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.,

J. MILLER AITKEN

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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) epidemic¹ 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 communication² 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.³

The case incidence of both the Dalston¹ and Iceland⁴ 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.⁵ This effect was neutralized by serum from one of these patients. Then Inoue *et al.*⁶ 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.,

J. GORDON PARISH

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¹ Wallis, A. L., M.D. Thesis, University of Edinburgh, 1957.

² Parish, J. G., *British Medical Journal*, 1970, 3, 47.

³ Sigurdsson, B., Gudnadóttir, M., and Pétursson, G., *Lancet*, 1958, 1, 370.

⁴ Sigurdsson, B., *et al.*, *American Journal of Hygiene*, 1950, 52, 222.

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

⁶ 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

respect, with relatively small sections of tissue yielding cell lines which are capable of self-reproduction time and time again. Furthermore, human fibroblasts are probably used to at least the same extent as cells derived from animal tissue and the cultures thereof are likely to produce data more relevant to man than those of animal origin. It is appreciated that organ culture is more extravagant of material, but even so it is seldom that complete organs are required. In any event, the number of projects based on cell culture alone demanding investigation is such that existing research resources cannot realize the potential inherent in them within a reasonable time.

The second issue concerns the alleged necessity to use vast numbers of animals for comparative purposes when developing in-vitro techniques. Professor Gowans will doubtless have knowledge of such techniques developed without the sacrifice of the numerous additional animals which he implies is inseparable from such research. We have already obtained confirmation of the premise that this is indeed possible during the first year of operation of the Lord Dowding Fund. Grants for several such projects have been awarded and assurances received that no animals would be involved at any stage. Furthermore, a vast amount of data must have been compiled from millions of past experiments on animals, and it is realistic to assume that much of this is being utilized for assessing replacement techniques in course of development.

It is, perhaps, not surprising that Professor Gowan's observations are confined to research almost entirely concerned with in-vitro systems. This justifies our assertion that in existing research establishments there is a far too rigid demarcation, and separation into specific disciplines, of projects with little co-ordination between them. If real progress is to be made towards what we, the National Anti-Vivisection Society, would like to regard as a common objective (however euphemistic that may seem at times), the importance of co-ordinating the research of several disciplines is a factor that surely must be acknowledged and acted upon.

We appreciate the dedicated work of many scientists in the often successful attempts to develop viable alternative procedures. Likewise, we recognize that there remain fields where no such systems can at present be visualized as having viability. Nevertheless, the evidence available indicates there remain many other fields where these systems might well be developed, if only more concentrated and better co-ordinated resources were devoted to the perfecting of techniques (admittedly, often exceedingly difficult and complex) in this category.—I am, etc.,

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Effects of Entonox on Respiratory Function

SIR,—Entonox is a self-administered analgesic used in childbirth, consisting of equal volumes of nitrous oxide and oxygen.¹ It is also used in dentistry, ambulance work, and postoperative care. Previous investigations of the effects of such a mixture on pul-

monary function appear to have been restricted to measurement of the forced vital capacity capability of patients following upper abdominal surgery as an indication of pain relief (using 25% nitrous oxide, 75% oxygen).^{2,3}

We have investigated the bronchoactivity of Entonox in five adults with normal ventilatory indices by means of a whole-body plethysmograph. It is usual practice to express resistance of the airways as specific airways conductance, which is conductance divided by lung volume. After control values had been obtained the subject remained seated in the plethysmograph and inhaled from a standard B.O.C. Entonox apparatus (portable model) through the face mask provided. The exhalate was voided externally to avoid contamination of the plethysmograph. The subject took 10 deep breaths, after which the specific conductance was remeasured immediately.

The five subjects all showed a drop in specific conductance (range 19-31%; mean 26%). The subject with the largest change still showed a reduction of 13% after three minutes though the other subjects were almost back to normal within three to four minutes. The observer noted a more regular breathing pattern on the oscilloscope immediately following exposure.

Thus we feel that consideration might be given to using an alternative analgesic (for instance, methoxyflurane, which has a bronchodilator action in normal subjects⁴) for those patients presenting with airways obstruction or with a history of asthma.—We are, etc.,

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¹ *Lancet*, 1965, 1, 1229.

² Parbrook, G. D., and Kennedy, B. R., *British Medical Journal*, 1964, 2, 1303.

³ Parbrook, G. D., Rees, G. A. D., and Robertson, G. S., *British Medical Journal*, 1964, 2, 480.

⁴ Douglas, R. B., and Forsey, S. M. F., *British Medical Journal*, 1973, 4, 106.

Chlorodyne Dependence

SIR,—I was interested in the cases of chlorodyne dependence reported by Dr. R. R. Parker and his colleagues (9 March, p. 427) as I was involved with a similar problem which came to notice in an unusual manner.

In November 1970 a female infant was born by rapid spontaneous delivery to an unmarried mother who had attended the antenatal clinic only once late in the pregnancy. At the age of 48 hours the infant was reported to have vomited and to have become jittery and rigid, with a shrill cry. The blood sugar level at that stage was 72 mg/100 ml. Despite intramuscular phenobarbitone the baby's general state remained the same, with repeated short-lasting jerking movements, constant tongue protrusion, and sucking and grimacing movements.

On the fourth day the baby's father told the nursing staff that the mother had been taking two bottles of Collis Browne's compound a day for many months, having

started this on the advice of a chemist for "nervous diarrhoea". Bottles of the mixture were found in the mother's locker. The baby was put on chlorpromazine and gradually settled. The chlorpromazine was discontinued after a week. The mother was seen by a psychiatrist and follow-up arrangements made for both. The baby was seen once aged 5 months, when she appeared to be developing normally, but after this the mother failed to keep appointments.

The child was next heard of in November 1973, when her general practitioner informed us that her mother had continued to take Collis Browne's compound, had failed further psychiatric attendances, had deteriorated physically and mentally, and was suspected of administering the mixture to her daughter. Although this was not proved, the constant failure to co-operate with the G.P., psychiatrist, and social services led to the little girl being admitted to a foster home under a court order.—I am, etc.,

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Vaginal Cytology in Pregnancies Associated with Congenital Fetal Defects

SIR,—Vaginal cytology during pregnancy associated with a number of abnormal maternal conditions has been described.¹ However, studies of hormone-induced cytological changes in association with abnormalities of the conceptus have mainly been concerned with hydatidiform mole.^{2,3} During the course of an investigation at St. Mary's Hospital, Manchester, into the use of vaginal cytology in pregnancy^{4,5} a series of vaginal smears were taken from 340 patients throughout their antenatal period. The smears were all taken by the same person from the lateral vaginal wall with an Ayre's spatula. The examinations carried out have been described elsewhere.⁵

In addition to maternal abnormalities in several cases there were eight babies with congenital malformations. The mothers of four of these (one with Treacher-Collins syndrome, one with imperforate anus, one with pyloric stenosis, and one with hydrocephalus) had essentially normal vaginal smears during pregnancy.

Three babies were anencephalic. The cytological features are summarized in the table. The abnormalities in the vaginal smears appeared early in pregnancy, long before the clinical diagnosis of anencephaly was made, and were probably associated with the abnormally low levels of urinary oestriol excretion described in this condition.^{6,7} In Case 2 *Trichomonas vaginalis* was present in the initial smear but otherwise there was no evidence of inflammatory disease in the vagina in this group.

The eighth patient, a 40-year-old primigravida with a clinically normal pregnancy, had persistently high karyopyknotic indices, some in the mid-trimester reaching 40%. The degree of clumping and the bacterial flora were normal, but the eosinophilic indices were, as in the anencephalic group, at the upper limit of normal. There was no vaginal inflammation. She was delivered of a mongol baby. The placenta was normal in all the parameters examined.⁴ However, she