

a report without the fictitious aid of the other eminent men whose names were associated with them in this inquiry. All who have been on committees will agree with me that, as a rule, the work falls upon one or two of the willing attenders. The Committee, I am pleased to see, recognise this truth, as the report states "that the success will much depend upon the energy, perseverance, ability, and judgment of the secretary". I have no doubt the largest share of work will fall upon him; and he ought to be an histologist, physiologist, physicist, therapist, hygienist, for such a position. I do not desire to offer any cautious criticism; but I would respectfully urge the following considerations on this part of the scheme.

Admirable scientific work and original research have been done in England by private workers, without subsidy from any fund. Stimulated by ambition or love of science, men such as Ferrier of London, Ross of Manchester, Allbutt of Leeds, have contributed to the elucidation of problems of disease at their own expense, and have published the results of their researches on the usual marketable terms. Other workers have done the same; and in the future, as in the past, there will always be found men ready and willing to throw themselves into scientific work, in order to clear up some of the problems which disease opens out. Private workers will be handicapped by the devotion of the funds of the Association to such a scheme as the one now proposed; and private effort will be to a certain extent discouraged. This certainly is not the aim of the Association. I believe individuals will do the work of investigation better than committees; in all cases the individual worker can secure the co-operation of some fellow-worker, should the inquiry lead him into ground with which he is not familiar; where information has to be collected, he can send out circulars, as was done by Dr. Crichton Browne when he wished to gather some data upon left-handedness. Upon these grounds I object to the scheme. If the Association is financially in such a good position as to be able to afford a permanent outlay of from £300 to £600 a year, I think the money might be better expended by giving three prizes annually, of £100 each, and a gold medal, for the best essay on any of the subjects they desire to have cleared up, as osteo-arthritis, the origin of contagious diseases, the incubation of infectious diseases, cancer, tubercle, etc. Thus they will encourage private workers; all members of the Association will have a chance of distinguishing themselves; and they will obtain even better results, because they will secure competitors stimulated by a love of science and research, or by ambition, and will not have to depend upon the service of a paid officer, who may not have his heart in his work.—I remain, yours faithfully,

Horton House, Halifax, June 17th, 1881.

T. M. DOLAN.

WOOLSORTERS' DISEASE.

SIR,—I dislike very much to write to the Press in any case, but most of all when, in doing so, I must differ from opinions expressed by a friend and colleague. I have, therefore, waited a week, in the hope that some of your readers would reply to the letter of Dr. Tibbits in the JOURNAL of June 18th. No one having done so, I must write a few lines, because I am anxious that the prevalence of the disease (bronchial anthracæmia) should be correctly estimated, that its nature should be known, and that erroneous opinions should not be entertained respecting it.

In the first place, allow me to say that the tone and temper of the letter, its positive statements, unsupported by carefully recorded facts, and its general denials, in opposition to the exact observations of experienced workers, are not favourable to the scientific investigation of a disease, concerning which there is yet much to learn.

The statements to be noticed are—1. That "Dr. Greenfield could not mention one single character which separated the bacillus found in anthrax from any other." Surely, Dr. Tibbits is mistaken. No one who knows anything about bacilli will say that there are no characters (physical, as regards size; and physiological, as regards effect on inoculation) which separate the bacillus anthracis from the bacillus septicæmiæ, and other well-known bacilli. Their appearances have been repeatedly figured in our medical periodicals and other works within the last two years (Koch on *Traumatic Infective Diseases*, plate ii). I really cannot understand such a statement.

2. Dr. Tibbits has seen bacilli "in blood and fluids taken from bodies after death" from various diseases, and also under other circumstances. He states "That the bacilli observed under all those circumstances are indistinguishable from so-called 'anthrax bacilli' has been proved by several independent and well-known authorities". If he had given the names of these "several independent and well-known authorities", the statement would have been more complete, if not more valuable. It is well known that some of the bacilli mentioned do appear the same as the bacillus anthracis; but I am not aware that

any well-known authority, excepting Dr. Tibbits, believes that they are all alike. For myself, I prefer other "independent and well-known authorities", who are able to distinguish differences between most of these bacilli—namely, Pasteur, Cohn, Klebs, Koch, Bollinger, Toustaint, Lister, Tyndall, Burdon-Sanderson, Klein, Greenfield, Ewart. When Davaine, in 1863, pointed out the presence of bacteria in the blood in cases of malignant pustule, other observers soon found what they considered to be identical organisms in the blood of those who had died from many other diseases; and they further satisfied themselves that these organisms did not cause the disease referred to. The question was discussed by the French Academy with all the warmth, determination, and ability which have distinguished some of the most memorable debates of that learned body. Why Dr. Tibbits has not referred to the labours of his predecessors in this field of inquiry, I will not presume to say. It is sufficient to know that the advocates of these opinions have long been silent respecting them.

3. Dr. Tibbits appears to rejoice in what he considers to be an indication of the "explosion" of the idea: that the inhalation of these organisms, or their spores, was the cause of woolsorters' disease. For one, I believe the "original idea" is unquestionably correct. No facts have been published to the contrary. Perhaps Dr. Tibbits can supply them.

4. Two cases were referred to, which were published in the JOURNAL of June 11th. One died after thirty hours' illness; the other, forty-seven hours after leaving work. A quarter of a drop of blood from the more rapid case, given to a mouse, produced death in twenty-eight hours. The same quantity of blood from the other was fatal to a mouse in thirty-six hours. The fluids of the tissues of these animals, examined a few hours after death, were crowded with bacilli. Dr. Tibbits, who did not see the cases on which he favours us with his opinion, writes:—"It seems to me that there was no difficulty in diagnosis. Most probably they were cases of congestion or inflammation of lung or lungs." This appears to be his opinion as to the nature of woolsorters' disease. I need scarcely say that such an opinion, which was not satisfactory to the profession and the public forty years ago, is not likely to be more so now.

It is suggested that the virus of anthrax could not exist in effective quantity in the soap-sud and material these men were handling when at work. The man who succeeded one of these at the machine, soon after he commenced work grazed his forearm slightly, and washed the blood off with a bit of wool wet with the warm sud-water. This caused external anthrax, from which he died after a few days' illness. Blood and serum taken during life contained bacilli similar to those found in the other cases after death. The soap-sud was also found to contain numerous bacilli. Dr. Tibbits discreetly says nothing about this case, but inquires, "Is this evidence sufficient to satisfy the scientific mind of the medical public?" I think it is. I do not know what more can reasonably be expected.

The letter is altogether out of date, and of no scientific value. It will not mislead those who are moderately acquainted with the literature of anthrax.—I remain, yours etc., J. H. BELL, M.D.
Bradford, June 26th, 1881.

MANAGEMENT OF HOSPITALS.

SIR,—On reading over your report on the Management of Hospitals, page 1019 of last week's JOURNAL, I find that your reporter misunderstood the few observations I addressed to the meeting. My remarks referred to a state of things that existed for many years at Charing Cross Hospital, but happily, and chiefly through my efforts, has for the last ten or twelve years ceased to exist. At the present time, happily, there is a medical committee, which exerts its due influence over the affairs and management of the hospital; and the medical school is no longer called upon to hand over a considerable portion of its fees towards the support and maintenance of the hospital.—I remain, sir, your obedient servant,
June 28th, 1881. JABEZ HOGG.

OPIUM TRAFFIC.

SIR,—Under the above heading, Dr. Murrell suggests the desirability of arriving at some "more definite opinion as to the effects on the system of the habitual use of opium". But this is hardly fair. The question of "the opium traffic" is a twofold one. It is (1) a question whether, in the eighty years of conflict betwixt the Chinese Government and our own on this subject, the former has received the fair and upright dealing it had a right to claim on a point which it has never failed to allege to be of vital importance to its people. And it is (2) a question of the measure of physical and moral injury produced by the habitual use of opium, as affording or not a ground *per se* why Christian