

Semmelweis, and to him alone, we owe the modern etiology and pathology of childbed fever—"Puerperal-Fieber ist Wund-Fieber: Wund-Fieber ist Wundvergiftung," and "all infection comes from without." Upon these doctrines our prophylaxis and our therapeutics are entirely based.

I am, Sirs, yours faithfully,

Manchester, Jan. 21st, 1907.

WILLIAM J. SINCLAIR.

To the Editors of THE LANCET.

SIRS,—Your annotation on "The Respective Merits of Semmelweis and Oliver Wendell Holmes in the Treatment of Childbed Fever" does not give credit to the man who first proclaimed its highly contagious character—viz., Gordon of Aberdeen; and although according to Dr. T. C. Allbutt of Cambridge "the benefactor to whom the world's gratitude and memorial are due is he who *makes the thing go*," we must not forget him, who at such an early date did what was possible in his day to enlighten the profession on the subject.

I am, Sirs, yours faithfully,

JOHN HADDON, M.D. Edin.

Denholm, Hawick, Jan. 22nd, 1907.

THE INTRAMEATAL OPERATION.

To the Editors of THE LANCET.

SIRS,—May I be allowed to once again encroach upon your valuable space in order to reply to the inaccuracies contained in Mr. F. Faulder White's letter in THE LANCET of Jan. 5th, to which my attention has only just been drawn.

Mr. White is under a serious misapprehension when he says that he is gratified to find that I am "now in favour" of the intrameatal operation. If he will consult the *Clinical Journal* of May 24th, 1899, he will find a paper by me on Ossiculectomy which will convince him that I have been "in favour" of that procedure for some considerable time. As regards the paper on the mastoid operation to which he refers, I in no way withdraw one single word of what I then wrote. Mr. White (who at one time advocated the curing of every case of otorrhœa by irrigation) has written much about his operation, to which he has given the name of "otectomy" (literally, removal of the ear), but I have never read any account of it that does not plainly show it to be merely an ossiculectomy, with removal of the outer attic wall. The latter addition I, in common with other otologists, have done for several years past, as reference to the Transactions of the Otological Society will show. Mr. White writes as if he alone had advocated ossiculectomy and that in the face of opposition from every otologist; as a matter of fact, Mr. White, starting with universal irrigation, now pushes universal "otectomy" as zealously as he once insisted on the wickedness of operating at all.

In conclusion, I would like to add one word of comment upon Dr. Frederick Spicer's letter in THE LANCET of Jan. 5th. Mr. Charles J. Heath's former paper was read before the Otological Society and was there severely criticised, and most justly so.

I am, Sirs, yours faithfully,

MACLEOD YEARSLEY.

Upper Wimpole-street, W., Jan. 17th, 1907.

ABORTION AMONG THE ANCIENTS.

To the Editors of THE LANCET.

SIRS,—Referring to an annotation under this title published in THE LANCET of Jan. 19th, p. 178, where a recent paper by me is discussed, allow me to state briefly that your critic or reviewer does not seem to have understood me perfectly.

I never dreamed of saying that "abortion was not considered immoral by the early Christians," but I stated that neither the Old nor the New Testament mention that subject. I showed that among the Pagan philosophers opinions varied about the immorality of abortion, but that there is good reason for supposing that the so-called Orphic sects condemned abortion very strongly. The same horror for the practice of abortion was found to prevail in early Christian literature (later than the Gospels), is in accordance with the morals of Orphism, and may very well have borrowed its expressions from that source. All those who have studied the figures of Orpheus in the Roman catacombs and the texts of the early Fathers, claiming Orpheus as a disciple of Moses, will readily acknowledge the possibility of an Orphic influence on the written morals of Christianity, wherever the point at issue has been left untouched by the Holy Writ.

I never said or thought that Virgil, in the passage, *Æneid* VI., 426-30, had *meant* the infant victims of abortion. But it is well known to scholars that Virgil, especially in that part of his poem, imitated and combined Greek sources, often at second hand, and without understanding them as a modern scholar would try to do. Now, I have shown that in the passage in question victims of an unjust, premature, and violent death are grouped together; the only *apparent* exceptions are the newly born babes. So I ventured to suggest that, in the Greek source lost to us, but known to Virgil and followed by him with some sort of sluggishness, that apparent exception did not exist and that the newly born babes were the victims of abortion. Further, by a quotation of the pseudo-Petrine gospel, which has many points in common with Virgil's Greek and Orphic source, I made it pretty clear that my hypothesis may almost be considered as a certitude, inasmuch as we have a right to reason about a text which we do not possess, with the help of other texts derived from it. I would be astonished if any professional scholar maintained a contrary opinion after having taken cognisance of mine.

I am, Sirs, yours faithfully,

Paris, Jan. 22nd, 1907.

SALOMON REINACH.

THE OPERATIVE TREATMENT OF ADHERENT PERICARDIUM AND CARDIAC HYPERTROPHY.

To the Editors of THE LANCET.

SIRS,—On July 28th of last year you did me the favour to publish in THE LANCET a paper on Pericarditis in Childhood. My chief incentive in publishing that paper was the hope—not realised—that my proposals for the surgical treatment of adhesive pericarditis and cardiac hypertrophy might be discussed by physicians and surgeons whether by way of approval or condemnation. I endeavoured to show by a study of the growth of the heart both in embryo and under pathological conditions that what the labouring heart in certain circumstances requires is *more room*, whether to grow in or to act in. I mentioned that to the derision of a reviewer of my book on "Cardiac Failure" (1897) I had indicated cardiac symphysiotomy as a possible triumph of surgery and was met with the retort that I had in any case exhibited a triumph of imagination. Towards the end of my paper I remarked: "I am aware that in speaking thus (in advocacy of the procedure) I do so on theoretical grounds and not from actual experience, but such thought has frequently preceded action ultimately justified by results, and I am not unhopful that surgery, which has accomplished so much, may also in well-defined circumstances find a place in some cases of adherent pericardium. To return to our embryo, the growing organ requires a surplus of room to grow in; the overgrown organ requires more room to work in, and it may be that tethered by extraneous adhesion, or *not so restricted*, the hypermyotic heart may in the future and in some cases be provided with such by the genius and courage of some surgeon bold enough to undertake the task."

I was not aware when I wrote this passage, and indeed not until I read an interesting article on Some Points in the Pathology and Treatment of Adherent Pericardium by Professor Wenckebach of Groningen, which appeared in the pages of your contemporary the *British Medical Journal* (Jan. 12th, 1907), that all this had already been successfully done by surgeons acting under the inspiration of Professor Brauer of Heidelberg. The paper of the latter is published in Langenbeck's *Archiv für Klinische Chirurgie* (Vol. LXXI., 1903), and relates the result of the procedure in three cases. His patients were two men and a youth, aged respectively 50, 25, and 16 years. The principle of the operation undertaken was to provide the heart with more room to act in and less obstacle to pull against, and no attempt was made to open the pericardium or to break down possibly existent adhesions either within the sac or beyond the area of the external opening in the thorax which was made by the removal of ribs and cartilages and a portion of the sternum sufficient to allow of freer movement of the heart. In fact, there is nothing positive to show, notwithstanding the criteria of differential diagnosis given, that such adhesions existed, except that in the case of the man, 25 years of age, who died from influenzal pneumonia ten months after decidedly beneficial operation, the pericardial sac was proved post mortem to be adherent to the heart. In Professor Wenckebach's case also, although

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