striking and highly significant improvement in the clinical outcome at three months was observed in the nimodipine limb. Among all 554 patients, 235 of whom underwent operation, a bleeding event led to nimodipine being stopped in only one patient. This patient suffered a colonic bleed of moderate severity, which the investigator reported as possibly being associated with nimodipine. The patient required a blood transfusion but recovered uneventfully.

"Bleeding" events were reported as an adverse event in two further patients, both of whom had received nimodipine. The first patient was reported to have a rash and haematoma on the back, which the investigator stated was almost certainly related to the use of ceftazidime. The second patient was reported as having thrombocytopenia and anaemia. This was observed 15 days after the treatment with nimodipine was completed, and the investigator reported that these adverse events were possibly related to atenolol, which the patient was receiving at the time.

Thus the data from the British aneurysm nimodipine trial give little support to the findings reported by Wagenknecht and colleagues, which are possibly artefacts, reflecting the authors' post hoc analyses.

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1 Wagenknecht LE, Furberg CD, Hammon JW, Legault C, Troost BT. Surgical bleeding: unexpected effect of a calcium antagonist. *BMJ* 1995;310:776-7. (25 March.)

2 Pickard JD, Murray GD, Illingworth R, Shaw MDM, Teasdale GM, Foy PM, et al. Effect of oral nimodipine on cerebral infarction and outcome after subarachnoid haemorrhage: British aneurysm nimodipine trial. *BMJ* 1989;298:636-42.

Conflict of interest: Gordon Murray gave statistical advice to the British aneurysm nimodipine trial, which was funded by Bayer, but has no other links with the company.

Authors' reply

EDITOR,—Much of the information provided in the responses to our paper seems to be anecdotal. To settle the question of whether calcium antagonists have a prohaemorrhagic effect a systematic collection of data on blood loss is essential, preferably in a randomised trial. This was a strength of our study. Further support for our observation is the more frequent use of the procoagulant aminocaproic acid (Amicar) in the group treated with nimodipine compared with the placebo group (56% v 36%, $P=0.01$).

Surgical bleeding may possibly be caused by an interaction between calcium antagonists and heparin or thrombolytic drugs, or both. The fact that only 13% of patients receiving nimodipine responded with excessive bleeding perhaps suggests that only some patients were susceptible.

A full report of the study and an extensive literature review are being prepared. Pre-existing blood disorders were found in both study groups, as would be expected in a randomised trial. Removing from the analysis the two patients mentioned in our report is a simplistic approach that breaches the integrity of the original randomisation. We still believe that our unexpected finding requires further exploration.

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Faulty blood bags

Reputation of English blood transfusion service has been unjustly damaged

EDITOR,—I wish to comment on recent reports of faults in some of the blood bags used by the English National Blood Authority.¹ Several of the points made have been misleading and may have given rise to inappropriate public and professional concern.

Firstly, international and published experience confirms that minor defects in plastic blood bags occur infrequently but regularly and that virtually all such defects are screened out during routine quality assurance programmes before blood is issued to hospitals from regional blood transfusion centres. Exceedingly rare (certainly with an incidence of less than one in a million blood transfusions), a defect remains undetected and causes bacterial contamination and consequential septicaemia in the recipient.

Secondly, the English National Blood Authority is to be congratulated for having introduced blood bags from a second supplier into England. This much needed break of the longstanding monopoly on the supply of blood bags must already have saved the NHS in England substantial amounts of money and considerably enhanced the security of quality systems. The competition thereby created must be generating savings approaching £1m a year, much of which will be coming from the original monopoly supplier.

Thirdly, the notion that the authority's second supplier of blood bags (Tuta, Australia) provides products of inferior quality is false. The Scottish National Blood Transfusion Service rejected the proposed monopoly by Baxter in 1966 and for almost 30 years has been purchasing blood bags from both Tuta and Baxter. We have used many millions of both blood bags and have no doubt that Tuta produces a high quality product that, in terms of blood safety, competes equally with the product from Baxter. Both manufacturers have had occasional problems with quality but have always responded expeditiously and in close collaboration with their customers. If doubts have arisen then both manufacturers have had no hesitation in replacing (at no cost to the NHS) any suspect batches of blood bags.

Finally, it is unusual, but not unique, for a product to be recalled from hospital blood banks. Of much greater concern, however, is the perception that this event was deliberately seized on to damage further the public image of the blood transfusion service in England; this is regrettable.

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1 Dyer O. Blood authority investigates faulty blood bags. *BMJ* 1995; 311:145. (15 July.)

Faulty bags were withdrawn from routine use

EDITOR,—Owen Dyer's report on the faulty blood bags withdrawn by the National Blood Authority may have misled readers.¹ He quotes our spokeswoman as saying that "only 5000 of the estimated 7000 Tuta bags had been disposed of immediately because some hospitals had no other kind." When the fault in the Tuta bags was first discovered it was thought that the leaking seals had occurred in only a limited number of batches. Initially 5000 bags were withdrawn. The remaining 2000 bags were withdrawn the next week after the manufacturers informed us that the fault was sporadic and not confined to certain batches. The advice issued to hospitals in the Oxford and Southampton areas, some of which had only Tuta bags in stock, was that when no alternative was available their use

"must be at individual clinical discretion and the blood pack must be carefully inspected before use." This clearly indicated that the bags should not be issued for routine use but should be used only in emergencies.

Dyer also alludes to the costs of the current structure of the National Blood Authority. Before the authority was established the 14 centres in England were the responsibility of the regional health authorities. Each centre had its own management team and senior medical staff. In addition there was a national directorate, based in Manchester, whose role was to coordinate the activities of the centres. If the proposals for reorganisation are approved the new management structure will consist of only three administration centres with senior management teams. The chief executive posts in other centres will disappear, and the result will be a more streamlined management structure, less bureaucracy, and lower administrative costs.

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1 Dyer O. Blood authority investigates faulty blood bags. *BMJ* 1995;311:145. (15 July.)

Screening for melanoma

EDITOR,—When Duncan Keeley states in his commentary to our paper that "feasibility studies . . . that fail to address these issues [workload, earlier diagnosis, and anxiety] should not be undertaken,"¹ he ignores appropriate study design. Ours was a validation study: validity must be proved before money is spent on feasibility studies. Furthermore, such pragmatic outcomes are not appropriate in validation studies because the intensive assessment may well affect patient behaviour.

Keeley suggests that workload would "substantially increase." However, opportunistic screening when patients attend for other reasons would minimise the workload of mole counts—for example, general practitioners could send instructions for self screening or give a leaflet or perform a mole count, or both, when checking blood pressure. Alternatively, community campaigns could advise self screened people at high risk to avoid being sunburnt. The workload would partly depend on the potential to induce anxiety, and here Keeley also automatically assumes "significant adverse consequences." This ignores evidence that reassuring messages, practical advice to minimise risk, and emphasising benefits can allay fears without stopping a change in behaviour (M Johnston, King's Fund Centre conference, London, November 1994). To assume that outcomes will be poor is unjustified for both anxiety and workload.

Targeting populations who have greater need is apparently "unacceptable," despite improving the efficiency of the intervention. Should community or population approaches never be targeted in higher risk areas, where melanoma is common, either?

Keeley's preferred research areas may suffer the limitations of screening. Population approaches could also increase anxiety and workload, and thus not necessarily be any more cost effective, particularly as a fifth of the population (patients with many moles or freckles) account for most cases. Improving doctors' skills will work only if patients attend with moles that are changing—that is, self assessment or screening would need to be encouraged.

The implication is that screening would have a high ratio of cost to benefit. It takes seconds to count moles (the median count for trunk or arm is 2-3), and with reassurance and advice screening could feasibly take 2-3 minutes. If every patient

was screened by a nurse (and possibly many fewer if patient self screening was feasible) this would cost £4000 in a practice of four partners, in which 8-16 melanomas would be diagnosed over 10 years. If one of these melanomas could be prevented or diagnosed earlier, and other costs minimised, 15-20 life years could be saved with a cost effectiveness comparable with screening for that of hypertension.

Although Keeley reinforces some valid points, he ignores suitable study design, automatically assumes pessimistic outcomes, and unnecessarily polarises the debate when research is at such an early stage.

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1 Little P, Keefe M, White J. Self screening for risk of melanoma: validity of self mole counting by patients in a single general practice. [Commentary by Keeley D.] *BMJ* 1995;310:912-6. (8 April.)

Place of tricyclics in depression of young people is not proved

EDITOR,—P Hazell and colleagues address an important issue in their meta-analysis of the efficacy of tricyclic antidepressants in child and adolescent depression.¹ Owing to deficiencies in both the material and analysis, however, it is vital that their results are taken to mean that the efficacy of such depressants is “not proved” rather than that they are ineffective in this situation.

Firstly, the authors do not discuss the diagnosis and severity of depression in the patients. In adult psychiatry “minor” and mild depression show less specific response to antidepressive drugs than more severe, “major” depression.² It seems very likely, in view of the high placebo response in the studies analysed, that this might be true of the child and adolescent patients studied. Heterogeneity in the patient groups would weaken the power to detect differences in responses to drugs and placebos. Better definition of patients may identify a group of patients for whom drug treatment is beneficial.

Secondly, the studies used a variety of rating scales to measure change. Hazell and colleagues do not discuss the validity of combining effect sizes from different measures; they assume that these measures are similar in their ability to detect change, which may not be warranted. Thirdly, the total number of patients from all studies combined is very small (76 and 85 in active and placebo groups respectively). Even in a single, well conducted study, 80 patients in each group gives less than the conventionally required power of 0.8 to detect an effect size of 0.35 at 5% significance.³ It is therefore not possible to exclude a difference in efficacy between drug and placebo with these small numbers. Related to this point is the magnitude of effect size that the authors consider to be clinically significant. Requiring a difference in effect size of 2 over placebo is wholly unrealistic, and few, if any, treatments could claim this. Cohen gives 0.5 as a medium effect size likely to be of practical significance and 0.8 as a large effect size.³ In a recent meta-analysis of the efficacy of selective serotonin reuptake inhibitors and tricyclic antidepressants in adults with major depression, the mean difference in effect size compared with placebo was 0.4—not very different from the 0.35 in this meta-analysis except the larger numbers

made the result highly significant. Also, the confidence interval in the meta-analysis by Hazell and colleagues (0.86) encompasses a large effect size.

While the authors may be correct that drugs should not be the first line of treatment for child and adolescent depression they cannot conclude this from their study. The conclusion that they should draw from this meta-analysis is that larger studies with careful definition of patients are needed to determine whether antidepressants are effective.

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- 1 Hazell P, O'Connell D, Heathcote D, Robertson J, Henry D. Efficacy of tricyclic drugs in treating child and adolescent depression: a meta-analysis. *BMJ* 1995;310:897-900. (8 April.)
- 2 Paykel ES, Hollyman JA, Freeling P, Sedgwick P. Predictors of therapeutic benefit from amitriptyline in mild depression: a general practice placebo-controlled trial. *J Affect Disord* 1988; 14:83-95.
- 3 Cohen J. *Statistical power analysis for the behavioral sciences*. Orlando, FL: Academic Press, 1977.
- 4 Anderson IM, Tomenson BM. The efficacy of selective serotonin reuptake inhibitors in depression: a meta-analysis of studies against tricyclic antidepressants. *J Psychopharmacol* 1994;8: 238-49.

Labour and birth in water

Safety has yet to be determined

EDITOR,—The recent survey by Fiona Alderdice and colleagues of labour and birth in water in England and Wales shows that once again a new form of intrapartum care has been introduced without adequate assessment.¹

The survey was based on data collected through a short postal questionnaire and telephone interviews without any validation. The retrospective nature of the survey led to a high incidence of “good” and “rough estimates.” Unfortunately, even the place of birth was not accurately documented and the number of patients delivered in water was estimated. The inclusion of the good estimates in the study results raises doubt about the accuracy of the results. The analysis of two unmatched groups (labour in birthing pools and giving birth in water) without discrimination adds to the difficulty in interpreting these results.

As most units restrict the use of water birth to women at low risk, the death of 12 babies is an unacceptably high incidence. Although, these are reported as not being directly attributed to the use of water by the respondents, it would have been helpful if this had been validated by the survey team.

The report of the Confidential Enquiry into Stillbirths and Deaths in Infancy included deaths associated with home water births in 1993,² and these were presumably included in the survey. The confidential enquiry commented on the “general lack of details in the description of where and what had occurred” in deaths associated with home water births. Perhaps this applies to all death reports in such cases?

The survey also reports 51 cases of morbidity in neonates but does not state whether these were attributed to water births.

Unfortunately the survey by Alderdice and colleagues, which has been quoted by some members of the press to emphasise the safety of water births based on the large number of mothers in the study, once again shows that safety has not yet been proved and will not be proved without a randomised controlled trial being performed.

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- 1 Alderdice F, Renfrew M, Marchant S, Ashurst H, Hughes P, Berridge G, et al. Labour and birth in England and Wales. *BMJ* 1995;310:837. (1 April.)
- 2 Confidential Enquiry into Stillbirths and Deaths in Infancy. *Annual Report for 1 January-31 December 1993*. Part 2. London: Department of Health, 1995.

Temperature of pool is important

EDITOR,—Fiona Alderdice and colleagues reported on the safety of labour and birth in water and recommended that information about such practices should be collected routinely as part of local audit.¹ We have audited 112 case records selected by hospital number sequence from those of the 353 women who either laboured or delivered in a birthing pool in our unit between January 1990 and December 1993.

The mean cervical dilatation on entry to the pool was 4.8 (SD 2.0) cm (range 1-10). The time spent in the pool ranged from 10 minutes to a total of 13 hours 25 minutes (mean 2 hours 28 minutes). Fifty one (46%) women delivered in the pool while 61 (54%) left permanently at a mean cervical dilatation of 6.5 (2.7) cm (1-10), before delivering a mean of 4 hours and 27 minutes (range 5 minutes to 21 hours) later with a vaginal delivery rate of 95%. The length of time spent in the pool did not influence whether the birth took place under water.

The table shows the reasons for leaving the pool. Four women left because fetal tachycardia was detected on intermittent auscultation with a portable waterproof Doppler ultrasound machine. A fifth woman was also noted to have fetal tachycardia but did not leave the pool. Before entry all had recorded oral temperatures of 36.8°C or less and a recorded baseline fetal heart rate between 120 and 150 beats/minute. While in the pool the baseline fetal heart rate increased to between 170 and 190 beats/minutes. In four cases this was associated with a recorded maternal oral temperature of between 37.5°C and 38.4°C; in one case, although maternal oral temperature was not recorded, the water temperature in the pool was noted to be >38°C. Four women immediately left the pool and subsequent monitoring by continuous cardiotocography confirmed baseline tachycardia with good baseline variability and no other adverse features apart from early decelerations in one case.

Reasons why 61 women left birthing pool before delivery

Reason for leaving birthing pool	No (%) of women
Need for alternative analgesia:	
TENS	1
Nitrous oxide/oxygen	9
Pethidine	8
Epidural	15
Poor progress in first stage of labour	5
Appearance of meconium stained liquor	3
Decelerations auscultated in first stage of labour	3
Fetal tachycardia auscultated in first stage of labour	4
To facilitate pushing in second stage of labour	5
Mother did not want to deliver in water	3
Fetal bradycardia in second stage of labour while pushing	2
Contractions worn off in pool	2
Raised blood pressure	1

TENS=Transcutaneous electrical nerve stimulation.

The baseline fetal heart rate then fell to normal over about one hour. The woman who did not leave the pool had cold water added and over the next hour the fetal heart rate and maternal temperature returned to normal. Within three hours all women were afebrile and had no further episodes of fever. They subsequently delivered vaginally with no further complications.

Fetal tachycardia associated with maternal fever or excessive water temperature was unexpected. In all cases, the tachycardia resolved on cooling. It is of concern because the fetus is 1°C warmer than