at the Serbsky Institute of Forensic Psychiatry—at which visiting Western psychiatrists were manipulated in an effort to whitewash Soviet psychiatry (see pp 303-320)—to gain an impression of the devious tactics resorted to by the core group. No informed Western psychiatrists could possibly be sympathetic to this group. We should, however, make every effort to support those of our colleagues—the majority of Soviet psychiatrists—who find the political misuse of their profession as abhorrent as we do, and particularly the courageous few who are being punished for exercising their professional conscience.

We are also puzzled by Dr Hare's comments on the dissenters. His suggestion that they seek martyrdom and obsessively persist in their campaigns demeans the whole human rights movement in the USSR. All the dissenters we have interviewed were more than aware of the risks they were taking, including, ironically, the possibility of internment in a mental hospital. Certainly none of them relished this possibility!

We would entirely agree with Dr Hare's final comment that there is a need for British psychiatrists to look into their own backyard. However, ethical issues in medicine transcend geographical boundaries and a concern about our own practices and those of our colleagues elsewhere are not mutually exclusive.

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Science, race, and intelligence

SIR,—The question of race and intelligence is not as simple as would appear from your leading article (22 October, p 1040). It would be better were we able to research and discuss the problem in an emotionally neutral scientific environment, but nowadays this is not possible even in the scientific press. There are too many people eager to distort the facts for their own political ends. May I draw your attention to the following facts, which you appear not to have considered?

(1) The Black man in a predominantly White society sees himself in an uncertain, even overtly hostile, environment. As intelligence is commonly equated with worth it is not surprising that he reacts to the suggestion that he and his children may be innately less intelligent than their White neighbours.

(2) When race and intelligence are discussed the assumption always seems to be that there is a clear distinction between Blacks and Whites. Blacks are people who are seen to be not White. The mixed-raced child is classified as Black and accepted by both ethnic groups as such; thus White is equated with purity and Black with impurity.

(3) Leaving aside this philosophical consideration, the prejudices of many researchers lead them to lump together all the non-White people into the Black group. It is a matter of history and common observation that the Blacks of the West Indies and America are ethnically heterogeneous people. The genetic mixture of the West Indian Blacks is mainly of African, European, and Indian. It is possible to find an average intelligence and draw a normal distribution curve of intelligence in these people, but the meaning and application are doubtful. Even were it provable that our

Black population are less intelligent than the White majority this fact would be meaningless when faced with a child who could be "95% White" and yet still look very black.

It may be that we shall one day answer the question, but it behoves researchers to take heed of the feelings of the people they are investigating. Until we have an unequivocal answer uninformed speculation in public is mischievous. Kite-flying is a fine occupation for the scientist, but it is dangerous in stormy weather.

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Incidence and mortality of acute pancreatitis

SIR,—Your leading article (22 October, p 1043) gave valuable emphasis to the growing weight of evidence linking acute pancreatitis with a wide variety of drugs. However, I feel that the quoted incidence of 2500 people a year in Britain (± 46 cases per million) with 500 deaths (20% mortality rate) may present a somewhat pessimistic impression of the disease.

The incidence of acute pancreatitis probably reflects the thoroughness with which the disease is sought as well as the criteria which are accepted to define its existence. Using conventionally acceptable criteria for diagnosis such as a consistent clinical presentation and course, a serum amylase level of at least 1000 Somogyi units, or characteristic findings at laparotomy or post-mortem examination, 35 cases of acute pancreatitis were treated in this hospital during 1976. This is an approximate incidence of 110 cases per million of population. As there is no evidence to suggest that the disease is more common in Leeds than elsewhere, this would indicate that some 6000 cases per year might be expected in Great Britain. The mortality rate in this hospital, in common with several other centres in Great Britain, is at present less than 10%.

A simple mathematical calculation might lead to the conclusion that we are doubling the incidence by uncovering many mild cases and thus apparently achieving a halved mortality. Although this may be true to some extent I believe that a thorough screening of all acute admissions enables many cases to be diagnosed and treated early enough in the course of the disease to achieve a real reduction in mortality.

M J McMAHON

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SIR,—The purpose of focusing attention on the problem of iatrogenic pancreatitis, in particular that associated with certain drugs, was admirably achieved by your recent leading article (22 October, p 1043). However, the figures quoted in the introductory section regarding the incidence and mortality of acute pancreatitis warrant comment. The total number of patients suffering from acute pancreatitis was quoted as about 2500 per annum in Britain. This figure was presumably reached by simple arithmetic reckoning, assuming the Bristol¹ and Nottingham² incidence of 50 per million of the population to be representative for the whole country. It is not justifiable to present these figures as national statistics without clearly stating that an extrapolation

from regional surveys has been performed. There are very few areas of the country in which even short-term prospective statistics are available, but in one area where almost six years of continuous prospective analysis has been performed the incidence is at least twice the suggested national average.^{3,4}

The quoted mortality rate of 20% takes little cognisance of more recent evidence from prospective studies of over 630 patients, indicating a much lower mortality from acute pancreatitis.³⁻⁵ A study of 213 patients reported two years ago revealed a mortality rate of 11%³ and since that time the multi-centre MRC study has confirmed that figure with an identical mortality rate in 257 patients.⁵ One further study of 161 patients has reported an even lower mortality rate of under 9%.⁴ The time has now surely come to lay aside the concept of mortality rates in the region of 20% being part of acceptable clinical practice in this country.

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¹ Trapnell, J E, and Duncan, E H L, *British Medical Journal*, 1975, **1**, 179.

² Bourke, J B, *Lancet*, 1975, **2**, 967.

³ Imrie, C W, and Blumgart, L H, *Bulletin de la Société Internationale de Chirurgie*, 1975, **34**, 601.

⁴ Imrie, C W, et al, *British Society of Gastroenterology Meeting* (York), 1977.

⁵ Cox, A G, et al, *Lancet*, 1977, **2**, 632.

Role of the hospital in primary paediatric care

SIR,—Dr A J R Waterston (15 October, p 1021) has missed the main point of my lecture (24 September, p 787) when he suggests that I am "reverting to the 'disease palace' for the cure of all ills." This is the antithesis of my real concern to improve the standard of general practice in large urban areas with their declining living standards. I believe that in the immediate future this can be achieved only by the hospital going out into the community to give assistance and I have suggested a way by which this might be done. Dr Waterston has given undue emphasis to the "ill child presenting at the door," implying that this would be a new situation. As I emphasised, this already happens and is a considerable burden and responsibility for departments which have not the establishment to handle these problems. By creating a better relationship and communication with the general practitioners in these areas and with an intelligent use of child health visitors and liaison health visitors it will be easier and safer to return these children to their homes and also to implement a home nursing scheme, as mentioned in my lecture.

In no way did I propose that this would be a casualty-based service. It is clearly stated that the project would be based in the paediatric department and the children's section of the accident and emergency department of the hospital concerned, and I suggested that the pilot trial should be based on the three major children's units in Manchester (two of them being children's hospitals). Again, there is nothing to indicate that I am trying to get extra casualty sessions for general practitioners. I stated that "one or more of the practitioners on duty could be on call at the hospital, and this night and weekend duty could be shared by the general practitioners, child health practitioners who had agreed to

join the rota for experience, and the senior house officer on rotation."

Dr R J Brereton (15 October, p 1021) takes me to task for my careless sentence construction and asks for enlightenment. I had thought that it was obvious from the text that good antenatal supervision and intensive care after delivery, when necessary, contributed to an improved perinatal situation, whereas competent antenatal investigation would reduce the number of children born with disorders such as Down's syndrome and spina bifida.

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Names of drugs

SIR,—I would agree with your leading article (15 October, p 980) defending the non-use of proprietary drug names in medical articles.

Moving from British general practice to an academic job in the USA, I learnt the generic names, where I did not already know them, of all of the drugs I had become used to prescribing by proprietary names. Whether I had been right to do that is open to discussion, but patients and pharmacists (at least in my locality) found proprietary names often easier to use (and therefore less open to error, which is surely the main point). However, on arriving here, to my surprise I found that not only are the proprietary names different, as expected, but the approved names frequently are too (for example, paracetamol is acetaminophen), even with recently introduced drugs (for example, sodium cromoglycate is cromolin sodium).

Neither the *British National Formulary* nor the American Medical Association's *Drug Evaluations* gives these equivalents, and with increasing dissemination of medical personnel and literature I would say that a publication such as Dr G D J Ball suggests (15 October, page 1024), but listing the equivalent generic names as well as proprietary names of products still under patent or widely prescribed under their proprietary names, would be essential in any medical library that contains American as well as British publications (that is, most medical libraries). It would certainly have helped me.

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ECT and the media

SIR,—I have been interested by the comments concerning ECT and the media (22 October, p 1081, and 5 November, p 1215) as I am in the process of carrying out a study into the attitudes of patients to ECT and the effects of the media on these patients. My attention was drawn to this subject by a patient who refused to have ECT despite considerable morbid depression. He gave as his reasons the comments made on the television programme "Panorama." He had interpreted them as being an indictment of ECT. He was particularly worried that he would need repeated courses of ECT for many years and that his memory would be impaired, both being suggestions which came from the programme and both being very far from the truth, as several studies have shown.

I am sure that many psychiatrists have come across similar problems. The interesting point is, how much effect does this publicity, biased or not, have upon the people who are recommended for ECT? These people are almost all depressed by definition; they are therefore more likely to be hopeless and fearful about any treatment. They are also more likely to pick up, or misinterpret, hopeless messages about treatments for depression—"Nothing can really help me." It seems to me that if in fact the media are contributing to the worsening of a clinical condition (that is, depression), then they should rethink their policies.

I am pleased to report that the patient mentioned eventually, after several weeks of worsening depression, was persuaded to have a short course of ECT and is now happy, back at home, and grateful for the treatment.

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SIR,—Dr J F Anderson and his colleagues (22 October, p 1081) complain of the unfavourable "image" of psychiatry conveyed by the media. They argue that especially in respect of ECT the picture painted is not only biased but is likely to deter patients who would benefit from this treatment. They themselves consider that ECT is the most consistently effective treatment for depression and they cite instances of patients being persuaded against it by relatives who had seen television programmes.

I think that these consultants may perhaps overestimate both the extent to which their optimism is shared by former recipients of ECT and the unwillingness of people in severe depression to take risks. In the latter regard one could describe cases of persons improvising their own methods of shock treatment that they know perfectly well are dangerous. The motive, however, is not suicidal but rather an attempt at self-help when all other remedies have apparently failed. Also there were those among us who, before anaesthetics were introduced, asked for ECT. This was despite the fact that each such treatment was almost like going to the electric chair.

In the former regard there are those who, when psychiatrists talk about ECT working "wonderfully well," think about Lourdes. We have had ECT, antidepressant drugs, and group therapy, and none of them helped. We believe we eventually got well not through these techniques but through "natural causes" assisted by some supportive commonsense talk.

It is true that psychiatry is sometimes unfairly attacked, even hysterically. But psychiatrists will not advance their own cause if they ignore a simple fact. This is that the experience of so-called mental illness can rarely be publicly discussed by those who have actually undergone it. They fear, often justifiably, that careers and social reputations will be jeopardised if it is known that they have had treatment, especially as inpatients. These people have, in consequence, very little outlet for their frustration if dissatisfied with psychiatry.

Perhaps this is why there are so many campaigns that are, from the point of view of the Cupar consultants, irrational and even "malign."

FORMER PSYCHIATRIC PATIENT

Gamma-glutamyl transferase in ascitic fluid in primary hepatoma

SIR,—One wonders if the high levels of γ -glutamyl transferase in the ascitic fluid of patients with hepatoma, as reported by Dr T J Peters and others (18 June, p 1576), are not simply secondary to the high serum levels of the enzyme which undoubtedly were present.

The inclusion of this information in their report would have permitted a better appraisal of the potential usefulness of this test in distinguishing hepatoma from other hepatic diseases which also produce greatly elevated serum levels of γ -glutamyl transferase.

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* * * We sent a copy of this letter to Dr Peters and his colleagues, whose reply is printed below.—Ed, *BMJ*.

SIR,—In answer to Dr Noll the *serum* levels of γ -glutamyl transferase were equally elevated in the patients with hepatoma and those with cirrhosis without hepatoma. Several patients with cirrhosis had high serum levels of γ -glutamyl transferase but low levels of the enzyme in ascitic fluid. It is well recognised that assays of serum γ -glutamyl transferase are not helpful in distinguishing patients with hepatoma from those with other forms of liver disease. It is therefore unlikely that the levels of the enzyme in ascitic fluid merely reflect serum enzyme levels.

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Problems with IUCD tails

SIR,—The assessment of intrauterine contraceptive device (IUCD) usage has naturally been mainly concerned with method failure, leading to pregnancy, and with side effects such as bleeding and pain, leading to IUCD removal. Although Gräfenberg¹ felt that the presence of a cervical appendage led to the introduction of bacteria into the uterine cavity, its possible role in the production of serious infection did not achieve prominence until the controversy over the multifilamentous tail of the Dalkon Shield.² Since then it has been shown that all types of IUCD tail result in the continued presence of small numbers of vaginal bacteria in the uterine cavity.³

Oates⁴ has recently described a lesser-known problem. Long tails may become coiled and knotted. The regular use of vaginal tampons may lead to the adhesion of small particles of cotton wool to the knot and the formation of a small mass of foul-smelling material whose presence soon becomes apparent to the patient.

A more important problem is the disappearance of the Gravigard (Copper-7) tail with the device still in utero. The Gravigard is supplied partly loaded into the introducer with a loop of tail alongside it. If the tail is pulled down into the vagina after insertion and cut it is liable to withdraw into the uterine cavity and revert to the looped position it was accustomed to in the introducer. It can then no longer be seen on speculum