Letters

More widespread public debate on rationing is essential

EDITOR—We find it significant that none of the clinicians who contributed to the conference "Rationing in the NHS: Time to Get Real" signed the open letter to Frank Dobson.[1] In the letter, rationing is defined as occurring when not all health services can be provided to everybody who might benefit from them, a definition so broad that it can be said always to have existed. Yet most British clinicians would not recognise the examples given as rationing. A young woman with high cholesterol as a single risk factor for coronary heart disease may be "denied" drug treatment and given dietary advice instead simply because the risks of side effects outweigh the marginal benefit; this is rational, not rationing. Again, "denying" magnetic resonance imaging for uncomplicated migraine in a young person has nothing to do with cost: the risks of iatrogenic anxiety and distress are far greater than any possible benefit from such intervention. The authors do the cause of so called smart rationing a disservice by these examples. This debate is too important to be "dumbed down" in this way.

Whenever rationing is discussed there is a tension between those whose professional discipline (health economics, medical ethics, or policy analysis) allows them to work with broad underlying principles and those who are constantly brought face to face with the limitations of these principles through daily contact with patients. The former are much more likely to believe that it is possible and appropriate to base all rationing decisions on a series of well argued principles. Life for the latter is far messier: it is often impossible to apply broad principle to individual cases, let alone find a "currency of comparison"[2] that will enable the claims of, for example, a dying older person on an understaffed geriatric ward to be ranked against those of a patient with multiple sclerosis who might benefit from interferon beta. Struggling to make these sorts of comparisons is painful; the examples given in the open letter tax no one and thereby offer false hope of easy solutions.

It is because the choices are difficult that more widespread public debate is essential. Only through such a debate can we all, as citizens, realise what is happening to the NHS, consider if that is the NHS that we want, and decide how much of our taxes an NHS worthy of the name deserves.

Iona Heath *General practitioner*
Stephen Amiel *General practitioner*
Caversham Group Practice, Kentish Town Health Centre, London NW5 2AJ

1 Kennedy I, Levvy G, Macara S, Maxwell R, Maynard A, Smith R, et al. Dear Mr Dobson . . . . *BMJ* 1997;315:147. (19 July.)
2 Klein R, Day P, Redmayne S. *Managing scarcity: priority-setting and rationing in the National Health Service.* Buckingham: Open University Press, 1996.

Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters, whether typed or sent by email, should give each author's current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

Patients' assessments of disability in multiple sclerosis

Most patients have difficulty in rating themselves on visual analogue scales

EDITOR—The recent finding that patients' self assessments on a visual analogue scale did not correlate with other measures of disability in multiple sclerosis comes as no surprise to us.[1] Visual analogue scales, which are used widely in studies of health status and quality of life, ask patients to rate themselves on a calibrated scale, usually from 0 to 100. The language of the end points or anchors of the scale varies, but, usually, 100 represents best possible health or quality of life and 0 represents the worst possible health or quality of life. In our experience, of several hundred interviews with patients with neurological disorders (epilepsy, Parkinson's disease, Gilles de la Tourette syndrome, and mild dementia), most patients express difficulty in rating themselves on such a scale. Some patients attempt to come up with a figure, but others are unable to do so and leave this question blank. The problems are multiple. For example, many patients with epilepsy are unable to express numerically the constant anxiety due to the unpredictability of the next seizure or occasional difficulties with finding words. Patients across all groups have difficulty reconciling such diverse subcomponents of wellbeing as good physical health but poor psychological wellbeing. The picture is further confounded in chronic illness, in which psychological mechanisms such as denial or coping can lead to patients rating themselves higher than healthy volunteers.[2] Self assessment of health status or quality of life is a complex process involving introspection and judgment using several cognitive processes.[3] While the apparent simplicity of using a visual analogue scale is seductive, many patients do not find the task easy. In these cases it is not clear what, if anything, is being measured.

Caroline E Selai *Research psychologist*
Michael R Trimble *Professor in behavioural neurology*
Institute of Neurology, London WCIN 3BQ

1 Rothwell PM, McDowell Z, Wong CK, Dorman PJ. Doctors and patients don't agree: cross sectional study of patients' and doctors' perceptions and assessments of disability in multiple sclerosis. *BMJ* 1997;314:1580-3. (31 May.)
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Authors' reply

EDITOR—We agree with Caroline E Selai and Michael R Trimble that some patients experience difficulty in rating their overall health related quality of life with visual analogue scales such as that included in the EuroQol questionnaire.[1] These difficulties, however, may reflect problems in conceptualising this relatively vague and subjective entity rather than problems specific to visual analogue scales. Intuitively, we share Selai and Trimble's concerns about the validity of the assessments of overall health related quality of life. Although we did not attempt to address this issue directly, our data did support the validity of these assessments in

patients with multiple sclerosis. As table 2 in our paper shows, we found that the patients' overall assessments of health related quality of life were strongly correlated with four of the eight domains of the SF-36—that is, vitality ($r=0.57$, $P<0.0001$), general health ($r=0.49$, $P=0.001$), mental health ($r=0.44$, $P=0.004$), and physical role limitations ($r=0.42$, $P=0.006$); the first three of these domains were the very domains that patients identified as particularly important determinants of their quality of life. If these measurements were truly haphazard, as Selai and Trimble suggest, we would not have expected such strong correlations. Furthermore, similar assessments of overall health related quality of life were both valid and reliable when used by healthy individuals to assess their own,[2] or hypothetical, health states[1 3] as well as when applied to patients with a variety of other diseases.[4] Therefore, although the concept of overall health related quality of life is undoubtedly difficult to define, it clearly has a meaning for patients.

Paul Dorman *MRC training fellow*
Neurosciences Trial Unit, Western General Hospital, Edinburgh EH4 2XU

Peter Rothwell *Lecturer in neurology*
Department of Neurology, Radcliffe Infirmary, Oxford OX2 6HE

1 EuroQol Group. EuroQol—a new facility for the measurement of health related quality of life. *Health Policy* 1990;16:199-208.
2 Brazier J, Jones N, Kind P. Testing the validity of the Euroqol and comparing it with the SF-36 health survey questionnaire. *Quality of Life Res* 1993;2:169-80.
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Screening people with a family history of cancer

Benefit of screening for ovarian cancer is unproved

EDITOR—Dunlop and Campbell highlight the ad hoc development of NHS screening services for people with a family history of colorectal cancer.[1] This problem is not confined to colorectal cancer; women with a family history of ovarian cancer are also at increased risk, and increasingly in Britain such women are being offered screening with ultrasonography and measurement of CA125 antigen.

We recently undertook a systematic review of research into screening for ovarian cancer.[2] Unlike with colorectal cancer, there is no evidence that screening reduces mortality. No randomised controlled trials have been completed, although several are under way; such trials are necessary to establish whether screening improves outcomes. It has been argued that screening for ovarian cancer should be offered to women with a family history simply because they are at increased risk. The increased prevalence of cancer in high risk women does not, however, alter the effectiveness of screening; it merely reduces the number of women screened to identify each case and increases the predictive value of a positive test result.

While any benefits of screening are unproved, there are certain to be harms. Women in whom the result of screening is positive undergo surgical intervention to establish whether they have ovarian cancer, which carries risks of appreciable morbidity and even death. With a false positive rate of up to 5%, around 10 operations are needed to identify one ovarian cancer, even if screening is restricted to women with two or more affected close relatives, who have about a 15% lifetime risk of developing ovarian cancer.[2] Most women with a family history, however, have only one affected relative and are at modestly increased risk of death from ovarian cancer; they have around twice the average risk by age 70.[3] The absolute risk for these women of developing ovarian cancer therefore remains low.

Dunlop and Campbell suggest that it may be more efficient to confine screening to proved carriers of genetic mutations who are at very high absolute risk. But the process of systematically identifying high risk subjects in itself constitutes screening. The relative efficiency of such a strategy cannot be determined until both the costs and consequences of identifying carriers have been evaluated. The identification and screening of subjects at higher risk should be evaluated on the same basis as other screening programmes, to establish a consistent approach within the NHS.[4] Where the benefit of screening remains to be established, as for ovarian cancer, investment in services to identify women with a family history in order to offer screening cannot be justified.

Ruth Bell *Senior registrar in public health medicine*
Mark Petticrew *Research fellow*
NHS Centre for Reviews and Dissemination, University of York, York YO1 5DD

1 Dunlop M, Campbell H. Screening for people with a family history of colorectal cancer. *BMJ* 1997;314:1779-80. (21 June.)
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Taking a family history in primary care is important

EDITOR—We agree with Dunlop and Campbell about the need for care when making screening decisions on the basis of family history: high risk patients may be overlooked, and low risk patients may be screened unnecessarily, with potentially serious consequences.[1] This problem takes on a different emphasis, however, when it is viewed from the perspective of primary care.

A recent survey, by one of us, of general practitioners in Oxfordshire found that these doctors believed that they should be involved in genetic screening. However, they were not prepared to take family histories or offer genetic counselling without further training. They also reported that 4.1 patients (95% confidence interval 3.3 to 4.9) per 1000 consulting them did so about their own health risks associated with a diagnosis in a family member. Personal experience suggests that many of these patients are concerned about inherited cancer. Taking a family history in this situation is the only tool available to general practitioners to enable them to decide whether to refer or reassure the patient. Family cancer clinics are already seeing rapid rises in their referrals, and general practitioners definitely need to exercise their gatekeeper role.

Primary care must also receive a consistent response from all the specialties that deal with these cases in secondary care. One of us (PWR) has a patient with a 1:12 lifetime risk of colon cancer based on the family history who was advised against a screening colonoscopy. A sibling with the same risk has undergone a screening colonoscopy in another district.

General practitioners therefore need to develop the skills to take accurate family histories, and national guidelines are needed to advise them which patients should be referred. Although some people at high risk may fall through the net, this is probably the best strategy that we can offer patients currently.

We agree that screening for cancer in primary care needs careful evaluation because of the potential to raise anxiety. Women with a family history of breast cancer, however, do not accurately estimate their risk of disease.[2] Evans et al found that roughly the same number of women overestimated as underestimated their risk. After counselling, more women were able to assess their risk accurately, especially if they had overestimated it originally. If the same is true of colon cancer there is also the potential for reassurance by taking an accurate family history.

Peter W Rose *General practitioner*
Mill Stream Surgery, Benson, Wallingford, Oxfordshire OX10 6RL
prose@server.dphpc.ox.ac.uk

Marc A Suchard *Howard Hughes Medical Institute predoctoral fellow*
Wellcome Trust Centre for Human Genetics, University of Oxford, Oxford OX3 7BN
marc.suchard@well.ox.ac.uk

1 Dunlop M, Campbell H. Screening for people with a family history of colorectal cancer. *BMJ* 1997;314:1779-80. (21 June.)
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Unsupervised surgical training

Logbooks are essential for assessing progress

EDITOR—Wilson's conclusion from her questionnaire study about unsupervised surgical training must be taken seriously but must also be considered in context.[1] We would agree that "unsupervised first time surgery is not ideal training."

Surgical trainees vary in experience, from recently qualified doctors to those

about to become consultants. During this period they will assist in operations, perform operations under supervision, and then perform operations without direct supervision, progressing to more complex procedures. It is essential that all trainees can carry out major and complex operations without supervision by the time their training is complete.

Logbooks are an essential requirement for both senior house officer and specialist registrar levels. At senior house officer level these books are inspected regularly and must be satisfactory before the trainee is allowed to take the final part of the MRCS/AFRCS examination, a requirement for entry to higher surgical training. At specialist registrar level logbooks are inspected yearly as part of the annual appraisal. Every operation in which the trainee is involved must be entered in the logbooks, each entry indicating whether the trainee assisted at the operation, performed it under supervision, or performed it without direct supervision. The logbooks are inspected to ensure that the trainee's progress is satisfactory and that he or she is getting the correct exposure to surgery and also as part of the appraisal of the training post itself.

The royal colleges regard the supervision of surgical training as one of their most important duties. For some time trainers have completed an assessment form, which is shown to the trainee before submission to the supervisory body. About 18 months ago the Joint Committee for Higher Surgical Training introduced a "training post assessment form" to be completed by the trainee; this form is not shown to the consultant but is submitted direct to the supervisory body. In this way trainees can identify, without fear of recrimination, consultants who are not fulfilling their training responsibilities. Training posts are appraised regularly, and examination of these assessment forms helps to identify unsatisfactory training posts. The fact that Wilson refers to registrars and senior registrars suggests that the survey refers to operations performed before the introduction of the assessment form.

If the surgical royal colleges identify a consultant who is not performing his or her duties as a training supervisor adequately, then he or she ceases to be recognised as a surgical trainer; 259 surgical senior house officer posts in England and Wales have had educational approval withdrawn.

Charles Galasko *Chairman, training board*
Colin MacKay *Chairman, Joint Committee on Higher Surgical Training*
Royal College of Surgeons of England, London WC2A 3PN

1 Wilson JA. Unsupervised surgical training: questionnaire study. *BMJ* 1997;314:1803-4. (21 June.)

Surgical training teaches surgical method, surgical anatomy, and operative skills

EDITOR—Wilson seems to have missed the point about surgical training.[1] Surgical training cannot be designed to teach every operation. Life is too short (and is being shortened artificially by the introduction of the Calman reforms), and the advance of surgical knowledge is too rapid.

Surgical training is intended to teach surgical method, surgical anatomy, and operative skills to trainees. Add to that the clinical skills necessary to be able to meet patients, take an appropriate history, do an appropriate examination, investigate suitably, and undertake postoperative care and follow up, and you have a surgeon.

No surgeon will ever feel inadequate if training has run along these lines and what needs to be done is an operation that he or she has never done before. It has been happening to me for 26 years. I did a new operation last week because that was what the clinical situation presenting to me required—and my patient has done well.

Richard G Notley *Surgical trainer*
Spindlewood, 59 Pewley Hill, Guildford GU1 3SW

1 Wilson JA. Unsupervised surgical training: questionnaire study. *BMJ* 1997;314:1803-4. (21 June.)

Other specialties can learn from level of supervision of surgical training

EDITOR—Wilson's data on unsupervised training relate to a survey conducted in 1994,[1] since when training has been improved. Nevertheless, the study highlights some areas of severe concern and will help to shift the emphasis on to the quality of surgical training.

At all stages the safety of patient care must be paramount. What is essential is that the level of supervision is matched to the complexity of the operation and the experience of the trainee. Trainees must have the maturity to know when to seek help, and that help must be freely and readily available.

It would not be feasible or desirable to rule that no trainee should ever perform any operation for the first time unsupervised: trainees who are more senior may well have the skills to perform such operations safely. In addition, the aim of training is to make trainees fit for independent consultant practice, and even consultants are called on to perform new operations.

In the past, the "see one, do one, teach one" culture encouraged trainees to fill their logbooks with as many different operations as possible that could be performed solo. This culture is now shifting towards one in which a well trained surgeon is defined by the number of operations performed under supervision.

All surgical trainees collect data on operations they perform and on their supervision. Currently, these data are not systematically collated to identify good and bad training units. The royal colleges and specialist advisory committees should insist that these data are submitted in a standard computerised format that can be analysed. Units that do not supervise trainees appropriately should lose their trainees.

It should also be remembered that, as a result of the national confidential inquiry into perioperative deaths[2] and other initiatives, the surgical establishment has made major efforts to improve the quality of training and service. We perform procedures that can be easily measured, and other specialties can also learn from the level of supervision of surgical training. We have both encountered trainee physicians on call who have been envious of the level of consultant support that we were able to call on. We must continue to strive to ensure safe training of the highest quality.

Dermot C O'Riordan *President*
Nick Shaper *Honorary secretary*
Association of Surgeons in Training at Royal College of Surgeons of England, London WC2A 3PN

1 Wilson JA. Unsupervised surgical training: questionnaire study. *BMJ* 1997; 314;1803-4. (21 June.)
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Discontinuation of cervical spine immobilisation

Immobilisation should not be discontinued in unconscious patients

EDITOR—Gupta and Clancy highlight a common management dilemma, which I have experienced myself, in requests to "clear the cervical spine" in unconscious patients with multiple injuries either in the resuscitation setting or, more commonly, after a period in intensive care.[1] The request is often made after the three films recommended for advanced trauma life support have been obtained.[2] A response that "the risk of potential for spinal cord injury cannot reasonably be 'cleared' until a patient is conscious, cooperative, and controlled in respect of distracting sources of pain and has a normal neurological and physical examination" is often met with a request for a consultant's opinion.

Even in a recent instructional article on the care of patients with multiple injuries, advice is unhelpful; it states that "when the neurological status cannot be verified (as in unconscious patients) a complete radiological assessment is necessary with anteroposterior and lateral views followed by [computed tomography] when this is indicated."[3] My understanding is that computed tomography will strongly reassure that bony injury or appreciable paraspinal haematoma is unlikely, but the ideal imaging modality would be magnetic resonance imaging,[4] which at present is not practicable in patients with multiple injuries.

So what will I say next time I am asked to clear the cervical spine, even knowing that the incidence of isolated ligamentous disruption, putting the cord at risk, is low? I will continue to give the same reply as before, and a consultant's opinion will again be asked for and will usually be to clear the cervical spine and remove the immobilisation.

A M Collier *Specialist registrar in orthopaedics*
York District Hospital, York YO3 7HE

1 Gupta KJ, Clancy M. Discontinuation of cervical spine immobilisation in unconscious patients with trauma in

intensive care units—telephone survey of practice in South and West region. *BMJ* 1997;314:1652-5. (7 June.)
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In children, cord injury may be present despite normal radiographic appearances

EDITOR—Gupta and Clancy found that in 19 of the 25 general intensive care units that they surveyed, spinal immobilisation was discontinued in unconscious patients with multiple injuries while the patient was unconscious provided that the cervical spine radiograph showed no abnormality.[1] It must be emphasised that this study concerned only adult patients. In children, spinal cord injury without radiological abnormality is well documented.[2] In my experience, three of 31 patients with cervical spine injury had cord injury despite the *x* ray film showing no abnormality.

In children who are at risk of cervical spine injury, adequate spinal immobilisation must be maintained even if the appearances of the *x* ray films are normal. It should be continued until the stability of the cervical spine and the absence of cord injury have been confirmed by clinical examination when the patient is conscious (supplemented if necessary with magnetic resonance imaging or computed tomography), even at the cost of occasional pressure sores.

David A Lloyd *Professor of paediatric surgery*
Institute of Child Health, Department of Child Health, Alder Hey Children's Hospital, Liverpool L12 2AP

1 Gupta KJ, Clancy M. Discontinuation of cervical spine immobilisation in unconscious patients with trauma in intensive care units—telephone survey of practice in South and West region. *BMJ* 1997;314:1652-5. (7 June.)
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Resuscitation

New advisory statements on life support have been published

EDITOR—Two important developments in cardiopulmonary resuscitation have occurred since Ballew wrote his article on recent advances in the techniques used.[1] Firstly, Ballew's plea for the development of a standardised reporting system for cardiac arrests that occur in hospital has been answered by the publication of recommended guidelines on "the in-hospital Utstein style."[2] Secondly, the International Liaison Committee on Resuscitation (representing the European Resuscitation Council, the American Heart Association, the Heart and Stroke Foundation of Canada, the Australian Resuscitation Council, the Resuscitation Council of South Africa, and the Resuscitation Council of Latin America) has now published its advisory statements on basic life support,[3] advanced life support,[4] and paediatric life support.[5] These will promote the move to global guidelines and have already been adopted as the 1997 resuscitation guidelines for use in Britain. As such, they are being assessed on behalf of the European Resuscitation Council, which is likely to adopt them, with any necessary modifications, in June 1998.

The basic life support guidelines are now being tested by the research team in Cardiff to establish how they compare with the previous guidelines regarding ease of learning and retention of skills. Anyone who is interested in replicating this assessment may contact us for a copy of the methodology and test protocol.

P D Donnelly *Honorary senior lecturer*
donnelly @caphm.demon.co.uk

D Chamberlain *Cochairman, International Liaison Committee on Resuscitation (ILCR)*
A J Handley *Chairman, working group on basic life support, ILCR*
D Assar *Associate director*
C A Lester *Research fellow*
Centre for Applied Public Health Medicine, Lansdowne Hospital, Cardiff CF1 8UL

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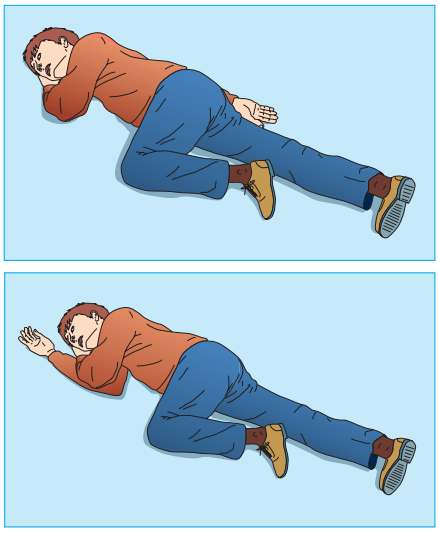
Resuscitation Council (UK) wants everyone who uses new recovery position to report experiences

EDITOR—In 1992 the European Resuscitation Council issued guidelines for resuscitation which recommended a new recovery position.[1] Subsequently, attention was drawn to possible vascular and neurological damage to the dependent arm when volunteers were placed in this position.[2-4] After reviewing the evidence the Resuscitation Council (UK) decided not to recommend any change to the recovery position, at least until advisory statements of the International Liaison Committee on Resuscitation were published.

In the event, the international committee chose not to describe a specific recovery position but, instead, to suggest certain criteria for the management of unconscious, breathing casualties.[5] In particular, these criteria included the avoidance of any injury to the casualty.

Because there was demand for a uniform recovery position for use in the United Kingdom, the Resuscitation Council (UK) decided to recommend a return to the position adopted before 1992 (and currently used by the American Heart Association) with minor modifications (figure (top)). This was chosen because no adverse reports on its use had been received by the council or, as far as was known, by any of the voluntary lay organisations that had taught it. In the past few months, however, the council has received a few reports of pain or discomfort



Recovery position currently recommended for use in United Kingdom (top) and previous position, which may cause vascular and neurological damage (bottom)

in the arm and shoulder experienced by trainees when being turned into this recovery position. These reports have been mostly anecdotal, the problems occurring in volunteers being trained. The council has no way of knowing if the recommended technique was used in each case. No direct reports of injury sustained by a casualty or patient have been received.

In view of these reports, the Resuscitation Council (UK) invites all those who use or teach the new recovery position to report their experiences, both favourable and unfavourable, to the council. It is essential that all reports should be first hand and contain as much detail as possible, including a note of any deviation from the exact technique as described in *The 1997 Resuscitation Guidelines for Use in the United Kingdom*, published by the council. In the meantime, it is important to ensure during training and in practice that particular care is taken when turning a person into the recovery position to avoid any injury to the nearer arm, which should be fully extended with the hand, palm upwards, tucked well under the buttock or upper thigh.

Finally, I must emphasise that, despite possible problems during training, placing an unconscious, breathing victim into the recovery position can be life saving.

Anthony J Handley *Chairman*
Resuscitation Council (UK), 9 Fitzroy Square, London WIP 5AH

1 Basic Life Support Working Party of the European Resuscitation Council. Guidelines for basic life support: a statement. *Resuscitation* 1992;24:103-10.
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Study to predict which elderly patients will fall shows difficulties in deriving and validating a model

This letter was intended to be published as a commentary on the paper by Oliver et al in the issue of 25 October,[1] but, unfortunately, it was omitted from the published journal.

EDITOR—The study by Oliver et al, of the development and evaluation of a risk assessment tool to predict which elderly patients will fall,[1] is an excellent example of the difficulties to be overcome when trying to reconcile pragmatism with methodological purity. The authors set out to develop a risk score that could be used routinely by nurses looking after elderly patients. Such a tool would be of clear value.

Some aspects of the study can be questioned. Firstly, they made the important decision to take each fall as the unit of analysis rather than the patient. I am not convinced by their argument here. While it is true that some patient characteristics are not constant, the effect of this approach is largely to duplicate the data of those patients who fell more than once. These would not be typical patients, and thus the estimated sensitivity and specificity could be biased. Also, the aim of the study was to allow targeting of preventive measures. If effective, these would presumably help to prevent all falls by high risk patients, not just the next fall, so that patients should have been the focus of the whole study. Unfortunately, the authors do not state how much duplication there was in the datasets, although there is a reference to some patients falling several times. Analysing events rather than patients is generally unsound and may lead to overoptimistic conclusions.[2]

Another issue is the choice of controls. In the case-control study (phase 1), for each patient who fell, the patient in the next bed was taken as the control. Patients who fell more than once might well have had the same control more than once, increasing the duplication in the dataset. Also, some controls might have been in hospital for a very short time. It would have been better to choose a control who had been in hospital for the same length of time as the faller but who had not fallen.

The authors gave the five predictor variables equal weight despite their clearly unequal importance. The odds ratios ranged from 2.1 to 20.9, although the more relevant log odds ratios varied by a smaller factor of 4. While there is a good case for simplicity in this study, there must be some loss of predictive ability. The magnitude of this effect is not reported, so that the impact of this decision is unclear. Finally, whereas the phase 1 study looked at all falls, the two subsequent evaluations (phases 2 and 3) considered only falls within the week after assessment.

Predictive models need to be evaluated elsewhere before their true value can be assessed.[3] Few researchers expose their models to this test. There are reasons to expect reduced performance in a different location; what matters is whether the system performs well enough to be clinically valuable. Despite my concerns, the external validation of the STRATIFY risk score in a second hospital suggests that it will be useful. Further evaluation elsewhere is certainly desirable.

Douglas G Altman *Head*
ICRF Medical Statistics Group, Centre for Statistics in Medicine, Institute of Health Sciences, Oxford OX3 7LF
altman@icrf.icnet.uk

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Determining precise role of ethnicity in disease will be difficult

EDITOR—Bhopal's article debates whether research into ethnicity and health is racist, unsound, or important science.[1] This is particularly challenging to us, working in South Africa, which has several interethnic populations.

Apart from in the classical deficiency diseases, much of the causation of most chronic diseases of lifestyle remains unexplained and hence presumably lies in the "black box" that the authors mention. For example, despite huge sums being spent on research on coronary heart disease, known risk factors explain only about half of the variance in its occurrence. An example of the perplexity that still continues is that cardiovascular mortality at age⩽64 in the Russian Federation has been reported as having risen to twice as high as the maximum reached in the United States and Finland in the 1960s, for reasons "not known."[2] In an investigation into risk factors and mechanisms behind the fact that mortality from coronary heart disease is four times higher in Lithuanian than Swedish middle aged men, it was concluded that the huge dichotomy was not caused by the traditional risk factors alone. On the same subject, but in a different context, cord blood lipid and apolipoprotein profiles were studied in Chinese, Malay, and Indian infants in Singapore. The differences were considered to underlie the differences in susceptibility to coronary heart disease in the adult population of that city.[3]

In Africa, many interethnic differences in patterns of health/ill health are not explicable by known risk factors—for example, the high prevalence of obesity in African women. Interestingly, the same contrast prevails in the United States. A study of African-American and white girls showed significant ethnic differences in body composition and differences in the strength of association between abdominal adipose tissue and insulin sensitivity in the two populations.[4] Furthermore, an investigation of obese postmenopausal African-American and white women showed plasma leptin concentration to be a fifth lower in the African-American women. The authors judged that this difference may have a role in the higher prevalence of obesity in this population.[5]

An enormous amount of research has been undertaken into coronary heart disease and obesity, so it is difficult to see what further investigations can be pursued to define more closely the role of the ethnic factor.

A R P Walker *Head*
Human Biochemistry Research Unit, South African Institute for Medical Research, Johannesburg, South Africa

I Segal *Head*
Gastroenterology Unit, Baragwanath Hospital, Johannesburg

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New connections between medical knowledge and patient care

Human condition is full of decisions that aren't simple yes/no decisions

EDITOR—Weed's arguments in favour of introducing information tools into medical practice are fundamentally flawed.[1] He has failed to give any example of how knowledge coupling software works in facilitating medical decision making, let alone in supplanting the unaided medical mind, as he advocates.

The problem is that computer software is essentially binary. Its natural default is to a yes/no decision. The human condition is full of indecisiveness, and, whatever the character of the doctor, the patient remains human. There can be no contest against the craftsmanship of history taking by an experienced doctor. Heaven forbid that this should be replaced by a questionnaire and a computerised home doctor.

The profession is less in the backwoods than Weed allows. The success of tools for updating one's knowledge, such as the *Oxford Textbook of Medicine*, is an example of our openness to the support of information technology. The shift to problem based learning in the curriculums of many medical schools is an acknowledgement of the futility of overloading minds with facts, which do not endure.

By all means let us harness good software to aid our decision making when we are convinced that it is good. But we must

remember that the supreme human qualities of happiness, love, and beauty are impervious to the discipline of digitisation.

Austin T Carty *Consultant radiologist*
Royal Liverpool University Hospitals, Liverpool L7 8XP

1 Weed LL. New connections between medical knowledge and patient care. *BMJ* 1997;315:231-5. (26 July.)

Intervention of health professionals acts as an inductance, not as a resistor

EDITOR—Weed states that voltage drops occur along the pathway from the origin of medical knowledge to the end professional user, and he calls for an information infrastructure directed at patients as the primary decision makers.[1] This hypothesis fails in three ways. Firstly, it assumes that a core knowledge base can be defined. Unfortunately, the relations between health interventions and outputs are often ambiguous and do not always yield to a rigorous analysis. Secondly, with the increasing demands on limited resources, there will always be a conflict between the requirements of individual patients and those of society. A balanced advocacy for both parties can be achieved only by informed professional intervention. Thirdly, a patient centred decision framework based on information systems gives advantages to those who have over those who do not, increasing differences in equity further.

The intervention of a health professional does not act as a resistor inducing a voltage drop along a knowledge pathway. Rather, it acts as an inductance, modulating and attenuating frequency—balancing the validity of the evidence with the unique needs of the individual and ensuring that resources are distributed equitably between the conflicting demands of patients and society. The next generation may be driven by an information technology/designer label/ soap opera culture, but much of society will still need a shaman/healer/medicine man/health advocate/friend and will lose him at its peril.

D P Kernick *General practitioner*
St Thomas Health Centre, Exeter EX4 1HJ

1 Weed LL. New connections between medical knowledge and patient care. *BMJ* 1997;315:231-5. (26 July.)

Patients in most need of medical attention are least able to operate computers

EDITOR—Weed provocatively suggests that medical care will improve if expensively educated doctors are bypassed after patients have been educated and empowered to interact directly with intelligent computers.[1] He would benefit from spending some time in a primary care doctor's practice. The vast majority of symptoms are not markers of serious disease. The fact that only a tiny proportion ever get presented to a doctor (from 1 in 33 episodes of sore throat to 1 in 456 episodes of fatigue in one study)[2] is of general benefit because the dangers of medicalisation—what Illich called "medical nemesis"—are prevented.[3] An important role of general practitioners is often to reassure patients about the normality of having "symptoms" and to educate them about self care and simple remedies; 90-97% of all consultations are competently managed without specialist referral being required.[4] Replacing the doctor with a computer would lead to a massive increase in anxiety as patients tapped in a list of their symptoms to generate lists of differential diagnoses. Just as psychiatrists should not treat their own depression or cardiologists their own high blood pressure, so super-well informed patients cannot be expected to make objective judgments about their own medical care and reasonably assess risks; Weed's analogy with being a traveller choosing a holiday is facile.

There is also no evidence that ill people would want to interact with a microchip. The human qualities of a doctor, such as listening and not being rushed, are often cited as the key desired aspects of a medical consultation. Also, those patients in most need of medical attention are least able to operate computers or make well informed decisions. Medical information technology would most benefit those who already extract quality care from the NHS through being informed, articulate, tenacious, and unintimidated and insisting on expert opinion; thus inequality in health would be increased.

It is true that current medical education involves excessive cramming of facts, and rapid access to and ordering of large amounts of information by the human brain are clearly not possible. It has been shown, however, that computer prompting systems, linked to protocols, are an effective way of changing and maintaining best clinical practice,[5] and this approach should be encouraged. Although the arrogant, intuitive practices of the expert should be left behind, medical care will not be improved by taking away the patient's impartial doctor and letting him or her swim unaided in a sea of information technology.

John Edwards *General practitioner*
Hillview Surgery, Greenford, Middlesex UB6 7HQ

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Information technology has much to offer certain aspects of health care

EDITOR—Weed has written a helpful article on the place of information and computer technology in medicine in the future.[1] His analogy between travel and patient care, however, is inappropriate. Travellers are primarily interested in reaching their destination in the shortest time, in comfort, and in safety. The route is usually irrelevant. Nowadays maps are used by only a minority for personal travel. Air travellers have no opportunity to influence technical decisions before or during their flight. It would be much more appropriate to compare medical care with a repair industry such as the motor industry—except that doctors are not able to "write off" their patients when the repair is too costly.

I believe that there is an enormous place for "expert systems" in medicine, and I see nothing wrong with cookbook medicine when the recipe is robust and effective. Unfortunately, patients are as fearful about relying on computers for medical decisions as doctors are reluctant. But we already rely on many expert systems, such as the algorithms involved in computed tomography and ultrasonography.

In my opinion, there are essentially three areas in which information technology and artificial intelligence can lead to a huge improvement in health care. One is making medical knowledge available in an easily assimilated form. Although medical knowledge has always been freely available in modern times, it has been in the user unfriendly form of large textbooks and technical journals. Secondly, information systems could build research and audit into every act of health care. Thirdly, when an aspect of healthcare management becomes robust and effective, sometimes in a small and specific area, it could be handed over to an expert system, with technicians being used, for example, to palpate the spleen when necessary.

David J R Hutchon *Consultant obstetrician and gynaecologist*
Darlington Memorial Hospital, Darlington, County Durham DL3 6HX

1 Weed LL. New connections between medical knowledge and patient care. *BMJ* 1997;315:231-5. (26 July.)

General practitioners want to know whether a treatment works and is safe in practice

EDITOR—Froom et al question the benefit of routine use of antimicrobials for acute otitis media.[1] I agree with their arguments but believe that the inclusion solely of randomised double blind placebo controlled trials in their study is insufficient to make the point. Studies undertaken in tertiary care centres cannot dispel practitioners' doubts about the need for antibiotics in acute otitis media.

Observational studies that Bollag-Albrecht and I carried out on the natural course and management of acute otitis media in an office based general and paediatric practice were meant to fill the gap between academic research and research in practice. After a practice audit of the management of acute otitis media we developed and tested a protocol in which we defined criteria for the diagnosis and treatment of acute otitis media.[2] In a two year study of 168 children aged 16 and under with acute otitis media we found that antibiotics were not needed in the first instance; treatment with analgesics, and saline nose drops plus inhalation of steam to keep the mucous membranes moist,

supported the self healing process and gave relief to the child. We confirmed these results in a study of another 62 children over one year.[3] We did not see any case of clinically overt mastoiditis or meningitis. We have adhered to this regimen without experiencing any failure to this date.

Last year colleagues and I reported another study on the occurrence and importance of middle ear effusion.[4] With careful clinical observation and follow up, and using acoustic reflectometry as an objective measurement, we found that middle ear effusion is common. It probably reflects a pathogenetic feature of the mucous membranes in the upper respiratory tract during a catarrhal episode.

The pathological importance attributed to middle ear effusion by specialists and academic researchers needs to be reassessed. If we are to make use of the statistical rigour of randomised controlled trials in guidelines for practical use we need to conduct studies in primary care practices serving unselected populations.

Ueli Bollag *Specialist in child health*
Familienpraxis beim Eigerplatz,
Mühlemattstrasse 61, CH-3007 Berne, Switzerland

1 Froom J, Culpepper L, Jacobs M, De Melker RA, Green LA, van Buchem L, et al. Antimicrobials for acute otitis media? A review from the International Primary Care Network. *BMJ* 1997;325:98-102. (12 July.)
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Pigeon fancier's lung

Current methodology is not sensitive enough to monitor effectiveness of avoidance measures

EDITOR—The final sentence of Bourke and Boyd's editorial on pigeon fancier's lung states that sequential measurement of circulating antibody to pigeon related antigen is important in patient management.[1] Experience within the United Kingdom National External Quality Assessment Scheme (UKNEQAS) performance testing programme for antibodies in extrinsic allergic alveolitis suggests that this is far from being a practical proposition at the present time. The quality assessment scheme for antibody to fungal and related antigens currently has 57 participating laboratories, 12 of which are outside the United Kingdom. Two serum samples are distributed every eight weeks, and the participating laboratories are required to test one of the samples for antibody to pigeon, budgerigar, or antigens associated with farmer's lung and to submit both qualitative and quantitative results. The methods used to detect antibody to pigeon related antigens are: double diffusion (35 laboratories), counterimmunoelectrophoresis (20), enzyme linked immunosorbent assay (ELISA) (2), and fluoroimmunoassay (1); one laboratory uses two methods. Thus only three of the laboratories are using methods that can be regarded as more than semiquantitative, and only one is using a method that is standardised in terms of the IgG antibody concentration.

During January 1995 to March 1997, 11 samples were tested for antibody to pigeon related antigen and elicited 577 qualitative responses. There were 19 false positive and 22 false negative returns, giving a total of 41 misclassifications. Only nine laboratories routinely return quantitative data, but with even less agreement. For the most recent sample that was designated as positive, the responses included titres ranging from 1:8 to 1:12 800, and semiquantitative assessments from weak through + to + + +. The fluoroimmunoassay was alone in giving a truly quantitative response of specific IgG of >200 mg/l.

We would consider the methods used in most laboratories for detecting antibody to pigeon related antigens to be unsophisticated and of insufficient sensitivity or precision to permit reporting of antibody levels in quantitative terms. If the concept of monitoring a patient's compliance is to be judged by sequential antibody levels then the methodology used will have to be changed to reflect the clinical need, and the current methods will have to be consigned to the archives, where they belong.

A Milford Ward *Organiser*
P A E White *Manager*
UKNEQAS for Immunology, PO Box 894, Sheffield S5 7YT

1 Bourke S, Boyd G. Pigeon fancier's lung. *BMJ* 1997;315: 70-1. (12 July.)

Pigeon racers will not want to reduce time spent in lofts

EDITOR—Having raced pigeons with some success as a teenager and been the then youngest secretary of a pigeon racing club, I believe that I have an inside view of the sport. I therefore wish to add several points to Bourke and Boyd's editorial on pigeon fancier's lung.[1]

The authors state, "When racing, the birds are transported to a liberation point; a ring is placed on one leg; and, when released, the bird returns to its loft." In fact, the birds are "ringed" with a numbered rubber band around the leg at the competitors' local clubhouse. This is usually done on Friday night, by the elected committee members. The birds are then transported with the other local clubs' entries to the liberation point for the race the next day.

More importantly, the editorial, in recommending minimal exposure to pigeon antigen, did not mention one important method of loft management—the deep litter system. This is used by fanciers so that they do not have to scrape perches and place fresh sand on the loft floors every day. The droppings accumulate, dry, and become a fine chalk-like powder over a period of months. Fresh droppings are then absorbed by this guano, which acts like cat litter. Unfortunately, this system is even more dusty than the alternative. I would strongly discourage a fancier with lung problems from using it.

Another important point is that lofts are more prone to become dusty in certain months. During the autumn and early winter the pigeons undergo the major part of their moult. Fifty birds shedding their feathers at the same time create a tremendous antigen load. Is there any evidence that the acute disease is more common in the autumn?

Finally, asking a pigeon racer to spend less time in the loft is like asking a leopard to lose its spots. However fast a pigeon may be, if it is not trained to enter the loft on return, vital time may be lost in retrieving the unique numbered rubber band. Getting the birds to enter the loft quickly entails spending time with them in the loft so that they are not frightened of the owner. It is impossible to get a ring off the leg of a pigeon that will not enter the loft.

Daryl Leung *Pigeon fancier on sabbatical and senior registrar in medicine and geriatrics*
New Cross Hospital, Wolverhampton WV10 0QP

1 Bourke S, Boyd G. Pigeon fancier's lung. *BMJ* 1997;315: 70-1. (12 July.)

Changing face of ectopic pregnancy

Medical treatment of ectopic pregnancy preserves reproductive potential

EDITOR—We agree with Mascarenhas et al that laparoscopic or medical treatment should now replace laparotomy in the treatment of ectopic pregnancy.[1] We disagree, however, with the statement that methotrexate treatment does not seem to be a replacement for laparoscopic salpingotomy. The authors suggest that methotrexate injection may be associated with increased abdominal pain six to seven days after the treatment as well as a transient increase in human chorionic gonadotrophin titres within the first four days. They suggest that the increased titres may cause undue distress and lead to additional treatment.

It is true that 15-20% of patients experience abdominal pain six to seven days after methotrexate treatment, but this responds to mild pain relief. The rise in titres of human chorionic gonadotrophin up to day 4 is irrelevant as protocols for methotrexate treatment take into account the titres on days 4 and 7. If the titres between days 4 and 7 drop by $\geq 15\%$ then a second injection is not needed.[2]

The outcomes of laparoscopic linear salpingostomy showed that 95% of procedures were successful and over four fifths of women tested had patent tubes. The rate of recurrent ectopic pregnancy was 22%. After laparoscopic surgery, postoperative complications such as bleeding, increased titres of human chorionic gonadotrophin, or other persisting symptoms occur in up to a fifth of cases.[3]

A single intramuscular injection of methotrexate 50 mg/m^2 has a success rate of over 90%. Tubal patency is conserved in

90% of women tested, and the rate of recurrent ectopic pregnancy is 12%.[2] More recently it has been shown that adding a single oral dose of 600 mg mifepristone to the methotrexate regimen significantly improves the resolution rates in patients with a titre of human chorionic gonadotrophin of >450 IU/l.[4]

The intrauterine pregnancy rate is 60% after conservative tubal surgery, 87% after medical treatment, and 40% after salpingectomy.[5] It must also be emphasised that laparoscopic surgery requires the selection of patients, use of special instruments, higher training, and skill which may not be readily available in every hospital.

We agree that haemodynamically stable patients should be managed with a conservative approach. The rationale is the preservation of reproductive potential. Medical treatment, especially with the addition of mifepristone, deserves attention if not priority over the surgical options, but bigger trials are necessary to support this conclusion.

Mehmet R Gazvani *Research fellow in reproductive medicine*
Ian Aird *Lecturer in obstetrics and gynaecology*
Simon J Wood *Research fellow in reproductive medicines*
Angus J M Thomson *Research fellow in reproductive medicine*
Charles R Kingsland *Consultant in obstetrics and gynaecology*
Liverpool Women's Hospital, Liverpool L8 7SS

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Each centre should validate diagnostic algorithms for its own patients

EDITOR—Mascarenhas et al's call for the routine use of laparoscopic and medical treatment for ectopic pregnancy is long overdue.[1] The ready availability of transvaginal ultrasonography and quantitative assays of serum human chorionic gonadotrophin mean that it should be possible to diagnose most ectopic pregnancies well before rupture, at a time when conservative treatments are an option.

We would suggest caution, however, when introducing non-invasive diagnostic algorithms into clinical use,[2 3] particularly if methotrexate is to be used without confirmatory laparoscopy. All these diagnostic algorithms rely on the concept of a discriminatory threshold, meaning that there is a human chorionic gonadotrophin concentration above which all viable intrauterine pregnancies will be seen on transvaginal ultrasonography. If the uterus appears empty at a higher concentration then a presumptive diagnosis of ectopic pregnancy is made and, if appropriate, systemic methotrexate can be given.

Reported thresholds of human chorionic gonadotrophin have fallen progressively with improvements in transvaginal ultrasonography, and diagnostic algorithms that use a threshold of 1500 IU and even 1000 IU (international reference preparation) have been published.[2 3] This threshold, however, is very dependent on the scanning equipment used and skills available, so the external validity of (the often relatively small) studies validating these diagnostic algorithms[2-4] is questionable. In fact, only one study has attempted to address the issue of external validity at all, and its authors suggested that a human chorionic gonadotrophin threshold of 3000 IU (international reference preparation) should be used.[5] Similar concerns apply to algorithms that use the rate of increase in human chorionic gonadotrophin concentration over 48 hours to determine the non-viability of a pregnancy of uncertain site.

Although non-laparoscopic diagnostic algorithms for ectopic pregnancy should be introduced, it is important that each centre considers validating these algorithms for its own patients. Using a diagnostic algorithm with a low sensitivity will result in delayed diagnosis. If the specificity of an algorithm does not approach 100% then the inadvertent administration of methotrexate to women with viable intrauterine pregnancies will be inevitable.

Mascarenhas et al also comment that a randomised trial of laparoscopic salpingotomy and intramuscular methotrexate is still awaited. One such study has recently been completed in the Netherlands, although it is as yet unpublished, and similar studies are currently being undertaken in Denmark, the United States, and New Zealand (by us). Hopefully, issues such as the success of treatment, cost, and which treatment patients themselves prefer can be addressed through these studies.

Martin Sowter *Research fellow*
Cindy Farquhar *Senior lecturer*
University Department of Obstetrics and Gynaecology, National Women's Hospital, Epsom, Auckland, New Zealand

1 Mascarenhas L, Williamson J, Smith S. The changing face of ectopic pregnancy. *BMJ* 1997;315:141. (19 July.)
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Anaesthetists in Poland are on hunger strike

EDITOR—All doctors in Britain are concerned about the effects of chronic underfunding of health care, but a recent letter from two of our former senior house officers, Dr Jolante Sliwowska and Dr Przemek Jakubowski, highlights how bad things are in Poland.

On 14 October 1997 a number of the anaesthetists in Warsaw, Poland, went on hunger strike, and at present about 250 of a total of about 300 are reported to be starving themselves. The anaesthetists seek, firstly, an improvement in the monitoring equipment and the other equipment that they work with; they wish to reduce the unnecessary risks that their patients are being exposed to every day. They also seek an improvement in their own salary, which is currently below the mean for all workers in Poland. There have been protests in all medical specialties for the past year, including formal negotiations and two protest rallies of 10 000 doctors through Warsaw. Because of the lack of any response, and in despair, the anaesthetists' medical society decided that a hunger strike was the only way that they could get the attention they deserve. The anaesthetists have continued to work until they have been ordered not to when they become medically unfit.

We are obviously concerned for Jolante, Przemek, and their colleagues, who by this time are placing their health seriously at risk. We wish them well and express our support in what they are trying to achieve.

C R Eatock **P S Laurie**
R S Sanders **B A Thornley**
C M Wait **C A Wareham**
D G Willatts
Consultant anaesthetists
Horton Hospital NHS Trust, Banbury, Oxfordshire OX16 9AL

If a wound is "neatly incised" it is not a laceration

EDITOR—In their short case report in Minerva, Mark Bailey and Chris Luke describe an injury to a finger as a neatly incised laceration and comment that the wound was caused by broken glass.[1] A "neatly incised laceration" is a contradiction in terms. An incised wound is an injury to the skin caused by a sharp cutting implement such as a knife, broken glass, or a surgeon's scalpel. A laceration is a tearing or splitting of the skin caused by blunt trauma, such as a blow from a fist or foot or with a hammer or baseball bat. We regularly review reports for both the prosecution and defence in criminal proceedings, prepared by all grades of doctor, in which wounds are described as lacerations but have clearly been caused by a knife. While poor description of injuries is common and is a reflection of the poor state of medicolegal teaching in medical schools, the correct use of terms will help the courts and save some embarrassment in the witness box.

The distinction is important, because courts rely to a greater or lesser extent on the medical profession to determine the cause of wounds. The difference in sentencing for conviction for an assault with a punch, which produces a laceration, and a slash with a knife, which results in an incised wound, could be the difference between a non-custodial punishment and four years in prison.

C M Milroy *Senior lecturer*
G N Rutty *Senior lecturer*
Department of Forensic Pathology, Medico-Legal Centre, Sheffield S3 7ES

1 Bailey M, Luke C. Minerva. *BMJ* 1997;314:1702. (7 June.)