Letters

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Suboptimal ward care of critically ill patients

Suboptimal care should have been defined

EDITOR—McQuillan et al show that most patients receive suboptimal management of oxygen therapy, airway, breathing, circulation, and monitoring before admission to intensive care.[1] In an area of medicine renowned for objective measurement it is surprising that this study should rely on the subjective opinions of two assessors about what constituted suboptimal care. Understandably, their opinions often disagreed.

The authors accept that there are difficulties in relying on assessors' opinions, but we must not underestimate these limitations. The assessors knew the outcomes of the patients, which must have biased their opinions, particularly since suboptimal care is not defined. How suboptimal care was defined is crucial to the paper's message, and more information about the data evaluated by the assessors would have been preferable to the lengthy discussion, much of which was not directly related to the data.

Unfortunately, many of the data are self fulfilling. It is unsurprising that the suboptimally managed group scored badly on oxygen therapy and airway, breathing, and circulation and that 67% of this group were late admissions to intensive care since these were presumably the factors used to determine suboptimal management.

Nevertheless, a key message is that most of the well managed patients were admitted to intensive care units within the first day of admission, with presumably some going straight from accident and emergency. These acutely ill patients are perhaps more easily identifiable as going to need intensive care. Conversely, those patients who arrived at hospital less ill and who deteriorated while on general wards were those who received suboptimal care. There was a longer time between admission to hospital and admission to intensive care in these patients. We are not told if any of the admissions to intensive care were delayed because of lack of beds. Although there is no excuse for suboptimal care, sometimes admission to intensive care is requested because a ward with overstretched nursing staff and no high dependency beds recognises that it is unable to provide optimal care for an acutely ill patient.

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1 McQuillan P, Pilkington S, Allan A, Taylor B, Short A, Morgan G, et al. Confidential inquiry into quality of care before admission to intensive care. *BMJ* 1998;316:1853-8. (20 June.)

Assessment of quality of care was flawed

EDITOR—McQuillan et al made striking claims about avoidable admissions and the contribution of suboptimal care to subsequent mortality and morbidity on the basis of a study which was deeply flawed in two crucial respects.[1]

Firstly, the research relied on implicit judgments of the quality of care made by two external assessors, who were presented with data abstracted from the clinical records. The authors argued that they had to use implicit assessments of the quality of care because it was too difficult to set out objective or explicit definitions of what constituted suboptimal care. If it is hard to define explicit quality standards or criteria, it will be equally hard to reach a valid and reliable implicit assessment of the quality of care. The extensive literature on implicit reviews suggests that their interrater reliability is very mixed.[2 3] The kappa statistics cited in this study, ranging from 0.42 to 0.53, would be regarded as at best indicating moderate reliability.[4] The authors could have increased the reliability of the assessments by using more assessors for each case and by undertaking some training and feedback of results to assessors before the study.

Secondly, the two assessors who made the judgments about the quality of care were apparently aware of the eventual outcomes in each case. In other words, they knew about subsequent morbidity and mortality when they were making judgments about the quality of care. Implicit judgements about the quality of care are likely to be inappropriately influenced by knowledge of eventual outcome. Assessors are more likely to rate the care as suboptimal if they are told that the patient died, even though the process of care is unchanged.[5] This means that the association between assessors' ratings of the quality of care and patients' subsequent mortality, which is made much of in the paper, may simply be an artefact of the methods used.

If implicit professional judgments about the quality of care are to be used in future, the reliability and validity of those judgments should be more rigorously examined. More information about the training of assessors should be sought, better evidence of interrater reliability should be presented, and implicit reviews of the process of care should be blinded to the subsequent process and outcome to avoid bias. Because implicit and explicit review methods each have advantages and disadvantages, it may be advisable to use both and compare their results rather than to opt for one or the other.

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Active management should prevent cardiopulmonary arrests

EDITOR—Garrard and Young[1] asked whether McQuillan and colleagues' findings were representative of care across the United Kingdom.[2] We conducted a similar study in a Manchester teaching hospital aimed at identifying the incidence of preventable physiological deterioration before cardiopulmonary arrest on general medical and surgical wards.[3]

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We analysed 47 consecutive arrests and found abnormal vital signs in 24 patients during the 24 hours before the arrest call was made. Appropriate tests were performed but results were often not acted on; senior staff were consulted before arrest in only six cases. Two patients were referred for intensive care before arrest; both were deemed unsuitable. Most importantly, cardiopulmonary resuscitation was largely unsuccessful. Nine of the 47 patients survived the arrest, and five went home alive. In patients with premonitory signs, only three survived the arrest and none left hospital.

Though we approached the subject from a different angle, our findings support and complement those of McQuillan et al. In over half our patients the arrest was preceded by a more gradual physiological decompensation and therefore opportunity existed for intervention. Ward staff need to appreciate the importance of abnormal signs and investigations and seek help promptly from experienced clinicians. Intensive care may be appropriate but is more likely to benefit patients if they are referred early. We believe that some of the cardiopulmonary arrests in our survey could have been prevented. The proposed medical emergency team would have been invaluable in assessing these patients.

We agree that a new model of treatment of critically ill ward patients is required with emphasis on early referral and treatment. However, some patients who are approaching cardiopulmonary arrest are so sick that cardiopulmonary resuscitation will not succeed and intensive care would be inappropriate. We would urge earlier, wider consideration of "do not attempt resuscitation" orders in this group. The trend should be towards proactive management, either to expedite referral for intensive care for those who need it or to allow a dignified death for those who are destined to die in any case. Too often we see a haphazard trial of cardiopulmonary resuscitation followed by hasty referral to intensive care. This is inhumane, futile, costly, and demoralising.

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1 Garrard C, Young D. Suboptimal care of patients before admission to intensive care. *BMJ* 1998;316:1841-2. (20 June.)
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Inadequate staffing means problems are missed

EDITOR—As an intensive care resident, I was unsurprised by the results of McQuillan et al.[1] Their recommendations for improving management of patients before intensive care, including the medical emergency team and better training, should all be supported. A problem not dealt with is detecting the acute physiological disturbance in the first instance.

I was on call for anaesthesia last weekend. On Sunday the preregistration house officer for surgery had 102 ward patients to look after. The medical preregistration house officer was caring for 114 patients, with the help of one half of a senior house officer. Even with the best acute medical emergency training these doctors cannot be proactive in the care of this number of patients, most of whom they have never met before. Under such pressure these doctors can only react to problems identified to them. We now seem to rely on the ward nurses to call the "physiology police," but with more than eight patients per trained nurse on the medical and surgical wards, detection of something physiologically abnormal is not reliable. I am sure this hospital is not unique in this situation.

To have any chance of improving the quality of acute medical care on general wards there must be either fewer patients or more medical and nursing staff. Treatment cannot start until the patient's acute problem is identified.

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Doctors don't review patients that nurses identify as highly dependent

EDITOR—McQuillan and colleagues report suboptimal care before admission in 54% of patients admitted to intensive care units in two hospitals.[1] In 1993 we performed an audit at our hospital to assess the number of patients in selected general wards who would be more appropriately cared for in intensive care or a high dependency unit. The survey was performed daily over two weeks and included general medical, general surgical, and cardiology wards. The night sister initially identified the most dependent patients. During the study 56 assessments were made of 39 patients. We recorded the grades of medical staff attending the patients and the frequency with which the patients were seen. Severity of nursing workload was assessed with the therapeutic intervention score.

Requirement for more nursing was given as the reason for referral for assessment in 34 (87%) patients. Twenty eight were thought to require more monitoring, and 18 were thought to require more intensive treatment or organ support. The nursing staff directly looking after each patient were then asked to judge whether admission to intensive care or high dependency units was required. In 20 of 56 cases (36%) nurses thought admission was necessary. There were significant overlaps between the therapeutic intervention scores of ward patients judged to require high dependency or intensive care (13-36), ward patients judged not to require such care (11-32), and patients in intensive care units at the time of the audit (24-70).

Of the patients identified as requiring more intensive care by the nursing staff, only 11 (55%) were reviewed daily by a consultant and only four (20%) were reviewed at least four to six hourly by a registrar or consultant.

These data suggest that nursing staff on general medical and surgical wards identify a significant number of patients whom they feel warrant admission to a high dependency or intensive care unit. Worryingly, most of such patients identified during this audit were not reviewed regularly by experienced medical staff.

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Checklist may help improve referral

EDITOR—McQuillan et al[1] highlighted an important question facing hospitals today—namely, how can patients receive a tailored continuum of care in the face of the effects of Calman training and the pressure to reduce ward nursing numbers and grades? Their recommendations, although exhaustive, are not all achievable within an acceptable time frame. Individual hospitals must initially find a solution that is locally achievable within present resources.

After a critical incident involving a patient admitted through the accident and emergency unit to a medical ward and belatedly referred to intensive care we compiled a list of conditions for which senior medical and intensive care advice must be sought (box). Unlike most guidelines these do not dictate a clinical pathway but serve as a trigger for more senior involvement in the management of patients at an earlier stage. A second major difference was the involvement of intensive care staff for patients that may not necessarily

Patients who must be referred for intensive care advice

- All patients with suspected meningococcal septicaemia
- Poisoned patients with altered level of consciousness and arrhythmia, including tachycardia (120 beats/min)
- Asthmatic patients who are not responding to maximal medical treatment, are becoming exhausted, or have a high normal carbon dioxide pressure
- Status epilepticus (seizure activity 30 minutes)
- Patients with signs of inhalation injury (oxygen saturation is unreliable)
- Patients with unstable facial fractures
- Victims of near drowning
- Cerebrally agitated patients with brain contusion, undiagnosed hypoxia, or poisoning
- Head injured patients with Glasgow coma score <10 or rapidly falling

require ventilation but need correction of their physiological parameters. Since the implementation of these guidelines referrals to intensive care have been earlier and appropriate. We plan to augment this list with physiological variables[2] and distribute it to the acute medical and surgical wards.

With the increasing subspecialisation of general medicine the management of medical emergencies has been sidelined. This has occurred at a time when the specialty of accident and emergency medicine is beginning to come of age. All undiagnosed emergency patients should be admitted to hospital through accident and emergency departments so that an accurate assessment and appropriate transfer can be made.

The recent disquiet at unfavourable clinical outcomes makes it increasingly untenable to rely on cardiac arrest teams and intensive care units to salvage ward patients near to death. Time to put systems in place to ensure the matching of health care to the continuum of illness is one thing we do not have.

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1 McQuillan P, Pilkington S, Allan A, Taylor B, Short A, Morgan G, et al. Confidential inquiry into quality of care before admission to intensive care. *BMJ* 1998;316:1853-8. (20 June.)
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More intensive care beds are needed

Editor—McQuillan and colleagues show that patients often receive suboptimal care before admission to intensive care.[1] We would like to highlight another factor adversely affecting the care of patients. This is the frequency with which hospitals cannot admit patients to their own intensive care unit because of a shortage of staffed and available beds.

In the Northwest region each day an average of three patients are transferred to another intensive care unit. This can rise to nine a day during peak periods. All intensive care units in the Northwest region are contacted four times daily by the Intensive Care Bed Information Service to ascertain bed availability. When only 10 of the 183 adult general intensive care beds remain available an amber alert is declared by the NHS Executive Northwest Regional Office, and this information is faxed to all trusts. When only five beds remain a red alert is declared. During June amber alert conditions were met 17 times and there were six occasions when a red alert could have been issued. The true situation is worse since paediatric and specialist services are not included and there are no alerts at night or weekends.

We audit transfers against published standards[2 3] and over the past two years have clearly shown that transfers are increasingly caused by a lack of staffed intensive care beds in the host hospital. Transfers for this reason have increased by 300% in Greater Manchester and 200% in the rest of the Northwest.

Despite close liaison with local and regional managers, the health authorities appear unable to address the fundamental issue of insufficient investment in intensive care and high dependency units in the northwest of England. Political direction is aimed at reducing waiting times for elective surgery.

Unless McQuillan and colleagues' strategy to improve the care of the acutely ill patient succeeds, in the absence of sufficient high dependency and intensive care beds, it seems inevitable that patients will continue to be transferred unnecessarily.

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Medical training should focus on basic skills

Editor—Two recent articles have suggested that care is suboptimal on NHS general medical and surgical wards.[1 2] McQuillan et al showed that over 50% of admissions to intensive care may have been avoidable with improved care in the preceding hours and days. Smith and Power[2] reviewed a recent Audit Commission report that showed problems with provision of pain control after surgery. Both suggest that changes in organisation and service provision are required.

The common denominator in these (and many other) issues is not a lack of care but an inability of nursing and medical staff to give effective treatment. One aspect of this is insufficient resources. Effective monitoring, treatment, and review of acutely ill and postoperative patients takes considerable time. This time is not available within the current funding. Many are already fighting to improve this situation.

The second and perhaps more fundamental aspect is that of training. McQuillan and others have noted poor application of fundamental principles of airway, breathing, and circulation; pain control; physiology; etc. Care of emergencies and basic acute care, postoperative care, and pain control are bread and butter for senior and preregistration house officers. Yet we are increasingly seeing how inadequately medical training prepares us for this.

Rather than increasing consultant input and specialist teams, surely it would be more effective to train our medical students in these skills early on. Current training prepares well for exams but leaves students ill prepared for meeting the needs of patients. I had minimal practical training in spotting the signs of a patient in physiological decline. Thus junior doctors may discuss the intricacies of the surgery on the consultant ward round while the patient travels further into renal failure. This is not a failure of care by them (although would be seen as such by the public and the court) but of their training.

I had to wait six years after qualifying to have the opportunity to be taught how to recognise a sick child and to give the treatment needed while waiting for further help. Most medical students can quote all the causes of polyarteritis nodosa (which they may never see) but few of electromechanical dissociation (which they will see often). This list is almost endless.

Although pain, intensive care, and anaesthetic specialists will always be required to intervene with ward patients, they should need to be called only when basic measures are already well under way. Most aspects of basic monitoring; maintaining airways, breathing, and circulation; fluid management; and pain control should be well within the ability of properly trained students by the time of qualification.

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Course is available for surgical trainees

Editor—McQuillan et al and the accompanying editorial document current deficiencies in critical care.[1 2] Efforts to improve surgical critical care—that is, management of emergencies and unexpected complications and perioperative care of patients having major surgery—have been under way for some time. Four years ago, the Royal College of Surgeons of England commissioned a working party of intensivists, anaesthetists, and surgeons to develop a consensus programme to improve training in surgical critical care for junior doctors. A practical three day course on the care of the critically ill surgical patient (CCrISP) has been developed which deals specifically with many of the deficiencies identified in the articles.

The aim of the course is to try to prevent surgical patients deteriorating—often to the point where they require intensive care—by identifying and correcting problems early. The course emphasises the use of a system of assessment to avoid simple errors which account for many avoidable adverse episodes. The system begins with the correction of airway, breathing, and circulation but moves rapidly on to the identification and treatment of the underlying cause. Candidates learn this system, discuss it in a range of realistic clinical scenarios, and then practise it on simulated patients. Candidates read a course manual beforehand and through

lectures and practical sessions cover theory and practice necessary for surgical critical care in the ward or high dependency unit. Topics include monitoring techniques, nutrition, sepsis, renal failure, communication, and pain management in addition to detailed control of airway, breathing, and circulation. Calling for help and seeking timely senior input is emphasised throughout.

The college has run the course successfully for two years, and it has now been established at Hope Hospital in Manchester and in Leeds. Feedback from candidates three months after their course shows that 85% were influenced considerably in their approach to critically ill patients and that 90% used the advocated system of assessment frequently. Six other centres are establishing the course in their region. Many postgraduate deans have indicated their support, and the college has advised that the course is highly recommended for all basic surgical trainees. Trainees from other disciplines may benefit from similar courses.

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Medical emergency teams improve care

EDITOR—McQuillan and colleagues suggest using medical emergency teams to help overcome deficiencies in acute care.[1] Our unit pioneered this concept and a medical emergency team system has operated since 1990.[2] The identification of patients early on in their physiological deterioration is intuitively sensible; the potential benefits are outlined by McQuillan and colleagues. Once such patients are identified, however, there must be provision to monitor them more closely and use treatments that cannot be safely provided on a normal ward. In short, medical emergency team systems must be coupled with adequate high dependency unit facilities.

Our hospital has a total of 532 beds, with eight ventilated intensive care beds and 12 high dependency beds. Of 493 responses by medical emergency teams in 1997 (only 10% for cardiac arrests), 92 (19%) resulted in patients being admitted to intensive care or high dependency units. Thus, the teams not only identify deteriorating patients but take intensive care expertise to the wards.

The parlous state of intensive care bed provision in the United Kingdom is well known.[3] High dependency provision is at best patchy or, if available, caters solely for single specialties such as neurosurgery. If Britain is to address seriously the issues raised by McQuillan and colleagues, creation and expansion of high dependency facilities will be required.

Adverse effects of the medical emergency system include deskilling of ward medical staff. This can be ameliorated by having trainees rotate through intensive care. Deskilling of ward nursing staff does occur, and this risks an increase in the number of calls to medical emergency teams and greater need for high dependency unit facilities as staff become uncomfortable and unwilling to manage sick patients on the ward. Resistance from primary specialty consultants to the transfer of patients to high dependency units is also a concern that needs addressing.

The cost effectiveness of this approach is difficult to quantify. Savings may come from reduced admission to intensive care and length of stay. Irrespective of this, however, we believe that the system improves quality of care for our sickest patients. McQuillan and colleagues show that this is desperately needed.

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Authors' reply

EDITOR—Gorard and Walshe criticise the method and analysis of data in our study. We based our methods closely on confidential inquiries such as those into perioperative and maternal mortality in which outcome is evident to assessors and definitions of quality of care are not predetermined. As medicolegal cases attest, disagreement between experts is common. More assessors and greater training may not improve interrater reliability.

Our study was conceived to develop a tool to assess quality of care before admission to intensive care. Pilot studies are rarely perfect first time. McGloin et al confirm our findings (blinding assessors to outcome, not allowing interobserver disagreement); 37% of their patients received suboptimal care with a significantly increased mortality.[1] Despite imperfections, these studies are compelling and concur with the experience of most British intensivists and other clinicians. As intensive care is required for about 1% of patients, about 0.5% of people admitted to hospital may receive suboptimal care.

Wood and Smith's findings confirm previous studies showing that 60-80% of patients who have cardiorespiratory arrests show premonitory signs. Amalgamation of data on 33 612 patients from three large UK databases[2] (Intensive Care National Audit and Research Centre, Critical Care Audit, personal communication) shows that cardiopulmonary resuscitation occurs within 24 hours of admission to intensive care in 3.5% of patients referred from theatre or recovery (mortality 49.5%), 14.3% of accident and emergency referrals (mortality 65.1%), and 15.9% of ward referrals (mortality 73.3%). Thus ward patients may be exposed to a high risk of avoidable cardiorespiratory arrest which carries a particularly grave prognosis.

Youngs and Gorard suggest that staffing shortages contribute to suboptimal ward care. However, greater ward presence requires more staff or reorganisation of work patterns. Consultant expansion has increased subspecialisation and diluted on call rotas. There is little evidence of increased consultant involvement in the care of acute patients or in teaching the necessary skills to trainees, despite rising numbers of emergency admissions and the effects of the Calman recommendations. Ringrose and Garrard found that few sick patients were reviewed daily by consultants. Contracting, competition, and waiting list initiatives have overemphasised elective work, leaving conflicting pressures between elective and emergency duties. Improvements in quality of acute care, an integral part of the government's white paper *The New NHS,* may not be possible without reducing elective workload.

The Royal College of Physicians[3] seems to share McAllister and McGovern's concern that subspecialisation has sidelined acute general medicine. Mechanisms to ensure alerting of the intensive care team or an appropriate acute care physician are important developments but should not be an alternative to the responsible consultant being part of the receiving team. Too often the first consultant input occurs on the "post-take" round. In many hospitals consultants do the elective work and trainees deal with the emergency workload, often with little or no supervision.[4]

Our assessors identified that delays in admission to intensive care were caused by late referral and not bed availability. We believe that, even when all beds are occupied, the intensive care unit has a responsibility to ensure that other critically ill patients receive appropriate and timely care. Essential intensive care interventions can be initiated on the ward. Once stabilised, the patient may be transferred to another intensive care unit. This ensures that appropriateness of intensive care takes precedence over local bed availability.

It is time to challenge the traditional view of the intensive care unit as an isolated area of technological medicine and to develop the role of the intensive care team into a critical care service central to hospital acute medical care.[5] Pritchard's call for improved training echoes our belief that training in critical care management should begin at undergraduate level and involve critical care doctors as teachers. This can be consolidated by postgraduate courses such as described by Anderson and Rowlands. Improved early intervention, using systems such as the medical emergency teams and "calling criteria" outlined by McAllister and McGovern, would then dovetail with high

dependency and intensive care units to provide a seamless acute care service.

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Complaints of pain after use of handcuffs should not be dismissed

Editor—Handcuffs are commonly used to restrain prisoners. It is not unusual for them to be applied in violent circumstances and for the prisoner to struggle. This can lead to overtightening of the handcuffs and considerable trauma to the structures around the wrist. We have recently seen fractures, lacerations, and injuries to the radial, ulnar, and median nerves (table). This is probably the tip of the iceberg, as many people with such injuries fail to attend for assessment, follow up, or investigation.

Superficial radial handcuff neuropathy is the most common injury,[1-3] although injuries to the median, ulnar, and multiple nerves have all been described.[4 5] Nerve conduction studies both confirm the organic basis of the patient's complaint and help to define the prognosis. Fortunately most lesions are not degenerative.

Kwik-cuffs, the most commonly used handcuffs in the United Kingdom, are applied by allowing the cuffs to spring shut on a ratchet. This can lead to direct trauma and allows overtightening to occur. We postulate that bony injuries are caused at the time the cuff is applied or by levering on the cuffs afterwards, which causes a considerable torque at the wrist joint. While a double locking mechanism exists to limit further tightening of the handcuff, this may be omitted when the prisoner is violent or aggressive, or time is lacking.

Data on men arrested and handcuffed

Case No	Age (years)	Violent/ resisted arrest	Consumption of drugs or alcohol	Skin breach	Fracture	Nerve injury	Neuro-physiological examination	Outcome
1	37	Yes	Yes	None	None	Superficial radial—bilateral and right median	Confirmed lesions	Median nerve explored, nerve conduction tests recovered but symptoms persist
2	23	No	Yes	Severe bruising	Radial styloid	Superficial radial—bilateral	Confirmed lesions	Full recovery
3	69	Yes	Yes	Lacerations, extensor carpi ulnaris tendon pain	None	Ulnar—dorsal sensory branch	Failed to attend	Still unable to grip or work 1 year after injury
4	46	Yes	No	None	None	Ulnar and superficial radial	Confirmed lesions	Required ulnar nerve exploration
5	25	Yes	No	None	Scaphoid	None	Not performed	Required fixation
6	27	Yes	Yes	None	None	Superficial radial	Failed to attend	Failed to attend follow up
7	32	Nil	Nil	None	None	Superficial radial—bilateral	Confirmed lesions	Function returned to normal at 2 years but still had sensory symptoms
8	46	Nil	Yes	Grazes	None	Bilateral median nerve injury	Not performed	Failed to attend follow up
9	45	Possible	Nil	Local abrasions and swelling	None	Superficial radial—bilateral	Confirmed lesions	Improving at 10 weeks then stopped attending
10	34	Nil	Nil	Local scarring	None	Superficial radial—bilateral	Confirmed lesions	Still symptomatic at 5 weeks
11	38	Nil	Nil	None	None	Superficial radial	Not performed	Full recovery

Police officers are aware of the potential dangers. Kwik-cuffs are used only by those who have received the relevant training. Officers are nevertheless encouraged to use them to maintain control and for self protection. Moreover, they are instructed not to remove or adjust handcuffs until a safe controlled environment is reached. This may mean that detainees' complaints of overtight handcuffs are addressed only after a considerable time.

It is probably inevitable that any restraint procedure offering reasonable safety for the police force entails a potential risk for those who lash out against the restraining structures applied to the wrist. It would be difficult to implement other ways of detaining them, although greater awareness of the possibility of handcuff related lesions may lead to an earlier reappraisal once events are proceeding in a controlled manner.

Complaints of pain, sensory symptoms, or weakness after use of handcuffs should not be dismissed. While neuropraxia of the radial nerve may not lead to motor dysfunction, it can none the less be persistent and severe. Damage to the ulnar or median nerve and fractures can be extremely debilitating.

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Electronic preprints can be categorised

Editor—McConnell and Horton have put a critical issue on trial: the role of scientific journals in the age of the internet.[1] Posting protocols, results, and preliminary "publications" on an individual or institutional website allows for intellectual discussion and a healthy exchange among coworkers internationally. Working by fax or email does not allow for the same level of flexibility or impact. Penalising authors for this practice by not considering for print the articles that have been posted on private websites stands in the way of true scientific progress in this era of internet democracy. On the other hand, I agree wholeheartedly with Kassirer and Angell that the indiscriminate distribution of non-peer reviewed articles could have a harmful impact.[2] People, be they doctors or the lay public, have a tendency to believe what they see in print, especially if they happen to see it on the website of a reputable scientific journal like the *BMJ*, *Lancet*, or *New England Journal of Medicine*. The reputation of both the journal and the internet could be damaged.

To resolve this unfortunate but inevitable dichotomy, I propose we classify

electronic preprints—"eprints"—into four categories:

- The electronic draft (e-draft), material posted at an individual or institutional website that is used for collaborative purposes within the medical community but not for public consumption;
- The electronic preprint (e-preprint), completed journal articles that have been peer reviewed, accepted, corrected, and are awaiting publication in hard copy. This material could be put on the journal's website for everybody's consumption and comment;
- The electronic letter (e-letter), electronic correspondence that can be posted almost immediately on receipt. This keeps the discussion current, topical, and vibrant; and
- The electronic print (e-print), the electronic version of the printed article, which would be located within the appropriate electronic journal (e-journal) with its volume and page numbers.

Far from seeing the imminent death of biomedical journals,[3] I perceive an ever increasing role for them as the last bastion of properly filtered (peer reviewed) information. In a world where anyone can post any material on the information superhighway, practising clinicians and researchers alike need an oasis where they know there is "somebody to select, filter, and purify research material and present them with a cool glass of clean water."[4]

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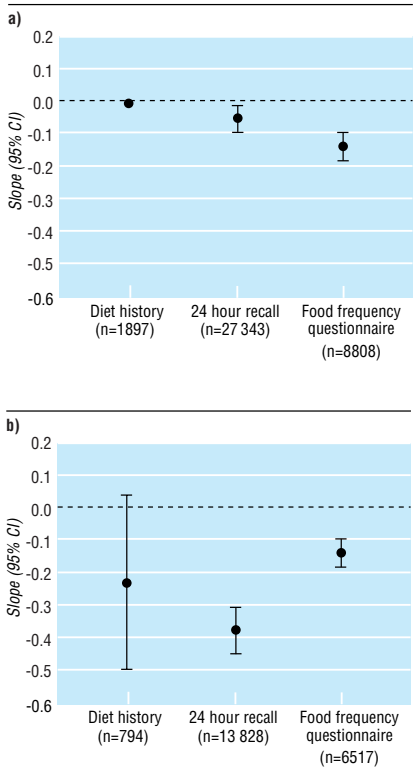
Meta-analyses of observational data should be done with due care

Editor—In our review of meta-analyses of observational studies we pointed out that all these are susceptible to all the biases inherent in observational research[1] and that it is easy to generate seemingly plausible explanations for findings of observational studies that are in fact spurious.[2] Birkett's critique of one of our examples illustrates these points.[3]

Cappuccio et al showed a weak inverse association between calcium intake and blood pressure.[4] Stratified analysis showed that the studies in which food frequency questionnaires were used showed a much greater association than the studies in which diet history or 24 hour dietary recall were used (figure, top). Cappuccio et al argued that this could be expected since food frequency questionnaires assess habitual diet and long term calcium intake was likely to be the important factor influencing blood pressure.

Birkett showed that for one study included in the meta-analysis standardised regression coefficients (the difference in blood pressure associated with a standard deviation difference in calcium intake) were taken to be regular regression coefficients (the difference in blood pressure associated with 100 mg difference in dietary calcium).[3] Since the standard deviation of calcium intake is more than an order of magnitude less than 100 mg this led to the inclusion of erroneous data and to one of these studies taking over 99% of the weight of the meta-analysis of food frequency trials. Correcting the meta-analysis for this error (and several other mistakes) leads to a different picture (figure). There is no suggestion that the seemingly plausible explanation for differences between studies in which different dietary methodologies were used holds true.

Meta-analysis can distance readers from original data and leave them dependent on the care (or lack of care) taken by the meta-analysts. Plausible but spurious reasons for differences found between groups of trials can easily be generated. Had Cappuccio et al avoided the errors pointed out by Birkett, they might have produced an equally plausible explanation for differences in the opposite direction. They could have argued, for example, that food frequency questionnaires are less accurate than 24 hour recall, thus leading to weaker associations.



Relation between dietary calcium and systolic blood pressure by method of dietary assessment. Erroneous data published by Cappuccio et al[4] (panel A) and corrected analyses by Birkett[3] (panel B). Slopes with 95% confidence interval (mmHg/100 mg dietary calcium)

Examples of misleading meta-analyses of observational studies should not lead us to conclude that a return to subjective narrative reviews is warranted. Any worthwhile review should be systematic and employ strategies to avoid bias, but the statistical combination of studies is rarely appropriate in observational research. A clearer distinction is needed between systematic reviews and meta-analysis to prevent the former being discredited by poor versions of the latter.

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Infantile spasms and vigabatrin

Study will compare effects of drugs

Editor—In his letter about the use of vigabatrin in children Appleton reports the consensus guideline from a "paediatric advisory group."[1] The longer version of Appleton's letter (on the *BMJ* website) includes the information that "the advisory group [was] supported by an educational grant from Hoechst Marion Roussel," the manufacturer of vigabatrin, but the letter in the paper journal does not say this. The guideline states that "vigabatrin currently remains the drug of choice for infantile spasms." Many paediatricians and paediatric neurologists, in both the United Kingdom and North America, would dispute this statement and continue to use tetracosactrin (ACTH) or prednisolone (or prednisone) as first choice.

The only published randomised controlled trial comparing vigabatrin and ACTH as first line treatment in infantile spasms showed cessation of seizures in 48% and 74%, respectively, of 42 cases analysed by intention to treat (difference 26% (95% confidence interval – 3% to 54%)),[2] which seems to exclude a significant treatment advantage for vigabatrin. Side effects (drowsiness, hypotonia, and irritability in the vigabatrin group; irritability and hypertension in the ACTH group) were seen in 13% and 37% respectively (24% (– 2% to 50%)). Estimates of side effects are difficult to interpret, especially as hypertension was not clearly defined and visual field defects due to vigabatrin cannot be detected in infants.

The United Kingdom infantile spasm study has received approval from a multicentre research ethics committee to study the epidemiology of infantile spasms and to compare the effects of vigabatrin, tetracosactrin, and prednisolone in a randomised

clinical trial. Outcomes to be studied include cessation of seizures, improvement in the electroencephalogram, and neurodevelopmental progress by 12-14 months of age. Infants with definite or possible tuberous sclerosis will be excluded from the drug trial as we (the steering committee of the study) believe that vigabatrin is the first choice for treatment of infantile spasms due to tuberous sclerosis.[3] Infants with tuberous sclerosis will, however, be included in the epidemiological study.

At present, the limited evidence suggests that steroid treatment may be more effective, albeit with a clinically important risk of hypertension. There is also concern about possible unmeasured adverse effects of both steroids and vigabatrin. This leaves most paediatricians and paediatric neurologists in a state of equipoise with respect to these treatments and requires a large randomised trial to provide more precise estimates of the size of treatment effects and adverse effects.

The United Kingdom infantile spasm study will be of sufficient size to achieve this, although it will not provide a short term answer with respect to visual fields. Any treatment effects will have to be weighed against emerging data on drug safety.

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**It was an error that the competing interest was not made clear in the version of the letter published in the paper edition of the journal. We apologise and take this opportunity to remind authors that there is nothing wrong with having a competing interest but there is with not declaring it.—Editor, *BMJ*

Visual field defects may be permanent

EDITOR—We welcome Appleton and colleagues' recommended guidelines for the use of vigabatrin in children[1] in view of the recent finding of visual field defects in association with treatment with this antiepileptic drug.[2] Similar guidelines are needed in adults, but can be drawn only on the basis of a properly designed clinical study to answer specific questions about this side effect. It is still not known if this is dose related or if it is related to duration of exposure. It is also not yet known if certain age groups are more susceptible to it, if there is an adverse synergism with other antiepileptic drugs, and if these visual field defects are reversible on withdrawal of treatment.

There is currently insufficient evidence (at least in adults) to say that if no visual problem is apparent after two years of treatment it will not occur. In three cases, symptoms started after 28, 37, and 38 months of treatment,[2] and it is impossible to assess the precise moment at which these first began. There is anecdotal evidence that duration of treatment might be an important factor, with problems more likely to be found in patients receiving chronic treatment. Rather paradoxically, the authors of the guidelines also speculate that six months may be too short a period for the development of visual field defects. Reports so far have suggested that visual field defects in adults may occur well before six months. In one report, visual symptoms started after 2-38 months of treatment.[3] The reversibility of visual field defects, whether in adults or children, is uncertain.

Though a panicked and hurried withdrawal of the drug might have adverse consequences, visual deficits may be permanent, thus necessitating vigilance and close follow up when the drug is prescribed. Guidelines are extremely important in this context and those laid down by this panel need to be strengthened and revised on the basis of adequate studies.

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Sex education should begin in primary school

EDITOR—We read with interest the news story about the ChildLine report in which children expressed concerns about puberty.[1] We were especially concerned about the dearth of information and support provided by the children's parents.

During the past century, there has been an acceleration in the rate of physical growth of children and adolescents, leading to faster and earlier maturation. There has also been a downward trend in the age of menarche by about four months per decade since 1850.[2] However, it seems that neither parents nor schools have made any effort to respond to these changes. Consequently, the concerns expressed by the children who contacted ChildLine are common.

In the United Kingdom, there is no statutory requirement to teach sex education in primary school. Some schools provide information the year before pupils leave for secondary school, when the children are 11 years old. In some instances, information is directed only at the girls and the boys are left out of discussions completely. Within families boys tend to receive less information about sex than girls.[3] Among boys, this may result in the attitude that girls are responsible for birth control.

The high rate of pregnancy among teenagers is evidence of early sexual activity. This rate has risen since 1980, and nationally the rates for 1996 show a further increase of 11%.[4] A high number of abortions are performed on girls younger than 16; between 1992 and 1994 in Walsall 30 girls younger than 16 had abortions.[4]

There is also a lack of understanding of sexuality among teenagers. The West Midlands young people's lifestyle survey, which collected information from 27 000 children between 11 and 16 years of age, reported that 26% (146/562) of year 9 pupils (aged 13-14) did not know that they could become pregnant the first time they had sex.[5]

Parents and governors of schools must understand the importance of providing good quality, appropriate sex education at an earlier age. In Walsall we have started working with primary school teachers to raise awareness of these issues and to plan an appropriate programme of sex education.

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Improved command of English may explain why non-English speaking countries get more published

EDITOR—Wise reports that the United Kingdom maintains its research position in the world despite reduced funding.[1] An interesting feature of the accompanying graph in her article is that the United States' share has gone down; the United Kingdom has more or less maintained its ranking; whereas non-English speaking countries such as France, Italy, Germany, and Japan have improved their positions over the past 10 years.

The Wellcome data on which this report was based count the percentage share of

science publications. Since most of the important journals are in English, authors' command of this language becomes important in having an article published. With this confounding in the background, the only inference that can be made from the graph in Wise's article is that the level of English has improved in non-English speaking countries in the past 10 years. This is supported by the recent audit by the *BMJ* of its publications during 1990-7 (which found a 40% share of publications between United Kingdom and Britain in 1996).[2]

It is important to keep this issue in mind before we start assuming that the number of publications accurately reflects the level of scientific research in the world and so make even further cuts in research funding.

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Is it time for an Illich Collaboration to make available information on the harms of medical care?

EDITOR—The editor's choice on the dark side of medicine[1] has prompted me to ask if it is perhaps time to establish an Illich Collaboration (on the lines of the Cochrane Collaboration) to make readily available objective evidence of harms of medical care. The harms go beyond the side effects of drugs or inappropriate use of high intensity treatments to the social and psychological consequences of unchecked medicalisation of the aspects of modern life of which Illich warned.[2] Archie Cochrane's salutary challenge for proof of effectiveness to the medical profession[3] took nearly a quarter of a century to be taken seriously. Illich's challenge appeared again in the perceptive warning in the last chapter of Roy Porter's brilliant account of medical history, in which he writes that the irony is that the healthier Western society becomes the more medicine it craves.[4] His allegation is that doctors and consumers are becoming locked within a fantasy that all people have something wrong with them and that everyone and everything can be cured.

It would be naive to leave Illich's medical nemesis in the category of "doctor bashing" literature because unless the arguments are taken seriously they are likely to return as greater challenges. Today, with the ever higher expectations of society, professional error or incompetence, drug side effects, and unexpected harms of medical intervention are tragically more significant.

A new debate is urgently needed about the fundamental issue of the meaning of health and health care and the need to redefine consumer expectations. It is questionable whether market consumerism can be translated to cover healthcare issues. A readily accessible source of information in the Cochrane Library (or elsewhere) of harms as well as effectiveness[5] for healthcare professionals, the users of care services, and society at large would inform a responsible debate about the appropriate use of care services and the need to avoid unnecessary interventions to avoid harm to individual people and waste of resources to society.

A foundation of mutual trust necessary for this debate would be encouraged by the idea of stakeholder altruism—that is, joint ownership and responsibility for the NHS—so that reciprocal altruism can be a sustainable reality.

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Clinical outcome in relation to care in centres specialising in cystic fibrosis

Cross infection with *Pseudomonas aeruginosa* is unusual

EDITOR—Mahadeva et al's paper provides valuable evidence of benefit for paediatric and adult patients attending specialised cystic fibrosis centres.[1] Their conclusion, however, that the mean age at colonisation with *Pseudomonas aeruginosa* is lower in patients who received paediatric and adult care in a centre (11.1 years; group A) than in patients who received adult but not paediatric care in a centre (18.1 years; group B) cannot be accepted on the basis of the data presented.

Early detection of infection with *P aeruginosa* is a major preoccupation for staff working in paediatric cystic fibrosis centres. Colonisation is associated with a rapid decline in pulmonary function, but it can often be delayed or prevented,[2] and long term use of nebulised antibiotics improves the prognosis if it does occur.[3]

Mahadeva et al define colonisation using sputum culture. Most young children with cystic fibrosis are unable to produce sputum so that paediatricians have had to depend on cough swabs, with a high rate of false negative results on culture. Bronchoscopy has only recently been used to obtain specimens in children who have symptoms that do not have an obvious explanation.[4]

Patients in group B were presumably managed solely by general paediatricians, and their treatment would therefore usually have been less aggressive than that in group A. It is likely to have been reviewed less frequently than in the paediatric centre, and neither sputum nor cough swabs are likely to have been sent after every visit to the clinic. Many of these patients may have been colonised with *P aeruginosa* for years without their doctors knowing—*P aeruginosa* is commonly detected for the first time shortly after children who have been managed solely by a general paediatrician are referred to a cystic fibrosis centre. The mean age for detection of colonisation in patients not previously managed in paediatric centres (18.1 years) is the age at which most patients are referred to adult centres, so colonisation must have actually been diagnosed at the adult centre in half of these cases.

Cross infection with *P aeruginosa* between patients attending paediatric cystic fibrosis centres is unusual and has only been proved for an unusual multiresistant strain.[5] The fear of cross infection in patients and parents is high, and it would be regrettable if these findings were to be misinterpreted as evidence that attending paediatric clinics is hazardous.

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Author's reply

EDITOR—We agree with Spencer that our findings should not be interpreted as suggesting that attending centres specialising in cystic fibrosis is hazardous. On the contrary, our data suggest that despite an apparent earlier age at colonisation with *Pseudomonas aeruginosa*, care at a centre yields better results in terms of lung function and body mass index—whether related to better surveillance, more aggressive eradication treatment for *Staphylococcus aureus*, or the possibility of cross infection.

As Spencer points out, cross infection has been shown only with a multiresistant strain of *P aeruginosa*. Some centres, however, have adopted a policy of segregating patients colonised by *P aeruginosa* from those colonised by *S aureus*, with the specific aim of minimising any risk of cross infection and possibly delaying colonisation by *P aeruginosa*. Our study was not designed to address this question and should not be regarded as hard evidence in the debate. Further study into the possibility of delaying colonisation by *P aeruginosa* by segregating patients is required so that practical evidence based recommendations can be made.

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Treatment of gastro-oesophageal reflux disease in adults

Efficacy of surgery needs to be compared with that of proton pump inhibitors

EDITOR—Galmiche et al reviewed the treatment of gastro-oesophageal reflux disease in adults and claimed that surgery is an efficient treatment with a success rate of up to 90%.[1] Among the few available controlled trials comparing surgery with medical treatment they cited the study of Spechler et al, which showed that surgery is more effective than medical treatment in improving symptoms and oesophagitis for up to two years.[2] Unfortunately, this trial is flawed. At the time of the study the most efficient drugs (proton pump inhibitors) were not available, and the medical arm used ranitidine, metoclopramide, and antacids. Altogether 247 patients were included, but after randomisation 40 of them refused to participate, 32 of them being allocated to the surgery group. Follow up data were available at two years for only 106 patients, which invalidates all the results. The grade of oesophagitis (range 1-4) on endoscopy in the surgery group and in the continuous medical treatment group was better at two years (1.5 (SD 0.2) and 1.9 (0.1) respectively) than at baseline (2.9 (0.1) for both groups). But no direct statistical comparison was made between the two groups. The patients' satisfaction was also assessed; this was in favour of surgery. This result tells us little, since it was evaluated by a technician aware of the treatment received by the patients. Lastly, an activity index score (range 74-122) was better in the surgery group (78 (2)) than in the continuous medical treatment group (88 (2)). This evaluation was also not blinded, and the authors did not discuss the clinical relevance of a 10 point difference. This trial cannot be taken into consideration.[3]

Another controlled trial, which Galmiche et al did not cite, compared ranitidine 150 mg twice daily with fundoplication and concluded that surgical treatment was superior.[4] This trial also gives rise to major criticisms: only 31 patients were included, no randomisation or blinded evaluation was carried out, and the ranitidine and surgical groups were not compared.

With the availability of powerful proton pump inhibitors, the notion of refractory oesophagitis tends to disappear.[5] Indications for surgery are now mostly limited to recurrent oesophagitis in young patients refusing continuous treatment. But the efficacy of surgery still needs to be proved in comparison with that of proton pump inhibitors.

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1 Galmiche JP, Letessier E, Scarpignato C. Treatment of gastro-oesophageal reflux disease in adults. *BMJ* 1998;316: 1720-3. (6 June.)
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"Step down" strategy of treatment would be expensive

EDITOR—We are concerned by Galmiche et al's assertion that, for patients with mild to moderate gastro-oesophageal reflux disease, the "step down" strategy (starting treatment with proton pump inhibitors) may be more cost effective than the traditional "step up" strategy (starting treatment with less powerful interventions).[1] The evidence quoted in support of the step down approach was a modelling analysis undertaken in the context of the American healthcare system that specifically excluded patients with mild disease. A more recent empirical analysis of prescribing in mild to moderate gastro-oesophageal reflux disease in the United Kingdom emphasised the comparative cost effectiveness of the step up approach.[2]

The cost effectiveness of the step down approach would be even more uncertain were it to be applied routinely in primary care. Most patients presenting with heartburn and associated symptoms are managed by primary care doctors without recourse to specialist advice. Treatments are normally started empirically, and only when these are unsuccessful will an endoscopy or specialist advice be sought. Thus the step up approach is particularly relevant for such patients. In contrast, gastroenterologists treat a highly selected cohort who bear little resemblance to most patients presenting in primary care, and they need to bear this in mind when making recommendations.

The advantage of proton pump inhibitors is only that they can be used in more severe disease. The evidence quoted by Galmiche et al in support of their use in mild disease is a short term placebo controlled trial, but substantial evidence exists in favour of first line use of less powerful acid suppressants. One meta-analysis identified more rapid healing with a proton pump inhibitor than with histamine receptor antagonists in patients with gastro-oesophageal reflux disease but included patients with more severe disease[3]; the more detailed analysis of the literature undertaken by Kahrilas indicates that mild disease is equally well managed by proton pump inhibitors or the alternative drug treatments available.[4]

Proton pump inhibitors are already the most costly drugs for the NHS. There is substantial evidence of their overuse when less expensive drugs might be equally effective.[5] Stepped down treatment depends on careful clinical review and patient education, and the deficiencies of repeat prescribing in this regard are well recognised. The danger of a recommendation to use high dose proton pump inhibitors more generally as first line treatment is that many patients will receive drugs that they do not require, in either the short or the long term. The wasted costs to the NHS would be substantial.

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Authors' reply

EDITOR—The results of a large randomised trial comparing open antireflux surgery with maintenance treatment with omeprazole are now available.[1] They confirmed that both treatments were effective, allowing patients' quality of life to return to normal within two months. Analysis of time to failure of treatment, however, showed a significant superiority of surgery after three years of follow up. Therefore, despite the methodological biases emphasised by Chassany et al, all controlled trials have concluded that open antireflux surgery is more effective than traditional treatment with H_2 receptor antagonists and at least as effective as maintenance treatment with proton pump inhibitors. A blind evaluation, as suggested by Chassany et al, is difficult because a sham operation is not feasible ethically. Now seems an appropriate time to compare laparoscopic surgery and proton pump inhibitors and to include costs and quality of life as important end points.

Haycox et al dispute the merits of a step down approach. In our opinion, severity is the most important determinant of whether a step up or a step down strategy is appropriate. Severity applies to the entire range of the disease. A study in primary care clearly showed that quality of life is impaired in patients with gastro-oesophageal reflux disease with no abnormality at endoscopy as well as in reflux oesophagitis.[2] From a cost effectiveness standpoint there is no evidence to recommend either the top down or the stepwise approach.

We agree that a modelling analysis does not provide sufficient evidence in support of the top down strategy. However, Eggleston et al's study is also flawed as it is a retrospective analysis of data from a database of patients treated in the United Kingdom.[3] The reasons for choosing ranitidine, cisapride, or omeprazole for treatment were not specified. The efficacy of the different treatments and of their impact on quality of life were not evaluated. It is also difficult to

extrapolate the results of cost effectiveness analysis to countries with different healthcare systems. Finally, the cost of drugs may change dramatically when, for instance, drugs such as omeprazole lose their patent or additional competitors become available.

We agree that empirical treatment without referral to a specialist is the preferred approach in primary care. Bytzer et al showed, however, that this approach was associated with higher costs than management of dyspepsia guided by results of endoscopy and that patients treated empirically were more frequently dissatisfied.[4]

In conclusion, proton pump inhibitors are probably more cost effective in moderate or severe gastro-oesophageal reflux disease. The issue in mild disease needs further investigation.

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Communicating with patients

Specialist training should include communication skills

Editor—Blennerhassett eloquently describes the horrors of her treatment for anal cancer.[1] Her story is shocking but depressingly familiar from other patients' accounts of their treatment. The difference in the two commentaries published with the account throws some light on why these stories keep appearing.

The well referenced and scientific response from the cancer specialists Tattersall and Ellis reminded me of André Gide's remark that a person attempting to understand life by merely using his reason is like a man trying to take hold of a flame with the tongs. Blennerhassett's account does more than exemplify "the often slow and reluctant response of the medical profession to health 'consumerism.' " It powerfully describes the brutal results of poor communication. Tattersall and Ellis extinguish the flame of this message by trying to take hold of it with the tongs of science. The impact of the message is all but lost when the impersonal language of science is used: "Communication with the patient during consultation . . . is an important and sometimes overlooked component of optimal and efficient cancer care."

A different approach is taken by Metcalfe, formerly a professor of general practice, who does not use science to deflect Blennerhassett's emotional message. He acknowledges that the account "was a journey through hell" and that something went wrong. He addresses the need for changes in postgraduate training and makes useful suggestions for ways to approach consultations. He manages without a single academic reference. And so my stereotypes are confirmed: the generalist writes about the art of medicine and the specialists, the science.

The problem is, as Aneurin Bevan almost said, that I would rather have my cancer brutally but effectively treated than expire in a gush of sympathy. Is that stark choice from the 1940s still here, or can high technology and effective medical treatment now be delivered with understanding? Postgraduate training in communication skills is taken seriously in general practice. When will it be taken seriously in specialist training?

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1 Blennerhassett M. Truth, the first casualty. *BMJ* 1998;316:1890-3. [With commentaries by Tattersall M, Ellis P; Metcalfe D.] (20 June.)

Hostility can be a barrier to effective communication

Editor—It is regrettable and irresponsible of the *BMJ* to publish an uncorroborated, one sided account of allegedly poor communication on the part of doctors treating Blennerhassett.[1] As with patients' stories in the tabloid press, the *BMJ* has allowed Blennerhassett to say whatever she wishes, although the other side cannot be presented because of issues of patient confidentiality. Most doctors who read Blennerhassett's account will be familiar with her psyche: multiple complaints about many aspects of her care, and still complaining seven years later. It is impossible to be sure whether Blennerhassett's dissatisfaction could have been avoided. However much effort is made, some patients will never be satisfied. Hostility from a patient can be a barrier to effective communication. Healthcare professionals are an easy target for a patient having difficulty coming to terms with cancer and the morbidity of treatment.

Blennerhassett emphasises her concern about both the early and long term side effects of radiotherapy. The aim of radical radiotherapy for anal cancer is to avoid a colostomy. Presumably she would have been equally or even more critical of the impact of a colostomy had she had an immediate abdominoperineal resection instead.

The choice of Tattersall, Ellis, and Metcalfe as commentators was unfortunate as none practises clinical oncology in the United Kingdom. They do not understand the issues involved in the management of anal cancer and communication in the "real world." They discuss the importance of good communication which is, of course, critically important in the management of patients with cancer. As a group, oncologists are more aware than most of the importance of communication. There have been welcome improvements in undergraduate and postgraduate training in communication, and this trend must continue. However, for those of us who believe strongly in the importance of effective communication, the cause has not been advanced by the *BMJ*'s decision to publish Blennerhassett's extreme account.

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Conflict of interest: Taylor has not been involved in Blennerhassett's care at any time.

1 Blennerhassett M. Truth, the first casualty. *BMJ* 1998;316:1890-3. [With commentaries by Tattersall M, Ellis P; Metcalfe D.] (20 June.)

Author was never appointed to Bristol inquiry

Editor—In his editorial about regulation of doctors and the Bristol inquiry, Smith states that a doctor "was appointed to the public inquiry"[1]; this is part of his comment on an article by Barnes.[2] I regret that Barnes feels misled; in fact, he has misled himself. He was never appointed to the inquiry.

Should any individual or organisation wish to know anything about the inquiry's work, they should feel free to contact me. The inquiry has an internet website (BristolInquiry.org.uk), an email address (inquiry@doh.gov.uk), a special local rate telephone number (0845 3000613), and a fax number (0171 972 4602) to help to keep people informed about its work and to hear their suggestions and views.

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1 Smith R. Regulation of doctors and the Bristol inquiry. *BMJ* 1998;317:1539-40. (5 December.)
2 Barnes N. (Very) short service on the Bristol inquiry. *BMJ* 1998;317:1577-9. (5 December.)

⁂This response is insulting, pathetic, and disturbing. It's insulting because it suggests that Dr Barnes, a well respected and senior paediatrician, has "misled himself" and because it comes not from Ian Kennedy (the chairman of the Bristol inquiry) or a senior politician but the press officer. It is pathetic because it is wholly unconvincing and doesn't address at all the central issue of whether a doctor will be included on the inquiry. Even if there has been a "misunderstanding," those responsible for the inquiry could have used the articles as an opportunity to clarify what is happening. The response is disturbing because it does nothing to restore the collapsing credibility of the inquiry in the eyes of doctors.

The government may need to rethink this inquiry. What will be achieved by a two year inquiry that may have lost credibility before it even starts?—Editor, *BMJ*