

there must be some means of organizing refresher courses, seminars, etc., but it must not be allowed to dictate terms of entry to practice.—I am, etc.,

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### British Academy of Psychopharmacology

SIR,—The Collegium Internationale Neuro-Psychopharmacologicum (C.I.N.P.) was founded at the second World Congress of Psychiatry in 1957. At that time—five years after the introduction of chlorpromazine into psychiatric therapy—the foundation of this international association was an important contribution to establishing a platform for scientists from various branches to discuss and handle problems of mutual interest. Since then this international association has proved to be of considerable value for collaboration between basic scientists and clinicians. In the course of the past 15 years associations or academies have been founded in several countries aiming to promote development of psychopharmacology and pharmacopsychiatry. Collaboration between the C.I.N.P. and all national associations is close and effective. The C.I.N.P. Executive Committee always regretted that there had so far been no society or academy of psychopharmacology in Great Britain. This fact has been particularly regrettable as pioneering studies in many fields of psychopharmacology and pharmacopsychiatry were carried out by British scientists.

With pleasure we heard the news telling that a British Academy of Psychopharmacology has now been established (2 March, p. 391). It is my conviction that foundation of this academy will further stimulate psychopharmacology in Great Britain. Therefore I have pleasure in congratulating this newly established academy on behalf of the C.I.N.P. Executive Committee and in wishing it all the best for their future development. The C.I.N.P., as an international organization, will promote with emphasis the future work of the British Academy of Psychopharmacology.—I am, etc.,

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### Epidemic Neuromyasthenia

SIR,—The finding of abnormal lymphocytes in some of the patients affected by epidemic neuromyasthenia (E.N.) in the Great Ormond Street epidemic (23 February, p. 301) is reminiscent of the Dalston (Cumberland) epidemic<sup>1</sup> in 1955, in which atypical lymphocytes were found in 30% of the patients and could be detected for as long as 18 months after the onset of the illness. This suggests that the prolonged convalescence in some cases is due to persistent smouldering of an infective process.

In a previous communication<sup>2</sup> I have drawn attention to the peculiar association of E.N. with poliomyelitis. E.N. appears to alter the normal epidemiological pattern of poliomyelitis. In 1955 the spread of an extensive type-1 poliomyelitis epidemic around the coast of Iceland was blocked by the appearance of a concurrent epidemic of E.N.

in two towns and also in a district (Akureyri) in which there had been a severe epidemic of E.N. a few years previously. Children in one of the towns affected by E.N. showed unexpected antibody responses to poliomyelitis vaccination the following year.<sup>3</sup>

The case incidence of both the Dalston<sup>1</sup> and Iceland<sup>4</sup> types of E.N. shows an almost equal overall sex ratio, with male preponderance in some of the children's age groups, suggesting an infective, non-hysterical aetiology.

A new clinical entity, subacute myelo-optic neuropathy (S.M.O.N.), which appeared in Japan over 10 years ago has certain features in common with outbreaks of E.N. in other parts of the world. Controversy has existed as to whether the condition is caused by a virus infection or by the use of clioquinol for the treatment of diarrhoea. Both agents have been shown to produce similar neuropathic lesions and, when combined, appear to produce a severe neurological disorder with a mortality rate of up to 5%. I have watched the Japanese investigations with interest since a virus was isolated from patients suffering from S.M.O.N. which inhibited the growth of poliovirus.<sup>5</sup> This effect was neutralized by serum from one of these patients. Then Inoue *et al.*<sup>6</sup> reported the isolation of a virus, from which they prepared an anti-serum which neutralized the C.P.E. (incomplete cytopathic effect) produced by other viruses from the stools and also the C.P.E. produced by all viruses isolated from the spinal fluid of S.M.O.N. patients. They considered that the low neutralizing antibody titres in sera from S.M.O.N. patients might explain the subacute and relapsing course of the disease.

It was most unfortunate that the attempt to isolate the virus from the Great Ormond Street patients by Dr. M. J. Dillon and his colleagues was frustrated by a mechanical breakdown during a vital stage of the isolation procedure. However, until it is known whether Inoue's virus can be neutralized by sera from patients with E.N. it would be wise to avoid the use of clioquinol for diarrhoea in patients presenting with features of this syndrome.—I am, etc.,

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<sup>1</sup> Wallis, A. L., M.D. Thesis, University of Edinburgh, 1957.

<sup>2</sup> Parish, J. G., *British Medical Journal*, 1970, 3, 47.

<sup>3</sup> Sigurdsson, B., Gudnadóttir, M., and Pétursson, G., *Lancet*, 1958, 1, 370.

<sup>4</sup> Sigurdsson, B., *et al.*, *American Journal of Hygiene*, 1950, 52, 222.

<sup>5</sup> Okuda, K., *et al.*, *Brain and Nerve (Tokyo)*, 1965, 17, 895.

<sup>6</sup> Inoue, Y. K., Nishibe, Y., and Nakamura, Y., *Lancet*, 1971, 1, 853.

### Attitudes to Abortion

SIR,—Your leading article (13 April, p. 69) is a sad reflection of the confused thinking in current medical and national ethics.

From the time of Hippocrates until 1967 abortion was unacceptable to the majority of doctors. You admit yourself that it "is indeed distasteful to many people" (you do not suggest why this should be). Yet now you consider it "useful" that the Lane report may cause the fading away of "shrill

and emotional argument," and you accept that "no major changes will be made in abortion law in Britain in the foreseeable future."

Your use of these words implies an attitude of censure or at least distaste. But what is distasteful or reprehensible in arguing for the retention of a profession's ethical standards? And why should people not strive for the repeal of a law which permits the wholesale destruction of human life? Perhaps this is what you dislike as being "shrill and emotional"—perhaps the profession and the country would like to forget that this is what the Abortion Act permits. Yes, Sir, this is a matter for human emotion—the emotion of pity for the human being deliberately liquidated as surely as by bullet, bomb, or gas chamber. And yet you surely would lament the latter.

And if abortion is morally right in some cases, why not in all? What right has any of us to say that one fetus shall die and not another? Why not accept abortion on demand? Except in quantity it cannot be any more wrong than selective abortion, and if morally right, why deny any fetus the right to be killed? You must be consistent.

So do not decry the argument and protest, for if these fade we shall all suffer further devaluation of human life.—I am, etc.,

MICHAEL MORRIS

Buckden, Hunts

SIR,—In your leading article (13 April, p. 69) on the Lane Committee Report you state: "A conscience clause was manifestly essential when the Act came in, since many gynaecologists had sincere moral or ethical objections to abortion on some of the grounds introduced by the new Act. Seven years later the situation has changed." This to me implies that you do not consider a conscience clause necessary any longer. I hope that I am not quoting you out of context.

This is an extraordinary statement with wide implications involving doctors and their assistants. The Act recommended a fundamental change in historical ethics. The fact that abortion has become "conventional medical practice" does not reassure gynaecologists and others who have sincere moral and ethical misgivings. It merely confirms their worst fears.—I am, etc.,

P. GOLDING

Redruth, Cornwall

### Alternatives to Animal Experiments

SIR,—We welcomed the Stephen Paget Memorial Lecture on this subject by Professor J. L. Gowans (23 March, p. 557), and we studied both it and your leading article (p. 528) with interest.

There are several points in both we would like to comment upon concerning certain conclusions drawn from the data presented, but confine these to the two issues implicit in the final paragraph of the lecture. For surely Professor Gowans would not claim that the twin assertions therein can be fully substantiated in the published literature.

The first concerns the number of animals required for the provision of culture material. One of the recognized advantages of such systems is their economy in this