Bombay. The matter has since been most carefully investigated and reported upon, without discovering, however, a definite cause for the outbreak or the exact channel of infection. But many very serious defects in the sanitary condition of the hospital have been brought to light, and we shall take another opportunity of adverting to the subject.

The *Victoria* sailed from Bombay on Nov. 9th with fourteen patients for Netley. She is due at Portsmouth about the 29th inst.

The next examination for commissions in the Army Medical Service will commence in London on Feb. 15th next.

Surgeons who are eligible for the rank of Staff-Surgeon will be examined at Haslar Hospital on Jan. 6th next.

Correspondence.

"Audi alteram partem."

"THE GENERAL MEDICAL COUNCIL AND THE BRITISH PHARMACOPŒIA."

To the Editors of THE LANCET.

SIRS,—May I be permitted respectfully to demur to your suggestion (in the leading article on the above subject in THE LANCET of Dec. 15th) that, in relation to the Pharmacopœia, I had "overlooked" a clause (of the Medical Act of 1858) which deprived my argument of force. You quote the clause in question, in which the Pharmacopœia is described as "a book containing a list of medicines and compounds, and the manner of preparing them, together with the true weights and measures by which they are to be prepared and mixed, and containing such other matter and things relating thereto as the General Council shall think fit." I read the whole of this clause to the Council, and am still of opinion that it sustains my argument and that it affords no justification for converting the Pharmacopœia into a sort of Whitaker's Almanac. In English composition, as I understand it, the relative refers to the last antecedent; and the meaning of "thereto" is, unquestionably, "to that," and not "to these." In the clause quoted, therefore, "relating thereto" is an abbreviated form of "relating to the true weights and measures by which medicines and compounds are to be prepared and mixed," and it is nothing else. Even if we suppose that the draughtsman of the Act did not know how to express his meaning correctly, and if we improperly so extend the scope of the relative as to make it include the aggregate of the antecedents, we do not carry the matter much further; and the suggestion that the Pharmacopœia should state the supposed curative properties of medicines, or the times of high water at London Bridge, or the birthdays of European Sovereigns, could only be seriously entertained by those who either hold, with the late Dr. Tupper, that a complete treatise on any subject would embrace all others, or else that the General Medical Council, like the Emperor Sigismund, should be "supra grammaticam." I think it should be borne in mind, when considering the whole question, that the Pharmacopœia has one object, and one only—that of securing uniformity of strength and composition in all preparations which are sold under certain names. Hence, an important matter "relating to" its province is a list of synonyms, such that, for example, a druggist may not with impunity omit opium from compound tincture of camphor in order to sell it as "paregoric elixir," or alter the composition of compound powder of ipecacuanha in order to sell it as "Dover's powder." An attempt to expand the Pharmacopœia into a treatise on pharmacology, or into a guide to the uses of medicines, would, in my humble opinion, be an unwise interference with private authors, and would commit the Council to current hypotheses which time might not confirm; while additions to, or omissions from, the number of included preparations should be solely governed by the consideration whether those in question were sufficiently used by practitioners to render uniformity in preparing them a matter of importance.

May I also make a very small correction in your generally excellent report of the proceedings of the Council? I did not describe myself as the "helpless" (which I hope, indeed, would not be an entirely accurate description) but as the "innocent and hapless" cause of a certain debate in which I felt called upon to take a part.

I am, Sirs, your obedient servant,
R. BRUDENELL CARTER.

Harley-street, W., Dec. 17th, 1894.

VACCINATION AND SMALL-POX.

To the Editors of THE LANCET.

SIRS,—Do you not think it would be desirable to insert in the weekly returns of small-pox furnished by the various boards a statement regarding the vaccination of the fatal cases—i.e., how many have been vaccinated, and of these, when and with what success it was carried out; and how many cases had not been vaccinated? It appears to me that a statement of the age of the patients who have died should also be inserted.

We require evidence of this sort to be constantly before the public mind, as well to strengthen the faith of the adherents of the practice as to convert those who are faithless. The cases reported weekly do not seem very numerous and I am sure THE LANCET, through its widespread agencies, could easily collect the desired information. It would probably be enough for the educational purpose now in view to publish in the above form the returns from the six largest towns in the three kingdoms, or say, three in England and two each from Scotland and Ireland.

I am, Sirs, yours faithfully,

JOHN SMYTH, M.D., Surgeon-Major.

St. James's-square, S.W., Dec. 4th, 1894.

** The desirability of giving such information is unquestionable, and as regards London it could be given weekly. In regard to the large towns showing from time to time any notable prevalence of small-pox the data, so far as obtainable, might perhaps be noted quarterly. It would not seem practicable to give the facts at shorter intervals.—ED. L.

EPIDEMIOLOGY AS A "DISCIPLINE."

To the Editors of THE LANCET.

SIRS,—Mr. Shirley Murphy, in his presidential address to the Epidemiological Society,¹ was good enough to mention me as having made an excellent translation of a German word used by Professor Hirsch in the preface to his standard work on the Comparative Study of Diseases in all Climates. It may seem ungracious, but I am unable to accept Mr. Shirley Murphy's compliment, notwithstanding its obvious sincerity and high intrinsic value. The word in the preface was so far from being excellently rendered by me that it has actually misled Mr. Shirley Murphy himself as to the meaning of the original German. Professor Hirsch is claiming (and justly) to have made his subject for the first time "eine Doctrin," or to have "made good its right to a place among the medical sciences." Perhaps the best illustration of this is that the author was called from being a general practitioner at Dantzic to represent his subject in the Berlin Faculty of Medicine when he had published only one half of his work. The subject was "eine Doctrin" because it was a body of knowledge so well discussed, digested, and agreed upon that it might be taught and ought to be taught from the chair. It seemed to me that the English word "doctrine" might convey some other meaning than this (as in "the doctrine of the Nicolaitanes, which thing I hate"), and I took instead the word discipline, which was, indeed, somewhat old fashioned in its scholastic sense (*disciplina*) and a subject matter that might be and ought to be learned by pupils. Mr. Shirley Murphy thinks "discipline" excellent for the reason (if I understand him aright with the help of your leading article) that there is some suggestion in it of a rod for the fool's back—for the incorrect reasoner, the hasty generaliser, the visionary, the enthusiast, the sciolist, and the like. I quite agree with Mr. Shirley Murphy that all such persons should be sternly discouraged, but I do not think that "discipline" in the context of Professor Hirsch's preface meant more than a body of knowledge fitted for didactic use, or in a fit state to be imparted to those who were still in their pupillage and almost bound to receive implicitly what was delivered to them from the chair. It was in this sense that Cullen spoke of delivering the doctrine; for example, "I have no proper doctrine to deliver under the general head of alteratives." A meaning is sometimes made clearer by its opposite. Two instances occur to me of departments of study, each of them attended by an enormous

¹ THE LANCET, Dec. 15th, 1894.

amount of writing, which have not yet attained the indisputable rank that Professor Hirsch claimed for his subject. The first is so-called Darwinism, which Professor Virchow some years ago protested, in the name of scientific freedom, was not an educational subject at all, or one that lent itself to the didactic and somewhat aggressive manner of Professor Haeckel and other exponents of it at that time. The second instance was the other day, when the General Medical Council carried "the previous question" against a motion, "That in every course of professional study and examination bacteriology be contained."

I am, Sirs, yours truly,

Dec. 17th, 1894.

C. CREIGHTON.

"THE INFLUENCE OF HEREDITY IN PHTHISIS."

To the Editors of THE LANCET.

SIRS,—The extremely short time at my disposal at the end of the discussion on Dr. E. Squire's paper on the above subject at the Royal Medical and Chirurgical Society¹ on Tuesday last prevented me from touching on several points of importance upon which I shall be greatly obliged if you will allow me to supplement my remarks. One of the chief objects of the paper was to show that "the influence of heredity cannot be put higher than 9 per cent. of cases amongst the children of phthisical parents in excess of the cases occurring amongst the children of non-phthisical parents." The difference was attributed to the greater liability to infection in the home to which the children of consumptives are exposed. I desire to point out what I believe to be a fallacy in the method followed by Dr. Squire in arriving at the apparent influence of heredity. The method is as follows. In 474 cases of phthisis the total number of children in the family to which the patient belonged was ascertained. In 275 of these families the parents were non-phthisical; in 199 one or both parents were phthisical. In the former class 24·87 per cent. of the children attaining to adult age were consumptive, as against 33·71 per cent. of the latter. The difference, 9 per cent., is supposed to show the influence of heredity. The information was obtained from the verbal statements of the patients.

Now it will be observed that Dr. Squire starts with a consumptive member of a family and works back to the parents, thus practically assuming that there is a consumptive in every family. No account is taken of the fact that in every 474 families, not selected, there will be many in which no member is consumptive. This is the fallacy which, I submit, completely vitiates the conclusion that "the influence of heredity cannot be put higher than 9 per cent."

In Dr. Reginald Thompson's work on Family Phthisis there is a table precisely similar to Dr. Squire's, showing the incidence of phthisis upon the children of consumptive parents, in which, although the percentages differ, the gradation of hereditary influence is the same:—

	Dr. Squire. Per cent.	Dr. Reginald Thompson. Per cent.
Father phthisical	31·8	34·0
Mother phthisical	34·4	39·3
Both parents phthisical ...	39·5	56·4

This is an important point, and strongly supports the view that there is "a direct hereditary predisposition" to the disease transmitted, and not merely a "general inherited delicacy." The general tone of the discussion clearly showed a tendency to ignore the importance of the hereditary factor and to insist upon infection as the only point worthy of consideration. I am fully impressed with the necessity of adopting all measures tending to diminish the chances of infection; but I hold that it will be a grave error if the profession should be led by the fascination of this element of the problem to cast aside the experience derived from centuries of observation. The fact that the President was obliged to ask Dr. Squire to define the term "phthisis" shows that the disadvantages which attended its use still remain. I have been compelled to apply it here, but I am glad to say that I have not seen a case for years. There is no greater difficulty in deciding whether or not a patient is suffering from "pulmonary tuberculosis" than attends the diagnosis of many other diseases, and I can promise those

who use the latter term that their non-tuberculous cases will at once assume an increased interest.

I am, Sirs, yours faithfully.

Dec. 18th, 1894.

J. K. FOWLER.

"THE ETIOLOGY OF TYPHOID FEVER."

To the Editors of THE LANCET.

SIRS,—In reply to Dr. Kenwood's first question,¹ it being a bacteriological one I do not feel competent to discuss it. I mentioned in my former letter that I entered this discussion solely from the epidemiological point of view, and with all deference to Dr. Kenwood I do not intend departing from that standpoint. The bone of contention between us is that Dr. Kenwood believes typhoid fever, considering it epidemiologically, can arise *de novo*, because in a large number of cases of typhoid fever we are unable to trace the source of the disease to a previous case. I, on the contrary, contend that the disease under these conditions does not arise *de novo*. I prefer to believe that when we are unable to trace the disease to a previous case it is due either to our having failed in tracing every known means by which infection is spread, or, more probably, that we do not yet possess complete knowledge of the various media by which the disease is communicated from one person to another. With reference to Dr. Kenwood's question as to whether I contend "that, in the case of small-pox and the other infectious diseases mentioned, it is equally easy [as it is in typhoid fever] to exclude all the possibilities of such infection," I reply in some, Yes; in others, No.—I am, Sirs, your faithfully,

Piazza di Spagna, Rome, Dec. 1st, 1894.

JOHN J. EYRE.

SYME'S TREATMENT OF URETHRAL STRICTURE.

To the Editors of THE LANCET.

SIRS,—In common, I am sure, with all your readers, I have been greatly interested in Mr. Pemberton's Bradshaw lecture upon James Syme, fully reported in the last number of THE LANCET. James Syme has been called the "Napoleon of surgery," but I would rather liken him to Wellington, for I yield to no one in my admiration for all the manly and surgical qualities that Syme possessed. But the best surgeon as well as the best general is only he who makes the fewest mistakes, and there is one point in Mr. Pemberton's lecture which I think calls for remark. Mr. Pemberton gives unqualified praise to Syme's treatment of urethral stricture by external division, and asserts "that this treatment maintains at this day the value claimed for it by Syme, bringing about a permanent relief from an intolerable and complicated malady." On Nov. 13th, 1852, Professor Syme addressed a letter to the Imperial Academy of Medicine in Paris on this subject, and stated as the principles of his proposal: "(1) That there is no stricture truly impermeable, and that, with time and care, an instrument may be passed through it and serve as a guide for the knife; (2) that all strictures which cannot be remedied by simple dilatation admit of effectual relief only through a free division of the contracted part of the canal; (3) that the object can be attained with certainty and safety only by an external incision, in a line corresponding with the raphe of the perineum, upon a grooved director passed through the stricture; (4) that the only after-treatment required is the introduction of a catheter during forty-eight hours, with the subsequent use of a full-sized bougie at distant intervals; (5) that the operation, if properly performed, is free from any risk whatever of hæmorrhage, extravasation of urine, or fistulous opening." These principles have now been before the profession for forty years and may well be reconsidered. I for one, while gratefully acknowledging what I believe to be the absolute truth of the first two propositions, strenuously controvert the third, fourth, and fifth, and do so from a sense of surgical duty.

Beginning with the first principle enunciated by Syme, I am satisfied that we may add the following two riders to it: (a) that when an instrument has been fairly passed into the bladder through a strictured urethra, the instrument may be withdrawn and replaced by one a size larger, if the surgeon trusts himself; (b) that all strictures may in this way be dilated up to No. 6 or 7 (English), and when so dilated a

¹ THE LANCET, Dec. 15th, 1894.

¹ THE LANCET, Nov. 24th, 1894.