year there had not been a single prosecution for a breach of the sanitary laws. Even when notices had been served they were not followed up.

FORSTER GREEN HOSPITAL FOR CONSUMPTION, BELFAST.

The annual meeting of this institution was held in Belfast on January 26th; the President, Mr. G. Herbert Ewart, occupied the chair. The medical report, read by Dr. Thomas Houston, Honorary Secretary of the Medical Staff, stated that 68 patients were in hospital at the beginning of the year, and 269 were admitted, or an increase of 15 per cent. over 1909. There were 111 incipient, 138 advanced, and 24 far advanced cases. On discharge, in 66 cases the disease seemed to be arrested, 148 were improved, 24 not improved, and 2 died. On an average, the patients remained twelve weeks in hospital, which the physicians considered much too short a period to ensure the most lasting results of treatment. Last year, however, nearly double as many incipient cases were admitted as in 1909. The medical staff pointed out two grave defects: the committee was often compelled to admit advanced and so unsuitable cases, while many early cases were compelled to return to work at too early a stage, and so relapsed. In other places consumption colonies were being established. In the dispensary there were 858 new cases and 3,071 attendances; the dispensary nurse had visited 104 new patients and paid 959 visits. The chairman, in moving the adoption of the reports, referred to the overlapping of work with kindred societies; one patient had been looked after at home by the public health department, the Women's National Health Association, the Nurses for the Sick Poor, and the Forster Green Hospital.

Correspondence.

EFFECTS OF ELECTRICAL CURRENTS UPON BLOOD PRESSURE.

SIR,—You recently published in the BRITISH MEDICAL JOURNAL a paper by Dr. Ettie Sayer on the effects of high-frequency currents on blood pressure.[1] As the physiological action of these currents is frequently misunderstood, even by those using them, I should like to give the following very brief summary of the conclusions I have come to on this subject after a regular employment of this form of treatment for about ten years.

High-frequency "currents" are said to raise the blood pressure when low and to lower it when high; but sometimes we desire to lower it when relatively low, and to raise it when relatively high. The important question, then, is what is the normal blood pressure under normal circumstances in each case? The pulse-rate will often be a guide. If this is high it will generally indicate a change in the blood pressure from what is normal to the patient. These currents, when suitably applied, act by lowering the peripheral resistance, and also as a cardiac tonic. If the pressure is low, even moderate doses may depress the heart to a dangerous extent, due to a further lowering of the pressure; whilst small doses may raise the blood pressure by giving tone to the heart. Where the pressure is abnormally high, due to autointoxication, these currents act beneficially, probably as a gastro-intestinal tonic and antiseptic, for, as I have proved, the current from auto-condensation passes easily through the alimentary canal. In order to obtain the best results from high-frequency treatment, the patient must rest before, and especially after, the treatment.—I am, etc.,

Glasgow, Jan. 30th. SAMUEL SLOAN.

RHEUMATIC PURPURA AND PELIOSIS RHEUMATICA.

SIR,—Dr. F. J. Poynton, in his recent lecture on some of the rarer occurrences in the rheumatism of childhood (BRITISH MEDICAL JOURNAL, January 7th, 1911), makes reference to the difficult subject of "rheumatic purpura."

In this connexion, two questions, essentially distinct, require answers: (1) Is purpura ever a rheumatic manifestation? (2) Is the condition so commonly diagnosed as "peliosis rheumatica" usually or ever a true manifestation of rheumatism?

1. The view that purpura is never a rheumatic manifestation Dr. Poynton regards as "certainly an error," and one is not here concerned to deny the possibility, even the probability, of rheumatic purpura. Such an infection as rheumatism, which in its severe forms produces haemorrhagic arthritis and pericarditis, haematuria and haemorrhage into and around its subcutaneous nodules, is more than likely to produce in certain instances a haemorrhagic rash.

2. Upon the relationship between peliosis rheumatica and the rheumatic infection Dr. Poynton does not so clearly give us the benefit of his opinion. He quotes one case in which there were "arthritis, endocarditis, and the well-known peliosis rheumatica"; he refers to others "in which it has been more difficult to assure oneself of the rheumatic origin of the purpura," and appears to summarize the matter in his conclusion that "in a long history of rheumatism, purpura may be an incident pointing to activity of the process." From these passages it would seem that he is prepared to give his support to the theory that peliosis rheumatica is, or at least can be, a rheumatic manifestation.

To the view that peliosis rheumatica is usually a manifestation of rheumatism there are at least two grave objections. In the first place, from all we know of the rheumatic infection, its haemorrhagic lesions tend to occur in the most severe examples of the disease, while in peliosis rheumatica such symptoms as are most suggestive of rheumatism suggest, as a rule, only a mild form of that disease—for example, sore throat and muscular pains without serious arthritis, carditis, or chorea. Secondly, one may see a very large number of cases of definite rheumatism with joint, cardiac, and nervous symptoms, without meeting an example of peliosis rheumatica among them. For these reasons there seems no theoretical likelihood of peliosis rheumatica being under ordinary circumstances or usually a rheumatic manifestation.

That peliosis rheumatica is never of rheumatic origin is hardly capable of proof. Nevertheless it is noteworthy that although both it and acute rheumatism are common enough conditions, yet the occurrence of peliosis rheumatica in a rheumatic subject, especially during an attack of acute rheumatism, is quite uncommon, as Dr. Poynton's paper suggests. It would appear, therefore, more reasonable to explain such instances as the result of a dual infection than as rare or atypical examples of true rheumatism. Owing to the damage done to the tonsils by rheumatism, added infections are not rare in rheumatic subjects (for example, non-rheumatic types of malignant endocarditis).

Taking the view that peliosis rheumatica is due to an infection distinct from, although occasionally co-existent with, the rheumatic infection, it becomes, as regards its relationship to rheumatism, analogous to erythema nodosum, which Dr. Poynton refuses to include among the rheumatic manifestations.

I am sure that it would be of interest to many if Dr. Poynton would state clearly if he teaches that peliosis rheumatica is usually or exceptionally a direct rheumatic manifestation.—I am, etc.,

London, W., Feb. 6th. REGINALD MILLER.

SWABS LEFT IN THE INTESTINE.

SIR,—May I join with Mr. Lynn Thomas in congratulating Mr. Charles Ryall? By defending the action brought against him, and by his success in obtaining a verdict, Mr. Ryall has rendered a service to our profession. In the performance of our work the risk of an ungrateful or aggrieved patient bringing an undeserved action against us is ever present, and every successful defence discourages the bringing of such actions, which, however unwarranted they may be, must necessarily entail great anxiety to the defendant.

The suggestion made by Mr. Lynn Thomas as to the necessity for some ethical code to guide those members of the profession who feel it their duty to support plaintiffs against alleged wrongful treatment is an excellent one. Under certain circumstances to do so may be perfectly right and proper, but when it involves detailed criticism of the technique of an operation a

[1] BRITISH MEDICAL JOURNAL, 1910, vol. ii, p. 1052.

which one was not present—criticism which must necessarily be ill-directed and may be quite wrong—such action, I maintain most emphatically, indicates a grievous lack of consideration for, and fairness to, a colleague.

When encountering any of the serious complications which may arise in the course of a formidable operation, a surgeon has to decide instantly the best course to pursue, and has to act on the judgement of the moment. Many of us know by experience that such difficult operations as the one which led to the recent action, tax one's resources to the utmost. Suturing the intestine in such circumstances is always difficult, and sometimes impossible, and it is undertaking a grave responsibility to say that the use of a piece of gauze inside the bowel (not in the peritoneal cavity, as counsel's opening statement led one to imagine) is an unjustifiable procedure.

As Mr. Lynn Thomas points out, Professor Roux is accustomed to place pieces of gauze in the jejunum. If this is permissible and safe in the jejunum, surely it is even more so with regard to the sigmoid. Mr. Lynn Thomas maintains that the use of the sigmoidoscope in the case under discussion would have been highly dangerous. In this I cordially agree with him.

There is another point in the evidence of the surgeon called for the plaintiff to which I wish to allude. It was suggested that the hand might have been introduced into the rectum to search for the gauze. It would be interesting to know whether he has ever adopted this procedure, and, if so, with what success, and also how many surgeons there are in London with a hand sufficiently small to admit of its being introduced into the bowel? I once saw the original suggester of this procedure attempt to remove a foreign body in the rectum in this manner, but unsuccessfully. It is a method available for a very few, and, if any attempt is made to pass the hand beyond the reflection of peritoneum, one fraught with grave danger to the patient.—I am, etc.,

London, W., Feb. 1st. HERBERT J. PATERSON.

SALVARSAN ("606").

SIR,—In his able letter (JOURNAL, January 28th, p. 226) on the subject of "606," whilst expressing incontrovertible views as to the great value of mercury and also of iodides, Dr. Marshall is a little unfair, I think, in his attitude towards the new remedy. As soon as I was able to judge objectively of the results obtained, both at home and abroad, I came to the conclusion that we had in the Ehrlich-Hata preparation a most valuable antisyphilitic weapon, though, owing to my fundamental scepticism, I did not expect to see the fulfilment of all the wonders that were expected or reported of it, the more so as I had seen a good many therapeutic rockets come down like sticks. Yet there could be no doubt as to the importance of keeping an open mind on the subject. Just as intramuscular injections of mercury were a distinct advance, so "606" has now, I consider, taken us a step further.

We can now deliver a blow straight from the shoulder against the *Treponema pallidum* in the earliest stages of the infection. Though the ideal *sterilisatio magna* has not been attained and relapses have occurred, mainly after one injection only of "606" or too small doses, the fact remains that a great deal can be done in the way of breaking up many of the parasites by this means. When this first attack is followed up by one or two more injections of "606" and a vigorous course of mercury, one has done one's best to knock the disease on the head. As things stand at present, cases must be kept under strict observation, mercury and iodides being exhibited from time to time, according to circumstances and indications. Everything is relative (one needs to be constantly repeating this) and we must be guided by results as we go along.

As to the mode of introducing "606" into the organism, I have come to the conclusion, after doing several intravenous injections, that this is the best. The advantages of the intravenous injections over the intramuscular (for I take it the subcutaneous has been jettisoned) are that the pain is done away with and the time spent in bed greatly abridged. But no doubt both the intravenous and the intramuscular methods will find their place and special indications.

The great point about this new remedy is to keep an even mind, not exaggerating either one way or the other. We must be guided by events, and, as we gain experience, be ready to modify our opinions and practice as regards the value of "606" and the way of using it.—I am, etc.,

London, W., Jan. 31st. GEORGE PERNET, M.D.

ALCOHOLISM AND DEGENERACY.

SIR,—Professor Pearson undertakes, at the beginning of his letter in your issue of February 4th, to "dismiss Dr. Saleeby." That, of course, may conceivably prove a long business, and there is an ancient observation, derived from a race which is conspicuous for its non-alcoholic parenthood, and is going on still, about boasting before and after putting on one's armour. Let us see then.

Professor Pearson heartily admits his belief that it was the "stronger and more virile parents" who took alcohol, "and because of their physique, not because of their alcohol, had healthier children"; and he points out that he stated this (less clearly) in the original memoir. My criticism was and is that "plainly, therefore, on his own showing, his report has no relevance to the subject with which it professes to deal . . . for, in order to study statistically the subject posed, would not one have to compare comparable parents?" Later in his reply Professor Pearson argues that Sir Victor Horsley and Dr. Sturge may find it "absolutely impossible to investigate whether alcoholic and non-alcoholic are initially of the same stock. Without this equality any investigation of the effect of parental alcoholism on the offspring must be idle."

Thus Professor Pearson accepts from me and uses against others a criticism which proves his memoir to be, in his own word, "idle." I had hoped to convince you, Sir, and your readers, on this point, but had never expected to break all records and convince Professor Pearson in controversy. That he should introduce his instant acceptance of my argument by asking your leave to dismiss me is doubtless "only his fun," and that he should continue to defend his memoir after teaching us to call it idle is doubtless only Newton's first law of motion.

But the memoir which Professor Pearson now teaches us to call "idle" has made me busy, and, since writing my last letter, I have, *inter alia*, paid a visit, under the guidance of my friend Dr. Leslie Mackenzie, to the Edinburgh school now in question. The population under discussion lives mainly in the Canongate, and the school is the North Canongate school. (The name is not in the schedule, and it will interest Professor Pearson, no doubt, to know where the Edinburgh people live whom he studied in London.) It will further interest him, as he has necessarily had to maintain that he was dealing with a fair sample of the working class population, to know that it was the absolutely appalling condition of the children in this school that led the official inquirers into "physical training" after the Boer war to ask Dr. Mackenzie to make studies of other schools; the contrast between the North Canongate school and, for instance, the Bruntsfield school, being all-significant. Hence, as a matter of well-known history, grew the study of school children in other cities, the committee and report on physical deterioration, the medical inspection of the present, and the school clinics of the future. It was the ghastly state of the children of this school that led to the detailed inquiry into their families, which, though thus grotesquely unsuitable for the purpose, was innocently used by Professor Pearson. And so it came about that the very population the degeneracy of which directly led to all these subsequent developments and the present national interest in the subject, was used by Professor Pearson as a fair and suitable population for his purpose of comparing normal and alcoholized stocks. It is not mischance that led Professor Pearson to choose the perfect and original example of a population which no one but a fool, having seen it, would have chosen. This is the Nemesis for the credulous employment of a "method" which Professor Pearson regards as independent of the quality of the data submitted to it. That claim, as he makes it, was never made by his master and mine, Sir Francis Galton, whose method it is, and whose noble and seminal life, now ended, yet scarcely begun, will achieve its more immediate fruition only when his method is employed by those who are willing and competent to judge of the quality of the data under review.