

astonishing how little known this form of treatment is to the profession in England. Take a sprained ankle. I do not suppose I am far wrong in saying that 90 per cent. of general medical practitioners would put such a joint in a stiff bandage—e.g., plaster-of-Paris—enjoining the patient at the same time to keep the limb at rest for a couple of weeks. When the bandage is removed the joint is found stiff and painful on movement and the muscles must necessarily have lost power from disuse. The duration of discomfort with this form of treatment may be estimated as one of weeks if not months. Compare this with the mode of action of massage and movement. The pathology of a sprain is well known: exudation of lymph and blood through torn lymphatics and blood-vessels, stretched or ruptured ligaments. The rational plan of treatment would surely be (1) by massage to promote the absorption of the exudations as rapidly as possible, for the extravasated fluid causes pain by pressure on the nerve endings and if left is likely to become organised and to form bands of adhesions which will afterwards have to be broken down; (2) by moving the ankle-joint to exercise the toe- and calf-muscles in order to prevent them from losing power and becoming atrophied. The length of time necessary to enable the sufferers to get about with comparative comfort does not, as a rule, exceed a fortnight and in many cases the cure is still more rapid. The treatment only requires to be known to be appreciated. The plaster-of-Paris treatment is barbarous.

I am, Sirs, yours faithfully,

OTTO HOLST, M.R.C.S. Eng., L.R.C.P. Lond.

Eastbourne, Nov. 12th, 1902.

THE METRIC SYSTEM IN PHARMACY.

To the Editors of THE LANCET.

SIRS,—It is a great encouragement to find Sir W. Gowers advocating the metric system in pharmacy. I have for some years been interested in the matter and remember a petition was got up with the result that the metric system was made legal for buying and selling; on the other hand, the inscription on the florin, "one-tenth of a pound," was removed. The only way of getting the system made compulsory is to bully the Government, as by our "coach-dog" method of legislating only those things are done which are cried for by a "majority." Hence the advantage of the work of the Decimal Association. The difficulty in the metric system in general practice would be altering the bottles. I should suggest that for the present they should not be interfered with. It would be very inconvenient to have bottles with only five or as many as 10 doses. I arranged a table some months ago for use in general practice on these lines. Most mixtures are six, eight, or 12 doses ("part every four hours," &c.) and the standard, or most frequent quantity ordered, is 30 grains or 30 minims, or multiples thereof, thus:—

30 grains	= 2 grammes.
3i.	= 4 "

Similarly—

Fl. 3i.	= 3.5 cubic centimetres.
40 minims	= 2.3 "
3iss.	= 5.3 " "

While for smaller doses a table is easily made for the more common fractions. It is certainly more convenient for all liquids to be measured instead of, as in France, being weighed. To those of us who are familiar with quantitative analyses the change would not be any more difficult than that from "grains per gallon" to "parts per 100,000" and would be more satisfactory from every point of view.

I am, Sirs, yours faithfully,

Uxbridge, Nov. 17th, 1902

A. CHARPENTIER.

RELIEF OF THE DYSPNŒA OF PHTHISIS.

To the Editors of THE LANCET.

SIRS,—Dr. J. O. Brookhouse in his letter to THE LANCET of August 9th, 1902, p. 403, referred to some cases of phthisis in which the structural change did not correspond to the amount of dyspnœa. Some years ago I noticed such a condition and in an experimental way introduced an O'Dwyer tube. This gave relief. On examining the patients with the laryngo-cope it was seen that there was a certain amount of perichondritis with fixation of the chords. Why such cases should have intercurrent attacks of dyspnœa could be explained by the fact that under favourable conditions enough air could be introduced to prevent positive discomfort, but when hyperæmia of the

larynx occurred the chink of the glottis became so far reduced as to cause dyspnœa. In the cases under observation it was noticed that the dyspnœa came on at night, due probably to the hyperæmia referred to. The introduction of the tube not only acted directly in relieving the stenosis but indirectly in overcoming the fixation of the chords. Whatever the explanation may be it will probably be found that one introduction will produce a result continuing for some time after its use.

I am, Sirs, yours faithfully,

New York, Oct. 29th, 1902.

JNO. J. REID.

A CASE OF ACUTE GLAUCOMA (BILATERAL).

To the Editors of THE LANCET.

SIRS,—It will no doubt interest some of your readers to learn that the patient mentioned in the Mirror of Hospital Practice in THE LANCET of Nov. 15th (p. 1321), on whom I operated six years ago still retains all that she gained on that occasion. 12 years ago she hopelessly lost the sight of the right eye from an attack of glaucoma fulminans. Six years ago, when 67 years of age, she was aroused at three o'clock in the morning by a similar attack upon the left eye and all perception of light was instantly abolished. She could not tell night from day or say when the gas was lighted. This condition lasted for three weeks and I then saw her for the first time. I at once excised a large piece of iris and succeeded in restoring sufficient sight to enable her to go about unattended and to read the newspaper with moderate facility.

I am, Sirs, yours faithfully,

C. BELL TAYLOR, M.D. Edin.,

Surgeon, Nottingham and Midland Eye Infirmary.

Nov. 18th, 1902.

ALCOHOLISM.

To the Editors of THE LANCET.

SIRS,—I observe that you have an annotation in THE LANCET of Nov. 15th, p. 1341, on Mr. Charles Smith's paper on Alcoholism in which you make mention of me. For a good many hundreds of years it was universally believed that the inhabitants of the south of Europe were more temperate than those of the north. No one questioned the correctness of that belief until quite recently deductions were drawn from it unfavourable to temperance "reform." Then it was suddenly discovered that the people of the south were more drunken than those of the north. It seems that the people of the south drink more alcohol per head than those of the north. But in the south almost every man, woman, and child drinks alcohol as a beverage. They do not as a rule, according to the ancient belief, use it to any extent as an intoxicant. Total abstainers are very few. There is no temperance propaganda nor apparently any need of it. In the north alcohol is dear and there is a vigorous temperance propaganda. Many people do not use alcohol at all. Many more cannot purchase as much of it as they desire. Its use is usually debarred to children. Practically speaking, therefore, only a section of the community uses alcohol in the north, whereas the whole community uses it in the south. Under the conditions it is evidently fallacious to judge the intemperance of a community country by its *per capita* consumption of alcohol. If a party of 100 men consumed two bottles of whisky between them, while another party of 100 consumed one bottle, all of which was drunk by a single man, it would surely be absurd to regard the first party as twice as temperate as the second. The sale of alcohol to American Indians and Australian blacks is forbidden; their *per capita* consumption of it is very small. But the aborigines drink themselves to death when given the opportunity. They can hardly be regarded as particularly temperate. To estimate the comparative intemperance of the peoples of the north and south of Europe we must consider them under similar surroundings. Wellington's British army in the Peninsula almost dissolved when they captured large wine stores. The allied Spaniards and Portuguese were lauded for their temperance by Napier and every historian of the war. British sailors in Italian ports are notoriously drunken. Italians in British and American slums are remarkably sober. Formerly, on the payment of a very trifling sum, Italian peasants drank in

their tavern by the hour. The British publican who permitted his customers to drink for 2*d.* as much as they pleased would be ruined.

Mr. C. L. Rothera finds fault with Dr. Robert Jones's admirable paper on Drink and Insanity. He wishes for temperance "reform" and desires medical men to enlarge on the evils of inebriety. But can Mr. Rothera—to repeat a challenge made for months in THE LANCET—tell us of a single race, dwelling under the ordinary conditions of civilised life, which has not been made more drunken by repressive measures? I must confess I find it difficult to understand the importance attached by some members of our profession to the teaching of pathological details to the public. The public—and drunkards most of all—know perfectly well that drunkenness is the cause of many diseases. "Drinking himself to death" is a colloquialism in the language.

I am, Sirs, yours faithfully,

Southsea, Nov. 17th, 1902.

G. ARCHDALL REID.

NOTE ON THE DISCOVERY OF THE HUMAN TRYPANOSOME.

To the Editors of THE LANCET.

SIRS,—We have recently seen in the medical press several very inaccurate accounts regarding the authorship of the important new discovery of trypanosomes in human blood and of the disease caused by them. For instance, the *Journal of Tropical Medicine* of Nov. 1st (in giving an anonymous description, supported by an editorial article, of a case just observed by Dr. C. W. Daniels and Dr. P. Manson) attributes the original discovery to Mr. R. M. Forde. It does not mention even the name of Dr. J. Everett Dutton. Dr. Dutton is an old student and assistant in this laboratory and is now away on the West African coast, and we are of opinion that he has a claim to be considered in the matter of this discovery. Another periodical, the *Hospital* (Nov. 8th, p. 1902), while also omitting Dr. Dutton's name, states that the discovery was made "within the last few days" by the London School of Tropical Medicine. We believe that such statements are calculated to distort the history of the discovery and should therefore like to have an opportunity for correcting them promptly in your pages.

The facts regarding the history of the discovery, which was made nearly a year ago, have already been publicly and adequately stated both by Mr. Forde¹ and by Dr. Dutton.² Mr. Forde, colonial surgeon, British Gambia, tells us that the case in which the parasites were first observed came under his notice in May, 1901; that he found in the blood "small worm-like, extremely active bodies which I prematurely pronounced a species of filaria," although this conclusion "became doubtful after repeated observations of the parasite"; and that he showed the case in December, 1901, to Dr. Dutton, then upon a mission of the Liverpool School of Tropical Medicine to the Gambia, and that Dr. Dutton "at once recognised" the parasite "as a species of trypanosoma." Dr. Dutton's two papers corroborate these statements of Mr. Forde. After the recognition of the new organism Mr. Forde gave the first records of the case to Dr. Dutton. Dr. Dutton it was, as Mr. Forde says, who recognised that the fever was of a peculiar undulant type; Dr. Dutton it was who positively excluded malaria as the cause of the symptoms; it was he who saw that those symptoms roughly resemble those of tsetse fly disease and surra; it is he who has published accurate and able descriptions, drawings, and charts of the parasites and of the case; and it is he who is now, with Dr. C. Todd, investigating the subject in West Africa for the Liverpool School of Tropical Medicine.

Mr. Forde is undoubtedly deserving of great credit for his part in the matter and we think that his name should be associated with the discovery. But, until Dr. Dutton was called in, he published no account of the case and did not recognise the nature of the parasite or the peculiarity of the symptoms. In order to make a discovery it is not sufficient merely to see an object; it is necessary also to recognise the nature of the object seen and to publish accurate and adequate descriptions of it. For example, Virchow and others long ago saw the parasites of malaria without recognising their parasitic nature; but it is to Laveran, who did recognise their nature, that science gives the credit for the

discovery of them. It is certain that Dr. Dutton was the first clearly to observe and to signal the existence of trypanosomes in human blood, and the first to give accurate descriptions of the new organism, and it is to him that science will give the principal credit for the new observation.

It seems to us particularly unfortunate that the *Journal of Tropical Medicine* should have omitted the name of Dr. Dutton at the moment when it was engaged in giving great prominence to a case of Dr. Manson and Dr. Daniels which after all would probably have escaped notice but for the previous work of Dutton. We may mention also—and this is another point which the *Journal of Tropical Medicine* appears to have forgotten—that before his departure for Africa Dr. Dutton gave at this laboratory a detailed demonstration both of the parasite and the clinical features of the case to Dr. Manson and Dr. Daniels and to one of the editors of the periodical referred to. The omission, then, appears to be due rather to want of memory than to want of knowledge. The journal also states that while the first case—namely, that of Dr. Dutton and Mr. Forde—was regarded only as a "curiosity," the "discovery of a second case" (namely that of Dr. Daniels and Dr. Manson) "opens up a new field for investigation and elucidation," and so on. This view of the relative importance of an original discovery and of a mere confirmation of that discovery is somewhat novel. But the case of Dr. Manson and Dr. Daniels is not the second case at all. The second case—also discovered by Dr. Dutton—was that of a child in British Gambia.

It is unnecessary, after what has been said, to deal with the statement made in the *Hospital*. It affords, however, an instance of the curiously rapid manner in which such errors are often propagated in the press. We should note that Barron and Nepveu have also claimed to have found flagellates in human blood, but, as will be seen from their writings, their descriptions are so inadequate as to fail to convince us of the accuracy or even the nature of their observations.

Your obedient servants,

RUBERT BOYCE, M.B. Lond., F.R.S.,

RONALD ROSS, F.R.C.S. Eng., F.R.S., C.B.,

CH. S. SHERRINGTON, M.D. Cantab., F.R.S.

Thompson-Yates Laboratories, University College,
Liverpool, Nov. 18th, 1902.

THE PRICE OF A TRAINED NURSE.

To the Editors of THE LANCET.

SIRS,—My boy is at a public school and last May, as usual, I paid the charges in advance for "boarding and tuition fees as well as those for laundry and sanatorium." As a reward for good work and without my interference he was sent home for Saturday and Sunday, June 29th and 30th. Of course, he was still under the house-master's authority. There was no possibility of infection in my house and his little sister who played with him remained well. On Monday, July 7th, I had a letter to say that he was in the sanatorium with scarlet fever. I saw him that night and the rash had already disappeared. The boy felt well and remained so all through his detention. There was practically no nursing required. We were not allowed to have him home for five weeks and there, I thought, the matter ended. He returned to school next term and on Nov. 14th I received his belated school bill with a charge of £18 11s for nurse—the first intimation that any payment was expected from me. Surely this is a heavy fine to a man who already pays what he regards as an insurance for "sanatorium." Will any father amongst your readers kindly tell me what "sanatorium" means and if I am liable? I was never consulted about the arrangements in any way.

I am, Sirs, yours faithfully,

Nov. 17th, 1902.

COUNTRY SURGEON.

ROYAL METEOROLOGICAL SOCIETY.—The first meeting of this society for the present session was held on Nov. 19th at the Institution of Civil Engineers, Mr. W. H. Dines, M.A., the President, being in the chair.—Mr. F. Campbell Bayard read a paper on English Climatology, 1881–1900, in which he discussed the climatological data printed in the *Meteorological Record* from the 40 stations of the Royal Meteorological Society, which have been continuous for the whole of the 20 years. The paper formed a valuable contribution to the climatology of the British Isles.—A paper by Mr. C. V. Bellamy, M.Inst.C.E., on the Rainfall of Dominica was also read.

¹ *Journal of Tropical Medicine*, Sept. 1st, 1902.

² Thompson-Yates Laboratory Reports, vol. iv., part ii., May, 1902 and Brit. Med. Jour., Sept. 20th, 1902.