

## PROPOSED MIDWIFERY SCHOOL IN WORKHOUSE WARDS.

The Wandsworth Board of Guardians, at their meeting on January 23rd, adopted a scheme embodying the establishment of a training school in midwifery for nurses at the lying-in wards at the workhouse. It is proposed, with the sanction of the Local Government Board, that eight senior probationers shall be allowed to enter on a fourth year of training, during six months of which they shall be taught midwifery, the remainder of the time being spent in the ordinary wards. They are to be paid £18 a year. No charge is to be made for the special training, but the probationers are to sign an undertaking to remain for the whole of the fourth year in the guardians' service. Dr. A. E. Dodson, the medical superintendent at St. James's Infirmary, is to undertake the teaching. The guardians, in agreeing to the proposal, decided to ask the Central Midwives Board to recognize the lying-in wards at the workhouse as a training school.

## Correspondence.

## THE POST-MORTEM EXAMINATION OF NAPOLEON.

SIR,—No one is more inclined to accept everything that Dr. Arnold Chaplin writes concerning the fatal illness of Napoleon as authoritative than myself; and yet his letter in your last issue, where he cites Surgeon Rutledge against our mutual friend, the pathologist Antommarchi, confirms me in my belief that the evidence produced in my Hunterian Lecture is convincing proof of the authenticity of the Napoleonic specimens which are now in the museum of the Royal College of Surgeons. The chief point which my opponents have to meet is: How was it possible for O'Meara to hand to Astley Cooper specimens which tally exactly with morbid appearances incidentally described by Antommarchi in his *post-mortem* report? They are specimens which could be obtained only from a case of chronic infection such as we have good reason for supposing Napoleon to have been the subject. Dr. Chaplin has overlooked that part of my evidence and addressed himself to the strictness of the watch over Napoleon's body. Surgeon Rutledge had a vigil by the corpse and the vessels containing the heart and stomach of over twenty-four hours; during that time Antommarchi stole Napoleon's mask, and, we may suppose, could, from his experience in Florence, have easily invented a manœuvre to obtain anything else he desired.

I know that Dr. Chaplin agrees with me in regarding Antommarchi's account of the *post-mortem* appearances as by far the most complete and reliable document we now have; without it we should know nothing of Napoleon's disease, except that there was cancer of the stomach. Surgeon Rutledge's statement that he cut the liver out is in contradiction to every other account available to me, and no one knows better than Dr. Chaplin that every attempt to discredit Antommarchi's statements would receive the most ready encouragement from the Lowe party, to whom he owes Rutledge's statement. Antommarchi's account of the condition of the liver must stand; the clinical symptoms of Napoleon's case leave not a shadow of doubt that at one period there was an inflammatory disturbance of the diaphragm and of the liver.

There is also another aspect of Napoleon's case to which Dr. Chaplin has not done justice—the evidence that Napoleon's illness was of the nature of a recurrent fever accompanied by enlargement of the lymphoid tissues, and the fact that the specimens in the museum of the College of Surgeons show an enlargement or hyperplasia of a part of this system.

As I corrected the proofs of the above part of this letter I received a communication from Sir Alexander Russell Simpson which throws a welcome light on the manner in which the watch was kept over Napoleon's remains. Many must have come across the tradition that the rats of Longwood attacked the heart, which was preserved in a separate vessel. My friend, Mr. Mackellar, first told me of this tradition; I have also come across it in newspapers, but have hitherto failed to trace the account to any reliable source. Sir Alexander Simpson has kindly

allowed me to make the following extract from his letter:

When my uncle, Sir James Simpson, came back from a professional visit in Berwickshire, where he had met Dr. Arnott, he was full of what Arnott had been telling him of his time in St. Helena. What specially impressed him was that Dr. Arnott had charge of the heart and other things that had been removed at the autopsy for the following night. Afraid that some of the Napoleonic retainers might come in and carry off the vessel in which they had been put at the time of the dissection, he emptied them into his wash-hand basin, covered them up with water, and lay down to sleep with loaded pistols under his pillow. He slept lightly. Hearing a splashing sound he jumped up, expecting to see Bertrand or some one at the preparations, and found that it was only rats trying to get at the flesh. "Fancy rats trying to make away with Napoleon's heart!" said Sir James, in repeating the story.

This account corroborates Antommarchi's statement that Arnott was the officer on watch. Probably he relieved Rutledge. In my lecture I stated that these two men kept alternate watch; the point I wish to emphasize is that Arnott was predisposed towards Antommarchi.

I should like to take this opportunity of thanking—not only Dr. Chaplin, but the other correspondents who have sent me valuable information—Mr. Cyril H. Howkins and Major F. S. Irvine. I wonder if any of your readers could help me in tracing those two parts of the mould of Napoleon's bust taken by Dr. Burton, who was a cousin of Graves, the famous physician of Dublin. The missing parts are: (1) The mould giving the impress of the back of the head and neck; (2) the front of the neck. They were, according to Graves, in the possession of Dr. Burton and may still be preserved by some of his descendants who have not realized their value. If they were recovered, an accurate cast of the head of the great Emperor could be obtained.—I am, etc.,

London, W.C., Jan. 27th.

A. KEITH.

## RESEARCH DEFENCE SOCIETY.

SIR,—It is said that the fifth year in the life of any society is the critical period of its fortunes. The Research Defence Society was founded on January 27th, 1908. To all who are interested—and who is not?—in medical research, we beg you to let us say that the Society has its hands full of work, and only wants more money to do more work. Much has already been done, by lectures and by distribution of literature, to bring home to people the truth about experiments on animals in this country, and the great value of them, not only to mankind, but also to the animal world. The expenses of our society are heavy, but the good results of our work are extended far and wide. We have lately opened a bureau and exhibition at 171, Piccadilly (opposite Burlington House). We are exhibiting pictures, portraits, charts, anaesthetics and inhalers, germs in pure culture, tsetse flies and mosquitos, and so forth. This little exhibition, every day and all day long, displays to "the man in the street" the facts of the case. We are the only society which is doing work of this kind; but, of course, it cannot be done without money. Our record for the last four years gives us the right to hope for a great increase of our membership, and of our funds, in the coming year.—We are, etc.,

DAVID GILL,

President.

F. M. SANDWICH,

Honorary Treasurer.

STEPHEN PAGET,

Honorary Secretary.

21, Ladbroke Square, W., Jan. 24th.

## ANTIVIVISECTION IN GLASGOW.

SIR,—Dr. Hadwen repeats the statement, "Not one word about alcohol drinking or vaccination escaped my lips *the whole evening*" (the italics are mine). He then professes to quote what he now says he did say upon these subjects, with the reservation that they were said during the discussion, and not during the lecture. Dr. Charles Bennett, one of the gentlemen whose names I quoted in my last letter, was not present at the discussion, but left before the end of the lecture. He remembers, as do the rest of us, that Dr. Hadwen spoke at length on these subjects. My object in writing was to show that many of Dr. Hadwen's statements to you were incorrect. Besides

my own evidence as to this, I have been able to bring several reputable witnesses with regard to a positive matter of fact; and with regard to two other matters of fact, I sent you a copy of the report in the *Glasgow Herald* in order to verify my statements with regard to them, and to prove the inaccuracy of Dr. Hadwen's. As all the cards I care to play are on the table, I can do no more than leave you and your readers to judge as to who is accurate.—I am, etc.,

Glasgow, Jan. 25th.

CHARLES WALKER.

#### THE TREATMENT OF CANCER.

SIR,—Under this title you published a review on p. 121 of your issue of January 18th. Kindly allow me space in your pages to note one or two points in the review, and to correct one or two errors. You say that I "first invited a general trial" of the pancreatic enzymes without myself being "very clear as to how it (the method) should be applied." I never "invited" any trial at all. On your invitation I published in the *JOURNAL* (January 20th, 1906) a brief account of some preliminary experiments upon cancerous mice. That sufficed. At once, from all parts of Europe and America, but not from Great Britain, I was overwhelmed by requests from medical men either for preparations of ferments or for addresses where such could be obtained, or for general directions for the use of such preparations in cancer. To these I responded because I wished to help my fellow-men. How could I, or any one else, then, seven years ago, know how such preparations should be used to obtain the best results? Like Lord Lister's method of antiseptic surgery, or tuberculin, or salvarsan, much had to be learnt from actual experience of the method; and, moreover, the manufacturing chemists had to find out how to put up active, strong, and keeping injections of the ferments. In 1879 I often saw Professor Edward Lund and Mr. Sam Bradley perform major operations under Listerian methods as they then were. But if either of those departed surgeons could now witness a major operation, undoubtedly he would recognize little or nothing of the Listerian methods they employed. None the less, you would not venture to suggest that Lord Lister should have waited until his methods were made perfect before giving them to the world. Even his first attempts at antiseptic surgery yielded some instances of success; in the same way the very first preparations of trypsin and amylopsin made "for Dr. Beard," as the makers put it, in 1906 furnished a very few cures. I am hardly responsible for damage done by inert preparations.

The preparations of pancreatic ferments, to which I was obliged to refer medical men in 1906, for instance, had only *one-twentieth part of the potencies* of those I should name to-day, and the latter are put out by the same firm of specialists. Therefore, if Dr. Baetzner, who has just published a paper upon the treatment of surgical tuberculosis by means of trypsin,<sup>1</sup> had had to employ the trypsin of 1906, he would scarcely have got the brilliant results he has recorded. Moreover, any one using such a weak trypsin, or some of the useless, even inert, injections still on sale, would, if endeavouring to test Baetzner's finds, conclude, as so many have done after using weak or inert ferments in cancer, that the method was worthless and the author not what he will turn out to be, a very great surgeon. You refer to an "army officer," now in Burma, who cured 3 out of 4 cases of cancer treated by him. If my account of the treatment be puzzling, in his own words his own procedure is recorded in the book reviewed. But, although the book was published nearly fifteen months ago, and although preparations of pancreatic ferments, which appear to fulfil every scientific requirement, that is, the ones Dr. Baetzner employed, have been on sale in London since April, 1912, at this moment I do not know of a single case of cancer in this or any other country which has had a full course of treatment on the lines laid down by this army officer, with such powerful preparations, and in the doses he employs. Indeed, one might imagine cancer had ceased to be the curse of the human race, but for such facts as that only a day or two ago in the weekly summary of deaths it was stated that last week there were eleven deaths from malignant disease in this city. Probably it would not be too much to assert, that not one

<sup>1</sup> *Practitioner*, January, 1913, p. 203.

of these cases was treated with pancreatic ferments after my methods.

In examining any scientific find, or supposed find, experimentally, nothing is easier than to get a negative result, especially if inert reagents be employed. But in the chemical experiment the observer must satisfy himself as to the true nature of his reagents, and, as the late Professor D. J. Cunningham once remarked to me, "Negative results never prove anything in science." There are many members of the medical profession still living and treating cases of cancer who had declared that the pancreatic ferments were "useless" or "futile" in cancer, and who had in good faith drawn this conclusion after using preparations which were inert, and without even knowing that they were inert. In fact, in the use of inert ferment preparations, in the lack of knowledge of the inert nature of their reagents, and in the erroneous conclusions drawn from such vitiated experiments these medical men agreed with the official researchers.

Any "disappointment" I may feel is in a recognition that mankind would rather die than believe the truth. I have never once spoken, or written, or even thought of "the greatness of his (my) achievement"; on the contrary, I have described the whole thing as merely a side-issue, which it is. Shortly now, for it is in the printer's hands, you will be given an opportunity of reading an account of a small piece of experimental work of mine. This does not deal directly with cancer, and its title is the very simple one, "On the Occurrence of Dextro-rotatory Albumins in Organic Nature." To use a phrase which in a similar connexion has been employed by a distinguished living investigator, in this small paper "*the unchallengeable proof*" is given, not only that dextro-rotatory albumins similar to those of cancer do occur in organic nature, but that they are widely represented. Incidentally, the paper confirms once more my statement, first made some years ago, that the cancer conclusions were merely a side-issue.

Possibly, as you say, "many workers . . . hold that his (my) views are entirely erroneous." If so, they are very careful to avoid pointing this out in places where a reply from me would also be published. Their silent "arguments" and "evidences" cannot be very convincing, otherwise you would scarcely add, that possibly one day these "erroneous" views may turn out to have contained the germ of the final solution of the cancer problem. Again, the successful case of sarcoma you mention is not by any means the only case of cure, and scientifically it is not at all clear why "erroneous" views should lead to successful results. Even one cure is a new fact, and Pasteur held, rightly, that erroneous theories never produced new facts. Pardon me for insisting that we are not dealing with the question of the number of swallows which makes a summer, but with the problem of what is a *crucial scientific test*. "If a doctrine be challenged," said Pasteur, "it happens seldom that its truth or falsity cannot be decided by some crucial test. Even a single experiment will often suffice either to refute or to consolidate the doctrine." An instance of this kind, where a *single scientific experiment* suffices to establish the truth of my doctrines, is the case of the pancreatic ferments, trypsin and amylopsin, *versus* cancer. Finally, permit me to express complete agreement with the following, written by a modern author: "'And,' he added, 'speaking of gratitude, those who lead the way do not expect gratitude. It is enough for them to have led the way.'"—I am, etc.,

Edinburgh, Jan. 22nd.

J. BEARD.

#### BOVINE AND HUMAN TUBERCULOSIS.

SIR,—I do not know whether the omission was the printer's fault or mine, but the part of my letter relating to the geographical distribution of tuberculosis should have been inverted commas. I got the information from Green's *Encyclopaedia and Dictionary of Medicine and Surgery*. I am sorry to say I have forgotten the name of the writer, but he evidently knew what he was talking about. It is a pity Dr. Fisher did not deal with the portion of my letter referring to the condition of affairs as to tuberculosis in Guernsey, where Dr. Bishop tells us that "only  $\frac{1}{2}$  per cent. of the cattle are tuberculous, and consequently that cases of bovine tuberculosis in human beings are extremely rare." On the other hand, may I repeat that in 1909, 10,000 children died in England and Wales from