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 passaged 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 Hosptal, 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-

ing " is absent. I feel pretty certain of this point if the injury is really posterior and we understand by " locking" inability to extend the knee completely. But if the lesion comes forward it will cause the typical " locking," in which case, however, the injury will be detectable from the front. I might have put the statement in another way, by saying that if the anterior part of the cartilage is intact locking does not occur. It is really a question of how posterior the damage is.

Dr. Surls says he has often been able to excise the whole cartilage from the front. Of course it is possible when it is sufficiently loose, as every surgeon must know. What I confess myself unable to do is to remove an intact cartilage from an anterior incision. If he would read the text of the paper again he will realize that what I do so strongly deprecate is the attempt to remove the cartilage when, on opening the joint, the cartilage appears to be intact. If the looseness does not extend sufficiently far forwards the manœuvre will cause undue damage to the joint. I believe I have his support in this view.

As I indicated, the necessity for the removal of the whole of the cartilage in every case is a debatable point. There always is some uncertainty about the condition of the periphery, and Mr. Fisher's experience in operating a second time on many knees where the partial removal has been done must carry great weight. I am not influenced by the feeling that removal of the whole cartilage weakens the joint, because I know this not to be true. I do not, however, believe with him that the internal cartilage moves bodily towards the interior of the joint in every case at the primary injury. It may, and very likely does, move only in association with the tibia as it rotates on the femur, without becoming detached at its periphery. Certainly at operation in many cases of bucket-handle deformity, the peripheral segment is firmly fixed, and certainly very many patients have been cured by removal of the bucket handle alone. This simple procedure is such a minor intervention that I should be loath to abandon it altogether. However, it must be admitted that sometimes either such peripheral mobility is present or the patient with his peripheral remnant may suffer another injury, as I pointed out in my paper. This, indeed, is the weak point in such a policy. Whether it is a great defect or not depends upon the frequency of such happenings. I believe it not to be great because, unlike Mr. Fisher, I have not detected such constant hypermobility of the peripheral fragment as he has done.

Dr. Surls objects to my statement that lateral displacement of the patella is not justifiable for exploration of the knee-joint in these cases. I did not give reasons for this attitude because I thought other surgeons must have come to the same conclusion. This procedure necessitates a long incision, and when the patella has been displaced to the lateral side access for removal of the internal semilunar cartilage is, in my experience, not improved in any way, whilst the removal of the external cartilage is not easy either. When in doubt as to which cartilage is at fault a much less disturbing exploratory operation is to open the inner side of the joint both in front and behind (a small incision in the synovial membrane will suffice), and then, if no injury of the internal cartilage be found, to make another opening on the outer side of the patella.

Like Mr. Fisher I have often been in the dilemma, after the first accident, of having to choose between operation, with quick return to game-playing, and conservative treatment, with a fair prospect of being forced to operate when much of the season has gone. Every surgeon must settle this problem for himself, and the solution will not be the same for every patient. The general policy which I myself pursue I indicated in my paper. The views expressed in that paper are the conclusions come to after some considerable experience as a general surgeon. They are always open to revision as fresh facts come to light, and it is for this reason that I particularly welcome the kindly criticism of both Dr. Surls and Mr. Timbrell Fisher.—I am, etc.,

CHARLES A. PANNETT.

St. Mary's Hospital, W.2, Oct. 14th.

---

SIR,—Dr. J. K. Surls's letter offers somewhat severe criticism of Professor Pannett's statement that locking is absent in posterior horn lesions of the semilunar cartilages. He suggests that this view is unusual, that it has never before been reported in the literature, and that it is quite unorthodox. Unless Dr. Surls's comments are amplified they do an injustice to Professor Pannett, and they may obscure a valuable feature of his article where attention is again drawn to a clinical syndrome far too rarely recognized.

It is surely obvious that the presence or absence of locking depends entirely on the size of the fragment which is separated. A peripheral tear may loosen the greater part, or even the whole, of the posterior third of the cartilage, and when this large thick fragment slips into the joint locking results. Such a case offers no more difficulty in diagnosis than does the ordinary complete bucket-handle tear. But Professor Pannett was describing an entirely different clinical entity, in which diagnosis is difficult because there is never locking of the joint—a clinical entity which has been recorded in the literature, and which probably represents the commonest posterior horn lesion of the cartilage. In this case there is a tear, not of the thick peripheral margin, but of the thin free margin of the posterior horn, with the separation of a thin pedunculated fragment. When the fragment is displaced there is a sharp twinge of pain, a sensation of weakness, or a feeling that the joint is about to lock or to give way, but the fragment is neither thick enough nor mobile enough to cause actual locking. A similar pathology and a similar clinical picture is seen in most cases of incomplete removal of the cartilage where a posterior horn tag has been allowed to remain. There is probably no knee-joint injury which is more frequently overlooked. With a characteristically vague history, the only conclusive physical sign is the typical click elicited by Mr. T. P. McMurray's very valuable rotation manipulation of the fully flexed knee—a click which is quite different from the external cartilage thud accompanying active extension of the joint.

In view of Professor Pannett's recognition of this lesion it seems unfortunate that he permits himself to remove only the displaced central half of a cartilage with a bucket-handle tear, because the two lesions are quite often associated, and the small pedunculated fragment may arise from the peripheral half and not be visible until the peripheral half has been actually removed. When a torn cartilage has been subject to recurrent displacement secondary injuries have frequently been sustained, and it is no uncommon thing to find two or three different types of tear in one and the same cartilage. Any of these lesions may be invisible until the whole cartilage has been removed.—I am, etc.,

R. WATSON JONES.

Liverpool, Oct. 14th.

---

## Radium Treatment of Naevi

SIR,—I thank Dr. Herbert Brown (*Journal*, October 12th, p. 702) for his kind interest in my paper on the above subject. The lecture, as he surmises, was given to general practitioners, and exact details of dosage would, of course, have been useless to them: one could only indicate the general principles.