powders were retained by the stomach, but the infusion was rejected, as had been previously the case with all fluid medicines; it was therefore discontinued. Fifty grains of powdered gamboge, and five scruples of powdered scammony, having been taken by the patient without any effect upon the bowels, an enema, composed of four ounces of spirits of turpentine, and four ounces of concentrated essence of senna in hot gruel, was administered. In about a quarter of an hour, slight relief was for the first time obtained. In the course of two hours, the bowels were again freely and copiously acted upon; the gamboge and scammony were therefore discontinued, and thirty-five drops of Battley's sedative solution of opium given, with the object of procuring repose to the patient, who was now in an exhausted and restless state, and also of aiding the action of the purgatives by its antispasmodic powers. An hour or two after taking the opiate, he was again freely purged, and subsequently passed a comfortable night. Very little uneasiness or pain was afterwards felt; and the bowels, having been kept regularly open by mild aperients, he progressed favourably, without the appearance of any bad symptom, or having to complain in the slightest degree of the powerful remedies used. His usual good health was soon established. Wishing to condense this account as much as possible, a daily statement of the condition of the tongue, pulse, skin, urine, &c., has been omitted. It may be sufficient to state, that throughout the tongue was moist, pale, and covered with a whitish fur; the pulse full, and beating from 100 to 110; the skin was not much affected; the urine scanty, high-coloured, and offensive. During the attack, the patient refused all nutriment, with the exception of tea, barley-water, and lemonade with cream of tartar.

This case shows the great extent to which powerful purgative treatment may be carried, in obstruction of the bowels, when not dependent upon inflammation, milder measures having failed. It gave me much satisfaction to find that Dr. Horner's view of the case was similar to my own, in considering it, in this instance, not inflammatory, but probably owing to accumulations in the bowels, which had for some time previously been in a very torpid state; time, therefore, was not lost by having recourse to remedies that had previously been found inefficient; but more active aperient treatment was at once resorted to, premised by bleeding, as a preventive of inflammation, and to relax constriction.

Hull, April, 1846.

CASE OF TETANUS, IN WHICH ACONITE WAS ADMINISTERED; AND REMARKS.

By WILLIAM NEWTON, Esq., M.R.C.S. Eng., Newcastle.

On the 1st of April, 1846, I visited Matthew S—, a poor person, residing in my medical district in the Newcastle-upon-Tyne Poor-Law Union. He was labouring under a contused wound of the right great toe, which had happened to him on the 28th of the previous month. The toe was greatly swollen, and was the seat of diffusive suppuration. Pain was severely felt, and the patient complained of its extension along the limb. Two or three free incisions were made, and the pus evacuated was so fetid, that the women who were present could scarcely stay in the room. Poultices were now applied, and the bowels acted on with aperient medicine. A portion of bone was discharged along with the pus, and the wound appeared to be going on well. When I called, on the morning of the 7th, he complained of stiffness about the lower jaw, and could not protrude his tongue. These symptoms, he said, had seized him suddenly about breakfast time. The man being sixty years of age, an old sailor, who had, in the course of his life, suffered two or three fractures of the limbs, and who was much reduced in health, beef-tea and wine were given. The bowels, which had become costive, were opened with croton-oil, and under this treatment there was an apparent amendment. But during the night of the 11th and the morning of the 12th, general tetanus had set in; and when I called at nine o'clock A.M., (for I had not been sent for,) I found him in dreadful convulsions, presenting, in fact, the usual symptoms of acute tetanus. The head was drawn backward and fixed, and his body described an arch. Mr. Preston, surgeon, kindly accompanied me in one of my visits, and the following prescription was ordered:—Solution of muriate of morphia, three drachms; tincture of aconite, forty drops; tincture of tobacco, forty drops; camphor-water, sufficient to make a mixture, of which a drachm is to be given every hour.

I made three subsequent visits, and saw that the medicine was duly administered. I could not discover that any one of

the ingredients had produced its usual physiological effect on the system; certainly none was exerted on the disease, for the patient died at half-past three o'clock next morning, convulsed to the last moment. No post-mortem examination could be obtained.

Remarks.—This was an ordinary case of tetanus, and that disease is better described in books than I can hope to define it. But it may be useful in the investigation as to the utility or otherwise of the properties of aconite. I will not be so rash as to state that aconite has never cured tetanus, or that it was in this instance given to a favourable subject, or at a favourable time. Still, it appears partly to negative—at least to limit—its alleged virtues. A negative result is not beset with more difficulties than a supposed positive effect,—nay, it is surrounded with fewer, in reasoning on the effects of medicine over disease.

My chief reliance would have been, in compliance with evidence, on tobacco as a therapeutic agent; but the present case seemed to forbid its use. The patient was advanced in years, and his constitution broken down; and it seemed to me, that by giving tobacco so as to produce a decided effect, it would be more likely to arrest the heart's action than to stay the progress of the disease.

REMARKS ON MR. TYTE'S CASE OF RUPTURE OF THE UTERUS.

By DAVID G. JONES, Esq., Surgeon, London.

IN No. 6 of the present volume of *THE LANCET* is a case quoted by Mr. Tyte, of Harrow, which proved, on a post-mortem examination, to be rupture of the uterus; and in No. 8 of the same volume, are a few inquiries by myself, with a summary of his case, wishing to ascertain the correctness of his report, the case in question exhibiting quite a new mode of treatment, the result of which is not very satisfactory. In No. 11 Mr. Tyte states, that the summary of his case is not correctly quoted by me, inasmuch as his statement runs as follows—viz., that the child's head descended low in the pelvis, slightly dilating the perineum, whereas, my statement was, that the child's head was pressing on, and dilating, the perineum. May I be permitted to ask a question here—i.e., when the perineum is being dilated, after rupture of the membranes, what is it that dilates the perineum? Again: this was observed at ten o'clock, seven hours after she was first taken in labour, when the pains were very severe, recurring with very little intermission. This state of things continued for two hours. There was no distortion of the pelvis or impaction whatever, the child was of an average size, and the soft parts were dilatable. What, therefore, must be inferred from these circumstances? Why, that the child must be almost expelled, of course. Mr. Tyte likewise states, that I neglected mentioning his attendance on the patient, three years before, with a tedious labour. Was this her first or second labour? From the time that had elapsed, of course I concluded it was the first, which is generally rather tedious than otherwise, and therefore I did not think it worth mentioning, from the healthy state of the patient and capacity of the pelvis. Mr. Tyte observes, that I characterize the treatment as unusual. I am still of the same opinion. What was the necessity for performing craniotomy and evisceration in the present case? None whatever; but it was disadvantageous and most unjustifiable, as it delayed the expulsion of the child. Mr. Tyte, moreover, remarks, that if he has erred in his mode of conducting the case, he has the satisfaction of having done so in excellent company, rehearsing the following quotation from Dr. R. Lee's "Lectures on Midwifery:"—"When the head of the child presents, and rupture of the uterus takes place, the best plan is, without delay, to deliver with the perforator and crotchet, and to prevent the child, during the operation, from passing through the rent into the peritoneal sac, pressure should be made over the fundus uteri, and the opening made in the head in a most gentle manner." That he, Dr. Lee, had not seen a case of ruptured uterus in which it was possible to deliver with the forceps, and that he was not aware of any case of ruptured uterus in which the child was born alive with the forceps. I would advise Mr. Tyte to look over Dr. Lee's lectures more carefully, as he will find this treatment applicable when the head has not descended into the pelvis. In three cases of rupture of the uterus recorded in Dr. Lee's "Clinical Midwifery," delivery was effected by version; thence Dr. Lee's expression, that he had seen no case of ruptured uterus where the patient could

be delivered with the forceps, from the situation of the foetus in utero, of course. Again, in favour of his treatment he quotes the following:—"Efforts have frequently been made to deliver with the forceps in such cases, (says Dr. Collins,) but this instrument is seldom applicable, as the introduction of the blades generally forces the head out of our reach; besides, but little would be gained, for the child dies shortly after the rupture takes place. The dimensions, too, of the pelvis, are, in such cases, for the most part defective, (therefore impeding the descent of the child,) all which circumstances would seem strongly opposed to this mode of delivery." In all these remarks I perfectly coincide. Mr. Tyte does not take into consideration the capacity of the pelvis, by which condition we mostly find such cases governed. In his case there was nothing of the kind. There is a material difference here, so that Mr. Tyte will find that the above quite accords with my views; not, as he expected, that with my modern views they would not accord. I think this word *modern* is more applicable to himself, for if he refers to Drs. Denman, Blundell, Ryan, Davis, Lee, Rigby, Collins, &c., he will not find the dreadful operation of evisceration performed under the same circumstances. Mr. Tyte, in attempting to answer my inquiries, remarks that the difficulty of delivering with the vectis and forceps arose from the frequent recession of the head. Would the recession not be greater in attempting to perforate the head, than applying the forceps in a capacious pelvis? Dr. Ramsbotham says in these cases there is but one mode of practice that affords the least chance of life—viz., speedy delivery. If the head has entered the pelvis, and has not receded, the child may be extracted by the agency of the long or short forceps; but he generally found the head had receded beyond their reach. He would then introduce his hand, and turn. In Mr. Tyte's case it had receded somewhat, but was in the cavity of the pelvis, and had descended after that. It is seldom under rupture of the uterus that the perforator can be necessary or available. Mr. Tyte, to make his case appear more clear, (as he imagines,) asks, who would question that delivery ought to be effected at once?—as he contended, from the general circumstances of the case, he was imperatively called upon to deliver, and that he did not believe any competent judge would question it. This is certainly the best remark that he has yet made, and I perfectly agree with him, from facts stated above. But had this practice been put into operation, the case probably would not have terminated as it did. Mr. Tyte, towards the conclusion of his letter, mentions that I wish to impress upon your readers that I could have performed the operation in little less than half an hour. Far from it: Mr. Tyte is labouring under a very great mistake here. I said, that the operation with the forceps could have been performed, with any management, in one-sixth of the time occupied by him—viz., four hours, under the circumstances that presented themselves in the case in question. I should be very sorry to undertake the operation under such a presentation and combination of circumstances. I may observe, that after the post-mortem examination, it was ascertained that rupture existed. This is what he calls having the honour of pursuing Dr. Lee's practice. But here he does not forget to leave the last part of the business unmentioned.

High Holborn, March, 1846.

MEDICAL SOCIETY OF LONDON.

MONDAY, APRIL 20, 1846.—MR. DENDY, PRESIDENT.

ALBUMINURIA.

MR. PHILP exhibited an enema syringe, which, by means of an exhausting pump, had the recommendation of affording a constant stream, instead of acting by jets, as was the case with the old-fashioned syringe.

MR. FISHER related a case of affection of the kidney, in which the most prominent symptom was the presence of a large quantity of albumen in the urine for a period of several months, as was evidenced by testing the fluid with nitric acid and heat. Under the treatment pursued, which was, in the commencement, antiphlogistic, including the use of mercury to ptialism, followed by the tartrate of iron, the patient got quite well, the urine becoming, and continuing normal. The patient shortly afterwards contracted typhus fever, of which he died, and after death, the kidneys were found congested only, no marks of disorganization, even under the microscope, being detectable. The case was brought forward to show the uncertainty of the signs of disease as exhibited in some cases; in this instance, in particular, it would seem that

a case of Bright's disease had been cured, but the post-mortem examination showed that no such disease had existed.

DR. COPLAND had listened to the case with interest, as it confirmed some views which he had advanced in his article, "Dropsy," to show that the presence of albumen in the urine did not afford sufficient evidence that the kidney was in a granular condition. This condition of urine might be present when the kidney was inflamed, or simply congested, as was often the case after scarlet fever.

DR. ROBERTS mentioned a case in which albumen was found in the urine, probably as the result of the application of a blister. He mentioned another case, also, in which there was a peculiar foetid condition of the urine in an old gentleman with enlarged prostate; there appeared to be no cause for the phenomenon, and he asked an explanation of it. The urine was usually acid; once, after a shivering fit, it became suddenly alkaline, but after a sleep, again assumed its acid character.

MR. HILTON observed that the urine was often offensive when it had been long alkaline.

THE PRESIDENT had observed a peculiarly offensive condition of urine in cases of scurvy.

CHEMISTRY, PHARMACY, AND MATERIA MEDICA.

A LETTER, ADDRESSED TO BARON LIEBIG, PROFESSOR AT THE UNIVERSITY OF GIESSEN, (G. D. OF HESSE.) By MM. LAURENT and GERHARDT.

MONSIEUR LE BARON,—You have just published a brochure,* in which you have attacked our labours in the most injurious manner.

You are at liberty, doubtless, to find our labours to be bad, and you may also attack us in a style at which any well-bred man would blush; you alone are responsible for your own expressions.

But you have no right to calumniate us; you have no right to designate us forgers, highway robbers, to say, in the face of the world, that in order to refute your theories, to expose your errors, we have deceived the Academy of Sciences, in presenting to that body, on the 22nd of September last, experiments which do not exist, which we never made.

Were you a Frenchman, Monsieur le Baron, we might call you to account for these calumnies, before the legal tribunals, for those experiments were made in Paris, in the laboratory of M. Pelouze, for the most part in the presence of M. Pelouze, or his pupils, and upon substances furnished by that chemist, and also in Montpellier, in the laboratory of the Faculty of Sciences, partly with materials furnished by M. Pelouze, and partly on products prepared in the latter laboratory, and which were shown to many persons, amongst others to the Dean of the Faculty.

This will serve to show the value of your assertions.

Since your being a Foreigner insures your impunity, we have no other resource against you than an appeal to public opinion; we beg you, therefore, Monsieur le Baron, to have the goodness to give our reclamation its right, by retracting your words, and to accept our *salutations empressées*.

AUG. LAURENT,
Corresponding Member of the Academy of Sciences,
CH. GERHARDT,
Professor to the Faculty of Sciences, Montpellier.

In publishing this letter in his journal *The Annalen*, Baron Liebig tells us, the authors have sent no less than six copies, to be placed in a public reading-room at Giessen, besides very liberally distributing copies of it elsewhere; he says, "I think proper to accompany this letter with a few remarks."

It will be immediately perceived that the authors of this letter have only tried to refute one point of my article on M. Gerhardt and his relation to organic chemistry. Their object is to prove, *by witnesses*, that the statements they communicated to the Academy, in their letter of the 22nd of September, last year, are the results of experiments really instituted. By the addition of his name, M. Laurent has incurred part of the responsibility of these statements; and I think I am not deceived if I say that the only object of the reclamation is his justification.

The statements contained in the communication of the 22nd of September, 1845, correspond, however, word for word, with the note laid before the Academy in January, 1844,

* In the present volume of THE LANCET, p. 84.