Letters

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Guideline for primary care management of dementia

Such guidelines should consider all relevant effective treatments

EDITOR—Evidence based guidelines have a responsibility to consider all the relevant effective treatments and not to concentrate only on those with which clinicians are familiar. I was surprised by a serious omission from the North of England evidence based guidelines for the primary care management of dementia[1]—of category I evidence for the effectiveness of *Ginkgo biloba* extract in dementia, from a large randomised controlled trial.[2] The number needed to treat for a 4 point improvement in the cognitive subscale of the Alzheimer's disease assessment scale at one year of follow up has been calculated as 7.9 (95% confidence interval 4.2 to 67); for a significant improvement in the geriatric assessment by relative's rating instrument (a daily living and social behaviour score assessed by family members) it was 7.0 (3.3 to 97). The dose of *G biloba* extract was 120 mg a day.[3]

G biloba extract is available over the counter, and the cost of a year's treatment (from one major supermarket) is £85. A year's treatment with donepezil, by contrast, costs £891 for 5 mg and £1248 for 10 mg.

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Competing interests: None declared.

1 Eccles M, Clarke J, Livingstone M, Freemantle N, Mason J for the North of England Evidence Based Dementia Guideline Development Group. North of England evidence based guidelines development project: guideline for the primary care management of dementia. *BMJ* 1998;317:802-8. (19 September.)
2 Le Bars PL, Katz MM, Berman N, Itil TM, Freedman AM, Schatzberg AF for the North American RGb Study Group. A placebo-controlled, double-blind, randomized trial of an extract of Ginkgo biloba for dementia. *JAMA* 1997; 278:1327-32.
3 Dementia: diagnosis and treatment. *Bandolier* 1998;5(2): 2-3.

Guideline should cover differential diagnosis

EDITOR—The summary version of the guideline for primary care management of dementia is inadequate and in some respects inaccurate.[1] Not to cover differential diagnosis is a major omission; treatments for Alzheimer's disease now exist, and general practitioners and the primary healthcare team need to identify patients with vascular dementia and cognitive impairment (many such patients have treatable risk factors) as well as those with Lewy body dementia (for which diagnostic advice is in fact provided). The guideline should at least have recommended referral to specialist services, since distinguishing between the dementias is one of our most important clinical problems and an area where guidelines are most urgently required. Advice about when to refer to social services is essential.

The section on physical screening failed to recommend a physical examination, which is an essential part of the assessment—to identify treatable vascular risk factors, for example. Recommended routine screening tests should have included vitamin B-12 assay and liver function tests. The search strategy and synthesis included only the findings of studies published before 1996, apart from two 1998 references. Thus many of the statements about drug treatment are inaccurate, and the recommendations are misleading. Rivastigmine, recently licensed for Alzheimer's disease, is not mentioned; tacrine is not available in the United Kingdom; and velnacrine was never licensed.

Adequate evidence now exists that donepezil improves cognitive and global functioning in some patients with Alzheimer's disease and that these benefits may be maintained with long term use.[2] Cost analysis models suggest that donepezil is cost neutral,[3] and therefore treatment should not be withheld on financial grounds. Specialists should start and supervise such treatment,[4] but no evidence based support exists for the recommendation that general practitioners should not continue to prescribe it if it seems beneficial.

The guideline states that respite care does not reduce the burden of caring for a person with dementia. Research commissioned by the Department of Health found that almost four fifths of carers reported that respite care had made their life better.[5]

Finally, the guideline should reflect the fact that in many practices the lead professional for detecting and managing dementia will be a nurse rather than a doctor.

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Competing interests: None declared by RE or DS. JH is partly employed to undertake research into new treatments for dementia and has acted as an adviser to a pharmaceutical company developing treatments for dementia. GW has been reimbursed by Novartis, which makes drugs for Alzheimer's disease, for attending a symposium and has received a fee for speaking; he has also received funding from the pharmaceutical industry for clinical trial work on drugs for Alzheimer's disease, mostly on a "per patient" basis, and this funds staff salaries. The pharmaceutical companies include and have included Janssen, Eisai, and Shire Pharmaceutical Development.

1 Eccles M, Clarke J, Livingstone M, Freemantle N, Mason J for the North of England Evidence Based Dementia Guideline Development Group. North of England evidence based guidelines development project: guideline for the primary care management of dementia. *BMJ* 1998;317:802-8. (19 September.)
2 Rogers SL, Friedhoff LT. Long-term efficacy and safety of donepezil in the treatment of Alzheimer's disease: an interim analysis of the results of a US multicentre open label extension study. *Eur Neuropsychopharmacol* 1998;8: 67-75.
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4 Standing Medical Advisory Committee. *The use of donepezil for Alzheimer's disease.* London: Department of Health, 1998.
5 Levin E, Moriaty J, Gorbach P. *Better for the break.* London: HMSO, 1994.

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GPs may want to continue prescribing donepezil for patients

EDITOR—The guideline for the primary care management of dementia seems to confuse the lack of evidence of benefit with evidence of lack of benefit regarding the effects of donepezil in improving function in patients with dementia.[1] The recommendation that general practitioners should not continue donepezil started in hospital is given a strength of A (that is, it is directly based on evidence from well designed randomised controlled trials, meta-analyses, or systematic reviews). This is clearly inconsistent with the authors' acknowledgment of the limitations of current knowledge.

We have one patient aged 63 with severe dementia who was prescribed donepezil and whose wife reported considerable benefits. The responsible consultant withdrew it after one year to assess whether it was still useful. The patient's wife reported deterioration in his functioning, which has been reversed by restarting the drug. Although this is not a placebo controlled individual trial, it is one means of assessment in the real world. As general practitioners we have not yet been asked to prescribe this drug, but if secondary care were to stop prescribing it we would continue it. Failure to do so would seriously damage our relationship with the patient's wife, who believes that she has convincing evidence of its benefit. To use the guidelines to claim that "we know better" in the absence of relevant evidence seems the height of professional arrogance.

Evidence based medicine loses some credibility if it attempts to control behaviour in the vast number of clinical situations for which hard evidence is lacking. As professionals we must be prepared to admit to the limits of our knowledge and share these limitations with our patients.

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Competing interests: None declared.

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Some recommendations given are not based directly on evidence cited

EDITOR—We are concerned that some of the recommendations in the evidence based guidelines for the primary care management of dementia are not based directly on the evidence cited, thus undermining the evidence based approach.[1] Some recommendations are tentative ("General practitioners should consider using formal cognitive testing") despite good evidence to support them. Others are quite categorical ("General practitioners should not initiate treatment with donepezil") but not based on any published studies of the prescription of donepezil in primary care. The advice that general practitioners should not initiate treatment with donepezil (which is out of step with the Standing Medical Advisory Committee's guidelines[2]) is based only on a judgment made by the authors.

The recommendation (cited as based on category I evidence) that general practitioners should not continue prescriptions of donepezil started in hospital is again not founded on any published evidence. In fact there is evidence from category I studies that, in patients who have responded to donepezil, stopping the drug leads to clinical deterioration.[3] This is supported by our experience in Southampton Memory Clinic, where we have used discontinuation of donepezil as a useful adjunct to clinical assessment and rating scales in some patients when response was uncertain.

If evidence is lacking this needs to be made clear; the authors seem to confuse no evidence of effectiveness with evidence of no effectiveness. Statements that are said to be based on evidence but are not lend a spurious scientific respectability to the recommendations. This blurs the boundary between evidence and opinion, which the article aims to keep separate.

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Competing interests: HM has received a grant from Eisai and Pfizer towards running costs of the Southampton Memory Clinic. DW has undertaken phase III trials for Eisai, Pfizer, Novartis, and Bayer of cholinesterase inhibitors and has received sponsorship from these companies for attending conferences and as a member of their advisory boards. CH has no competing interests.

1 Eccles M, Clarke J, Livingstone M, Freemantle N, Mason J for the North of England Evidence Based Dementia Guideline Development Group. North of England evidence based guidelines development project: guideline for the primary care management of dementia. *BMJ* 1998;317:802-8. (19 September.)
2 Standing Medical Advisory Committee. *The use of donepezil for Alzheimer's disease. London: Department of Health, 1998.*
3 Rogers SL, Farlow MR, Doody RS, Mohs R, Friedhoff LT and the Donepezil Study Group. A 24 week, double-blind placebo-controlled trial of donepezil in patients with Alzheimer's disease. *Neurology* 1998;50:136-45.

Authors' reply

EDITOR—Marshall and Eastley et al identify potentially relevant studies that were published after the period covered by the systematic review on which the guideline was based. Such studies will be incorporated when the guideline is reviewed.

Three of the letters question the evidence for and the subsequent recommendations on the use of donepezil. The meta-analysis of the effectiveness of donepezil within the guideline is based on all the data that were available (data from 1102 patients). It shows a significant effect on cognitive function, no measurable changes in quality of life, and inadequate evidence of effects on activities of daily living. The average effect on the cognitive subscale of the Alzheimer's disease assessment scale (an improvement of 2.8 points) was remarkably consistent, with only 1 patient in 40 achieving a benefit of more than 3.4 points. The guideline development group was thus unconvinced of the clinical importance of the effect of donepezil on cognitive function and questioned the widespread use of the drug by general practitioners. It also felt that responsibility for prescribing should lie with clinicians likely to prescribe the drug regularly. This is compatible with the Standing Medical Advisory Committee's guidance.

Eastley et al raise the issue of whether we should have included differential diagnosis. We chose explicitly not to do so, as we regarded this as a function of secondary care. Dementia with Lewy bodies was specifically mentioned because of the need to avoid neuroleptic drugs in patients with dementia of this type. Eastley et al further suggest that recommended routine screening tests should have included vitamin B-12 assay and liver function tests. We could not identify any evidence to support such recommendations. Their final point, about who is likely to be the lead responsible clinician in a practice, is an implementation issue to be decided at a local level.

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MONICA did not deliver on task it set out to accomplish

EDITOR—What MONICA actually said grows murkier and murkier. I am baffled by Tunstall-Pedoe's criticism of the newspaper reporting of the project.[1] His first target is the *Daily Telegraph*, the first paper to spot the story, which reported on 25 August: "The largest ever cardiology study has failed to find a link between heart attacks and the classic risk factors, such as smoking and high cholesterol levels."

According to Tunstall-Pedoe, this account of the study was fantasy. He describes telling a researcher from the BBC who telephoned him about it to "discount what was written in the *Daily Telegraph* and use the project's press release." I have the press release in front of me. It is headed "Surprises from world's largest and longest heart study," and at the bottom of page 2, in bold type, it says: "Changing rates of heart disease in different populations did not appear to relate at all well to the change in the standard risk factors." The *Daily Telegraph* seems to me to have got it spot on, and I and others wrote pieces in its wake.

Tunstall-Pedoe makes some accurate points about the way the media feed off each

other. But his identification of motes in others' eyes would carry more weight if he recognised the beam in his own. When MONICA was launched in the mid-1970s there was fierce disagreement, I understand, about whether such a large cross sectional study could deliver on the task it set out to accomplish. The "surprising" results seem to confirm that its original detractors were right.

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1 Tunstall-Pedoe H. Did MONICA really say that? *BMJ* 1998;317:1023. (10 October.)

Congenital abdominal wall defects in the United Kingdom

Sources had different reporting patterns

EDITOR—Stone et al have a very different view from ours of abdominal wall defects in our regions (former South East Thames and North West Thames).[1] As they acknowledge, the cited rates for abdominal wall defects were derived from sources with different reporting patterns. In particular, the rates quoted for England and Wales were from data from the Office for National Statistics,[2] which take no account of terminations for abnormalities diagnosed prenatally, whereas the Glasgow and northern England registers have tried to be comprehensive.

More appropriate north-south comparisons could have been achieved from the rates reported by individual regional registers such as those in the former South East Thames and North West Thames regions. These registers obtain data in a similar manner to the northern regional registers, with active data collection from multiple sources and inclusion of terminations and stillbirths. The table shows the data for abdominal wall defects from the South East Thames and North West Thames registers in 1992-6. These data confirm a higher incidence of abdominal wall defects in the south of England than that quoted by Stone et al. Consequently, the gradient hypothesis is not supported when comparable data are used. The apparent excess of exomphalos in Scotland does not distinguish between cases with isolated exomphalos and those with multiple defects or inherited syndromes, which may have completely different causes; it is therefore inappropriate to speculate on possible causal factors for all defects.

What Stone et al's study does show, however, is the need for fully funded comprehensive regional registers for congenital malformations, which take account of the changes in perinatal practice and the frequent fragmentation of prenatal and postnatal tertiary services. The data derived from the Office for National Statistics are an exhaustive attempt to ascertain incidence on a national level but cannot be seen to be as efficient as data from regional registers.[3] Until a national network of comparable registers is achieved, information derived for planning and public health issues will be seriously flawed at best and at worst misleading.

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1 Stone DH, Rimaz S, Gilmour WH. Prevalence of congenital anterior abdominal wall defects in the United Kingdom: comparison of regional registers. *BMJ* 1998; 317:1118-9. (24 October.)
2 Tan KH, Kilby MD, Whittle MJ, Beattie BR, Booth IW, Botting BJ. Congenital anterior abdominal wall defects in England and Wales 1987-1993: retrospective analysis of OPCS data. *BMJ* 1996;313:903-6.
3 Chitty L, Isakaros J. Congenital anterior abdominal wall defects. *BMJ* 1996;313:891-2.

Analysis should be restricted to regional data

EDITOR—Stone et al recently tested the hypothesis of a north-south variation in the UK prevalence of abdominal wall defects and confirmed the greater prevalence in the north of England and Scotland.[1] We noted that the regional data used did not extend below the Mersey region in England. Many health regions now have congenital anomaly registers, including the West Midlands (table). Stone et al use several different time periods for comparing regional data, and an increasing prevalence of gastroschisis with time has been reported. A more complete assessment of variation in prevalence, however, may be noted using more recent data for all regions.

Analysis of abdominal wall defects should be restricted to the use of regional data. National systems do not include terminations and fetal losses, which make up many of these cases, and in our experience comparisons with data from the Office for National Statistics have indicated low ascertainment levels in the National Congenital Malformation System.

We would choose to consider omphalocele and gastroschisis separately owing to their differing causes, and the classification of abdominal wall defects into either group must be reviewed. The majority of omphaloceles are lethal because of the association with major abnormalities of other systems. Gastroschisis is associated with low maternal age[2 3] and social class.[2 4]

The West Midlands congenital anomaly register has recently completed a review of its data on congenital anterior abdominal wall defects. The register was set up in July 1994, modelled on the northern region Congenital Anomaly Survey, with an emphasis on notifications from multiple sources and prenatal diagnoses. The register runs in conjunction with the regional perinatal mortality survey, ensuring high ascertainment of anomalies resulting in termination, fetal loss, and infant death.

The prevalence rate for gastroschisis in 1995-6 in the West Midlands was higher than that reported by Stone et al. This implies either that the geographical trend does not extend to the West Midlands or that the prevalence of gastroschisis is continuing to increase.

Ascertainment levels of regional anomaly registers should be high with the availability of termination data and information from departments of paediatric surgery. Data on maternal age allow age standardised rates to be produced for gastroschisis. In this way any real differences in the north-south prevalence of abdominal wall defects could be identified.

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1 Stone DH, Rimaz S, Gilmour WH. Prevalence of congenital anterior abdominal wall defects in the United Kingdom: comparison of regional registers. *BMJ* 1998; 317:1118-9. (24 October.)
2 Tan KH, Kilby MD, Whittle MJ, Beattie BR, Booth IW, Botting BJ. Congenital anterior abdominal wall defects in England and Wales 1987-93: retrospective analysis of OPCS data. *BMJ* 1996;313:903-6.
3 Roeper PJ, Harris J, Lee G, Neutra R. Secular rates and correlates for gastroschisis in California (1968-1977). *Teratology* 1987;35:203-10.
4 Hemminki K, Saloniemi I, Kyyronen P, Kekomaki M. Gastroschisis and omphalocele in Finland in the 1970s: prevalence at birth and its correlates. *J Epidemiol Community Health* 1982;36:289-93.

Incidence of abdominal wall defects according to former South East Thames and North West Thames registers, 1992-6

	South East Thames		North West Thames		Both regions	
Abdominal wall defect	**No of cases**	**Incidence**	**No of cases**	**Incidence**	**No of cases**	**Incidence**
Total No of births and terminations*	252 462	—	237 614	—	490 076	—
Exomphalos	63	2.5	108	4.55	171	3.5
Gastroschisis	67	2.8	49	2.06	116	2.37
Abdominal wall defects	130	5.15	157	6.61	287	5.86
Ratio of gastroschisis: exomphalos	1:0.9	—	1:2.2	—	1:1.47	—

Incidence per 10 000 total births *Includes live births, still births, and terminations.

Prevalence of abdominal wall defects according to West Midlands congenital anomaly register, 1995-6

Abdominal wall defect	**No of cases**	**Prevalence* (95% CI)**
Total No of births	135 420	—
Omphalocele	47	3.5 (2.5 to 4.5)
Gastroschisis	40	3.0 (2.0 to 3.9)
Both	87	6.4 (5.1 to 7.8)

*Cases per 10 000 total births.

Bruising associated with paediatric fractures

Each case should be treated individually

Editor—I am concerned that a letter I wrote in 1987 has been misquoted in Mathew et al's article.[1 2] They attribute to me the assertion that "the force needed to fracture a normal bone is thought to result invariably in external evidence of trauma."[2] At no time have I made such a statement. What the letter actually said (in relation to infants with large numbers of fractures) was that "there was remarkably little clinical evidence of the trauma that would have been needed had the bones been normal."

The presence or absence of external evidence of trauma is a factor that should contribute to the assessment of the cause of fractures in a child. The lack of bruising or other evidence of trauma is a more significant pointer to a disorder of bone when the fractures are transverse (implying local force), recent, and multiple. To this can now be added the insight of Mathew et al, that displaced fractures are more likely to be accompanied by bruising than are undisplaced ones. Each patient needs to be considered individually for evidence of accidental injury, non-accidental injury, and bone disorder.

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1 Paterson CR. Child abuse or copper deficiency? *BMJ* 1987;295:213.
2 Mathew MO, Ramamohan N, Bennet GC. Importance of bruising associated with paediatric fractures: prospective observational study. *BMJ* 1998;317:1117-8. (24 October.)

Arterial blood gases and acid-base balance

Knowledge of bicarbonate concentrations is needed to assess respiratory failure

Editor—Williams has not emphasised the importance of looking at the bicarbonate concentration in patients presenting with respiratory failure.[1] Depressingly often, intensive care doctors are presented with patients rendered unnecessarily hypoxic by the casualty or medical teams: any patients with a raised arterial carbon dioxide concentration are immediately starved of oxygen in case they stop breathing. The teams should look at the bicarbonate concentration. If it is normal this virtually proves that the respiratory failure is of acute onset, metabolic compensation having not had time to occur; it is then safe to give a high inspired oxygen concentration. If the bicarbonate concentration is abnormally raised this suggests that the patient has long term carbon dioxide retention; then a more cautious approach to oxygen treatment is justified.

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Allen's test is not routinely used before radial arterial puncture

Editor—In his article on arterial blood gas analysis Williams repeats the common advice to perform a modified Allen's test before attempting radial artery puncture.[1] My impression is that this advice is never carried out in practice, and a survey of anaesthetist colleagues confirmed that none of six specialist registrars and eight consultants (with a combined experience of several thousand radial artery punctures) used the test routinely.

Allen's test has a poor sensitivity and specificity for complications after radial artery cannulation. In a series of 1699 patients undergoing arterial cannulation for coronary artery surgery, 16 of 411 who had an Allen's test had abnormal results. None of these 16 had complications from radial arterial cannulation.[2] Mandel and Dauchot have reported serious complications in 2 of 982 patients who had a normal result of an Allen's test before radial arterial cannulation.[3]

The available evidence does not support the routine use of Allen's test before radial artery puncture. Nevertheless, because of the rare incidence of serious complications, common sense suggests that all patients should have regular clinical observation of their hand and finger blood supply after arterial puncture or cannulation.

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1 Williams AJ. ABC of oxygen. Assessing and interpreting arterial blood gases and acid-base balance. *BMJ* 1998; 317:1213-6. (31 October.)
2 Slogoff S, Keats AS, Arlund C. On the safety of radial artery cannulation. *Anesthesiology* 1983;59:42-7.
3 Mandel MA, Dauchot PJ. Radial artery cannulation in 1000 patients: precautions and complications. *J Hand Surg* 1977;2:482-5.

Professional self respect

Professional foul?

Editor—The provenance of a wine often determines its worth, and so must it be for authors. Hence the *BMJ* asks for "each author's current appointment and full address." It remains intriguing why in October Richards chose to describe himself in his article as president of Hughes Hall, Cambridge,[1] a post he had recently started in, when for three years until September 1998 he had been employed as medical director to Northwick Park and St Mark's Hospitals in Harrow. To limit his location to Cambridge is perhaps not a high crime or misdemeanour, but he knew that he would be back in November 1998 as part time medical director of Northwick Park and St Mark's Hospitals for another five months. No conflict of interest? Or simply an interest in conflict from afar?

Richards asserts that a local strategy developed that allowed time for non-NHS commitments in exchange for a "real and regular commitment to NHS emergency services at nights and weekends" and partly by "giving up one paid session."[1] He floated this idea at Northwick Park and St Mark's NHS Trust, but it foundered quickly. As chairman of the BMA local negotiating committee I responded to his strategy document with an open letter attempting to clarify contractual issues as they existed nationally; it could not as he hoped be manoeuvred unilaterally. Alas, he quoted only a small part of this letter (sent to all members of the medical staff committee of the trust) in his article.

Richards contends that the contract benefits only the "minority of consultants who earn substantial amounts outside the NHS." In a report on private medical services the Monopolies and Mergers Commission stated that 17 100 (74%) of the 23 100 consultants in the health service in 1992 were engaged in private practice[2]—hardly a minority. The median net private earnings were £17 000 a year—not substantial but certainly helpful.

Richards's discussion of consultants' contracts exposes his antipathy to private practice. He ignores problems in the NHS of low staff morale and motivation and poor retention and recruitment of staff. Surgeons cannot operate because beds and nurses are unavailable, not because they are moonlighting in the private sector. He suggests a pay for work contract but does not appreciate the enormous difficulty there is in trying to equate workload and productivity with health care. Any system built on differentials of basic pay between similar professionals will lead to fragmentation of the NHS. Richards recommends altruism. He should show this and avoid unwarranted criticism of his colleagues' acknowledged professionalism.

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1 Richards P. Professional self respect: rights and responsibilities in the new NHS. *BMJ* 1998;317:1146-8. (24 October.)
2 Monopolies and Mergers Commission. *Private medical services.* London: HMSO, 1994.

Potential conflicts of interest were not made clear

Editor—From the sanctuary of Hughes Hall, Cambridge, Richards writes at length about the current consultant contract.[1]

In the days when he was medical director at Northwick Park and St Mark's NHS Hospital Trust (as he still is, although part time) and I was chairman of the medical staff committee, he instituted the annual review of consultants' job plans with the chief executive officer, the medical director, and the relevant clinical director. To my knowledge, no part time consultant was found to be deficient in commitment and work for the trust. Some were found to be overzealous and advised to reduce the number of fixed sessions. A considerable number were able to show that they would be able to work more efficiently and productively if the hospital support services—for example, secretarial, operating theatre, paramedical, laboratory, and so on—were not being shredded by annual recurrent cost savings. This was the thrust of our SOS letter to the secretary of state.[2]

Interestingly, there were serious deficiencies among a group of consultants whose job plans were new. Newly appointed consultants, especially surgeons, were grossly deficient in that the necessary facilities for them to be able to work were not available. I surveyed newly appointed consultants and only by 6-9 months were they starting to do useful work, often in "borrowed" outpatient or operating theatre sessions.

Besides not mentioning that he would be medical director (albeit part time) from November 1998 for several months, Richards also did not declare a relevant major financial interest. In addition to his salary as medical director, he has continued to benefit from a maximum distinction award in thoracic medicine by dint of an honorary ex-officio clinical contract to cover a minimal clinical commitment in general medicine.

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1 Richards P. Professional self respect: rights and responsibilities in the new NHS. *BMJ* 1998;317:1146-8. (24 October.)
2 Richards P, Gumpel M. Save our service. *BMJ* 1997;314: 1756-8.

More laboratories should test for *Dientamoeba fragilis* infection

Editor—*Dientamoeba fragilis* was first described by Jepps and Dobell in 1918[1] and has subsequently been shown to be an important enteric pathogen. It is therefore surprising that only a few laboratories look for this pathogen. *D fragilis* infections are effectively treated with di-iodohydroxyquinoline or tetracyline.[2] In contrast, no treatment is available for *Cryptosporidium* spp, which most laboratories screen for routinely.

Successful diagnosis of *D fragilis* is closely associated with the use of stained faecal smears. After using a suitable faecal stain as part of our routine methodology we found *D fragilis* to be the most common enteropathogen (occurring in 5.1% of faecal samples) in the Sultanate of Oman.[3] A similar incidence (5.0%) has been reported recently among American soldiers stationed in Egypt.[4] In the United Kingdom, however, few laboratories stain faecal smears. The laboratory reports of the Communicable Disease Surveillance Centre show that in 1992, 68 cases of *D fragilis* infection were reported from seven laboratories and that by 1996 this figure had increased to 231 cases reported from 20 laboratories (unpublished data). These results reflect an increase in the number of laboratories performing faecal stains. It can be assumed, however, that the true incidence of *D fragilis* infection is many times higher: there are an estimated 450 diagnostic laboratories in the United Kingdom, most of which do not look for this pathogen.

Clinicians should add dientamoebiasis to their differential diagnosis in patients presenting with abdominal pain, diarrhoea, unexplained flatulence, nausea, and vomiting. Indeed, requests from clinicians to their laboratories to look for this organism will result in this neglected pathogen taking its rightful place alongside the more established enteropathogens. Ultimately, patients will be the true beneficiaries.

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Compensation for needlestick injury is profoundly mistaken

Editor—A doctor has received £460 000 compensation for a mental health problem which she developed after a needlestick injury at work. I too have had needlestick injuries at work; I have also been physically assaulted by patients three times. On one occasion I was half strangled and repeatedly beaten over the head with a piece of broken furniture and kicked so that my body was covered with bruises. I had a cup of tea and a cry and completed my day's work. I did receive £750 from the criminal injuries compensation board, which I gave to a charity, but I would have preferred my drug abusing assailant to be punished, which he was not. Nor did the health authority send me a note of sympathy, a bottle of wine, or any kind of recognition—in fact it threatened me with dismissal when four years later I refused to have my assailant, who never served a day in jail for the assault, allocated to my list.

I suffered far more as a result of a formal complaint by another patient which was energetically pursued by the health authority despite its obviously malicious and trivial nature. I was completely exonerated, but the whole procedure ruined half a year of my life. Although the complainant proved himself a liar in his conflicting evidence, which the investigating board acknowledged, I received no apology, nor he any reprimand. I was kept waiting for five weeks to be told the result of the hearing, for purely administrative reasons, although I had been exonerated unanimously. I would have thought I had a better case for compensation against the authority for this incident than if I had managed to get a needle into my skin.

Thousands like me have kept the NHS going by being very brave boys and girls. Few doctors I meet believe that their sacrifices of personal and family wellbeing for the health service have been sufficiently rewarded with either thanks or money; indeed, some of our bad experiences have been caused by managerial insensitivity.

I believe that the decision to award one doctor £460 000 is profoundly mistaken. If it sets any kind of precedent then it will no longer be just disgruntled patients suing the living daylights out of the NHS, but also its workers, who in some ways have stronger cases.

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Most doctors see consent from functionalist perspective

Editor—Alderson and Goodey's article considers the different theoretical perspectives on the practical issue of consent.[1] A study recently conducted by a multidisciplinary team at Queen's Medical Centre, Nottingham, supports the authors' assertion that consent is understood in different and sometimes conflicting ways by medical staff.

A questionnaire asked preregistration house officers, senior house officers, and nurses from various specialties about training received in obtaining consent, training required, and important issues regarding consent. The questionnaire included vignettes or case studies where respondents stated what action they would take with regard to consent in certain situations—for example, when faced with a confused elderly man requiring surgery. The questions relating to the case studies aimed at determining respondents' knowledge of legal issues involved in obtaining consent (for example, the age at which a person can consent to treatment) and which elements constitute valid consent and how they would respond to patients who are confused or refuse treatment.

The findings show that doctors and nurses understand consent by using differing theoretical models. Doctors often stated that the main problem encountered when obtaining consent from patients was their own lack of knowledge and experience of specific procedures and the risks involved. They highlighted this as a training need. Most doctors saw consent from a functionalist perspective, as a one sided delivery of information. Few doctors commented on communication issues, which would require a more critical theory approach, where information is a two way exchange between doctor and patients. This approach was more favoured by the nurses in the sample.

When asked about patients refusing proposed treatments, many doctors suggested sectioning under the Mental Health Act (1983), although, given the context of the scenarios, this would often constitute a misappropriation of the act. This supports Alderson and Goodey's view that "functionalist consent is … a token of respect that is hardly necessary because benign, expert doctors contribute to the smooth functioning of society; refusal and non-compliance are irrational."

We agree with Alderson and Goodey that consent is too complex to be explained by any one theoretical model, although our study confirms that most doctors take a functionalist approach. We intend to use the survey to implement a training programme that, in addition to meeting the training needs expressed by the respondents, allows them to consider conflicting ways of approaching the theoretical as well as the practical issues involved in obtaining informed consent from patients.

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Consent of relatives is neither necessary nor sufficient for treating incompetent adults

EDITOR—Hooker is right to point out that the consent of relatives is neither necessary nor sufficient for treating an incompetent adult.[1] Despite this, the practice of seeking relatives' consent is widespread; many practitioners even get relatives to sign consent forms. This is not only legally ill founded but also ethically questionable. The main worries are, firstly, that it diverts the clinician's attention away from what should be his or her main focus, the patient's best interest and, secondly, that we cannot always be sure that relatives do have the best interest of the patient at heart. In practice problems are rare, but, as Morris shows, they do occur.[2]

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Full information about trials might be given retrospectively to participants

EDITOR—One of the central issues in the debate about informed consent in randomised controlled trials[1] has been the ethics of withholding the fact of randomisation from participants—for example, in the trials of support after stroke[2] and of management of sore throat.[3] Psychologists recognise that it is sometimes necessary to withhold full information from participants, to minimise bias. Our professional code of conduct (http://www.bps.org.uk/charter/codofcon.htm) states that in these situations we must "provide … full information retrospectively about the aims, rationale and outcomes of the procedure as far as is consistent with a concern for the welfare of participants." Maybe a similar requirement could be introduced for medical researchers. The planned debriefing of participants would avoid the potential distress of those who discover from sources other than the investigators that important information about the research they had participated in was withheld from them.[4]

The most practical option would be to include the debriefing information with a summary of research findings offered to the participants, with an invitation to contact the researcher for a verbal discussion if the participant wishes. The possible psychological harm that may result from partial informed consent has been much discussed.[4] The introduction of debriefing may help to alert investigators to the existence and extent of any such harm. Formal assessment of participants' feelings after debriefing, and research investigating lay views about informed consent in different types of trials, would provide additional evidence to help guide research practice.

I agree with Warnock that we need to "distinguish things that differ."[5] The introduction of debriefing, and research on patients' views and experiences, would help us to do this. For example, do patients feel differently about being randomised to different treatments without full informed consent (as in the trial in sore throat[2]) than they do about being randomised to other, non-treatment interventions, such as additional social support,[1] information leaflets, or health promotion advice? Do participants believe that not being informed of randomisation is acceptable if they are assigned to the control group receiving routine care but not if they are assigned to the intervention group receiving a new type of care?

The answers to questions like these would help investigators to make their own informed decisions about the amount of information to provide at the time of consent.

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Control of multidrug resistant tuberculosis

DOTS-plus strategy will be hard to implement

EDITOR—Farmer and Kim propose a "DOTS-plus" strategy to attempt controlling multidrug resistant tuberculosis.[1] As much as I sympathise with them, after following several patients with multidrug resistant tuberculosis and watching them die, I am concerned and sceptical about such a strategy being implemented in low income areas.

The development of multidrug resistant tuberculosis is a complex multifactorial process, and wide distribution of new drugs will not solve the problem. Need to generate an income, family duties, religious misconceptions, social stigma, and mismanagement by health practitioners constitute only a limited list of some of the obstacles that patients must face before successful treatment. Drugs used to treat multidrug resistant tuberculosis are not very effective, often have undesirable side effects, and must be given for prolonged periods. Who will supervise such complex regimens? Who will observe the prescribers? Directly observed therapy (DOT) requires directly observed doctors,[2] but DOTS-plus will make double direct observation of doctors mandatory.

We must always attempt to treat and cure individual patients, but initiating a "DOT-plus" strategy at a national level is, at present, a dream; it risks diverting our limited resources and causing epidemiological havoc. We should not awaken one day only to realise that our dream has become a microbiological nightmare.

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National cancer centre is good idea

EDITOR—Britain is indeed one of the few countries not to have a single focus for teaching and research for cancer, as Waxman and Gibson point out.[1] Even in many low resource environments considerable effort has been made to concentrate expertise. At a time when cancer treatment is likely to change owing to the impact of new molecular treatment strategies, the need for a leading institution has never been greater. But Waxman and Gibson stop short of being specific.

Where should such a centre be located, and how should it relate to existing institutions? Do we really need massive new funding, or can it be created out of existing resources? Most national cancer centres are based in capital cities. London already has the United Kingdom's largest concentration of cancer research, care, and education in an amalgamation of the Institute of Cancer Research, the Royal Marsden Hospital, Imperial College (University of London), the Hammersmith Hospitals, and the Imperial Cancer Research Fund. Already good clinical and scientific collaboration exists between these groups. If we brought in the huge volume of international practice that is in many small and poorly organised private

hospitals in west London we would really have a powerful grouping, both intellectually and financially.

What is needed to bring this concept to fruition is political will and capital investment by the public and private sectors, jointly, to create a single site. This would defuse the usual interpersonal bickering that characterises hospital and university mergers. The centre would be a natural site to coordinate the new structure of cancer centres and units that is gradually building up throughout the United Kingdom.

As well as developing the treatments of the future such a centre would monitor the availability of care throughout the United Kingdom and ensure equity of access; act as a gold standard for cancer care and form part of the National Institute for Clinical Excellence; be an attractive site for the pharmaceutical industry, which spends an estimated £150m a year on cancer research in Britain; and provide an international focus at a time of great global change in the way in which cancer is managed.

Persuasion, imagination, and determination are the keys to this ambitious project; let us hope that now this debate has been started it will gather momentum.

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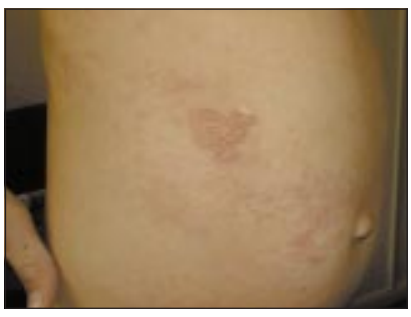
Dermatology opinions via intranet could reduce waiting times

EDITOR—We have just piloted a new telemedicine program called Dermaclinic (from Agora Healthcare) which runs on any computer that can access the internet. It permits doctors to send, via a confidential intranet, digital photographs (figure) and clinical histories for a returnable second opinion via the same medium. Its clinical database is backed up on a server owned by the practice and accessible only through a password held by practising clinicians.

Our results have been encouraging. During the three month pilot, 37 photographs from 26 cases were transmitted, and of these only three images (8%) were recorded as unsatisfactory for diagnosis and in only five cases was further direct referral requested. The service was considered unsafe for pigmented lesions or any situation that required visualisation of the whole patient, palpation of the skin, etc—cases for which there is no substitute for a live consultation. Consultant opinion over a wide range of cases was obtained with Dermaclinic within 1-18 days (average 10.3 days), whereas the current dermatology waiting list is 13 weeks. During the last month of the pilot only eight cases were referred to outpatients, but in the subsequent month, without the Dermaclinic service, referrals rose by 75% to 14 cases.



Digital photographs and case histories can be sent via intranet for a second opinion

The average time taken by general practitioners to enter a case was 10 minutes. The consultant's time was shorter but variable and is being monitored.

The cost of the Dermaclinic service includes tailored software, digital camera, and appropriate support. The intranet service on which it runs is available free (apart from local telephone charges) on www.mdintranet.org.uk. A dermatology opinion is one of the most frequently requested NHS services, and these Dermaclinic costs must be set against an average local cost of £62 for an ordinary referral.

If Dermaclinic becomes established it should be made equally available to all patients in a district and established within consultants' existing workload. Its low cost and simplicity are very appealing, and it could reduce NHS waiting. The system has the added bonus of accumulating valuable local general practice teaching material as well as baseline records to evaluate outcomes.

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Competing interests: The authors have no commercial interest in the Dermaclinic software. MD receives sponsorship to run the free intranet that is one possible vehicle for the software.

Authors defend methods used in their paper

EDITOR—In his editorial[1] Appleby discusses our paper on obstetric care and the proneness of infants in traumatic births to suicide as adults.[2] He gives the incorrect impression that we did not consider the potential confounding effect of year of birth and fetal hypoxia (asphyxia). We did so by including these variables in the multivariate regression—as is evident from the text and table A in the additional information given on the *BMJ*'s website. If year of birth is forced into the regression the estimated relative risk as well as the significance of the trauma score is not reduced.

In the editorial Appleby claims that our findings are presented as supporting the imprinting hypothesis and that imprinting is the causal mechanism. This is not correct. The hypothesis served only in the design of the study and to predict the results. We would never have said that our data support the hypothesis. We could have said that the hypothesis cannot be rejected on the basis of the results of the study, which is an entirely different conclusion. But we did not even do this, and the hypothesis in not mentioned in the discussion. The essence of our paper is not the hypothesis but the statistical findings.

Appleby suggests that any link between obstetric care and violent suicide occurs through mental illness, possibly caused by fetal hypoxia. As we stated in our paper, we cannot exclude the possibility that it may be the circumstances giving rise to the need for a traumatic intervention that cause the increased risk of suicide, rather than the intervention itself. Our results, however, do not support the notion that fetal hypoxia is the causal factor.

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Severe deep white matter lesions and outcome in major depressive disorder

Might vasculitis be cause of these lesions in elderly depressive patients?

EDITOR—The study by O'Brien et al clearly shows that deep white matter lesions on magnetic resonance imaging are relevant to the outcome in elderly depressed patients.[1] It is not only psychiatrists for whom this result is important. Although the authors excluded all patients who had a known history of other diseases, depression with such an organic correlate might be secondary to a distinct disease of the central nervous system.

Neuropathological studies are difficult to perform. Interestingly, deep white matter lesions have also been described in vascular diseases such as systemic lupus erythematosus[2] and Behçet's disease.[3] Patients with these diseases may present with depressive symptoms, possibly as a correlate of vasculitis in the central nervous system.

There is a good chance that the deep white matter lesions described by O'Brien et al represent localised vasculitis in the central nervous system. As patients with these lesions had a poor outcome, with a median survival time of only four months, clinical trials of immunosuppressive treatment would be justified despite lack of a proved pathogenetic mechanism. In severe forms of vasculitis in the central nervous system, regimens of corticosteroids, chlorambucil, and cyclophosphamide are well established.[4]

Lower doses of immunosuppressants might help to prevent serious side effects in this group of geriatric patients.

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Further investigation of deep white matter lesions is necessary

Editor—O'Brien et al have shown that, in elderly patients with depressive disorder, severe deep white matter lesions are associated with a poor outcome, as measured by the quality of recovery from depression and time to relapse or recurrence, or both.[1] Our group too has completed a follow up study. Of 44 subjects who had undergone magnetic resonance imaging three years earlier,[2] 37 were evaluated for follow up with a structured review of case notes with or without personal interview, the same categories being assigned as in O'Brien et al's study.

Our findings broadly correspond with those of O'Brien et al. The presence of deep white matter hyperintensities was associated with poor overall clinical outcome, the poorest mean survival time (31.58 months to death in those with large confluent deep white matter lesions *v* 33.11 months in those without), higher residual depression rating scores (14.7 *v* 8.7), and a higher average number of relapses (3.63 *v* 1.05; all $P < 0.05$).

There were two additional findings. Firstly, specific lesions were associated with incomplete recovery or chronicity of depression: those in the pontine reticular formation and more than five Virchow-Robin spaces in the basal ganglia. Secondly, grade 3 periventricular lesions (deep irregular lesions) were associated with an increased risk of developing a dementia syndrome (*Diagnostic and Statistical Manual of Mental Disorders* (DSMIV) criteria being used). Twelve patients had deep irregular hyperintensities and five developed a dementia syndrome (χ^2 test for association of periventricular change to development of dementia during follow up = 18.09, df = 9, $P = 0.034$; table).

Association of periventricular change with dementia during follow up in 37 patients

	Dementia developed	
	Yes	No
Periventricular hyperintensity:		
Absent	0	10
Capsulated	0	11
Smooth halo	1	3
Deep irregularity	5	7

Whether depression in old age is associated with a higher rate than expected of developing dementia is unclear. Certain subgroups—notably those with cognitive impairment at the outset of their depression—have a much increased risk.[3] Periventricular lesions and deep white matter lesions may have different clinical implications. Periventricular lesions may be more relevant in the prediction of dementia, whereas deep white matter lesions influence the outcome depression. Further investigation of deep white matter lesions should be undertaken to establish more precisely their relevance to prognosis and treatment strategies, but periventricular lesions should not be overlooked as a potential marker of later dementia.

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Ipratropium does indeed reduce admissions to hospital with severe asthma

Editor—We agree with Plotnick and Ducharme that inhaled anticholinergics should be added to β_2 agonists for treating acute asthma in childhood and adolescence.[1] In a follow up to our original study[2] we conducted a large (434 children) prospective double blind study in children with moderate or severe asthma treated with two doses of ipratropium bromide.[3] Briefly, we found that the rate of admission to hospital was reduced from 52.6% to 37.5% in children with severe asthma. Thus, 6.6 children with severe asthma (95% confidence interval 3.7 to 29.4) would need to be treated with ipratroprium to avoid one admission to hospital. Ipratropium had no effect on the rate of admission of children with moderate asthma. Our study therefore strengthens the authors' conclusions regarding the use of ipratropium bromide in acute asthma.

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UK trial of extracorporeal membrane oxygenation gave biased estimate of efficacy

Editor—Roberts et al say that extracorporeal membrane oxygenation lowers neonatal mortality at an acceptable cost.[1] Their analysis, however, is based on the United Kingdom trial of extracorporeal membrane oxygenation, in which babies were randomised to receive either conventional mechanical ventilation in one of 55 neonatal units or extracorporeal membrane oxygenation in one of only five intensive care units.[2] The five units providing extracorporeal membrane oxygenation had substantially more facilities than many of the 55 neonatal units.

Fewer children died in the group given extracorporeal membrane oxygenation, but we cannot be sure that this difference was because of the extracorporeal membrane oxygenation. Mortality may have been lower in this group because of better care in the units that provided extracorporeal membrane oxygenation: there is substantial evidence that centralisation of intensive care services for children reduces mortality.[3] Indeed, the relative risk of 0.55 in the trial of extracorporeal membrane oxygenation is strikingly similar to the odds ratio of 0.48 in our comparison of paediatric intensive care in Victoria (centralised) with that in Trent (decentralised).[3]

There were good practical and ethical reasons for the design of the United Kingdom study of extracorporeal membrane oxygenation. The decision to leave control babies at the referring hospital (rather than sending all babies to the centres for extracorporeal membrane oxygenation) means, however, that we do not know whether the observed difference in mortality was due to extracorporeal membrane oxygenation or to other differences in management.

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