Letters

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Consultants at Alder Hey look to the future

Editor—During the past two years the issue of organ retention has placed Alder Hey Children's Hospital under a political and media spotlight. The chief medical officer's report on retained organs clearly identified longstanding practices that were widespread and well recognised by the medical profession.[1] The early revelation at the Bristol inquiry about heart collections, and subsequent discoveries in our own trust, led to an independent inquiry and intense media attention.[2] We are proud that all staff continued to provide the highest standards of care to the children and their parents while facing the difficult issues uncovered in the course of the inquiry. The trust has accepted the report of the independent inquiry and replied to its recommendations. Consultant members of our medical board have apologised for the distress caused to parents and families, and we take this opportunity to repeat our apology.

During the period of the inquiry we accepted that we would be in the eye of the storm over organ retention. We were prepared for a collective acceptance of the distress and discomfort that would result from the independent inquiry's report.[2] We recognised there were circumstances particular to Alder Hey but also trusted that matters would be seen in the wider national context established in the chief medical officer's report.[1] It was with great distress that we witnessed the indiscriminate referral of our colleagues to the General Medical Council by Professor Donaldson in an action that displayed poor judgment and political expediency. This action demonstrated a need to seek individuals to blame rather than acknowledge the culture that created the circumstances for "scandals" that were manifestly widespread and had been an accepted part of the system. We subsequently witnessed the demeaning nature of the GMC processes in its handling of our colleagues.

The GMC has deemed that there is no evidence to take any further proceedings against these colleagues, who continue to work alongside us. Without reference to the individuals involved in the proceedings and without any forewarning, the GMC recently chose to break with its usual policy of confidentiality concerning referrals to the preliminary proceedings committee by disclosing further information to a third party. This action serves only to magnify our colleagues' distress and perpetuate the view that this national issue be seen in terms of individual blame.

Throughout the deliberations of the committee we have remained publicly silent, not wishing to prejudice our colleagues' position in a highly politicised situation with a weak regulatory body. We have seen the extreme personal distress caused to highly esteemed colleagues, who have continued to work throughout this period with undiminished commitment, professionalism, and compassion for the children and families in their care. We wish to record publicly our thanks to them for maintaining such commitment to their clinical work throughout this period. We also thank colleagues across the country, from the royal colleges, and from the BMA for the personal and public support they have given to us and to them during this difficult time.

We wish to move on from the national issue of organ retention being played out in media cliché as the Alder Hey scandal and ask for the support of all the profession in this. We are deeply committed to building reconciliation with all who have been touched by the events of the past two years. At Alder Hey we wish to direct our energies into continuing to build and develop the clinical services that have made us one of the country's leading children's hospitals not only in fact but also in public esteem. We welcome the secretary of state's new concordat of cooperation with doctors. We look to his energy and support, together with the resources of the Department of Health, to help us push forward with the developments in clinical services that have been hindered over the past two years by issues surrounding organ retention.

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1 Department of Health. *Report of a census of organs and tissues retained by pathology services in England conducted in 2000 by the chief medical officer*. London: Stationery Office, 2001.
2 *Royal Liverpool Children's Inquiry. Report.* London: Stationery Office, 2001.

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One Bristol

Doctors were to blame, if not wholly to blame

Editor—Although I am sympathetic to much of the argument in Smith's editorial, I must challenge the use of the word scapegoats.[1] The key individuals in Bristol declined to face up to growing evidence and growing anxiety among their colleagues about their own standards of work. There are other examples, in pathology to name one, where subsequent review of failures in standards and governance could be attributed in part or in full to the unwillingness of senior doctors to consider that they might in some way be wrong. This is linked to training and management of doctors, which continues to encourage a strong degree of individual rather than team working.

I have visited regional specialist units where senior clinicians have not spoken with or met each other for 10 years. They seemed proud of this, management felt powerless, and so a situation that was inevitably damaging to the services given to patients was allowed to continue. It is taking the point about wider difficulties in the health service too far to say those involved in Bristol were scapegoats. Nor is it fair on the many in the NHS who have not allowed similar lapses in standards to occur. Those at the centre of the problems in Bristol were indeed to blame even if they were not wholly to blame.

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1 Smith R. One Bristol, but there could have been many. *BMJ* 2001;323:179-80. (28 July.)

Excellence may not be immediately achievable

EDITOR—With reference to Smith's editorial, the Bristol affair has added more support to the system of clinical governance that drives our new culture of delivering only excellence.[1] That is laudable—but what if this excellence is not immediately achievable?

There is a national crisis in the care of children with severe burn injuries. The historical mismatch of location of burn units and paediatric centres has left us with a tiny number of facilities where a child can receive expert burn care and intensive care. These few alone do not currently have the infrastructure to cope with the national workload.

The Paediatric Intensive Care Society has set a standard that children with burn injuries are better cared for in a paediatric intensive care unit with no burns expertise rather that in a burn centre without a lead paediatric intensive care unit. The national burn care review has set a standard that is the complete antithesis of this.

Several units, using the issue of clinical governance, have addressed this quandary by ceasing to provide a service for severely injured children. Most have done so without making alternative arrangements for their likely patients. Bristol continues to provide a service and now has to take additional cases from other regions. We do not meet all of the standards. Our results, however, are as good as any other unit in the United Kingdom. We have a long term strategy that will meet the standards but will take several years to implement.

Our service is now criticised locally for continuing to accept the more complex cases. The critics are clear that to provide no service is better than one that is flawed. If, however, there is nowhere that provides a better service, where do I send the patients? Unfortunately, the response to this question is often, "We don't care."

My fear is that this will become a more favoured option. Why take a risk after all? If we have to provide only excellence then should we be denying children a good service until we get there, and how do we get to excellence if we are not actually doing the work?

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Achieving accountability should be priority

EDITOR—The widow of a man who died in a private hospital after a minor operation recently received an award of over £500 000 for herself and two young children after a prolonged medicolegal action. In a letter of thanks for my contribution to the case she writes, "Despite all we have proved regarding the mistakes and inadequacies of his care there has been no admission of liability. More worrying is the fact that the case is at an end but nothing has changed as a result. The whole system will carry on as before so I know more people's lives will be shattered unnecessarily."

It is remarkable that while paying enormous sums, doctors and their legal advisers cannot admit the liability that they patently recognise among themselves. Any system of abolishing clinical negligence litigation with an administrative scheme for awarding compensation must meet the need for accountability that litigants have been demanding but not getting for far too long.[1] Achieving accountability is a major recommendation of the Bristol inquiry and one the medical profession should put high on the ranked list of priorities Smith's editorial finds wanting in the Kennedy report.[2] In terms of resources, little more is needed than a determination to be able to admit our errors. Our patients and their relatives deserve to know that lessons have been learnt and will be put to good use.

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1 Dyer C. Bristol inquiry condemns hospital's "club culture." *BMJ* 2001;323:181-2. (28 July.)
2 Smith R. One Bristol, but there could have been many. *BMJ* 2001;323:179-80. (28 July.)

Patients' concerns are still not being heeded

EDITOR—With reference to Kmietowicz's news item,[1] the publication of the inquiry into surgeons at the Bristol Royal Infirmary will further fuel the rise of complaints against doctors. The reasons are obvious—when things do not go smoothly patients are still not treated as valued customers but as an annoying nuisance.

As a general practitioner and clinical governance lead I try to suppress my irritation when yet another seemingly trivial complaint surfaces in the context of us all working very hard at serious issues, yet it is my job to put a patients' perspective into clinical governance in my local health group so that these complaints are treated with courtesy and respect. Yet as a patient my experience is that this is just not happening.

Recently I asked to see my notes under the data act for an episode of care at a centre of excellence two years ago. I had been referred to that hospital as a tertiary referral from my consultant locally but found that I was not getting the treatment I needed so I went privately to a hospital acknowledged to be the best centre in the country. The problem for which I had been referred was cured. Two years on I felt that, in the interest of good clinical governance, I would like to find out why things went so wrong, and I asked to see my notes under the data act, paying the statutory £10—a difficult operation compared with getting notes out of a general practitioner's surgery. I indicated that I was a clinical governance lead for an area whose patients were regularly referred to this hospital but that I was writing as a patient with no intention of filing a complaint. I also said that I would like a clinician to look at the case for interest to see whether there was anything that would be useful to improve matters for patients in the future.

I thought that an organisation that purported to provide a tertiary service to non-local patients might be interested in a case where a patient found it necessary to leave its service to go to a competing one. I expected a short letter from a clinician, saying that my letter would be considered in the context of clinical governance. I got a letter from the manager, saying that a consultant had read my letter and it was to be filed in notes. End of story. Perhaps I should have put in a complaint after all. Nothing seems to have been learnt from the events of the past few years.

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1 Kmietowicz Z. GMC steps up hearing to deal with rise in cases. *BMJ* 2001;322:129c. (21 July.)

Doctors' positioning of defibrillation paddles

Level of evidence should have been assessed

EDITOR—Heames et al assessed the performance of doctors in placing defibrillation paddles in the correct positions on the chest of a training manikin.[1] They forgot to assess the level of evidence that allows for the determination of correct placement. Volume 46 of *Resuscitation* presents only two references about paddle placement: one is the original work by Lown in 1967, the other a 1981 study by Kerber et al.[2–4] The text in *Resuscitation* is almost word for word that of the American Heart Association's guidelines published in 1992 in the journal of the American Medical Association, which offered the same paucity of references.[5]

I conclude that the evidence on which Heames et al base their assessment is the original work done in 1967 and the comparison made by Kerber et al. The work in 1967 was done with quite different equipment, timings between shocks, and wave forms. The comparison by Kerber et al was between anterolateral and anteroposterior placement, with only one version of either except for the pad's diameter, and exclusively addressed the cardioversion of atrial fibrillation.

So far as the references quoted go, there is no evidence that the variation detected by the study is of any clinical importance.

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1 Heames RM, Sado D, Deakin CD. Do doctors position defibrillator paddles correctly? Observational study. *BMJ* 2001;322:1393-4. (9 June.)
2 Part 4: the automated external defibrillator: key link in the chain of survival. *Resuscitation* 2000;46:73-91.
3 Part 6: advanced cardiovascular life support. Section 2: defibrillation. *Resuscitation* 2000;46:109-13.
4 Kerber RE, Jensen SR, Grayzel J, Kennedy J, Hoyt R. Elective cardioversion: influence of paddle-electrode location and size on success rates and energy requirements *N Engl J Med* 1981;305:658-62.
5 Emergency Cardiac Care Committee and Subcommittees, American Heart Association. Guidelines for cardiopulmonary resuscitation and emergency cardiac care. Part IX. Ensuring effectiveness of communitywide emergency cardiac care. *JAMA* 1992;268:2289-95.

Other factors have not been assessed

EDITOR—Heames et al report a study of 101 doctors who were asked to place the paddles on a manikin who they were told was in ventricular fibrillation.[1] The doctors placed the sternal and apical paddles incorrectly in 35% and 78% of cases, respectively.

This study has not targeted the health professionals who perform defibrillation in clinical settings. Trained nurses and paramedics are commonly called on to perform defibrillation, but neither of these groups was investigated. Twenty per cent of the study group included consultants, who rarely carry out defibrillation. No details were given of whether the doctors enrolled formed a part of the hospital's cardiac arrest team, which commonly performs defibrillation.

Heames et al ask whether doctors position the paddles for defibrillation correctly. Their assumption is that incorrect paddle placement reduces the chances of successful defibrillation. This study defined incorrect paddle placement as more than 5 cm from the position stated by the guidelines issued by the European Resuscitation Council. This is an arbitrary distance, as the distance of incorrect paddle placement that results in unsuccessful defibrillation is unknown. In addition, there is no hard evidence that incorrect paddle placement is an important cause of reduced survival in patients with ventricular fibrillation. There is, however, evidence that several other factors do affect survival of patients in ventricular fibrillation. In cardiac arrests occurring out of hospital, cardiopulmonary resuscitation through bystanders, and shorter ambulance response times, strongly predict survival to hospital discharge.[2] Other initiatives such as the provision of intelligent defibrillators in public places may also be important.

In hospital the use of monitored beds in coronary care units improves survival in cardiac patients, mainly by reducing the time to defibrillation. Although the provision of defibrillators on each ward, the level of staff training, and the response time of the "crash team" are also likely to be important, more research is needed into these subjects.

The study by Heames et al is a small, observational study in an artificial setting using a manikin and an unrepresentative group of health professionals. The question of whether incorrect paddle position is a cause of unsuccessful defibrillation of patients in ventricular fibrillation remains unanswered. Other factors that are more likely to be important in outcome for such patients have not been addressed.

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1 Heames RM, Sado D, Deakin CD. Do doctors position defibrillator paddles correctly? Observational study. *BMJ* 2001;322:1393-4. (9 June.)
2 Pell JP, Sirel JM, Marsden AK, Ford I, Cobbe SM. Effect of reducing ambulance response times on deaths from out of hospital cardiac arrest. *BMJ* 2001;322:1385-8. (9 June.)

Authors' reply

EDITOR—The current recommendations for paddle position during defibrillation emanate from the International Liaison Committee on Resuscitation (ILCOR).[1] The evidence for these recommendations is based on limited human and animal studies and physiological modelling but represents what is considered an optimal position. Contrary to Calinas-Correia's assertions, there is plenty of evidence that misplaced paddles are of clinical significance. Calinas-Correia overlooks the aim of our study, which was to assess whether defibrillation guidelines are followed—it was not to provide a review of the evidence for the guidelines, which has recently been carried out by ILCOR.[1]

In hospital all doctors participating in clinical practice are expected to be able to perform basic life support, including defibrillation. Whether they are members of the cardiac arrest team or not does not excuse them from their ability to defibrillate correctly. Khiani's statement that consultants rarely carry out defibrillation is not evidence based and certainly does not apply to the centre in which this study was carried out.

Paramedics do not, and nurses rarely defibrillate patients in our hospital. Defibrillation should be performed by the first competent person to reach the patient, whether he or she is a member of the cardiac arrest team or not. All doctors in acute medical and surgical specialties were therefore studied as it is this group that is likely to be performing defibrillation. Khiani says that it is still unclear from this study whether the issue of incorrect paddle placement is a notable problem among staff who perform defibrillation in clinical settings, be they doctors, nurses, or paramedics. We disagree, having studied a group representative of those performing defibrillation in a typical hospital.

We stated that incorrect paddle placement results in a greater proportion of the current passing through non-cardiac tissue and a reduced chance of successful defibrillation. Khiani challenges this, saying that these assumptions have yet to be proved and their effect on survival to hospital discharge is unknown. He is unaware of studies showing that adjacent placement of electrodes creates a low impedance pathway along the chest wall, which shunts current away from the heart and may result in failed defibrillation,[2] confirmed by finite element analysis.[3] Paddle position is an important determinant of the success of cardioversion for atrial fibrillation,[4–6] and although optimal paddle position may not be the same as that for ventricular fibrillation, it does suggest that paddle position is of equal importance in determining the success of defibrillation for ventricular fibrillation.

We agree with Khiani about the importance of cardiopulmonary resuscitation through bystanders, shorter ambulance response times, and the provision of advisory defibrillators in public places, but these factors are not relevant to our study. We do not agree that a survey of 101 doctors is "small" in the context of this study. The use of a manikin has produced results similar to those that we have observed during actual cardioversion. The aim of the study was to assess whether doctors position defibrillation paddles correctly. Our study aims have been met.

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1 Part 6: Advanced cardiovascular life support. Section 2: defibrillation. *Resuscitation* 2000:46:109-13.
2 Catherine MR, Yoerger DM, Spencer KT, Kerber RE. Effect of electrode position and gel-application technique on predicted transcardiac current during transthoracic defibrillation. *Ann Emerg Med* 1997;29:588-95.
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4 Ewy GA. The optimal technique for electrical cardioversion of atrial fibrillation. *Clin Cardiol* 1994;17:79-84.
5 Botto GL, Politi A, Bonini W, Broffoni T, Bonatti R. External cardioversion of atrial fibrillation: role of paddle position on technical efficacy and energy requirements. *Heart* 1999;82:726-30.
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Testing for *Helicobacter pylori* in dyspeptic patients

Did paper have statistical discrepancies?

EDITOR—I am confused by Weijnen et al's description of the statistical methods used in their study and how they fit with the data presented.[1] The methods section states that all variables found to be univariate predictors of peptic ulcer with $P < 0.25$ were entered in the multivariate regression model. However, the results section says that age was included in the model, although table 2 shows that it was not predictive ($P = 0.67$).

Table 2 also shows that $P = 0.24$ for both hiatal hernia and pain after meal, so these should have been included in the multivariate model, but neither of them was. Are these discrepancies due to a typing mistake, or is there another explanation?

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Authors' suggestion muddies waters in debate

EDITOR—Weijnen et al suggest that we should test and treat patients at high risk of peptic ulceration.[1] This seems to muddy the waters in the debate about testing for *Helicobacter pylori* infection in primary care. Of the 38 patients they identified as having a peptic ulcer, only 22 gave a positive result to a non-invasive *H pylori* test, although the rate of detection overall was increased from 31% to 41% by using invasive tests of culture or histology, which suggests that serological testing is not as sensitive.

Agreus and Talley reported that the sensitivity of *H pylori* enzyme linked immuno-

sorbent assay (ELISA) kits had an average sensitivity of 85% (low and high extremes 49% and 99% respectively).[2] Why are the rates of detection so low in Weijnen et al's study (33 of the 38 patients had a duodenal ulcer), when it has been shown that virtually all patients with a duodenal ulcer have *H pylori* infection?[3] This apparent discrepancy will make it difficult to generalise their results into primary care.

My practice will continue to follow the recommendations in *Guidelines* (a free publication to all general practitioners), which summarises current evidence.[4] This gives a suggestion from the Primary Care Society for Gastroenterology: that routine testing of patients with dyspepsia that has not been investigated is not recommended at the first presentation, but at subsequent presentations testing and referral for endoscopy are appropriate.

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4 Ford-Kelcey G, ed. *Guidelines. Summarising clinical guidelines for primary care.* Berkhamsted: Medendium Group, 1999.

Authors' strategy would leave many patients with ulcer uncured

Editor—Weijnen et al recommend that use of the *Helicobacter pylori* test should be restricted to dyspeptic patients with a history indicating a high risk of underlying ulcer.[1] This would include patients with a history of peptic ulcer and those who were smokers and experienced pain on an empty stomach.

Their recommendation is based on their finding that the prevalence of underlying ulcer in such patients with a positive result of an *H pylori* test was 26%, compared with only 7% in their other dyspeptic patients with a positive result. We agree that the proposed strategy is attractive in reducing the number of patients treated with antibiotics per ulcer cured. But because of the insensitivity of clinical history in predicting ulcers it will deprive a substantial proportion of dyspeptic patients of a simple long term cure of their underlying ulcer. Indeed, the paper shows that the strategy would leave 36% of the patients with ulcer who were positive for *H pylori* uncured of their chronic disease, at risk of subsequent ulcer complications, and requiring long term acid inhibitory treatment.

We also disagree with the authors' assertion that the likelihood of underlying ulcer is the only factor in favour of treating *H pylori* infection in dyspeptic patients. Benefits of treating the infection in patients without ulcer include curing symptoms in 9% of such patients,[2] removing their recognised increased risk of subsequent ulcer,[3] removing a recognised risk factor for gastric cancer and lymphoma,[4] and removing the risk of the patient developing atrophic gastritis with subsequent proton pump inhibitor treatment.[5]

For the above reasons, it seems inappropriate to restrict the *H pylori* test and treat strategy to patients whose history indicates a higher risk of ulcer.

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2 Moayyedi P, Soo S, Deeks J, Forman D, Mason J, Innes M, et al on behalf of the Dyspepsia Review Group. Systematic review and economic evaluation of Helicobacter pylori eradication treatment for non-ulcer dyspepsia. *BMJ* 2000;321:659-64.
3 McColl KEL. Should we eradicate Helicobacter pylori in non-ulcer dyspepsia? *Gut* 2001;48:759-61.
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5 Kuipers EJ, Lundell L, Klinkenberg-Knol EC, Havu N, Festen HPM, Liedman B, et al. Atrophic gastritis and Helicobacter pylori infection in patients with reflux esophagitis treated with omeprazole or fundoplication. *N Engl J Med* 1996;33:1018-22.

Clinical importance of predictive values is dubious

Editor—Weijnen et al stated that testing for *Helicobacter pylori* in dyspeptic patients provided additional diagnostic information in patients deemed to have a high risk of peptic ulcer.[1] Closer scrutiny of table 4 shows that the test actually performed similarly in the low and high risk groups. Positive likelihood ratios calculated for the two groups are 1.8 (95% confidence interval 1.1 to 3.1) and 1.8 (1.2 to 2.7) respectively. The negative likelihood ratios were 0.7 (0.4 to 1.1) and 0.6 (0.3 to 1.0) respectively.

If a prevalence of 16% is assumed for peptic ulcer this equates to a post-test probability of 18.6% to 34% if the confidence interval for the positive likelihood ratio in the high risk group is used. If the test result was negative in this group the post-test probability would be 5.4% to 16%. On the basis of this the test adds little further information to that obtained by history taking. The authors do not state whether the changes in predictive values (16% to 26% and 16% to 10%) were significant, but their clinical importance, in terms of diagnosing peptic ulcer, seems dubious even for the high risk group.

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Authors' reply

Editor—Jacobs is confused by our criteria for including predictors in the multivariate analysis. Multivariate analysis was performed with all variables of $P<0.25$ in univariate analysis plus variables that are considered clinically relevant and were important predictors in earlier studies.[1] For the latter reason age was selected for multivariate analysis and not because it was associated with peptic ulcer, as stated in the second paragraph of the results section. Hiatal hernia and pain after meal should also have been mentioned here as they were also selected for multivariate analysis on the basis of their univariate P value of $P<0.25$.

We do not agree with Williams that our results muddy the waters of guidelines for *Helicobacter pylori* testing. We do not believe that the relatively low *H pylori* infection rate in our patients with duodenal ulcer is due to poor test performance; a trend towards *H pylori* negative duodenal ulcers in countries with low infection rates has been reported previously.[2] This means that the role of *H pylori* testing in primary care management of peptic ulcer needs closer consideration; we aimed at defining more precisely the diagnostic contribution of testing to finding cases.

We agree with McColl and Murray that applying our strategy to all dyspeptic patients would mean that a minority of the patients with *H pylori* infection and ulcer would not immediately be treated optimally. As the evidence base for testing and treating all dyspeptic patients is poor and prompt endoscopy in all cases is unrealistic, we think our algorithm represents the best compromise between overtreatment and optimal treatment for patients with ulcer in primary care. We realise that many colleagues consider *H pylori* treatment beneficial for several other indications. So far, however, the effectiveness of this treatment has not been shown in these patients.

Sultana comments that the additional effect of the *H pylori* test is limited, and wonders whether the change from prior to posterior probability was significant. In the high risk group these changes after a positive test (from 16% to 26%) and a negative test (from 16% to 10%) were significant, as is also indicated by the 95% confidence intervals of the two posterior probabilities estimated by Sultana. In the low risk group no significant changes were seen. In addition, the cost effectiveness of *H pylori* testing in the high risk group compared with the overall group is much higher (40 out of 54 *v* 152 out of 174 treated unnecessarily). This underlines the clinical value of *H pylori* testing in the high risk group only.

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1 Numans ME, Van der Graaf Y, de Wit NJ, Touw-Otten F, de Melker RA. How much ulcer is ulcer-like? Diagnostic determinants of peptic ulcer in open access gastroscopy. *Fam Pract* 1994;11:382-8.
2 Ciociola AA, McSorley DJ, Turner K, Sykes D, Palmer JB. Helicobacter pylori infection rates in duodenal ulcer patients in the United States may be lower than previously estimated. *Am J Gastroenterol* 1999;94:1834-40.

Ireland lacks consensus on neonatal vitamin K prophylaxis

EDITOR—Ansell et al reported considerable variations in policies concerning the use of prophylactic vitamin K in the United Kingdom.[1] With the published data supporting and not refuting the association of neonatal vitamin K with childhood malignancies,[2 3] the current practice of several policies is of serious concern. A lack of consensus was also shown in a national survey we conducted in the Republic of Ireland.

Relevant information was collected by using a questionnaire from all the 23 maternity units, and the range of practices was compared. Dose, frequency, route, and time of vitamin K prophylaxis in breastfed and bottlefed infants among term and preterm categories was determined. The survey was sent to the sister or midwife in charge of the labour ward and neonatal unit of each hospital during February and March 2001.

All 23 maternity units in the eight health boards responded to the survey, giving a 100% response rate. This represents vitamin K prophylaxis given to all the in hospital live births in Ireland. We observed that although vitamin K is being routinely administered to all neonates at birth, no consensus in the dose or route of administration could be established even among the maternity units within the same health board.

For term infants, 11 (48%) hospitals administered 1 mg intramuscularly, whereas 0.5 mg is given intramuscularly and 2 mg orally in seven (31%) and five (22%) units, respectively. Eighteen (78%) hospitals preferred the intramuscular route, and five (22%) the oral route. Among hospitals giving intramuscular injections to term neonates, no differences were noted on the basis of infant feeding practices. For preterm neonates, the intramuscular route was preferred for both breastfed and bottlefed, in 20 (87%) and 21 (91%) hospitals, respectively. Fifteen (65%) gave 0.5 mg intramuscularly to preterm babies, whereas 1 mg (total dose) and 0.4 mg/kg were preferred by five (22%) and three (13%) units. In total, six different policies were followed among the 23 maternity units in Ireland, and no two health boards shared the same guideline.

Our observations indicate a lack of national consensus at the point of delivery of vitamin K prophylaxis against haemorrhagic disease of the newborn. A similar survey in France reported a range of practices in neonatal vitamin K policies.[4] Although recommendations are put forward by Ireland's paediatric faculty, a lack of clear guidelines from the Department of Health and Children is probably contributing to the diverse policies implemented by the various health providers.

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Oral rehydration solution

Rice water is cheap and effective

EDITOR—Hahn et al report that reduced osmolarity rehydration solution is associated with a better outcome with regard to use of intravenous infusion, stool output, and vomiting than is standard WHO (World Health Organization) oral rehydration solution in acute diarrhoea.[1] As Fuchs points out in the accompanying editorial, output and duration of diarrhoea are important clinical outcomes when the efficacy of an oral rehydration fluid is considered.[2]

Rice water decreases stool output and can be used in mild to moderate gastroenteritis. Cheap and easily available, it is a common home or folk remedy for mild gastroenteritis in infants and children in many South East Asian families. It has also been used in hospital paediatric practice with good results.[3] Almost 20 years ago Wong highlighted the superior efficacy of rice water compared with WHO oral electrolyte solution for gastroenteritis in children.[3] Rice water significantly decreased the number of stools a day, and intravenous intervention was not necessary.

One notable property of rice water that may be responsible for its efficacy is its low osmolality when compared with oral electrolyte solution ($P<0.0001$).[4] In a study of two infants with ileostomies fed either oral humanised milk or rice water, rice water led to significantly lower ileal fluid osmolality and volume than did milk ($P<0.02$).[5] It is believed that hypo-osmotic solutions result in increased luminal absorption of water and thus may lead to lower ileal fluid volume. Furthermore, in gastroenteritis absorption of monosaccharide (glucose) may be affected more than that of polysaccharide (starch).[3]

Many of the infants and children who are at increased risk of gastroenteritis and susceptible to complications of dehydration live in underdeveloped or developing countries. Rice water should be considered as an option for a rehydration fluid, since it combines the theoretical advantage of low osmolality and the proved efficacy of reduction of stool output.

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Doctors must increase use and acceptance of oral rehydration solution

EDITOR—Rice water, mentioned in Ho and Yip's rapid response (www.bmj.com/cgi/eletters/323/7304/81#EL4, and printed as the letter above), has insufficient electrolytes to replace sodium and potassium losses during acute diarrhoea, in contrast to rice based oral rehydration solutions (to which these and other electrolytes are added). Moreover, the superiority of cereal-based solutions has been proved only in patients with cholera infections; children with non-cholera diarrhoea given cereal-based oral rehydration solution do not have a reduction in stool output when compared with children treated with standard glucose-based oral rehydration solution.[1]

The low level of use and acceptance of oral rehydration solution by clinicians in all countries of the world is a tragedy in the light of the widespread evidence of its efficacy.[2] I hope that the data presented by Hahn et al will help to reinvigorate efforts by policymaking bodies to establish oral rehydration solution as the standard of care for all patients with diarrhoea.

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Doctors in India still seem not to be convinced

EDITOR—I agree that reduced osmolarity oral rehydration solution is "an important step, but not a leap forward." (1) Now there are enough studies to suggest the superiority of low osmolarity oral rehydration solution over the standard WHO (World Health Organization) solution. The issue in India, however, is not which one is a better product but how to make existing oral rehydration solutions more popular among doctors.

A recent survey, conducted among doctors all over India in June 1999 by the large marketing company ORG-MARG, found that only 18% were prescribing oral rehydration solution for children aged under 3 with acute diarrhoea whereas prescriptions for antidiarrhoeals were written for 49% of cases. In certain parts of the country the rate of prescription of oral rehydration solution was just 8.3%. These were startling findings in a country where 600 000 children die annually because of acute diarrhoea. The question arises, "Why are the doctors, especially in this part of the world, still prescribing drugs, not oral rehydration solution, for acute diarrhoea in children?" Several factors play a part:

- Lack of a proper understanding of the pathophysiology of diarrhoea among most doctors
- Lack of faith in the product
- Fear of losing patients to some other doctor if drugs are not prescribed
- Children's acceptance of oral rehydration solution is poor (because of its taste and colour)
- Lack of enough time to explain and educate mothers about oral rehydration solution and diarrhoea
- Peer pressure
- Pressure from the pharmaceutical industry
- Lack of a flexible approach among the practitioners
- Lack of initiatives by the government and professional bodies engaged in child health promotional activities.

The lack of any initiative is appalling. Even the Indian Academy of Paediatrics, the sole representative body of paediatricians in India, was slow to address this critical issue. It needed aid from a Western agency to spur it on to pursue the matter further. PACT/CRH, the programme for advancement of commercial technology/child and reproductive health, was launched by the United States Agency for International Development in the middle of 1999 but was taken up by the academy only in 2000. Health comes quite low in the priorities of the establishment. To expect a government that is wasting money in patrolling deserted hills around Kashmir to give substantial funds for the purpose is definitely asking for too much.

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Oxygen treatment for acute severe asthma

Home oxygenation would be more effective

Editor—Inwald et al in their article report that the use of oxygen in general practice may result in decreased asthma mortality because progressive hypoxaemia is probably an important cause of death, and oxygen should be the first treatment given to any patient with acute severe asthma.[1] There is, however, another measure to implement a similar intervention policy for more specific groups at high risk of death from asthma.

Since the patients with even a single episode requiring intubation for severe asthma are at very high risk of recurring life threatening attacks and death, and since most asthma deaths take place at home, a trial of providing oxygen at home for emergency use for possible severe attacks in this group was conducted and was successful in reducing fatal events.[2 3] Although further large scale trials using home oxygenation are necessary to confirm this result, the emergency use of home oxygen would be more effective than oxygen in general practice for patients at high risk.

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Oxygen saturation may help identify patients in need of intensive management

Editor—We agree with Inwald et al that the use of nebulisation of β_2 agonists without oxygen could cause or worsen hypoxaemia in some patients.[1] The measure of oxygen saturation by pulse oximetry can indicate which patients presenting with acute severe asthma may be in respiratory failure and therefore in need of more intensive management. The goal of treatment should be to maintain oxygen saturation at $>92\%$.

Most published data show that salbutamol does not have a clinically important effect on oxygenation of patients with acute asthma. In an unpublished study, we enrolled 176 adult patients (forced expiratory volume in one second (FEV_1) $<50\%$ of predicted) with acute asthma, using a prospective, observational design. All patients were treated with albuterol and ipratropium bromide delivered by a metered dose inhaler into a spacer device, in a dose of four puffs at intervals of 10 minutes over 3 hours (24 puffs or 2880 mg albuterol and 504 mg ipratropium each hour); they were given oxygen only when oxygen saturation decreased to $<92\%$. We did not find a deterioration in oxygen saturation, but saturation at 180 minutes was significantly increased compared with baseline (96.9% (SD 1.8%) *v* 95.9% (1.7%), $P=0.001$).

We do not agree that the use of metered dose inhaler and spacer must be restricted to the treatment of mild or moderate acute asthma, and that nebulised β_2 agonists should be the standard treatment of acute severe asthma. A Cochrane review supports the equivalence of metered dose inhalers plus spacers and nebulisers.[2] Although patients with the most severe asthma exacerbations were excluded from the studies (patients considered for ventilation), this review included trials with patients with deterioration in blood gas concentrations and severe acute asthma ($FEV_1<25\%$ of predicted; range 9-24%).[3 4] The review also implies that paediatric patients given β agonists by holding chamber and metered dose inhaler may have shorter stays in emergency departments, less hypoxia, and lower pulse rates than patients receiving the same β agonist by nebulisation.

In our experience, metered dose inhaler plus spacer constitutes the only way to deliver quickly high doses of bronchodilators to patients with acute severe or life threatening asthma with a reduced level of consciousness; so we can administer oxygen, if necessary, almost all the time.

Administering pure oxygen to acutely ill asthma patients can result in respiratory depression with retention of carbon dioxide, particularly in patients with severely obstructed airways.[5] Since arterial hypoxaemia is governed primarily by ventilation-perfusion defects, it can be corrected promptly by the administration of moderate doses of inspired oxygen (0.4 to 0.6).

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Authors should make their data available

Editor—Hutchon discussed the desirability of publishing the raw data used in medical research articles, and Eysenbach and Sa have outlined some of the difficulties.[1 2] The least that should happen at the moment is that authors should be able to make their data available on a journal's website. One journal that already does this is *Clinical Chemistry*, which will include data supplements in the material sent to reviewers.[3] Journals should encourage authors to post their data on the website.

One area where there should be neither debate nor difficulty is that of systematic reviews. Readers of the review ought to have access to the numerical results of the primary studies being reviewed to allow the analyses to be checked and for other analyses to be investigated. Also, this enables readers to examine the actual results rather

than the authors' aggregation or summary; for example, they can assess the variation in event rates across the studies, without which an odds ratio is impossible to interpret.

We have each separately had the experience of authors of systematic reviews published in the *BMJ* refusing to release these data, in one case to enable the data to be used in an educational article. Such obstruction is worrying and suggests that some suspicion is appropriate where none had existed at the time of the request. Given that in almost all cases the results will already be in the public domain, we can see no valid excuse for not including the data in the report or making them available electronically. The missed opportunity is even clearer in those cases where an extended version of a paper appears on the web page but the trial results are still not given, although in this case the authors have made the data available to us.[4]

The *BMJ* and other journals should insist that authors of systematic reviews adhere to one of the key recommendations the QUOROM statement—namely, to present simple summary results for each treatment group in each trial, for each primary outcome.[5]

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Prehospital care of trauma must be improved in UK

Editor—Lockey et al attempt to relate survival of patients with trauma given prehospital tracheal intubation without the aid of anaesthetic drugs.[1] Their comment that "it was surprising that the outcome was almost always fatal" when a tracheal tube could be passed without anaesthetic drugs deserves further discussion, as this highlights the suboptimal prehospital management of severe trauma in the United Kingdom. Prehospital endotracheal intubation has been associated with improved survival in patients with blunt injury and a score on the Glasgow coma scale of <8 at the scene in North America[2] and elsewhere.[3]

In multiple trauma the main reason for a decreased coma score is an associated head injury. The authors' data do not indicate how many of the patients who were intubated had head injuries, what proportion of them was intubated without drugs, and what their coma score was before intubation.

The extent of neurological damage is aggravated by the secondary insults of hypoxia, hypercapnia, hypotension, and increased intracranial pressure. Early tracheal intubation in severe head injury (defined as a score on the Glasgow coma scale of <8) is recommended not only to protect the airway in patients with obtunded airway reflexes but also to aid ventilation and prevent some of these secondary insults. Even if a patient's airway is secured by tracheal intubation, inadequate ventilation may lead to high arterial carbon dioxide pressure and concomitant brain swelling.

Intracranial pressure will increase during laryngoscopy and endotrachraeal intubation when anaesthetic drugs are not used. Laryngoscopy and intubation also produce a pronounced rise in blood pressure, and the rapidity of this rise may outstrip cerebral autoregulation, causing the intracranial pressure to rise.[4] The use of induction agents, such as thiopentone, and muscle relaxants when a patient's airway reflexes are still present, can counteract these effects. Might these factors be contributing to the poor outcome shown in the report?

Outcome after head injury is closely correlated with the initial score on the Glasgow coma scale.[5] Many patients might still do well, however, if secondary insults to the brain could be prevented. A patient with isolated head trauma who develops an extradural haematoma and subsequently loses consciousness but is managed early (that is, in the prehospital environment) and optimally will do better than a patient with the same injury but also the secondary insults.

The United Kingdom urgently needs to adopt an established prehospital scoring system[5] and a management algorithm that will allow staff to identify and treat patients at risk; leaders in prehospital and immediate care must put such systems in place.

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Story on smoking and poor people is incomplete

Editor—Wiltshire et al have added to the tobacco control debate by documenting that smokers in deprived areas perceive a lack of support for cessation.[1] Their findings need to be interpreted in the totality of the evidence.

Firstly, the study did not pose or answer the key question: "If the government were to subsidise nicotine replacement or other cessation programmes, would they take advantage of the subsidy and try to quit?" A "yes" answer would deserve respect and a public response, such as increased length of nicotine replacement under the NHS.

Secondly, the study discussed perceived benefits but not perceived or actual risks. About half of long term smokers will be killed by their addiction, losing about 20-25 years of life.[2] Smoking seems to account for much of the observed socioeconomic differences in adult male mortality.[3]

Thirdly, as with any other consumer tax, increases in cigarette taxes are regressive among those who continue to consume (smoke). But people on lower incomes may well respond more to price changes than those on high incomes.[4] Higher tobacco taxes would thus narrow differences in consumption between rich and poor. If more of the poor smokers quit, then the recent tobacco tax increases in the United Kingdom may well be progressive, even though overall tobacco tax itself is regressive.

What matters in defining regressivity is the overall system of expenditure and taxation, not simply one tax. A priori, one would expect greater welfare losses among continuing poor smokers, as the study notes. Moreover, many welfare-enhancing health interventions, such as child immunisation or family planning, are often more costly to poor households. For example, poor families may have to spend more time in transport to attend clinics than rich families and may lose income in the process.

Finally, the study implies that individual smuggling is the key source of contraband. In fact, the key source is large scale tobacco smuggling involving criminal organizations. The tobacco industry uses smuggled cigarettes to argue for lower taxes on cigarettes and gain market share for their brands. But even in the presence of smuggling, higher taxes reduce consumption. Lowering taxes is a poor way to reduce smuggling. Cheaper cigarettes are more likely to increase smoking among poor and young people. For example, when Canada lowered taxes in 1994 in response to organised smuggling, smoking among teenagers increased dramatically. A better solution is to crack down more aggressively on criminal suppliers.[5]

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Health of socially excluded groups: lessons must be applied

EDITOR—The editorial by Watt points out that projects targeting socially excluded groups tend to address consequences rather than addressing the causative issues.[1] It is often the provision of evidence of shocking health figures, such as those for children looked after and accommodated by the local authority (in care), that underpins the release of funding.[2] These initiatives will have a beneficial effect on the health of the nation only if lessons learnt from them are applied to mainstream services.

In Lothian, funding has been obtained from the Scottish Executive for the residential care health project, targeting children and young people in residential units provided by the local authority. By the time a young person enters a residential unit, he or she will have moved around the care system several times. Only 46% of children looked after continuously for four or more years have spent at least the preceding two years in the same placement, and 18% of looked after children experience three or more placements in the course of one year.[3] Lynch and Gough acknowledge that the healthcare system breaks down when people move away from their general practitioners.[4]

We found that 54% of young people in our units currently have no health information in their files for the information of those who care for them. The health care of these young people starts off at a disadvantage because of the chaotic homes they come from, but this figure reflects largely the effect of inadequate basic health supports to a disadvantaged group. The health information that is required for anyone to care safely for a child is held in many different places, and rectifying this has been a particular challenge for us. We believe that it is the place of the health system to support social services in collating this information for the benefit of children and young people.

Lynch and Gough make the further point that it can be difficult to find general practitioners sympathetic to the lifestyle of socially excluded groups of children. These children do not choose their lifestyles: they are imposed on them by the lottery of their birth. If this type of work with a small group of looked after children can be put into mainstream child health services, both paediatric and primary care, then we can move effectively away from the concept of providing a "special" service.

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Bias in alternative medicine is still rife but is diminishing

EDITOR—In 1995, journals of alternative medicine published virtually no studies with negative results, which suggests that the literature was far from objective.[1] To determine whether the situation has changed we analysed last year's volumes of three journals originally evaluated and compared our results with those from 1995. The journals were *Complementary Medicine Research*, published since 1994 (six times a year, published in both German and English, with abstracts in both languages); *Complementary Therapies in Medicine*, published since 1993 (four times a year, published in English); and *Alternative Therapies in Health and Medicine*, published since 1995 (six times a year, published in English).

The 207 articles published in 2000 were categorised as positive (a particular intervention is helpful for a particular condition), neutral (no clear conclusion), or negative (intervention is unhelpful). The longitudinal comparison (2000 *v* 1995) showed that the percentage of negative articles was still minute, at 5% (10/207) in 2000 compared with 1% (1/179) in 1995. The percentage of neutral studies had increased from 44% (78/179) in 1995 to 52% (107/207) in 2000, and the percentage of positive articles had fallen from 56% (100/179) in 1995 to 43% (90/207) in 2000.

These findings imply that bias is still rife but is diminishing. The discipline of alternative medicine may have started its process of maturation, but it still has a long way to go.

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Is it denial or wisdom to accept life threatening illness?

EDITOR—Like Cuddihy,[1] I have a life threatening illness. What I find interesting is how I can spend most of my time not thinking about my prognosis. Is this denial or wisdom? Calling it denial makes my relative comfort into a pathological mental mechanism. Perhaps I should not complain about it. Most of us prefer ignorance about how our sausage was made. I like to think that I'm learning that the future and the past actually don't exist except as they affect the present; that I won't live six months or 20 years but only today, and every today.

This does not make me avoid reasonable planning about the future and pleasurable and informative recollections of the past, because such activities are part of the present. When we go through training we do so because of our expectations about what we will do with it, yet the training itself, especially in retrospect, is as important and fulfilling as the future career.

Does my medical knowledge help or hinder? I am a psychiatrist, not a cardiologist, and have had to learn much cardiology to understand my illness (myocardial infarction, coronary artery bypass, ventricular tachycardia) and its treatment (many pills and an indwelling cardioconverter). But I find myself uninterested in the technical details, and I don't rummage through the literature to read about the risk:benefit ratios of various treatments and my estimated life span. Again: denial or wise acceptance of the inevitable?

I've learnt during my long years of psychiatric practice to have less concern about untangling the web of causality of symptoms and blind spots and more concern with marshalling intact skills. Do I encourage ignorance? At best we understand very little anyhow. I consider most important what we do with our limited knowledge.

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Surgeon with worst performance figures might be best option

EDITOR—When I was a student at Guy's Hospital in the 1930s Sir Arthur Hurst said that if he found he had carcinoma of the stomach he would choose to have the surgeon with the worst figures[1]—because that surgeon would have a go without worrying about his performance figures.

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