

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 unhopeful 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