cerned, the cases quoted by Professor Saundby can scarcely be claimed as instances of bacillary dysentery as known in the tropics.—I am, etc.,

E. A. C. Matthews, M.B.(Cantab).
Cawnpore, July 12th. Captain I.M.S., 10th Lancers.

SUPRAVAGINAL AMPUTATION AND TOTAL HYSTERECTOMY FOR FIBROIDS.

Sir,—Dr. Haultain, in the British Medical Journal of August 4th, makes some observations on the relative value of total hysterectomy and supravaginal amputation for fibroids on which as an advocate of total hysterectomy I should like to make some comment.

In the first place, it should be borne in mind that by excluding cases of total hysterectomy, which is the only admissible operation for some of the most dangerous cases of fibroids (those growing in the cervix and those complicated with malignant disease), the statistics of the partial operation do not give the full mortality of hysterectomy for fibroids, but only that for the simpler cases. Dr. Haultain himself, who has had such excellent results with supravaginal amputation, had two deaths out of 15 cases of total hysterectomy.

Dr. Haultain gives (without details or reference) the mortality of hysterectomy during the last five years as less than 5 per cent.; he does not clearly state whether this figure has reference to the total operation or the partial operation or both; but, if it refers to the partial operation only, it is, in addition to the fallacy alluded to, clearly unfair to compare statistics of operations performed during the last five years with statistics extending from many years back up to six years ago. Dr. Haultain says:

> Panhysterectomy, or removal of the entire uterus, has been advocated by some operators, but has little specially to recommend it, except in the rare cases in which the cervix is involved by the growth, or some malignant uterine complication is present. It certainly is an efficient bar to subsequent cervical cancer, but this sequel is so seldom met with as hardly to warrant the prolongation of the operation which is necessarily involved, or the increased mortality which is shown by statistics collected from the work of the best-known operators as 8.27 per cent. in 1,668 cases.

The figures in this paragraph are Praeger's, and have been published by me in the *Obstetrical Transactions* for 1905 (vol. xlvii, p. 403). I at the same time gave the mortality for supravaginal amputation with intraperitoneal treatment of the stump from the same source—Saenger and Herff (*Encycl. der Geb. und Gyn.*, 1900)—namely, 8.64 per cent. (2,025 cases, with 175 deaths). I am sure that others besides myself who are interested in the above question would like to know why Dr. Haultain has omitted these figures and states that statistics show an increased mortality for the total operation, when exactly the opposite is shown by Praeger's statistics, from which apparently he quotes.—I am, etc.,

London, W., Aug. 7th. Herbert R. Spencer.

"THE FETISH OF CLASSICS."

Sir,—While the study of the classics has come in for much condemnation, I never yet knew a distinguished classical scholar to be filled with "furious indignation" that he had received a classical education. I have known the lazy and indifferent to blame their subject instead of condemning their own indolence.

We ought to be men and gentlemen first before we are doctors or men of science. There is too little literary culture among us. We have to deal with human beings, not with machines. The problem we have constantly to face is complex, so complex that laboratory methods can only be ancillary to it. A nice estimation of probabilities is often our only guide to diagnosis and treatment, and this fine mental balancing of probabilities is one of the things that classical study promotes.

Anyone who has been a thorough student of the two classical languages will find the acquisition of French and German easy. A diligent schoolboy can acquire a fair knowledge of one or both languages on the classical side at our public schools.

What I have personally seen of schoolboy science, that is, chemistry, physics, etc., does not encourage me to think it worth while sacrificing any substantial knowledge for such a smattering.—I am, etc.

July 30th. Alexander.

P.S.—I may add that one of my sons was recently elected to a classical scholarship at one of the great public schools. Besides the usual unseen passages and compositions in Greek and Latin, he had to do two papers in French, one in mathematics, a general paper containing questions in modern history and geography and in English literature, and to write an English essay. For a scholarship in Natural Science at this examination *no boy qualified*.

Sir,—I cannot refrain from writing to express my heartfelt sympathy with your unfortunate correspondent "Alexine." It must indeed be particularly galling in later life to find that a little thought on the rationale of the educational system of one's youth might (if exercised at the right moment) have prevented such a terrible waste of the best years of a boy's life. Your correspondent had much better, of course, have been an "abnormal" boy; thought about the value of technical education, and revolted against the classics at a suitably early age. No doubt his experience of the classical scholar as one "who is incapable of appreciating the importance of any mundane affairs beyond the language and ideas of classical authors," is the experience of most of us; we have also occasionally met worthy scientists whose thoughts and views are similarly limited to the subject to which they have given their life-study.

As to the ideal head master whom "Alexine" sketches for us, I seem to remember a "classical" sixth form where essays on matters of contemporary importance were of frequent occurrence; but perhaps the head master was not an orthodox pedagogue, whatever that may be.

I am afraid, however, that "Alexine" fails to go to the root of the matter. His misguided parents and guardians hoped to be able to educate him, whereas what he really required was merely *instruction*. But there is still hope. He has doubtless forgotten the classical knowledge he despises, just as he would probably have forgotten French or German, or biology, or any other subject learnt as a matter of school routine; he is therefore no worse off than he would have been had he been sent to a "modern" school, and may with a clear mind continue to instruct himself technically without the least fear of ever becoming educated.—I am, etc.

July 30th. J. M. F. B.

THE BY-EFFECTS OF HYSTERECTOMY AND OÖPHORECTOMY.

Sir,—I have read with no little interest the admirable paper by Mr. C. J. Bond and the letter of Mr. Doran, who hardly echoes Mr. Bond's views of the functions of the endometrium in his general statements.

As I am at present particularly interested in this question, and am, in conjunction with Professor Benjamin Moore, working at various points in connexion with it, I trust you will spare me a little space for a few comments.

On May 3rd last I read before the Liverpool Medical Institution a preliminary note (vide *Liverpool Med.-Chir. Journal*, July, 1906), in which I made the definite suggestion that there was an internal uterine secretion, which I called "uterin," and which plays an important part in female genital activity. This note is, I believe, the first definite pronouncement of this view that has been published, although clinical facts which support it have been noticed and commented upon from time to time by various surgeons; for instance, Zweifel, and, following him, Mr. Doran, have stated that it is advisable to leave endometrium, whenever possible, in supravaginal hysterectomy—a practice I always adopt myself.

In my note, which was only a preliminary communication, I purposely kept back all clinical and experimental evidence in order to complete my investigations, being content to indicate the lines on which the work was proceeding.

With regard to Mr. Bond's paper I should like to call attention to some experimental errors which seem to negative at least one of the conclusions arrived at, namely, the action, or inaction as he concludes, of uterine secretion upon the ovaries.

In his own words, Mr. Bond's conclusion on the point is: "The presence of the uterine or endometrium tissue is not, on the other hand, necessary for the carrying on of ovarian function, either ovulation or the production of the internal secretion associated with oestrus."

This conclusion is arrived at after one single experiment—No. 1—which is described in the following words: "The whole uterus, including the cervix uteri, *but excluding the Fallopian tubes* (italics mine) was removed."

It is then stated that oestrus and coition occurred, and that when the animal was killed, he goes on to say: "Both ovaries are normal in appearance; each contains several red, raised, and recently ruptured follicles. . . ." The conclusion is then made: "This experiment shows that panhysterectomy has no deterrent effect on ovulation. . . ." I would like to ask Mr. Bond (1) How he knows that none of the uterine cornua was left? (2) How he could expect any other result than the one obtained if he left the Fallopian tubes? for I quite agree with what he says later in his paper that "the cavity of the Fallopian tube and uterus may be regarded as one membrane," although, perhaps, the English might be a little clearer. I gather, also, that he considers, as I do, their secretion, or secretions, to be similar, to some extent at least.

In my note, alluded to above, I pointed out that in the human subject the leaving of Fallopian tubes and cervix uteri must make any case of doubtful value in a study of this question.

Further, in regard to this experiment I would like to ask if he took the precaution to operate upon an animal before ovulation had ever occurred? I would not accept the evidence of ovulation as conclusive unless he had taken the precaution mentioned, but about which he says nothing. One of the great difficulties we have had in our experiments has been the youthful age of our subjects, and the consequent evil effects of shock upon them.

There are several other points in Mr. Bond's experiments that are open to criticism, but I will not occupy more of your valuable space than to ask him how he reconciles his mind to expect that the fluid which he collected in the artificial hydrosalpinx would be "absorbed into the circulation under pressure." He certainly produces no evidence to show that the collection of fluid was absorbed, in fact, in the recent words of *Mr. Punch*, it seems to be a case of "on the contrary."

I do not in these friendly criticisms intend in any way to detract from the great value and interest that attaches to Mr. Bond's excellent work and paper, but, since at the present moment I am interested in the question of internal secretion of "uterin," for which I hold a brief, I want to clear the ground; at the same time I feel I owe some apology for doing so until I have backed up my preliminary note by the publication of the detailed work and investigations, which I hope to bring forward in due course in conjunction with Professor Moore.—I am, etc.,

Liverpool, August 6th. W. BLAIR BELL,
Assistant Gynaecological Surgeon, Royal Infirmary, Liverpool.

AMOEBIC AND BACILLARY DYSENTERY.

SIR,—Practitioners in tropical medicine will take heart on appreciating the value of Dr. Foulerton's remarks, made in his letter which appeared in the BRITISH MEDICAL JOURNAL, July 21st, p. 170, under the heading Endemic Sporadic Dysentery (with Shiga's bacillus) in England—a subject which has of late been dealt with at some length in the medical press. I have not had the good fortune to see Dr. Foulerton's paper on the Etiological Significance of *Bacillus dysenteriae* (Flexner), as tested by the agglutinative reaction with the serum of patients suffering from dysenteric symptoms, but I have been considerably baffled by a very exceptional case of dysentery, contracted three years ago in Java, which has been under my charge here since last August.

I have considered this case to be one of mixed infection, this being due to the presence of the *B. shiga* as well as the *Entamoeba hystolytica* (Schaudinn), evidence of the former being obtained by agglutinative reactions in varying dilutions (5 per cent. and ½ per cent.) with cultures of the *B. shiga*, while the presence of the *Entamoeba hystolytica* (Schaudinn) was noted in the stools at the bedside microscopically. The *B. shiga* was not detectable, and Dr. Foulerton's concluding remark that Flexner had never found *Amoeba dysenteriae* side by side with *B. dysenteriae* in the acute cases, the former being found in chronic cases only, has greatly reassured me. Moreover, the absence of toxaemia, after the brief early period of a relapse, in these chronic cases would indicate the absence of the toxic bacilli, in the later stages, when evidence of the presence of the protozoon amoeba is more easily obtained at the bedside.

It should be noted that according to Professor Hewlett[1] these amoebae have not so far been shown to produce either intracellular or extracellular toxins, further, that they are practically extracorporeal, as in the case of the cholera vibrio, their transportation from the gut to within the organism, either in the liver or other abdominal glands is due more to accidental circumstances, and not the rule.

Shiga has tested the agglutinative reaction in hundreds of cases, and he found it generally parallel in intensity with the severity of the disease, except in very grave cases, usually fatal, in which he found the reaction but slightly marked. He has seen the reaction present as long as eight months after the attack, and he states that agglutination appears only in the second and third week of the disease and reaches its highest point during convalescence—a view which Dr. Foulerton would not seem to accept, as instanced in his case when the disease was contracted in China, the patient being just convalescent, and "no reaction was obtained with a dilution as low as equal parts of the serum and broth culture of the bacillus." On the other hand, Professor Metchnikoff, in his recent Harben Lectures, implied that a high agglutinating power, accompanied by a high opsonic index, sometimes forebodes a relapse. In the case of my patient here, his opsonic index to *B. shiga*, as well as the agglutinating reaction of his serum, were both carefully obtained simultaneously, at the laboratory of the Clinical Research Association, at different periods, during this his fourth and last relapse, and in this single case, when a high opsonic index, accompanied by a high agglutinating power, was obtained, recovery took place, and convalescence was maintained. No relapse has occurred during the past two months. I propose shortly to submit a detailed report of this case, with opsonic chart, etc., in connexion with the serum treatment of this case, with M. Vaillard's antidysenteric serum, which was kindly supplied to me by his instructions from the Pasteur Institute last March.—I am, etc.,

Bournemouth, July 23th. HENRY D. MCCULLOCH.

OBITUARY.

EDWARD FRANCIS WILLOUGHBY, M.D.LOND.,
D.P.H.LOND. AND CAMBRIDGE, M.R.C.S.ENG.

DR. E. F. WILLOUGHBY died at his residence, Finsbury Park, N., on July 29th, after a week's illness. He was in his 67th year. His medical education was acquired at University College Hospital, London. He became M.R.C.S. in 1865; graduated M.B.Lond. in 1869, with honours, and M.D. in 1889; and took the Diploma of D.P.H., both at London and Cambridge, in 1881. He was a Member of the Epidemiological Society, of the Society of Medical Officers of Health, and of the Royal Sanitary Institute, and was Honorary Medical Officer to the Mansion House Council on the Dwellings of the Poor. He was the author of several well-known works on sanitary subjects, including *Hygiene for Students*, of which the fourth edition was published in 1901: the *Health Officers' Pocket Book*, second edition, 1902; and contributed various articles on cognate subjects, such as disinfection, milk, variola and the varioloid diseases of animals, criminal responsibility of the insane, statistical fallacies, etc., to various encyclopaedias and medical and other journals. He had also edited Chaucer's *Prologue to the Canterbury Tales*.

FELIX BRANNIGAN O'FLAHERTY, M.B., C.M.EDIN., D.P.H.CAMB.,
LIVERPOOL.

DR. F. B. O'FLAHERTY, whose early death we record with much regret, was born in Constantinople forty-five years ago. His father, who was first an officer in the army and afterwards a clergyman in the Church of England, was a very distinguished linguist, and was interpreter to Lord Raglan during the Crimean war. The family subsequently settled in Liverpool, where the father was appointed curate. The son, who was educated at the Liverpool

[1] *Clinical Journal*, May 16th, 1906.