with remorse and guilt, not to mention fear of having contracted V.D., that he underwent complete investigation and had a course of prophylactic penicillin. He was told that he was free from infection but was not convinced, and even after repeatedly negative W.R.s, etc., his obsession became stronger and he was eventually invalided home to this country on psychiatric grounds. He came to see me about it and I had his W.R. done again, but yet another negative result failed to convince him of his freedom from infection, and a few days afterwards he committed suicide by hanging himself in a neighbour's garage, leaving his wife and two young sons to fend for themselves.

—I am, etc.,

Auckland, New Zealand.

D. C. FAULL.

Prognosis in Schizophrenia

SIR,—Dr. Henry R. Rollin's letter (April 15, p. 1104) must make disheartening reading to anyone with insufficient knowledge of the effect of modern drugs on schizophrenic states. Some of your correspondent's conclusions appear to be based on the premise that modern methods of treatment (of schizophrenia) can do nothing more than expedite the advent of a remission. Dr. Rollin argues that modern methods of treatment do not prevent recurrences, and, in support of this contention, submits that the number of readmissions must represent an alarming number of relapses. Unlike Dr. Rollin, I have been impressed by the efficacy of the phenothiazine group of drugs in controlling and suppressing schizophrenic symptoms. In my experience, relapses are exceptional in those numerous cases of schizophrenia in which a prompt and adequate remission has followed the use of one or other of the phenothiazine drugs, *provided* the patient continues to take the appropriate drug conscientiously and in accordance with instructions. To be sure, a steady trickle of patients who responded well initially to phenothiazine medication have to be readmitted, but the majority of these frankly confess that they have ceased to take their tablets. A high proportion of those patients who respond favourably to tranquillizing drugs are destined to become useful members of society, and to work competently in the home, office, or factory. Indeed, so potent are these modern drugs in controlling the manifestations of schizophrenia that selected cases can be treated from first to last as out-patients.

Unhappily, not every schizophrenic shows a favourable response to drugs, long-standing cases being especially refractory to their influence. It is these patients whose schizophrenic symptoms cannot be satisfactorily controlled by treatment, or who have travelled too far along the road to dementia, who need to remain within the confines of a psychiatric hospital. In my view, it is wrong to allow patients to leave hospital who are not only not well enough to do so but who are burdened with the additional handicap of having no home, no job, and no family to which they can repair. I have no doubt that a significant number of such people would (as Dr. Rollin suggests) find their way to prison, the doss-house, or the embankment, should they be discharged from hospital with no visible means of succour or support. The fact that Dr. Rollin has examined in prison over a period of months no less than 17 seriously disturbed schizophrenics, all of whom were former patients of psychiatric hospitals, does suggest that the admission of patients to psychiatric hospitals on an *informal* basis has been somewhat overdone. The root of the trouble lies, I suspect, in the activities of those "forward-looking" psychiatrists who

tend to think in terms of the maximum, rather than the *optimum*, number of open wards and informal patients. It would be tragic if recent hard-won gains in the guise of increased freedom for the inmates of psychiatric hospitals were to be jeopardized by incautious thinking of the kind mentioned above, for public opinion—ever fickle—could soon grow hostile to any system which failed to protect the community from dangerous patients, or patients from the dangers of the community.

—I am, etc.,

Ilkley, Yorks.

J. TODD.

Routine Lumbar Radiography

SIR,—In his letter (April 15, p. 1106) Mr. D. J. Reid suggests that for patients complaining of back pain an adequate diagnosis can be made without routine radiological examination. This is a courageous statement and a most attractive proposition in many ways, and its implementation throughout our out-patient departments, especially in the orthopaedic and physical medicine clinics, would doubtless result in considerable economy and saving of time, but surely at the expense of lowering an acceptable clinical standard. Many patients suffering from backache, with the exception of those with obvious evidence of prolapsed intervertebral disk, are referred for the purpose of eliminating the presence of serious disease. However thorough the clinical examination, it cannot be denied that radiological investigation is a very helpful adjunct.

Osteoporosis, vertebral haemangioma, Paget's disease, and osteochondritis need give no specific clinical signs and no increase in the sedimentation rate, which is suggested as a cheap alternative, but many of these conditions might be harmfully treated by manipulation. A full clinical examination very often fails to reveal the primary focus of a spinal metastasis. In mechanical derangements of the lumbar spine radiological examination is of primary assistance in revealing lumbar instability, spondylolisthesis, spondylolysis, or significant disk degeneration. A neoplasm within the spinal canal or an aneurysm anteriorly may give no physical signs but suspicion of the true diagnosis be aroused by a routine film. In the early stages of many other comparatively serious conditions such as tuberculosis, low-grade osteomyelitis, ankylosing spondylitis, and myelomatosis clinical signs may be minimal but x-ray evidence obvious.

The experienced bone-setter, chiropractor, or osteopath develops a clinical sense which helps him to avoid most of the pitfalls of manipulation, and, recognizing this, the public accept this element of risk wholeheartedly, but to whom should a doctor send a patient for more comprehensive assessment if the consultant is prepared to do the same?—I am, etc.,

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P. H. NEWMAN.

Surgical Treatment of Parkinsonism

SIR,—I am writing in regard to your leading article (November 12, 1960, p. 1437) concerning the surgical relief of parkinsonism. I am particularly happy to note your interest in this field, and I am very pleased that through the work of Mr. F. J. Gillingham (p. 1395) and others in Great Britain it is becoming apparent that this type of surgical therapy should be made increasingly available to suitably selected patients who suffer from this disease.

Although I find your attention to this matter gratifying, and although I am grateful for your kind reference to my own contributions to this field, I must respectfully call your attention to an inaccuracy in interpretation of my own work, which is worthy of note because it applies to the fundamental concepts of basal ganglia surgery for parkinsonism and other diseases. In your leading article you state, "There can be no justification for magnifying the possible errors by the use of inaccurate apparatus. The use of non-stereotaxic apparatus limits the application of the operation to early and preferably unilateral cases." These statements are somewhat paradoxical, since one would not be willing to use any inaccurate method for early cases and unilateral patients, as these patients deserve at least the same respect and meticulous attention that more advanced cases would require. The subsequent sentence of the leader appears to infer that our own attention has not been directed towards anatomic accuracy. This inference is incorrect.

Although, along with practically everyone else engaged in this type of work, I lay extremely heavy emphasis upon suitable selection of patients for surgery, I do not feel that this type of surgery should be used only in early or unilateral cases. Our own series of 1,800 cases includes patients ranging in age from 8 to 79 years, and varying from early symptomatology to total invalidism. Moreover, although I have stressed that anatomic accuracy by itself will not provide for a successful surgical intervention, nevertheless since 1953 I have been using an apparatus which combines the principles which you refer to as especially safe and accurate. The instrument and technique of operation are described in my monograph on this subject.¹ Since we have operated on more than 1,800 patients with extrapyramidal diseases, with a mortality rate presently less than 2%, and with a follow-up system of observation of patients followed both at our own centre and by a special group from other centres, as described in my monograph, the inference that I would be willing to settle for an inaccurate method of surgery deep within the human brain is one which I could not pass without calling this inaccuracy to your attention.

If I may follow this particular line of thought one step further, I should like to state that you have actually emphasized in your leader precisely the steps which we have stressed which make this type of surgery safe, reasonably certain, and practical for a large group of parkinsonian patients. These steps are: (1) Accurate, but simple, instrumentation combined with accurate x-ray localization to provide rapid, reproducible basal-ganglia surgery. (2) Clinical physiologic testing of a surgical lesion deep within the brain, before inflicting a permanent lesion. Because of the fact that parkinsonian patients may demonstrate (a) various networks of cortical and subcortical lesions, (b) various degrees of ageing of the brain, (c) various anatomic variations, and (d) varying symptomatology, it is absolutely necessary to realize that from time to time the lesion must be varied to suit a particular patient. For this reason some type of physiologic testing of the lesion site is necessary. We prefer the method of balloon inflation which we describe. However, others have other reliable methods which are useful and practical. It is only the principle of clinical physiologic testing which I wish to stress, not necessarily the method for its production. (3) The importance of permitting a cannula to remain within the brain to produce gradual destruction over a period of time so that this lesion may be checked roentgenographically as well as clinico-physiologically during its production has been one of the cornerstones of this type of surgery so far as I personally am concerned.

Thus, although anatomic accuracy in this type of surgery is obviously of the utmost importance, it is only one of several equally important factors in this particular type of brain surgery. For example, you refer to the fact that accuracy within one millimetre is now possible with various types of instrumentation. However, one must not lose sight of the fact that, even if it were possible to destroy the same cubic millimetre of

tissue in each individual human brain, this would not necessarily be desirable because of the anatomic variations, pathologic variations, ageing variations, and other factors which must be taken into account. Moreover, in many instances an electrode with an exposed surface as large as ten millimetres is employed to produce the desirable lesion. Consequently, what must be sought after is practical accuracy which suits the needs of each individual patient. The point which I have tried to make in this regard is that with good three-dimensional roentgenography one can achieve this degree of accuracy with a much simpler type of instrumental approach than has heretofore been generally believed to be the case. As a corollary to this simplification of approach, I might add that this is what has made it possible to apply basal-ganglia surgery to the aged and to the far-advanced case as well as to the earlier and unilateral case. The aged and incapacitated patient cannot tolerate long, tedious, complicated procedures. It is for that reason that the simplified but practically accurate approach which you advocate in your editorial is so important. It is precisely this type of approach which I have advocated, which does not appear to be apparent in your editorial.

I have sought by careful documentation, such as that presented in the monograph, to provide a firm, scientific basis for the support of these principles. Purely anatomic stereotaxis is too stereotyped. Certainly anatomic accuracy, clinico-physiologic testing, and gradual infliction of the lesion are basic fundamental concepts to be applied to this type of surgery. However, on the basis of our own experience, I would emphasize as well that careful selection of candidates for surgery, including both early and advanced cases, is as important as meticulous surgical technique.—I am, etc.,

Division of Neuromuscular Diseases, IRVING S. COOPER.
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New York.

REFERENCE

- ¹ Cooper, I. S., *Parkinsonism: Its Medical and Surgical Therapy*, 1961. Charles C. Thomas, Illinois.

Waste of Medical Manpower

SIR,—Once again my *B.M.J.*—the issue for February 11 with Sir Douglas Robb's Presidential Address (p. 375)—has reached me full of articles encouraging men to help the underdeveloped countries.

I would first like to agree whole-heartedly that there are many parts of the world, such as the one in which I am now living, where doctors are desperately needed. My husband, who works in the Agricultural Department, and I are stationed in a division of 10,000 square miles, with a population of a quarter of a million people, whose only hospital is separated from the majority of its people by the River Benue, which is in spate, and therefore unnavigable by local transport, for about six months of the year. The only hospital within our reach is 100 miles away by bumpy dirt road, which is liable to be cut by the rains. There are nine dispensaries in the area, of which four are, at present, staffed by "trained dispensary attendants"; there is, of course, no doctor this side of the river. The only ambulance broke down six months ago and has not been repaired.

Until I married in June last year, I had been working as an obstetric registrar in a London teaching hospital, and as soon as I arrived here I applied for employment. Six months later I was put on the register. Eight months later I was told second-hand that "it was considered a waste to employ a temporary doctor on a month-to-month basis away from a hospital." It seems