

Correspondence

Letters to the Editor should not exceed 500 words.

Multiple Sclerosis and Poliomyelitis

SIR,—We were very interested in Dr. E. D. Acheson's suggestion (10 July, p. 107) of a method which would test the Poskanzer hypothesis¹ that multiple sclerosis with clinical poliomyelitis represents the occasional neurological manifestation of a widespread subclinical enteric infection. Dr. Acheson believes: "In order to demonstrate that the epidemiology of poliomyelitis and multiple sclerosis in Australasia are consistent with Poskanzer's hypothesis it is necessary to show that where multiple sclerosis is thought to be rare (Queensland and Western Australia) poliomyelitis tends to attack *younger people* than where multiple sclerosis is thought to be common (New Zealand and Tasmania)."

He has studied the notification rates by age for single poliomyelitis epidemics in some regions of Australasia and elsewhere (one epidemic for each region), and has not found an earlier age of onset of poliomyelitis in the areas in which multiple sclerosis is uncommon. However, we wonder if it might be necessary to study the behaviour of poliomyelitis over some years rather than in a single epidemic before conclusions are drawn, in case different intervals between epidemics in various regions tend to play a part in determining the notification rate by age for poliomyelitis in any one epidemic.

Dr. Acheson in his letter also expresses the view that our method of studying the relation

between poliomyelitis notifications and multiple sclerosis mortality in various parts of Australasia (5 June, p. 1471) employs an unsuitable correlate. We could accept his view if our intention had been what Dr. Acheson seems to imply it was—to test the Poskanzer hypothesis by the approach he advocates. But we had merely intended to see if there was a relation between the geographical distributions of multiple sclerosis and poliomyelitis in parts of Australasia, as it was on the basis of such a relation throughout the world that Poskanzer based his hypothesis, though he did not publish his data. Thus our study was intended merely to supply one of the prerequisites to the Poskanzer hypothesis, using data perhaps not readily available outside Australia, not to test the validity of the hypothesis itself; and in these circumstances we feel the method we employed was not inappropriate.—We are, etc.,

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REFERENCE

- ¹ Poskanzer, D. C., Schapira, K., and Miller, H., *Lancet*, 1963, 2, 917.

Treatment of Stein-Leventhal Syndrome

SIR,—I read with interest Professor T. N. A. Jeffcoate's lecture on amenorrhoea (14 August, p. 383). I should like to comment upon one aspect.

When dealing with the Stein-Leventhal syndrome—a subject which I have been interested in for many years^{1,2}—Jeffcoate says that, "treatment of this condition is generally accepted as being by resection of a wedge from each of the polycystic ovaries." With this, of course, I agree. However, he continues by stating that "good results . . . attend symptomatic treatment, such as the correction of obesity, sympathetic investigation, and pre-variation," and that "resection of polycystic ovaries should not be the first line of treatment for patients suffering from this defeminizing syndrome." I agree also in the main with these statements, but would wish to give warning that palliative treatment in these cases of bilateral polycystic ovaries and other degenerative ovarian changes consequent upon gonadotrophic dysfunction should not be carried on for too long in established clinical cases. One must remember that the patient in the late twenties complaining of relative functional amenorrhoea and infertility

and showing evidence of one of these conditions exemplifies the type of case in which time does not work on her side. In my own relatively long series of cases I have found that the great majority show progressive atrophic change in the uterus and endometrium and that the functional outlook becomes progressively poorer as these changes become more established. My own practice has always been to allow for at least one year for general and hormonal treatment. In the U.S.A. they tend to operate at a much earlier stage.

Finally I have found that the results from wedge resection with eversion have been so good in otherwise intractable cases that in some respects I would consider this operative treatment, which results in the ovum being produced by the patient's own pituitary activity, preferable to treatment by another human pituitary influence.—I am, etc.,

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REFERENCES

- ¹ Bailey, K. V., *J. Obstet. Gynaec. Brit. Emp.*, 1937, 44, 637.
² — *ibid.*, 1959, 66, 556.

Medical Education and Medical Practice

SIR,—Professor L. J. Witts (18 September, p. 699) has put an interpretation on my remarks which was not intended. I know that some teachers in university departments (he and I for two) have never lost interest in human behaviour, but I would contend that the research and teaching interests of such departments have been almost wholly concerned with the physical aspects of organic disease, and that they have not greatly contributed to knowledge of human behaviour.—I am, etc.,

Cambridge.

ROBERT PLATT.

SIR,—May I comment on Sir Robert Platt's sad and wise "Thoughts on Teaching Medicine" (4 September, p. 551), on the Bradshaw Lecture by Dr. Alastair Hunter (p. 552) in the same issue, and on Professor L. J. Witts's characteristic reaction to Sir Robert's thoughts (18 September, p. 699)?

Both latter utterances unwittingly illustrate Sir Robert's quotation from an *Observer* article, which, in effect, is his theme. This article deplors the virtual exclusion from academic institutions of studies, now 70 years old, of "the workings of the unconscious mind"—that is, the growing corpus of knowledge in dynamic psychology and psychological medicine based on Freud's work. Sir Robert, at a farewell occasion, generously confessed that he had a share in the blame for withholding this knowledge about "man himself" from medical education, whose leaders "pretend to believe that the secrets of medicine are revealed only" to men with proficiency in the physico-chemical disciplines.

Without taking similarity too far, I was reminded of my one-time revered chief, Sir Francis Fraser's like reflections, shortly before his death, in a Harveian Oration. During all Sir Francis's immensely influential active work in medical education, in the battles I had with him, he would only concede the need for the study of man as an organism or a "statistic," and saw the future of medicine as measurement of "hard data." Dr. Hunter, in more guarded terms, expresses the same caution and doubt about "what to teach" (p. 555). I read him as saying, in effect, how dangerous it is to let students have access to "what to many seemed fantasy attaching to some concepts of psychopathology and psychotherapy." No names, no packdrill. We are ever so tolerant now.

This reaction is more banteringly revealed in Professor Witts's brief letter to you. "What!—that old Aunt Sally again!" Why, we have let psychiatrists and "social medical workers" (*sic!* he is not sure of what these people really are called) loose on our students. For the rest—are there not Dickens, Dostoevsky, and Freud (*et al.*) even if they are not kept in the medical library. So this subject, though the student must have some nodding acquaintance with it, is not really at the heart of medicine, as Sir Robert Platt has realized it ought to be.