based on the CAPD technique survival of 41% at three years (remarkably similar to that reported by the Newcastle group in the same issue (p 938)). This means that only a minority of patients can expect to have CAPD on a long term basis, which is quite different from the general experience of haemodialysis. Because a failed trial of CAPD carried a risk to survival we asked ourselves whether there were any patients in whom it might be wise to avoid it altogether. Our answer was yes, when transplantation was contraindicated (so that dialysis would be necessary indefinitely) and circumstances were such as to make home haemodialysis convenient. In many patients, particularly those over age 65, home haemodialysis might not be possible and CAPD would clearly be the treatment of choice. However, on first principles those who are both elderly and frail seem most likely to suffer if CAPD is unsuccessful and should perhaps be managed by hospital haemodialysis from the beginning. There is no contradiction with our statement that we can, in many cases, treat elderly patients as well by haemodialysis as by CAPD.

Peritonitis is certainly a major limiting factor in CAPD but Dr Oreopoulos is not right to infer that the rate is particularly high in Nottingham. He could compare the 17 of 33 patients who moved to haemodialysis because of peritonitis with the 44 of 74 who moved for the same reason in Newcastle. Like Dr Oreopoulos, we could quote statistics to show that our current success rate is improving, but we were reporting what we have actually achieved so far rather than what we hope to achieve in the future. We have one CAPD nurse to 13 patients, the peritonitis rate is about one episode per 10 patient months, and, as we emphasised in our paper, the quality of life which the treatment provides is good.

Of course, attention to detail and training are important to get the best results from CAPD and we hope that Dr Oreopoulos is not implying that we think otherwise. It is also important to tailor renal replacement therapy to the individual patient's needs, recognising that no treatment is likely to be inherently better or worse than another in all circumstances.

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SIR,—While agreeing in broad terms with Drs A G Morgan and R P Burden (11 October, p 935) about the overall usefulness of continuous ambulatory peritoneal dialysis (CAPD), review of our own programme does not confirm that unsuccessful CAPD *per se* increases the risk of death.

Since the inception of the CAPD programme our policy has been one of negative selection. All the patients had initially been considered unsuitable for haemodialysis or had experienced difficulties with haemodialysis. This group is therefore at high risk, consisting as it does of older patients often with frequent multisystem disorders. It could be argued therefore that CAPD was made to work and was abandoned only as a last resort, any additional mortality being a consequence thereof.

Yet of over 150 patients starting CAPD, only 20 have transferred to haemodialysis, of whom five have died. One patient succumbed to primary oxalosis after six months, one to disseminated herpes simplex infection after subsequent transplantation (19 months later), and three to cardiovascular catastrophe (after intervals of 6, 6, and 9 months). This latter group all had long term severe

hypertension (mean age 62). We have not seen any deaths related to sepsis, intraperitoneal or otherwise.

Most of the transfers were the consequence of infective problems during CAPD: seven were the result of specific episodes of peritonitis (4 pseudomonas, 1 *Staphylococcus aureus*, 1 candida, and 1 bowel perforation) and five the result of recurrent peritonitis (mean 6 episodes, range 4-9). Five patients were deemed unable to cope after a mean of 5.4 months (range 2-10) and the remaining three were transferred after postsurgical complications unrelated directly to CAPD (pretransplant ulcer surgery, bilateral nephrectomy, and bleeding polycystic kidneys).

In addition, the dropouts from CAPD have placed minimal extra burden on the haemodialysis programme. Of the transfers, seven received transplants, three were converted to home haemodialysis, and 10 required in centre dialysis.

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SIR,—We read with interest the "post honeymoon" experience of Dr A Heaton and others (11 October, p 938) with continuous ambulatory peritoneal dialysis (CAPD) in Newcastle upon Tyne. While we accept that disillusionment may creep into any marriage, we must question "the evidence that survival of patients receiving CAPD may be reduced compared with that of those receiving haemodialysis."

To date studies comparing survival on CAPD and haemodialysis using data derived from renal programmes in which both modalities have been used freely throughout all patient strata have almost universally shown a similar survival pattern on both treatments.^{1,2} On the other hand, studies using data derived from programmes in which CAPD is principally reserved for the elderly and less fit have generally shown inferior survival prospects on CAPD. This is not unexpected and is likely to be due to the selection bias. Most studies which have attempted partially to correct for selection bias, using stratification or case pairing, have shown equivalent life expectancies on either treatment.³ However, one or two have suggested a slight trend against CAPD.⁴

Our experience (as presented to the European Dialysis and Transplant Association, 1986) confirms that raw survival data are likely to show inferior survival prospects on CAPD. However, having comprehensively adjusted for selection bias using the proportional hazards method of Cox⁵ we showed that CAPD offered a slightly better life expectancy than either haemodialysis or transplantation (though this superiority was far from being statistically significant).

The bulk of recent evidence suggests that CAPD and haemodialysis offer a very similar life expectancy (although CAPD data are currently limited to five years of follow up). We therefore do not consider the survival prospects of patients on CAPD to be a major reason for "marital disharmony" at the present time.

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Prognosis for infants born at 23 to 28 weeks' gestation

SIR,—Professor V Y H Yu and colleagues (8 November, p 1200) reported that "there was no significant difference in the incidence of impairment between the 23-25 week and 26-28 week groups," suggesting that impairment was unrelated to gestational age between 23 and 28 weeks. However, the rate of severe neurological impairment in the 23-25 week group was 31% (8/26), almost double that in the 26-28 week group of 17% (28/166). The authors would have concluded differently if the impairment rates had been compared between the 23-26 week and the 27-28 week groups, where there was a significantly higher rate of impairment in the more immature infants ($\chi^2=4.0$; $p<0.05$). However, this reanalysis is open to criticism because it is made post hoc.

Possibly the rate of impairment does increase with decreasing immaturity but the authors failed to find a significant difference because the sample size was too small, particularly in the 23-25 week group. A larger sample can be achieved by waiting for one centre to accumulate enough cases or by combining data from several centres. In our study of two year survivors born before 29 weeks¹ the criteria for severe neurological impairment were identical to those of Professor Yu and others. Thus we combined the two studies and reanalysed the relation between gestational age and severe neurological impairment (table). Rather than

Severe neurological impairment and disability related to gestational age (data combined from Yu et al and Kitchen et al¹)

Gestational age (weeks)	Total	No (%) impaired	No (%) disabled
23	2	1 (50)	1 (50)
24	15	2 (13)	1 (7)
25	17	5 (29)	3 (18)
26	56	14 (25)	11 (20)
27	91	13 (14)	9 (10)
28	122	16 (13)	8 (7)

collapsing the data into two gestational age groups, which may result in an arbitrary and insensitive analysis, we analysed the data by Mann-Whitney U test.² The rate of severe neurological impairment rose with decreasing gestational age between 23 and 28 weeks ($t=1.99$; $p<0.05$). We also classified our children as disabled or not following the criteria of Professor Yu and others. The rate of severe neurological disability also rose with decreasing gestational age (Mann-Whitney U test $t=2.38$; $p<0.05$).

In the only other study reporting long term outcome in relation to gestational age³ the outcome of survivors was defined on different criteria; in particular a mental developmental index below 80 was the criterion for mental retardation. In that study neurological impairment was unrelated to gestational age.

The observations of increasing rates of severe neurological impairment and disability with decreasing gestational age in infants of borderline viability need to be confirmed. If more studies reported long term outcome with respect to gestational age, as well as, or instead of, birth weight, such confirmation or negation could be rapidly forthcoming. Unfortunately, assessment of gestational age is not easy; weighing infants is simple and accurate.

If decreasing gestational age is related to adverse neurological outcome in infants of borderline viability it is important. Firstly, future studies

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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- 2 Moses LE, Emerson JD, Hosseini H. Analysing data from ordered categories. *N Engl J Med* 1984;311:442-8.
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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-