merely exchanging one form of death for another.—I am, etc.,

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¹ Pearce, M. L., and Dayton, S., *Lancet*, 1971, 1, 464.

SIR,—In your leading article on “Clofibrate in Ischaemic Heart Disease” (25 December, p. 765) mention is made of the numbers and percentages of patients in the clofibrate and placebo groups who “died suddenly from myocardial infarction.” This statement embraces both sudden deaths and fatal myocardial infarction. In the analysis of the results we deliberately distinguished sudden deaths from fatal myocardial infarcts and believe that this distinction may be important in such clinical trials.—We are, etc.,

H. A. DEWAR

Newcastle

M. F. OLIVER

Edinburgh

** We regret that the word “or” was inadvertently omitted from the sentence in question. It should have read “died suddenly or from myocardial infarction.”—ED., *B.M.J.*

Health Service or Sickness Service?

SIR,—While I agree with much of the wisdom that Dr. Eliot Slater propounds (18 December, p. 734) I must take him to task about babies born with spina bifida. The doctor does not have “the duty of keeping them alive” nor of ensuring their death. The real choice is not between life and death, but whether or no a human being should be allowed to develop the preventable complications of an already serious disability in a community which has established a National Health Service. The routine control of infection in the new born nursery has saved the lives of more handicapped babies than all the paediatric surgeons in the Kingdom. That there is an ethical problem cannot be disputed. Let us be quite sure that it is discussed on the right basis.—I am, etc.,

London W.1

ALFRED WHITE FRANKLIN

SIR,—The article by Dr. Eliot Slater (18 December, p. 734) was timely, forthright, and logical. His comments under the heading of “perinatal risks” are overdue and merit the serious consideration of us all, not the least the paediatricians.

What the public press refer to as “break-throughs” in their ever-glamourized reporting on hospitals’ life-prolonging procedures in infancy are all too frequently transformed into domestic disasters, when time has shown that the sequel is so often a life of dependency—on the mother, the family, and/or the State.

The problem resolves itself into deciding whether measures aimed at prolonging infant life—which often call for the most expensive deployment of medical personnel and the use of sophisticated equipment—are justified where the probability of the child attaining a fully independent existence are very slender by reason of extreme physical and/or mental disability. Today the amount of clinical and statistical data available are

sufficient to remove the decision from the realms of guesswork into an acceptable probability. We have all encountered the domestic human tragedies of a mother wearing herself out by the continuous and unremitting care given to her child who can never expect anything approaching a normal full life.

Priority in money and staff in our hospitals today is too frequently directed towards what Dr. Slater calls high prestige procedures—that is, haemodialysis units, organ transplants, paediatric resuscitation units, etc., all of which have a high news value.

I would suggest that priorities need reversing. Make the first priority attention to, research into, or where appropriate propaganda towards a reduction in morbidity from the common diseases affecting the age group 20–50 years—namely, lung cancer, coronary thrombosis, rheumatism (in a broad sense), and those psychiatric disorders that tend to fill a doctor’s waiting room.—I am, etc.,

Hyde, Cheshire

ALAN S. SIMPSON

Psychiatrists’ Attitudes to Abortion

SIR,—During 1967 the Society of Clinical Psychiatrists conducted a survey of its members’ attitudes to therapeutic abortion. The result of the first hundred replies was published in a letter to *B.M.J.*¹

During 1971, with an increased membership, an essentially similar questionnaire was sent out to Society members. A statistically significant shift in opinion towards a more permissive approach is clearly demonstrated in the replies of that 40% of the Society’s United Kingdom members who returned the completed questionnaire.

A choice of one of four opinions was sought as to the conditions under which termination of pregnancy was considered advisable:

(1) Free choice of abortion by the woman in the first trimester of pregnancy; (2) termination should be recommended if the woman’s health or life is seriously threatened, the decision to include appraisal of the whole social situation; (3) termination possible only if the woman’s health or life is seriously threatened—the medical viewpoint before present legislation; and (4) termination insupportable on any account at any stage.

Opinion	1967 (n = 100)	1971 (n = 140)
1.. ..	24%	59 42%
2.. ..	56%	69 49.25%
3.. ..	16%	10 7.25%
4.. ..	4%	2 1.50%
Totals ..	100%	100%

χ^2 11.824 d.f. 3 P < .01

The most conspicuous change over the four years, as identified from the replies of admittedly limited samples of senior psychiatrists, has been a 75% increase in the proportion of psychiatrists replying in favour of free choice by the woman during the first three months as to whether she wishes or not to carry on with her pregnancy.—I am, etc.,

J. C. LITTLE

Honorary Secretary,
Society of Clinical Psychiatrists
Dumfries, Scotland

¹ Howells, J. G., *British Medical Journal*, 1967, 2, 53.

Proprietary Drugs

SIR,—In a leading article (25 September, p. 724) you said: “the fact remains that a well-known firm’s brand name on a product is the best guarantee there is of its purity, efficacy, and potency.” The truth of this statement has been emphasized by the report in the *Prescribers’ Journal*¹ of 11 patients at University College Hospital who were well controlled on a British brand of cortisone, and who were then given other cortisone tablets, made in England from continental bulk materials. These second tablets were found to comply with pharmaceutical standards, including assay and disintegration time. Soon after the change of tablets, three patients went into Addisonian crisis. On being given the original tablets, they quickly began to improve. That this is no isolated example of “B.P. equivalent” drugs not being as good as proprietary ones is shown by the *Drug and Therapeutics Bulletin*.² Under the heading of “Non-equivalent preparations of the same drug” it quotes 10 drugs where B.P. equivalents failed to have an equivalent effect. Even where a “B.P. equivalent” drug is pharmacologically identical it may have a taste so unpleasant that patients fail to continue their treatment.

We are always encouraged to be scientific and so we should be able to repeat accurately our work. Some people are now trying to encourage us to prescribe B.P. drugs, but there are no such things in scientific terms. We have to give somebody’s brand of a preparation. It is therefore more specific and scientific to prescribe a drug by its branded name rather than by a B.P. name. It is only by the use of the branded name that we can be sure that our patients get the drugs we want them to have. If we do not use proprietary names we cannot accurately know what treatment they are having.

We must ask those who are trying to make us prescribe only in B.P. nomenclature to let us revert to the more scientific habit of prescribing our drugs accurately by name, and we must make sure that no-one is allowed to substitute a “B.P. equivalent” when we prescribe a specific drug.—I am, etc.,

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¹ Whittet, D., *Prescribers’ Journal*, 1971, 11, 48.
² *Drug and Therapeutics Bulletin*, 1971, 9, 65.

Fibrin and Cancer

SIR,—Your leading article “Fibrin and Cancer” (11 December, p. 641) mentions the various results found in animal experiments using anticoagulants. These results can probably be explained by considering anticoagulant action as separate from the direct effect on the cancer cell. Both plasmin and warfarin are cytotoxic.^{1,2} Warfarin must be given to a rabbit for five to seven days to inhibit the locomotion of transplanted cancer cells. This action is likely to be due to uncoupling of oxidative phosphorylation for recovery of cells occurs in four to five days if warfarin is stopped and vitamin K is given.³

Your reference to A. S. Ketcham and his colleagues’ work with warfarin mice, misprinted 1961, reminds me how long it took them to change from heparin to long-term

warfarin therapy. Between 1961 and 1967 we started our first trial of oral anti-coagulants in patients with advanced cancers and 29 patients out of 96 responded for one to five years.⁴

In 1967-8 it was not possible to organize a controlled clinical trial because of the small number of suitable patients available to us and the multiplicity of factors in each case. As an alternative, however, we used patients as their own controls by starting warfarin therapy when their disease became resistant to cytotoxic therapy. The results were presented at the annual meeting of the Irish Association for Cancer Research in December 1971 and are being published. Briefly, 30 cases of lymphoma and chronic leukaemia being maintained on a single cytotoxic agent—busulphan, chlorambucil, or cyclophosphamide—were given warfarin therapy to double the prothrombin time when their disease became resistant. The same cytotoxic agent was continued in each case. Response was measured by calculating the total dosage of cytotoxic agent, per three month periods, required to maintain the patient in remission before and after the addition of warfarin. Twenty-one cases responded and the three-monthly average requirement of cytotoxic agent for these patients decreased to 25% of the pre-warfarin level after three months.

From our experience with warfarin in the treatment of human cancer it would appear to be a useful agent for maintenance therapy. This supports the observation of Michaels, quoted in your leading article, that mortality from cancer is lowered by oral anticoagulants. We found that morbidity is also lowered and this is usually the first sign of response to therapy occurring seven to 14 days after the prothrombin time is doubled.—I am, etc.,

R. DOUGLAS THORNES

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¹ Thornes, R. D., and Martin, W. T., *Irish Journal of Medical Science*, 1961, 6, 487.

² Lisnell, A., and Mellgren, J., *Acta Pathologica et Microbiologica Scandinavica*, 1963, 57, 145.

³ Thornes, R. D., Edlow, D. W., and Wood, S., jr., *The Johns Hopkins Medical Journal*, 1968, 123, 305.

⁴ Thornes, R. D., *Journal of the Irish Medical Association*, 1969, 62, 426.

** The reference to A. S. Ketcham *et al.*, should have read: *American Journal of Roentgenology*, 1971, 3, 42.—Ed., B.M.J.

Hospital Advisory Service

SIR,—When the Hospital Advisory Service's courteous teams visited one mental hospital in 1971 I found much of their advice instructive, stimulating, and helpful. I have also had the pleasure of reading Dr. A. A. Baker's Annual Report for 1970.¹ Nevertheless, I strongly endorse Dr. J. L. Crammer's plea (18 December, p. 746) that they ought to improve their methods of analysing work loads and the staff needed to labour under them; they should publish their standards, and key statistics of the hospitals they visit. Inadequate staff ratios may delay the closure of mental hospitals, but the greater risk is that the standard of care afforded existing inpatients will fall below an acceptable level.

Wing and his colleagues² have already established in three mental hospitals that the progress of earlier years has begun to slip back, and they emphasize how difficult it is

to sustain social innovations. One explanation is that because of the present steadily increasing work load "the attention that doctors can give to long-stay patients, and the number of staff who can be spared for rehabilitation, must inevitably diminish."

A logical strategy, where general hospital units have yet to open, is to arrange the existing mental hospital (or each catchment division within it) in two sections: admission wards and other wards. Each admission unit would look after the total inpatient load of its catchment area (and in doing so would not bequeath patients to the other wards) until the general hospital department opens.

To do this staff ratios similar to those of the general hospital units are essential. A common ratio for junior medical staff is three doctors (senior house officers or registrars) for 60 beds serving a population of 120,000. For the remainder of the mental hospital it would be inequitable if 100 patients had fewer than one junior doctor to look after them, particularly when the aim is to rehabilitate and support in the community as many as feasible. The numbers of other staff also need careful and local evaluation. For example the nursing load provided by incontinent severely demented patients, a proportion of whom are cared for in mental hospitals, is extremely high.³

If I understand the role of the Hospital Advisory Service correctly one of their main tasks is to consider staff ratios within the hospitals visited. Yet there is a notable lack of facts about these in the Annual Report for 1970. The mental hospital already mentioned has 18 junior doctors (below senior registrar grade), but on the unexceptional staff ratios outlined it needs about 30. Yet the Hospital Advisory Service considered that this hospital was relatively well provided with non-consultant psychiatric staff. The Advisory Service's standard was not revealed. Should it be revised?—I am, etc.,

H. G. S. SERGEANT

London N.W.3

¹ Department of Health and Social Security. *National Health Service Hospital Advisory Service, Annual Report for 1969-70*, London, H.M.S.O., 1971.

² Wing, J. K., and Brown, G. W., *Institutionalism and Schizophrenia*, Cambridge University Press, 1970.

³ Hulten, A., Kerstell, J., Olsson, R., and Svanborg, A., *Scandinavian Journal of Rehabilitation Medicine*, 1969, 1, 117.

Immune Suppression, Gliomas, and Tuberculosis

SIR,—The possibility that some cancers may be due to defects in immunological surveillance has been widely discussed of late.¹ The theory states that many thousands of cell divisions occur each day within the human body and a number of faults in the mitotic process are inevitable as the copying or template mechanism for DNA replication must break down from time to time. Some of these "mutants" may well be "potential tumours," and it is suggested that the immune system recognizes and destroys them. Thus several potential cancers may occur in an individual every day but these are usually completely suppressed. It is envisaged, therefore, that the immune system has two main functions—its classically recognized role as a defence mechanism against external attack by foreign organisms, mediated usually by humoral antibody, and the problem of internal security, perhaps

effected by the cellular immune system. Within the complex ecology of the body involving massive cell populations the latter role of internal security may well be the more important.

Evidence for this hypothesis has been admirably documented and discussed by Burnet in his book on *Immunological Surveillance*. Two of the most important pieces of evidence are, firstly, that there is a high risk of malignancy, particularly intracerebral lymphomas, after immunosuppression in transplant recipients, and, secondly, that neoplasms are a common complication in certain immune deficiency states of genetic origin. It therefore occurred to us that it might be worthwhile looking into the medical histories of patients with cerebral gliomas to see whether there was an increased incidence of conditions likely to be associated with a deficiency in the immune system.

We accordingly determined the previous medical history of 26 patients with cerebral gliomas, and as a control series took 52 patients with other conditions. The positive finding related to the incidence of old tuberculosis as defined by the history and/or radiological changes. There were six cases of old tuberculosis among the 26 gliomas compared with 5 cases among the 52 controls. The difference is not statistically significant, but the finding that nearly a quarter of the patients with gliomas had a previous history of tuberculosis is certainly impressive. Also of interest is the fact that among the 26 gliomas there were two cases of diabetes (one also had tuberculosis), one of hepatic cirrhosis, and one of long-standing chronic chest infection—this last in a female. Thus a third of the patients with gliomas had a previous medical history containing a major disease which could conceivably be associated with some alteration in immune response.

This is a small and preliminary study and the results could well be due to chance, but, if confirmed in larger series, the results would carry some very interesting implications. They would provide further evidence that infection with tuberculosis is in large part governed by the resistance of the host, and that many patients with this disease may have occult immune deficiencies. They would also provide evidence in support of the theory that neoplasia is, at least in part, due to a deficiency in immunological surveillance.—We are, etc.,

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D. W. WARD
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¹ *Lancet*, 1971, 2, 143.

² Burnet, M., *Immunological Surveillance*. Oxford, Pergamon Press, 1970.

International Convention on Psychotropic Drugs

SIR,—The new international convention on psychotropic substances adopted at a United Nations conference in Vienna in 1971¹ raises some fundamental questions about the control of scientific research—particularly research that requires possession of controlled drugs for research purposes. Some of the clauses of the new convention are open to a wide range of administrative and legal interpretation.

One clause in the preamble reads: "Recognizing that the use of psychotropic