

asked whether Dr. Hare had had any experience of allergy in these cases.

Dr. DOROTHY HARE replied that she believed that in some of these cases there was a strong allergic factor, but whether this had much bearing on the colitis she could not say. She thought that transfusion helped in a crisis sometimes, but certainly it was transitory in its effects. She had tried the apple diet often recommended; one case improved remarkably, another got worse, and the amount of roughage or undigested apple passed in the stools showed that it might be disadvantageous. As regards lavage with potassium permanganate, she had gathered that this was used mostly in ambulant cases. Such cases did stand lavage, but it was in the case which was going under with a very acute condition, and which had an extremely sensitive mucous membrane liable to haemorrhage, as well as a great tendency to spasm, that lavage caused extreme pain, and she could not think that in such cases it was a suitable form of treatment.

BONE GRAFTING

At the October meeting of the Section of Surgery of the Royal Academy of Medicine in Ireland, with the president, Mr. SETON PRINGLE, in the chair, Mr. H. F. MACAULEY read a paper on the use of bone grafting in modern surgery.

Mr. MacAuley subdivided his procedures into the following groups: (1) for loss of substance, (2) for mechanical blocking of vicious joint action, (3) for plastic purposes, and (4) as an accessory method in treatment. For the treatment of non-regeneration of the tibia following diaphysectomy he proposed a new method—namely, transference of the lower pointed end of the upper fragment of the obliquely osteotomized fibula anteriorly across the interosseous membrane and its implantation into the lower tibial metaphysis. End-results, illustrated by radiograms, were shown of this method, of the posterior ankle-bone-check operation, of the formation of a new acetabular rim in congenital dislocation of the hip, and of the results of bone grafting in cases of tuberculous disease of the hip and spine.

The PRESIDENT commended Mr. MacAuley for bringing out the good and the bad points in the various procedures which he had described. He asked if the operation described for non-regeneration of the tibia could be done at an earlier date after subsidence of the infection than could grafting. Mr. MacAuley's results in tuberculosis of the hip were a strong argument in favour of grafting more of these cases. Mr. F. J. HENRY drew attention to one condition which Mr. MacAuley had not mentioned—namely, sacro-iliac disease. He himself had carried out a Verrall grafting on one occasion, and would like to know what method Mr. MacAuley adopted in these cases.

Mr. C. SOMERVILLE-LARGE agreed with Mr. Henry and the president that this was a remarkable series of cases and the results obtained extraordinarily good. In paralytic deformity of the foot he had occasionally performed lumbar ganglionectomy, with a view to improving the circulation. With regard to spinal cases he would like to know something about the fitting of the graft round the kyphosis, which he himself had found most difficult. Dr. J. P. SHANLEY, referring to spinal grafts, said that Mr. MacAuley had not mentioned Hibbs's operation, which he knew he sometimes carried out.

Mr. MACAULEY, in reply, said that the procedure he had described for non-regeneration of the tibia could, he thought, be carried out at an earlier date than could grafting. He believed the Smith-Petersen operation to be the best for sacro-iliac disease, but these cases were very difficult to deal with. In congenital hip dislocation it was difficult to know whether one would sacrifice mobility to stability; besides mobility it was very desirable to obtain correct length. Referring to spinal cases, he said that Hibbs's operation was better than those which he had described, but it took a great deal longer to carry out and was a most difficult operation to do properly. The double-graft operation was a good one, and probably quite as efficient in the long run.

CORRESPONDENCE

The number of letters received for publication is so large that we must ask correspondents to make their points briefly and to regard 600 words as the upper limit of length.

The Maclean Stomach Powder

SIR,—A point of considerable interest to the medical profession is raised by the recent issue of a prospectus by a firm of manufacturing chemists—Macleans Ltd. In this prospectus proprietary rights are claimed in the "Maclean Brand Stomach Powder," a powder made up according to a formula previously published by me in the *British Medical Journal* and the *Lancet*.

The high ethical standard of our profession in this country demands that any useful treatment or discovery likely to benefit humanity should be published so that it may be tested and utilized, if desired, by any member of the profession. Having effected some simplifications in the method of treating certain gastric lesions by means of alkalis, I published, in your columns and in the *Lancet*, the method I had worked out and found most beneficial, giving the formula of a powder which I had found particularly efficacious in the treatment of gastric ulcer. This I did at the request of a number of medical men who had from time to time heard me lecture on the subject.

Subsequently I learned that a powder was being made up by certain chemists with my name attached to it. In 1931 I appealed to the British Medical Association for help in dealing with this matter, and I desire most gratefully to acknowledge the assistance I received from Dr. Alfred Cox and from Dr. Anderson. In response to our representations some firms withdrew my name, but others refused to do so. Unfortunately one of the firms who persisted in selling the powder possesses the same name as my own, and in a recent prospectus in which they offered their shares to the public they describe themselves as having proprietary rights in the "Maclean Brand Stomach Powder," a powder first sold by them as being made up "according to the formula of Professor Hugh MacLean," and subsequently, after strong representations had been made to prevent this abuse of my name, as the "Maclean Brand Stomach Powder."

Since the medical profession in general know that the powder sold by Macleans Ltd. is made up according to the formula published by me, many people have been led to believe that I am in some way connected with this undertaking, or derive, or have derived, financial benefit from the sale of this powder. I would like, therefore, to state emphatically that I have never given any authority to any firm or person to make use of my name, and that I do not derive, and never have derived, any financial or other gain from the sale of this powder. The whole affair has caused me immeasurable annoyance and trouble, aggravated by the fact that I was, when this first occurred, suffering from a serious illness. I understand that I have no obvious legal remedy in the matter other than a possible action for libel which, whatever the issue might be, would certainly involve me in great expense, in serious loss of time, and in very undesirable publicity.

The subject seems to be one of such importance to the medical profession in general that I venture to suggest that the chief bodies representing the profession should combine to give their attention to the general question which is involved and to consider whether it would not be possible to introduce legislation to amend the law of copyright so as to prevent the exploitation by unscrupulous persons of a prescription or method of treat-

ment published in medical journals for the information of the medical profession. It is obvious that the widespread sale of a remedy prescribed for use in certain conditions might do a great deal of harm if used in other conditions for which it was not intended, and where serious injury might be caused.

In my own case the unfortunate accident of the existence of a firm of manufacturing chemists bearing the same name as my own but entirely unconnected with me has apparently enabled them to attach the name "Maclean" to my powder and to claim some special virtue in the art of dispensing it. If there is no legal means of preventing this sort of practice, it is obvious that some amendment of the law is urgently required to prevent gross injustice to members of the medical profession.—I am, etc.,

London, Oct. 3rd.

HUGH MACLEAN.

Monkey Malaria in G.P.I.

SIR,—The inquiry into the use of *Plasmodium knowlesi* in malaria therapy at the Horton Mental Hospital, which is mentioned in your leading article (October 12th, p. 672), was begun in consultation with Colonel S. P. James, F.R.S., in April, 1934, and is being continued in collaboration with Professor Ciuca and his colleagues at mental hospitals in Bucarest and Jassy, to which we transmitted blood from an infected *Macacus rhesus* in May this year.

The work is yielding valuable results in adding to knowledge of malaria, but the information obtained on the use of the parasite for the malaria treatment of general paralysis does not support the view that it would be advantageous and practicable to employ it on a considerable scale as a substitute for *P. vivax*. The chief disadvantage of *P. knowlesi* for this purpose is that not many patients suffering from general paralysis are sufficiently susceptible to it to react with a therapeutically effective malarial attack. According to the records at Horton and in Rumania—seventy-six cases in all—the intramuscular or intravenous inoculation of blood from highly parasitized *Macacus rhesus* failed to produce fever and other clinical symptoms in 56 per cent. of patients suffering from general paralysis who had never previously had any form of malaria, and in all except one of sixteen patients who had a previous history of this disease. Moreover, in 25 per cent. of the successful infections the resulting attacks of malaria were of an abortive character, with few parasites in the blood and spontaneous recovery in less than a week. In the remaining cases the infection developed actively, as in the examples described by Drs. van Rooyen and Pile (*Journal*, October 12th, p. 662), but there were only a few in which the temperature during the febrile paroxysms exceeded 104.4° F., which is considered to be the lowest temperature likely to be therapeutically effective in general paralysis. Drs. van Rooyen and Pile seem to have had the same experience. At Horton, on account of the mildness of the fever and the tendency to spontaneous recovery, it was considered necessary to give nearly half the patients who had been treated with *P. knowlesi* a supplementary course of malaria with *P. vivax* or *P. malariae*.

As regards practicability, the chief disadvantages are that *P. knowlesi* quickly loses its pathogenicity for patients suffering from general paralysis when it is passed from person to person, and that as yet it has not been successfully cultivated in mosquitos. For these reasons the routine employment of the parasite would necessitate the continuous provision of a large supply of infected *Macacus rhesus*, which would be more costly than the existing arrangements for the continuous provision of mosquitos infected with *P. vivax*. As a therapeutic agent, the use of *P. knowlesi* is purely in the experimental

stage, and from experience in England the employment of *P. vivax* can be regarded as both safe and efficient.

At Horton the employment of a strain of quartan malaria has been of much value in being available for those cases which may be immune to benign tertian; moreover, the fever-free intervals in quartan render it useful for a more debilitated type of patient who cannot withstand a quotidian fever.—I am, etc.,

Horton Mental Hospital, Epsom, Oct. 12th. W. D. NICOL.

Treatment of Cancer by Proteolytic Enzymes

SIR,—In the *Journal of the Canadian Medical Association* for October, 1935 (p. 364), an article entitled "The Study and Treatment of Cancer by Proteolytic Enzymes," by Dr. H. C. Connell, appears. In this article reference is made to the fact that the Imperial Cancer Research Fund had been asked by Dr. Connell to investigate experimentally his claim to have discovered a method of killing cancer cells *in vivo*—potentially a cure for cancer. We have prepared from mouse tumours, in accordance with directions received, the solutions which Dr. Connell calls "ensols," and have tested these "ensols" on the appropriate tumours of mice. In no case has the growth of a tumour been checked or affected in any way.—I am, etc.,

8-11, Queen Square, W.C.1., Oct. 14th.

W. E. GYE.

Views on the Cancer Problem

SIR,—Professor Blair-Bell's abusive letter which appeared in the *Journal* of September 21st requires but brief reply. The reference made to the letters in the *Lancet* (1925, ii, 1142 and 1196), under the heading "The Trophoblastic Hypothesis of Cancer," was unfortunate. May I quote the concluding paragraph of Dr. J. A. Murray's letter (p. 1142)?

"The patronizing attitude towards other workers which pervades Dr. Bell's whole lecture comes ill from one who, it is charitable to assume, is himself ignorant of the recent literature of the subject."

At a meeting of the scientific staff of the Liverpool Cancer Research Committee (not the L.M.R.O., as stated), I pointed out that the reply to Dr. Murray's letter as drafted and read by Professor Blair-Bell contained incorrect statements of Beard's views on the nature of the trophoblast as expressed in his numerous writings. In the amended letter, which appears on page 1196 of the *Lancet* (1925, ii), similar misstatements occur.

The letter in your correspondence columns of September 21st exhibits the same patronizing attitude towards the work and views of other investigators (to which Dr. Murray referred) and a similar lack of knowledge of the literature of the period 1893-1933, which includes papers, too numerous to mention here, by many well-known British, American, and Continental embryologists, dealing with the early developmental stages of the fertilized ovum in many mammalian species prior to the appearance of any embryo.—I am, etc.,

H. E. ANNETT,

Turner Research Laboratory,
University of Liverpool.

October 10th.

Injuries to the Semilunar Cartilages

SIR,—It is very interesting to read in the *Journal* of October 12th the comments of Dr. J. K. Surls on my paper "Injuries to the Semilunar Cartilages." Both he and Mr. Timbrell Fisher speak with authority. Their differences from me are, I think, rather apparent than real. Both of them disagree with the statement that in injuries of the posterior extremity of the cartilage "lock-