the company of transcendental theories of heredity which have nothing to do with the subject, by one who professes to take his stand upon scientific grounds. Dr. Reid's edifice, built upon a solid scientific base, is to be ignored while Dr. Clouston raises flags in exultation over half a dozen loose bricks. But your correspondent is not content to magnify the importance of a few physiological experiments and microscopic pathological facts and to proclaim Dr. Reid's work to be of no importance; he goes on to use the vague stock utterances of those to whom nobody looks for scientific enlightenment. What ground has he for the assertion that "adverse hereditary influences" and "toxic interference with the germ plasm," &c., "make for national decay"? How does he propose to prove that drunkards "hand down a progeny with all their own tendencies accentuated"? If he possesses evidence to prove these statements then, Sirs, once more let us beg that it may be forthcoming. But if, like "M.D., B.S. Lond.," and Mr. A. Marsden, your correspondent shrinks from being hailed the first great discoverer of this century—the discoverer of a transmitted acquired character—I trust that at least he will examine the contentions of Dr. Reid more closely.

Dr. Clouston's second letter, curiously enough, is directly concerned with the doctrines he declares to be of no account, but he has exercised his mind during a week only to collect opinions similar to his own that the acquired effects of disease and alcohol are transmissible. It ought to be unnecessary to remind Dr. Clouston that preconceived opinions do not alter facts and often must vanish before a scientific exposition of facts; he will find confirmation of this truism in the history of Darwinism and of Listerism and of every epoch-making theory, and also, alas, precedents for his own remarkable attitude. Even in recent times the opinions of the medical profession have, despite opposition such as your correspondent now offers, undergone transformation—e.g., regarding the transmissibility of tuberculosis and even regarding the value of alcohol in the treatment of disease. His appeal to authority is of no scientific value—nay, it is of no controversial value—in face of the fact that Dr. Reid's views have lived already for over five years and that not one of his arguments has been proved fallacious by his crowd of assailants. If Dr. Clouston had read "The Present Evolution of Man" he would not have found it necessary to inquire for Dr. Reid's proofs that neither disease nor drink affects the offspring; he will find them in that book founded on Darwinism, on Evolution, which he will remember Huxley pronounced to be no longer a speculation but an historical fact. A study of them will place him in a position to bring forward any conflicting evidence which he may possess or will compel him to go through the painful process of revising his convictions.

The last point to which I must refer is contained in the following sentence: "Another thing the medical profession is specially fitted to do, and that is to judge rightly the safe limits of the pleasures and the social and personal amenities which alcohol in physiological quantities may secure, and to pick out the persons who can safely indulge in such pleasures." This I take to be a grave statement that Kraepelin's experiments or some other researches which are not specified have conferred on the profession the power to measure with precision the personal equation in regard to alcohol. But we are left to wonder how the power is cultivated, whether it is anything more than that possessed by every good judge of character, and how the statement quoted is to be reconciled with the equally emphatic one of those who maintain on the ground of experiment that alcohol is injurious to all men, even when taken in moderation. Why does not Dr. Clouston in the courage of this conviction propose amongst his legal provisions an examination of every individual at the right age by a board of experts in order to determine this personal equation and issue certificates permitting the proper amount of pleasures and amenities? Perhaps Mr. W. S. Gilbert may be induced to supply the omission. It is scarcely necessary to examine in detail the proposals for legal provisions with which Dr. Clouston's first contribution closes. He is no doubt aware, even if he has perused Messrs. Rowntree and Sherwell's admirable work only in a cursory manner, that practically all of his proposals have been tried on a large scale in America, New Zealand, Scandinavia, and elsewhere and have failed dismally; but he advances no reasons why such measures, even when rechristened "scientific," should be successful at home.

Allow me, Sirs, with all deference, to suggest to your correspondent that it is unfair as well as unscientific first to

proclaim as of no account, then to condemn on no evidence, and lastly to misrepresent views with which he is not in accord. Whatever may be the characteristics of "the pursuit of a fatalistic inertia" nothing can be further from the truth than to suppose that the Reid doctrine implies in regard to alcohol that natural selection must be left to take its course, or to suppose that there is any desire to "put a spoke in the wheel of health reform and social amelioration." Dr. Clouston evidently is unacquainted with Dr. Reid's remedy just as he is with his facts. The Reid school takes its stand on science, is zealous for the race as well as for the individual, and believes that they may be benefited together once the true (and at first sight not obvious) scientific facts are recognised more generally.

I am, Sirs, yours faithfully,

H. LAING GORDON.

Honor Oak-road, S.E., April 15th, 1901.

To the Editors of THE LANCET.

SIRS,—After Dr. T. S. Clouston's able defence in THE LANCET of April 13th, p. 1103, of the position taken up by me in my letter of March 23rd, p. 892, I need scarcely trouble you with any further remarks in reply to Dr. Reid's onslaught. After reading Dr. Reid's letter I could only conclude that he knew nothing of the homes of the drunken poor and the lives that their wretched offspring have to live and that the lessons of all past sanitary and social reforms have been lost upon him. We have heard of the "ocean and the broom" before, but will Dr. Reid please tell me if the chances of two young pretty girls growing up into healthy, respectable mothers are the same, when you place one behind a bar and the other in, say, his own house, to look after his children? There seems to be a difference of point of view in the case of Dr. Reid (and other members of the Society for the Study of Inebriety) and that of social reformers. I agree with Dr. Reid that if you surround a drunkard with public-houses and give him his freedom no legislation will make him sober, but I still maintain that society has the power to limit the creation of drunkards. I am, Sirs, yours faithfully,

April 16th.

M.D., B.S. Lond.

CONTINUOUS ADMISSION OF AIR DURING ETHER ANÆSTHESIA.

To the Editors of THE LANCET.

SIRS,—Some months ago I pointed out in THE LANCET the ease with which chloroform could be administered from a Clover's ether inhaler when the bag had been removed. Modifying the same idea to the administration of ether, I find that, after inducing full anæsthesia with nitrous oxide followed by ether, it is possible in many patients to maintain the anæsthesia with the bag removed and the indicator turned nearly or quite on to full. By so doing the patient gets a supply of fresh air mixed with the ether at each inspiration, instead of one or more inspirations of air at intervals when the face-piece is raised, the patient between these intervals breathing in and out of the bag. The supply of air is thus made continuous instead of intermittent. The result is that the asphyxiation symptoms of ether anæsthesia are largely avoided, the breathing being comparatively quiet and the face pink instead of cyanosed, these differences being very marked. The method involves the use of a larger amount of ether than when using the bag, but this is more than compensated for by the improved type of anæsthesia.

I am, Sirs, yours faithfully,

C. HAMILTON WHITEFORD,

Senior Anæsthetist, South Devon and East Cornwall Hospital.

Plymouth, April 12th, 1901.

MAJOR-GENERAL BADEN-POWELL'S DESPATCH *re* MAFEKING.

To the Editors of THE LANCET.

SIRS,—The following appears in Major-General Baden-Powell's despatch: "Victoria Hospital (Base Hospital), 70 beds: Major Anderson, R.A.M.C., principal medical officer: Dr. W. Hayes (*acted as P.M.O. during the first part of the siege*), &c., &c." Further on again: "On outbreak of war," the General writes, "I took over the Town Hospital [Victoria Hospital], but at first the administration was not satisfactory, on account of the want of supervision over

expenses of stores and sanitation. I therefore appointed an issuer and store-keeper and a sanitary inspector, &c., &c." The orders of the day published at Mafeking on Oct. 10th, 1899, contains the following: "*Defences*: W. A. Hayes, Esq., M.R.C.S. Eng., L.R.C.P. Lond., is placed in charge of all medical and ambulance arrangements in connexion with the defences of the town of Mafeking." The orders of the day dated April 30th, 1900, contain the following: "*Appointments*: Owing to the illness of Dr. W. Hayes, Major Anderson, principal medical officer of the Defence Forces [the Protectorate Regiment], will take over charge of the Victoria Hospital with effect from the 27th inst." On April 27th I got severe blood-poisoning and was confined to bed—that is, 20 days before the siege ended. From the despatch quoted it would appear that Major Anderson, R.A.M.C., acted as principal medical officer during the greater part of the siege; as a matter of fact, he only acted as such for 20 days.

I am, Sirs, yours faithfully,
W. A. HAYES.
Mafeking, March 21st, 1901.

AUTHORS' AGREEMENTS.

To the Editors of THE LANCET.

SIRS,—The reviewer of the current "Literary Year-book" in THE LANCET of April 6th, p. 1020, has a right to his opinion that my article on the above subject is "ill-informed"; but you will allow me, I feel sure, to correct him as to a matter of fact. He does me a grave injustice in calling attention to what he considers a damaging omission, where I neglect to mention "the royalty which an author might fairly expect to receive under the royalty system." Need I say that the omission is intentional; that the avowed intention of the article is to consider *forms of agreement* in general—not any particular instance; and that, finally, if I were required to give the "royalty which an author might fairly expect to receive under the royalty system," it would be first necessary for him to submit to me data governing a particular case before I could do so—i.e., "an author's" name, his manuscript, and the name of the publisher considering it. In trade there is no such thing as a fixed, abstract, or "ideal price" for anything irrespective of its market.

May I add that your reviewer has perhaps written in ignorance of the fact that no less than 13 pages of the latter part of this work are devoted to "tables of royalties," from which any person of average intelligence might, without distressing it, have inferred: (1) that an average royalty is 10 per cent., since it comes exactly mid-way between the two extremes—2½ and 20 per cent.; and (2) that, postulating a certain sale for a book at a certain price, an author is at once placed in a position to decide from the results shown on these tables whether a royalty offered him by a publisher affords a fair return for his labour.

I am, Sirs, yours faithfully,
Burleigh-mansions, W.C., April 12th, 1901. CHARLES WEEKES.

* * Our reviewer, to whom we have submitted Mr. Weekes's letter, replies as follows: "I had no doubt that the omission of all indication as to what royalties should be expected by an author was intentional. But by the omission the whole section of the article dealing with royalties is rendered useless to authors who are surely concerned in the contents of a literary year-book. The publication of books in general was not apparently being discussed by Mr. Weekes under the royalty system, but only that of fiction (see page 28), a limitation which I pointed out before criticising the article. It should have been perfectly easy for Mr. Weekes to indicate what sort of novel by what sort of author would carry a royalty of 25 per cent., and what writers must put up with 10 per cent., the royalty that he now tells us is an "average" one, whatever that may mean. I knew that the book contained tables of royalties, though Mr. Weekes makes no reference to them in his article. The tables are arithmetical guides, quite useful, but in no way supplying the deficiency in Mr. Weekes's article—a deficiency which, to my mind, stultifies his editor's prefatory promise of impartiality."—ED. L.

SUCCESSFUL OPERATION IN FULMINATING APPENDICITIS.

To the Editors of THE LANCET.

SIRS,—In the report in THE LANCET of April 13th, p. 1083, of a case of appendicitis related by me at the last

meeting of the Kidderminster Medical Society it should have been stated that the consultation was held and the operation was performed on the Monday when acute diffuse peritonitis had occurred. Had it been a case of "localised peritonitis," as appears in your report the patient's recovery would not have been remarkable, but the mortality after operation in these fulminating cases is so appalling that successful cases are worthy of record.

I am, Sirs, yours faithfully,

J. LIONEL STRETTON,
Senior Surgeon, Kidderminster Infirmary and
Children's Hospital.

Kidderminster, April 14th, 1901.

"GNATS AND MOSQUITOES."

To the Editors of THE LANCET.

SIRS,—My recently published "Handbook" on this subject is out of print, and my publisher has telegraphed to me in India that a reprint, to supply an urgent demand, is necessary. Our knowledge of the life-history and habits of the malaria-carrying species is, however, growing so rapidly that a mere reprint appears to me out of the question—at any rate as regards the first or general part of the work. Will you kindly permit me to state through your columns that a new issue brought as far as may be up to date will be ready in May?

I am, Sirs, yours faithfully,

G. M. GILES,
Lieutenant-Colonel, I.M.S.

"POST-PARTUM HÆMORRHAGE."

To the Editors of THE LANCET.

SIRS,—I have been much interested in the graphic paper by Mr. E. Stanmore Bishop on Post-partum Hæmorrhage in THE LANCET of April 13th, p. 1069. Having had an extensive midwifery practice in the past amongst the "well-to-do" I have had what I presume would be considered a fair average number of these bleeders, and I recall my terrible anxiety as to the result even now with a shudder which he so vividly depicts in his paper. These cases would come on regularly time after time in the same people. The labour would be quite an ordinary one, the placenta and membranes coming away without exertion or hurry, the uterus contracted, abdominal bandage put on, and everything seeming right. In 20 minutes' time the uterus would relax and the most alarming hæmorrhage come on. This has been the history of a large number of these bleeders in my experience. It is not necessary for my object to narrate the different methods employed; but though I never had a death from post-partum hæmorrhage it is a marvel to me that none succumbed. When engaged to attend these ladies in a subsequent confinement I knew what would happen and I went in dread—ergot in full doses, hot-water intra-uterine injection, &c., notwithstanding. At last one day, when everything seemed to fail, in desperation I plugged the vagina well up to the cervix, at the same time holding the uterus firmly down into the pelvis. Then having the patient on her back, I fixed a large firm cushion pad, book or pin-cushion, just above the uterus and bound it so firmly by bandage that it was impossible for the uterus to expand. In most cases except in very fat persons this is very easily accomplished. There was no more hæmorrhage. In six hours' time the plugs were removed and the uterus was thoroughly syringed out with lukewarm water till not a trace of clot was visible. No further hæmorrhage resulted in any of these cases. This practice, I am aware, was very much opposed to the teaching of my day, but the result was so good in my first case that ever after when these bleeders were delivered it became my practice always to plug more deliberately and carefully as soon as the labour was completed and to fix down the uterus as I have described, being very careful not to allow more than six hours to elapse before removing the plug. The clots that were then removed were not large, proving that the uterus could not distend. It always seemed to me that the small soft clot of blood which remained stimulated the contraction of the uterine muscular tissue, and that the early removal of clots and plugs prevented the coming on of any septicæmic conditions. Certainly all hæmorrhage was stopped at once by this procedure. Having found such confidence and comfort from this in my own practice, and believing that with adequate cleanliness and antiseptic precautions it will be found a safe