conclusion without repudiating the whole inductive method. But *why* such diet causes the changes; what constituent in that diet is the efficient cause of the alteration of structure; whether the addition of inorganic material would counteract the effect of the excessive meat; these and the like are questions which belong to a different stage of our investigations.

Dr. Spriggs contends that "whether the results produced in rats are due to the want of lime and other bases or whether they are an expression of the failure of a naturally omnivorous animal to rapidly adapt itself to a purely flesh diet it is clear that they cannot be directly applied to the case of civilised man who never in ordinary circumstances lives upon a diet of meat alone." As stated that is merely a truism. It would never occur to a scientific investigator to apply a general rule competently deduced under a given set of conditions to another set of conditions materially different; and, for my part, I would not suggest that the results of my observations on rats in confinement could be rightly predicated even of rats taking the same diet but enjoying a large amount of exercise.

But it is evident from the terms of your correspondent's latest letter that he means something much more, and more serious than this. He says: "Until the causation of the results is more exactly known their application to modern human dietetics is unjustifiable and discussion useless." Before discussing this statement I must remark that I do not consider it quite ingenuous for Dr. Spriggs to take up this position and at the same time ignore the question I have already quoted regarding the histological appearance of the bones of the infant. To admit that there was a substantial identity in the appearances would have raised too powerful a presumption in favour of the application of my results to the human subject. This is a course which, in the opinion of Dr. Spriggs, is unjustifiable until the causation of these results is more exactly known. It seems to me that this opinion places your correspondent in a serious dilemma. It either commits him to the condemnation of the comparative method in physiology or it places him in the position of a man who says, "I know this is hurtful and that is beneficial, but I don't know why they are so, and until the reasons are determined I will not use my knowledge." The latter position is impossible and we know that Dr. Spriggs does not condemn what I have described as the comparative method. As Oliver Sharpey lecturer he recently delivered two lectures on Metabolism to the Royal College of Physicians of London. The subject of nitrogenous metabolism in man specially includes the subject of urea formation. Has the author of these lectures failed to realise that practically all we know about urea formation in man is knowledge directly applied from physiological experiments on animals which have been made under conditions no more "ordinary" than those of my experiments? In the same lectures the author gives us a further admirable corrective to his present judgment. He quotes with approval the masterly researches of Pawlow and makes a direct application of these results to human dietetics. He appears to be unaware that the actual experiments indirectly referred to by him were carried out under conditions further removed from "natural" than my own. I would suggest to Dr. Spriggs that in matters of science consistency of reasoning is of some importance and that in entering the field of discussion on matters new it is unwise to forget the attitude adopted in matters old.

I am, Sirs, yours faithfully,

Edinburgh, Feb. 10th, 1907.

CHALMERS WATSON.

THE CURRANT AS A FOOD.

To the Editors of THE LANCET.

SIRS,—That Mr. James Cantlie should inveigh so concernedly against the currant in his letter appearing in THE LANCET of Feb. 16th, p. 462, is a matter that elicits surprise at first thought but amazement afterwards when he gives further his reasons for his conclusions. He has gone diametrically opposite to the majority of observers in condemning the currant; now I will make bold to give a reason why the currant should not be so peremptorily stigmatised that is diametrically opposite to the one he has given. It is in bread that the currant will cause the least harm, while in soft puddings, rice, &c., it may certainly be a danger, for bread must be masticated before swallowing, and the currants in it will be broken up of necessity. Soft milky

puddings with unbroken currants in them are often swallowed without chewing; thus the whole currants may cause trouble.

I am, Sirs, yours faithfully,

HAYDN BROWN, L.R.C.P. Edin.

Caterham, Surrey, Feb. 17th, 1907.

THE ETIOLOGY AND PREVENTIVE TREATMENT OF CEREBRO-SPINAL MENINGITIS.

To the Editors of THE LANCET.

SIRS,—May I be permitted to point out that the causation of this disease is not summed up in the statement that it is due to the diplococcus of Weichselbaum or to any other microbe? Granted that it is immediately due to a microbe attacking the meninges, the question arises, How does it get there? The importance of tracing causation in all its steps lies in the fact that it is only by so doing that we can apply preventive treatment.

With the view of providing a clue to this preventive treatment I beg to submit the following hypothesis. One necessary preliminary to the advent of the microbe to the meninges is the existence of some abrasion or ulceration, however slight, of the faucial, tonsillar, or pharyngeal mucous membrane. Such lesions afford at once a nidus for the growth of the infective microbe and an open door for its admission to the various channels by which the meninges may be reached. *Primâ facie*, this hypothesis is rendered highly probable by the coincidental prevalence of sore throats and throat coughs, and it indicates the line that investigation ought to follow in order to prove it. That is to say, the throat ought to be examined whenever possible and the presence or absence of cough, irritation of the throat, or discharge from the nostrils should be noted. Moreover, the history of the case previous to the development of the characteristic symptoms ought to be ascertained by careful questioning of those who are most likely to know it and in this way some antecedent illness during the preceding days or weeks will in most cases be brought to light. In some of these cases, at least, it is safe to say that illness will have been influenza, though not necessarily in its typically acute form. And I am also convinced that the throat lesion in many is that very prevailing one—a superficial ulceration of the fauces which frequently extends to the post-nasal mucous membrane. I refer to that characteristic lesion of influenza which I briefly described in the *British Medical Journal* in 1899 under the title of "The Influenza Throat." This mode of causation is rendered all the more likely by the fact that the diplococcus of Weichselbaum has been found in the nasal discharge in some cases of epidemic meningitis.

If the hypothesis I have suggested is the correct one, it shows us, without going any farther, that the prime requisite for prevention is the disinfection of the throat, nostrils, and post-nasal area by suitable antiseptics, preferably applied in the form of spray. In the case of children too young to submit to this, a harmless disinfectant that might be swallowed with impunity could be applied to the mouth and nostrils with a small ordinary syringe. May we hope that those who come into contact with such cases will put these suggestions to the proof and report the result to the medical press?

I am, Sirs, yours faithfully,

W. FLEMING PHILLIPS, M.B. Glasg.

Fairlie, N.B., Feb. 16th, 1907.

THE PUBLIC HEALTH OF TENERIFFE.

To the Editors of THE LANCET.

SIRS,—Will you kindly allow me to draw attention in your columns to the changes that have taken place in the sanitary affairs of the island of Teneriffe since the publication of the letter of your correspondent, Dr. W. H. Peile? The infectious disease referred to by him was limited to the town of Santa Cruz and its immediate radius. From Orotava, which is the principal health resort of the island, Santa Cruz is distant 25 miles, and not one of the ten practitioners in this valley has seen a single case of the disease. In Santa Cruz the facts are now as follows. There has been no fresh case for 20 days. The lazaretto has been closed and the doctor and his staff relieved of their duties. Communication with the interior of the island is freed from all restrictions. Travellers

are not submitted to inspection or disinfection, and their luggage is not interfered with. In fact, the epidemic is at an end.

It is certain, therefore, that none need be deterred from sending patients to Teneriffe by the recent alarms, and I may take advantage of this opportunity to call attention to the fact that there remain four of the best months of the season, and this does not include June, which is usually one of the pleasantest of the year, the mean temperature being 69.5° F. and the country more beautiful than at any other time. The value of the Canary Islands climate has never been more apparent than lately. In the last six weeks weather of great severity has been general over Europe. The cold has been severe at almost all the reputed resorts of the Mediterranean littoral, while snow and frost are reported from not a few. At Orotava during the same period there has been unbroken brilliant weather, the sunshine averaging eight hours a day, the mean temperature 62°—the actual lowest temperature recorded being 49°. Owing to the enterprise of the hotel companies at Orotava the accommodation for invalids has recently been brought up to a high pitch of excellence. Neither as regards arrangements for comfort nor amusement is there anything lacking to meet every reasonable requirement.

I am, Sirs, yours faithfully,

FREDERICK LISHMAN, M.D. Edin.

Puerto Orotava, Teneriffe, Feb. 7th, 1907.

A LARGE VESICAL CALCULUS (18 oz. 5 drms.) SUCCESSFULLY REMOVED BY THE SUPRAPUBIC OPERATION.

To the Editors of THE LANCET.

SIRS,—Anyone who has had to deal in this country or abroad with large vesical calculi must heartily congratulate Mr. H. Littlewood upon the success of his operation, as described under this heading in THE LANCET of Feb. 16th, p. 422, and upon the fact that the patient lived in very fair health for some nine and a half months after the operation. But when Mr. Littlewood states that so far as he can ascertain his is the largest stone which has been successfully removed, I feel constrained to refer him to your columns of Sept. 16th, 1893, where on p. 687 he will find an account by Mr. H. M. N. Milton of an Extraction by Laparotomy from the Human Bladder of the Largest Stone ever Recorded as Successfully Removed. This stone weighed 34½ ounces and now reposes at the museum of the Royal College of Surgeons of England, within a few feet of Mr. Littlewood's splendid specimen, and is numbered H 2 a. Mr. Milton's stone was removed in Cairo and its largest diameters were six inches and four inches. On the thirty-fifth day after the operation a second stone, weighing 23 grains, was removed from the bladder of the patient, who was about 60 years old, and he died from advanced kidney disease some two and a half months after the operation. The ultimate result was disappointing because he had gone through a period of apparent convalescence in the hospital garden. Even larger stones than Mr. Milton's have been taken after death from the human bladder.

I am, Sirs, yours faithfully,

F. M. SANDWICH, M.D. Durh.

Cavendish-square, W., Feb. 19th, 1907.

IDIOPATHIC CYANOSIS.

To the Editors of THE LANCET.

SIRS,—I read with interest the article on Idiopathic Cyanosis by Dr. Samuel West and Dr. T. Wood Clarke in THE LANCET of Feb. 2nd, p. 272, as well as your leading article in your issue of Feb. 16th, p. 443. In both articles reference is made to the case reported in your columns last July by Dr. G. A. Gibson of Edinburgh and myself. In the paper published on Feb. 2nd the writers state, with reference to our bacteriological results, that "these findings were not confirmed," while in the leading article of Feb. 16th the sentence "since they were unable to confirm these findings" occurs. I should like to know on what assumption Dr. West and the writer of the leading article base this statement. Neither Dr. Gibson nor I have as yet sent them any further information of our case and they are certainly not in a position to make any statements whatever regarding it.

Dr. West's paper makes full allusion to the literature

on the subject, but I would point out that all these references occur in our communication.

I am, Sirs, yours faithfully,

CARSTAIRS C. DOUGLAS, M.D., D.Sc. Edin.

Glasgow, Feb. 19th, 1907.

GAS FIRES.

To the Editors of THE LANCET.

SIRS,—Will you allow me space for a short reply to Mr. J. H. Brearley's letter in THE LANCET of Feb. 9th, p. 385? He thinks he supplied me with information which should have prevented my criticising the Coal Smoke Abatement Commission's test, and he believes he supplied information to Dr. H. A. Des Vœux and Dr. J. S. Owens which enabled them to "deal more effectively with me than they might otherwise have done." I do not think the doctors were in need of his assistance and that is shown by the fact that they used my modification of the permanganate test which is described in the "Air of Rooms" and nowhere else. Clearly, therefore, they had my pamphlet in their possession and knew the size of my room and the details of my investigation. Mr. Brearley accuses me of concealing the size of my room, forgetting that its dimensions were given by me in the original paper and but for me would not be known to him or anyone else.

I am asked why I found the percentage of carbon dioxide higher when a gas fire was in use than with a coal fire, and this fact is taken as "conclusive proof that either the stove was defective or badly fitted, and no other evidence is necessary." But in this statement Mr. Brearley has forgotten that respiration was going on in the room in which my tests were made, and the problem with which I dealt included not only the carrying away of the products of combustion up the flue but also the extent to which the carbon dioxide due to respiration was removed from the room. The ideal ventilation would give us air in a room of the same purity as that outside, but everyone knows that this is practically unattainable in inhabited rooms. As Mr. Brearley does not trust my experiments I would refer him to the investigations on this subject by Angus Smith, Pettenkofer, Roscoe, Carnelley, Carleton Williams, Bedson, and others, not forgetting Dr. Des Vœux and Dr. Owens who found a very slight increase in the amount of carbon dioxide in the room as compared with the amount in the air admitted.

I am, Sirs, yours faithfully,

FRANCIS JONES, M.Sc., F.R.S.E.

Manchester, Feb. 9th, 1907.

A QUESTION OF PUBLIC POLICY.

To the Editors of THE LANCET.

SIRS,—The other night I was asked by a midwife to attend a case of hers which she thought was one of puerperal fever. As I do a large midwifery practice myself, doing between 160 and 170 cases a year, I absolutely refused. I do not see why I should run the risk of giving infection to any of my own patients or alternately having to give up my midwifery practice. In fact, I could not afford to lose four or five guineas a week to attend a case for which I would probably never get paid. I consider that if the State has created a class of so-called qualified practitioners it should also provide a hospital for these septic cases and a properly qualified man to attend them. I would like your opinion on the question and would likewise like to know what the custom is of practitioners in other districts. The question I need hardly say is a very important one for the general practitioner.

Feb. 17th, 1907.

I am, Sirs, yours faithfully,

FORCEPS.

* * Our correspondent acted correctly. He would have been wrong to jeopardise his patients and wrong also to withdraw from attendance on them after they had secured his services. With reference to the pecuniary question, there should, of course, be provision for payment of medical men called in by midwives in accordance with the Act.—ED. L.

LITERARY INTELLIGENCE.—Dr. H. Charlton Bastian's just published work on the Evolution of Life is to be translated into French and published by Félix Alcan in the International Scientific Series.