must allow for any discrepancies in gestational age between categories of interest to avoid reaching false conclusions related to neurological outcome. Secondly, it is increasingly difficult to survive at lower gestational ages, and use of neonatal intensive care resources escalates for more immature babies. Unfortunately, resources are not infinite; at some point the costs must outweigh the marginal gain in intact survival, particularly if resources are wasted to the detriment of potential survivors with a much better prognosis. We do not pretend to have the answer as to what lower gestational age represents the cut off point; more experience will help to define such a point if it exists. In any case, individual circumstances will always arise to make such a cut off point non-binding. What we do wish to indicate is that the neurological outcome for the most immature survivors, of 26 weeks' gestation or less, may be worse than that for more mature survivors of borderline viability, contrary to what has previously been reported.

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- 1 Kitchen W, Ford GW, Doyle LW, *et al*. Caesarian section or vaginal delivery at 24 to 28 weeks' gestation: comparison of survival and neonatal and two-year morbidity. *Obstet Gynecol* 1985;66:149-57.
- 2 Moses LE, Emerson JD, Hosseini H. Analysing data from ordered categories. *N Engl J Med* 1984;311:442-8.
- 3 Milligan JE, Shennan AT, Hoskins EM. Perinatal intensive care: where and how to draw the line. *Am J Obstet Gynecol* 1984;148:499-503.

Computer aided diagnosis of acute abdominal pain

SIR,—We read with interest the paper by Dr I D Adams and others (27 September, p 800) and the ensuing letters (18 October, p 1025; 15 November, p 1305). We work in an accident and emergency department using such a system and would like to comment on its advantages and disadvantages.

The computer and form filling do indeed improve the accuracy of diagnosis. For some inexplicable reason this leads to fewer inappropriate admissions and laparotomies. The end result is a saving in iatrogenic trauma for some patients as well as a large financial saving to the health authority.

Unfortunately for this system to work three things need to be done: the senior house officer has to fill in the form, type it into a computer, and wait for the result. On the surface these are simple tasks but they become major problems in the accident and emergency department. Firstly, there is the SHO's attitudes to the computer; these have been described by Dr P N Hall (p 1025). They look on it as either a "spy" in the department or, at best, a means to audit patients with abdominal pain. Indeed, it can be used for these purposes but its role as a teaching aid is usually lost on them. Consequently forms are not always filled in. The workload of the department further enhances the misuse of the computer. When it is busy the SHO is under pressure to get the patients referred or discharged, so he looks on typing as a waste of time. To try to alleviate this problem we employed a clerk to feed the information into the computer. When we analysed the results six months later we found that there had been no improvement at all in the SHO's diagnostic accuracy. By interfering with the SHO-computer interaction you seem to lose its advantages. Once the SHO knows somebody else is dealing with the form he loses interest. It becomes yet another test done in the department, the results of which will be sorted out by the surgical registrar or the admitting ward. For the SHO the most important thing is to reduce the waiting time.

As a training aid the computer certainly helps when it is correctly used. But when it is not properly used the SHOs quickly lose their improved diagnostic accuracy. If they carried on using the forms they

would not fall back to their baseline levels. To the SHO, however, form filling with no computer is the ultimate in time wasting. In our opinion the fall off in accuracy is due to the computer system being a "passive" system. The SHO does not really have to think to use the computer and so usually doesn't. Consequently when it is no longer available there is little they have learnt from it. A good way of "actively" teaching is by direct feedback from the surgical registrar who is called to see the patient. Unfortunately, in a busy department the registrar may arrive when the SHO is tied up with another patient or has finished his shift and gone home. Consequently the teaching becomes patchy. At least the computer, when used correctly, gives some feedback with each patient.

The advantages of the computer aided system are too important for it to be rejected. For it to be used effectively, however, it needs to have willing SHOs with time to use it. As a teaching aid it needs to be enhanced by a more "active" training programme—such as the SHOs' following up the patients or a regular test during their six month attachment.

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SIR,—I read with interest the comments of your correspondents (18 October, p 1025; 15 November, p 1305) concerning the trial of computer aided diagnosis of acute abdominal pain which we reported (27 September, p 800).

There seems to be general agreement that when presented with this aid to clinical decision making doctors' performances did improve significantly. This is what the system was designed to achieve. Moreover, there seems to be general agreement that this improvement was due not simply to the presence of a clever computer but to an educational package of which the computer was merely a part. Again, this restates the often expressed aim of the system's designers, as does the general agreement that the quality of data fed into the computer is vital.

The letter from Dr P N Hall contains interesting observations—"If it were a medical student [the computer] would surely fail its finals" (so presumably would an x ray or electrocardiographic machine, which one assumes Dr Hall uses from time to time)—and misunderstandings, possibly due to the fact that Dr Hall did not take part in the reported study but may have encountered an "interim" version of the system written after DHSS funding ceased.

Dr Graham C Sutton and Dr Jeremy Wyatt and colleagues raise a number of well taken points. In the *BMJ* article we chose to concentrate on pragmatic points which arose from the study, the explanatory phase being detailed in a more lengthy report to the DHSS, in which the relative contributions of feedback, data forms, and computer are analysed with much greater sophistication. The points raised by the Westminster group are addressed in a current crossover study of "artificial intelligence versus Bayes" to be reported shortly.

The comments from Professor H A F Dudley and colleagues seem rather odd, especially their advocacy of laparoscopy for "uncertain" cases (over 50% in many hospital series, implying several hundred laparoscopies per year). They quote a bad management error rate of 3.2% in their series; but in our own series the bad diagnostic error rate was 2.7% and the actual bad management error rate 0.9%, even in the baseline (during the trial it dropped to 0.2%). Although we can agree that the rate they quoted was not significantly worse (their numbers were small), it is arguable that faced with a choice between shoving one's data through a computer and shoving a laparoscope through one's abdomen most would opt for the former.

To those of us closely associated with the trial the main drawback of the system was that it took up time—a particularly valuable commodity in the

emergency department. This problem was accentuated by the use of old and slow computers, and since the trial ended much effort has been expended to make the current system faster and more attractive to the busy doctor.

Finally, Dr Sutton makes the point that the baseline results constitute a serious indictment of current postgraduate training. Indeed, many of the users of the system saw it as a postgraduate educational device rather than a surrogate consultant. This seems a reasonable summation, set alongside the perceptive comments of Dr Brooks and Dr Cramp and those of Dr Tabone-Vassallo. If the computer does no more than remind us of current inadequate performance levels and focus our attention on an "old fashioned" approach to medicine (where talking to patients and listening carefully to what they say are more important than rushing to a battery of expensive and time consuming investigations) then it will have been worth its weight (at least) in silicon.

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Prolonged pregnancy

SIR,—We read the paper by Miss Linda Cardozo and colleagues (25 October, p 1059) with interest and accolade for the scientific study by randomised trial of a common obstetric decision. The trial is good but does not support the conclusion that conservative management is less desirable than induction of labour for all prolonged pregnancies. The reason for this is twofold.

Firstly, their results pertain to a particular method of induction. Their findings of lower cord pHs and more frequent caesarean section in the actively managed group could be the result of early membrane rupture following the induction of labour. The effect of membrane rupture on the physiology of fetal homeostasis has been debated by the leading obstetricians of our time. Donald's concept of a "protective bag" around the baby, buffering and protecting the infant from the immense forces of uterine contractions, was supported by Caldero-Barcia, who suggested that membrane rupture leads to a decrease in the pH of the cord venous blood.¹ The early rupture of the membranes has been shown to cause an increase in fetal heart rate abnormalities on the cardiotocographic trace.^{2,5} This will in itself lead to an increase in the use of caesarean section.⁶ Further evidence that the loss of amniotic fluid may lead to decelerations in the fetal heart rate comes from the finding that deliberate infusion of saline into the cavity after active membrane rupture can abolish these abnormalities.

In our unit we are presently evaluating the effect of membrane rupture on all aspects of labour, in particular the effects on fetal heart rate decelerations and acid base state, by means of a randomised trial. Thus a plausible explanation for the adverse findings in the actively managed group is that labour was induced with membrane rupture and oxytocin infusion only three hours after the administration of prostaglandin E₂. Furthermore, prostaglandin E₂ and oxytocin have a synergistic effect and, together with membrane rupture, there is a definite risk of uterine hyperstimulation. This may account for the fact that, although the Bishop's score was significantly lower in the actively managed group than in the conservative group, the duration of labour was the same.

Our second objection pertains to the end points used. Fetal acidosis is a surrogate measure, and a very distant one at that, for fetal damage and death. The study does not show that neurological mor-

bidity or fetal or neonatal death was lower in the actively managed group, and one cannot help noticing, despite its statistical irrelevance, that one stillbirth occurred among the conservatively managed patients. To say induction of labour is the less desirable treatment would depend on showing either that this leads to increased fetal damage and death or that any reduction in these end points was so small that women would accept them in preference to the psychological disadvantages and risk of caesarean section with active management. A randomised trial to show any decrease in mortality from say 3 to 1 per 1000 will require 18 478 patients in each arm of the trial to have a 90% chance of showing a difference at the $p < 0.05$ significance level. Such a trial would be difficult to organise but is the only way in which a meaningful conclusion to this long discussed problem may be reached. This, of course, is one of the greatest dilemmas of modern obstetrics. The risks which we are called on to balance are so low that only an enormous study will show the safest method of management. Even this evidence, however is not a perfect guide, as many patients might consider a small risk worth while for other perceived advantages.

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- Caldero-Barcia R. Perinatal effects of early amniotomy during labour. *Modern perinatal medicine*. Chicago: Year Book Medical Publishers, 1981.
- Martell M. Blood acid-base balance at birth in neonates with early and late rupture of membranes. *J Pediatr* 1982;89:963-7.
- Andrews S. Effect of membrane rupture on fetal heart rate patterns in induced labour. *Br J Obstet Gynaecol* 1984;83:454-9.
- Caldero-Barcia R. Effects of the rupture of membranes on fetal heart rate patterns. *Int J Gynaecol Obstet* 1978;42:465-70.
- Smis B, Thiery M. Fetal heart rate patterns before and during amniotomy. *Eur J Obstet Gynaecol Reprod Biol* 1980;11:163-71.
- Leveno KJ, Cunningham SG, Nelson S, et al. A prospective comparison of selective and universal electronic fetal monitoring. *N Engl J Med* 1986;315:615-8.

Prevalence of HBsAg in UK population

SIR,—We feel we must again modify the King's College estimate of the expected prevalence of carriage of hepatitis B surface antigen (HBsAg) in the UK (11 October, p 956). Drs Graeme Alexander and Roger Williams refer to and extrapolate from some data on screening of women, regardless of social history or ethnic origin, carried out in The Netherlands. An overall prevalence of 0.8% was reported, with a higher prevalence in urban areas (about 2.0%), reflecting the uneven distribution of immigrants.

Our own experience in the West Midlands relates also to unselected screening of all antenatal patients since 1974. The West Midlands has a population of about 5 million with a high level of immigrants within the conurbation centred on Birmingham. The region does, however, also cover a large area of rural England. Using passive haemagglutination (Hepatest, Wellcome Laboratories) as a screening test, a figure of 1 in 850 (0.13%) has been reported.¹ The introduction of radioimmunoassay has allowed a greater sensitivity of HBsAg detection. Rather than calculate the prevalence of the carrier state simply on the results of screening tests, where bias may be introduced by mothers found to be HBsAg positive being more likely to have follow up blood samples rechecked, we calculated the prevalence on the basis of number of babies born to carrier mothers against the total number of babies born in the region. By this calculation we could also eliminate

those women who were later shown to have had a subclinical hepatitis B virus infection. The figures are shown in the table.

Year	Total No of babies born	No born to HBsAg carriers	Prevalence
1983	68 687	154	1/446 (0.22%)
1984	69 157	152	1/453 (0.22%)
1985	70 793	168	1/421 (0.24%)
Mean			1/440 (0.22%)

Any adjustments to these figures to account for infertility problems due to major liver disease must be minor. We accept that adult men more commonly become carriers after infection than women, but probably most of our immigrants become infected as children, when there is no sex bias in those becoming carriers.² Our figure of 0.22% could therefore be adjusted by a factor of 2—that is, to 0.44%—for men. This would result in an overall prevalence in the population of 0.33% (assuming roughly equal numbers of men and women), a figure which is still significantly lower than the King's liver unit estimate of 1.2%.

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- Derso A, Boxall EH, Tarlow MJ, Flewett TH. Transmission of HBsAg from mother to infant in four ethnic groups. *Br Med J* 1978;i:949-52.
- Wheeler SM, Boxall EH, Tarlow MJ. What happens to hepatitis B carrier children. *Br Med J* (in press).

SIR,—We still believe that the prediction by Drs G Alexander and R Williams (5 April, p 915; 11 October, p 956) of hepatitis B surface antigen (HBsAg) carriage rates in the United Kingdom is considerably overestimated at 1.2% because they have in part based their calculations on data collected outside the UK.

While the incidence of carriage in the antenatal populations of Holland¹ and Germany² might exceed 1%, even in London the figure has reached only 0.6%.^{3,4} Outside London the incidence among the antenatal population is probably much lower, for during 1981-5 we found the incidence in Oxfordshire and Northamptonshire to be only 0.14% (102 in 70 679), very similar to that in the West Midlands.⁵

Furthermore, the carriage rate among new blood donors in the Oxford region during 1973-84 was only 0.06% (152 in 253 790), which is close to the incidence for the whole of England and Wales. During this period, before those in high risk groups started to exclude themselves, 48% of the donors and 58% of carriers were men. Even given the relatively high male:female carrier ratio of 2.6:1 in USA donors,⁶ we find it hard to believe that the overall carriage rate in the United Kingdom would approach that predicted.

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- Mazel JA, Schalm SW, de Gast BC, et al. Passive-active immunisation of neonates of HBsAg positive carrier mothers: preliminary observations. *Br Med J* 1984;288:513-5.
- Rosendahl C, Kochen MM, Kretschmer R, Wegscheider K, Kaiser D. Avoidance of perinatal transmission of hepatitis B virus: is passive immunisation always necessary? *Lancet* 1983;i:1127-9.

- Roddick LG. Obstetric patients with serum hepatitis B surface antigen. *J Obstet Gynaecol* 1983;3:153-6.
- Barbara JA, Howell DR, Contreras M, et al. Indications for hepatitis B immunoglobulin for neonates of HBsAg carrier mothers. *Br Med J* 1984;289:880.
- Derso A, Boxall EH, Tarlow MJ, Flewett TH. Transmission of HBsAg from mother to infant in four ethnic groups. *Br Med J* 1978;i:949-52.
- Bastiaans MJS, Nath N, Dodd RY, Barker LF. Hepatitis associated markers in the American Red Cross volunteer blood donor population. *Vox Sang* 1982;42:203-10.

Improvement in bronchial hyperresponsiveness in patients with moderate asthma after treatment with a hypnotic technique

SIR,—The findings and conclusions of Drs Timothy C Ewer and Donald E Stewart (1 November, p 1129) are open to question.

The demonstration of the efficacy of the hypnotic technique is based on reduction of bronchial hyperreactivity to methacholine, improvement in symptoms, decrease in use of bronchodilator therapy, and improvement in pulmonary function testing in a group of asthmatics susceptible to hypnosis when compared with controls who were not hypnotised and asthmatics with low susceptibility to hypnosis.

Examination of the data shows that the groups were not well matched. In particular, the high susceptibility group had more severe asthma as shown by peak expiratory flow rate (371 l/min v 434 l/min in one of the control groups, $p < 0.001$, unpaired t test), greater bronchial hyperreactivity, and more treatment (46 puffs/day v 23.5 puffs/day in controls, $p < 0.001$, unpaired t test).

The methacholine challenge test data show an improvement of 75% but in reality the PC₂₀ went from 9.09 to 15.9 µg/l. As the concentrations are doubled on successive challenges this change is likely to represent only one dilution in some patients and no change in others, findings which could occur on a weekly basis in any group of asthmatics. It would be helpful to see the individual challenge data as well as the normal and asthmatic ranges using this particular technique. Pulmonary function in the treatment group showed an improvement; in the case of forced vital capacity this measured only 29 ml, and a 23 ml increase was noted in controls. The peak expiratory flow rate improved but started off at a lower level in the treatment group. The forced expiratory volume in one second, which is more likely to be independent of patient effort, did not change.

The use of bronchodilator therapy fell in the treated group from 46 to 34 puffs/day, but this was still greater than that in the control groups. The drop may simply reflect the fact that the level of therapy was higher in the treated group in the first place.

The highest level of significance was observed in the symptom scores ($p < 0.001$ for improvement in activity), but, as the authors appreciate, such data do not represent objective evidence but may reflect the patients' attitude to symptoms rather than the asthma itself.

While there may be a place for hypnosis in asthma, particularly in improving patients' symptoms, it would be wrong to assume that on the evidence provided in this paper there is improvement of objective measures of the asthmatic state.

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