Letters

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PFI rides again

Scepticism remains at the grassroots

EDITOR—I was surprised that McGinty thought that clinicians in Hereford were "satisfied with the result" of the plans for the private finance initiative.[1] Few of the hospital doctors in Hereford whom I have talked to are confident that the new smaller hospital will have enough beds to cope with local demands, and general practitioners have consistently maintained through the local medical committee that they believe that the new hospital will be too small. The county already has an efficient network of general practitioners and community hospitals in the market towns, and there is little slack in the system.

Perhaps one reason why McCloskey and Deakin maintain that hospital admission rates have not risen in Hereford[2] is because it is such a struggle to have a patient admitted. Indeed, the hospital was recently closed to admissions. Being told that my patient with pneumonia and status epilepticus was number four on the waiting list for admission gave me little confidence in the hospital's current ability to cope. I recently opted to manage a patient with a pulmonary embolus and a patient with a haematemesis and a haemoglobin concentration of 75 g/l at home because of the problems in finding beds, which I was very unhappy about. I am also dubious about trusting information from a new computer system that has not been able to give us waiting list figures since the summer.

In planning our hospital under the private finance initiative scheme the size seems clearly to have been dictated by the affordability and then the planners have tied themselves in knots working out how such a small hospital could possibly meet local needs. Construction companies are not philanthropic bodies: they are profit making organisations that will extract money from the public purse to benefit their shareholders. No amount of sophistry will make me believe otherwise.

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1 McGinty F. Private finance initiative. *BMJ* 2000;320:250-1. (22 January.)
2 McCloskey B, Deakin M. Private finance initiative. *BMJ* 2000;320:251. (22 January.)

The future does not bode well

EDITOR—McGinty's letter gives the impression that everything is under control in Herefordshire and that future patients' care is assured.[1] This, however, is far from our view as general practitioners. He talks about more home care within the NHS led by primary care. We, as yet, have not even been approached about this policy—surely the cooperation of general practitioners is the first thing that is needed to see whether it can work.

We do not have a community hospital in the city to replace the one that was abandoned some 10 years ago. So far as I know, we are unlikely to ever have one. A few beds are planned in nursing homes, but this will be an inadequate replacement. As general practitioners, we have consistently found it difficult to get patients admitted to our hospital over the past few months. What will happen when the bed numbers are so much smaller with the new hospital is a cause of worry to us all.

The local medical committee has argued against the proposed drastic reduction in inpatient facilities and has always been told that this is all that can be afforded. It is not a question of need but of cost limitation.

Inevitably, with more ill patients in the community, more time will be needed from general practitioners. However, the medical practices committee is unlikely to allow an increase in our numbers.

I do not want to be alarmist, but I do not think the future bodes well.

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1 McGinty F. Private finance initiative. *BMJ* 2000;320:250-1. (22 January.)

Scheme was the lesser of two evils

EDITOR—McGinty's letter on the private finance initiative in Hereford is potentially misleading.[1] Hereford's hospital clinicians were presented with the option of accepting a new hospital on one site but with fewer beds or continuing on three separate sites in obsolete and decaying buildings. The clinicians reluctantly accepted the private finance initiative scheme as the lesser of two evils.

We believe that a hospital of 340 rather than 250 beds would best serve the needs of the population, and this view is now vindicated by the national beds inquiry.

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1 McGinty F. Private finance initiative. *BMJ* 2000;320:250-1. (22 January.)

Whatever happened to the NHS's response to the *BMJ*'s articles?

EDITOR—Last July the *BMJ* published four articles criticising the private finance initiative for building new hospitals.[1] Towards the end of them Colin Reeves, director of finance and performance in the NHS Executive, said that the arguments were mistaken and that he would submit an article defending the initiative.[2]

What has happened to that article?

An official statement last year from the Labour party said: "[The government would] build on the success of the private finance initiative and ensure it continues to thrive when the existing Treasury task force is wound up. We have established a second review of the PFI under Sir Malcolm Bates to ensure that a steady flow of PFI and private-partnership deals contribute to a higher sustainable level of investment in public sector infrastructure."[3]

I have tried more than once, as a member of the Labour party, to obtain details of the party's arguments in favour of the private finance initiative, particularly in

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relation to the UNISON report on north Durham hospitals,[4] but in vain; my letters have gone unanswered.

So the private finance initiative goes ahead in a cloud of secrecy about the final cost. Is there any way of dispersing the cloud?

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1 Gaffney D, Pollock AM, Price D, Shaoul J. NHS capital expenditure and the private finance initiative—expansion or contraction. *BMJ* 1999;319:48-51. (3 July.) (First of four articles on the private finance initiative. The three other articles were published in *BMJ* 1999;319:116-9; 179-84; 249-53)
2 Reeves C. Economics of PFI in the NHS. *BMJ* 1999;319: 191. (17 July.)
3 *Consultation document on economic policy.* London: Labour Party, March 1999. (Published for the Labour party's local policy forums.)
4 Gaffney D, Pollock A. *Report to UNISON northern region on the North Durham Acute Hospitals PFI scheme.* London: UNISON, 1999. (www.unison.org.uk)

We're still waiting

We have not had an article on the private finance initiative submitted to us from the NHS Executive. We are not assuming, however, that this means that it now agrees with our criticisms of the initiative.

We have heard rumours about activities in the executive. The first rumour was that the executive had hired management consultants to produce a response. The second rumour was that a paper was written but was unacceptable because it mostly comprised attacks on the authors of the articles rather than cogent arguments in favour of the private finance initiative.

Thus we are still waiting, but, in the meantime, we are preparing articles on alternatives to the initiative. Perhaps we'll get in first.

Richard Smith *editor, BMJ*

Choosing between home and hospital delivery

Home birth in Britain can be safe

Editor—Drife's assertion that hospital birth is three times as safe as planned home birth is misleading.[1] Since the study groups were dissimilar it is about as helpful as saying that a man and a dog have an average of three legs. He is also wrong to say that "no recent audit of the safety of home delivery in Britain is available." Just such an audit has been running here for 18 years.[2] There has been no intrapartum death and only one neonatal (0-27 day) death in the past 15 years among the estimated 3400 mothers (0.6%) who were booked for home birth when labour started. The comparable figure for all such births in this region for these years (1984-98), after lethal malformation and babies weighing less than 2.5 kg are excluded, is 1:921 (587/540 830). That home birth has become statistically "safer" than hospital birth is not, of course, unexpected, as high risk mothers seldom press for home delivery.[3]

National figures also exist. The comparable figure for all booked home births in 1994-5 nationally, as established by the Confidential Enquiry into Stillbirth and Death in Infancy, was 1:1113 births (22/24 484), although this denominator includes unplanned home birth and excludes transfers in labour.[4] This is similar to the rate in non-malformed births of ≥2.5 kg in these two years (1143/1 224 856, or 1:1072 births). The National Birthday Trust study, which did collect accurate denominator data during 1994, encountered two stillbirths and three neonatal deaths among the 4665 mothers still booked for a home birth at 37 weeks' gestation (1:933 births).[5]

We agree that women should be able to choose between home and hospital delivery. They also need accurate and balanced information. Unfortunately, that is not what Drife gave those who read his letter to the *Times* of 20 May or the letter he sent the *BMJ*. He did not compare like with like, and he merged groups who should be advised differently. Most women can be told that, as long as they continue to accept professional advice, they are as safe delivering at home as in hospital. For others with a twin, breech, or post-term pregnancy the increased risk of home birth is probably even greater than Drife's figure suggests.

The current polarised argument is futile. Doctors and midwives would be better employed collecting the information needed for women to be given more individually specific advice. Women would then be more likely to believe what they are told during pregnancy and, even more importantly, during labour.

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1 Drife J. Data on babies' safety during hospital births are being ignored. *BMJ* 1999;319:1008. (9 October.)
2 Northern Region Perinatal Mortality Survey Coordinating Group. Collaborative survey of perinatal loss in planned and unplanned home births. *BMJ* 1996;313:1306-9.
3 Davies J, Hey E, Reid W, Young G. Prospective regional study of planned home birth. *BMJ* 1996;313:1302-6.
4 Confidential Enquiry into Stillbirths and Deaths in Infancy. *Fifth annual report.* London: Maternal and Child Health Research Consortium, 1998:51-62.
5 Chamberlain G, Wraight A, Crowley P. *Home births. Report of the 1994 confidential enquiry by the National Birthday Trust Fund.* Carnforth: Parthenon, 1997:107-13.

There is no evidence that hospital is the safest place to give birth

Editor—Drife's conclusions, arrived at after relating data from the confidential enquiry into stillbirths and deaths in infancy (CESDI) in England, Wales, and Northern Ireland in 1994 and 1995 to deaths in two studies in the United States and one study in Australia, are seriously flawed because he has not compared like with like.[1]

Direct comparisons cannot be made between these four datasets as there was no consistency in the definitions of categories of death included in the groups of births in which the deaths were compared, in the types of birth attendant, or in the content of the maternity care available. Although lessons can be learnt from the experience of other countries, conclusions should not be extrapolated from one healthcare system to another. This is why both editions of *Where to be Born?* focused on data collected in the United Kingdom.[2]

Drife did not mention any research on the subject in the United Kingdom published since 1994. Neither the National Birthday Trust Fund survey of 6044 planned home births in the United Kingdom[3] nor the prospective and retrospective studies in the former Northern Region of England[4 5] yielded results that would alter the key conclusion of *Where to be Born?*, which was that "there is no evidence to support the claim that the safest policy is for all women to give birth in hospital."[2] Furthermore, although the confidential inquiry's data on 22 intrapartum deaths among planned home births have been cited as "proof" that home births are dangerous, the inquiry's fifth annual report (1998) drew no such conclusions.

We strongly support the view that continuing audit is needed, however. CESDI's report highlighted the lack of "denominator data" about planned and unplanned home births. Such data can be collected at national level in England, using the existing infrastructure of the maternity hospital episode statistics. We therefore urge trusts who do not currently submit complete "maternity tail" data to do so. In addition, the former Northern region of England has led the way in auditing home births at a regional level. We look forward to seeing this audit extended southwards to Yorkshire and beyond.

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1 Drife J. Data on babies' safety during hospital births are being ignored. *BMJ* 1999;319:1008. (9 October.)
2 Campbell R, Macfarlane A. *Where to be born? The debate and the evidence.* 2nd ed. Oxford: National Perinatal Epidemiology Unit, 1994.
3 Chamberlain G, Wraight A, Crowley P. *Home births. Report of the 1994 confidential enquiry by the National Birthday Trust Fund.* Carnforth: Parthenon, 1997.
4 Davies J, Hey E, Reid W, Young G, for the Home Birth Study Steering Group. Prospective regional study of planned home births. *BMJ* 1996;313:1302-6.
5 Northern Region Perinatal Mortality Survey Coordinating Group. Collaborative study of perinatal loss in planned and unplanned home births. *BMJ* 1996;313:1306-9.

Risk of home birth in Britain cannot be compared with data from other countries

Editor—Drife has asked for recent audits on the safety of home and hospital deliveries in Britain to be made available.[1] He quotes data from home births in the United States and Australia, which include cohorts of women that were not so tightly screened as a UK population would have been. Hence they include many more women at higher risk of problems. Furthermore, in these countries transport arrangements from home to hospital in case of emergency differ from those in the United Kingdom.

Drife has not referred to the National Birthday Trust survey of home births in the United Kingdom.[2] In this survey, a group of 3896 women booked at home and delivered at home was compared with a group of similarly low risk women who were booked at hospital and who delivered at hospital. There was one neonatal death but no stillbirths in the home delivered group, and there were two stillbirths and two neonatal deaths in the hospital booked, hospital delivered group of 3319 women. These mortality figures were small compared with the national mortality rates, for the women had been screened for home booking and so were at lower risk. The perinatal mortality rate was not considered to be a useful measure when so few babies in each group died, and so we looked at other medical problems such as postpartum haemorrhage, resuscitation of the newborn, and those factors that the women thought important to their satisfaction. We concluded that there was no evidence that women who had been screened properly in the antenatal period and planned a booked delivery for home had any higher risk than a similar group of women who delivered in hospital.

These data have been considered reliable for the United Kingdom by most people who have considered them. Drife should bear them in mind when extrapolating statements for the United Kingdom from data from other countries where the population is cared for differently. Such data allow women to choose between home and hospital delivery, for, as he says, they have the right to be provided with up to date information.

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1 Drife J. Data on babies' safety during hospital births are being ignored. *BMJ* 1999;319:1008. (9 October.)
2 Chamberlain G, Wraight A, Crowley P. *Home births. Report of the1994 confidential enquiry by the National Birthday Trust Fund.* Carnforth: Parthenon, 1997.

Author's reply

Editor—When problems occur during a labour at home the woman is usually transferred to hospital. Chamberlain refers to one death among "3896 women booked at home and delivered at home," but his original report continued as follows: "There were two stillbirths and two neonatal deaths in the home booked/hospital delivered group (769 women). There were also three deaths (one stillbirth and two neonatal deaths) in the smaller group of women who had registered in the study but did not return their questionnaires (379 women)."[1] This makes a total of eight deaths, not one, and a rate of 1 death in approximately 600 births.

The fifth report of the confidential enquiry into stillbirths and deaths in infancy recorded 22 deaths among women booked for delivery at home.[2] The denominator can be calculated from the rate of home deliveries (1.84% in the previous year) and the total number of deliveries (677 759). This gives 12 471 home births and a death rate of 1 in 567. Both rates are similar to those from the United States and Australia quoted in my original letter,[3] though I agree that they differ from the remarkably low figure among Young and Hey's estimated 3400 mothers.

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1 Chamberlain G, Wraight A, Crowley P. *Home births. Report of the 1994 confidential enquiry by the National Birthday Trust Fund.* Carnforth: Parthenon, 1997:107.
2 Confidential Enquiry into Stillbirths and Deaths in Infancy. *Fifth annual report.* London: Maternal and Child Health Research Consortium, 1998.
3 Drife J. Data on babies' safety during hospital births are being ignored. *BMJ* 1999;319:1008 (9 October.)

Helicobacter pylori and myocardial infection

Excluding group with potentially higher rates of infection with H pylori could bias estimated odds ratio

Editor—We are writing in response to the paper by Danesh et al on infection with *Helicobacter pylori* and early onset myocardial infarction.[1] We believe that this thorough study is of interest because of its large sample size, and inclusion of sibling pairs and young people. We would, however, like to address the following points. Firstly, Danesh et al did not explain clearly how the controls were selected for the early onset case-control study. We concluded that they were chosen from the pool of spouses and relatives of the cases, which would make them unrepresentative of the general population.

Secondly, Danesh et al say that exclusion of cases with any history of gastrointestinal bleeding or peptic ulceration (both of which could be caused by certain cytotoxic strains of *H pylori*) would not have caused any underestimation as controls who reported these conditions were also excluded. Excluding this group with potentially higher rates of infection with *H pylori* could, however, bias the estimate of the odds ratio. For example, considering the case-control study, if we assume the same numbers (183) of controls as cases were excluded, and that 50% of the excluded cases and controls were positive for *H pylori*, the odds ratio calculation would be as in the table.

The odds ratio in the study was calculated as 2.28. Using our adjusted results, the odds ratio is decreased to 1.97. This shows that exclusion could have a significant effect. We have identified the following residual confounding factors. Danesh et al used current smoking as an indicator of smoking status. This may be inadequate as it does not take into account the duration and amount smoked. Also, they have not controlled for hypertension or diabetes, which are more prevalent in the cases than in the controls.

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Exclusion criteria were inappropriate

Editor—In response to the paper by Danesh et al regarding infection with *Helicobacter pylori* and myocardial infarction, we would like to draw attention to a possible source of bias in the study design.[1] This study relied on the pre-existing framework of the third international study of infarct survival (ISIS-3). Patients included in the *H pylori* study were selected from those recruited to ISIS-3, which tended to exclude patients with a history of gastrointestinal bleeding or peptic ulceration because of the nature of their studied treatments. As a result, Danesh et al set these conditions as exclusion criteria for remaining cases and all controls.

Although this was an appropriate measure in response to the limitations imposed by ISIS-3, we believe that this policy may have introduced a source of underestimation of the role of *H pylori* in coronary heart disease. *H pylori* is a known causal agent in peptic ulcer disease,[2] and exclusion of subjects with peptic ulcer disease therefore results in exclusion of those with *H pylori* infection. If there is an association between *H pylori* infection and coronary heart disease, we would expect a greater proportion of people with heart disease to be seropositive than people without heart disease. We would therefore also expect a greater proportion of people with heart disease to have peptic ulcer disease than controls.

Thus exclusion of subjects with peptic ulcer disease would have a greater effect on the prevalence of *H pylori* in cases than in controls. This would in turn lead to a reported association between *H pylori* infection and coronary heart disease that is weaker than in reality. This possible bias casts doubt on the conclusion by Danesh et al that a strong association can be excluded.

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Odds of non-fatal myocardial infarction in people aged 30-49 who were *H pylori* seropositive and in those who were seronegative

	Seropositive	Seronegative	Totals
Myocardial infarction	563	742	1305
No myocardial infarction	363	942	1305
Totals	926	1684	2610

1 Danesh J, Youngman L, Clark S, Parish S, Peto R, Collins R. Helicobacter pylori infection and early onset myocardial infarction: case control and sibling pairs study. *BMJ* 1999;319:1157-61. (30 October.)
2 NIH Consensus Conference. Helicobacter pylori in peptic ulcer disease. *JAMA* 1994;272:65-9.

Authors' reply

EDITOR—Our case-control study gave an adjusted odds ratio of 1.87 (99% confidence interval 1.42 to 2.47; $P<0.00001$) for the association of early onset myocardial infarction with seropositivity for *Helicobacter pylori*. This finding, together with a meta-analysis of previous reports, renews the possibility of some association between chronic infection with *H pylori* and coronary heart disease, but additional studies are needed to confirm or refute causality.

Some of the issues raised by Armitage et al and Das and Lehal were addressed previously in our report. Firstly, as is described in the methods section, controls for the study of early onset myocardial infarction were selected at random (matched for sex and age in five year bands) from among the eligible controls aged 30-49 years. The effects of any "overmatching" as a result of using controls who were family members of patients with myocardial infarction is not likely to be large (and the seroprevalence among our controls was about that expected in the general British population).

Secondly, exclusion of cases and controls with a history of peptic ulceration (or gastrointestinal bleeding) could have resulted in underestimation of the relevance of *H pylori* to myocardial infarction if certain strains were more strongly related both to peptic ulceration and to myocardial infarction than other strains, but so far there is no good evidence for the existence of such strains.[1] Any material overestimation owing to exclusion of such cases and controls is still less plausible since, as the example provided by Armitage et al demonstrates, it requires the assumption that *H pylori* is associated with myocardial infarction among those without a history of peptic ulceration or gastrointestinal bleeding but not among those with such a history.)

Thirdly, treated diabetes and hypertension were not strongly associated with *H pylori* serology in this study or in a synthesis of previous studies with information on 10 000 participants,[2] and adjustment for these factors in our study did not materially alter the odds ratios. Similarly, our study had detailed information on smoking habits,[3] but its inclusion in adjusted analyses left our findings unchanged.

We also previously discussed how the role of *H pylori* in myocardial infarction may have been overestimated owing to other aspects of the study design. For example, some residual confounding is suggested by the reduction in the odds ratio for myocardial infarction from 2.28 ($\chi^2_1=79$) to 1.87 ($\chi^2_1=35$) after adjustments for smoking and some imperfect indicators of socioeconomic status. The fact that such crude adjustment reduced the odds ratio so substantially means that exact adjustment for all confounders would have produced an even greater reduction, but such considerations are difficult to quantify. Hence, we concluded that unless epidemiological studies of *H pylori* subtypes give much more extreme relative risks, randomised trials of anti-infective interventions may be needed to help determine causality. Even if a causal link does exist, however, any effect might not be rapidly and fully reversible (as with other risk factor interventions), so trials of interventions against infection might need to randomise large numbers of participants and to observe them for several years to assess reliably any effects on coronary heart disease.

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1 Whincup P, Danesh J, Walker M, Lennon L, Thomson A, Appleby P, et al. Prospective study of potentially virulent strains of Helicobacter pylori and coronary heart disease. *Circulation* (in press).
2 Danesh J, Peto R. Risk factors for coronary heart disease and infection with Helicobacter pylori: meta-analysis of 18 studies. *BMJ* 1998;316:1130-2.
3 Parish S, Collins R, Peto R, Youngman L, Barton J, Jayne K, et al. Cigarette smoking tar yields and non-fatal myocardial infarction: 14 000 cases and 32 000 controls in the United Kingdom. *BMJ* 1995;311:471-7.

Treatment of schizophrenia

Value of diagnosis of schizophrenia remains in dispute

EDITOR—Proponents of clinical effectiveness in mental health argue that the problems of psychiatry will be solved by more focused research and a better flow of information from academics to clinicians. A good example of the limitations of this approach is McGrath and Emmerson's review article on the treatment of schizophrenia.[1] The authors fail to note that the diagnosis of schizophrenia remains in dispute. The concept has little explanatory power and is scientifically suspect.[2] Inclusion in the *Cochrane Library* does not make it any less controversial.

We are not convinced by their calls for prompt diagnosis. In recent years the word schizophrenia has increasingly taken on negative connotations in the public imagination. Telling a young person that he or she has schizophrenia can have devastating results. In our clinical work with patients we manage perfectly well without using the diagnosis at all. McGrath and Emmerson's review is devoted almost entirely to drug treatments, with only a small section on psychosocial interventions. Users complain that drugs are often all they receive when they are in crisis and in need of human interaction and practical support. Perhaps the affiliations noted as the authors' competing interests—substantial links with the pharmaceutical industry—go some way towards explaining their narrow vision.

We believe that the clinical effectiveness paradigm in mental health will do more harm than good if it is not balanced by a discourse on what we would call ethical issues. These issues are an examination of the values underlying diagnoses and treatments; a questioning of priorities in our work with people in crisis; an examination of the burden we often inflict with our diagnoses and treatments; and a genuine attempt to listen to what service users are telling us about the nature of care.

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1 McGrath J, Emmerson WB. Treatment of schizophrenia. *BMJ* 1999;319:1045-8. (16 October.)
2 Boyle M. *Schizophrenia: a scientific delusion.* London: Routledge, 1993.

What in fact is schizophrenia?

EDITOR—Without doubt the question "What is schizophrenia?" is a fascinating one. In their review article McGrath and Emmerson are bold enough to give an answer to the question,[1] but in my view it misses the mark by a mile. They attempt to list various neuropsychiatric symptoms and use broad terms (for example, "a group of illnesses"). They hope that one day the "cause of schizophrenia" will be made clear by the advances expected in the neurosciences. "Watch this space," they cry. But this will not do.

At the Bradford Home Treatment Service I have been involved in the care of people with acute and severe mental health problems, including those who would ordinarily be considered to have schizophrenia. The service has been able to work with, and help, these people without using the notion of schizophrenia at all. This has been done by taking a step back from the concept of a syndrome called schizophrenia and instead focusing on what the client is actually feeling, thinking, and experiencing.

Instead of trying to discover if a person has auditory hallucinations in an attempt to support a possible diagnosis of schizophrenia, we would want to learn of the person's voice hearing experience (asking such questions as "Can he or she describe the identity of the voices?", "What do the voices say?", "How does he or she cope with them?", "What is helpful and unhelpful in coping with them?"). Attempts are made to see a person's symptoms in the context of his or her life, not as evidence of some underlying neurochemical abnormality. This approach is certainly valued by our clients, and they report that it is more helpful and less abusive and stigmatising than traditional medical approaches.

I would recommend that the authors read Boyle's critique of the notion of schizophrenia.[2] In it she shows that the notion of schizophrenia is unsupported by scientific evidence and is unsustainable. Maintaining that schizophrenia exists is dishonest. It would be of more help to those in distress, and move forward the research effort to understand madness, if we stopped trying to fit their symptoms into a bogus diagnostic category.

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1 McGrath J, Emmerson WB. Treatment of schizophrenia. *BMJ* 1999;319:1045-8. (16 October.)
2 Boyle M. *Schizophrenia: a scientific delusion?* New York: Routledge, 1990.

Review should have paid more attention to psychosocial interventions

EDITOR—I was concerned that McGrath and Emmerson's review on the treatment of schizophrenia focused mainly on the pharmacological management of schizophrenia, and in particular on the atypical antipsychotic drugs.[1] This presumably largely reflects the research and other stated interests of the authors in these more recently introduced agents.

I am certainly a proponent of many of these drugs, particularly because their greater tolerability compared with the tolerability of standard drugs should produce enhanced compliance. But other treatment modalities seem to have been given short shrift. In this enlightened age psychosocial interventions should surely not be "outside the scope" of any review on the treatment of schizophrenia. The authors could have usefully outlined some of the techniques used in cognitive behavioural interventions and considered the role of family interventions, although a recent Cochrane review casts doubt on the efficacy of family interventions.[2] As the article focused on the atypical antipsychotics, it was surprising that only some of the agents currently available were considered, notable exceptions being amisulpride and zotepine. Because of the authors' stated links with companies producing many of these products, these omissions might lay them open to criticism. In addition, while commenting on the advantages of the atypical antipsychotics, the authors neglect to mention the cost implications of their widespread use.

Certainly in the United Kingdom, as long as rationing exists, there will be pressure on doctors to think twice before prescribing new treatments that are considerably more expensive than standard treatments.

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2 Pharoah FM, Mari JJ, Streiner D. Family intervention for schizophrenia (Cochrane review). In: Cochrane Collaboration. *Cochrane library*. Issue 3. Oxford: Update Software, 1999.

Review was based on fashion, not evidence

EDITOR—In reviewing the treatment of schizophrenia McGrath and Emmerson make several statements that are difficult to justify.[1] At best their review is based on a selective reading of the literature; at worst it reflects how fashion can have a greater impact on prescribing than evidence from randomised controlled trials, systematic reviews, and meta-analyses (grade I evidence). This probably relates to their use of a series of clinical practice guidelines that consist of non-systematically gathered evidence and the opinions of distinguished experts (grade IIIb evidence).

The authors state that new antipsychotic drugs should be prescribed as early in the course of schizophrenia as possible, are the treatment of first choice for schizophrenia of recent onset, "obviously" improve quality of life, may reduce the rate of relapse, and should be continued for at least five years in those with two or more episodes. There is no good evidence for any of these statements.

None of the Cochrane reviews they cite addresses any of these issues. The new antipsychotics may reduce side effects, but most, or even all, of this apparent benefit may be attributable to comparisons of relatively low doses of new and high doses of old antipsychotics in the randomised controlled trials. A sensitivity analysis in one of the meta-analyses they cite makes this clear.[2] There is certainly no evidence that the new drugs improve quality of life or reduce relapse rates. Maintenance treatment can only be justified for 9-12 months on evidence from systematic reviews of randomised controlled trials (of old antipsychotics).[3] There are no published randomised controlled trials that directly address the value of early treatment in schizophrenia or its prodrome.

In short, McGrath and Emmerson recommend dramatically extending the indications for antipsychotic treatment to before schizophrenia develops and for five years after a second episode. They advocate new and relatively unproved treatments, rather than the cheaper and more thoroughly assessed older drugs, on the basis of dubious claims of fewer side effects.

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1 McGrath J, Emmerson WB. Treatment of schizophrenia. *BMJ* 1999;319:1045-8. (16 October.)
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Authors' reply

EDITOR—We are pleased that several correspondents agree with our recommendations about the importance of psychosocial treatments for schizophrenia. As clinicians delivering and evaluating psychosocial treatments we share the correspondents' advocacy for these interventions on the basis of the robust evidence supporting their use. But to suggest that the relative amount of text we devoted to the new antipsychotic drugs versus psychosocial treatments is proportional to the quality of the evidence base for each, or even the strength of our recommendations of each, is inaccurate. Our article is described in the opening sentence as a selective review, with the scope of the article clearly outlined in the first paragraph.

Bracken and Thomas and King question the validity of schizophrenia as a diagnosis. As in many areas of medicine with an imperfect knowledge base, we have to use interim diagnostic labels based on best available evidence. We are not sure why they link the imprecision of current diagnostic practice with "ethical issues" and with the importance of understanding the impact of psychotic illness on the individual. We agree that good clinical practice should be able to balance uncertainty of diagnosis and the broad range of "meta-issues" related to designing optimal mental health services, and that the input of consumers is essential to this process.

Other topics related to care in schizophrenia are equally deserving of detailed review. In particular, both psychosocial interventions and drugs need to be embedded in a balanced and integrated programme of mental health services, accommodation services, vocational rehabilitation, and disability support. Many patients remain symptomatic and very disabled despite optimal treatment, and reducing their suffering requires a whole community approach.[1] This report also draws attention to the unmet needs of those with psychosis and substance abuse or dependence. This topic alone warrants a separate, detailed review.

We regret that we could not cover all the recently introduced antipsychotic drugs available worldwide in the space available. Readers who have access to drugs not included in our review are urged to consult the *Cochrane Library* for updates on these products. Cochrane reviews will soon be available on sertindole, ziprasidone, molindole, loxapine, and amisulpride. There will also be new reviews of psychosocial and service related interventions in future editions, including one on the efficacy of cognitive rehabilitation in schizophrenia cowritten by one of us (JM).

McIntosh et al correctly show that many of the areas of treatment of schizophrenia lack evidence derived from randomised controlled trials. We agree that such data are needed.[2] Meanwhile, wide confidence intervals should be placed on much of what we do in caring for people with schizophrenia.

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1 Jablensky A, McGrath J, Herrman H, Castle D, Gureje O, Morgan V, et al. People living with psychotic illness: an Australian study 1997-98. Commonwealth of Australia, 1999. (This report can be downloaded from www.health.gov.au/hsdd/mentalhe/pubs/psych.htm.)
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Preventing pressure sores

Good nursing care should prevent pressure sores

EDITOR—Bliss and Simini state that "postoperative epidural analgesia … has been associated with the development of severe sacral sores in elderly patients."[1]

Pressure sores may develop as a result of decreased sensation and mobility, but

awareness of this possibility and good nursing care should prevent this complication.

Conversely, because epidurals can provide excellent analgesia, patients will require no systemic sedative analgesia, which is also identified in the editorial as a cause of immobility. Patients who have good analgesia are easier to nurse and faster to mobilise, especially in the high risk group.[2]

In this present climate of evidence based medicine we are surprised that an editorial in the *BMJ* is making such a sweeping statement based only on a personal communication.

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1 Bliss M, Simini B. When are the seeds of postoperative pressure sores sown? *BMJ* 1999;319:863-4. (2 October.)
2 Buggy DJ, Smith G. Epidural anaesthesia and analgesia: better outcome after major surgery? *BMJ* 1999;319:530-1.

More research is needed into the origins of pressure sores

EDITOR—The origin of postoperative pressure ulcers troubles many clinicians.[1] It is traditional to quote Versluysen to support the assertion that pressure sores develop during surgery.[2] She showed that 66 of 100 elderly patients with hip fractures developed some stage of pressure sore during their stay in hospital. This high incidence has long been used to implicate conditions during surgery as the prime cause of such wounds. However, 18/66 (27%) of the subjects developed their sores before surgery, with a further 20% (16/66 patients) exhibiting sores on the day of surgery. Furthermore, seven subjects developed sores but did not undergo surgical fracture repair. Neither was any relation between duration of surgery and sores proved. It is probable therefore that their pressure ulcers may have originated from lying on the floor for prolonged periods. Eight of the 66 subjects (12%) spent more than 20 hours (up to three days) undiscovered.

The prevention of pressure ulcers needs to be considered from the moment the ambulance journey starts. A randomised controlled trial of the prevention of postoperative pressure sores, comparing viscoelastic polymer and standard operating table surfaces in 416 patients, showed that the viscoelastic pad significantly reduced sores (odds ratio 0.45; 95% confidence interval 0.26 to 0.82, $P = 0.01$).[3] Associations between the length of preoperative stay, the length of operation, the duration of operative hypotension, and the development of sores were also shown. After adjustment for these variables, the odds ratio for pressure sore development on the viscoelastic pad was 0.5 (0.27 to 0.89, $P < 0.02$), which was still significant. The sacral interface pressure of four different theatre mattresses of different densities (33-56 kg/m^3) evaluated using a Force Sensing Array pressure monitor (Vista Medical, the Netherlands) showed that changes in foam type resulted in statistically significant differences in interface pressures.[4] Furthermore, the pressures were influenced by the patients' body mass index, so weight specific pressure relief policies may be indicated.

Finally, pressure ulcers are individually "auto-regulated" and the duration and intensity of pressure that are acceptable vary from patient to patient.[5] Much of the information about mattress choice is derived from small case studies and interface pressures. Although data are slowly accumulating, more research into when, why, and how pressure ulcers develop is essential.

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Authors' reply

EDITOR—Hahn and Hall's reproach that our statement about the danger of pressure injuries resulting from epidural analgesia is based on a personal communication is undermined by their comment that "awareness of this possibility and good nursing care should prevent this complication." The link between epidural analgesia and postoperative pressure injuries has not been systematically addressed, possibly because anaesthetists and surgeons are no keener to draw attention to their failures than anyone else. We are grateful for the reminder of the need for meticulous postoperative nursing (including pressure relief) in all patients.

We fully agree with Russell et al that pressure injuries may occur before surgery, and, indeed, we discuss possible precipitating factors. Pressure injuries may also occur before admission, but in that case a careful initial examination of the pressure areas is likely to show tissue injury. The observation of Russell et al that the "duration and intensity of pressure" which can cause harm "vary from patient to patient" is another way of saying that pressure by itself cannot account for the whole pathogenesis of bedsores.

Methods of pressure relief in the prevention of intraoperative sores need to be effective if they are to be relied on to prevent tissue death (it is difficult to reposition patients in theatre). Anything less efficient is likely to be useful only as a defence against litigation, which is not the same as help to the patient. Measurements of interface pressure on different density foam mattresses are informative, but they do not show whether the supports will prevent tissue death in sick or hypotensive patients in theatre. Monitoring transcutaneous oxygen or carbon dioxide concentrations in the pressurised areas in healthy and compromised patients during operations could provide invaluable pointers to the effectiveness of different supports before randomised controlled trials are performed.

We endorse Russell et al's conclusion that "research into when, why, and how pressure ulcers develop is essential." Our editorial's title is a question, one of the objectives of which is to increase awareness of the problem of perioperative pressure sores; such awareness is the first step before any research.

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World Trade Organisation agreements should be subject to health impact assessment

EDITOR—Drager's editorial reminds us that the international agreements negotiated in the World Trade Organisation have wide ranging implications for public health.[1] The agreements are enforceable, unlike other international agreements on the environment, human rights, and social welfare. Governments can be challenged for implementing laws intended to protect public health if they restrict free trade. In settlements of World Trade Organisation disputes so far, trade issues have been placed above public health.[2]

The editorial made only passing reference to the likelihood of a new agreement on investment. The European Union is pressing for the World Trade Organisation to start negotiating an investment agreement.[3] This would prevent countries placing any restrictions on foreign investment or implementing any regulations that might disadvantage a foreign investor. Last year negotiations on a similar investment agreement fell through after a campaign by a wide range of groups worried about the implications for the environment, health, and human rights.[3]

An investment agreement could be damaging to public health. Regulations to protect public health and the environment could be challenged if they disadvantaged a foreign company. For example, tobacco control is one measure that could be threatened.[2] Tobacco firms could claim compensation for expropriation of trademark rights if advertising or sponsorship was restricted, or expropriation by taxation for losses from raised taxes. In 1997 the Canadian Public Health Association passed a resolution opposing the proposals for an agreement on investment, stating that it would "constrain governments' ability to regulate investment to achieve and protect citizens' social, economic, environmental, health and other national interests."[4] The latest proposals do not address these concerns.[3]

Saving Lives: Our Healthier Nation states that the government will undertake health impact assessment of major new government policies.[5] The latest round of World Trade Organisation negotiations could produce agreements with major direct and indirect health impacts. The government should honour the commitment made in the white paper and undertake health impact assessment of all the organisation's proposals before signing any new agreements.

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1 Drager N. Making trade work for public health. *BMJ* 1999;319:1214. (6 November.)
2 Koivusalo M. *World Trade Organisation and trade–creep in health and social policies.* Helsinki: STAKES, 1999. (Occasional paper No 4.) (www.stakes.fi/gaspp/gaspp.htm#NETWORKING; accessed 3 February.)
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Britain is ahead of US in dealing with misconduct

Editor—Christie reports on a consensus statement on research misconduct drawn up at a consensus conference last year. In his report he writes that "The two day conference, held at the Royal College of Physicians of Edinburgh, heard that Britain is 20 years behind countries such as the United States."[1]

I beg to differ. Decades of American debates about misconduct have not produced anything like a consensus of professional, academic, and governmental representatives, parties who normally have been at odds with each other in that country. Furthermore, the British statement's definition of research misconduct—"behaviour by a researcher, intentional or not, that falls short of good ethical and scientific standards"—and its insistence that even this should not be read narrowly, contrasts with the "professional" lobbying in the United States for a purportedly narrower definition. I am sure that many observers, both inside and outside science, would judge the British actions as more progressive and more professional, expressing a broader and deeper social responsibility.

Where the British statement seems to flag is in its vague prescriptions for action when misconduct is alleged or confirmed, although even this shortcoming should be put into perspective. The more elaborate procedures in the United States directly involve only government offices and institutions that receive public funding. Professional associations are accorded no definite, active, and positive role in this scheme; nor have they asked for one. The British colleges' offer to help in investigations into misconduct and their promise to publish information about verified incidents certainly go beyond the efforts of their American counterparts.

A key question remaining for the colleges (and their counterparts elsewhere) is whether, in the event of confirmed misconduct by one of their members, they would levy sanctions, graduated according to the severity of the act, and within the colleges' legitimate authority. In particular, would they be prepared to "excommunicate" one of their members, given confirmation of sufficiently serious misconduct on his or her part? Explicitly promising to do so would at least put them on a par with schoolteachers, lawyers, and other professionals.

To find a country that is behind the times in misconduct policy or regulation, one need look no further than one's own. It will only take another international scandal of the order of those of the early 1990s to show the primitive state of Canadian policies.

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1 Christie B. Panel needed to combat research fraud. *BMJ* 1999;319:1222. (6 November.)

Chinese hypnosis can cause qigong induced mental disorders

Editor—Qigong ("exercise of vital energy") is a Chinese healing system based on trance. It consists of meditational or movement exercise, or both, induced by use of a highly culture syntonic set of suggestions based on the concept of qi (vital energy). It has been estimated that about 5% of China's 1.3 billion people practise qigong, so this may be the most common form of "hypnosis" practised globally. Vickers and Zollman have rightly pointed out that qigong is similar to hypnosis, but it may be premature to conclude that adverse events associated with this form of Chinese hypnosis are extremely uncommon.[1]

In the past two decades many reports of mental disorders induced by qigong have been published in the Chinese psychiatric literature. In the *Chinese Classification of Mental Disorders*, second revised edition (CCMD-2-R), qigong induced mental disorder is found as a culture related mental disorder. In psychologically vulnerable individuals, qigong induced health disturbances or pian cha are believed to arise from the inappropriate application of qigong or the inability to "terminate the qigong" (shougong), or both. When severe they are known as zou ("run") huo ("fire") ru ("enter") mo ("devil"); this means that the flow of qi deviates from the jing luo conduits and becomes fire, as a result of which a devil enters the person (metaphorically, referring to the emergence of psychotic symptoms).

The condition violates the paradigms of quietness, relaxation, and internal harmony that are followed in qigong practice and has many symptoms, from more minor ones to bizarre and violent behaviour. As qigong induced mental disorders usually do not last long, some cases probably never come to medical attention. The exact proportion of people engaging in qigong and developing psychiatric complications remains unknown, but similar problems are much less frequently described in the meditational practices of other cultures.

Although qigong induced mental disorders have recently become a political issue in China, empirical studies of the adverse effects of this form of Chinese trance practice are worthwhile. For further information interested readers are referred to an article that I wrote.[2]

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1 Vickers A, Zollman C. ABC of complementary medicine: Hypnosis and relaxation therapies. *BMJ* 1999;319:1346-9. (20 November.)
2 Lee S. Cultures in psychiatric nosology: the CCMD-2-R and international classification of mental disorders. *Culture, Med, Psychiatry* 1996;20:421-72.

Dental recalls are useful for detecting oral cancer

Editor—Two letters have been prompted by Kay's editorial questioning the validity of the six monthly dental recall.[1] I agree with Kay: there should be evidence supporting recall intervals. I am not convinced, though, that the argument should be focused on disease progression in either the dentition or the periodontium.

The role of the dental practitioner is not that of tooth technician but that of oral physician. Over 1900 new cases of oral cancer occur per year.[2] Oral cancers carry a poor prognosis (death:registration ratio = 0.46 for England and Wales, 1991-5, compared with 0.41 for breast cancer and 0.37 for cervical cancer). Evidence suggests that, despite attempts to make the public aware of the risk factors, oral cancer is increasing.[3] Early identification of carcinomas and early intervention are generally thought to decrease morbidity and mortality. This is the rationale for screening protocols for breast and cervical carcinomas.

A dental practitioner can examine the oral and perioral structures at each recall. One study showed that a soft tissue examination could be carried out as part of a five minute dental inspection.[4] Ironically, those benefiting most from such screening—elderly people—have fewer teeth and seek less dental care.

In 1998 the Scientific Committee on Tobacco and Health recommended that dentists attend training courses and updating courses for oral cancer.[5] It also advised that the National Screening Committee should consider screening programmes for early detection of oral cancers. The yield of malignant or premalignant lesions would be low because of the large numbers of patients screened. The perceived cost to benefit ratio

to the public would be regarded as exorbitantly high. Currently there is no increased financial burden for this service, which is incorporated into the dental examination; this cost effective service is already being provided to those routinely attending dental recalls.

It is good to see further research on the optimum frequency for bitewing examinations but irresponsible to rely solely on the progress of caries and periodontal disease to determine the length of recall intervals because of lack of evidence on the progress of oral cancers. Until such data exist it would be prudent to follow the advice of the American Cancer Society, which recommends annual screening for oral cancer in those aged over 40.

However great the demands are for evidence based clinical practice, we must not succumb to making decisions based on inappropriate evidence.

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1 How often should we go to the dentist? [Letters.] *BMJ* 1999;319:1269-70. (6 November.)
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Carbon monoxide poisoning

Carboxyhaemoglobin can be measured with standard blood tests

Editor—In their editorial about carbon monoxide poisoning, Walker and Hay note that it is tissue poisoning rather than merely the effects of carboxyhaemoglobin that contributes to its toxicity.[1] We recently reported that metabolic acidosis was a better indicator of the severity of poisoning than carboxyhaemoglobin,[2] as the acidosis reflects tissue poisoning.

Walker and Hay concentrate on cerebral toxicity. The heart, however, as the next most vulnerable organ may help to give a clue to the diagnosis. We reviewed 139 electrocardiograms from patients with acute severe carbon monoxide poisoning who had been referred for treatment with hyperbaric oxygen, and we found that 41% were abnormal (unpublished data). Previously, 3% of patients presenting with unstable angina were found to have significant carbon monoxide intoxication.[3] Thus the possibility of carbon monoxide poisoning should be considered when patients present with non-specific symptoms and have abnormalities on their electrocardiograms. Patients with known coronary disease who present with unstable angina and carbon monoxide intoxication should be given high flow oxygen via a tight fitting mask and reservoir bag (aiming to give 100%) in addition to standard treatment.

Arterial blood gases need not be used to measure carboxyhaemoglobin, as venous and arterial concentrations are not significantly different.[4] Thus carboxyhaemoglobin can be measured simultaneously with other standard blood tests, without the need for an additional arterial puncture.

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1 Walker E, Hay A. Carbon monoxide poisoning. Is still an unrecognised problem. *BMJ* 1999;319:1082-3. (23 October.)
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Doctors should inform Employment Medical Advisory Service

Editor—Walker and Hay's editorial was timely but did not put the issue into a proper public health context.[1] A review by the Institute for Environment and Health on the health effects of carbon monoxide in the home is a useful starting point.[2]

About 50 people die from carbon monoxide poisoning in the home every year; about 30 of those deaths are associated with natural gas or liquefied petroleum gas. These figures remain similar year after year, but in 1998-9 the provisional data show a small increase. The risk of death from carbon monoxide poisoning associated with gas at 0.4 per million is, however, much lower than that from falls in the home (22.8 per million), poisoning (13.2 per million), and fire (7.5 per million). Nevertheless, carbon monoxide poisoning tends to affect old and disadvantaged people and is preventable.

The Health and Safety Executive is currently undertaking a fundamental review of gas safety and, in its discussion document, invites comments on a range of gas related issues such as carbon monoxide poisoning, monitors, and others.[3]

Doctors, especially in accident and emergency departments, need to be more aware of the possibility of carbon monoxide poisoning. The general public is already showing an increased awareness. Doctors can also help in another way. When a faulty appliance is reported and subsequently investigated, and especially when people in the household concerned show symptoms, the appliance is usually disabled and a notification of suspected carbon monoxide poisoning made to the local office of the Health and Safety Executive. Such a report puts in train a detailed investigation which may be unnecessary if, as is frequently the case, the incident is not one of carbon monoxide poisoning. It would be extremely helpful if details of patients' investigations (including carboxyhaemoglobin concentrations) could be released on request to a doctor in the Employment Medical Advisory Service so that the investigation by the Health and Safety Executive can be quickly concluded if no poisoning has occurred or, if it has, completed with full knowledge of the extent of the poisoning.

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1 Walker E, Hay A. Carbon monoxide poisoning. Is still an unrecognised problem. *BMJ* 1999;319:1082-3. (23 October.)
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More reluctance in accepting evidence on smoking and cancer

Editor—It is sad but true, as Cowen points out,[1] that many doctors were long unconvinced by the clear evidence about smoking and lung cancer produced by Doll and Hill.

An exception was Dr Horace Joules, who urged the health ministry's Standing Advisory Committee on Cancer and Radiotherapy of the need to warn the public immediately their 1950 paper came out.[2] But the committee's distinguished chairman, Sir Ernest Rock Carling, a lifelong heavy smoker, rejected the idea and continued to do so until he was at last outvoted, the sole dissenting voice. A minute on the Ministry of Health file in November 1953 records: Sir Ernest "feels that the evidence is insufficiently conclusive" (file MH 55/1011, Public Records Office).

Moreover, Dr Joules had made himself a nuisance and so lost his place on the committee. This was perhaps unsurprising, since the ministry's medical officers—notably, Dr Neville Goodman and the chief medical officer Sir John Charles—were remarkably concerned to water down the committee's draft advice.

At the Medical Research Council this sceptical attitude at the ministry had already been noticed. Dr Goodman minuted a private meeting with Dr Ernst Wynder, who with Evarts Graham had published a similar study just before Doll and Hill's[3]: "He is a young man 'far gone in enthusiasm' for the causal relationship between tobacco smoking and lung cancer. (I had been told when I was in New York this spring that he was the son of a revivalist preacher and had inherited his father's antipathy to tobacco and alcohol.)"

Dr F H K Green at the Medical Research Council recorded the comment: "Dr Goodman's slightly 'sour' minute … seems to me symptomatic of the great reluctance of the Ministry's MOs [medical officers] to accept what we regretfully believe to be the 'facts of life (and death)' on smoking and lung cancer (file1.2009, Public Records Office).

That reluctance persisted until Sir George Godber took over as chief medical officer in 1960, when, with Enoch Powell as minister of health and Lord Hailsham responsible for the Medical Research Council, at last a genuine attempt was made to

reduce smoking. The whole story is told in my recent book *Denial and Delay*.[4]

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1 Cowen P. The price of coffins: specious arguments by eminent doctors against the dangers of tobacco. *BMJ* 1999;319:1621-3. (18-25 December.)
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Student publications

Students in Birmingham have published projects for more than 10 years

Editor—Hanratty and Lawlor's enthusiasm for students publishing their work evokes a sensation of déjà vu in this department.[1] For over 10 years our students have undertaken project work in their third year, and we currently have a list of nearly 40 peer reviewed publications that have resulted. Most of these are papers rather than letters, but there are also oral and poster presentations at conferences, and in one recent case the content of a question in the House of Lords.

Publications by students include findings in relation to the following: ethnicity, cot deaths and sleeping position; antioxidants and red wine; communication between deaf patients and their general practitioners; dietary fat purchase in different ethnic groups; views on donated ovarian tissue; long term trends in risk of death or injury in railway accidents; and so on.

We think that it is good for students to publish their work, and the project work they do with this department is structured to facilitate this. We are happy to provide full details of the above projects, or the complete bibliography of publications our students have achieved, on request. We are pleased that other medical schools have discovered the benefits of what our students have been doing since at least 1989.

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1 Hanratty B, Lawlor D. Getting letters published in journals is good aim for medical students. *BMJ* 1999;319:1198. (30 October.)

Students' letters in all journals need to be included

Editor—Hanratty and Lawlor make a good point encouraging medical students to submit letters for publication.[1] Their research is, however, inadequate evidence to support their conclusion. They limit their search to the letters pages of the *BMJ*, and bias results towards letters submitted by students working in departments of public health medicine and epidemiology. This is illustrated by checking the origins of letters published in Volume 319 (July to December 1999).

Medical students at Newcastle scored well, as all publications came from the above departments. A more appropriate way of assessing teaching quality in university would be to include letters and articles in all journals of individual specialties.

Assessing 3842 letters and articles from a whole month of all journals would be more representative of the contribution made by medical students to publications. This would limit the bias of the above study and may show different strengths at different universities.

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1 Hanratty B, Lawlor D. Getting letters published in journals is good aim for medical students. *BMJ* 1999;319:1198.(30 October).

Students should seek to publish not just in medical journals

Editor—With reference to the letter by Hanratty and Lawlor on student publications,[1] since 1992 I have taught a course in public health media advocacy in the master of public health at the University of Sydney. Each year, students are required to write a letter to the editor of a big newspaper on any public health matter that is newsworthy. If a letter is published the writer gains 10 bonus marks in his or her assessment. Most elect to write to the *Sydney Morning Herald*, which daily receives an average 200 letters and publishes about 30. Our class size is around 25, and our class publication record is 14 published letters over the six week course. The exercise is very popular with most students, and some—once infected with the publication bug—metamorphose into helpless, chronic letter writers.

The circulation rates and readerships of newspapers greatly exceed those of medical journals. The letters page is one of the most avidly read sections of newspapers, and competition to get your letter selected is far greater than for most medical journals. Politicians and other decision makers in the health system read newspapers too—probably far more than they read medical journals. Newspapers are often disdained as fish and chip wrappers, but they can be highly influential in steering the public health agenda.

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1 Hanratty B, Lawlor D. Getting letters published in journals is good aim for medical students. *BMJ* 1999;319:1198. (30 October.)

Meningitis C immunisation is low among young people who are not in education

Editor—The Department of Health aimed for vaccination of young people aged 15-17 against meningitis C to be completed by December 1999. We report the failure of low cost publicity to achieve satisfactory uptake among those who were not attending school or college.

The Loddon community trust serves a population of 225 000. Eight clinics in three different locations were held during the late afternoons or on Saturdays from 16 November for young people aged 15 to 17 who were not in education. Posters giving the clinic details were sent to general practices, community pharmacists, NHS Direct, public libraries, post offices, and over 50 places frequented by young people—for example, cinemas. Electronic messages were sent to the local acute and community trusts, health authority, and four local authorities.

Posters were displayed in public areas in trust premises, which included services for young people dealing with sexual health and drug misuse. Thirty major employers were sent posters and advised that information was available on the health authority's website. The youth service and careers service agency for young people were sent posters and fliers for people to take away. Reminder letters and fliers to general practices requested primary care staff to promote the clinics. Health staff were encouraged to display posters on their local community notice board. Articles advertising the clinics appeared in four local newspapers and a health promotion newsletter. Headline coverage in the free local newspaper reached 52 000 homes. Local radio publicised each clinic.

The catchment population of young people aged 15 to 17 who were not in education was estimated to be 1500 (25% of school years 12 and 13). The number vaccinated was 264 (about 18%) compared with 3619 (85%) of 4279 year 12 and 13 children attending school or college.

The clinics were held early in the government's campaign when public awareness was low. Nationally produced posters and advertising carried the message "wait to be called," which may have caused confusion.

The risk of meningococcal infection is widely thought to be lower among young people who are not in education than among those who are. Without confirmatory data for this age group, immunisation coverage needs to be optimised among those who may already be disadvantaged in health terms through smoking and poverty. This may, however, require new ideas and increased resources.

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