Letters

Cyclists should wear helmets

Increasing the number of cyclists is more important

Editor—Ronald M Davis and Barry Pless's letter about the value of cycle helmets once again illustrates the worrying reductionist tendency in research into health promotion and illness prevention, which often results in sight being lost of the ultimate goal of promoting health.[1] Narrow sectoral approaches—in this case the view that wearing a helmet reduces head injuries among cyclists—address only part of the issue. Purely medically based prescriptions for change fail to consider behavioural responses to environmental changes. The case for all cyclists to wear helmets, as argued, fails to acknowledge the disbenefits that have resulted when such strategies have been enforced through legislation. Evidence from Australian states where laws have been enacted to require the use of helmets suggests that "the greatest effect of the helmet law was not to encourage cyclists to wear helmets, but to discourage cycling."[2]

We are familiar with the evidence of the benefits of regular physical activity and of the levels of inactivity in the population.[3] Cycling has been viewed as an ideal form of aerobic physical activity: it is available to a large section of the population and can be incorporated into daily life without requiring additional time. In the past year or so several high level statements have been made about the value of moderate physical activity as part of the routines of daily living. These have come from the surgeon general and the National Institute for Health in the United States, the World Health Organisation and the International Federation of Sports Medicine, and, not least, the Department of Health in Britain.[4]

The evidence is that the health benefits of cycling outweigh the dangers posed to cyclists.[5] There is now a government led national cycling strategy, with a target to quadruple levels of cycling to a modest 8% of all trips by 2012 (from a 1996 baseline). This requires intersectoral action in the spirit of the Health for All strategy. Its achievement would promote health, including reductions in heart disease and strokes, and would encourage children (the potential habitual car drivers and coronary care unit patients of the future) to adopt physically active lifestyles.

Health promotion requires researchers to break free from narrow sectoral paradigms and to collaborate if "benefits" gained through some advances are not to be outweighed by knock on effects in efforts to promote health and wellbeing.

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1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)
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Motorists are the cause of the problem

Editor—It is strange that two medical editors of health promotion journals should write about legislation in favour of wearing cycle helmets without mentioning measures directed at cars, the cause of the problem.[1] This smacks of victim blaming and is akin to insisting that smokers use a filter tip rather than banning tobacco advertising. I agree that helmets are protective, and I always wear one. But no one in the Netherlands does, and the Dutch are famed for their high cycling rates.

Many cyclists object to the continual advocacy for helmets without equal (preferably greater) emphasis on restrictions aimed at motorists—separating them from cyclists, reducing their speed, and stopping them from entering city centres. Has the United States Centers for Disease Control and Prevention, cited by Ronald M Davis and Barry Pless, issued an attack on the low prices of petrol in the United States, which feed the car addiction of Americans? I am less prepared to listen to its views on cycle helmets until it does.

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1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)

Australian laws making helmets compulsory deterred people from cycling

Editor—Ronald M Davis and Barry Pless report that, after the implementation of a law requiring all cyclists to wear helmets, the number of cyclists admitted to hospitals in Victoria, Australia, was 40% below that expected.[1] Figure 1 shows this effect, for subjects with and without head injury.[2] Both head and non-head injuries showed a considerable decrease when the legislation was enacted, with non-head injuries outnumbering head injuries both before and afterwards by roughly 2:1. Changes in the relative proportions seem to have been a second order effect, hardly noticeable compared with the overall reduction, which was probably due to a reduction in cycling and safer roads.

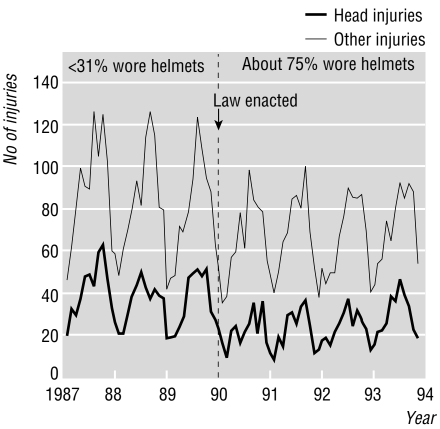

Fig 1 Number of cyclists admitted with head and other injuries to hospitals in Victoria before and after implementation of law making wearing of cycle helmets compulsory[2]

The deterrent effect on cycling was substantial. In New South Wales identical surveys of children were carried out before and after the law was implemented. When possible the same observers were used, with the same sites, observation periods, and time of year; the weather was excellent for both surveys.[3] Reductions in cycling in rural areas (35%) and Sydney (37%) were almost identical, with similar reductions at road intersections (32%), recreational areas (29%), and school gates (45%).[3] Overall, the increase in the number of cyclists wearing helmets was only half the reduction in cyclists counted. An identical survey carried out a year later under generally sunny conditions found even fewer cyclists.[3] This deterrent effect was confirmed by a survey of 1210 secondary school children two years after the law was enacted. Among those who had not ridden in the past week, having to wear a helmet and not owning a bicycle were the commonest reasons given (both 34%), whereas unsafe roads were cited by 12%.[4]

In Victoria identical surveys found that cycling by children fell by 33% and 36% in the first and second years after the law was enacted.[5] Cycling by adults was not measured, but counts of adult riders (which are highly correlated with use of cycles by adults) were 29% lower after the law was enacted.[5] As in New South Wales, the increase in numbers wearing helmets was less than the overall reduction in numbers counted. A survey of 64 sites a year later[5] showed (after one, atypical, site had been omitted) that counts two years after the law was enacted were 27% below counts before it was enacted, indicating a sustained decrease in cycling similar to that in New South Wales.

Minerva is right: we do not want to deter people from healthy, pollution free transport. The data suggest, however, that the main effect of the helmet laws was precisely that.

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1 Davis RM, Pless, B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)
2 Carr D, Skalova M, Cameron M. *Evaluation of the bicycle helmet wearing law in Victoria during its first four years.* Melbourne: Monash University Accident Research Centre, 1995. (Report No 76.)
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Better to control the demand for fast cars

Editor—I am disappointed to read that "the benefits of wearing helmets are so patently obvious" when in fact the evidence is thoroughly inadequate.[1] Three phenomena combine to mislead Ronald M Davis and Barry Pless.[1]

Firstly, cyclists who voluntarily wear helmets represent a group who take fewer risks than average. This can reasonably account for their lower accident rate in many observational studies. Davis and Pless's letter seems to confuse this phenomenon with the second—risk compensation.

Secondly, cyclists who don helmets, voluntarily or otherwise, may obtain a false sense of security and so take greater risks than they would have done before. This effect, risk compensation, has prevented other forms of "safety" intervention on the roads from having their intended effect.[2]

Thirdly, the authors refer to a study of admissions to hospital in Victoria, Australia, where helmets were made compulsory for cyclists in 1990.[3] The study concluded that the law reduced the number and severity of head injuries to cyclists. Its statistical analysis tried to take into account the considerable reduction in the number of cyclists that followed implementation of the law, the coincidental local efforts against speeding and drink-driving, the changes in hospital funding arrangements, and the large changes in injury rates that form part of the economic cycle.[2] It did not take into account any reluctance among cyclists (after the law was passed) to report honestly on head injuries that were associated with their breaking the law by failing to wear a helmet. It does not mention that the most dramatic fall in claims for head injury after the law was introduced came from pedestrians aged under 12.[4] It also covered too short a period to generate robust predictions over an economic cycle. Neither the Victoria data nor their analysis are sufficient to justify any enthusiasm about helmet wearing, let alone compulsion. Retailers and manufacturers of helmets are the only people likely to benefit from compulsion, which may well discourage many people from obtaining the health benefits of cycling.[5]

If we ever get good evidence that compulsion confers enough health benefits to be worth its expense, inconvenience, and infringement of civil liberties then I would support compulsory wearing of helmets. But we do not have that evidence, and it seems likely that we never will. Cycle helmets, after all, were not designed to give protection against impact with a motor vehicle. Any exhortation or compulsion should be directed to controlling the demand for fast motor vehicles, the main source of hazard on the roads.

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1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)
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Health benefits of cycling greatly outweigh loss of life years from deaths

Editor—Ronald M Davis and Barry Pless criticise Minerva for not wearing a helmet when she is riding a bicycle, pointing out that significant reductions in the number and severity of head injuries among cyclists have been achieved where wearing a helmet has been made mandatory.[1] They also call for evidence to substantiate Minerva's claim that the health benefits of cycling outweigh the risk of serious injury if helmets are not worn.

The great majority of serious head injuries among cyclists result from collision with a motor vehicle.[2] Lowering the risk of collision in the first place is a far more effective way of preventing these injuries than is wearing a helmet. The risk can be lowered by reducing the volume and speed of traffic[3]; encouraging greater awareness among drivers of the vulnerability of cyclists (and pedestrians), and among cyclists of their vulnerability so that they maintain a high level of vigilance; and establishing safe and convenient networks for cyclists. In countries such as Denmark and the Netherlands where this has been done, and where few cyclists wear helmets, the injury rate is about a tenth of that in Britain.[2]

It is important to bear in mind that the specification for helmets is restricted to providing protection for the head in the event of a fall,[2] not after the cyclist has been run into by a car or lorry. While some people would argue "better some protection than no protection at all," the danger stems from cyclists who wear a helmet feeling safer than they would without a helmet and then riding with an exaggerated sense of security. The law should be changed so that manufacturers are obliged to print on helmets the limited protection they afford.

In Australia, where helmet wearing has been made mandatory, the greatest effect has been to discourage cycling. Although the proportion of cyclists treated for head injuries after collision with a motor vehicle has declined there, the reduction has been similar to that among pedestrians, which suggests that the major road safety initiatives on speeding and drink-driving that were introduced at the same time as the helmet law largely account for the reduction.[5]

Finally, the health benefits of regular cycling, in terms of life years gained, have been shown greatly to outweigh the loss of life years from deaths of cyclists—even in Britain's traffic environment, which is hostile to cyclists.[2 4]

Perhaps the reputation of advocates of helmet wearing is more at risk than the heads of cyclists who do not wear helmets.

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1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)
2 Hillman M. *Cycle helmets: the case for and against.* London: Policy Studies Institute, 1993.
3 Plowden S, Hillman M. *Speed control and transport policy.* London: Policy Studies Institute, 1996.
4 BMA. *Cycling: towards health and safety.* Oxford: Oxford University Press, 1992.
5 Robinson DL. Head injuries and bicycle helmet laws. *Accid Anal Prev* 1996;28:463-75.

Do gooders' intolerance is counter productive to their aims

Editor—Ronald M Davis and Barry Pless assert that the only downside to wearing a cycle helmet is the "mussing of Minerva's hair."[1] Judging by their jobs (as editors of *Tobacco Control* and *Injury Prevention*), I assume that they are risk averse and wish to impose their views on those who are not. The message seems to be that when persuasion fails then there must be prohibition. This intolerance of the do gooders is objectionable and, indeed, counter productive to their aims.

The tone of the authors' letter merely encourages me to continue both smoking my pipe and cycling without a helmet, which I do because I enjoy them. I hope that our legislators will continue to value personal freedom more than the mandarins of health promotion and disease prevention seem to do.

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1 Davis RM, Pless B. Evidence shows that cyclists should wear helmets. *BMJ* 1996;313:629. (7 September.)

Looking after elderly sick people is core work in general practice

Editor—A recent news article seems to condone the fact that "general practitioners in several areas of Britain have successfully negotiated extra payments for the medical care of residents in nursing homes that they do not consider part of their core work." As a retired general practitioner and hospital practitioner in geriatric medicine and now (for the past eight years or so) a designated old age pensioner, I write to criticise this apparently growing practice among my former colleagues in general practice.

As you grow older you often need medical attention; this is acknowledged in the NHS by an extra payment to general practitioners. Such attention is best given, in the first place, by the elderly person's own general practice, where he or she has often been registered for many years and has built up a close rapport over this time. Should elderly people end up in a nursing home, for whatever reason, then they deserve the same medical attention (when required) from their general practitioner as they would get if they were in their own home.

To claim that "patients in nursing homes take up a disproportionately large amount of [general practice] time at the expense of the rest of the community," as Dr Sadiq Ali is quoted as saying, is a gross overstatement. Even if it were partly true, how does claiming more money to look after these patients remedy the situation?

My advice to my colleagues who are at present in general practice is this: remember that you will be old one day and that you will not wish to be categorised as someone who is, as this proposed claim implies, an extra burden on your doctor's practice. Bear in mind, too, that quite a number of elderly people already feel that they are a trouble to their families.

Looking after elderly sick people, wherever they happen to live, is both satisfying and an honour. General practitioners should certainly include such care in their core work. Maybe those who consider it to be "non-core" are in the wrong vocation.

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1 Bunce C. GPs negotiate payment for nursing home patients. *BMJ* 1996;313:1163 (9 November.)

Midwives can help increase uptake of antenatal screening for HIV

Editor—In their short report about a descriptive survey of antenatal testing for HIV Sandra E MacDonagh and colleagues raise concerns about the effectiveness of the current programmes operating in London to detect women infected with HIV.[1] They show depressingly poor detection rates in all units, particularly those with selective testing policies and low uptake (<1%) of HIV testing. Even in units with an uptake of over 40% only a fifth of women in whom HIV infection had not been diagnosed previously were detected.

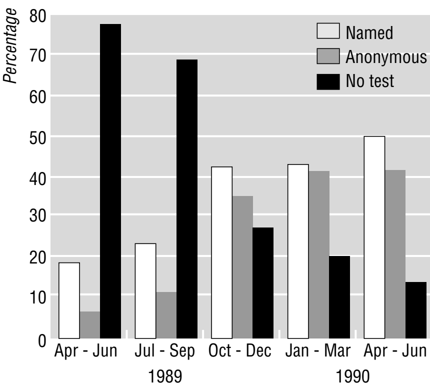

Fig 1 Uptake of HIV testing by women booking in for antenatal care at two hospitals in Manchester by quarter, 1989-90

We have previously published our results of a study in which HIV testing was offered to all women booking in for antenatal care at two hospitals in Manchester with a low seroprevalence.[2] What we did not report then was how the uptake of both named and anonymous testing varied during the period of study. Figure 1 shows these results.

We interpret the dramatic increase in the uptake of named testing from <20% to >45% over 15 months as reflecting the midwives' increasing confidence in their ability to provide information and counselling about the merits of HIV testing. Before the study an experienced HIV counsellor provided 10 hours of training for all midwives who were to receive women booking in for antenatal care, and these midwives remained in post throughout the study, with the counsellor providing continued support. This system allowed the total uptake of both named and anonymous testing to increase to over 85%.

MacDonagh and colleagues report huge variations in uptake among similar populations attending different hospitals. We suggest that these variations reflect differences in the training, experience, opinions, and confidence of the midwives who initially offer testing. We believe that efforts should be directed towards providing training and professional support for midwives involved in this important counselling process. Indeed, subjective reports from the midwives in our study highlight advantages for their personal development and the acquisition of skills beneficial to other aspects of their midwifery practice as a result of their participation in the study.

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1 MacDonagh SE, Masters J, Helps BA, Tookey PA, Ades AE, Gibb DM. Descriptive surveys of antenatal HIV testing in London: policy, uptake and detection. *BMJ* 1996;313:532-3. (31 August.)
2 Miller E, Miller CL, Killick SR, Craske J, Waight PA. Voluntary antenatal HIV testing—results of a pilot study. *Commun Dis Rep CDR Wkly* 1991;1:147-8.

When is referral of Heaf test positive schoolchildren worth while?

More data are needed

Editor—The short report by Helen Booth and colleagues does not answer the question posed—namely, when is referral of Heaf test positive schoolchildren worth while?[1] From the introduction I had expected the report to address the issue of whether children should be referred for further assessment if they are found to be positive on Heaf testing before being given BCG vaccine as part of the routine schools programme. Unfortunately, the data provided are not detailed enough to do this. As the children included in the study ranged in age from 5 to 14 years some of them must have been tested because they were at high risk (the schools programme applies to children in secondary school). One would expect a higher incidence of positive results in children tested because they are at high risk than in those routinely tested.

As the debate centres on children found to have a grade 2 positive result on Heaf testing the following information is needed: the number of children found to have such a result on routine testing; the number of these children who had a positive result of a Mantoux test and the number who had been given BCG vaccine previously; and the number of these children thought to be in need of treatment or follow up. It would also be valuable to know what proportion of these children came from the Indian subcontinent.

I am sure that Booth and colleagues would be able to provide these data; readers would then be in a position to look at their policies.

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1 Booth H, Pollitt C, Jessen EC, Hendrick DJ, Cant AJ. When is referral of Heaf test positive schoolchildren worth while? Prospective study. *BMJ* 1996;313:726-7. (21 September.)

Evidence is not sufficiently persuasive to overturn current recommendation

Editor—Helen Booth and colleagues state that they found five cases of active tuberculosis among children with a grade 2 result on Heaf testing before BCG vaccination under the schools scheme, and they suggest that all such children should be fully assessed.[1] The authors, however, provide few details about these children.

All the children had received BCG vaccination in the past, so that a positive result on tuberculin testing was not unexpected. Active tuberculosis was diagnosed on the basis of tuberculin testing and "abnormal results in chest radiographs," but did these children have any other clinical evidence of active tuberculosis such as fever and failure to gain weight, and did the radiological abnormalities improve with treatment? If not then the diagnosis of active tuberculosis and the decision to notify the case are questionable: the chest radiographic findings could have been due to inactive tuberculosis (in which case a course of antituberculous chemoprophylaxis would have been appropriate) or to some other cause. In children with active tuberculosis it is common to find the source case among adult contacts, but Booth and colleagues found no case of active tuberculosis among the contacts of these five children.

For many years the district in which I work has had a policy of neonatal BCG vaccination for neonates from ethnic minorities at risk of tuberculosis, and this has recently been expanded to cover all neonates. Many of these children therefore develop moderate and sometimes considerable tuberculin sensitivity and have a grade 2 result on Heaf testing later as part of the schools BCG programme. Until the British Thoracic Society's latest code of practice was published in 1994 we routinely examined children with a grade 2 result on Heaf testing, but, irrespective of their BCG status, we never identified a single case of active tuberculosis, and we stopped screening such children when the revised guidelines appeared.[2] We continue to offer screening to children with a grade 3-4 result on Heaf testing.

The evidence presented by Booth and colleagues is not sufficiently persuasive to overturn the current recommendation that no action need be taken in children routinely found to have a grade 2 result on Heaf testing before BCG vaccination at school.

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1 Booth H, Hendrick DJ, Politt C, Jesson C, Cant A. When is referral of Heaf test positive schoolchildren worth while? Prospective study. *BMJ* 1996;313:726-7. (21 September.)
2 Joint Tuberculosis Committee of the British Thoracic Society. Control and prevention of tuberculosis in the United Kingdom: code of practice 1994. *Thorax* 1994;49:1193-2000.

Authors' reply

Editor—David Elliman asks about the young children whom we screened. Only three of the 78 children were below secondary school age (one aged 5, two aged 10). All were screened for routine reasons, and only the 5 year old was found to have tuberculosis. Exclusion of these children does not alter our main finding that children with a grade 2 result on Heaf testing who have not previously received BCG vaccine need to be referred for confirmation of their tuberculin positivity.

Elliman's and Geoffrey E Packe's confusion about the number of children with a positive result on Mantoux testing after previous BCG vaccination was answered in our table 1: only 22% had had BCG vaccination. None of the children found to have tuberculosis after referral because of a grade 2 result on Heaf testing had had BCG vaccination. Table 1 also showed the proportion of children from the Indian subcontinent (22%). Active tuberculosis is difficult to diagnose in children because, unlike in adults, symptoms are often few or non-specific. Our children were referred only because of the result of the Heaf test. All, however, were diagnosed according to internationally agreed criteria.[1]

The children's disease was confirmed by the fact that further cases of tuberculosis were found by contact tracing. When we started this study we too thought that routinely screening children found to have a grade 2 result of a Heaf test on screening at school was probably not worth while. Our results surprised us, but it should be noted that we investigated only children with a positive result of both Heaf and Mantoux tests. As we showed, only 28% of children with a grade 2 response and 64% of those with a grade 3 response were also positive on Mantoux testing. Britain is the only country that relies on a multiple puncture to diagnose childhood tuberculosis, and perhaps the greater specificity of the Mantoux test, together with the pattern of tuberculous infection in the deprived inner city areas of Tyneside, explains our rate of tuberculosis of 11% in those with grade 2 results of Heaf tests who were also positive on Mantoux testing. The rate of childhood tuberculosis is one of the best monitors of the effectiveness of our national tuberculosis control programme.

Our results have persuaded us to continue screening children and chasing contacts of those with grade 2 results of Heaf tests. Perhaps Elliman and Packe should conduct similar studies in their patients, as only in this way can they rationally decide on the validity of their protocols.

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1 Smith MHD, Starke JR, Marquis JR, Tuberculosis and opportunistic mycobacterial infections. In: Feigin RD, Cherry JD, eds. *Textbook of pediatric infectious disease.* Philadelphia: Saunders, 1990:1344.

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

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

Letters should be typed and signed by each author, and each author's current appointment and address should be stated. We encourage you to declare any conflict of interest. Please enclose a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

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

Letters will be edited and may be shortened.

Statistical basis of public policy

Epidemiology does not need Bayesian inference

Editor—R J Lilford and D Braunholtz propose that a Bayesian perspective in the interpretation of epidemiological results may prove fruitful.[1] Their central thesis is that public health decisions are often based on the results of significance tests. Instead they propose that other prior knowledge of the association be incorporated into the calculation of a subjective Bayesian probability. It is unlikely for several reasons that this will happen in epidemiology.

Firstly, they are wrong in suggesting that conventional significance tests are a basis for public health policy. Policy decisions made on the basis of epidemiological data are determined by a whole host of reasons, including the perceived validity of the results, the absolute risks, and the consequences of inaction. It is rare that the probability of the association, whether frequentist or Bayesian, plays more than a minor part.

Secondly, Bayesian statistics does not address the main challenges when interpreting epidemiological data. Any effect measure can be contaminated by three factors—confounding, bias, and chance. Confounding presents the most serious challenge because its possibility can never be excluded. Lilford and Braunholtz do not mention the role of confounding when interpreting results, and it seems that Bayesian statistics has little to offer in this area. A Bayesian assessment of bias also seems to offer little. The authors present an example of how the potential for bias can be incorporated into a final analysis by saying that if epidemiological studies tend to overestimate results by 30% then this may be corrected by reducing the effect estimates by a similar amount. An alternative is to anticipate which selection and information biases are likely to be problematic and to design better studies that minimise them.

While Bayesian statistics has proved valuable in certain areas—for example, in diagnostic medicine, where prior beliefs are based on hard data and not on subjective notion—it has little to offer in judging the validity of disease associations. As has also been discussed extensively in the past, epidemiology has little use for traditional frequentist statistics, with its overemphasis on significance tests and its "penalty for peeking" through the insistence on corrections for multiple comparisons.[2] While a valid form of statistical inference has yet to emerge for epidemiology, the consequences of this may be minimised by a form of scientific inference, which places issues of bias and confounding above those of chance [3].

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1 Lilford RJ, Braunholtz D. The statistical basis of public policy: a paradigm shift is overdue. *BMJ* 1996;313:603-7. (7 September.)
2 Poole C. Beyond the confidence interval. *Am J Public Health* 1987;77:195-8.
3 Brennan P, Croft P. Interpreting the results of observational research: chance is not such a fine thing. *BMJ* 1994;309:727-30.

Qualitative and quantitative aspects should not be confused

Editor—Klim McPherson's editorial about four case-control studies of the risk of venous thromboembolism associated with oral contraceptives[1] has generated an eloquent account of the Bayesian approach to such issues by R J Lilford and D Braunholtz[2] and a supportive editorial.[3] While these make some valuable points, ultimately they obfuscate the issues.

The starting point of the commentaries is that "conventional" statistical analysis dichotomises results into significant and non-significant and that this dichotomy is a false basis for decision making. The first statement is untrue, and the second merely reflects what the careful accounts of statistical methods emphasise. The appropriate conventional analysis is a set of error bands for the relative risk; there is no controversy in giving these a Bayesian interpretation if desired. These error bands provide a summary of the information in the data, provided that the studies are free of systematic error and that the error calculations are technically correct. From any reasonable viewpoint, a decision or overall conclusion influenced by the data must take account of relevant additional information and, in the case of a decision, of the consequences of "incorrect" action. Any contentious aspect is concerned not with whether such further considerations are relevant but with whether such aspects are entered qualitatively or quantitatively.

A key issue in the present instance is the possibility of bias in the case-control studies. If there were directly relevant statistical information on such bias its incorporation via Bayes's theorem would be entirely uncontroversial. Otherwise, it seems most sensible to show the effect on the error limits of various levels of bias. This may lead, for example, to the conclusion that a 20% bias would be enough to leave considerable uncertainty about whether a raised relative risk is indeed present. It would be a matter of qualitative judgment how likely it is that such a bias could be present in the studies concerned. Similar results apply to the incorporation of the initial opinion of distinguished colleagues .

Vigorous discussion of the principles underlying the design, analysis, interpretation and application of research studies is surely to be welcomed, but it would be a great pity if differences of technical approach were exaggerated into differences about the qualitative issues.

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1 McPherson K. Third generation oral contraception and venous thromboembolism. *BMJ* 1996;312:68-9.
2 Lilford RJ, Braunholtz D. The statistical basis of public policy: a paradigm shift is overdue. *BMJ* 1996;313:603-7. (7 September.)
3 Freedman L. Bayesian statistical methods. *BMJ* 1996;313:569-70. (7 September.)

Present remembrance of priors past is not the same as a true prior

Editor—A Bayesian is "one who asks what you think before an experiment in order to tell you what to think afterwards," but in R J Lilford and D Braunholtz's version[1] this must be amended to "one who asks what you would have thought if you had not seen the data in order to tell you what to think now." But present remembrance of priors past is not the same as a true prior, and are not Lilford and Braunholtz being disingenuous when they claim, "here we start with prior belief which is measured and made explicit"? Did they really contact Professor James Drife and Dr Nicholas Johnson before the results on which Klim McPherson's editorial about deep venous thrombosis and the third generation contraceptive pills[2] was based were known, just on the off chance that such data might be discussed? If not then they presumably invited these experts to subtract data subjectively, which they then subsequently "objectively" added back for them. Why did they not just ask them what they thought?

Although I agree with many of Lilford and Braunholtz's strictures against significance tests, if there is a dilemma facing the chairperson of the Committee on Safety of Medicines then it is to choose between two actions. Whether the evidence is dichotomous or not, the dilemma remains: to warn or not? This is not to say that the "evidence" may be represented by a significance test. Neither a significance test nor a set of Bayesian posterior distributions, whether genuine or pseudo, constitutes evidence. If there is such a thing then it is presented in McPherson's editorial. I am at a loss to see how Lilford and Braunholtz's analysis improves on this. Of course, what the authors do show is how the chairperson of the committee can use McPherson's editorial with his or her own prior to produce a posterior distribution, and how any patient can do the same. This demonstration is useful. What they do not show is that, Bayesian or not, anything more than the sort of summary presented by McPherson is worth communicating, and I do not understand their objection to it. The irony is that it is because McPherson presented conventional estimates with associated measures of precision that they were able to perform their Bayesian calculations. Had McPherson presented his own Bayesian posterior distribution they would not have been able to do so.

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1 Lilford RJ, Braunholtz D. The statistical basis of public policy: a paradigm shift is overdue. *BMJ* 1996;313:603-7. (7 September.)
2 McPherson K. Third generation oral contraception and venous thromboembolism. *BMJ* 1996;312:68-9.

Bayesian analysis should be used instead of league tables of performance

Editor—There is an increasingly important area in which Bayesian analysis[1] is unfortunately not implemented—namely, league tables based on supposedly objective performance indicators. An excellent, and largely non-technical, discussion is provided by Goldstein and Spiegelhalter.[2] For example, the New York State Department of Health is required, by law, to publish information on the mortality of patients operated on by individual surgeons. Table 1 shows the mortality adjusted for risk for 17 of 88 doctors performing coronary artery bypass surgery.[3] The adjustment for risk is a worthy attempt by the state department to adjust individual mortality for each hospital's client populations and the seriousness of the cases. The relative ranking for these 17 doctors is given in the style of a league table in the second column, with the "worst"

Table 1 Mortality adjusted for risk, naive rankings, and empirical Bayes rankings for 17 surgeons performing coronary artery bypass surgery in New York State

Surgeon	Mortality adjusted for risk	Naive rank	Mean empirical Bayes rank (95% confidence interval)
Bergsland	1.04	1	2.8 (1 to 9)
Tranbaugh	1.54	2	4.8 (1 to 12)
Yousuf	1.90	3	5.3 (1 to 13)
Britton	1.88	4	5.6 (1 to 14)
Raza	2.19	5	6.2 (1 to 13)
Vaughan	2.21	6	7.1 (1 to 15)
Quintos	2.28	7	7.3 (1 to 15)
Ferraris	2.40	8	7.4 (1 to 15)
Bennett	2.79	9	9.0 (1 to 17)
Foster	3.05	10	9.4 (2 to 16)
Cunningham	3.06	11	9.6 (2 to 16)
Bhayana	3.21	12	10.3 (2 to 16)
Lewin	3.43	13	11.0 (3 to 16)
Borja	4.46	14	13.7 (6 to 17)
Older	5.45	15	14.1 (6 to 17)
Canavan	5.02	16	14.3 (7 to 17)
Lajos	5.14	17	15.4 (9 to 17)

surgeon having a mortality nearly five times that of the "best."

This analysis, however, suffers from some serious drawbacks. The most important of these is that the data are for a snapshot in time (1990-2), and the numbers of deaths for each surgeon are quite small. Measuring the performance of a surgeon on the basis of these data is rather like watching a football match for 10 minutes (selected randomly) and deciding that, if one team scores in this period, then it will win the match. Obviously there is a great deal of uncertainty in this judgment. In a Bayesian sense, however, we can state that we will judge the performance of each doctor on the basis of prior knowledge of the distribution of mortality among all the doctors in our sample. Using this information, we can produce empirical Bayes mean rankings with 95% confidence intervals by simulation methods.[4] From table 1 we see that none of these 17 doctors can confidently be placed in the top or bottom half of their performance league table.

The second problem is that someone has to come last, although because of random variation this person is unlikely to come last next time. In fact, nearly half of the 88 surgeons moved from one half of the table to the other in a subsequent analysis. Unfortunately, some surgeons were dismissed because of the naive analysis of their patients' mortality. Given the complexities of their occupation, surely these surgeons deserve to be judged with more sophisticated methods of analysis of their performance.

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1 Lilford RJ, Braunholtz D. The statistical basis of public policy: a paradigm shift is overdue. *BMJ* 1996;313:603-7. (7 September.)
2 Goldstein H, Spiegelhalter D. League tables and their limitations: statistical issues in comparisons of institutional performance. *Journal of the Royal Statistical Society A* (in press).
3 New York State Department of Health. *Coronary artery bypass surgery in New York State, 1990-92.* Albany: New York State Department, 1993.
4 Langford IH, Leyland AH. Comment on Goldstein and Spiegelhalter's paper. *Journal of the Royal Statistical Society A* (in press).

Bayesian statistics is valuable provided it is based on data

Editor—In their paper on the statistical basis of public policy R J Lilford and D Braunholtz suggest a shift towards the Bayesian approach.[1] The authors say that "Bayesians view probability as a degree of personal belief. Personal belief changes as evidence (data) accrues, but no data at all are necessary." I have found Bayesian statistics to be extremely valuable in determining future action in a commercial or manufacturing environment, provided it is based on data.[2] However, all attempts that have been based on opinion have proved useless—so much so that it seemed that the more firmly an opinion is held without supporting data the less likely it is to be valid. It is to be expected that the true believer will not look for further evidence: he or she is not a doubting Thomas.

Readers may question whether industrial experience is relevant to medical or pharmacological practice, but they should remember that it was once strongly held that six eggs and 33 ounces of full cream milk daily with a little sugar and less raw beef (Lenhartz's diet) made a suitable treatment for gastric and duodenal ulcer.

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1 Lilford RJ, Braunholtz D. The statistical basis of public policy: a paradigm shift is overdue. *BMJ* 1996;313:603-7. (7 September.)
2 Durrant NF. Some case studies in acceptance sampling by attributes. *Quality Assurance* 1981;7:31-2.

Bowel preparation at home in elderly people

Patients should be warned not to drink too much or too little fluid

Editor—T D Heymann and colleagues report on the relative safety of bowel preparation at home in elderly people; none of their patients required admission to hospital.[1] We wish to report on two patients admitted in February and April of this year with serious complications of bowel preparation with sodium picosulphate (Picolax, Nordic) at home.

The first patient was an 85 year old woman who presented with a score on the Glasgow coma scale of 5/15 and a tonic clonic seizure, having drunk some 5 litres of water with the sodium picosulphate the previous day (patients receive typed instructions saying "drink plenty of clear fluids"). On admission her serum sodium concentration was 111 mmol/l. She was treated with intravenous hypertonic saline and recovered fully over the next five days, her serum sodium concentration having returned to normal.

The second patient was admitted the day after bowel preparation with sodium picosulphate. She presented with diarrhoea and vomiting and was fluid depleted. Her score on the Glasgow coma scale was 6/15, and she had twitching of her lips. On admission her serum sodium concentration was 121 mmol/l, having been 142 mmol/l two months previously. She was treated with intravenous normal saline and recovered fully in three days.

Although bowel preparation at home is safe, potentially serious complications may arise if abnormal fluid intake or losses occur simultaneously. Advice sheets should be carefully worded, particularly with regard to oral fluid replacement, and admission to hospital should always be considered for frail patients undergoing bowel preparation.

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1 Heymann TD, Chopra K, Nunn E, Coulter L, Westaby D, Murray-Lyon IM. Bowel preparation at home: prospective study of adverse effects in elderly people. *BMJ* 1996;313:727-8. (21 September.)

Give a simultaneous infusion of saline in frail patients

Editor—T D Heymann and colleagues should temper their enthusiasm for bowel preparation at home before colonoscopy.[1] After some episodes of hypotension occurred in elderly patients before colon surgery, colleagues and I investigated the haemodynamic effects of bowel preparation with sodium picosulphate (Picolax, Nordic)[2]. We found a mean reduction in weight of over 2 kg in the treated group compared with a control group. The treated group showed an increase in mean heart rate from 77 to 92 beats/min on moving from supine to erect posture and a small decrease in systolic blood pressure. The two frailest patients had appreciable and symptomatic postural hypotension and required resuscitation before surgery could start. The surgical unit's policy was changed, so that frail elderly patients are now given a prophylactic infusion of 2 litres of saline during bowel preparation.

Bowel preparation causes considerable dehydration, particularly in elderly people, and the resultant haemodynamic changes may be hazardous to frail patients. Bowel preparation should not be given to such patients outside hospital, particularly if they must stand or walk.

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1 Heymann TD, Chopra K, Nunn E, Coulter L, Westaby D, Murray-Lyon IM. Bowel preparation at home: prospective study of adverse effects in elderly people. *BMJ* 1996;313:727-8. (21 September.)
2 Barker P, Trotter T, Hanning CD. A study of the effect of Picolax on body weight, cardiovascular variables and haemoglobin concentration. *Ann R Coll Surg Engl* 1992;74:318-9.

What is the prior probability of a proposed new treatment being superior to established treatments?

Editor—Terence Stephenson and David A Walker ask whether parents and children who are considering participating in a clinical trial of a new treatment for acute lymphoblastic leukaemia should be made aware that "for the past decade the new experimental treatments [studied in randomised trials] have given better outcomes."[1] In other words, hunches based on non-randomised evidence about which developments in the management of acute lymphoblastic leukaemia would turn out to be real advances have had an excellent track record recently. In the light of this, what should be one's prior belief about the likelihood that the next new treatment proposed will also represent an advance?

This is an important general issue, but, as far as I am aware, it has only rarely been addressed systematically. How usual is the reported recent track record of new treatments for acute lymphoblastic leukaemia, and how likely is it that this successful run of new treatments is attributable to chance?

My impression is that there is a professional and lay tendency to assume that most developments in health care are advances. It would have been helpful if Stephenson and Walker had presented (or referenced) the evidence from the leukaemia trials to which they refer because I have encountered only three other relevant analyses, all of which suggest that new treatments are as likely to be inferior as they are to be superior to existing alternatives (D Machin and M K B Parmar, and M Buyse and O Dalesio, colloquium on long term clinical trial strategies, Worcester College and Radcliffe Infirmary, Oxford, 15-17 December 1989).[2] I would be grateful to anyone who could point me to other data, preferably derived from prospective cohort studies, that might help in an estimation of the prior probability of a proposed new treatment being superior to an established treatment.

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1 Stephenson T, Walker D. Gaining patients' consent. *BMJ* 1996;313:362-3. (10 August.)
2 Gilbert JP, McPeek B, Mosteller F. Progress in surgery and anesthesia: benefits and risks of innovative therapy. In: Bunker JP, Barnes BA, Mosteller F, eds. *Costs, risks and benefits of surgery.* New York: Oxford University Press, 1977:124-69.

Oral contraceptives are drug of choice for menorrhagia in the Netherlands

Editor—John Bonnar and Brian L Sheppard studied, in an academic setting, only 81 patients selected from a population of over 400 (referred?) patients complaining of heavy menstruation.[1] Unfortunately, they supply insufficient information to make general practitioners reconsider their policy on treating menorrhagia. We are members of a group of Dutch general practitioners that worked on a national guideline on the diagnosis and treatment of abnormal vaginal bleeding in primary care.[2]

For menorrhagia we consider oral contraceptives to be the drug of choice, reducing blood loss by half. To our surprise, Bonnar and Sheppard do not mention this treatment, even in their paragraphs titled "comparison with hormonal treatment." We consider tranexamic acid to be a powerful but third ranking treatment option, as it has more side effects than oral contraceptives and prostaglandin synthetase inhibitors such as naproxen.[3] A promising treatment not mentioned in the paper is the levonorgestrel releasing intrauterine device, which reduces blood loss by 80-90% and became available to all Dutch doctors this year.[4]

The authors' conclusion that ineffective medical treatment contributes to the large number of hysterectomies in Britain supposes a causal relation that is not supported by the study's results. It is also wrong for the authors to base their conclusion on a comparison of Britain and Scandinavia. Traditional values and cultural factors, not prescribing behaviour itself, explain the huge differences in the rates of operative procedures among Western countries.[5]

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1 Bonnar J, Sheppard BL. Treatment of menorrhagia during menstruation: randomised controlled trial of ethamsylate, mefenamic acid, and tranexamic acid. *BMJ* 1996;313:579-82. (7 September.)
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Surgery for mental illness has been proved effective

Editor—Sandra Goldbeck-Wood reports that Norway has offered compensation to all patients who have had a lobotomy in Norway in the past.[1] Her article seems to leave the opponents of psychosurgery on a high moral ground. What the Norwegian fiscal administration chooses to do with presumably surplus money is its own decision, but Goldbeck-Wood's use of the word "victims" implies that patients who had a leucotomy in the past were the subjects of medical misjudgment, if not frank assault; this requires correction.

The fact that the former patients who had a lobotomy in Norway are having to be located by advertisements in the media indicates that they are living out of contact with the hospital services and are not psychoneurological derelicts. The surgical procedure and the selection of patients who were thought likely to benefit were more uncertain decades ago than is the case today. Nevertheless, this must be set against the fact that no other treatments for severe psychotic illnesses were then available; consequently people with such illnesses were often consigned to a life of indescribable torment, usually in the back wards of mental asylums. That psychosurgery produced relief of this distress seemed to justify the difficult decisions involved in offering the treatment.

As Bryan Christie pointed out in his article on neurosurgery for mentally ill people in Scotland, psychosurgery has been tarnished by its image in *One Flew Over the Cuckoo's Nest*[2]—that is, of a procedure carried out by vengeful doctors in adversarial relationship with their patient. Since the introduction of psychosurgery the discovery of other methods of treatment has led to a considerable reduction in the need for it. The need does, however, still exist, and audits of the procedure (colleagues and I have just completed our own)[3] justify its retention.

Those who inveigh against the practitioners of psychosurgery, claiming some sort of unethical implication in its use, should be reminded of the unethical aspect of withholding information about a proved effective treatment from a patient on the grounds of their personal dislike of such an intervention. They should also be asked what better method they can offer to patients who have failed to respond to all other treatments available.

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1 Goldbeck-Wood S. Norway compensates lobotomy victims. *BMJ* 1996;313:708. (21 September.)
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Doctors' retainer scheme

Time limit of five years may be too short for some doctors

Editor—Alison Douglas and Ian McCann report their study of opinions on the doctors' retainer scheme among people who have been on the scheme and among general practitioner employers.[1] While I welcome the many sensible suggestions put forward and agree that membership of the scheme should be regarded as temporary, I believe that the suggested five year limit is far too inflexible. My situation illustrates why. I started on the scheme near the end of my first pregnancy. Five years later I had three children, aged 4½, 2½, and 10 months (and the next year I was expecting my fourth child). If I had been forced off the scheme at that point I would have been unable to work at all—which would seem to defeat the whole purpose of the scheme.

I suggest that the time limit should be tailored to the person's own circumstances. Do the authors really mean to imply that doctors on the retainer scheme should be restricted to only two children?

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1 Douglas A, McCann I. doctors' retainer scheme in Scotland: time for change? *BMJ* 1996;313:792-4. (28 September.)

Should be more widely available in hospital practice

Editor—The Medical Women's Federation helped to negotiate the setting up of the doctors' retainer scheme in the late 1960s and has watched its development (or lack of it) with concern. Thus I was interested to read Alison Douglas and Ian McCann's review of the scheme.[1] At meetings with Baroness Cumberlege, a parliamentary under secretary of state for health, over the past four years the federation has argued for reform of the scheme with limited success, but the recent support of the Royal College of General Practitioners and the General Medical Services Committee has encouraged us to think that the necessary upgrading may now happen.

However welcome improved flexibility and better terms and conditions would be to general practitioners on the scheme, I remain concerned that doctors working in other specialties will still have virtually no access to this kind of "mark time" scheme. The retainer scheme is almost unknown in hospital practice, mainly because of lack of funding and poor publicity. The advent of flexible training will help but will not entirely remove the need for a properly thought through retainer scheme. Douglas and McCann were unable to report on the small numbers of hospital based doctors on the scheme, and we have no information on the numbers of doctors lost to medicine and the NHS because they were unable to keep in touch through a difficult time. Do I have to point out that the vast majority of doctors on the scheme are women with small children and that the proportion of women medical students now exceeds that of men in many of our medical schools?

For all its current limitations, the retainer scheme has prevented doctors from leaving medicine altogether and has enabled them to rejoin the NHS in substantive posts within a few years. We cannot afford the loss of more trained doctors, from any specialty.

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1 Douglas A, McCann I. Doctors' retainer scheme in Scotland: time for change? *BMJ* 1996;313:792-4. (28 September.)

Results of American and European studies of thrombolysis in acute stroke are not conflicting

Editor—The recently published clinical trials of thrombolysis in acute ischaemic stroke have generated much discussion and an unnecessary discrepancy between American and European committees regarding the recommended use of alteplase for acute ischaemic stroke, as Julien Bogousslavsky points out in his editorial on thrombolysis in acute stroke.[1]

The different results in the American[2] and European[3] studies are not conflicting, because the methodologies differed in important ways. In the American study, which had a positive result, thrombolytic treatment was given considerably sooner and the total dose of alteplase was 18% lower than that in the neutral (American) study. This is doubly advantageous: earlier reperfusion resuscitates a larger portion of the ischaemic brain and is less likely to result in a haemorrhagic transformation because the blood vessels are less severely damaged. As Bogousslavsky points out, "time is probably the most important factor for defining the therapeutic window in acute stroke." This difference in the timing of treatment alone may have accounted for the different results between these two studies. Also, the risk of haemorrhagic complications decreases with decreasing doses of alteplase.

In addition, Bogousslavsky compares the 12% absolute increase in favourable outcome with the 6% absolute increase in symptomatic brain haemorrhage, but this is an unfair comparison. The 12% absolute increase in favourable outcome is an overall outcome and includes the 6% increase in symptomatic brain haemorrhages. This is therefore equivalent to counting the symptomatic brain haemorrhages twice. Also, this 6% increase in symptomatic brain haemorrhage refers only to complications occurring within 36 hours of the onset of stroke. During the entire study there was a 7.7% increase in symptomatic brain haemorrhages in the group treated with alteplase.

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Charity has published information booklet about clinical trials in cancer

Editor—Su Mason and Shona Haining draw attention to the small number of patients with cancer who are entered into clinical trials and suggest that incentives to recruit patients into trials are required.[1] BACUP, a national charity providing information and counselling for people affected by cancer, has recently published a booklet, *Understanding Clinical Trials*, which doctors can give to patients who may be eligible for clinical trials. The booklet sets out the key issues and should help reduce the time needed by the medical team to explain all the issues[2] and thereby facilitate greater participation in clinical trials.

We believe that a substantial proportion of the 6000 patients newly diagnosed as having cancer each week would be both eligible for national trials and willing to participate[3] if the often confusing and sometimes frightening issues concerning such trials were dealt with. We hope that this booklet will serve the dual purpose of helping patients understand more about clinical trials and thereby improving accrual.

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Assuming that developing countries could not afford xenotransplantation is patronising

Editor—Neville Goodman is referring to me when he describes a colleague throwing articles about xenotransplantation into the bin.[1] He sent me the articles when I questioned his opposition to xenotransplantation at a meeting of our local ethical committee. One of the articles discussed the gap between health spending in rich and poor countries but had the same difficulties as Goodman in reaching useful conclusions. The other was a newspaper letter questioning the value of high technology treatments in prolonging life and suggesting instead that we devote our wisdom to studying the acceptability of death. To me as a nephrologist, the suggestion that keeping patients with end stage renal failure alive and well is "grotesque" was indeed difficult to swallow.

No thoughtful doctor in the developed world can fail to notice that many of the treatments we offer here could not be dreamt of in poorer countries. Most come to the same conclusion as that reluctantly reached by Goodman—that reducing health spending in richer countries will not increase spending on public health in poorer countries. Resources for renal replacement therapy are extremely limited in Britain compared with other Western countries, and these cost pressures have led to overreliance on peritoneal dialysis and to an emphasis on transplantation wherever possible, given that successful transplantation is much cheaper than long term dialysis. In many developing countries transplantation is the only type of renal replacement therapy available. This has led to practices that to us in the privileged world do indeed seem grotesque, such as paid donation of unrelated live donors and the trade in organs retrieved from victims of judicial execution in China.

The increasing gap between demand for transplantation and supply of cadaveric organs (which should be welcomed in as much as it reflects better prevention of head injuries) has prompted interest in xenotransplantation. Goodman objects to this, supposing that the expense will prevent extension of the technology to poorer countries. This is not necessarily true. Xenotransplantation is likely to prove considerably cheaper than long term dialysis. It is patronising to assume that all developing countries (many of which spend huge sums on importing arms) will be unable to afford this treatment. Should we still "start to draw the line" here?

There are no easy answers. But to characterise me as blinkered for not keeping ill thought out and emotive articles is cheap and offensive.

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1 Goodman N. This is where I start to draw the line. *BMJ* 1996;313:696. (14 September.)

BMA asks for 53% pay increase

Morality of such claims is doubtful in light of growing disparity in society

Editor—In giving evidence to the Doctors' and Dentists' Review Body the BMA stated that fair comparisons with people in comparable positions should be the most important factor in setting pay levels.[1] There are a few drawbacks with this approach. Firstly, how does one define comparable professions or those in similar walks of life? Secondly, is it unanimously agreed that we want to be compared with all groups that are often quoted for this purpose, such as actuaries, accountants, and barristers—especially the last mentioned? Thirdly, there is an inherent assumption in this argument that these groups themselves receive justifiable levels of remuneration. I have reservations about whether society at large would accept as justifiable the earnings of some barristers and other high earners.

It must be remembered that the Doctors' and Dentists' Review Body makes recommendations on behalf of society—the British public. The question is therefore, will society accept it? Or for the business minded the question is, can Britain bear the cost? What do you call a doctor who does not listen to his or her patient?

This does not mean that I disagree with the chairman of the BMA's council when he states that doctors are undervalued and that for far too long the government has blatantly exploited the good will of the staff in the NHS. Recognition of the value of individuals or a group of individuals need not necessarily always be expressed in monetary terms.

Why does the BMA not compare itself with other dedicated professions and observe how underpaid they are in comparison with doctors? Do doctors think that teachers and nurses receive a satisfactory salary for their respective roles?

Before we brandish figures such as 53% in our pay claims we should also pause and reflect on the morality of such claims in the light of the growing disparity in society. Rather than trying to justify our claims on the basis of the unjustifiable earnings of other groups perhaps we should aim at a more justifiable socioeconomic climate and curtail the competitive attitude that has assumed dominance in the past few years. Those past few years have shown how this attitude is addictive and contagious. Today the comparisons are made with other high earners; how long will it be before this leads to competition and division among ourselves? Wouldn't that be a dream fulfilled for someone?

Our leaders did not get much support when they asked for a more united opposition to the NHS reforms. I wonder how much support there is for the current pay claim.

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1 Beecham L. British doctors need 53% pay increase to catch up. *BMJ* 1996;313:769. (28 September.)

General practitioners want parity with hospital consultants

Editor—The issue of 28 September contained a glossy, and expensive, circular about doctors' pay, distributed by the BMA. This may look superficially impressive and effective, but the chart on the second page (figure 1) is a particularly elegant display of its most basic flaw. Even a casual glance at the chart shows that the ratio between the incomes of doctors and others in supposedly comparable professions has been constant for the entire period that the chart illustrates. It shows not that doctors' pay has fallen behind that of other professions but that doctors are not in fact comparable to other professions and have not been so for two decades. Putting aside the credibility of any demand for a 53% increase, this document is a stunning own goal.

A more credible and constructive target for general practitioners would be to achieve parity with our hospital consultant colleagues, who should seek to catch up with 1980 levels. With hospital postgraduate training shortened and the greater responsibilities of general practice, it is impossible to justify the intended remuneration of an experienced general practitioner not being equal to that of an experienced consultant, rather than nearly £10 000 a year less. How does the Department of Health justify additional merit awards for specialists with praiseworthy records in teaching, research, and the care of patients when there is no comparable system of awards for general practitioners? Higher degrees are more prevalent among specialists only because specialists have greater opportunities for such study when training; but the comprehensive assessment of exemplary general practice recognised in the award of fellowship of the Royal College of General Practitioners is far in advance of any form of performance review developed in hospitals.

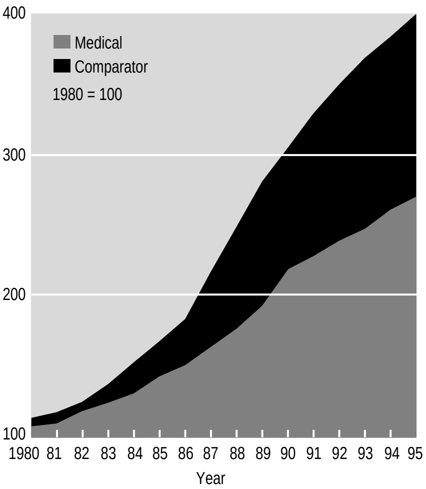

Fig 1 Widening gap between doctors' pay and that of comparable professions

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Reply from secretary of BMA

Editor—The decision to publish a section of the BMA's evidence to this year's review body was not taken lightly but hinged on two facts.

The first was the knowledge, which we gained in late July, that this year the Department of Health was considering publishing its own evidence. Consequently we needed to consider publishing our evidence at a time of our choosing and not as a reaction to some government initiative.

The second consideration was the evidence itself. On the basis of indexation, by comparison with those groups with which we had been compared in 1980, doctors' income had fallen behind by 53%. The shortfall started in 1981, and the gap has widened in each successive year. To answer A E A Joseph's point, therefore, we did not choose the groups out of thin air but are simply stating a fact. This evidence was largely substantiated by an independent analytical study commissioned by the BMA, which has also been submitted to the review body. The question that critics such as Joseph must answer in return is whether they would have preferred the BMA to suppress or ignore knowledge of this shortfall when presenting this year's evidence on their behalf.

Not only does Simon Fordham ignore the fact that the pattern of general practitioners' career earnings differs from that of hospital consultants, but he has missed the point that the evidence we have published is the general economic case on behalf of the whole profession. This is only part of the whole submission: the individual crafts have each made their own justification for this year's award for their own members, not only in writing but also in the usual oral presentations to the review body.

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Correction

Should breast reduction surgery be rationed?

An error occurred in the second letter in this cluster, by Margaret Somerville and others (7 December, p 1479). The position given for the second author, Gina Radford, is incorrect; she is in fact director of public health for South and West Devon Health Authority.