Correspondence.

"Audi alteram partem."

THE DANGERS OF ELECTRICITY.

To the Editor of THE LANCET.

SIR,—Those who were fortunate enough to hear the very able and illuminating address on "The Dangers of Electricity" by Dr. S. Jellinek of Vienna, given before the section of Electro-therapeutics of the Royal Society of Medicine on Nov. 15th, must have realised there were some important yet simple facts placed before us which in the public interest are not as generally known as they should be.

Much commendable work has been done in teaching the principles of rendering first aid in nearly all kinds of accidents, and one of the results of this teaching is that most people of average intelligence know what should be done in a case of drowning, for instance. It is an accident that we as a maritime nation are more or less familiar with. Every medical man and quite a large number of the public are capable of dealing with this emergency if called upon to do so. But if they were suddenly confronted with a case of severe electrical shock—the patient unconscious and to all appearances dead—probably few medical men, and still fewer laymen, would know how to deal with it in the correct way. Yet the principles involved are quite simple, and the proper method to adopt is practically the same as for drowning.

There are only two fundamental facts that need be impressed upon the minds of everyone. The first is that death from electrical shock is only an apparent death at first; and secondly, whatever the conditions or severity of the accident, there is practically always a time, varying from a few minutes upwards, during which it is possible to resuscitate the victim by artificial respiration if resorted to at once. So important is it to commence artificial respiration quickly that it is the first duty of the bystander to see to this before everything else. He must not leave the victim to summon medical aid—it may be too late by the time the doctor arrives—but he should send for medical assistance if there is anyone else present.

If the victim has fallen clear of electrical contact first-aid may be given at once, but if he remains in contact with the circuit the first step is to get him free. This is dangerous to the rescuer unless care is taken, but the necessary precautions are simple enough for any voltage likely to be encountered by the public—the highest being that used for railways and tramways, about 600 volts. If, then, the victim remains in contact with the circuit, his body must not be touched by his rescuer, but the latter may pull him out of contact by hauling on the clothing, or he may take off his own coat, insert his hands in the sleeves, and then handle the victim with little or no risk to himself. Almost any article of clothing or material may be used, provided it is dry and of moderate thickness. Artificial respiration must be persevered with for at least two hours, or until the patient revives; in many cases it may be only a few minutes. After return to consciousness complete rest is essential for a day or two, and under no circumstances must the man be allowed to return to work at once, as he may wish to do if the shock has been a light one. With the prompt application of first aid practically every victim can be revived, and of these nearly all will make a complete and permanent recovery provided they were previously in a normal state of health.

In view of the extensive use of electricity it is a matter for congratulation that severe electrical accidents are comparatively uncommon. For this we have to thank the more or less elaborate precautions insisted upon by the authorities, including the insurance companies. This, however, is no excuse for not being prepared for the emergency, and a wide dissemination of the simple facts bearing on the matter presents no real difficulty. If every organisation, Government and other, having to do with the treatment of emergency or accident will incorporate the necessary directions in their rules and teaching, this knowledge will very quickly become as widely known as that relating to drowning. The necessity for this is shown by an instance quoted by Mr. G. Scott Ram, of the Home Office, on the occasion

of the meeting already referred to. A medical man was summoned to an electrical accident such as I have mentioned. On being told that the victim had been in contact with a tension of 2000 volts he declared the man dead and ordered the latter to be taken away. The other workmen present were not satisfied and decided "to have another go at him." Result: the victim revived and recovered. The doctor was not to blame; he was but one of many who had the idea that a shock from a tension of 1500 volts or over would always cause death. We now know that the voltage is but one of many factors that determine the effect of an electrical shock, that practically every case can be revived, and that most of these will make a complete recovery if first-aid is promptly forthcoming and properly applied.

I am, Sir, yours faithfully,

REGINALD MORTON,

President, Section of Electro-therapeutics, Royal Society of Medicine.

Harley-street, W., Nov. 22nd, 1912.

THE HISTOLOGY OF EXPERIMENTAL RHEUMATISM.

To the Editor of THE LANCET.

SIR,—I hasten to assure Professor J. M. Beattie and Dr. E. W. Ainley Walker that I had no intention whatever of attributing to either of them anything but scientific motives in their work on the test for differentiating streptococci, and since the term "wreck" is a cause of offence I unhesitatingly withdraw it. Neither do I dispute the results obtained by these workers. It is in regard to the interpretation of their results that I find myself at variance with them.

Professor Beattie, for instance, in his paper in collaboration with Dr. Yates,¹ while carefully examining the reactions of a series of streptococci in sugar media with all precautions, makes no mention of the reaction given by these streptococci in neutral red broth anaerobically, yet this test is indispensable in my series for placing a streptococcus, and I am not surprised that without it he was unsuccessful. I venture to ask Professor Beattie to try the tests again on a series of streptococci isolated from the human body. I will not ask him to trouble himself with the rest of the tests, but let him examine these streptococci in neutral red broth for 48 hours anaerobically, and in lemco litmus raffinose and mannite for the same time aerobically. I shall be surprised if he does not find these three tests to help him to identify the three types salivarius, faecalis, and pyogenes respectively. For on recovery from the human body salivarius constantly reduces neutral red and fails to change mannite, and while in half the cases it changes raffinose, in the other half it does not. Pyogenes, on the other hand, fails to reduce neutral red or to change raffinose; and whereas in the majority of cases it fails to change mannite, in some cases it does so. Faecalis constantly reduces neutral red and changes mannite, but does not ferment raffinose, though it often decolourises the medium.

Now, as regards the pathogenetic functions of these three types of streptococcus, first for the rabbit (intravenous injection of a 48 hours broth culture), in my experience there is a great quantitative difference between the three types in regard to the dose necessary to produce a lethal effect. Whereas a comparatively small dose of pyogenes is fatal, large and perhaps repeated doses of the other two streptococci are required for this purpose. Moreover, while pyogenes kills in a few days with acute septicæmia, the other two streptococci produce a more chronic form of septicæmia, and the rabbit develops ultimately well-marked endocarditis. But in determining the pathogenetic ability of streptococci I have for some time past discarded rabbits in favour of mice, since the latter are in my experience better for this purpose and are also cheaper. I find that with mice, using a 48 hours broth culture of the streptococcus as soon after isolation as possible and injecting 1 c.c. subcutaneously, pyogenes kills the mouse, as a rule, within two or three days, whereas with salivarius and faecalis the animals escape in the vast majority of cases. Thus, of 40 mice injected by me with the streptococci most abundant in quantitative cultures from the saliva of 20 normal individuals 36 escaped, and of the 4 that died

¹ Journal of Pathology, vol. xvi., p. 247.

none gave after death positive cultures of streptococcus. Similarly, Houston tested 100 streptococci from normal faeces in the same way, and found 96 per cent. of them non-pathogenetic. On the other hand, I have tested numerous examples of streptococcus pyogenes in the same way, and have found almost invariably that the mouse succumbs in a few days with acute septicaemia, the streptococcus being readily recoverable from the heart's blood or spleen.

As regards Dr. Ainley Walker, I remember getting his letter informing me that he was about to challenge my streptococcus tests, and I remember also replying to the effect that he was welcome to do so, but that these tests had proved for some time past useful in practice. Then appeared a paper² indicting the tests generally on the ground of variation shown by certain streptococci (source not stated) on subculture; and as he seemed to get such extraordinary variations I ventured to criticise him in the same journal. If I have in any way overstepped the limits of absolutely fair criticism in that article I offer my sincere apologies to Dr. Walker, and I think I know him well enough to say that he will at once accept them. At the time I did not know that this paper of his was merely the precursor of the full paper which has since appeared. As regards his latest paper³ I fully accept his results, since they were clearly got with due precautions, but I reserve to myself the right to comment on his conclusions, mainly because they assail a position which I personally never took up. For since introducing the tests I have endeavoured to avoid the word species when referring to a type of streptococcus as distinguished by them. I have felt, and still feel, that whereas there are numerous types of streptococcus defined by them (more especially the three chief types already mentioned), it is premature to speak of species among streptococci. Similarly, when I have seen a typical bacillus coli, that on isolation from the intestine gave all the reactions of bacillus coli, lose when subcultured on gelatine over a period of three years its capacity of producing indol, its capacity of producing gas in glucose and lactose, and its capacity of clotting milk; when I have seen over and over again a culture of typhoid lose after several subcultures its specific capacity of agglutinating with serum of typhoid cases to such an extent as to become useless for Widal's test; and when I know by experience that perhaps the most specialised character of all—namely, virulence—disappears with extraordinary rapidity in many of the pathogenetic bacteria (e.g., the importance of primary cultures for a vaccine), but especially in the streptococci; I have grave doubts as to whether this fact of variability shown by streptococci in biochemical capacity on subculture, on which so much stress has been laid by Dr. Walker, really invalidates a provisional scheme of classification based on comparatively constant differences in biochemical and pathogenetic capacity exhibited by streptococci in the state in which they are on recovery from the human body. After all, it is something to be able to differentiate streptococcus pyogenes from other streptococci in cultures from the human body, and if the tests enable this to be done—and I maintain that they do so with ease—their introduction is more than justified.

I may add that I am particularly interested in Dr. Anderson's clinical observation confirmatory of the rôle of streptococci of the mouth in cases of rheumatism associated with bronchial catarrh. This observation is also of importance in reference to the recent article in your columns⁴ on the gastric juice in such conditions by Dr. A. S. Woodwark and Mr. R. L. Mackenzie Wallis.

I am, Sir, yours faithfully,

Nov. 24th, 1912.

M. H. GORDON.

To the Editor of THE LANCET.

SIR,—As the writers with Dr. E. H. Kettle of the original paper on this subject, one sentence of which has given rise to the correspondence in your last two numbers, we beg leave to make one or two observations, although the point at issue has no direct bearing on the actual investigations we published.

The "cultural classifications of streptococci" which we deemed to have been "proved unsound" were, as Dr. Gordon surmises, those introduced by him some years ago.

This statement of ours which Dr. Gordon "emphatically denies" was not made in ignorance of the discrepancy between the value Dr. Gordon places on his tests and their usefulness in the hands of the other bacteriologists whom we named. There is no necessity for us to support our opinion since Dr. Gordon's denial of our statement has been most thoroughly met in your current issue by Professor Beattie and Dr. Ainley Walker. We do not at the present time accept as proved the constancy of Dr. Gordon's tests, and cannot therefore accept conclusions drawn therefrom.

Dr. Gordon's suggestion that we should examine histologically lesions produced by the inoculation of animals with streptococci obtained from the alimentary tract seems to indicate that he regards the classification of streptococci ultimately to depend upon inoculation experiments rather than cultural reactions. But we may point out that our work is not primarily concerned with Dr. Gordon's tests but with "the essential cause of rheumatism." We are perfectly prepared to accept the possibility of the "rheumococcus" being the same as some normal inhabitant of the alimentary tract; indeed, on clinical grounds we have accepted this as likely enough for a considerable time. But wherein lies the use of insisting upon this similarity until the connexion between the "rheumococcus" and rheumatism is admitted?

To our minds the important question is this: A streptococcus, obtained from a case clinically, anatomically, and histologically one of acute rheumatic infection, reproduces experimentally, not only the gross lesions, but the peculiar histological characteristics of human rheumatism; is it, then, admitted that this streptococcus is the cause of the rheumatic infection in the patient from whose blood or tissues it was obtained? If so, then its cultural reactions and variations and its normal habitat become matters of great practical importance; but if not, insistence upon the cultural classification of streptococci will continue, we fear, to retard rather than to aid the solution of the question of the infective essential cause of rheumatism.

We are, Sir, yours faithfully,

REGINALD MILLER.

CAREY COOMBS.

Nov. 25th, 1912.

THE ANNUAL MEETING OF THE ROYAL COLLEGE OF SURGEONS: THE POSITION OF MEMBERS.

To the Editor of THE LANCET.

SIR,—I was very glad to hear Sir Alfred Pearce Gould's reply to Sir Victor Horsley on the question as to why Members of this College are not represented on the Council. Sir A. Pearce Gould is such an eminently fair and reasonable controversialist that it is a pleasure to listen to him, and we may be certain that he said everything that can be said from the point of view of the Council on this matter.

What did he say? (1) That the Council is not a Parliament nor a political body, and that therefore its members need not be elected as those of other political bodies are. (2) That the Council has only three chief functions to perform, all of which can be better performed without the assistance of Members. These are: (a) the care of the Hunterian Museum; (b) the supervision of the library; and (c) the ordering of the examinations. He further said (3) That it would be impossible to take a poll of the 17,000 Members.

As to this statement, I should like to be allowed to reply: (1) That the Council is already elected in the same way as most other bodies of a similar nature, but by a very limited electorate—viz., some 1500 Fellows—and that the proposal is only to extend the franchise to the larger number of Members. It is also proposed that the Members should only have a restricted representation on the Council, say some half dozen seats, and that the remaining 24 should be elected as at present by the Fellows.

Sir A. Pearce Gould cannot reasonably maintain that the discharge of the present limited functions of the Council would be in any way interfered with by the presence of Members, who are just as much interested in the welfare of the College as the Fellows. We know, however, from other sources, such as the reply of the Council to the Privy Council when this question was raised some years ago, that the College Council fears that the Members will introduce

² Proceedings of the Pathological Society, July, 1910.

³ Proceedings of the Royal Society, June, 1912.

⁴ THE LANCET, Oct. 5th, 1912, p. 942.