theria, received preventive injections. Of these, 13 children developed diphtheria; in 7 cases diphtheria appeared in less than twenty-four hours after the injection; in 6 cases more than twenty-eight days after; no case occurred in the intermediary period. M. Netter has by counter-proof shown the value of his investigations, for at the same time as the above families other parents sent their children affected with diphtheria to the hospital without their brothers or sisters having received any preventive inoculation. In this second category, among 491 children belonging to 200 families, there were 87 secondary cases of diphtheria in 69 families; the deaths were 18 in number, and there were 20 grave though not fatal cases, whereas in the inoculated series the cases had always been more or less mild. In the above two groups outside of the immunization all the conditions were the same; they came from the same social class, inhabited the same districts, etc.

Overcrowding in the Paris hospitals has reached such a pitch that no room is left for the admission of urgent cases. In order to remedy this, the Director of the Assistance Publique has decided that "exeats" will be given to patients as soon as their condition will allow of their being taken back to their homes, where they will receive medical attendance from the physician to the various *bureaux de bienfaisance*, and during one month an allowance of 1 franc a day. On the advice of the attending physician this allowance may be continued for two additional months. The above measures will only be carried out in the case of patients whose homes will permit of such treatment.

All the societies in France for the prevention of tuberculosis were recently formally federated at an inaugural meeting in the Hall of the Society of Civil Engineers, the President of the Republic being present. Professor Brouardel is the President of the new federation. Its object is to favour the spread of knowledge about tuberculosis and its prevention. Representatives from seventy-six societies and thirty-seven cities were present, and their power will now be increased by combined action on a systematic plan. It is proposed to organize a system of relief for patients to which the Friendly Societies will be asked to contribute; the railway companies will be asked to subsidize sanatoria in the districts served by their systems for the treatment of their servants, many of whom suffer from tubercle; and in the same way it is hoped that sanatoria will be provided for soldiers in their territorial districts.

At its last meeting the Municipal Council placed before a special Commission a proposal to fit up on one of the properties belonging to the Assistance Publique a sanatorium of fifty beds for the exclusive use of nurses who may have contracted tubercle in their service.

CORRESPONDENCE.

THE EVIDENCE FOR HETEROGENESIS.

SIR,—With many of the remarks of your reviewer in the BRITISH MEDICAL JOURNAL of April 12th, p. 904, concerning Part I of my *Studies in Heterogenesis*, I thoroughly agree. I am seeking to establish views "at variance with those generally accepted," and it is my wish that "the evidence tendered in their support should be narrowly scrutinized."

When he says "the more improbable the view asserted the more complete should be the chain of evidence in its support" I also agree; but then I recognize perhaps more fully than he does that the evidence brought forward ought not to be lightly tossed aside after mere superficial consideration.

If it had not been for the great strength and prevalence of the preconceptions opposed to my views, and the consequent difficulty in getting a serious consideration for them, I should not trouble you with these few lines.

The reviewer cites only two of the cases which I bring forward as instances of heterogenesis. He dismisses them for wholly inadequate reasons, and implies that a similar fate might be allotted to "each of the observations and the conclusions which are drawn from them." It may be useful therefore to see what value is to be set upon his criticism.

He takes first the alleged transformation of the contents of certain large confervoid cells into amoebae, and suggests that the changes seen and described are only instances of reproductive processes of a kind very common among cryptogamic plants—the existence of which are, of course, perfectly familiar to me. He does not state, however, that I watched the changes in question off and on for nearly a month in successive cells, and that I had the authority of Dr. Cook, author of the great work on the *Fresh Water Algae*, for saying that the specimens in question were vegetative and not reproductive cells of a conferva (p. 4).

The next case to which the reviewer refers is that of the alleged transformation of the substance of euglenae into flagellate monads (p. 13), and he strongly suggests that I have here confused the flagellate monads produced within the euglenae with the wholly different and typical zoospores of the fungus which so commonly attacks them—Polyphagus euglenae. But the zoospores of this fungus, as I may tell him, are not produced within the euglenae, but in characteristic outgrowths therefrom, many thousands of which I have seen and examined.

If the possible criticisms of other cases to which your reviewer alludes are similarly superficial, they might mislead some of your readers as to the value of my evidence, but they would not be likely to convince me as to the error of my views. I have devoted much of my leisure time during the last four years to the investigation of these questions, and have accumulated much evidence, the first instalment of which is soon to be followed by a second. I trust that the illustrations, from photographs, of the successive changes in several of the transformations cited will by many persons, if not by your reviewer, be regarded as strong evidence tending to show the reality of the occurrence of heterogenetic processes—the establishment of which is beset by many difficulties, both intrinsic and extrinsic.

By the proof of the occurrence of such processes, and in this way only, as I contend, can the fact of the existence of all the forms of infusorial organisms at the present day be logically harmonized with the general doctrine of evolution. Their presence otherwise seems to be a stultification of this general doctrine which the world of science has almost unanimously accepted.—I am, etc.,

H. CHARLTON BASTIAN.

Manchester Square, W., April 14th.

*** We have referred this letter to the writer of the review, who has sent the following note:

I am sorry that Dr. Bastian should think that his researches have been "tossed aside after mere superficial consideration," but I must remind him that in a matter of this kind the burden of proof lies wholly with him, and even if his facts and inferences be correct, it is still necessary that they should be supported by such evidence as shall carry conviction before he can hope that they will gain credence. Now, after carefully reading and re-reading his monograph, with as open a mind as I can command, such conviction is not borne in upon me.

I do not pretend to more than a general acquaintance with these low forms of life, far less to an expert acquaintance with them, but approaching the matter from the standpoint of a biologist, it does not appear to me that the sentence in the preface that "examples, however, have been selected for this memoir of such a kind as obviously to eliminate the first of these interpretations" (namely, that the forms described may be normal stages in the life-history of the same organism) is justified so fully as Dr. Bastian assumes. Nor do I agree with his application of another sentence in the preface, to the effect that "the substantiation of such modes of origin of the organisms in question—which is my main object—far transcends in importance the following out of their subsequent fate, so long as we can be reasonably certain that the first of the three possible interpretations above referred to has been legitimately excluded." I fail to see that its exclusion is legitimate until the life-history of everything under observation has been pursued to the fullest extent, which has not, on Dr. Bastian's own showing, been attempted. Surely, conceding for the purpose of argument that the observations are not open to sources of error, it is just as possible that Dr. Bastian has discovered hitherto undescribed transformations in the life-history of individual forms. If heterogenesis, which according to Dr. Bastian is of rather general occurrence and not difficult to light upon, has been entirely overlooked, is it not at least as likely that the life-history of these organisms is similarly imperfectly known? My respect for Dr. Bastian's attainments makes me very reluctant to criticize his methods adversely, but personally I may say that one single instance worked out with completeness would have gone further towards convincing me than a much larger number of observations which lack that completeness; and if I may venture to make a suggestion, it would be that he should select some one of the cases which he regards as typical, and devote himself to following that out to the utmost.

It is a minor matter, perhaps hardly worth alluding to, but in quoting in his letter the authority of Dr. Cook that the conferva cells under observation were vegetative and not reproductive, the wording used is rather stronger than the statement in his memoir, which is thus qualified: "he was only able to inform me that it was probably 'some vegetative form of one of the Confervaceae.'"

I may add that the facts cited in the review were not so much quoted as being possible actual errors into which the investigator had fallen, but as representative of a group of facts which call for great care in observation and caution in interpretation.

HEALTH IN SCHOOLS.

SIR,—At the last meeting of the British Association a Special Committee was appointed to consider the conditions of health essential to carrying on the work of instruction in schools.

The Committee consists of Professor C. L. Sherrington (Chairman), Mr. E. W. Braybrook, C.B., Dr. C. Childs, Dr. Clement Dukes, Miss Findlay, Dr. C. W. Kimmins, Miss A. Ravenhill, Mr. J. Russell, B.A., Professor L. C. Miall, Dr. Sydney Stephenson, Dr. C. Shelley, Professor H. L. Withers, Dr. W. H. R. Rivers, and Mr. W. White Wallis (Secretary).

In the first instance the Committee is collecting information upon the following subjects:

A collection and tabulation of records of original observations on the periods of day appropriate for different studies, the length of lessons, and the period of study suitable for children of different ages.

A collection and tabulation of anthropometrical and physiological observation forms in use in various schools, with a view to preparing a typical form for general use.

A collection and tabulation of anthropometrical and physiological observations recorded in different schools for a series of years on the same children.

A collection and tabulation of recorded investigations into the causes of defective eyesight in school children, and a definition of the conditions necessary for preserving the sight.

An inquiry into the practical knowledge of hygiene possessed by school teachers.

As the subject is one of general interest and importance, the Committee hopes that you may be able to find space in your columns to bring these investigations under the notice of your readers, and asks their co-operation in obtaining information on the several points; any facts or references relating to the subject under consideration, if sent to the Chairman or myself, will be very helpful.—I am, etc.,

E. WHITE WALLIS,
Secretary.

The Sanitary Institute, 72, Margaret Street, W., April 16th.

THE COURT AND THE ARMY MEDICAL SERVICE.

SIR,—With reference to the letter of "One of Them" in the BRITISH MEDICAL JOURNAL of March 15th, permit me to state that a similar occurrence took place at the King's visit to Portsmouth, and that the Surgeon-General there was omitted from the invitation, though several officers junior to him were asked. Of course it must be clearly understood that His Majesty, like all his subjects, has a clear and definite right to invite whom he chooses to the royal table, but on the other hand, custom lays down that senior officers on the staff are always invited, and the omission to do so is looked on in military circles as a grave slight.

In the present instances it happens that both the officers are Irishmen, but have most distinguished records of service, one being a Companion of the Bath, while the other was twice recommended for it, and was over and over again mentioned in dispatches.

These incidents have caused the very deepest feeling of grief and despondency in the medical service, and it is openly said that Mr. Brodrick's warrant may as well remain in its pigeon-hole as be promulgated, for no "self-respecting gentleman" will (as "One of Them" truly says) enter the service to be snubbed and insulted by Court officials and military clubs that take their cues from them.—I am, etc.,

April 15th. ANOTHER OF THEM.

HORSE SICKNESS IN SOUTH AFRICA.

SIR,—In the BRITISH MEDICAL JOURNAL of February 22nd, in an address by Mr. C. B. Ball on the reorganization of the Army Medical Service, and in the last paragraph of that address appears: "Lieutenant-Colonel Bruce, F.R.S., of the Royal Army Medical Corps, the discoverer of the cause of Malta fever and of horse sickness in South Africa."

Seeing that as a result of this statement it may be widely thought that the honour of priority of discovery of the cause of this devastating disease rests with Lieutenant-Colonel Bruce, I would ask you, in fairness to those engaged in the endeavour to ascertain the cause of horse sickness, to give prominence in your journal to a statement of the fact that the cause of horse sickness is as yet undiscovered, either by Lieutenant-Colonel Bruce or by any other worker.

What Lieutenant-Colonel Bruce did discover is the cause of tsetse fly disease, a disease prevalent in certain parts of Africa, near the coast, and so universal and so fatal that horses cannot live in the "fly belt."

Horse sickness is the common term in use for a definite and apparently specific disease of the horse in South Africa, is totally distinct from the tsetse fly disease, and much more widely distributed. Of this Mr. Ball may be unaware.—I am, etc.,

ERNEST HILL, M.R.C.S., L.R.C.P., D.P.H.,
March 18th. Health Officer for Colony of Natal.

THE TREATMENT OF DEAFNESS OF MIDDLE-EAR ORIGIN.

SIR,—The letter of my friend, Dr. McBride, in the BRITISH MEDICAL JOURNAL of April 12th, has suggested to me the possibility that a number of your readers may have misapprehended the scope and purpose of my recent communication on the treatment of middle-ear deafness (March 22nd, 1902). I should, therefore, like to emphasize one or two points which I thought had been already indicated with sufficient clearness.

The object of my paper was to bring to the knowledge of practitioners a method of treatment pursued for nine months with a decided measure of success. Having regard to the prevalence of this type of deafness, the grave consequences of its onset, and the confessed inability of practitioners to cure it or retard its development, I felt that publication had come to be a duty on my part. But although I had treated a fairly long series of cases, and had secured a fairly uniform result, the time was not ripe for an exhaustive communication dealing with the etiology of the disease and the *rationale* of my treatment. From this it follows that it would have been premature for me to make definite statements regarding the conditions best suited to the treatment and the limits of its efficacy. Any communication, therefore, which I could make was necessarily preliminary in character, and it was sufficient if the preliminary note was a statement of facts set forth with due caution and exactness. I cordially endorse Dr. McBride's caveat that special care must be taken to eliminate as far as possible all the elements which might vitiate an induction from the cases subjected to a particular course of treatment. Accordingly, as stated in my paper, a man was selected by Dr. McBride and myself as a subject of observation unlikely to be affected by a neurotic element. The figures provided by his case have therefore a particular value for the estimation of the results which may be expected from the use of myelocene; and the import of these figures is magnified when their evidence is supported by the independent testimony of the man's employers as well as of his wife and friends.

I cannot too strongly emphasize the fact that it would have been out of place in a preliminary note either to offer an estimate of the value of the net results obtained, or to expound the ratio upon which the treatment proceeded. I did, indeed, give a broad outline of the theory which underlay the application of bone marrow to middle-ear deafness, but I am sure that careful readers of my paper must have observed