

the diagnosis of adherent pericardium was made and the history warranted the diagnosis, costo-pericardial adhesions were not found and the sac was not opened. Nevertheless the result of allowing the hypertrophied heart more space to act in, and permitting it to gain that space through a non-resistant surface and not at the expense of the bone-bound thoracic contents, was decidedly beneficial. This is not the place to discuss the many interesting scientific questions which Brauer and Wenckebach raise in their papers, but I have mentioned the details given for this reason—namely, that Brauer's operation seems indicated, not merely in the comparatively rare cases of viscero-costo-pericardial adhesion, but also in the much commoner cases of hypermyosis of the heart with valvular lesion and without extraneous tethering.

The importance of a correct diagnosis, it appears to me, after reading Brauer's and Wenckebach's cases, is less as to whether we have actually to deal with adherent pericardium than as to whether we have to do with notable hypertrophy with or without dilatation, or with cases in which notable and persistent dilatation is the chief cause of a state which threatens the life of the patient. This is not always an easy matter to determine. For systolic recession of the interspaces may often be observed with a bulky heart which is untethered and which, from its very enlargement, is comparatively fixed in its extended area.

Thus, I had under my care last year at the Children's Hospital, Paddington Green, the case of a girl, 14 years of age, whose large, distressed heart with paradoxical pulse I regarded as one of adherent pericardium, but who improved so much under ordinary cardiac treatment as to render this supposition doubtful. She was indeed quite convalescent, but unfortunately contracted typhoid fever of a severe type and went through it for a month. At the end of that time she died exhausted but without notable retrograde stasis of the circulation, and at the necropsy her heart was found to be quite without hampering adhesions, and while hypertrophied and dilated not excessively so. It was a case of mitral stenosis. Even, however, should erroneous diagnosis in some cases lead to the operation of cardiolysis, as Brauer terms it, it is unlikely that the hypertrophied heart, judging from the results of the cases published, would be other than benefited by this procedure.

I must apologise, Sirs, for occupying so much of your space, but I consider the subject an important one, and it is one which has interested me for some ten years or so. I only blame myself for a conservative timidity which has prevented my putting these conclusions into practice sooner, and hope soon to have an opportunity of acting otherwise, in view of the results secured by Brauer and Wenckebach.

I am, Sirs, yours faithfully,

ALEXANDER MORISON.

Upper Berkeley-street, W., Jan. 26th, 1907.

TYPHUS FEVER IN TENERIFFE.

To the Editors of THE LANCET.

SIRS,—I have read with great interest the letter on this subject by Dr. W. H. Peile which appeared in THE LANCET of Jan. 26th. May I add the following? About a fortnight ago, whilst I was on board, the s.s. *Medic* called at the town of Santa Cruz, Teneriffe. She remained there 24 hours, but during that time no passengers were allowed to go ashore because of the existence of a few cases of typhus fever on the island. So far so good. The sanitary authorities of Santa Cruz, however, permitted dozens of semi-coloured hawkers to board the *Medic*. These hawkers, coloured, dirty, and repulsive, came aboard with fruit, silks, linens, &c. There is little doubt from what localities of Santa Cruz these people must have come—most probably from the fever-infested portions. Is this the usual way in which quarantine regulations are enforced?

I am, Sirs, yours faithfully,

Edinburgh, Jan. 26th, 1907. E. ARCHER-BROWN, M.B. Edin.

MEAT DIET AND THE TEETH.

To the Editors of THE LANCET.

SIRS,—In a letter on this subject published in THE LANCET of Jan. 26th Dr. Edmund Spriggs gives a candid criticism on some facts and opinions which I presented at a recent meeting of the Pathological Society, and which were reported in your issue of Dec. 29th, 1906. As some of the statements

in this letter may have given your readers an erroneous impression both as to the facts and to the author's interpretation of them, perhaps you will kindly allow me to reply to his criticism.

Dr. Spriggs states that "in the specimens shown at the Pathological Society on Dec. 18th, 1906, a distinct difference was apparent between a tooth germ in the meat-fed and one in the bread-and-milk-fed animal. The section of the head of a meat-fed rat, thrown upon the screen in order to demonstrate the bone changes, showed a developing tooth which was not a normal one," &c. The first of these statements is correct; the second is entirely wrong, Dr. Spriggs having been led into error through an imperfect acquaintance with the fallacies involved in this department of histological work. As the point may be of some interest to future observers and can be more authoritatively dealt with by an expert, I have asked Mr. Gibbs to reply to Dr. Spriggs's comment on the histological appearances of the teeth.

My critic further writes: "At that meeting of the Pathological Society I suggested, as reported in THE LANCET of Dec. 29th, 1906, that Dr. Chalmers Watson was mistaken in ascribing many of the effects observed by him to a meat diet and that they were probably due to a deficiency of lime and other bases." In my reply to this criticism I pointed out that my communication was restricted in its scope to a demonstration of the structural changes which had followed the use of an excessive meat diet; it did not include the consideration of the causation of any of the changes observed. The experiments referred to by Dr. Spriggs in support of his contention that the deficiency of lime in the diet was the cause of the structural changes were well known to me, as were also other experimental data relevant to this subject. I had also considered the view that the deficiency of lime might be an important factor in the production of some of the changes but had refrained from forming any definite conclusion on this point, and, with all due deference to Dr. Spriggs's views, I am still of opinion that it is inadvisable to form any theory as to the causation of the structural changes until the investigation is more complete. I would like, however, to point out that the changes in the osseous system, which seem to have particularly attracted Dr. Spriggs's attention, are, in my opinion, among the least important of the structural changes demonstrated at my meeting.

The most important point in Dr. Spriggs's letter remains to be dealt with—viz., the statement: "Indeed, whether the results produced in rats are due to the want of lime and other bases or whether they are an expression of the failure of a naturally omnivorous animal to rapidly adapt itself to a purely flesh diet it is clear that they cannot be directly applied to the case of civilised man, who never in ordinary circumstances lives upon a diet of meat alone." This criticism raises an issue of very great interest and importance and with your permission I would like to answer it in some detail. Before doing so, however, I would like to feel assured that I correctly interpret Dr. Spriggs's position in regard to some facts relevant to the issue which were demonstrated at the meeting of the Pathological Society. Might I ask him whether there is, in his opinion, any essential difference between the histological appearances of the bones in the second generation of meat-fed rats and those of the bones of the infant aged 16 months, the child of a tuberculous subject who had been fed for a prolonged period prior to and during gestation on a diet containing a great excess of meat? Photographic reproductions of the actual slides shown at the meeting are given in a paper published in THE LANCET of Dec. 8th, 1906.

I am, Sirs, yours faithfully,

Edinburgh, Jan. 27th, 1907.

CHALMERS WATSON.

To the Editors of THE LANCET.

SIRS,—In reply to the first part of Dr. E. I. Spriggs's letter in THE LANCET of Jan. 26th, p. 252, I must express my surprise that he should come to any conclusions as to the histology of a tissue merely from seeing thrown on a screen a photomicrogram of a low-power magnification of an object which he had never examined under the microscope itself. Dr. Spriggs is also possibly unaware of the very great difficulties that beset the histological investigation at the same time of such fully calcified tissues as enamel and dentine and of the soft tissues immediately in contact with them. Owing to very great numbers of sections having to be cut and examined the paraffin method was the only practicable

one, the result being that a certain proportion of sections, so far as the dental tissues were concerned, had to be discarded owing to the very obvious artifacts they showed. I have re-examined *microscopically* the sections, photographs of which Dr. Spriggs saw on the screen, and it may interest him to know that for the purposes of the investigation both of these sections were rejected for this reason. I might also point out that in investigating such a difficult subject one does not draw conclusions from a comparison of any two sections but from a comparison of a very large number.

The photo-micrograms shown by Dr. D. Chalmers Watson were not from any of the three rats I referred to as having hypoplastic teeth; and, as stated in a previous letter, neither Mr. G. W. Watson nor I was able to detect any sufficiently constant variations in the tissues, hard or soft, to warrant us in concluding that there were any essential histological differences between the teeth of meat-fed and of bread-and-milk-fed rats. I am, Sirs, yours faithfully,

Edinburgh, Jan. 27th, 1907.

J. H. GIBBS.

THE PROPHYLAXIS OF CANCER AS INDICATED BY THE PARASITIC THEORY.

To the Editors of THE LANCET.

SIRS,—No one can have followed the progress of cancer research in this and other countries without being impressed with the vast extent of the knowledge which has been acquired in recent years, and yet he must also be impressed by the present limitations of that knowledge and must feel how little progress has been made in the matter of actual dealing with the disease. In the course of this research certain facts relating to cancer seem to have come into special prominence of late. To the writer, as an outside observer, these seem to be the following: (1) the widespread and almost universal distribution of cancer; (2) the extraordinary powers of growth exhibited by the cancer cells within the body; (3) the great vitality exhibited by these cells without the body; and (4) the ever-increasing evidence that in a large proportion of cases of cancer infection is by direct inoculation from without.

These facts seem to throw some light on those portions of the life-history of cancer which hitherto have lain in obscurity. Of one part of that life-history we know something. We trace the primary growth presumably from a single cell. This by its rapid growth soon becomes a tumour containing thousands of cells, each of which when carried away by the lymph or blood may become a secondary growth. Thus by the end of life one cell has become a million, and then with the patient's death they once more pass out of our sight. These facts to which I have referred seem, however, to indicate that the life-history of cancer does not end with the life-history of its host, but that long persisting in the bodies of the dead they may in all probability withstand the forces which destroy those tissues and in some resistant spore-like form become by slow degrees disseminated through the soil, carried by worms or birds, or by the deep water channels and thus account for the far-reaching distribution of the disease, its universality, and its widespread incidence. If this be so, it bears some analogy to the dissemination of tubercle bacilli, with their wide distribution in nature, their great resistant powers during extra-corporeal life, their somewhat difficult "adhesion" to the human host, and their powers of growth when once they gain access to that host.

In the present argument the cancer cell is regarded as the infecting agent. This is generally admitted to be the case when dissemination takes place within the body and the effect of radical operation strongly supports this view. Evidence has been brought forward to show the extraordinary powers of resistance to chemical agents, cold, &c., of those cancer cells outside the body and the history of cancer-infected mice cages and of cancer houses is also in favour of their vitality being long maintained under such conditions. The primary incidence of cancer, so rarely on the lungs, but so commonly on external surfaces and on the alimentary tract at points of lowered vitality, or, again, at similar points where the blood stream might carry the virus, seems to indicate an infection by some relatively heavy organism, such as a cell, which is very widely distributed in nature, and which may, though with difficulty, be inoculated from food, clothing, or other means of contact, but which is hardly ever air-borne. The difficulty of

inoculation bears resemblance to that of tubercle, only is much more marked. Once established, however, the powers of growth in the human body are far greater, but the tissue reaction excited is much less, this, again, being in favour of an infection due to some cellular organism closely resembling the body cells rather than to any extraneous micro-organism.

Finally, the contrast between the growth and multiplication of these cells within and without the body is most remarkable. Within we can trace the development from a single cell of the millions of cells represented by the secondary growths of "general dissemination." Without, while we have much evidence of the maintained vitality of the cancer cells, we know of no conditions under which they may continue to grow or to multiply. In the human subject we recognise that the virulence and infectivity of cancer vary in direct ratio to the rapidity of cell growth—in other words, to the number of cells. Is it not, therefore, reasonable to suggest that the widespread distribution of cancer is due to those myriads of cells committed to the soil with every patient dying from cancer, which subsequently by slow degrees become disseminated throughout nature?

Turning now to the question of the treatment of cancer, we feel that so far little progress has been made. No specific has yet been found and no antiserum has been obtained. Vaccine treatment in the case of mice seems to have given some result, but the difficulties of applying any such treatment to the human subject can readily be imagined and we still remain with only one cure—namely, early and complete excision. In recent years medical science has been increasingly occupied with the prevention rather than with the cure of disease. In the case of tubercle the importance of this is being more and more realised, as we see in the active crusade against the dissemination of tuberculous sputa and the efforts at isolation of advanced cases. No effort, however, seems to have been made to deal with that large source of tubercle bacilli which is found in the dead bodies of tuberculous patients which probably contributes largely to the distribution of the bacilli in nature. This may be of importance in tubercle, but from the arguments above brought forward is far more likely to be so in the case of cancer. Hence the writer would put forward a strong plea for an attempt at the prophylaxis of cancer, a goal which should not be unattainable if the above suppositions are correct. Granted this, the prophylaxis should be relatively easy, for in this case the great source of infection must be the bodies full of secondary growth which are committed to the soil at death, while other sources, such as cancerous discharges and infected excreta, are comparatively unimportant and easily dealt with.

The cancer cells are readily destroyed by heat, and thus if cremation of all those dying from cancer can be legally enforced the great source of the disease will be eliminated, and it is not unreasonable to hope that by such methods a gradual diminution in the frequency of the disease or even its ultimate eradication may be attained. The writer can lay no claim to any share in the work of cancer research which has been carried on with so much ability and perseverance, but having followed its progress with the deepest interest he ventures to bring forward these suggestions which have been prompted by the work of others.

I am, Sirs, yours faithfully,

H. V. WENHAM.

St. Bartholomew's Hospital, E.C., Jan. 23rd, 1907.

ELECTRIC LIGHT IN MARYLEBONE.

To the Editors of THE LANCET.

SIRS,—Your reference to the failure of the electric light in Marylebone within the past few weeks and your warning to surgeons undertaking to perform operations with the aid of artificial light within the limits of the borough to provide against the recurrence of such a contingency is extremely opportune. However, the statement that while such events are unusual in other parts of London they have been frequent in Marylebone since the council undertook the lighting is more open to question. As a user of electric light, both under the old régime and since it has been taken over by the borough, I am in a position to say which is the greater sinner. It used to be a matter of frequent and almost weekly occurrence. Indeed, the failures were so frequent that a deputation of medical practitioners waited upon the council and asked them to take some action to put an end to