parison we recorded our results in the same way as Drs Lunn and Johnson.

Forty-four of the 60 persons with positive Mantoux tests had a positive and 10 a doubtful tine test. We even found 9% positive tine tests in Mantoux-negative patients, a fact we explain by the difficulty of strictly intracutaneous application of the Mantoux test in older persons with very thin skin. In our practice the tine test is an accurate and easily performed screening test for tuberculosis.

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¹ American Lung Association, *Diagnostic Standards and Classification of Tuberculosis and Other Mycobacterial Diseases*, supplement. New York, A.L.A., 1974.

Blood Products Laboratory

SIR,—May I correct the entry (Medical News, 7 October, p 1028) concerning my succession to Sir William Maycock as director of this laboratory? The Blood Products Laboratory is a central laboratory of the National Blood Transfusion Service, at present administered jointly by the North-west Thames Regional Health Authority on behalf of the DHSS. The Blood Products Laboratory is no longer associated with the Lister Institute of Preventive Medicine as stated.

The correction is important, because when the Lister Institute announced the closure of its operations many of the organisations which supplied the Blood Products Laboratory assumed, in view of our earlier association with the institute, that we had closed at the same time and restricted their business accordingly. This could have had extremely serious results on the process and was promptly rectified. In effect, the Blood Products Laboratory has always been a central laboratory of the Department of Health, the institute only acting as a legal employing authority.

R S LANE

Blood Products Laboratory,
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Perspectives in spina bifida

SIR,—Some of the facts concerning spina bifida are depressing, but your leading article (30 September, p 909) is full of gloom, without even a glimmer of hope. Initial forecasts may have been too optimistic, but your article is misleading because it suggests a prognosis that is over-pessimistic.

The operation to close the spinal defect is not necessarily "life saving," but a mortality rate of 25% would be unacceptable nowadays. The crucial decision is not whether the spinal defect should be closed, but whether or not the hydrocephalus should be treated.¹ It is difficult not to treat a thriving infant with hydrocephalus, even should the need arise several months following the predicted demise of the infant.

You state that the neurological condition is "potentially progressive," but it would be devious to suggest that major deterioration is to be expected in a large percentage of these children. Only a few adolescents develop major neurological problems in the upper limbs. Orthopaedic surgeons may have been over-

enthusiastic in their efforts to give all of these children an opportunity to "walk," but some paraplegic children can shuffle about sufficiently to keep themselves and their parents pleased. Who is to deny such a child this modicum of self-esteem? Many children with spina bifida are not paraplegic and these do not become "chairbound."

Children with high levels of paralysis usually develop spinal deformities, but not all of them have shunts, not all of those with shunts require "constant reintervention," and many do not have renal complications.² Not all of these children are incontinent of faeces or urine as implied.³

These children cause stresses at home, and the incidence of broken marriage among the parents is high, but is it statistically higher than among parents of children with other incurable, incapacitating diseases? Special schools have isolated the more severely handicapped children, but what percentage of the total with spina bifida are permanently resident in such schools?

A society which has accepted abortion so easily may progress to voluntary euthanasia, inducement to sterilisation, compulsory sterilisation, and eventually to "involuntary euthanasia." Such measures for a "final solution" are not without precedent, and no doubt we doctors would feel more comfortable without these deformed children to remind us of our inadequacies. However, I think that you have underestimated the dignity of many of these valiant children, and I am confident that at least one mother would agree.⁴

R J BRERETON

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¹ Robards, M F, Thomas, G G, and Rosenbloom, L, *British Medical Journal*, 1975, **4**, 12.

² Lister, J, Zachary, R B, and Brereton, R J, *Progress in Paediatric Surgery*, 1977, **10**, 161.

³ Brereton, R J, Zachary, R B, and Lister, J, *Archives of Disease in Childhood*, 1977, **52**, 703.

⁴ Smith, A M, *Nursing Mirror*, 1978, **147**, 10.

SIR,—The comment in your leading article (30 September, p 909) that "no one seems to have asked spina bifida sufferers themselves what value they put on their own lives" smacks of comfortable journalistic moral indignation not appropriate, I would have thought, for a scientific journal. What conclusions you would draw from any such studies remain unstated, and I cannot see that any interpretation would help greatly in what is essentially an ethical/religious problem. In addition the "morality" of asking these "sufferers" in effect whether they wish that they had been aborted seems questionable.

Surely the suffering of the world is not a constant to a medical mind; these patients by your own account suffer greatly and impose great suffering. In a society that accepts abortion for "social" reasons (rightly or wrongly) it seems to me unjustifiable not to offer freely the possibility of abortion to parents with a spina bifida fetus unhindered by such unhelpful comments as are expressed in your editorial.

J N THOMPSON

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Harrow, Middx

*.*The reason why a mother bearing a spina bifida fetus is offered abortion must have something to do with the suffering that this condition will cause a family and the sufferer.

The leader was an attempt to give a brief summary of the realistic picture from the point of view of the family. It merely commented that no data were available on the views of people with spina bifida on abortion as a method of preventing the birth of children with spina bifida. Parents faced with the problem of abortion under such circumstances might very properly, and logically, wish to know what people suffering from spina bifida felt about life under these circumstances. The fact that no one can at the moment tell them can hardly be a source of scientific or moral pride. Moreover, it might be said that someone who is unwilling to discover what a person feels about his own suffering before making life or death decisions is in danger of medical paternalism and spiritual pride, both of which rate below "comfortable journalistic moral indignation" in some people's scale of values.—ED, *BMJ*.

Poliomyelitis in developing countries

SIR,—In your leading article (23 September, p 845) you mention a level of crippling illness of 7 per 100 000 schoolchildren in Ghana. Most planners in Third World countries might at this level dismiss it as a disease of little importance. This, I believe, would be unfortunate. In the second of the references that you quote a figure in school-age children of 5.8 per 1000 is given. As in such a community severely paralysed children are unlikely to survive, a true level is more likely to be 7 per 1000. If this figure can be confirmed in other countries it will make in them an appropriate polio vaccine a priority.

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*.*The figure given in our leading article was an error of transcription for which we apologise. It should indeed have read 7 per 1000.—ED, *BMJ*.

Syringe-transmitted hepatitis

SIR,—Professor A J Zuckerman (14 October, p 1084) once again falls into the trap of incriminating intradermal BCG given by the school health service in the aetiology of hepatitis B because outbreaks of this disease have been associated with tattooing and acupuncture. There is, of course, all the world of difference between standards of hygiene in the school health service and some tattooists, etc. Indeed, Surrey district community physicians have long had a particular interest in infectious jaundice,¹ but remain unconvinced that a change from the present practice of administering BCG to 10 children—using individual needles and one syringe—is indicated.

The fact that it has been estimated that the economic burden of viral hepatitis in the USA in 1970 was more than \$650m is irrelevant to the financial problem previously posed (30 September, p 953). I shall now cease this public correspondence content in my mind that there is, at present, little scientific evidence to substantiate Professor Zuckerman's hypothesis that hepatitis B cases occur as a result of BCG vaccinations carried out by the school health service. No doubt the first recorded case will hit the headlines. Whether subclinical cases can occur without a fulmi-

nating one ever coming to light is doubtful. A collaborative study with Professor Zuckerman to test his hypothesis would be interesting.

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¹ Beynon, P, *et al*, *Communicable Disease Report* (Public Health Laboratory Service), 1977, 77, 36.

Drug treatment of psychiatric patients in general practice

SIR,—Dr Peter Tyrer (7 October, p 1008) seems to have stimulated a raw nerve with his article on the prescribing of psychotropic drugs in general practice—at least so far as Dr D C Hogg (28 October, p 1228) is concerned. This rather unfortunate response needs to be countered.

Having worked in general practice I know just what Dr Hogg means when he refers to "a horde of chronically depressed and anxious patients who are a constant source of work and difficulty." Surely, however, this can in no sense justify sloppy prescribing habits, which he appears to try to defend. Indeed, if it is the work generated by patients in mental distress to which Dr Hogg objects, then I am surprised that he has chosen to be a general practitioner. He uses shortage of consultation time as one reason for not adopting prescribing patterns which he knows may be the best: all the more reason, therefore, to use scant resources carefully. More time spent listening to patients' troubles often obviates the need to spend extra time with the prescription pad. Of course, in the first instance this usually means greater effort on the doctor's part, for there are no short cuts to good medicine. In the long run, however, it is time well spent. I also find the comment that "innumerable patients... are managed quite well" very telling. Dr Hogg has missed Dr Tyrer's point—namely, that many patients are manifestly *not* managed well by their family doctors so far as psychotropic medication is concerned.

Not that I entirely agree with some of Dr Tyrer's points. For example, his comments about the dosage levels of certain drugs—namely, prescription of neuroleptics being "inadequate" at a level of less than 75 mg chlorpromazine daily or "excessive" at 400 mg of chlorpromazine (or equivalent) daily—are of limited value in the absence of clinical data. I am sure that he is aware of pharmacokinetic studies^{1,2} indicating the great variations in plasma levels of such drugs in individuals on identical dosage regimens, together with considerable differences in clinical response. Furthermore, the picture is made much more complicated by polypharmacy,³ a feature stressed by Dr Tyrer. Added to this is the fact that up to 50% of patients prescribed antidepressants by their general practitioners default within a matter of three weeks or so.⁴ In short, there is no such thing as an "acceptable" dosage range for any psychotropic drug—it depends how it is handled (physically and metabolically) by the patient concerned. In the final analysis, therefore, administration is (or should be) dictated by the clinical response of the patient, not by absolute dosage schedules. Secondly, he suggests that some tricyclic antidepressants had "perhaps" been prescribed for too long. There is evidence⁵ to suggest that in certain cases long-term administration of tricyclics may have a worthwhile prophylactic

effect. Again, without clinical details it is difficult to evaluate Dr Tyrer's case.

Nevertheless, these points are generously outweighed by the remainder of the paper. Dr Hogg accuses Dr Tyrer of "GP baiting" and being destructively critical. This is nonsense: Dr Tyrer draws our attention to inadequacies in the dissemination of impartial information about psychotropic drugs and also to possible shortcomings in the pharmacological training of doctors. None of us is so good at his job that he doesn't need to be reminded to be judicious and vigilant. Dr Hogg's approach is a recipe for mediocrity: if we do not strive for excellence in prescribing habits we shall surely not attain it.

GRAHAM CLIFF

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King's Lynn, Norfolk

¹ Curry, S H, *Proceedings of the Royal Society of Medicine*, 1971, 64, 285.

² Sakalis, G, *et al*, *Clinical Pharmacology and Therapeutics*, 1972, 6, 931.

³ Braithwaite, R A, *et al*, *British Journal of Clinical Pharmacology*, 1976, suppl, p 29.

⁴ Johnson, D A W, *British Medical Journal*, 1973, 2, 18.

⁵ Mindham, R H S, *et al*, *Psychological Medicine*, 1973, 3, 5.

Menopausal therapy and endometrial pathology

SIR,—The letter of Professor I L Craft and colleagues (5 August, p 429) showing the protective effect of progestogens on the endometrium in postmenopausal women receiving oestrogen therapy is interesting, supporting our view that it is the duration of progestogen therapy which is important rather than the dose, or indeed the type of oestrogens.

We have updated the Dulwich Hospital's contribution to the paper by Sturdee and others (17 June, p 1575) and can now report the dose schedule and treatment of 34 patients with endometrial hyperplasia. These results are obtained from outpatient Vabra curettage specimens from a total of 530 patients receiving various regimens of hormone replacement therapy. It is not possible to give a precise incidence of individual hormone regimens because treatment was frequently modified during the course of therapy. However, at any time more than half our clinic patients have oestrogen implants and the remainder have oral therapy.

There were 29 patients with cystic glandular hyperplasia but only seven of these occurred in patients receiving any progestogen. Five patients had adenomatous hyperplasia and only one of these had been receiving progestogens. Twelve of the 16 cases which occurred with oestrogen implants were associated with failure to take the prescribed norethisterone each month. No hyperplasia has been found in more than 60 patients taking 5 mg norethisterone for 10 or 15 days with oral or implant oestrogen therapy. The endometrial pathology was reversed to normal by two courses of 15–21 days of 5 mg norethisterone in 28 of the 29 patients with cystic glandular hyperplasia and three of the five patients with adenomatous hyperplasia.

We have had no cases of endometrial carcinoma in patients whose hormone replacement therapy has been initiated and supervised from this clinic, but four cases have been referred from elsewhere. Three of the patients with adenocarcinoma had a history of from 10 to 30 years of continuous high-dose oestrogen ingestion with prolonged intermittent bleeding. The last patient had had

orthodox therapy of nine months of cyclical conjugated oestrogens (Premarin) followed by 50 mg oestrogen implants with five days of norethisterone. Hysterectomy following 15 mg of norethisterone for one month revealed moderate hyperplasia but no carcinoma in the specimen.

These four patients demonstrate well the problems of (a) making an accurate diagnosis of endometrial pathology in a patient taking oestrogen therapy; (b) self-medication without adequate supervision; and (c) unopposed oestrogen therapy without added progestogen, which has in our opinion been responsible for much of the anxiety concerning the apparent carcinogenicity of oestrogens.

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Paracetamol poisoning

SIR,—The letter from Dr A N Hamlyn (14 October, p 1089) commenting on our study of paracetamol overdose fatalities (16 September, p 832) concludes with the question of how 84 deaths before hospital admission can be excluded as being unlikely to have been caused by paracetamol.

As we observed, death due to paracetamol overdose is primarily caused by hepatic failure and occurs some days after ingestion of the overdose. The condition has an insidious onset and death does not occur rapidly but ensues after several days of progressive deterioration. We contend that in England and Wales it is unlikely that a patient with severe hepatic failure will not be admitted to hospital for medical attention. Of the 62 deaths directly attributable to an overdose of paracetamol alone, all but one occurred in hospital, the exception being a patient living alone who reported an overdose but did not receive medical attention and died at home.

The 84 patients referred to had taken at least one other substance in addition to paracetamol and had died at home or were dead on arrival at hospital. In 61 of these patients a fixed combination of paracetamol and the central analgesic dextropropoxyphene had been taken. An overdose of this combination causes early cardiorespiratory depression.¹

We therefore see no reason to modify our opinion that it was unlikely that paracetamol contributed to the deaths of these 84 patients.

J B SPOONER
J G HARVEY

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¹ Breckenridge, A, *Prescribers' Journal*, 1978, 18, 49.

Behçet's disease

SIR,—May I reply to the two interesting letters (30 September, p 952) provoked by your leading article (22 July, p 234) on Behçet's disease, in which reference was made to the studies of probands and their first-degree relatives which I have recently undertaken in Yorkshire¹? The form of the study owed much to previous family studies of the seronegative group of arthritides^{2–4} which had been suggested to form a genetically related group. This did not necessarily imply that all these diseases would subsequently all be found to be B27