and many children who, because they were unable to see words and figures on the blackboard, were looked upon as stupid and backward have proved as intelligent as most children of their class when provided with proper glasses. This work of school inspection was started a year ago through the efforts, and under the supervision, of the late Dr. Chyzer, the State Secretary for Public Health.

Correspondence.

THE MATHEMATICS OF MALARIA.

SIR,—With regard to your reference in the current number of the JOURNAL to this subject, may I point out that it was opened in my Report of the Prevention of Malaria in Mauritius in 1908, and further developed in my book on the *Prevention of Malaria*, of which, indeed, it forms the basis? You say that a writer in the Indian publication *Paludism* "questions the biological postulates" furnished by me. I have stated repeatedly that my postulates were, in the absence of exact knowledge, merely conjectural; but the writer in question gives no evidence whatever in support of his statements, which appear to be scarcely worthy of so much attention. The reason why repeated bitings of mosquitos are omitted from my equations is already given on page 164 of my book. I am much obliged to Professor Karl Pearson and Mr. H. Waite, M.A., B.Sc., for having taken so much interest in the subject, but have only just recently found the time to study the latter gentleman's very interesting contribution, which was written independently of my book, though not of my report. He admits that "there is complete agreement between us on the essential and fundamental points," and that my simpler methods "give results which are sufficiently accurate for practical purposes." In fact, my equation for "static malaria" (eq. 7, p. 161 of my book) is the same as the first term of his (eq. 111, p. 430 of his article in *Biometrika*, vol. vii, No. 4). The greater complexity of his formulae is, as he perceives, due to his having given a more continuous variation to the recovery and malaria rates among the human beings, while I adopted a fixed time unit of one month for my difference equation. There seems to me to be, however, some fallacy here, because he continues to accept my month unit for the number of infecting bites (n)—that is, for the malaria rate among the mosquitos. This has the effect which he describes, namely, that it makes both the increase and the decrease too slow. I purposely adopted a fixed time unit for all the variables together, and purposely made it one month in order to allow time (so to speak) for the parasites to mature in the mosquitos, and to develop a sufficient number of sexual forms in the men. But, as Mr. Waite says, our results are at present only tentative and suggestive, and I have reached a much fuller analysis since last year. Perhaps the most important point elicited is that a very small rise in the number of *Anophelines* above the no-malaria limit—a rise which would never be appreciable by the public—would produce a severe epidemic (my book, page 200).

Allow me to endorse very heartily your remarks regarding the utility of mathematics in medicine. Epidemiology is in fact a mathematical subject, and fewer absurd mistakes would be made regarding it (for example, those regarding malaria) if more attention were given to the mathematical study of it.—I am, etc.,

University of Liverpool, April 22nd. RONALD ROSS.

HEART FAILURE.

SIR,—The opinions of Dr. James Mackenzie deservedly command general attention. It is therefore important that they should be carefully weighed; and, while there is much in his recently delivered Oliver-Sharpey lectures that will be received with assent, there remain some statements which cannot be accepted without question or qualification.

For example, one would gather from Dr. Mackenzie's first lecture (April 4th) that functional murmurs and irregularities of heart frequency, especially in the young, should not only not be regarded seriously but looked on as positive evidence of a healthy heart. That contention appears to be largely based on the fact that in medical literature there is no record of any one so affected suffering from heart failure. I agree with Dr. Mackenzie that medical literature betrays signs of weakness from the clinical point of view, but five and twenty years of general practice, and more than fifteen in which attention has been mainly devoted to cardiac affections, have led me to conclusions differing from his. There are degrees of heart failure, and among boys and adolescents such symptoms not infrequently lead to abstinence from the more active pursuits and games enforced not by a doctor's academic opinion, but by positive distress and disability. In less pronounced instances they are the prelude and accompaniment of undue fatigue on exertion, of a general slackness, and of loss of power of mental concentration. In fact, they convey a note of warning, and, in a large proportion of instances, may be traced to a chronic autotoxis of the rheumatic or of the gastro-intestinal order, which is amenable to treatment to the great advantage of the patient.

Another matter to which I desire to call attention is Dr. Mackenzie's exclusive reliance on treatment by rest and his curt dismissal of baths and exercises as therapeutic agents. The daily experiences of those who have given the best years of their life to the study and practice of such methods afford proof of their pre-eminent value. Allow me to cite one example. A lady past middle age had been confined to her bed for several weeks by an enfeebling malady and heart weakness, with steadily increasing pulse-frequency, which had recently culminated in accesses of heart failure occurring during sleep or at the moment of waking, so severe as to suggest impending death. Four gently resisted exercises of the hands and arms, administered in the recumbent position, induced a fall in pulse-frequency from 120 to 92, doubled the volume of the pulse as judged by the finger, the sphygmograph, and the manometer, and brought a sense of precordial relief. Such results, which might be indefinitely multiplied, are not to be accounted for on psychic grounds.

I, for one, cannot admit that the teaching of Stokes, systematized by Oertel, and brought to a science by Schott, to which thousands owe their relief from suffering and return to health, may be lightly cast aside.—I am, etc.,

London, W., April 24th. W. BEZLY THORNE.

APPENDICITIS AND APERIENTS.

SIR,—Whilst I read with considerable interest Mr. Moynihan's address on abdominal emergencies in the JOURNAL of April 1st, I hardly expected to meet, a few days later, with the following case, which illustrates well certain points to which he has drawn attention:

I was called on April 5th to see a boy of 14 who had been ill since April 3rd. On the forenoon of that day he partook of some raw cabbage plants and then of a dinner of pork. Shortly afterwards, walking in a field, he was seized with a sudden acute pain in the abdomen, and vomited once. He was put to bed with a poultice applied to the painful area. On April 4th he was altogether better, but his mother deemed it advisable to give him a large dose of castor oil, following it up in the evening with a seidlitz powder. Next morning his pain was worse, and as the greatly desired catharsis had not yet taken place some cascara was administered. The bowels then moved several times, but no improvement resulted. Summoned at noon, I diagnosed appendicitis, and recommended immediate operation. There was pain all over the lower part of the abdomen and the usual localized tenderness and rigidity. Temperature, 99°; pulse, 80. At 1 p.m. he was suddenly taken with an agonizing pain, and on my return shortly afterwards prepared to operate, I found the tenderness and rigidity becoming generalized, the temperature 100·5°, the pulse 100. I cut down at once, to find a gangrenous appendix adherent to the ileum and caecum, and having a pinhole perforation in its side. Thanks to free drainage and continuous proctoclysis the boy is making a good recovery.

The case is an isolated one, but I think it shows how perforation may be brought about by the use of purgatives. It is surprising to find that there are still practitioners who pin their faith to saline aperients and the like when they ought to be boiling their instruments or summoning a surgeon. The necessity for a correct diagnosis, however, can scarcely be too strongly urged. What is wanted at the present time is not so much a fierce contest between physicians and surgeons over methods of treatment as a clearer conception of those conditions which are diagnosed as "a threatening of appendicitis." Some of these may