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[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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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

its mother so modest maternal fevers may lead to fetal temperatures approaching 40°C, which could be dangerous for the baby.[2] In addition, maternal raised body temperature leads to fetal tachycardia which may be misinterpreted as fetal distress, leading to intervention.[2] Unlike normal labour rooms (usually kept at 26-28°C), birthing pools are close to body temperature and can therefore prevent the normal loss of heat that is necessary to maintain maternal isothermia.

In our unit the protocol for use of the pool originally suggested keeping the water at 37-38°C. In the light of our audit, we now recommend maintaining the temperature at 36-37°C, with hourly monitoring. Prospective research is urgently needed to determine the optimum water temperature for safe use of the birthing pool. This is particularly important in the light of the surprising length of time some women spend in the pool.

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Treatment in the air

Patient might not have survived return to airport

EDITOR,—D R Farquhar-Thomson and T Skinner question aspects of the management of the tension pneumothorax that occurred on a flight from Hong Kong, when Professor Wallace and Dr Wong used equipment from the British Airways medical kit, together with some ingenious improvisation.[1] They query the decision not to land the aircraft or to decrease its altitude from 10 000 m. At cruising altitude the cabin altitude is maintained by the pressurisation system at the order of 1830 m. Further reduction in cabin altitude is limited by structural considerations: to achieve a cabin pressure equivalent to the pressure at sea level it would be necessary for the aircraft to descend to 3000 m. This was not possible at the time of the incident because of high ground and because of constraints imposed by air traffic control, but it would have increased the ambient air pressure by only 20%, which would have been of little significance in a tension pneumothorax. A return to Hong Kong was considered, but the passenger is unlikely to have survived the time that this would have taken.

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Strategy of descent is not without risk

EDITOR,—Some practical issues need to be considered in the debate on how best to treat a tension pneumothorax occurring during an aircraft flight.[1] The altitude at which the pneumothorax occurred is not stated. Although the aircraft was cruising at 10 000 m, D R Farquhar-Thomson and T Skinner fail to note that the cabin pressure there is the equivalent of the pressure at only 1500 m above sea level. Increasing the cabin pressure to that at sea level would reduce a static pneumothorax by only 20%. The aircraft might have had to descend from 10 000 m to achieve this, thus increasing the risk of encountering adverse weather conditions and navigation hazards. Re-expansion of a punctured lung often increases the leak, thus eliminating the beneficial effects of the change in pressure. Aircraft travelling from Hong Kong to London fly over the Himalayas. At the time that the incident occurred it might not have been possible to descend to a safe altitude for repressurisation of the cabin to the pressure at sea level. After take off a large aircraft is heavily laden with fuel and above its maximum landing weight, so the aircraft would not have been able to land until fuel had been dumped. It might have taken a few hours to divert to the nearest airport, and the safety of the other passengers needs to be considered.

The limited information available makes it difficult to comment on the exact management in this case. I admire Professor Wallace and Dr Wong for the ingenuity they showed. Given the condition of the patient, I am sure that they and the captain of the aircraft acted in the best interests of the patient without unduly compromising the safety of the other passengers and crew.

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Non-steroidal anti-inflammatory drugs in elderly people

Treat mild chronic pain with paracetamol initially

EDITOR,—In their editorial on the use of anti-inflammatory drugs in elderly patients D N Bateman and J G Kennedy conclude that an alternative should be regular paracetamol or co-codamol.[1] Even at the low concentration of 8 mg codeine phosphate may produce important adverse drug reactions or undesirable effects such as anorexia, constipation, and drowsiness in elderly people. Paracetamol and co-codamol should not be considered analogous, and initial treatment for mild chronic pain should be paracetamol alone.

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Enteric coated aspirin may reduce risk

EDITOR,—D N Bateman and J G Kennedy fail to mention enteric coated aspirin in their editorial on non-steroidal anti-inflammatory drugs and elderly patients.[1] Evidence indicates that enteric coated aspirin is not accompanied by the high risk of adverse effects found with plain aspirin or the much more expensive anti-inflammatory drugs.[2] While some patients may obtain better relief from the newer anti-inflammatory drugs than from aspirin, a recent study has confirmed the effectiveness of enteric coated aspirin with little risk of gastrointestinal complications.[3] In the same issue of the *BMJ* as the editorial, John Weil and colleagues report that they found no risk of peptic ulcer bleeding with enteric coated aspirin.[4] Their three references to possible small bowel problems with aspirin failed to show any such problems with enteric coated aspirin, although the possibility exists.

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Gastrointestinal bleeding is common

EDITOR,—In their editorial on non-steroidal anti-inflammatory drugs and elderly patients D N Bateman and J G Kennedy erroneously quote an incidence of upper gastrointestinal bleeding of 210 cases per million people over 60 compared with 35 per million under 60,[1] from a study carried out in our department.[2] In fact, the annual incidences in our study were 10 times these—that is, 2100 and 350 per million people over 60 and under 60, respectively. Although these rates may show wide international variability, they emphasise the public health impact of gastrointestinal bleeding compared with that of other severe diseases of which analgesics and non-steroidal anti-inflammatory drugs are potential causes.

The annual incidence of end stage renal disease, another possible adverse effect of prolonged exposure to analgesics and non-steroidal anti-inflammatory drugs, is of the order of 100 per million people,[3] and, according to the data from two case-control studies, analgesic drugs account for 11-13% of all these cases.[4,5] The annual incidence of agranulocytosis (excluding patients receiving antineoplastic chemotherapy or radiotherapy) is 4·7 per million people, and that of aplastic anaemia (same criteria) is 2·0 per million.[6] According to a study carried out in the Netherlands, the incidence of severe anaphylaxis from all causes is 13·5 per million per year; drugs in general account for 42% of these cases, and analgesic and anti-inflammatory drugs in particular account for 26% of all cases.[7] Preliminary data from a study being carried out in Barcelona suggest that the annual incidence of severe non-viral hepatitis is 5-10 cases per million people.

At least a quarter of all episodes of upper gastrointestinal bleeding are attributable to non-steroidal anti-inflammatory drugs.[1] The figures we quote show that upper gastrointestinal bleeding is by far the most common potential adverse effect of analgesics and non-steroidal anti-inflammatory drugs. Therefore, the risk of gastrointestinal bleeding associated with each of these drugs should be one of the main factors determining which drug to use.

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