

who, up to that period, had never even tried to walk. In spite of my having told him of this exceptional circumstance (upon the authority of Sir Richard Quain) Dr. Reid now complacently tells me and your readers generally that a child can only do this "through use and after long practice." The fact I have cited is certainly hostile to Dr. Reid's theories, and tends to show that when walking is delayed much beyond the usual time, such complicated motor acts may be exercised, independently of previous trials and failures, simply because in the mean time the motor mechanisms concerned with these acts which the child has inherited have had time to develop.

I am, Sirs, faithfully yours,

Manchester-square, May 17th, 1897. H. CHARLTON BASTIAN.

## DR. RENTOUL AND THE GENERAL MEDICAL COUNCIL.

*To the Editors of THE LANCET.*

SIRS,—From the letter of my colleague, Dr. R. R. Rentoul, published in THE LANCET of May 15th, it would appear that he only imperfectly realises his position as a Direct Representative. Having undertaken to act as representatives of the profession on the Council it appears to me that it is incumbent upon us, at whatever personal sacrifice, to attend the meetings of the Council and to do all we can to promote the interests of our constituents. The Direct Representatives are so few in number that for one to drop out of the ranks is to weaken the influence of the others. I trust, therefore, that Dr. Rentoul will, notwithstanding what he has said, take his seat at all the meetings during the forthcoming session of the Council.

I remain, Sirs, yours faithfully,

GEORGE BROWN,

Direct Representative for England and Wales.  
Hart-street, Bloomsbury-square, W.C., May 18th, 1897.

## OPHTHALMIA IN LIVERPOOL.

*To the Editors of THE LANCET.*

SIRS,—I am surprised to find a leading article in a journal like THE LANCET, that has for a motto "*Audi alteram partem*," supporting, upon what is admittedly scant evidence, the opinions of one medical man against three others.

Before replying to the leading article in detail allow me to say that I am visiting surgeon to the Liverpool Workhouse Hospital and have nothing to do with the Kirkdale or Sheffield schools and that my remarks below only apply to the workhouse. In the first place there is no question of severe ophthalmia. Mr. Fuller never used such words with reference to the workhouse. A second mistake is made at the very beginning of the article in reference to the "workhouse schools." We have no "schools" at the workhouse. The so-called "schools" are receiving or probationary wards, where children are temporarily placed until their condition and destination are accurately determined. For instance, cases of granular lids so slight as to be considered latent are sent there from the wards under medical observation to ascertain how they will do without treatment. Cases admitted from town at all hours of the day and night, apparently in good health or with such trifling complaints as not to require hospital treatment are also sent there. The "ins-and-outs" rarely get beyond this place, and its entire population is a floating one, varying in number from a dozen to fifty. Nearly every day cases are being sent to it from the wards and *vice versa*, and every fortnight it is cleared out as far as possible, the healthy sent to Kirkdale, and the residuum to hospital or to the country homes, according to circumstances. This is the only place in the workhouse where any real fault was found by the inspector. On Nov. 25th, 1896, Mr. Fuller visited these probationary wards, which on that day contained forty-four boys and girls, and next day, on going round the ophthalmic wards, I found eleven new cases that had been sent there through Mr. Fuller's influence. I examined these cases carefully and two had absolutely nothing the matter with their eyes, one had a congested eye, and the others showed signs of having had granular lids in scars and congestions, but there were no granulations and no discharge. Some were sent back at once and others were kept for a few days under treatment, and I thought no more about the matter till February, 1897, when Mr. Fuller's report was received. Fortunately for us the state of the eyes of every case that passes through the ophthalmic wards is recorded, and I was thus able to join

issue with him to the effect that his diagnosis was incorrect and that he had exaggerated the symptoms. Had it been otherwise we would have been helpless, for when his report came down only two cases out of the eleven were under the care of the vestry and the habitation of the others was quite unknown.

Now it was, with these doubtful cases in my mind, that I put the question to Mr. Fuller as to the value of the absence of discharge as a diagnostic sign of the latency of granular lids and of the danger of contagion, and, in fact, as a sign of the temporary cure of the cases. I agree that it takes years often to ensure that a permanent cure has been effected. Mr. Fuller's reply was that granular lids were always contagious in all stages, because when there was no apparent discharge the lids were found sticking together in the morning, so that there really was discharge. This sticking together of the lids in the morning would to me be evidence of discharge, and I always ask the nurse in such cases whether the children wake up with their eyes bright and clear and free from stickiness; and this is my most important sign that the disease is latent and that active-treatment should stop. I have not heard Mr. Fuller's opinion as to this condition. Bacteriology may help us in the future, but the importation of the gonococcus into the discussion seems to me to show that we know very little about this phase of the question as yet and may leave it on one side. And now allow me to enlighten the writer of the leading article about the authorities quoted by me.

Mr. Nettleship says in his "Diseases of the Eye," p. 968, where he is writing of chronic granular disease, "When accompanied by discharge the disease is contagious, otherwise not." Is not that explicit? Mr. Swarzy, in his "Handbook of Diseases of the Eye," p. 87, says in reference to acute and chronic granular ophthalmia, "Both forms are contagious, and probably the infection occurs only by transference of the secretion from one eye to the other by means of fingers, towels, handkerchiefs," &c. If the writer of your leading article will go through all the other authorities within his reach he will find that they are of opinion that the discharge of the eyes is the chief factor in spreading the infection, and consequently the less the discharge or secretion the less danger of infection. I think all of us will agree that distinct granulations should be treated and isolated, whether there is apparent discharge or not; but it is in the intermediate cases between healthy lids and distinct disease where disagreement will be found. For instance, before the Poor-law Schools Committee one medical inspector said that 90 per cent. of pauper children had "unhealthy eyes"; and another examiner will find 30 per cent.; another, perhaps, 20 per cent. of unhealthy eyes in the same school. At Liverpool we adopt a common-sense and practical view of the matter—neither an ultra-scientific nor a faddist one. We have separate wards for acute and chronic cases, and convalescent homes for those convalescent. Our supply of granular lids comes to us from town and affects mostly the younger children. As the children grow older the disease disappears, and only some of the "ins-and-outs" present any injury to the eyes. We cannot quite understand the reports of frequent serious eye disease in granular lids that come to us from the metropolis, and half suspected these to be due to *nimidia diligencia medicorum*.

The small lecture at the end of the article on granular ophthalmia is, I assure the writer, not necessary. I know all about eversion of the lids, follicular conjunctivitis, towels, damp soil, &c., for have we not read it before? and although not an oculist, my experience of granular lids exceeds that of most ophthalmic surgeons of my day, and I have always looked the disease fairly and squarely in the face. I therefore repeat that complete absence of discharge, with, of course, the unavoidable concurrent subsidence of other symptoms and signs, is one of the best practical tests of latency or cure of granular lids, and a sign that active or severe treatment should give place to freedom, fresh air, or school, provided that care and medical supervision is still exercised to detect relapse.

I am, Sirs, your obedient servant,

May 16th, 1897.

W. ALEXANDER, F.R.C.S. Eng.

## "VAGINAL DOUCHING."

*To the Editors of THE LANCET.*

SIRS,—Having to deal with many cases of gonorrhœa, I should like to make two brief comments on Dr. Giles's paper in THE LANCET of May 15th.

In the first place vaginal douching with any drug whatever is bad treatment for purulent vaginitis, simply because the solution cannot come in contact with the whole of the diseased surface; and if the patient douches herself, only the lower half of the vagina is douched. The best treatment is the wool tampon method; pledgets of wool, soaked in the drug it is wished to apply, are packed in the vagina through a Ferguson's speculum, and changed every day. Cases treated in this way do well if the disease has not spread to the uterine cavity.

With regard to the drugs best to use, Dr. Giles only mentions lysol and chinosol among the "new-comers." These are fairly good, but others are much better. The best in my experience, and, curiously enough, one that has not yet been advertised as a certain cure for gonorrhœa, is naphthol.

I am, Sirs, yours faithfully,

London Lock Hospital, May 17th, 1897. C. F. MARSHALL.

## THE CENTRAL LONDON OPHTHALMIC HOSPITAL AND THE REPORT OF THE HOSPITAL REFORM ASSOCIATION.

*To the Editors of THE LANCET.*

SIRS,—I am requested by my committee to ask you to kindly allow me space to correct the mis-statements in the above report as regards the Central London Ophthalmic Hospital. The tabular statement states:—

*Subscribers' letters.*—"Out-patients: Yes, about half. In-patients: Yes."—No subscribers' letters are required, and less than 2 per cent. of the out-patients come with them. The same remark applies to the in-patients, as they are admitted from the out-patients.

*Payment by patients.*—"Out-patients: 2d. a visit to 2s. 6d. per month; £426 a year. In-patients: all expected to pay 1s. a day; occasionally, £1 a week; £30 a year."—No charge is compulsory to out-patients, but some pay 2d. a visit, some 2s. 6d. a month in aid of the funds of the hospital. In the way in-patients are admitted free, but such as are able are expected to pay 1s. a day for their maintenance. As a matter of fact, the payment is very exceptional. Since the abolition of a private ward in 1890 no in-patient has paid more than 7s. per week.

*Inquiry officer.*—"Inquiry officer fills in forms, &c., and if he thinks proper brings the patients up to the secretary to be inquired into; has no power himself to question." "Inquiry officer only in name."—There is a proper inquiry officer, appointed in 1894, who is empowered to question patients as to their means, and who rejects those whose position does not entitle them to hospital relief.

I am, Sirs, yours faithfully,

JOHN GRIGGS BRYANT, Secretary.

Gray's-inn-road, May 17th, 1897.

## "THE SUCCESSFUL TREATMENT OF A WOUND OF THE HEART."

*To the Editors of THE LANCET.*

SIRS,—In your Berlin Correspondent's notice of the German Surgical Association a very interesting notice of Dr. Rehn's case of wound of the heart and of his opening the pericardium is given, and without in the least wishing to detract from the credit justly due to Dr. Rehn for his bold and sound surgery I write to remind your younger readers that the opening of the pericardium, the removal of foreign bodies from it, and the washing of the sac was practised and taught by John Bell—that great surgeon who anticipated so many of our modern surgical methods.

I am, Sirs, yours truly,

Dublin, May 11th, 1897.

GEORGE FOY.

## URIC ACID IN THE BLOOD.

*To the Editors of THE LANCET.*

SIRS,—With reference to the statement of Mr. D. F. Shearer in THE LANCET of May 15th that my experiments do not appear to justify the statement that blood contains no uric acid, it is evident that Mr. Shearer does not quite appreciate the extreme delicacy of the murexide test for uric acid. Not only can half a milligramme of uric acid be detected with ease by that test, but I find that the one-hundredth part of a milligramme gives a very evident

murexide reaction. Mr. Shearer has wrongly appreciated the extraction process employed by me when he refers to it as "a process in which the error of estimation is from one to two milligrammes." What I stated in my Goulstonian Lectures was that when I added known quantities of uric acid to blood I could only extract from 80 to 87 per cent. of it, so that if a given bulk of blood contained one milligramme of uric acid, the process would enable four-fifths of that milligramme to be extracted. Permit me to remind Mr. Shearer that if the uric acid contained in the urinary excretion is derived from the blood, the blood of birds should contain an abundant quantity of it, since their urinary excretion consists entirely of a compound of uric acid. As Sir Alfred Garrod originally showed, no uric acid is present in the blood of birds, an observation which was confirmed by my experiments, in which pints of birds' blood were operated upon at a time.

I am, Sirs, yours very truly,

Weymouth-street, W., May 19th, 1897.

ARTHUR P. LUFF.

## "POST-PARTUM HÆMORRHAGE AND ITS TREATMENT."

*To the Editors of THE LANCET.*

SIRS,—In his letter in THE LANCET of Jan. 16th last under the above heading Mr. E. Stanmore Bishop amongst other things indicates that he does not clearly comprehend my method of treatment as described in THE LANCET of Dec. 19th, 1896, and asserts that I speak with some doubt, &c., as to the efficacy of the procedure. I would crave of you a little space to make clear what may seem ambiguous, to show that there was little doubt in my mind, and also to make a few remarks on the position Mr. Bishop has taken up. I would state that when I wrote no one was more ready to rejoice with Mr. Bishop on his being able to point to some method of a definite nature to be pursued on being called upon to render help when a person's life is at stake—a means of freedom from, and a getting rid of, the confusion of the text-books, for I must say that with Mr. Bishop's beautiful description of the state of affairs found therein I am in the most perfect accord; but I wrote with a profound conviction that however effective in preventing the blood flow compression of the aorta may be, he was placing it entirely in the wrong position. I hope Mr. Bishop will be kind enough not to think that I wished to press his analogy too severely, and I really do not consider that I did so. I might have gone on and pointed out that in dealing with post-partum hæmorrhage we are assisting at a physiological function, but in dealing with a cut leg it is a different thing, &c., but that might have seemed unkind, and, as I thought, unnecessary. This reminds me that the simile of the plumber is also, I am sorry to say, a very unhappy one, for if a pipe is spouting and injuring valuable property a plumber is called and simply covers the aperture with his thumb instead of putty; I am inclined to think we would not consider him the kind of gentleman Mr. Bishop indicates. Now, in regard to the closing of the bloodvessels with one hand inside and the other one outside let me hasten to assure Mr. Bishop that his assumption that I meant what I said is quite correct. I meant it in all seriousness. The difficulty of compressing the two arteries and accompanying veins with the thumb need not be any annoyance to anyone, for on being reminded that uteri vary in size and condition and that men's hands are not equal in magnitude and power of grasp, and that it cannot be called a step from this to pressing one set of vessels against the pelvic wall and setting the left hand free to close the vessels of the other side—I say, on being reminded of this, we may safely assume that a moment's reflection on the great law of adaptability will settle the whole matter.

Let us look now at compression of the aorta as put forward by Mr. Bishop. As already stated, I hold he has placed it entirely in the wrong position; its fatal feature is that it does not meet the great requirement—a good working method. Let me quote his words: "From first to last, however, in every case, whether complicated by retained placental fragments, torn cervix, or any other condition, the one and only measure of primary importance in the treatment of post-partum hæmorrhage is compression of the aorta." This is a sweeping statement and requires a deal of reading. Let us see where it leads us, and let us suppose that there is