whole profession, and in the assurance that those who will kindly take a little trouble in this matter will confer a general obligation and at the same time assist materially in the development of clinical knowledge. All communications respecting patients should be sent to Dr. Colcott Fox, 14, Harley-street, or to Dr. Galloway, 21, Queen Anne-street. The meetings will take place in the Examination Hall on the Embankment. The opening meeting (for which tickets will not be required) will be on Tuesday, Aug. 4th, at 12 o'clock.

I am, Sirs, yours truly,

JONATHAN HUTCHINSON,
President of the Congress.

July 27th, 1896.

"A NOTE ON SURGEON-LIEUTENANT-COLONEL LAWRIE'S LECTURE ON MALARIA."

To the Editors of THE LANCET.

SIRS,—In an article by Dr. George Thin under the above heading the following passage occurs:—

"I shall be pleased to show any member of the medical profession in India who visits this country the preparations which I demonstrated at the meeting of the Royal Medical and Chirurgical Society or preparations of a similar kind, and provided he is already possessed of a sufficient knowledge of the blood I have no hesitation in expressing the conviction that I can convert him to believe in the parasitic nature of these bodies."

I accept this as a challenge from Dr. Thin. One of my students, who has helped me throughout, is now qualified, and possesses a sufficient knowledge of the blood, is proceeding to England next month, and will meet Dr. Thin at any time and place he may appoint in order to examine the preparations of malarial blood he may be pleased to show. The only stipulation I desire to make is that an independent representative of THE LANCET be present. I have no hesitation in expressing the conviction that my student will be able to explain satisfactorily the precise nature of all the appearances and structures in malarial blood, stained or unstained, which Dr. Thin brings forward as animal parasites.—I am, Sirs, your obedient servant,

Hyderabad, Deccan, July 8th, 1896.

EDWARD LAWRIE.

"TUBERCULOSIS AND LEPROSY: A PARALLEL AND A PROPHECY."

To the Editors of THE LANCET.

SIRS,—Dr. Arthur Ransome in a footnote to a lecture read before the Bournemouth Medical Society, and published in THE LANCET of July 11th, 1896, appears to make me attribute the great decrease of mortality from tuberculosis which has occurred during the last sixty years "amongst the British races to an increased resistance to the microbe, a power due to the accumulation of inborn variations." May I be allowed to say that in this he is mistaken? In my book I ascribed the superior powers of resistance shown by us, as compared, for instance, to the Polynesians, to the fact that, unlike the latter, we are descended from an ancestry which has long been weeded out by the disease, whereby, by the continual survival of the fittest, increased powers of resistance have been evolved in us. But this evolution can be ascertained only by comparing the British races with others which have been less, or not at all, affected by the disease. It cannot be measured by, and has nothing to do with, any sudden increase or decrease of the mortality amongst us. Sudden increases or decreases of the mortality may result in evolution or retrogression, but do not result from them. In other words all sudden changes of the death-rate are causes, not effects, of evolution or retrogression and are themselves obviously due to changes in the external conditions—e.g., to altered sanitation. I am, Sirs, yours very truly,

Southsea, July 13th, 1896.

G. ARCHDALL REID.

"A CASE OF FÆCAL FISTULA: A NEW MODIFICATION OF ENTERECTOMY."

To the Editors of THE LANCET.

SIRS,—The very interesting case of a new modification of enterectomy in a case of fæcal fistula recorded by Mr. W. H. Brown in THE LANCET of July 18th is one more case illustrating the advantage of resection of the injured parts of the bowel and restoring the continuity of the intestinal canal by the union of the divided gut end to end. The

chief modification in Mr. Brown's case appears to be the stitching of a flap of skin over the fistula, as suggested by Dr. Oswin, to prevent the escape of the contents of the bowel. Now, in all ordinary cases this can be quite as well accomplished by plugging the bowel through the fistula with wool or gauze. I have adopted this method quite recently in a case of fæcal fistula, the history and details of which will, I hope, appear in the columns of THE LANCET shortly. With respect to the method of uniting the divided ends, Mr. Brown does not mention by what method he accomplished this, but I have found for rapidity of application Mayo Robson's bobbins most useful, while in other cases I have performed Widenham Maunsell's operation with great success.

I am, Sirs, yours truly,

FRED. BOWREMAN JESSETT.

Buckingham Palace-mansions, S.W., July 21st, 1896.

"ON THE ORGANIC MEMBRANES AS INSULATORS."

To the Editors of THE LANCET.

SIRS,—We shall for the present, I think, have to be "content to look upon a membrane of the body, the pericardium, the periosteum, the capsule of the kidney, and any structure of the kind as a covering of the organ," &c., rather than to regard them as "electrical insulators" as set forth in THE LANCET of July 11th by Sir B. W. Richardson. The membranes in question when moderately dry are certainly of high resistance considered as conductors, but not sufficiently high to be regarded as insulators. But when an organ—say, the kidney—is *in situ*, its capsule, permeated as it is by its natural fluids, possesses considerable conductivity and nothing approaching insulation is possible, even if it were not, as it is, in conduction connexion with all the rest of the body through the blood. The resistance of these membranes is, of course, enormously increased by drying them, but with this we have nothing to do, since it is their insulating power under normal conditions in the living body that is under discussion. Sir B. W. Richardson mentions the difficulty there was in detaching the pericardial membrane for his experiments, but I cannot see why he should have taken that trouble considering the purpose he had in view; but whether detached or otherwise there is no difficulty in demonstrating their conductivity. If Sir B. W. Richardson will repeat his experiment and introduce a fresh kidney into the circuit by passing one of the ends of the divided conductor into the kidney, the other end, with the platinum disc as an electrode, being placed on the capsule of the kidney, a very sensible deflection of the needle of the galvanometer will be observed, even though the face of the disc be covered with a detached portion of the membrane as a precaution against any possible defect in the continuity of the capsule. Of course the galvanometer must be sufficiently sensitive for the purpose. The lining membrane of the eggshell may be examined in the same manner and will give similar results, though it is of higher resistance than the capsule of the kidney.

The unit of resistance for insulators adopted by electricians is the megohm (or million ohms) and their resistance often amounts to many millions of megohms per cubic centimetre, whilst the resistance of the above membranes under the conditions named I found roughly to vary from a few hundreds to a few thousands of ohms only per square centimetre. To compare the resistance of these structures to that of gutta-percha is to trifle with the meaning of words, seeing that the resistance of the latter is about 450,000,000 megohms per cubic centimetre. I need hardly add that Sir B. W. Richardson's view that the capsule of the kidney is "a perfect insulator" and "that the kidney as it swings in its place is completely insulated" is, in my opinion, entirely untenable. I am, Sirs, yours faithfully,

July 20th, 1896.

FREDERICK GEORGE, M.D.

THE EXAMINATION OF WATER SUSPECTED TO BE CONTAMINATED BY CHOLERA VIRUS.

To the Editors of THE LANCET.

SIRS,—Will you please publish the following and give your opinion on the same:—

A. contends that in every case of cholera a sample of drinking water should be sent to the nearest medical man for

chemical analysis (not bacteriological) "as it affords information as to the quantity of organic matter, the presence of which in excessive quantity must tend to the proliferation of cholera germs. It also would indicate a likelihood of contamination by excreta" B. contends that sending a sample of suspected water under such circumstances is not only useless, but positively injurious. It is useless because no chemical examination can lead to any conclusion about cholera (apart from the question of potability of the water under ordinary conditions), and bacteriological examination, however useful for scientific research when systematically performed, will not be of any practical utility. The most practical course, B. suggests, is to ascertain the source of water and by careful inquiry to ascertain if all or many using the same have been attacked with cholera. If found so, then we may safely conclude that the water is unsafe and arrange to disinfect it and prohibit its use for some time. Sending the water is positively injurious, inasmuch as it will tend (through very trivial oversight or mishap, such as breaking of the bottles, &c.) to convey cholera from one locality to another. Your opinion on the above will oblige.

I am, Sirs, yours truly,

Ghadechi, June 15th, 1896. N. F. BUNSHAW, L.M. & S.

* * No doubt the presence of the comma bacillus—is assumed to be the essential cause of cholera: ergo, a bacteriological examination is the true and proper one. But it is equally true that a chemical analysis will indicate the contamination of water with products which should not be there and being there may give rise to disease and afford a nidus for the proliferation of any micro-organism or germ. The bacteriological examination of water takes time, and cholera also does not stop pending an inquiry into its nature. It is again exceedingly difficult amid all the micro-organisms morphologically allied, or alleged to be allied, to that of the comma bacillus, to tell whether that found be of the true kind. No doubt the safest and simplest method of procedure is to ascertain the source of the water used and whether its use for potable purposes has been followed by any attacks of cholera, diarrhoea, or sickness, and to act accordingly—namely, to suspend or altogether prohibit its use, or to boil it before being used, if it be the only source of supply. This will not interfere with both a bacteriological and chemical examination being undertaken; the one is supplementary and complementary of the other, we should say. The probability of the conveyance of cholera from one locality to another by some accident to a sample of water seems to us too remote to require to be taken into calculation. If the essential cause of cholera be Koch's bacillus, it can scarcely light up an outbreak or epidemic in the absence of all the other necessary conditions.—ED. L.

THE BRADFORD SCHOOL BOARD AND MEDICAL CERTIFICATES.

To the Editors of THE LANCET.

SIRs,—We beg to thank you for the kindly annotation in THE LANCET of July 25th, in which you congratulate us on the victory we have won over the board on behalf of the medical profession. At the same time we wish to point out that our victory has been complete and very different indeed from the imperfect result which someone reported to you and which we would never have accepted as satisfactory or final. The School Board has agreed to accept *all* medical certificates as sufficient without question. It is a fact that the School Attendance Committee proposed that the medical officer of the board should be authorised to demand a consultation with the medical attendant of a child stated to be absent from school through illness, the Board to pay for the consultation. This indignity we would never have tolerated, and we are glad to say that the majority of the board refused to adopt such a proposal from the committee. One member, the Rev. Mr. Addison, designated it an "impertinence." Such an effect had our attitude on the Board that they did not even adopt the usual procedure of referring the objectionable minutes back to the School Management Com-

mittee for reconsideration, but promptly altered it by an amendment which compels the committee to accept *all* medical certificates and entirely withdrawing all suggestion of the medical officer investigating the honesty or the skill of the medical attendant as proposed by the committee. This highly satisfactory result has been gained by the complete unanimity and determination of the members of the society to maintain the dignity and independence of our honoured and honourable profession.

We are, Sirs, yours faithfully,

THOS. WHITESIDE HIME,
President, Bradford Medico-Ethical Society.

JOHN DUNLOP,
HERBERT E. HICK, } Joint Hon. Secs.

July 28th, 1896.

"THE RELATIVE EFFICIENCY AND COST OF PLUMBERS' WORK."

To the Editors of THE LANCET.

SIRs,—At a meeting held this day of this the London and District Branch of the National Association of Master Plumbers of Great Britain and Ireland it was moved and carried unanimously "that the executive hereby records its hearty and best thanks to the Editors and Proprietors of THE LANCET for the interest they have shown, for the benefit of all classes, that plumbers' work should be well and thoroughly done, as evidenced in the appointment of 'THE LANCET Commission to Inquire into the Relative Efficiency and Cost of Plumbers' Work' and the report of the Commission published in your journal, No. 3801, July 4th, 1896."—I am, Sirs, yours respectfully,

DANIEL THOMAS BOSTEL, SEN.,

Honorary Secretary; also Vice-President of the Association, London, S.W., July 22nd, 1896.

P.S.—I enclose you report of our annual meeting held at Nottingham last month.

"THE CAUSE OF MALARIA."

To the Editors of THE LANCET.

SIRs,—Having recently come from Netley, where—under Professor Wright's personal supervision—I saw various phases of the Laveran body, I shall be glad if you will allow me to describe, without regard to any theory, what I saw through the kindness of Surgeon-Lieutenant-Colonel Lawrie at the Afzulgunj Hospital on the morning of June 26th. Under the microscope was a fresh, unstained preparation of blood taken from a patient with a temperature of 104.2° F. suffering from irregular quotidian ague. In the centre of the field there was a most perfect rosette form of the Laveran body, consisting of a central mass of black pigment surrounded by eight non-nucleated spores, the so-called daughter cells.

Laveran body in the blood.

Whilst watching I observed the plasmodium become more oblong in shape and gradually two of the end spores became separated from the main body, the remaining spores closing round the central pigmented mass. In the course of an hour the plasmodium assumed various shapes and one more spore became detached. Eventually, however, the separated spores or daughter cells became re-united to the main body, the rosette shape was lost, and the individual spores lost their definite outline and became merged in the parent cell. The resulting body was apparently a white blood cell containing pigment, which was now scattered about both centrally