present influences the result as it reacts acid with this indicator and not with litmus. With litmus the optimum point I find to lie at what may be called the acid edge of the amphoteric zone. The same person will have no difficulty in getting a bouillon constant in reaction at different times, but might find a different end-point from that stated to be the optimum for him. The object of fixing the titration point is to secure freedom of the bouillon from the clumps of apparently dead bacilli which form a scum or sediment in the tube and give rise to very puzzling pseudo-reactions. Their influence on the result has been considered by Widal so serious a source of experimental error that the habitual examination of a control drop to which no serum has been added is insisted on in every case. This disturbing element is even more troublesome in dealing with solutions of dried blood than with fluid serum. We have, however, been able to eliminate it permanently by paying strict attention to the culture media and the reaction here recommended is that which has been used from the first in my own observations except during two periods of a few weeks each. On both these occasions an accidental departure from the method of preparing the culture led to unsatisfactory results, which promptly disappeared when the former routine was resumed.

If the culture is less acid than 3.5 per cent. to phenolphthalein the growth is more abundant and gives within twenty-four hours both scum and deposit in which clumps of motionless bacilli appear. On the other hand, with bouillon more acid than 3.5 per cent. the motility becomes greatly lessened and is lost at about 4 per cent. acid. The serum reaction, though still obtainable, is retarded. Between 5 and 6 per cent. it is difficult to obtain any satisfactory growth, the limit of growth varying in different races.

With serum in agar the reaction of the media has not the same degree of influence. With the colon bacillus the tendency to form clumps of motionless bacilli is greater than in the case of typhoid and the tendency to yield pseudo-reactions is correspondingly greater. We find, however, that by using a somewhat more acid bouillon than for typhoid (about 4 per cent. acid to phenolphthalein will answer) the density of the growth can be controlled in a similar manner and the tendency to pseudo-reactions disappears.²

I am, Sirs, yours faithfully,

Montreal, Nov. 30th, 1897.

WYATT JOHNSTON, M.D.

"PROPOSED AMENDMENTS OF THE NOTIFICATION ACT."

To the Editors of THE LANCET.

SIRS,—As you observe in your leading article of Nov. 20th and as Dr. Young and Mr. Garstang state in their letters in your issues of Nov. 6th and 20th it is generally admitted that some amendment of the present Notification Act is necessary; but surely a more general agreement of opinion is desirable before a Bill is drafted. There are two important matters which seem to have been so far omitted and which it is desirable should be included in any future legislation on the subject. The one is that certainly all cases of phthisis—preferably all cases believed to be tuberculous in origin—should be included in the schedule of diseases to be notified. The other that all cases of "fever" after childbirth and miscarriage should also be notified if occurring during the first fourteen days after delivery. With these important additions I should agree generally with Mr. Garstang's suggestions, the only great exception being that I should strongly support the present fee of half-a-crown being retained.

I am, Sirs, yours faithfully,

Braintree, Essex, Dec. 6th, 1897.

THOMAS CARR.

"THE COMPARATIVELY STATIONARY DEATH-RATE IN PRIVATE OBSTETRIC PRACTICE."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Oct. 2nd you granted space to a communication from me under the above heading. Upon thinking over the matter afterwards I regretted that I had

not adopted the literary methods so familiar to us all in boyhood during our researches into the writings of G. P. R. James *et hoc genus omne* and so have indicated the full scope of the matter with which I dealt by adding an alternative title. My subject would then have been expressed in some such terms as "The Comparatively Stationary Death-rate in Private Obstetric Practice or The Sorrows of carrying out Asepsis." I am firmly convinced of the tragic truth of the opening paragraph of Dr. Jellett's "Short Practice of Midwifery" which was recently so favourably reviewed in your columns: "It is not an exaggeration to say that the most essential knowledge in midwifery is the knowledge of asepsis. A practitioner who knows nothing of the science and art of midwifery, except that it is absolutely necessary that his hands and instruments be sterile, will save more life than the most accomplished obstetrician who does not practise asepsis." The overwhelming importance of the subject itself, together with the interest recently taken in the discussion upon the causes as to why, during the last twenty years, the relative positions of the great maternity hospital and private obstetric practice have been so radically altered, to the great disadvantage of the latter as regards the rate of mortality, lead me to hope that you may allow me to supplement and amplify a little my former letter. In it I attempted to show by the simple recital of an everyday experience that the difficulties to a great extent proceed from the ignorant misconception of the patient or her friends. In the case to which I referred the difficulty arose from a "nurse" objecting to my methods, firstly, because I washed and scrubbed my hands so much that I appeared over-fastidious, and secondly, because by refraining from making unnecessary vaginal examinations I did not earn my fee by "helping" the patient as I ought. A few days after my letter appeared in your columns I had, however, an equally instructive experience in which the difficulty arose from the patient herself. The case was a very tedious primipara, with contracted outlet, requiring forceps in the end, and when it was all over the patient expressed herself in most grateful terms for the care taken of her in her troubles. The nurse upon this occasion was a very intelligent and capable one and had been trained at Queen Charlotte's Hospital. Upon talking matters over with her she complained of the difficulties of carrying out asepsis. She said she was much amused by her patient after I had made my first call just at the beginning of her tedious labour expressing a hope that I would not come again. Upon the nurse asking for some explanation of this the patient had replied: "Oh, he rubbed his hands so much that they must have been very dirty beforehand, and it is such a curious thing for a doctor to have had such dirty hands as to require all that scrubbing."

As I have before said, the woman was herself in the end very grateful for services rendered, and the nurse soon removed her misconceptions as to the object of the scrubbing; but what can one think of the line of reasoning? It is such erroneous deductions, together with the old belief in empirical methods, which constitute the greatest difficulty in faithfully following asepsis in private practice. Amongst these empirical methods none can be more mischievous than the idea that an ordinary vaginal examination is made with the object of "helping" the patient out of her trouble—a belief which is practically universal amongst the women of London. It is in connexion with this that I wish to point out that the fault does not rest entirely with either the patient or her friends. It has long been one of the stock commonplaces of feeble wits that the practising of the art of medicine amounts to an infinite capacity for humbug. This criticism, however, is but an exemplification of the truth of Byron's famous lines:—

"Believe anything that's false before
You trust in critics who themselves are sore."

The critics in this case are simply ordinary specimens of human nature who are self-conscious of the fact that when they themselves seek medical advice they expect to be "humoured." But, granting all that can be urged on this score, surely there is no need for anything more than an explanation to the patient of the simple truth—namely, that a vaginal examination in an ordinary case

² The influence of the acidity or alkalinity of the culture medium has already been referred to by Bates Block and by myself before the American Medical Association (June, 1897), and by D. MacTaggart and myself in a communication at the Montreal meeting of the British

Medical Association not yet published. I would emphasise the importance of paying special attention to the condition of the culture in order to get the best results where dry blood is used. A very full consideration of standardising media by phenolphthalein is given by G. Fuller in the Journal of American Public Health Association for 1896 (October).