everything in their power to produce the above-mentioned unpleasant psychological states one could quite reasonably argue that alcoholism is indeed a political problem—at least in part.

With regard to physical dependence on alcohol, it is strange that this takes so long to develop. It is also strange that sudden withdrawal can be achieved in most alcoholic patients without any dramatic ill effects. Why does the alcoholic not demand alcohol once in hospital or make attempts to get some himself (the latter is easy in an open unit with frequent visiting). Again why do so many alcoholics relapse after weeks or months of total abstinence. This surely can have nothing to do with physical dependence on alcohol; it is much more likely to be due to the continued presence of the various factors which led to the patient getting into difficulty with alcohol in the first place.

W J STANLEY

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SIR,—Your leading article (10 February, p 361) opens by stating that the average per caput consumption of alcohol in Britain is higher now than it has ever been. This is not the case: the evidence is available in Spring and Buss's full historical account, in which they show that consumption was probably even higher during parts of the eighteenth and nineteenth centuries.¹ Professor R E Kendell (10 February, p 370) clearly knows that this is so.

J M FORRESTER

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¹ Spring, J A, and Buss, D H, *Nature*, 1977, **270**, 567.

SIR,—In his paper on alcoholism (10 February, p 367), Professor R E Kendell makes a number of refreshing and stimulating proposals. Many will agree with the suggestion that to consider alcoholism as a disease is now unhelpful and the problem is more political than medical. But perhaps he slips just a little into his own trap when he categorises the alcoholic as suffering simply from a condition of drug dependence. It may well be (as he says) that alcoholics differ little from other people. But that is hardly the point. Most of the patients seen at psychiatric outpatients differ little from other people except that they are suffering from, say, symptoms of anxiety and depression. I think it is up to me, as a psychiatrist, to try to understand what is happening to them in terms of individual conflict and stress in the context of each person's personality and relationships. Some patients can be helped by drugs and some (particularly those who are well motivated) by psychotherapy—individual, group, marital, and family. Is the alcoholic any different? Maybe the disease model has outlived its usefulness for many of our patients whose "illnesses" are often more usefully recognised as psychosocial in nature and derived from the way we all live in our cultural, political, and family systems.

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SIR,—A feature not mentioned in your leading article (10 February, p 361) on the Royal College of Psychiatrists' report on alcoholism is the alarming rise in morbidity and mortality from alcoholic liver disease in the UK.¹ In

Scotland alone the rise in morbidity from alcoholic cirrhosis has been threefold between 1968 and 1976.²

The so-called "drinks limits" in the report, in terms of daily alcohol ingestion, are as follows: beer (four pints per day) 60-140 g ethanol/day (depending on the strength of the beer); spirits (four double measures per day) 56-88 g ethanol/day (depending on the size of a single measure); and wine (one bottle) 60 g ethanol/day.³

Despite the benefits of moderate alcohol consumption in reducing the risk of ischaemic heart disease, recent epidemiological evidence suggests that risk of liver damage increases exponentially above daily consumption levels of 60 g ethanol/day in men, and 20 g in women.^{1, 2} This means that British beer drinkers in particular, although they may not become psychologically dependent on alcohol, could still damage their livers at the "target" consumption set out in our psychiatric colleagues' report, with all its attendant mass-media publicity.

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¹ Williams, R, and Davis, M, *Proceedings of the Royal Society of Medicine*, 1977, **70**, 333.

² Scottish Home and Health Department. Personal communication.

³ Mellor, C S, *British Medical Journal*, 1970, **3**, 703.

⁴ *Lancet*, 1978, **2**, 353.

⁵ Pequignot, G, *et al*, *Revue de l'Alcoolisme*, 1974, **20**, 191.

Kielland's forceps

SIR,—The paper on Kielland's forceps by Drs M L Chiswick and D K James (6 January, p 7) contributes little to obstetric knowledge, merely confirming that if misused these forceps are potentially lethal.

Kielland's forceps are not used as an alternative to spontaneous vertex delivery. They are used to expedite delivery in a variety of abnormal situations, and it will surprise no one that babies delivered in such abnormal circumstances compare unfavourably with those delivered normally. The alternative to the use of Kielland's forceps is some other form of intervention—non-rotational forceps, ventouse, manual rotation, or caesarean section—and it is with these alternatives that a more meaningful comparison may be made. A comparison of Kielland's forceps with other mediocrity forceps in Aberdeen¹ is briefly summarised in the table printed below. There were no significant differences in babies delivered by these two methods, and there were no deaths associated with trauma in either group.

An audit of 213 perinatal deaths in 10 539 deliveries over a four-year period (1974-7) in Brisbane² included four deaths associated with trauma. None of these was associated with any of the 384 Kielland's forceps deliveries in the survey. Adding together the figures from these two audits there were no traumatic perinatal deaths associated with 510 Kielland's deliveries. No experienced

obstetrician would deny that there are sometimes problems associated with the use of Kielland's forceps. However, the horrendous mortality stated by Chiswick and James, three deaths from tentorial tears among 86 babies delivered by Kielland's forceps, is reminiscent of an era which in most obstetric hospitals has long since passed.

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¹ Keeping, J D, Northern Obstetrical and Gynaecological Society, Perth (Scotland), October 1974.

² Chang, A, *et al*, submitted for publication.

SIR,—The excellent papers by Drs Malcolm L Chiswick and David K James (6 January, pp 7 and 10) have opened a much needed discussion on Kielland's forceps delivery. We previously recorded the same higher incidence of neonatal jaundice¹ but found it not to be associated with significant external bruising. Their finding of slightly higher morbidity with induced labour was similar to ours. The precipitation of delivery of a fetus which has not activated all its required adaptive parturition mechanisms will explain its unprepared state for delivery and postnatal life. The conjugation of bilirubin is but one of many perinatal biochemical changes, most of which we do not fully understand. The presence of fetal asphyxia and the excessive force used in delivery will further slow adaptation as well as having their own independent effects on the fetus. It seems logical to a paediatrician that a fetus already poorly prepared for life and in distress should not be exposed to more trauma than necessary. To some doctors the achievement of the almost impossible may become enough of a challenge to cloud judgment of what is best for both mother and child.

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¹ Eden, O B, Adjei, S, and Revolta, A, *British Medical Journal*, 1974, **3**, 573.

SIR,—The papers on Kielland's forceps delivery by Drs Malcolm L Chiswick and David K James (6 January, p 7 and p 10) and the subsequent correspondence (27 January, p 266 and 10 February, p 408) invite comment and criticism. There are several errors of assumption and omission. Firstly the authors' criteria for diagnosing fetal asphyxia are inadequate: an abnormal cardiotocograph trace without a fetal scalp pH measurement is not diagnostic of asphyxia. Fetal heart rate abnormalities may give a false impression of acidosis in up to 75% of cases.¹ This impression will often precipitate the doctor to interfere unnecessarily.² Statistically therefore one can say that in three-quarters of the cases associated with abnormal cardiotocography Kielland's forceps may have been used prematurely. Secondly, the statement that "some facial bruising from use of the Kielland's is probably

Mid-cavity forceps deliveries: Aberdeen 1972

	Maternal injury "	Mean Apgar score		Admission to neonatal nursery "
		1 min	5 min	
Kielland's forceps n = 126		20.6	7.3	9.1
Other mid-cavity forceps n = 149		22.1	7.5	9.0
				18.8

acceptable" must be rejected. Any method of delivery that increases perinatal morbidity is open to challenge, particularly when that method is directly responsible for a perinatal mortality rate of 35 per 1000.

In their second paper Drs Chiswick and James ignore the role of the active management of labour in avoiding the type of complications leading up to Kielland's delivery. Several papers have been published which conclusively demonstrate that these problems are avoidable.³⁻⁵

None of the correspondents appear to recognise the essential difference between intracranial haemorrhage due to trauma and that due to asphyxia. Tentorial tears, which cause subdural haemorrhage, have been shown to be 11 times more common when forceps have been applied, with or without intrapartum hypoxia,⁶ and virtually all cases of gross intracranial haemorrhage occurring during labour are due to trauma.⁷ In contrast, asphyxial intracranial haemorrhage in the mature infant is characteristically sub-arachnoid and petechial.

Finally, in the broader context of the role of Kielland's forceps in modern obstetrics my experience convinces me that they have no place. This personal experience is based on exposure to about 12 000 deliveries where labour was actively managed and Kielland's forceps unavailable (at the National Maternity Hospital, Dublin). The papers from Drs Chiswick and James support this conviction.

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¹ Beard, R W, et al, *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1971, **78**, 865.

² Edington, P T, Sibanda, J, and Beard, R W, *British Medical Journal*, 1975, **3**, 341.

³ O'Driscoll, K, Jackson, R J A, and Gallagher, J T, *British Medical Journal*, 1969, **2**, 477.

⁴ O'Driscoll, K, Jackson, R J A, and Gallagher, J T, *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1970, **77**, 385.

⁵ O'Driscoll, K, and Stronge, J M, *Australian and New Zealand Journal of Obstetrics and Gynaecology*, 1975, **15**, 1.

⁶ Butler, N R, and Bonham, D G, *Perinatal Mortality*. Edinburgh, Livingstone, 1963.

⁷ Potter, E L, and Craig, J M, *Pathology of the Fetus and the Infant*. 3rd edn. London, Lloyd-Luke, 1976.

SIR,—We were interested to read Drs Malcolm L Chiswick and David K James's articles on Kielland's forceps (6 January, pp 7 and 10). Their study comprised a group of 86 cases; we have examined the notes of 75 cases of delivery by Kielland's forceps carried out in this unit in the last 24 months. None of our patients had epidurals. We found certain interesting differences in the results and perhaps in our conclusions.

We detected no difference in the delayed onset of respiration (allowing for epidural) or the subsequent finding of transient irritability in the child. However, we were rather surprised that in their study the incidence of injury (facial or brachial palsy or cephalohaematoma) was 15.1% of patients, whereas in our study the incidence was only 6.7%. There were no stillbirths or neonatal deaths in our study whereas there were three in theirs.

In view of the indications for using Kielland's forceps, the complications in our study are acceptable. It would be unfortunate if Kielland's forceps were to develop a bad reputation for the second time in their life. Carefully used with the correct indications, with adequate safeguards so that the obstetrician can change his mind and carry out

a caesarean section where necessary, these instruments remain of considerable value.

Perhaps in these litigious times we should be using trial forceps in theatre much more frequently than in the past.

MICHAEL BURKE
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Retinal haemorrhages in infancy

SIR,—Last year you reported an innocent cause of extensive retinal haemorrhages in infancy, after a 2-month-old baby had been discovered as a near-miss case of cot death (4 February 1978, p 281). The problem was to decide if these haemorrhages were caused by hypoxia or the trauma of resuscitation, as such lesions normally provide important evidence of child abuse.

Since then I have looked for retinal haemorrhages in cases of sudden unexpected death in infancy before the necropsy. Such haemorrhages are frequently visible, provided that the cornea has not become too opaque. In all the cases in which they have been observed subsequent examination has shown the typical distribution of petechiae in the thoracic organs. No evidence has been found of the rare viral encephalitis which can produce retinal haemorrhages.¹

This work was funded by the DHSS.

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¹ Holzel, A, and Tobin, J O'H, *Lancet*, 1967, **2**, 723.

Cryptorchidism: a renewed plea

SIR,—Most surgeons with a paediatric practice will agree with Mr M C Bishop and Mr R H Whitaker (10 February, p 407) that too many boys present late for orchidopexy and that our aim should be to place both testicles in the scrotum by the time a boy starts school. I would make two practical suggestions.

Firstly, the true undescended testicle will not be in the scrotum at birth so a careful postnatal examination will identify all children requiring supervision. Just as discovery of a clicking hip leads to an appointment with the orthopaedic surgeon, so discovery of a testicle outwith the scrotum should lead to an appointment with the surgeon who would, in the course of time, be responsible for orchidopexy should it be required. The importance of the finding should be explained to the parents and an appointment given to them for the surgical clinic in six to nine months' time. This policy would not result in any major increase of work load in outpatients and should lead to a sharp improvement in quality of care.

The postnatal examination is an opportunity not to be missed because the next time that all children are examined is at school, in their 6th year, and it is clear, as Mr Bishop and Mr Whitaker point out, that all boys with true maldescent are not then directed to a surgeon. Only 21% of our patients have an orchidopexy before 5 years of age, and 32% have the operation between 5 and 7. This still leaves 47% who have their orchidopexy between their 7th and 13th birthdays, which is much too late. There seems to be a case for a specific

examination of the time and method of referral of boys in whom maldescent is found at the school examination.

Secondly, operative technique can make a contribution to earlier operation. It is not easy by the standard Bevan approach to mobilise an impalpable incompletely descended testicle from around the deep abdominal ring of a boy of 2 or 3 years and place it in a good position in the scrotum, so there is a natural tendency to defer this operation until the boy is older. Using the abdominal extraperitoneal approach, however, which has recently been described,¹ the separation of the hernial sac from the testicular vessels and the vas becomes comparatively easy, and a greater length of the testicular vessels can readily be freed. Thus we have operated on boys aged 4, 5, 8, 10, 12, and 16 weeks with troublesome inguinal herniae as well as maldescent, and the operation proved to be simple and was successful both immediately and on review at one year. I believe this approach can make a useful contribution to earlier operation.

PETER F JONES

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¹ Jones, P F, and Bagley, F H, *British Journal of Surgery*, 1979, **66**, 14.

SIR,—May I strongly support the plea which Mr M C Bishop and Mr R H Whitaker make (10 February, p 407)? There is no evidence that any significant descent occurs after about 3 months of age in the full-term baby boy. The diagnosis of the deformity should be made in the first three months of life and is therefore within the province of the general practitioner and the paediatrician.

Parents are usually anxious to co-operate and if an operation to bring the testis into the scrotum is not done between the 3rd and 6th months (possible in some 30% of cases) a yearly visit to the surgeon should be made. He can then plan to operate before the age of 5 years.

The importance of a recorded diagnosis made in infancy is that it eliminates the endless confusion of trying to make an accurate diagnosis in a boy aged 5 to 12 years, when spontaneous retraction of one or both testes is so common. If it is known that both testes were in the bottom of the scrotum at birth then the high testis in later years must be retracted even though the examiner's fingers cannot draw it down. It will "drop" as puberty approaches.

C G SCORER

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Guar crispbread in the diabetic diet

SIR,—Professor David J A Jenkins and others (23-30 December, p 1744) have again demonstrated reduction in glycosuria and a small but significant reduction in fasting blood sugar when guar gum was added to the diets of diabetic patients over a five-day period. The long-term effects of such supplementation are, however, less clear cut and, although a reduction in insulin dosage and glycosuria occurred over an eight-week period, there was no change in body weight and blood glucose concentrations were apparently not estimated.

In a study which we have just concluded (to be reported in full elsewhere) we studied the effect of adding either guar gum, bran, or