since been a healthy and promising child.—[New Jersey Medical Reporter.

New mode of Treatment for Delayed Non-Union of a Fractured Humerus. Read before the Medical Society of the County of Erie. By Frank H. Hamilton, M. D.

It has been observed by surgeons that non-union results more frequently after fractures of the shaft of the humerus, than after fractures of the shaft of any other bone. This observation is confirmed by my own researches.

Comparing the humerus with the femur, between which, above all others, the circumstances of form, situation, &c., are most nearly parallel, and in both of which non-union is said to be relatively frequent, I find that of forty-nine fractures of the humerus, four occurred through the surgical neck, twelve through the condyles and twenty-nine through the shaft. In one of the twenty-nine, the patient survived the accident only a few days. In four of the remaining twenty-eight, union had not occurred after the lapse of six months, and in many more was it delayed considerably beyond the usual time. Two of the four were simple fractures, and occurred near the middle of the humerus; the third was compound, and occurred near the middle also; the fourth was compound, and occurred near the condyles.

This analysis supplies us, therefore, with four cases of non-union, from a table of twenty-eight cases of fractures through the shaft.
Of eighty-seven fractures of the femur, twenty occurred through the neck, one through the trocanter major, and one through the condyles. The remaining sixty-five occurred through the shaft and generally near the middle, and in not one case was the union delayed beyond six months.

To make the comparison more complete, I must add that of the twenty-eight fractures of the shaft of the humerus, six were compound; and of the sixty-five fractures of the shaft of the femur, six were either compound, comminuted, or both compound and comminuted. The six compound fractures of the shaft of the humerus, furnished two cases of non-union. The six cases of either compound or comminuted, or compound and comminuted fractures of the femur, furnished no case of non-union.

I beg to suggest to the Society what seems to me to be the true explanation of these facts.

It is the universal practice, so far as I know, in dressing fractures of the humerus, to place the forearm at right angles with the arm. Within a few days, and generally, I think, within a few hours, after the arm and forearm are placed in this position, a rigidity of the muscles and other structures has ensued, and to such a degree, that if the splints and sling are completely removed, the elbow will remain flexed and firm; nor will it be easy to straighten it. A temporary false ankylosis has occurred, and instead of motion at the elbow joint, when the forearm is attempted to be straightened upon the arm, there is only motion at the seat of fracture. It will thus happen that every upward and downward movement of the forearm will inflict motion upon the fracture, and inasmuch as the elbow has become the pivot, the motion at the upper end of the lower fragment will be the greater in proportion to the distance of the fracture from the elbow joint.

No doubt it is intended that the dressings shall prevent all motion of the forearm upon the arm; but I fear that they cannot always be made to do this. I believe it is never done when the dressing is made without angular splints, nor is it by any means certain that it will be accomplished when such splints are used. The weight of the forearm is such when placed at right angles with the arm and encumbered with splints and bandages, that even when supported by a sling, it settles heavily forwards, and compels the arm dressings to loosen themselves from the arm in front of the point of fracture, and to indent themselves in the skin and flesh behind. By these means the upper end of the lower fragment is tilted forward. If the forearm should continue to drag upon the sling, nothing but a permanent forward displacement would
probably result. The bones might unite, yet with a de-
formity.

But the weight of the forearm under these circumstances is
not uniform, nor do I see how it can be made so. It is to the
slings that we must trust mainly to accomplish this important
indication. But you have all noticed that the tension or rela-

tion of the sling depends upon the attitude of the body, whether
standing or sitting—upon the erection or inclination of the
head—upon the motions of the shoulders, and in no inconsider-
able degree upon the actions of respiration. Nor does the
patient himself cease to add to these conditions by lifting the
forearm with his opposite hand whenever provoked to it by a
sense of fatigue.

This difficulty of maintaining quiet apposition of the frag-
ments while the arm is in this position, at whatever point the
arm may be broken, becomes more and more serious as we de-
part from the elbow joint, and would be at its maximum at the
extreme upper end of the humerus, were it not that here a
mass of muscles, investing and adhering to the bone, in some
measure obviates the difficulty. Its true maximum is therefore
near the middle, where there is less muscular investment, and
where, on the one hand, the fracture is sufficiently remote from
the pivot or fulcrum to have the motion of the upper end of
the lower fragment multiplied through a long arm, while on
the other hand it is sufficiently near to the armpit and shoulder
to prevent the upper portion of the splint and arm dressings
from obtaining a secure grasp upon the lower end of the upper
fragment.

It must not be overlooked that the motion of which we speak
belongs exclusively to the lower fragment, and that it is always
in the same plane, forwards and backwards; but especially
that it is not a motion upon the fracture as upon a pivot, but a
motion of one fragment to and from its fellow. This circum-
stance I regard as important to a right appreciation of the diffi-
culty. Motion, alone, I am fully convinced, does not so often
prevent union as surgeons have generally believed. It is ex-
ceedingly rare to see a case of non-union of the clavicle. Of
forty-seven cases of fracture of the clavicle which have come
under my observation, and in by far the greater majority of
which considerable overlapping and consequent deformity has
resulted—of this number only one has resulted in non-union,
and in this instance no treatment whatever was practiced, but
from the time of the accident the patient continued to labor in
the fields and hold the plow as if nothing had occurred. I
have therefore seen no case of non-union of the clavicle where
a surgeon has treated the accident. Indeed, what is most re-
markable, its union is more speedy, usually, than that of any other bone in the body, of the same size. Yet to prevent motion of the fragments in a case of fractured clavicle with complete separation and displacement, except where the fracture is near one of the extremities of the bone, I have always found wholly impracticable. Whatever bandages or apparatus I have applied, I have still seen always that the fragments would move freely upon each other at each act of inspiration and expiration, and at almost every motion of the head, body or upper extremities. It is probable, gentlemen, that you have made the same observation.

From this and many similar facts I have been led to suspect, for a long time, that motion has had less to do with non-union than was generally believed.

I find, however, no difficulty in reconciling this suspicion with my doctrine in reference to the case in question; and it is precisely because, as I have already explained, the motion, in case of a fractured humerus, dressed in the usual manner, is peculiar. In a fracture of the clavicle through its middle third (its usual situation) the motion is upon the point of fracture as upon a pivot; although, therefore, the motion is almost incessant, it does not essentially, if at all, disturb the adhesive process. The same is true in nearly all other fractures. The fragments move only upon themselves, and not to and from each other. I know of no complete exception but in the case now under consideration.

Aside of any speculation, the facts are easily verified by a personal examination of the patients during the first or second week of treatment, or at any time before union has occurred, both in fractures of the humerus and clavicle. The latter is always sufficiently exposed to permit you to see what occurs, and as soon as the swelling has a little subsided in the former case, you will have no difficulty in feeling the motion outside of the dressings, or perhaps in introducing the finger under the dressings sufficiently far to reach the point of fracture. I believe you will not fail to recognize the difference in the motion between the two cases.

Such, gentlemen, is the explanation which I wish to offer for the relative frequency of this very serious accident—non-union of the humerus.

I know of no other circumstance or condition in which this bone is peculiar, and which therefore might be invoked as an explanation. Overlapping of the bones, the reason assigned by some writers, is not sufficient, since it is not peculiar. The same occurs much oftener, and to a much greater extent, in fractures of the femur, and equally as often in fractures of the
clavicle; yet in neither case are these results so frequent. Nor can it be due to the action of the deltoid or of any other particular muscles about the arm, whether the fracture be below or above their insertions, since similar muscles, with similar attachments on the femur and on the clavicle, tending always powerfully to the separation of the fragments, occasion only deformity, but not non-union.

If I am correct in my views, we shall be able sometimes to consummate union of a fractured humerus where it is delayed, by straightening the forearm upon the arm, and confining them to this position. A straight splint, extending from the top of the shoulder to the hand, made of some firm but moulding material, and made fast with rollers, will secure the requisite immobility to the fracture. The weight of the forearm and hand will only tend to keep the fragments in place, and if the splint and bandages are sufficiently tight, the motion occasioned by swinging the hand and forearm will be conveyed almost entirely to the shoulder joint. Very little motion indeed can in this posture be communicated to the fragments, and what little is thus communicated, is a motion which experience has elsewhere shown not disturbing or pernicious, but a motion only upon the ends of the fragments as upon a pivot.

I do not fail to notice that this position has serious objections and that it is liable to inconveniences which must always, probably, prevent its being adopted as the usual plan of treatment for fractured arms. It is more inconvenient to get up and lie down, or even to sit down, in this position of the arm; and the hand is liable to swell. But I shall not be surprised to learn that experience will prove these objections to have less weight than we are now disposed to give them. Remember, the practice is yet untried—if I except the case which I am about to relate, and in which case, I am frank to say, these objections scarcely existed. The swelling of the hand was trivial and only continued through the first fortnight, and the patient never spoke of the inconvenience of getting up or sitting down, or even of lying down.

The following is the case to which I have just referred.

Michael Mahar, laborer, æt. 35, broke his left humerus just below its middle, Dec. 14, 1853. The arm was dressed by a skilful surgeon in Canada West, and who is well known to me as exceedingly "clever." After a few days from the time of the accident, "the starch bandage was put on as tight as it could be borne, and brought down on the forearm so as to confine the motions of the elbow joint."

Six weeks after the injury, Jan. 29, 1854, Mahar applied to me at the hospital. No union had occurred. The motion
between the fragments was very free, so that they passed each other with an audible click. There was little or no swelling or soreness. In short, every thing indicated that union was not likely to occur without operative interference. The elbow was completely anchylosed. His health was unimpaired.

I explained to my students what seemed to me to be the cause of the delayed union, and declared to them that I did not intend to attempt to reestablish adhesive action until I had straightened the arm. They had just witnessed the failure of a precisely similar case in which I had made the attempt without straightening the arm and without success.

Feb. 6, 1854. I had succeeded in making the arm nearly straight. I now punctured the upper end of the lower fragment with a small steel instrument, and as well as I was able, thrust it between the fragments. Assisted by Dr. Boardman, I then applied a gutta percha splint from the top of the shoulder to the fingers, moulding it carefully to the whole of the back and sides of the limb, and securing it firmly with a paste roller.

March 4th. (Not quite four weeks after the application of the splint.) I opened the dressings for the second time, and carefully renewed them. A slight motion was yet perceptible between the fragments.

March 18th. I opened the dressings for the third time, and found the union complete. This was within less than forty days.

The patient was now dismissed. On the 29th of April following the bone was refractured. Mahar had been assisting to load the “tender” to a locomotive. While the train was just getting in motion he was hanging to the tender by his sound arm when another laborer seized upon his broken arm to keep himself upon the car, and with a violent and sudden pull wrenched him from the tender and reproduced the fracture.

The next morning I applied the dressings as before, and did not remove it during three weeks, at the end of this time the union was again complete. The splint was, however, reapplied and has been continued to this time—a period of about six weeks.—[Buffalo Medical Journal.

Paralysis occurring during Gestation and in Childbed.

The Dublin Quarterly Journal of Medical Science (May, 1854) contains a very interesting article on this subject, with the particulars of 84 cases collected from various sources, by Dr. Fleetwood Churchill.

The following is a summary of these cases, with his remarks upon the more important points connected with them. The
number of cases he admits, however, are too few to justify decided conclusions from them:—[Am. Jour. of Med. Sci.

"Of the 34 cases, in 22 the attack occurred during pregnancy; in 12, either during or after labour.

"In 23 cases where it is mentioned, I find that with 10 it was their first child, with 1, the second; with 4, the third; with 2, the fourth; with 3, the fifth; with 1, the sixth; with 1, the thirteenth; and one had several children, but the number is not specified.

"Of the 34 cases, there were 17 of complete hemiplegia, and 1 partial; 4 of paraplegia, in 2 of which only one leg was affected; 6 of facial paralysis; 5 of amaurosis, and 3 of deafness; but in some of these latter local palsy were combined with the cases of hemiplegia. Of 14 cases of hemiplegia, in which the side affected is mentioned, I find that 11 were of the right, and 3 of the left side.

"Of the 34 cases, 4 died.

"It may be well, however, to consider these cases somewhat more closely, and for that purpose they may be divided into two classes, those which occurred during pregnancy, and those which were attacked during or after labour.

"Of the 22 cases in which paralysis occurred during pregnancy, 12 were examples of hemiplegia; 1 of paraplegia, which had occurred previously; 4 of facial paralysis; 2 of amaurosis; and 3 of deafness. There is no regularity as to the period of gestation at which the seizure took place, for of 13 cases in which this is mentioned, in 1 it occurred in the second month; in 1 in the third or fourth; in 1 in the fifth; in 1 in the sixth or seventh; in 3 in the seventh; in 2 in the eighth; and in 4 in the ninth month; from which it would seem, upon the whole, that it is in the latter months that pregnant women are most liable to the attack.

"Of 19 cases, 11 appear to have been cured before or by delivery, and in 8 the disease continued for a longer or shorter time afterwards.

"Of the 20 cases, only 1 died, and in this case it is evident that death was rather owing to disease of the brain, longer standing than the pregnancy, than to the paralysis which increased during that process; so that I do not think we can reckon it as impairing the comparatively innocuous character of these attacks during gestation.

"In 3 cases only was the paralysis preceded by convulsions. In most of the cases it does not appear that there were any premonitory symptoms, little or no headache, or any other circumstance calculated to excite apprehension until the paralysis supervened. The characteristics of the palsy resembled very closely those of similar attacks unconnected with pregnancy; the motor power was enfeebled or altogether lost; in some the sensibility was increased, diminished, or modified; but in others, I infer from the silence of the reporter, that it was little, if at all, changed from its natural condition. The intellect seems to have preserved its integrity in all the cases. A peculiarity of great interest in many of these cases, and to which I shall here by, is the presence of albumen in the urine, whenever it was carefully examined.
"The second class, consisting of 12 cases, is characterized by the attack occurring during or after labour. It is remarkable that in 3 cases only (Cases 23, 24, 25) did the paralysis take place during labour, and of these, 2 were cases of convulsions; in all the others it not merely succeeded labour, but in most cases after an interval sometimes considerable: for example, in Case 23, it took place on the first day after delivery; in Case 27, two days afterwards; in Case 32, three days; in Case 34, seven days; in Cases 26, 29, 31, eight days; in Case 30, ten days; and in Case 33, a month afterwards.

"Of these 12 cases, 5 were cases of complete hemiplegia; in 1 only the arm was affected; 1 was a case of complete paraplegia; in 1 the right, and in 1 the left leg only was paralyzed; 2 were examples of amaurosis; 1 of facial paralysis; and in 3 only of the cases of hemiplegia the face participated in the attack. In Dr. Levy's very remarkable case, the paralysis of the motor power of one side was accompanied by loss of sensibility on the other. In some of the cases the sensibility was diminished, in others unaltered, but in none increased. The phenomena of the disease were not peculiar: in the majority of the cases the attack occurred generally without warning, and without any obvious cause. In 2 cases convulsions terminated in amaurosis, but in Mr. Forrest's case the paralysis preceded the convulsions, and during the latter, the paralyzed limbs shared in the convulsive movements.

"The duration of the disease varied a good deal, the paralysis gradually subsiding in most cases: in Case 22, after several days; in Case 23, in six weeks; Case 31 recovered the use of the arm in a fortnight, but vision remained imperfect for some months; in Case 34, in a month; in Cases 28, 29, in two months; Case 22 recovered the power of walking in two months, but was then attacked by another disease which proved fatal; Case 26 left the hospital without improvement.

"In 3 cases death occurred: in Case 32, on the fourteenth day, and in Case 34, on the twenty-fourth day after the paralytic seizure. Dr. Ley does not mention on what day his patient died.

"I have already alluded to the fact that in most of the cases the attack occurred without warning, and without apparent cause. Some cause there must be, of course; but it is much easier, in most cases, to say what it is not than what it is. For example, in none of these examples except one, did it appear to depend upon any external influence—upon cold, exposure, violence, &c.—or upon mental distress; in few, if any, was there evidence of previous cerebral congestion, or disease of any other organ.

"It has been suggested that the palsy may be merely the termination of convulsions, and certainly some of these cases would seem to support this view; but if this were generally true, we should find convulsions more frequently preceding the paralysis; and, also, we should meet with more cases of convulsions terminating in paralysis; Now, in all the cases I have quoted, a large majority exhibited no convulsive movements at all, and, on the other hand, of all the cases of
convulsions related by Drs. Collins, and McClintock and Hardy, there is not a single instance of such a termination; we must therefore refer both convulsions and paralysis to some common or different cause.

"I have no doubt, as Dr. Romberg has observed, that in a number of cases, especially those which occur during gestation, the palsy is due to a reflex action from some organ or structure in a morbid condition, and in which the nervous system seems to be merely the channel of transmission, offering no central disorganization. In such cases the exciting cause may possibly be some injury or morbid condition of the generative organs, or perhaps merely a transient excitement, such as that of pregnancy. It is possible, also, that some of the instances occurring during gestation ought rather to be classed under the head of hysterical paralysis, as described by Drs. Laycock and Romberg; but it is not always easy to make the distinction.

"Obstruction of the arteries has been recently shown by Professor Simpson* to be an occasional occurrence in child-bed, either from arteritis, a coagulum, or a detached vegetation; and a degree of paralysis may be the result; but inasmuch as the death of the limb, and ultimately of the patient, is the direct consequence of such an occurrence, the history of the cases I have quoted removes from them the suspicion of being thus caused.

"It might naturally be supposed that the stress and exertions during labour, which give rise to such great congestion of the face and head by also occasioning congestion of the brain, might be considered one of the principal causes; but such a supposition is not borne out by facts, for, excluding the cases of convulsions, in only one case did the paralysis occur at the time of labour; in all the others it either supervened before labour, or subsequently, at a time when all such direct action must have ceased, and in some, after such an interval that we cannot suppose it even a remote effect of the parturient agony. On the other hand, when we remember the number of severe labours in which no such attack occurs, or compare its frequency with that of convulsions during labour, we can scarcely attribute much influence to this cause.

"Again, as we have seen, paraplegia has been attributed to severe and prolonged labour, and to the consequent mechanical pressure upon the nerves and muscles of the pelvis, and at first sight this seems an adequate and feasible explanation, and of which no one could deny the possibility; yet so far as our cases are concerned it can hardly have been so, for in all but one the labour was natural, easy, and not prolonged; in the exceptional case the patient had been delivered by the forceps: moreover, the period at which it occurred was too distant to justify our attributing it to this cause in the other cases. On the other hand, if we recollect the number of severe, prolonged, and instrumental deliveries which take place, without any such result, no example being recorded by Drs. Collins, McClintock, and Hardy, or with the exceptions I have quoted, in any of the reports of the British

and foreign hospitals, so far as I am acquainted with them, I think we must also reject this peculiarity of labour as a necessary or frequent cause.

"In two cases the attacks seem to have been connected with an anemic condition, consequent upon hemorrhage, either from the direct effect of a deficiency of the circulating fluid, or indirectly from the increased susceptibility of the nervous system, under these circumstances, to ordinary exciting causes. In another case paraplegia appeared to result from cold; but, in the majority of cases, as I have already observed, there was neither plethora nor anemia; neither exposure, want, injury, advanced age, mental distress, nor sudden shock; in short, there was no apparent cause.

"Unfortunately for the cause of science, there are very few post-mortem examinations on record, from which we might decide with some degree of certainty upon the nature of the affection. In all the slighter and more partial cases, life is preserved, and when death occurs in the more severe instances, permission to examine the body cannot always be obtained. Of the four fatal cases I have here detailed, two only were examined; in these, and I doubt not in the other two also, disease of the brain or its membranes existed. In Dr. Ley's case, he states that 'no positive disorganization of the brain could be detected. The ventricles, however, contained more than the usual serum; and there was found, more especially opposite to the original seat of pain, thickening and increased vascularity of the membranes, with moderately firm adhesions in some parts; in others an apparently gelatinous, transparent, and colourless deposit interposed between them.' In short, there appears to have been an attack of partial meningitis, and the contrast between the peculiar train of symptoms to which it gave rise, and the absence of all symptoms except the palsy in Dr. Duke's case, is very interesting, when we remember the remarkable disorganization we discovered in the latter case.

"Now in these cases we may fairly assume that the palsy and death itself were the result of the disease of the brain and its membranes, but to what are we to attribute the slighter and more numerous cases? Do they not appear to belong to the class described by Dr. Abercrombie, as 'depending upon a cause which is of a temporary nature, and capable of being speedily and entirely removed?'

"What is this temporary cause, producing so serious a disturbance, and yet scarcely, if at all, endangering life? May it be the one to which Dr. Latham refers, as observed 'in those convulsions and apoplexies which appear and disappear, the chief circumstance which attracts our attention being albuminous urine?' At any rate, it deserves our careful attention. Of the fact of the occurrence of albuminuria with certain affections of the nervous system during pregnancy and child-bed, there can be no doubt whatever. Both Drs. Lever and Simpson have detected it in cases of convulsions during pregnancy and labour; the former observes: 'I have carefully examined the urine in every case of puerperal convulsions that has since come under my notice, both in the Lying-In Charity of Guy's Hospital and in
private practice, and in every case but one the urine has been found to be albuminous at the time of the convulsions." 'I have further investigated the condition of the urine in upwards of fifty women, from whom the secretion has been drawn during labour by the catheter, care being taken that none of the vaginal discharges were mixed with this fluid; and the result has been that in no cases have I detected albumen, except in those in which there have been convulsions, or in which symptoms have presented themselves which are readily recognized as precursors of puerperal fits.' Dr. Simpson's observations about the same time, and those of more recent observers, Sabatier, Legroux, Richelot, and others, have confirmed the conclusions of Dr. Lever as to the presence of albumen in the urine in cases of puerperal convulsions, so that no doubt now exists as to the fact, although we occasionally meet with cases of convulsions without albuminous urine, and of albuminuria without convulsions.**

"Now, as paralysis in some cases occurs in connection with convulsions, if not as a consequence of them, we might, not unnaturally, expect albumen in the urine of such patients, and accordingly, in a patient of Dr. Lever's and in others, we find that it has been detected.

"But we may go a step further, and state that in cases where no convulsions have preceded the paralysis albuminuria has been equally observed. Dr. Lever says of his cases, that in none in which he examined the urine did he ever fail to find albumen, and the great experience of Professor Simpson is in close accordance with this, as may be seen by the quotations I have given, and by the cases with which he has favored me. This was observed also in Dr. Duke's case, where the paralysis succeeded the delivery; and in which I think there is ground for believing that the albumen had diminished at the time the urine was first examined. In all probability it would have been detected in many others, had an investigation been made.

"Thus we find that albuminuria may be a marked symptom in puerperal convulsions, whether terminating in paralysis or not; and in the palsy of pregnant and puerperal women, whether partial or complete, whether local or general; and if the observations are yet too few to draw any very positive conclusions, it is, I believe, because our attention has not been drawn to the subject. And when, in addition, we find, as Dr. Lever states, that as the albumen diminishes, the paralysis subsides, we can hardly doubt that there is some important connection between them.

* It may be of interest to append Dr. Seyffert's conclusions on this subject.

1. Albuminuria is not an essential accompaniment of normal, healthy pregnancy. 2. The theory, ascribing albuminuria to the pressure of the enlarged uterus on the renal vessels, is inadmissible. 3. When anasarca, from Bright's disease, occurs during pregnancy, the patients are seldom attacked by eclampsia. 4. The albuminuria, in cases of eclampsia, is occasioned by the interruption of the functions of the respiration and circulation by the attack. 5. In such cases the albuminuria terminates with the attack. 6. Albuminuria is not present in all cases of eclampsia. 7. Albumen is found in large quantities in the urine of epileptics, immediately after an attack; but not invariably after every seizure, or in every case of the disease. 8. Provided there be no Bright's disease, this albuminuria among epileptics ceases soon after the convulsions, and only returns after the next attack." — [Edinburgh Monthly Journal.]
"What, then, is the precise pathological significance of albuminuria? We may assume as established, that although it occurs in Bright's disease, it alone is no proof of the presence of that disease; but in the present state of our knowledge it is very difficult, perhaps impossible, to come to any very decided conclusion upon the matter. It is conceivable that an unusual, morbid, or noxious ingredient in the urine may be produced in either of three ways: 1. By simple elimination from the blood, in which it was present; 2. As the result of diseased action of the kidneys, excited either by some noxious principle in the blood, or by a morbid condition of these organs; or, 3. As a new compound, the result of chemico-pathological action, which we may or may not be able to explain.

"Now, albumen in the urine cannot be placed under the latter category, as it is not a new principle, but one already existing in the blood. Nor does it come under the first, for although it is possible that it might be eliminated from the blood in which it is present, it cannot be as a noxious element, nor would this simple elimination account for the condition of the kidneys or for the concomitant symptoms. So that it would appear this secretion of albumen must be owing to some disordered action of the kidneys, excited by some morbid element, in kind or degree, which they are endeavoring to separate from the blood. This seems at least to be the opinion of a high authority, Dr. George Johnson, of London, who, in describing acute desquamative nephritis, in which albumen is so largely secreted, observes: 'that all the changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products, some materials which do not naturally enter into the composition of the renal secretion.'* This view is further confirmed by a post-mortem examination into the state of the kidneys themselves in albuminuria. Dr. H. feld Jones, in a recent paper, has described three varieties: 'The first is the condition of engorgement, such as is seen in those who die in the early stages of acute anasarca, or in that of dropsy succeeding scarlatina. The organ is enlarged, dripping with blood in every part; its tissue not destroyed, but many of the tubes are seen, under the microscope, to contain coagula of exuded fibrin, entangling blood-globules, and more or less of epithelium.' 'The second form of diseased renal structure is that of the large, heavy, often mottled and pale kidney. In this there is no hyperemia, but rather the reverse state usually exists. The cut surface has not the appearance of healthy structure, and gives one the idea of some matter having been implanted among the natural constituents, so as to obscure them and to produce a confused aspect. The tubes are found impacted with epithelial matter, but not by any means constantly obstructed or blocked up, although they may be irregularly dilated, &c.' 'The third variety of morbid change is that so familiar to observation as the dwindled, glandular kidney.'†

"When we consider the temporary nature of the albuminuria in

* Diseases of the Kidney, p. 105.   † Medical Times and Gazette.
many of the cases of paralysis, we need have little doubt that the condition of the kidneys answers to the first variety here described, or that of extreme congestion, and this opinion is confirmed by the examination of Case 34, in which we found a high degree of congestion, which had indeed passed into a more advanced stage. I think, therefore, that we may fairly assume the albuminuria is due to a congested state of the kidneys, and I confess I cannot but think that the explanation given by Dr. G. Johnson and others, that this congestion is excited by the effort to eliminate some noxious element from the blood, is more in accordance with our present knowledge than any other, yet I must not omit to mention that by some this congestion has been attributed to pressure of the gravid uterus upon the renal vessels. Dr. Seyfert, as we have seen, rejects this mechanical explanation, and seems to attribute the albuminuria to the eclampsia, in consequence of the interruption of the functions of respiration and circulation.

"But, if the former theory be true, what is this morbid element, morbid in kind or degree? It is very difficult to answer this question. Dr. Simpson suggests that it may be an excess of urea or some morbid quantity or quality of caseine in the blood. Dr. George Johnson's observations seem to prove that in these cases, in addition to a change in the proportion of the normal constituents of the blood, of which the diminution of its albumen is one, there is always an excess of urea.

"Then it may be asked, 'to what is the effect upon the nervous system owing?' One can conceive that it may result either—1. From the continued presence of the noxious principle in the blood; or, 2. From the balance of the constituents of the blood having been destroyed; or, 3. From the diseased condition of the kidney—though to which of these we ought to attribute it, would be difficult to decide.

"But at whatever conclusion we arrive with respect to these interesting points, I am sure all will agree with me, that, taking the circumstances into consideration, it is probable the kidneys play a more important part in these paralytic affections than has been suspected, and that the subject deserves more attention than it has received. For, we find that in cases of convulsions terminating in paralysis, we may have albuminuria; in paralysis before delivery, without convulsions, we may have albuminuria; in paralysis occurring after delivery, we may have albuminuria; and further, that in the slighter cases, both the convulsions and paralysis diminish with the decrease of the albuminous secretion. Whether, therefore, the paralysis be caused by the state of the kidneys, or the renal congestion and paralysis be both the result of some morbid matter in the blood circulating through the system, it is clear that a new element may be added to those which have usually been considered as giving rise to paralysis.

"Nor is this barren theory only; but, if it be true, it has a direct bearing upon practice, inasmuch as our attention ought not to be confined to the secondary affection of the nervous system in such cases, but must be directed to the relief of the renal malady, and to the restoration of the kidneys to such a state of efficiency as may
enable them to remove the morbid constituents of the blood; and for our encouragement, we have seen that a diminution of albumen in the urine is followed by mitigation and cure of the paralysis. For the latter affection, bloodletting, general when the system will bear it, or local by means of leeches or cupping; blisters, purgatives, and mercury, are the remedies usually employed; these must be modified according to the condition of the patient, the circumstances of the attack, and the duration of the disease. When much blood has been lost during labour, bloodletting must be omitted, and we must confine ourselves to counter-irritation; perhaps a series of small blisters to the neck, down the spine, or along the limb, will be the best mode of proceeding. The patient's strength must be supported judiciously by good diet, and it is quite possible that some stimulant, such as ammonia or camphor, may be necessary. When the paralysis has become chronic, strychnia or galvanism may be found useful; and I believe Dr. Stokes has found galvanic acupuncture very beneficial in facial paralysis.

"The renal disorder should never be treated by diuretics, but by external irritants, such as mustard poultices, or rubefacient liniments to the loins, and internally by diaphoretics, as suggested by Dr. Osborne,* of this city, and when more chronic, by gallic acid, iron, &c."

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Treatment of Epilepsy.

Dr. R. B. Todd, in an interesting clinical lecture delivered at King's College Hospital, offered the following valuable practical observations on the treatment of epilepsy. In reference to the drugs which have been generally employed, he observes:

"Most of these have been used in consequence of their exhibiting a certain amount of physiological influence on the nervous system. As to others, it is difficult to trace the circumstances which led to their admission among the juventia in this disease. First in the list I would place those drugs which belong to the narcotic class—opium and its various preparations, belladonna, henbane, conium, camphor. These certainly exercise a very powerful influence in calming the excitable state of the nervous system which accompanies epilepsy. They are applicable chiefly to the more acute cases, and especially when epileptic delirium is threatened or present. You should never administer them except when you have full opportunity of watching your patient, or of confiding him to the care of another. This remark, I need hardly add, applies chiefly to Belladonna and the preparations of opium.

Belladonna has been greatly extolled by some French writers, far more than it deserves, as far as I can judge from my own experience. It is a remedy which leaves no permanent ill effect, and which, I think, is worthy of a more extensive trial in this country than it has yet received. That it exercises a physiological influence on the nervous system, as was formerly the opinion of many prac-

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system, no one can doubt, who has administered it for forty-eight hours; but as it induces a state of pupil very like that which is so common in epilepsy, one might fear that it rather favoured the epileptic state. A good series of clinical observations are yet wanting upon the effects of this drug. The preparation chiefly used in France is the powdered leaves of the plant.

Opium is often useful in cases in which the fit is apt to occur in the night or early morning. A full dose given at bedtime will prevent the development of the fit.

The valerian was used formerly in epilepsy more extensively than now. It is a medicine, however, which should not be despised or discarded. Its use promises most in the complications of hysteria and epilepsy. Its virtue depends upon the presence of an oil or acid which is capable of entering into combination with metallic bases, as zinc and iron, and which also combines with quinia and other alkaloids. You may give it either in scruple doses of the powder two or three times a day, or in infusion, or as an ammoniated tincture, or in combination with quinia, or the metals.

Other nervine remedies may be added to your list, and you should keep them in reserve, to employ as occasion may require, such as musk, castor, assafetida, stramonium, sagapenum, garlic, remembering the moral influence of a change of drug. I cannot say that the evidence of any special power of an anti-epileptic nature in any of these drugs is at all satisfactory.

In this class of drugs you may place the Sumbul, of which a tincture is prepared by Mr. Savory, of Bond-street. One of the earliest cases in which I employed it, seemed to benefit very decidedly by it; and I have since used it many times with unequivocal good, so as to lead me to look upon it as a useful stimulant, and antispasmodic remedy.

Digitalis has been greatly lauded by some. It may act favourably, partly by its diuretic properties, and partly by its influence on the heart’s action. There is quite sufficient evidence of its utility to warrant its being retained among the list of remedies applicable to this disease, and to justify its occasional cautious use.

The cotyledon-umbilicus may rank with digitalis as a remedy for epilepsy. I cannot say that I have met with a case distinctly benefited by its use; but the cases published by Mr. Salter, and those recorded by the late Dr. Graves, indicate that it possesses a certain antiepileptic power. I incline to think it acts by a diuretic influence.

There are other drugs obtained from the vegetable kingdom, which, I think, deserve a trial in epilepsy. One of these is the Achillæa millefolium, which, in the form of a decoction, exerts a very decided diuretic influence. On the same principle, taraxacum may do good in certain cases, as in those in which the epilepsy has relation to gout in the system.

The alkaline salts, especially the bicarbonate and the nitrate of potass, appear to me often to be very useful on a similar principle, namely, by increasing the activity of the kidneys.
The cardamine pratensis was greatly extolled by Sir George Baker. Turpentine has been used chiefly from its anthelmintic properties. It is a remedy that should be used very carefully, and never, if there be suspicion of organic disease of the brain.

The *selinum palustre*, or *peucedanum montanum*, is highly commended by Herpin, on, as I think, insufficient grounds. The plant is very difficult to procure, and I have not yet been able to try it.

The various metallic tonics have been and are extensively used in epilepsy. Of these, the safest is zinc, the sulphate, or the oxide. Either of these preparations may be given without any disadvantage that I know of, for a considerable time, and in large doses. Some time ago I gave to a patient in this hospital as much as half a drachm of sulphate of zinc, thrice a day without any sensible effect. The dose had reached that amount by gradual increase. Whether these drugs exercise any special favourable influence, I am unable to say; all I know is, that, under their long-continued use, patients sometimes cease to be troubled with fits.

The various salts of iron are also applicable to the treatment of this disease, and especially to cases in which the blood is poor in haematin. But they are not so harmless as the salts of zinc. Their tendency in some persons to excite headache precludes their use with them; and they often disturb digestion, and on this account cannot be persevered with. The saccharine carbonate, the sulphate, the citrate, and the tincture of the sesquichloride, as well as Griffith's mixture, are the preparations of iron which you will find most suitable to the generality of cases.

Salts of copper and of silver are also used for epilepsy, with, I think, at best, but very doubtful efficacy. To the cautious use of the ammonio-sulphate of copper, I see no material objection; in too large doses it may irritate the gastro-intestinal mucous membrane, but such irritation is easily removed.

There is, however, a much more serious objection to the use of the salts of silver. The nitrate and the oxide are the salts employed. Now, there is no doubt that the nitrate discolours the skin. We see repeated examples of this in the streets; scarcely a day passes that I do not recognize persons in the streets, as epileptics, who had been treated with nitrate of silver. If the nitrate of silver were a certain, or even a very frequent cure for this formidable disease, well and good; but, seeing that it is very far from being entitled to the credit of being such a remedy, I say, that we have no right to make blackamoors of our patients, and to stigmatize them forever as epileptics, and too often as epileptics uncured, on the mere chance of doing good by nitrate of silver. No doubt few would hesitate long between choosing a discoloured skin, and being the subject of a disease so fearful as epilepsy. But it seems to me, that the remedy ought to be a specific, to justify the practitioner in administering it, with the strong probability there is of producing such an effect. Remember, too, that the nitrate of silver stain communicated in this way, is probably indelible, and that, by an experienced eye, it is not to be mistaken. I have seen it after ten and fifteen years from the administration of the drug.
Finding, then, no special virtue in the nitrate of silver as against epilepsy, and that it is very apt to produce the serious result to which I have referred, with whatever care it may be administered, I have long since abandoned its use. I fear the oxide of silver is not more promising, either as to its influence on the disease, or its freedom from the discolouring power.

Indigo has likewise been given in epilepsy, but with no other result, as far as I can learn, than what Penal observed, that it turned the nails of his patient blue.

The time has, I think, now arrived, when we must look in another direction for an anti-epileptic remedy. We must turn our attention more particularly to those substances which are capable of being exhibited by inhalation, of being brought to bear upon the nervous system, without being subjected to the action of the gastric fluid.

Ether and chloroform, it is now well known, are capable of being taken into the blood in this way. Some years ago, I tried experiments as to their power of controlling the convulsions induced by strychnine, and with the result of finding that they possessed that power very completely. As long as an animal is fully under the influence of ether or chloroform, no convulsion can be produced. I was thus led to try their inhalation in epilepsy. I have chiefly employed for this purpose chloroform, because it is less disagreeable to patients to take. The results of my observations so far, are these—that in the more acute forms of epilepsy, the inhalation of chloroform has considerable influence in controlling and modifying the attacks; it has also a very decided power over epileptic delirium; and over some of the concomitants of epilepsy, as the violent convulsive jerks of the muscles of both upper and lower extremities, which are always most distressing, and often very dangerous. It has a marked and immediate power over puerperal convulsions; and I am glad to see that my friend, Prof. Simpson, of Edinburgh, sanctions, by his high authority, its use in such cases. It is also applicable, and with advantage, to the convulsions of infants, and to laryngismus stridulus.

There is no use in employing chloroform in epilepsy unattended by other symptoms, when the attacks are at uncertain times and at long intervals; nor do I advise you to attempt its use, unless the patient is fully under your control, or will heartily co-operate with you. And in cases where the heart is affected, either functionally or organically, it should never be used. Its inhalation, at two or three stated times of the day, exercises a very calming effect on the nervous system, diminishing its excitability very materially. But this must be carried on over a long period of time—many months, and even years; and the dose may vary from twenty to sixty minims, according to the effect produced, and you should aim at obtaining a gradual, not a rapid effect. The patient should always remain in the horizontal posture during the inhalation, and for at least half an hour after it.

You will expect me to say something on a proposal made within the last few years to open the trachea of patients suffering under severe epilepsy, with the twofold object of preventing the fits, or of
obviating their effect, in creating undue distension of the cerebral
bloodvessels, if they should occur.

Before a proposition of this kind can meet with general favour
from practical men, it must be satisfactorily proved that the hypo-
thesis, or theory, if you will, from which the proposal emanates, is
essentially sound: and next, it must be shown that the operation is
in itself one that is not seriously dangerous to life; and lastly, it
ought to be in the highest degree probable that the attacks will not
recur after the operation has been performed. It is, as you know,
contrary to all principles of sound surgery to perform an operation,
unless it be for the complete removal of the disease, or, at worst, to
stave off the reappearance of the malady to the latest period.

The hypothesis from which this proposal emanates assumes that
the exciting cause of all the evil in severe epileptic paroxysms, is the
contraction of the muscles of the neck and glottis, which induces and
maintains a congested state of brain, which, in its turn, excites and
keeps up the convulsions. I have elsewhere examined this congest-
tion theory in detail, and I am not aware that the arguments which I
then advanced against it have ever been fairly met. I have also
shown, by experiment, that epileptic convulsions may be excited in
dogs which had been freely tracheotomized previously, full provision
having been made for the free ingress and egress of the air; and
also in a dog in which the muscles of the glottis were paralyzed by
section of the recurrent nerve. These experiments, the advocates of
tracheotomy in epilepsy have found it convenient to ignore.

Again, I would ask, is tracheotomy either a very simple or a very
safe operation? I do not think that the results of the operation
either for other diseases, or for epilepsy, are very satisfactory. Pa-
tients often die from the effects of the operation; exhaustion, erysipe-
las, diffuse inflammation of the areolar tissue spreading into the me-
diastinum.

And lastly, what are the prospects that, the operation having been
done, the evil will not recur? This can only be ascertained by that
which I cannot recommend, frequent experiments. I would only
make this remark: that those who have the courage to try these ex-
periments, and feel themselves justified in so doing, must carefully
watch the results, and endeavour to draw a proper distinction between
the actual physical effects of the operation, and that mental influence
which, as I have shown you in a former part of this lecture, undoubt-
edly tends to stay the disease, when any new treatment is adopted, or
any considerable change takes place in the patient’s position and
circumstances.

But, gentlemen, let us look on, and watch the result of these oper-
ations in the hands of others. No one will be more ready to acknow-
ledge himself in the wrong than I shall be, should it turn out that, in
this proposal, an important remedy has been found for a most terrible
malady.

In conclusion, I have to express my fears that I have drawn but a
sorry picture of the power of our art to deal with this formidable
disease. But let not this discourage you; and do not fall into the notion, nor countenance it in any way, that epilepsy is an incurable malady, and that epileptic patients may as well be abandoned to their fate. There is no doubt that some may be completely cured, many very greatly alleviated, and that all should, as far as possible, be the objects of medical scrutiny and care, with at least the object of finding out more and more of the natural history of the malady, and of the juvantia as well as the ledentia; and with a hope that, at some time or other, a remedy may be vouchsafed to us, or at least that we may gain some insight into the intrinsic nature of this formidable scourge of mankind.—[Med. Times and Gazette.

EDITORIAL AND MISCELLANY.

Remarks upon the use of Beverages in Sickness. By the Editor.

Without intending for a moment to undervalue the importance of a judicious selection of the more active remedial agents in the treatment of disease, the writer nevertheless feels persuaded that much of the success of these, very often depends upon the use of proper adjuvants. The signal advantages frequently derived from the opportune administration of an enema, a foot-bath, cold affusion to the head, or even a cup of tea, broth, or gruel, must have been obvious to every discerning practitioner. And yet, it is only at the bed-side that the young physician can derive much information upon the subject, as these matters of detail cannot be or are not included in such works of general practice as are usually placed in their hands. Treatises and Lectures upon the general principles of Practice are unfortunately but little relished by students, while they read and listen with avidity to specific plans of treatment, and never fail to note down any recipe that may be proposed. The more violent, heroic and perturbing methods are, however, gradually giving way to milder and more judicious medication; and palliatives consequently increase in importance. The skill of the practitioner will be found to consist more in the relief of existing symptoms, than in the prescription of special formulae learnt by rote and aimed at a name.

The use of aqueous beverages, especially in acute affections, is now so common that it cannot be a matter of indifference whether the patient partake of the one or the other of the many varieties ordinarily resorted to. The belief that the water they contain is the sole agent of value in their administration, is too exclusive and prevails to too
great a degree. By the ingestion of large quantities of water, and the great facility with which it is imbibed by the coats of the stomach and intestines, carried into the portal system, and from thence introduced into the general circulation, the blood is diluted and rendered less plastic, whilst the repletion of the vessels thus induced, gives increased activity to the emunctories—viz., the skin, lungs, and kidneys. The experiments of Magendie demonstrate very satisfactorily that the secretions are increased in a direct ratio with the repletion of the blood vessels, and vice versa; that absorption is promoted in proportion to the diminution of the circulating mass. While, therefore, in the treatment of acute diseases, which are generally inflammatory, copious beverages are usually found to be useful, by diminishing the plasticity of the blood and promoting the elimination of noxious or effete principles, their propriety is very questionable when it becomes necessary to favor absorption, as is frequently the case in chronic engorgements, serous effusions, or other deposits. When venesection is practiced, the volume of blood abstracted is very soon replaced by water; whereas, by withholding such beverage, the partial vacuum of the vessels brings into them the circumjacent fluids with whatever disintegrated matters they may hold in solution. Thus it is that we may satisfactorily account for the agency of depletion and abstinence in the promotion of absorption. Yet it cannot be a matter of indifference whether the drink be acid or alkaline, stimulating or sedative, mucilaginous or acid, laxative or astringent, nutricious or not. We resort daily to beverages which, in addition to the diluent property of water, unquestionably present one or more of the peculiarities just referred to; and we should endeavor to select such as may be best adapted to each particular case. A brief enumeration of some of those in common use, and an appreciation of their peculiarities, may enable us to present our views more forcibly. They may be advantageously arranged under distinct heads indicative of their most prominent properties.

Diluents.—Of all beverages, water, at the ordinary temperature of spring or well water, will be generally found the most agreeable, as well as the best, when the desired effect be simply to allay thirst or to dilute the blood. Indeed, the cravings of nature so strongly indicate the propriety of cold water as a beverage in the fevers of our climate, that one cannot look back without a sense of horror upon the time when patients were pertinaciously denied this luxury, notwithstanding their heart-rending entreaties; when they were compelled to linger through long attacks of sickness with parched lips and crack-
ed tongue, lest a sip of cold water might perchance disagree with the stomach, check the perspiration, or expose them to mercurial salivation! In no particular has modern practice displayed more good sense and humanity, unless it be in the abolition of chains and the lash in the treatment of Insanity, than in allowing the sick the free use of cold drinks, especially in Southern fevers. A draught of good cold water will often act like a charm, quieting the stomach, and inducing copious excretions from the skin, kidneys and lungs.

The facility with which ice is now procured in most of our towns has led to the very free use of iced water; and, however grateful and beneficial this may be in many cases, there are circumstances in which the propriety of its use is at least questionable. In irritability of the stomach, with or without phlogosis of this viscus, iced water very generally gives great relief; but in affections of the bowels we think it decidedly objectionable. In both diarrhœa and dysentery, its bad effects are almost immediately marked by the supervision of pain and a desire to go to stool. It should also be avoided in all colicky affections, whether partaking of the nature of obstructions, of spasms or of flatulency. In bowel affections we always give the preference to warm or hot drinks. According to our bed-side observations, iced beverages should be also avoided in pulmonary diseases and in affections of the head. We have frequently found them to induce paroxysms of coughing and dyspnœa in lung complaints, as well as pain and cerebral disturbance in affections of the brain, while tepid or warm drinks do not produce such effects. The rationale of such consequences is so evident as scarcely to need an explanation. The principle is here the same as that upon which we account for the injurious effects resulting from the exposure of one part of the body to cold when another part is predisposed to or actually suffering from inflammation. No one would think of plunging in iced water the feet of a patient laboring under affections of the bowels, thorax, or head; nor should the stomach be filled with iced water under such circumstances, although this organ may be benefitted by cold applications of the kind to its own surface when this is affected. The same remarks may be applied to acute affections of the skin, and old women are therefore not wrong in objecting to iced drinks in scarlatina, measles, and smallpox, however much they may err in insisting upon keeping the body excessively warm.

In the cold stage of our fevers we think warm drinks preferable to cold ones. They hasten the termination of the chill and bring on perspiration much sooner; and though they may be more apt to induce
emesis, the very efforts to vomit materially determine the circulation to the surface and consequently abridge the cold stage.

**Demulcents.**—Under this head we may place all the mucilaginous infusions, as those of Flaxseed, Slippery-elm bark, Prickly-pear, Bene leaves, Gum arabic, &c. These are nothing more than diluents in combination with bland materials. They are regarded as especially appropriate in irritations, more or less intense, of the alimentary passages, of the respiratory organs, and of the urinary apparatus. Their use has been so long sanctioned by the Profession, that it is not without some hesitation that we intimate a doubt as to their real value, or rather as to their superiority over mere diluents. It can hardly be presumed that the gummy or mucilaginous materials they contain, pass into the circulation unchanged or without previously undergoing the digestive process. They cannot therefore be viewed as bland applications to any other than the surfaces of the digestive tube. Yet they are continually prescribed as though they were destined to reach unchanged the mucous surfaces of the lungs and urinary organs. We must confess that we have ourselves been so much in the habit of prescribing infusions of Slippery-elm and of Prickly-pear in affections of the kidneys, bladder and urethra, that we would dislike to abandon them, however much we may be led by theory to doubt their intrinsic efficacy and to attribute the relief to the water and other medicinal agents with which they are administered. We must also say that we, have never perceived any advantage in the use of demulcents, as such, in pulmonary diseases,—and that we really consider the one in most common use (flaxseed tea) often injurious, in consequence of the rancidity of the seed usually obtained from the shops, and the indigestibility of the infusion when made very mucilaginous, to say nothing of the unpleasantness of the dose. The other demulcents can be so readily procured in a fresh state, and are so much more agreeable, that we see no good reason for the very general use made of flaxseed tea.

The **Aromatic** beverages are infusions of Mint, Balm, Sage, Catnip, Sassafras, &c. Their chief merit consists in being generally palatable and therefore well received by the stomach. In many instances they will relieve nausea, when this unpleasant symptom would be aggravated by demulcents. They are also decidedly anti-septic, preventing the evolution of gas by averting the tendency to fermentation, and improving the general tone of the digestive organs, without exerting injurious stimulation of the general system. They are particularly well adapted to Typhoid fevers and diseases of similar character.
Catnip tea is a favorite prescription of mothers for crying babes, under the impression that the cries always indicate the existence of colic and that catnip is a specific for this. It cannot be denied that the little creatures very frequently become quieted and go to sleep shortly after partaking freely of the well sweetened tea; but whether this effect is to be attributed to relief from colic, to some anodyne or soporific property of the tea, or simply to the fact that this operates as a substitute for the nourishment the child required, remains to be determined.

Sassafras tea is not unfrequently used in the South as a substitute for Coffee and Hyson tea, and is certainly more palatable than either of these, when as wretchedly prepared as they are in many families. Sassafras has been long supposed to possess alterative properties, and has therefore entered into the composition of most of the so-called Diet Drinks. As we do not, however, profess to understand the true meaning of the term alterative, as used technically, and that we consider the Diet Drinks in common use as mere tonics or restoratives of the general stamina, we presume that Sassafras exerts a beneficial influence upon the digestive organs. And, yet, it is difficult to determine the origin of a prejudice which exists in the minds of many of our people against its habitual use, in consequence of its supposed tendency to the production of Intermittent fever. This prejudice is so general in Georgia, that it is supposed to have contributed largely, some years ago, to the defeat of a candidate for the gubernatorial chair, who had in Congress urged an increase of the duty upon tea and coffee, adding that if the enhanced price of these articles proved onerous to some, they might drink sassafras tea. The good people proudly refused to vote for any man who was willing to see them all take theague and fever, merely for the sake of filling the National Treasury! We believe the prejudice to be unfounded—but would like to know if any facts can be adduced in support of it.

Astringents.—The only beverages in common use in disease which possess any astringency, are the Green and Black table teas and the Sage tea. This effect is, however, so slight as to be unimportant in general.

Laxatives.—We may class as such the infusions of Tamarinds, of dried Apples, of dried Peaches, of Raisins and of Cream of Tartar; to which may be added Saratoga water. These are all more or less grateful and remarkably well adapted to a large class of our diseases, in which the intestines are disposed to be torpid. Those possessed of acidity promote an abundant secretion of bile as well
of gastro-intestinal fluids; hence their common use in warm climates.

Acids.—Lemonade and Orangeade are such general favorites in diseases of tropical climates, that they are in some of the West India islands considered as the most important medication in all affections implicating the hepatic secretion. As an antibilious remedy, Lemonade is held in an equally high esteem by the Creoles as Calomel is by the English, and those who borrow their views. Lemonade, besides being exceedingly grateful to the palate, is highly promotive of the mucous, hepatic, renal and cutaneous secretions. The free flow of salivary fluids excited by its contact with the mucous surface of the mouth and the orifices of the ducts that open upon it, will give some idea of its effect upon the gastro-intestinal surfaces and the glands whose ducts terminate in them. The capillary circulation of these mucous membranes and glandular structures must therefore be much relieved of congestion, if any exist. But besides this local action, Lemonade doubtless penetrates the general circulation by inhibition, and is carried to the kidneys and skin, whose secretions it manifestly increases. If the fluids of the system are alkaline, this is changed and they become acid by the free use of this beverage. Producing such decided local and general effects, it would seem more proper to class Lemonade among the potent agents of the materia medica, than among the mere adjuvants. We feel satisfied that the therapeutic value of Lemonade, in the treatment of our fevers, from the simple intermittent to the dreaded yellow fever, has not been fully appreciated by those who indite most of the books upon our shelves—the British and our Northern brethren.

Antacids.—There are states of the system in which Antacids may be eminently useful, especially if taken largely diluted or in the form of beverages. The officinal Lime water, or small quantities of Bicarbonate of Soda, or of carbonate of Potass, may be thus used with plain water. The well water of blue limestone districts is sometimes of great advantage to dyspeptics. A very common error prevails with the non-professional public, who believe that soda enters into the composition of the beverage vended in our cities under the name of "Soda Water," which is nothing but water strongly impregnated with carbonic acid gas, and without any alkaline properties. The name of Soda Water had its origin in the fact that the carbonic acid gas was formerly obtained for the purpose by the action of acids upon the carbonate of soda, whereas it is now usually derived from marble or some other carbonate of Lime. By the addition, however,
of a little bi-carbonate of soda to this aerated water, a very pleasant and useful antacid beverage may be made.

Sedatives.—During the prevalence of the Broussaisian doctrine, which regarded nearly all diseases as abnormal irritations or inflammations, sedatives were eagerly sought after, in the vain hope that they would prove to be of general applicability. The distinguished French reformer, however, refused to acknowledge as such any other articles than Prussic acid and Asparagine. We may perhaps then be excused for placing under the head of Sedatives the infusions of the leaves of the Orange tree, the Lemon tree and the Peach tree, all of which we believe contain more or less Prussic acid. Be this as it may, there is no doubt that they are exceedingly valuable beverages in our autumnal fevers. The orange-leaf tea is remarkably palatable to most persons, and in addition to being a good diluent, diaphoretic and diuretic, has a soothing effect that can scarcely be appreciated by one who has not realized it in his own person. To secure its full influence, it should be taken freely when hot and just made (by pouring boiling water upon the fresh leaves), for it very soon deteriorates and becomes insipid. In the nervous affections of females, and especially in nervous head-aches, it often acts like a charm. The French make great use of the distilled orange flower water, a tea-spoonful of which they add to a glass of sweetened water;—but we think the orange-leaf tea equally valuable, and this is within the reach of every one who has a garden, as the orange tree grows finely in this region of country, and with less trouble than is required to keep the usual supply of balm, sage, &c.

The infusion of Peach tree leaves is peculiarly useful in cases of irritable stomach, whether occurring in our fevers or after a debauch. In such cases, however, it should be made strong and given in small quantities at a time; say a table-spoonful or two, frequently repeated. In cases of Hooping-cough, if given freely three or four times a day, it tends materially to lessen the violence of the paroxysms and the duration of the disease. We took occasion many years ago to allude to this use of it, and to recommend it in plantation practice as safe and valuable.

The last class of beverages to which we shall allude, comprehends those in which Nutritious elements are added to the diluent. The most common are water holding in solution Gum Arabic, Sugar and the various syrups, and teas made with Toasted bread, Rice, Barley, &c. The value and applicability of these beverages are so evident, that we mention them merely for the purpose of completing the subject.
Indeed we have extended our remarks so much more than we had intended when the theme first presented itself to our mind, that we now entertain serious apprehensions that the reader will be poorly repaid for the trouble of perusing them. We would accordingly withhold them from our pages, were it not that we still feel that the subject is one entitled to more attention than it has heretofore received, and that the imperfections of this hasty paper may induce others to do better.

We find in the October No. of the American Journal of the Medical Sciences, Reports—by Dr. Charles A. Lee, of an interesting case of Paraplegia, caused by Concussion of the Spine; by Dr. F. M. Robertson, of a successful case of operation for laceration of the Perineum; by Dr. Alexander Dunlap, of several cases of successful Ovariotomy; and by Dr. J. B. Brown, of a case of Epilepsy, treated by Ligation of the common Carotid Artery. The same number of this valuable quarterly contains an able paper upon Neuralgia, by Ch. W. Parsons, M. D., being the Fiske-Fund Prize Essay.

"A Farmer" writes to the St. Louis Medical Journal, that he has found Soda (probably the bi-carbonate) to relieve, very promptly, the stinging of insects, and snake-bites. The soda, moistened with water, is to be applied to the part.

In addition to the Physicians reported in our last, as victims of yellow fever in Savannah, it is our painful duty now to record the names of Dr. J. M. Gordon, Dr. A. B. Brantly, Dr. Saussy and Dr. Cullen.

The community for the relief of which men are willing thus to sacrifice their lives, ought, in accordance with the plainest dictates of gratitude, to provide pensions for the support of the bereaved families. Why should not the pension system be applied to such cases as well as to those which occur in the army and navy? We understand that services of Plate have been awarded (very properly) to the Physicians who magnanimously came from a distance to the relief of Savannah. Will nothing be done for the widows and orphans of those who nobly stood to their post until stricken down by the pestilence?

On Glycerin Lotions of Morphia, Strychnia, Veratrina, and Atropia.—The importance of applying active medicines externally until absorption occurs sufficiently to cause their peculiar effects on the patient, has often been resorted to with great propriety in cases where their internal administration was rendered unadvisable from gastric derangement. Quite recently M. Soubeiran has proposed glycerin
as a vehicle for morphia, in lieu of the "oil of morphia" of M. St. Lager, and a writer in the *Bulletin Générale* takes advantage of this suggestion, and brings forward formulæ for similar preparations of strychnia, veratria, and atropia, under the generic title of "**Glyceroles,**" or glycerin lotions.

M. Soubeiran's formula is as follows:

Take of Acetate of morphia, three grains.
Glycerin, five drachms, troy. Dissolve.

The **Lotion of Strychnia** is made thus:

Take of Sulphate of strychnia, six grains.
Glycerin, five drachms, troy.

Dissolve the salt in the glycerin in a porcelain mortar.

A teaspoonful of this lotion is applied by friction in paralysis of the limbs,—on the vertebral column in chorea,—on the temples in certain cases of amaurosis. It is necessary to remember that it is not the alkaloids but their salts that are soluble in glycerin, and when only the free alkaloids are officinal in the materia medica, as veratria or atropia, it is necessary to dissolve the organic base in a little diluted chlorohydric acid.

**Lotion of Veratria.**

Take of Veratria, fifteen grains.
Diluted muriatic acid, q. s.
Glycerin, five drachms.

Dissolve and mix.

A teaspoonful applied by friction in chronic rheumatic pains of the joints, or, in the sacro-lumbar region to relieve painful menstruation.

**Lotion of Atropia.**

Take of Atropia, six grains.
Diluted muriatic acid, q. s.
Glycerin, two and a half drachms.

Dissolve and mix.

Forty or fifty drops applied by friction, repeated three times a day on the track of the sub and super orbital nerves, on that of the facial nerve, &c.—*[Jour. de Pharm. Amer. Jour. of Pharm.]*

**Urinary Calculus formed upon a Leather Shoestring.**—Dr. Brown, of Bangor, Maine, exhibited (to the Boston Society for Medical Improvement) the specimen, and reported the case. The patient was a man, æt. 27, and grossly addicted to onanism. Fifteen months ago he passed the string into the urethra after he had gone to bed, and went to sleep without removing it; in the morning it had disappeared. One or two months afterwards, urinary symptoms appeared; and these became so urgent, that he was obliged to give up work last autumn, and for some time past has kept his bed. About a week ago the calculus was removed by Dr. Rich, of Bangor, assisted by Dr. Brown; the lateral operation of lithotomy was performed, and the patient has done well since.

The calculus is of a regular, oval form, somewhat flattened, and measures nearly two inches in length. The chemical composition,
according to the analysis of Mr. J. C. White, student of medicine, is:

"Phosphate and carbonate of lime—the latter being slight—with a slight trace of double phosphate of ammonia and magnesia. No uric acid nor urates in the portions examined." One extremity of the calculus having been broken away in the removal, the foreign body has been fully exposed.

Prof. Mussey, of Cincinnati, who was present at the meeting, mentioned a similar case in which he had operated, about two years since. The patient was a young man who had introduced (in accordance with medical advice, as he stated) a piece of cord into the urethra, for the purpose of allaying irritation. While introducing it, an unexpected occurrence surprised him, and caused a suspension of the operation, during which time the cord disappeared.

Dr. Mussey saw the patient three months afterwards, and was led from the symptoms to suspect the presence of a calculus in the bladder; at that time, however, it was impossible to detect any stone on examination by the sound. Two months later, a calculus was at once distinctly felt, and the operation of lithotomy was performed. The calculus extracted resembles a petrified lumbricus. The nucleus was the cord introduced, as above mentioned.—[American Journal of Med. Sciences.

Charcoal Coverlet for the Prevention of Smell from Gangrenous Sores.—In some cases of hospital phagedæna recently under his care, in St. Bartholomew's Hospital, Mr. Wormald made an ingenious and very useful application of the disinfecting powers of charcoal. It is well known that dry charcoal will effectually absorb any noxious or offensive gas which can be made to pass through it. On this power, Dr. Stenhouse's disinfecting respirators depend for their efficiency. The difficulty in applying it in hospital practice has, however, arisen from the difficulty of keeping it at the same time dry and in a uniform layer around the part giving rise to effluvia. Dr. Wormald's plan consists in sprinkling freely between two sheets of cotton wool a tolerably thick layer of powdered charcoal, and then "quilting" them together in small segments, so that the powder is retained securely in its place. The pads thus prepared may be of any size, according as required to wrap round the end of a stump, or to cover a superficial ulcer. The sloughing sore having been dressed in the ordinary manner, and a little lint or wool so placed as to absorb any discharge which may flow, over all is laid the charcoal quilt, which is then lightly confined by a bandage. It forms, in addition to its disinfectant properties, a very soft and comfortable envelop, more especially if the sore be in such a part that the patient is obliged to lie on it.—[Medical Times and Gazette.

Medical College of Georgia.—The Course of Lectures in the Medical College in this City will not be commenced before the third Monday in November—in order that Students may feel entirely safe in coming.