

in the head and the abdomen than from injuries of other regions. This is attributed to the small calibre of modern rifles, the percentages in the other Japanese armies being, it is believed, about the same. Beri-beri has been a very prevalent and probably fatal disease among the Japanese armies but we have no estimate as to the number of deaths which it has caused during the campaign. The disease is the scourge of Oriental armies but the Japanese have of late years done much to mitigate its ravages, especially in their navy. Some very elaborate articles have appeared comparing the losses of the belligerent armies with one another during the year's war but at best the numerical results can only be approximate and untrustworthy at the present time.

Surgeon-General G. J. H. Evatt, C.B., retired list, A.M.S., has been finally adopted as Liberal candidate for the Southern Division of Hampshire at the general election.

Correspondence.

"Audi alteram partem."

THE CANCER PROBLEM AND CANCER RESEARCH.

To the Editors of THE LANCET.

SIRS,—No doubt you are aware that according to the Registrar-General every year nearly 28,000 people die from malignant disease in the British Isles alone. That appalling fact was sufficient reason for, and is still ample justification of, any feeble attempts of mine to solve the problem of cancer. If my findings be wrong that has still to be demonstrated, and unfortunately you and your medical colleagues in Great Britain have at present nothing else to offer those 28,000 annual victims! This reflection might serve to give you pause. You decided to publish my paper and for that courtesy I am grateful. In your leader some of my embryological conclusions are referred to in very scathing terms, but that cannot in the least impugn their truth. In the near future you may see cause to regret the terms of that leader and you may come to recognise that the paper contained more truth than your leader writer appreciated or suspected. If my conclusions be correct, there is no longer any vital embryological problem of cancer; if wrong, the cancer problem has almost certainly been lifted into a new region, that of physiological chemistry. I am not anxious to carry on a futile controversy with an editor in his own journal, even with editors so courteous as those of THE LANCET. But I would ask that courtesy to note or correct a few things in your leader. Before all else the following statement is open to very serious misconstruction: "The writer states that it is based on work carried out with the support of the ample funds for research at the disposal of the Carnegie trustees and the Moray Research Fund of the University of Edinburgh."

The funds for research are not ample. If you still think them to be so I would ask you to take the opinion of the secretary of the Carnegie Trust. The reference to these two funds was necessary because I happen to hold grants from them for certain research purposes and the Carnegie Trust lays down as a condition that in all published work the fact of a grant having been made shall be stated. The footnote refers to my work on various problems *during the past 16 years*. From 1890 to 1898 these researches were carried out from my own slender private means and without grants from any source. Every bit of my published work has left me poorer than when the research was commenced. The work of the past two years upon tumours has not cost the Moray Research Fund a single penny and the total charge for tumour researches on the two grants from the Carnegie Trust amounts at the moment to £26 8s. 5d. Of this sum since last October £16 has been paid in wages to a youth who, at the cost of much of my time and patience, has been made into a useful technical assistant, &c. He has, in fact, been trained, and is still under instruction, to be a skilled laboratory assistant for the University of Edinburgh. So that the real net cost of my tumour work has been £10 8s. 5d., or at the rate of £5 4s. 2½d. per annum. The amount is rather excessive owing to a loss of 50s., the price of a certain dog which was secured by me for the Liverpool

Cancer Research, but which was sacrificed at the altar of science accidentally by an Edinburgh surgeon. Certain other published balance-sheets of cancer research occur to one but comparisons are odious.

There is no call for a new defence of my embryological conclusions in your pages. Had the writer of your leader appended his name there would have been no occasion for any further reply from me. The theory of heredity based in "unconscious memory" is not mine but that of a very distinguished physiologist, Professor Hering of Prague. Had the quotations regarding the germ cells, &c., been made from your pages of Oct. 29th, 1904, instead of from those of your issue of June 21st, 1902, a revised modern version of my conclusions would have resulted. As to the mimicry of the tumours it suffices for me that Sir James Paget and Fleischmann recognised it long ago, but it has never been held by me that a cancer or sarcoma "takes on the histological character of the tissue in which it is growing." It only tries to do so, and often enough it resembles no normal tissue whatever. If a cancer be somatic in origin, how does this happen? Schmorl's finds of bits of the trophoblast in the lungs in pregnancy no more refute the existence of the unlimited powers of growth than would the failure of an attempt to grow rose cuttings in a cinder heap prove that the rose had only limited powers of growth. His finds were made *in pregnancy and with a normal embryo or fetus present*. The whole histories of hydatid mole and of chorio-epithelioma amply illustrate these powers, which can only on occasion be exercised in the absence of a normal embryo. As to the erosion of a bone by an aneurysm, I have heard that simile several times before. Presumably it is not suggested that the wall of the aneurysm or the blood does the erosion and possibly some enzyme of leucocytes, similar to, though perhaps not the same as, that found in malignant growths, may have some concern in the matter.

You, or your leader writer, wish to eliminate the germ-cell theory of cancer. Should you succeed there would always be a problem of cancer. Is any parasitic theory better? On p. 322 of your current issue you express the wish that my friend Dr. W. Ford Robertson and Mr. H. Wade "will succeed in establishing their thesis." I, too, would gladly share this hope but unfortunately it is impossible, for these supposed cancer parasites appear to be due to the following: (1) chromatolysis; (2) "tetrad" formation; (3) division of centrosomes; (4) degeneration of centrosomes; and (5) the so-called "Nebenkern." What the student nowadays is expected to profess about enzymes is no concern of mine; but before allowing my paper to appear in print, apart from the ordinary text-books a close study was made of the following: the latest editions of the two standard works of Verworn and Hoppe-Seyler, the literature on enzymes cited therein, some results recorded by botanists, and all the work of recent years published in the *Journal of Physiology* and in the *Boston Journal of Medical Research*, &c. My chief authorities were given, which is, it may respectfully be submitted, more than can be said for many of the statements contained in your leader. Regarding the point that "carcinoma is common to all vertebrates" and the relation between cancer and uterine gestation suggested by me in 1902, it is quite true that cancer is common to vertebrates though it is anything but common in the lower vertebrates. The investigators of the Imperial Cancer Research Fund got together ten cases from various parts of the world: I have an eleventh but it may be doubted whether two dozen have as yet been recorded in the literature. In the British Isles alone in 1902 27,872 people, zoologically members of the class mammalia, died from malignant disease. In view of these facts is it quite certain that uterine gestation has had nothing to do with its comparative frequency in man, for example?

A recent writer in your pages remarked that the way of the transgressor was hard. I have long realised how very hard and thorny it was, but it is comforting to think that your leader was written in the twentieth century and not in the fifteenth. Otherwise its effects would assuredly have been exceedingly painful for a certain person who has really very little enthusiasm for scientific research. But with all deference the points are not whether my embryological results be "very hypothetical," notwithstanding all that some very distinguished men may think of them, not whether the £10 8s. 5d. of Carnegie research money might have been devoted more profitably to some big cancer research, but whether the existence of an antithesis of enzymes be true and whether or not this be at the basis of

all the havoc wrought by cancer. Should this be correct, what scientific value, if any, would attach to the strictures in your leading article? Were the researches of Petry and of Hartog (which I meant to do myself) "wild speculation"? If a malignant tumour possess an enzyme totally different from that widely present in traces in normal tissues, is not that a fact of supreme significance and import? I ask your readers to judge my work for themselves. Its chief conclusions have been advanced in Liverpool before an audience including leading surgeons, physicians, and pathologists, and again more recently in the presence of a certain distinguished physiologist. On these occasions they were listened to with the deepest interest and they evoked no adverse and depreciatory criticism whatever, even on the Edinburgh research funds. Lastly, let it not be forgotten that at times my findings and conclusions have an awkward habit of turning out to be true.

I am, Sirs, yours faithfully,

Feb. 5th, 1905.

J. BEARD.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Jan. 14th, p. 120, under the heading "Local Irritation and Cancer," I suggested as an additional or alternative theory to cholesterine that glycogen in the same way leading to cell proliferation or alteration of type of division "may be due either to undue local deposition in the tissues or dependent on some nutritional disturbance in the liver or pancreas." Moreover, it had seemed to me, though I had not elsewhere seen it noted, that a similarity existed between this excess of glycogen in malignant growths and the excess of glycogen in foetal tissues which obtains from a very early period up to the fifth month: when histological differentiation has largely advanced it begins to be deposited in the usual place, the liver, and its use to the economy becomes that to which it is put in the ordinary life of the animal (M. Foster).

In view of my belief, therefore, that in malignant disease the tissues have in some sense reverted to glycogenic tissue, either from over-production or non-conversion of glycogen, locally or generally, it may be of interest to point out that on Dec. 8th, 1904, I applied to Messrs. Parke, Davis, and Co. more especially to ascertain if taka-diastase had any amylolytic action on glycogen, suggesting at the same time pancreatic ferment. They replied that taka-diastase had little or no action but that the glycerine extract of pancreas had. Meanwhile as a result of experiments made by Mr. F. W. Gamble, pharmaceutical chemist, with various ferments, trypsin was found immediately and most effectually to break up glycogen. It is of interest, therefore, to me to find that Dr. J. Beard in his paper on the Cancer Problem¹ independently corroborates the view expressed in the above extract from my letter that it is the pancreas which is at fault. Though his explanation of the use of trypsin in foetal life and presumably in malignant disease is different from mine it may interest him to learn that already on Jan. 19th, 1905, I had commenced to make trial of hypodermic injections of trypsin locally, together with the internal administration and outward application of pancreatic solution in a case of advanced recurrent cancer of the breast.

I am, Sirs, yours faithfully,

JOHN A. SHAW-MACKENZIE, M.D. Lond.

London, Feb. 6th, 1905.

THE PUBLIC HEALTH LABORATORY OF MANCHESTER.

To the Editors of THE LANCET.

SIRS,—Will you kindly allow me to rectify an error which has crept in in the account which you gave in your issue of Feb. 4th of the opening of the Public Health Laboratory in Manchester? Your informant states that there is a staff of 13 assistants, five students, one secretary, &c., in the laboratory. This statement is obviously the result of some confusion; there are only three medical assistants (Dr. Sidebotham, Dr. Carver, and Dr. Sellers) engaged in reporting to sanitary authorities, in addition to which Dr. Sidebotham is also lecturer in practical bacteriology and Dr. Sellers assistant lecturer in comparative pathology. Dr. G. Fowler has recently joined the staff as senior chemical assistant and Dr. Ramsden, who

is University Fellow in Sanitary Chemistry, acts occasionally as junior assistant. No student or unqualified assistant takes part in public health investigations. There is also a technical assistant (Mr. Simons). The staff, including the secretary and myself, consists, therefore, of eight persons; only six, including myself, are qualified to undertake investigations for sanitary authorities. Several other gentlemen take part in the teaching of public health or other advanced students but they are not assistants in the department. Although the matter is not one of much importance I do not think it desirable to allow what might reasonably be considered a great exaggeration to come under my notice without attempting to rectify it.

I am, Sirs, yours faithfully,

Feb. 7th, 1905.

SHERIDAN DELÉPINE.

THE PROGNOSIS OF ADOLESCENT INSANITY.

To the Editors of THE LANCET.

SIRS,—In an annotation in THE LANCET of Feb. 4th, p. 312, under the heading of "The Premature Dementia of Adolescents," you quote the opinion of Dr. Sérieux of Paris as to the curability of the "hebephrenic" and "katatonic" forms of "adolescent insanity" being only 1 in 10. In my opinion—and this has been formed on a careful study of the disease from statistical and clinical facts—the statement is calculated to produce a very unduly pessimistic and painful general effect on your professional readers who have to treat such cases and on readers who have relatives suffering from "adolescent insanity." The facts, I maintain, do not warrant such an unfavourable view. In 1873 I segregated and described a large group of mental cases with similar symptoms and called it "adolescent insanity." I subsequently gave an extensive series of clinical and statistical facts in support of my conclusion. The name and the description have been almost universally accepted by subsequent observers and authors, who, by the way, have seldom given me any credit for the induction. More recently German and American authors have adopted the term "dementia precox" to describe a sub-group comprised within adolescent insanity. Unfortunately no two of those authors mean the same thing by "dementia precox." Kraepelin has recently still further subdivided "dementia precox" into the divisions of "hebephrenic," "katatonic," and "paranoid." If any of your readers can form definite clinical pictures of those divisions from Kraepelin's descriptions and can then with confidence assort his patients under those groups I confess it is more than I have been able to do. I have always most strenuously contended that the term "dementia" should be reserved for incurable varieties of mental enfeeblement. This would save confusion, help to clarify our nomenclature, and, above all, would enable the general practising members of our profession to know better where they stand in using the word "dementia." I have no objection to the terms "hebephrenia" or "katatonia." I have much doubt about "paranoia," that fascinating and ever-changing rubbish heap of delusion, obsession, hallucination, and over self-appreciation into which some authors cast 50 per cent. of the insanities, and others—Kraepelin lately—restrict to about 5 per cent. of all the cases. But to attach those terms to forms of "dementia" seems to me an instance of medical classification gone to the verge of absurdity and confusion.

As to the curability of adolescent insanity, I took 1796 consecutive cases of insanity and found that 320 of these were in the developmental ages between 14 and 25 years inclusive. Of those 320 I had to deduct 90 as belonging to groups such as the epileptic and other forms. I found that the last five years, from 21 to 25, produced most of the 230 remaining cases, or about 70 per cent. of them. That is the time when the higher brain centres attain their full and final development. Especially do the highest emotional, volitional, and moral faculties then blossom and seed. To provide the infinitely delicate brain vehicle for such fruit of mental evolution must try to the utmost the potentialities of the developmental nervous process. It is then evidently that hereditary mental defects chiefly show themselves and cause psychical breakdowns, just as in the earlier years the lesser developmental neuroses, such as chorea, epilepsy, hysteria, somnambulism, asthma, and megrim, are most apt to appear. A careful study and summarising of the symptoms in the 230

¹ THE LANCET, Feb. 4th, 1905, p. 281.