

already infected cattle were imported, their progeny tended to remain free from tuberculosis. Nevertheless it was desirable that the milk traffic of the islands should be kept under observation and suitable measures applied when necessary. Dr. Clare also gave a detailed history of notification as applied to tuberculosis and of aëro-therapy, and drew attention briefly to the outstanding points of tuberculin treatment. In regard to the first subject, he urged the conference and the medical profession throughout the West Indies to insist on the advantages of notification. They should explain the enormous impetus that official knowledge and record of every case would bring to the progress of the tuberculosis crusade, and make it clear that notification was not a tyrannous inquisition but a procedure which would prove a beneficent influence to those already infected and a source of protection to the community at large such as was unattainable in any other way.

Dr. Masson's contribution took the form of an elaborate review of the prevalence and distribution of tuberculosis in Trinidad and Tobago during the last quinquennium, accompanied by diagrams and charts showing the mortality due to it, and comparing this with other rates. The tuberculosis death-rate was 2.29 per 1,000 in 1907-8, and 2.08 per 1,000 in 1911-12; while the same disease caused 8.81 per cent. of the total deaths. The fact was also brought out that quite early in the nineteenth century tuberculosis was believed to be more prevalent in the West Indies than elsewhere, and was then ascribed to the overcrowding of bedrooms and a dislike of ventilation at night. This cause still operated, and not merely among the poorer classes. Another factor of importance was the special susceptibility to tuberculous infection of the negro race, from which the majority of the population had sprung. In addition, both natives and Europeans showed a lack of resistance in lung affections; as in many other tropical countries, excessive heat and a high degree of atmospheric humidity appeared to have a prejudicial effect on the respiratory organs, and gave rise to a high death-rate from non-tuberculous respiratory disease, as well as from phthisis. Hence the practice prevalent among European medical men of sending to the West Indies cases of tuberculosis for the betterment of their health was ill-advised, especially in the hotter months of the year. Its unhappy results had no doubt at one time or another been painfully impressed on West Indian practitioners. A further possible factor in the increased incidence of tuberculosis in the West Indies was that the climate and the receptivity of the bulk of the population together served, perhaps, to supply the tubercle bacillus with an environment which enabled it to take on a greater degree of virulence than in more temperate zones.

The net outcome of the conference was the adoption of a series of resolutions to the following effect:

The first—moved by Dr. Cecil Wall, seconded by Dr. Noc (Martinique)—expressed the opinion that the tuberculosis measures adopted in the colonies and countries represented by the conference ought to include (a) notification, (b) housing reform, and (c) the establishment of tuberculosis dispensaries and other measures for the detection, prevention, and treatment of the disease.

The second—moved by Dr. J. E. Godfrey (British Guiana), seconded by Dr. J. F. Hutson (Barbados)—urged the foundation of an antituberculosis association in every West Indian colony in which one did not already exist.

The third—moved by Dr. MacDonald (Jamaica), seconded by Mr. P. T. Saunders (of the Department of Agriculture)—suggested that the measures adopted by the various Governments should include one aiming also at stamping out bovine tuberculosis where it existed and at preventing its importation.

The fourth—moved by Dr. M. P. Duke (Leeward Islands), seconded by Dr. W. Tucker (Bermuda)—advocated the addition of elementary hygiene and sanitation to the code of schools throughout the West Indian colonies.

The final resolution—moved by Dr. G. H. K. Ross (Jamaica), seconded by the Hon. Dr. E. F. Hatton (Windward Islands)—stated an opinion that the attempt to control tuberculosis would greatly be aided by efficient registration of births, deaths, and marriages.

This conference, which was undoubtedly a great success, included a tuberculosis exhibition and a cinematographic display of the work done at Edinburgh, accompanied by a verbal description from Dr. Cecil Wall. It is proposed to hold a second conference two years hence in British Guiana.

## Correspondence.

### DIACHYLON OR DUTY: A CALL TO ACTION.

SIR,—It is very gratifying to find Sir Thomas Oliver taking up the question of diachylon as an abortifacient with such vigour and thoroughness. At the same time his letter carries with it a certain sadness, for it shows that this pernicious evil is still spreading and that from its small beginnings in the Midlands it has now extended to the very North of England. In 1906 the late Dr. W. B. Ransom of Nottingham and I tried to arouse the profession and the public to this evil and published a joint paper in your journal on plumbism from the ingestion of diachylon as an abortifacient.<sup>1</sup> For the purpose of that paper I made inquiries from leading members of the profession in all the large centres of population in the United Kingdom, and at that time I was able to write that "at Newcastle-on-Tyne it appears to be quite unknown," whilst in and around Sheffield I obtained records of about 200 cases in two years. Sir Thomas Oliver's cases are dated 1911, so that within five years it has travelled far, but by so doing has happily secured us a strong champion. His statements as to the original place of origin of this evil, and his references "from memory" to the names of those who first brought it to light, are somewhat meagre and require considerable modification. They omit the names of some who have spent considerable time and trouble in trying to get this practice stopped. Some of those who were most active are, unfortunately, no longer with us, and I am sure Sir Thomas would be the first to desire that their work should not be forgotten. The earliest cases recorded in literature occurred not in the Potteries but at Leicester, and were published by the late Dr. F. M. Pope in this JOURNAL (1893, vol. ii, p. 9). From then onwards cases were from time to time reported by Crooke of Birmingham, Bell Taylor, Ransom, Scott, Jacob and Trotman of Nottingham, Wrangham of Leicester, Layton of Walsall, and others. Leicester and Nottingham were the chief seats of the trouble. It did not spread north to the Sheffield district until soon after the beginning of the new century. From 1902 onwards the number of cases increased very rapidly, and at the Yorkshire Branch of the British Medical Association meeting at Bradford in January, 1905, I was able to record no fewer than 30 cases I had seen personally.<sup>2</sup> The evil became so crying in the Midlands that in 1906 Ransom and I determined to publish a joint paper (referred to above), and to take such other steps as we could to awaken interest in the subject, and induce the authorities to take action if possible.

We succeeded in getting the British Medical Association to appoint a special committee of inquiry, before which Drs. Pope, Ransom, and myself appeared and gave evidence. The suggestions we then made were chiefly: (1) That diachylon should be placed on the poisons schedule, and (2) that all cases of plumbism in women should be compulsorily notified. We also approached the various members of Parliament representing the districts affected, and they promised their help. But it was largely in vain. In spite of reports from coroners of deaths from this cause, several of which have occurred in this district; in spite of prosecutions at Nottingham and Sheffield, in which vendors of these pills were given long sentences of penal servitude; and in spite of the strong resolutions of the British Medical Association Committee, the reply from the authorities was that "nothing could be done."

So the evil continues. Fresh cases are constantly cropping up; within the last month we have had two at the Sheffield Royal Hospital. The numbers in this district are not so great as they were a few years ago for the simple reason that trade is booming and wages are good. But let us once get back to a period of trade depression such as there was about 1903 to 1906, and the number of

<sup>1</sup> BRITISH MEDICAL JOURNAL, February 24th, 1906.

<sup>2</sup> BRITISH MEDICAL JOURNAL, March 18th, 1904.

cases will go up by leaps and bounds unless something drastic is done. For the misfortune is that it is a simple and effective means of killing the fetus, and the public know it! How great is the price to be paid in health and suffering they do not care.

Some months ago I received an invitation to read a paper on this subject in the Section of Medical Jurisprudence at the International Congress of Medicine to be held in London next August. Possibly with the support of Sir Thomas Oliver's weighty influence it might be possible to get that Section to express an opinion on the subject which would arouse the authorities to action more effectively than we have hitherto been able to do. Any movement in that direction would have my personal active support, for the present state of things is an intolerable scandal.

In conclusion, I should like once more to tender my thanks to Sir Thomas Oliver for his powerful and inspiring paper.—I am, etc.,

Sheffield, June 9th.

ARTHUR J. HALL.

SIR,—Now that Sir Thomas Oliver has lent the weight of his authority to the effort to stamp out this deplorable traffic, it is to be hoped that an earnest endeavour may be made to urge upon the Government the imperative need of an attempt to repress it. Notification, not merely of industrial, but of *all* cases of blood poisoning, would, I believe, be of real service, as Sir Thomas Oliver suggests. It was a proposition I also put forward twelve years ago in this JOURNAL (vol. ii, 1901). But to be effectual it is necessary that medical men should be ready to recognize the different pictures which diachylon produces. I could almost wish that it might be raised to the dignity of a separate disease, and have a heading all to itself in the textbooks of medicine.

Whenever a woman complains of (1) severe gastrointestinal symptoms, with headache, weakness, and a lemon-coloured skin; or (2) presents symptoms and signs of a polyneuritis, not infrequently with ocular symptoms, I would say: Inquire into her menstrual history and look at her gums. The artist's signature will often unexpectedly be found there.—I am, etc.,

Bradford, June 10th.

W. WRANGHAM.

#### ACUTE MENTAL HOSPITALS AND PSYCHIATRIC CLINICS.

SIR,—I do not think Dr. Stansfield would desire to claim a precedence in regard to the separate hospital idea in asylums which is not according to fact. He says that seventeen years ago he advocated the institution of acute hospitals in conjunction with the existing asylums, and fifteen years ago had this carried out at Bexley. Now in 1882, over thirty years ago, we instituted at the Royal Edinburgh Asylum such a separate hospital, with a nursing staff twice the strength of the ordinary wards, where, also, the probationers were trained. The Scottish Commissioners in Lunacy took up the idea warmly, and urged it on all the Scottish asylums. Montrose and Larbert Asylums followed in a few years, and soon almost every institution in Scotland had such hospitals. The Commissioners would, in fact, pass no new plans that had not separate blocks for new and acute and sick patients. As a matter of fact, England was somewhat slow in taking up the idea and carrying it out. Now it is a *sine qua non* in every up-to-date institution all over the world. The medical idea is thereby sustained, and should pervade the whole treatment of the unsound in mind.—I am, etc.,

Edinburgh, June 9th.

T. S. CLOUSTON.

#### RHEUMATOID ARTHRITIS.

SIR,—Dr. Chalmers Watson does not understand what I mean by parting with the substance for the shadow. Well, I suppose I should have recognized the fact that he is a Scotsman (some of my friends object to being called Scotchmen even when they take the barley-bree), but to make up for my apparent lack of perception I shall now proceed with a surgical operation. I was under the impression that I had given him a tangible cause for rheumatoid arthritis, but he immediately throws that to the one side and sets up in its place an imaginary bacterium of whose nature, or even existence, he offers no proof, because he thinks that such view is "most consistent with available clinical and pathological data." My

idea of a bacterial disease is one which comports with all the postulates of Koch, and when Dr. Chalmers Watson, or any one else, has proven to the satisfaction of any first-class bacteriologist, such as Sir Almroth Wright, that he has conformed to all the postulates of Koch, and has really discovered a specific germ which causes what we at present understand by rheumatoid arthritis, then I shall gladly welcome the discovery and praise the discoverer. It is most fortunate that Koch laid down his postulates, otherwise we would have been overwhelmed with numerous varieties of specific bacteria which would have been discovered by highly imaginative clinicians. The so-called practical clinician of the present day is a most hopeless individual; when he runs up against a difficulty he stares it in the face and passes on.

It is just over twenty years since Schüller<sup>1</sup> found, in his observations on 116 cases of "chronic rheumatic arthritis," "short thick bacilli, the central portions of which are notched or grooved, while the poles present a collection of bright granules." Cultures of these bacilli were made, and when injected into rabbits they produced non-suppurative joint affections; similar organisms were removed from the joints of the affected rabbits, and these bacilli could again be cultivated, and the fresh cultures again reproduced the disease in other rabbits. This is much better proof than anything that my present opponents offer. Fortunately or unfortunately these bacilli are not found in all cases of chronic arthritis, and there are many other organisms which set up an inflammation in the synovial membranes of the joints, such as the gonococcus, the pneumococcus, and the meningococcus.

In 1886 Dr. Alfred Mantle of Harrogate found micrococci and short bacilli in the knee-joints and blood of 16 cases of acute rheumatism. In 1892 Sahli found the *Staphylococcus citreus* in the joints of a patient who died from acute rheumatism. In the present day there are several bacteriologists who claim to have isolated a diplococcus or streptococcus which they say is the cause of rheumatic fever; but they are not agreed among themselves, and each one claims that his particular coccus is the specific one. The so-called practical clinician goes much further. He does not trouble his brains about any specific organism; it is sufficient for him to find, or rather get someone else to find for him, a streptococcus in the faeces, if it cannot be found elsewhere, and forthwith the patient is submitted to a prolonged course of a so-called autogenous vaccine. This is what they call scientific medicine, but the pitiable tales which the patients have afterwards to tell are somewhat disheartening.

Dr. Chalmers Watson thinks that I have "not quite grasped his position"; but candidly I think that his bacterial position is so nebulous that there is nothing to grasp. Not content with the cultivation of his own imagination, he wants to make your readers believe that I am in the same happy frame of mind though I do not know it. Because I would clear away all offending organisms, whatever their nature, that are in the outposts, and because I say that there are often acid-forming organisms both in the mouth and the stomach, Dr. Watson thinks that I must really believe in the bacterial origin of rheumatoid arthritis, and there is no use in my denying the soft impeachment. Be that as it may, I shall remain obdurate until Dr. Chalmers Watson can show me how these organisms can be made to comport with Koch's postulates. From my point of view it does not matter very much what the acid is so long as it does not, like most of the mineral acids, form stable neutral salts, and so long as it has a greater affinity for calcium than for the alkaline metals. Nor does it matter very much whether the acid is generated in the stomach or introduced from without. It is a question both of calcium starvation and calcium elimination.

His conclusion about the "instructive correspondence" is really past a joke, and I must bring my operation at once to a close. To be so blind he must be very much overweighted with the importance of his own contributions. In my opinion I have never previously in the whole course of my existence had to refute so much undulterated twaddle. Of course, it would be downright ingratitude not to exempt Dr. Chalmers Watson from this sweeping statement, and I can really assure him that I not only exclude him but I really never meant to

<sup>1</sup> American Journal of the Medical Sciences, 1893.