

Doctors should work towards elimination of all firearms and knives

EDITOR—With the publication of the Cullen report into the shooting of several children at a school in Scotland¹ and the cross party support in parliament to increase the controls on handguns, the moment is ripe for the medical profession to pursue a new health campaign. Over the past three or four decades the profession has had some success in raising public awareness of the risks of cigarette smoking and through that, probably, in saving lives. The reduction of the suicide rate is an objective of the Health of the Nation for England, yet suicide rates are still rising among young men.² Homicide is a major political issue. Weapons, especially firearms and knives, contribute to both these forms of premature death. Elimination of all firearms and knives that are surplus to domestic and industrial requirements would not solve either problem entirely, but it would make an important contribution.

We believe that the profession, through the BMA and one or two royal colleges (particularly the Royal College of Psychiatrists), could develop policies about weapons control. The prime minister has already indicated that he would introduce controls on knives if a workable definition for the surplus ones was drawn up.

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2 Secretary of State for Health. *The health of the nation*. London: HMSO, 1992. (Cm 1986.)

Rationing health care

Several other markers of fairness exist, besides age

EDITOR—Once again it seems that elderly people are being asked to bear the brunt of the rationing of scarce resources in the NHS.¹ I am not surprised that an economist suggests that it is fair to transfer funds from elderly to young patients; what is surprising, however, is the reasons that Alan Maynard gives for his views.

Maynard writes, "One possible definition of 'fairness' in health care is that

decision makers will use the NHS to reduce inequalities in people's lifetime experience of health." He then suggests that one of the ways that this can be achieved is by use of the "fair innings" argument. I accept that fairness is a difficult concept to define. But why has Maynard chosen to use only age as a marker of fairness, and not other things that are more relevant? Take the case of two people, one aged 25 and the other aged 75. The younger person may have already used up large amounts of NHS resources owing to a reckless lifestyle, whereas the older person may not have used the NHS at all but, at an advanced age, needs the hip replacement to which Maynard refers. Why would it be considered fair to give the younger person preference in such a situation?

Furthermore, it is not the function of the NHS to reduce inequalities in health. Surely the purpose of the NHS is to treat people who are ill, or, as Maynard suggests in his article, "to improve the health of the population." I appreciate that the population has a better record of health in countries where the distribution of income is narrow than in countries where inequalities in wealth are greater,² but it is the business of the government to address this matter (if it wishes to) and not those working in the NHS.

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2 Wilkinson RG. *Unhealthy societies. The afflictions of inequality*. London: Routledge, 1996.

Can a "fair innings" ever be fair?

EDITOR—Alan Maynard argues that the principles of rationing in the NHS remain implicit and confused.¹ He is right. The lack of explicitness is due largely to the complexity of the issues. Maynard raises one of the issues himself, discussing the idea that society should behave "fairly" when it comes to the allocation of health resources. This idea is based on the concept of a "fair innings" in life and that healthcare resources should be allocated so as to reduce the inequalities in people's lifetime experience of health. Once a person has reached 70 everything beyond that is a bonus and resources can be diverted to treat younger people so that their chances of achieving a fair innings are improved.

There are two arguments against this approach. Firstly, it is ageist, although, as Harris has pointed out, it allows us to preserve our feeling that the old who have had a good run for their money should not

be propped up at the expense of those who have not had the same chance.² That feeling does not overcome the unfairness of potentially ending one person's life because another person with the same desire to live has a greater life expectancy. This could lead to the situation illustrated by Glover, whereby we choose to save one person for a predicted span of 60 years rather than saving five people each for a predicted span of 10 years.³ In doing this we will have gained 10 life years at the cost of overriding the desires of four extra people. The consequences in terms of the effect on the wider family or society do not seem to be taken into account and therefore hold no value. Is that fair?

Secondly, if fairness is regarded as the basis for the argument then presumably any age could be set.³ A person aged 30 in competition for health resources with a person aged 40 could argue that the age for a fair innings should be set at 40 and that he or she should be given the chance of reaching it. The fairness of the fair innings argument is that each person should be given an equal chance of a reasonable length of life.

It is easy to see why the principles of rationing remain implicit and confused.

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3 Glover J. *Causing death and saving lives*. London: Penguin, 1990.

Two tier service will develop on basis of ability to pay

EDITOR—There is currently much debate over how and when rationing should be implemented in the NHS and who should do it. People talk of equity, opportunity costs, and beneficence, quoting esoteric ethical principles. All these points are discussed in an abstract way as if rationing affects anonymous groups; people forget that ultimately we have to deal with an individual person, as in the case of Child B. Nobody seems to have grasped that if rationing of health care is to be considered equitable then a suffering person must, at the time that rationing is applied to his or her condition, accept that it is fair. Yet all the emotions of a person who is ill are entirely focused on his or her condition. No person would agree to keep on suffering for the good of the community; that would require a degree of detachment from self that is available only to saints and suicide bombers.

Some time ago, when on a course in health economics, I posed a hypothetical question to the teachers: "Once rationing was established for any given condition would you tell your wife, who had that condition, that it was her duty to continue suffering from it for the good of the community?" Some did not answer the question. Only one teacher told me, "At that level I am not a health economist; I am a husband who will try his utmost to get the treatment that my wife requires." This is why German patients with kidney failure risk going to India to buy kidneys donated by poor people.

When there is rationing, either because of a shortage of donor organs or because of a lack of resources, people who can afford to do so will always be able to get treatment in the private sector. How does this equate with Alan Maynard's statement: "Let us assume that the purpose of the NHS is to improve the health of the population"?¹ What happens to the principle of equity when those members of the population who can afford to can improve their health but those who cannot afford to have to suffer in silence? This is how a two tier service in health care, based on ability to pay, develops.

Once downward pressure on the availability of health care is accepted and enforced on ethical grounds it will not be long before extra pressure is applied to reduce further services. Once this is allowed to happen the NHS will be the Nominal Health Service for those who cannot afford to buy private health care.

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Rationing may be good

EDITOR—One of my patients, a 70 year old woman with leukaemia, had the whole torture. She told me sadly that she thought that she had been given her final ghastly round of chemotherapy (when she was hardly more than a skeleton) only because the doctors did not want to risk being criticised for not doing enough after the massive publicity surrounding the case of Child B.¹

I constructed the thought experiment of supposing that the next round of treatment for Child B had cost only £750 instead of £75 000. I reasoned that, with the chances of benefit being so small and the disbenefits to a child's life being so great, the doctors would have explained their decision not to continue and the father would have accepted it because there would have been no perceived financial conflict. My wife (also a doctor) immediately pointed out that nearly all the hospital doctors she knew would go on to the bitter end unless halted by resource constraints, as indeed they did with the old woman. If nothing else, my thought experiment hints at the apparently ineluctable centrality of doctors' attitudes in the functioning of the system.

If, alternatively, the next round of treatment were to cost £7.5m and to have a

5% chance of "cure" then everybody would have settled for palliation. In addition, the validity and meaning of such percentage cure rates in new procedures might be more rigorously questioned.

Richard Smith laments the inevitability and ubiquity of rationing.² More recently, Alan Maynard has cast his gaze at the fuzzy thinking surrounding the problem.³ But rationing is good. Firstly, rationing is good in so far as it implies equity—for example, wartime food rationing. Secondly, in a world of J shaped curves, to ration your exposure to alcohol, dietary fat, physical inactivity, or sunshine is good because it's protective. Who doubts that there is a J shaped curve for medical care as well? Thirdly, many of the examples of rationing—for example, the case of Child B—are to do with experimental, unproved procedures, and these should be financed from a research budget. They cannot logically be "rationed" because there is no proved benefit. This point is made clearly in Bill New's article on behalf of the Rationing Agenda Group,⁴ and is reiterated by Maynard,³ but it is almost universally misunderstood.

The conflation of true service budgets (for procedures with proved benefits) with experimental activity, and the obfuscation of both with traditional clinical freedom, ensures that the reality is the dog's dinner that we see.

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Priority setting tools should be used to draw up principles of rationing

EDITOR—Alan Maynard's editorial on rationing health care makes bracing reading, but I do not agree with all he says.¹ I agree that the current range of priority setting tools, such as citizens' juries, are of limited use in a policy vacuum, but that does not mean, as he suggests, that they hinder policymaking. Perhaps they should have been used to draw up the principles of rationing in the first place.

Maynard gives two examples of what these principles could be: the "well years" and "fair innings" scenarios. These represent societal values which people may place on the NHS, and as such they have to emerge from some form of public consultation. I suggest that as well as using qualitative priority setting mechanisms as part of the solution to the rationing problem we also use them to define the problem in the first place.

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Certain minima must be attained

EDITOR—Alan Maynard seeks principles on which to base decisions about rationing.¹ Inevitably, several considerations will have to be balanced, but a civilised system should attain certain minima. For instance, people should not have to die in avoidable distress.

The term "fairness" should not be used, as its meaning will depend critically on the premises used. Rationing by age, severity of illness, or lifetime consumption of resources could all be said to be fair but would result in radically different distributions of care. The weakness of the "fair innings" argument in particular is that it ignores the importance of prognosis. For instance, for the money spent it will do far more good to replace hips in otherwise well 80 year olds than to try to postpone the inevitable for a short period by heroic treatments for advanced small cell lung cancer in younger people. It is true that by this argument it would be more rational to operate on people in their 60s than in their 80s, other things being equal.

Finally, the costs of inaction must be considered. While inpatient rehabilitation after stroke might well cost £10 000, if it saves £250 a week in nursing home fees (even allowing for the costs of domiciliary care) it becomes a cheaper option to the economy as a whole in less than a year. In stroke, as in other conditions, if people are not going to die it will often be cheaper to treat them properly than to neglect them.

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Angioplasty has been shown to have benefits over thrombolytic treatment

EDITOR—Susan Mayor's news item about angioplasty versus thrombolytic treatment in acute myocardial infarction gives a misleading view of the relative benefits of the two treatment strategies.¹ Although thrombolytic treatment remains the most easily and widely used method of attempting coronary artery reperfusion, controlled trials suggest that coronary angioplasty is more effective and offers major advantages in terms of morbidity and mortality. One cannot draw worthwhile conclusions on the relative benefit of the two treatments from a retrospective review of the data from the myocardial infarction triage and intervention registry because the selection of patients for treatment was probably biased.²

Thrombolytic treatment has limitations. Firstly, recanalisation fails in 20-30% of patients and only 54% of patients achieve normal coronary flow within 90 minutes. Secondly, 10-30% of patients in whom recanalisation is successful suffer reocclusion leading to reinfarction, worsening left ventricular function, and death. Thirdly, intracranial bleeding can occur in >1% of patients, causing death in 35-40%. More-

over, 15-20% of patients have some contraindication to thrombolytic treatment.

In contrast, primary angioplasty has a high initial success rate (90-99%) with few complications and low mortality. It can establish normal coronary flow in 95% of patients within two hours, and this has been our experience. Angioplasty is more effective because it mechanically disrupts thrombus and reduces any residual stenosis. These actions limit infarct size, improve left ventricular function, and reduce recurrent ischaemia and reocclusion and reinfarction rates. Pooled data from randomised trials show that primary angioplasty is associated with a significant reduction in the rate of death (odds ratio 0.56 (95% confidence interval 0.33 to 0.94)) and death or non-fatal reinfarction (0.53 (0.35 to 0.80)) compared with thrombolysis.³ These findings were supported by the global use of strategies to open occluded coronary arteries (GUSTO IIb) trial, in which the benefit might have been even greater if effective reperfusion had been achieved in >95% rather than only 80% of patients. Moreover, angioplasty avoids the risks of bleeding associated with thrombolytic drugs given systemically and can be used when such drugs are contraindicated. A further meta-analysis of data on 2611 randomised patients showed that angioplasty is superior to thrombolysis in terms of stroke (0.6% v 2.0%, $P=0.003$), haemorrhagic stroke (0.08% v 1.2%, $P=0.003$), death (4.5% v 7.1%, $P=0.006$), and death or non-fatal reinfarction (7.2% v 10.9%, $P=0.001$).⁴

Perhaps only when a randomised "megatrial" confirms the results of the smaller studies will the more important issues be debated—namely, whether facilities, funding, staff, and the infrastructure can and should be provided to enable angioplasty to be offered to patients with acute coronary artery occlusion.

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Fall in cholesterol after changes in composition of cooking oil in Mauritius

EDITOR—We have reservations about the methods, findings, and conclusions of Ulla Uusitalo and colleagues in their recent paper,¹ as well as those of its antecedent.²

On the purported fall in serum cholesterol concentration from 5.7 mmol/l

in 1987 to 4.6 mmol/l in 1992, it is not evident whether the samples were from the same subjects. Without a reference population the extent to which the changes documented represent true effects of intervention rather than underlying secular trends is not clear. The impact of the various interventional measures on serum lipid concentrations cannot be dismissed.

The assumption that there was no variation in diets or dietary intakes between 1987 and 1992 is unsubstantiated and is contrary to data provided for Mauritius by the Food and Agriculture Organisation.³ Without data on dietary intakes or fatty acid composition for 1987 it is alluded that there was no change in the cholesterol intake between 1987 and 1992. Only controlled human studies which reflect such data could have verified this. The data for 1992 on the per caput fat intake of 56.2 g per day based on a 24 hour dietary recall is a far cry from the 73.7 g reported by the Food and Agriculture Organisation.³

The figures for edible oil intake seem erroneous. In 1987 palm oil accounted for only 27.5% of the edible oils consumed and its saturated fatty acids contributed 1.89% of the total energy intake and this fell to 0.33% in 1992.^{3,4} Palm oil was used for blending only in certain months of the year. The paper makes no reference to other sources of saturated fatty acids in the diet including that of coconut oil and vegetable ghee containing *trans* fatty acids. To attribute the decline in serum cholesterol concentration by 0.79 mmol/l to alterations in the saturated fatty acids from palm oil is not consistent with current knowledge on the effects of palm oil on serum lipid concentrations.⁵ The observation of a reduction in energy from saturated fatty acids is based on assumptions and dietary recall data in 1992 and is not supported by food analysis data, which would have reflected the true situation.

Because of the weaknesses in measuring dietary intake data it is impossible to determine whether the reduction in serum cholesterol concentration accompanied by the various changes observed were related to the cooking oil or the fatty acids derived from other dietary sources.

Although we are also concerned about the need to reduce the prevalence of non-communicable diseases in Mauritius, we disagree on the kind of simplistic thinking and draconian measures advocated.

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- 1 Uusitalo U, Feskens EJM, Tuomilehto J, Dowse G, Haw U, Fareed D, *et al.* Fall in total cholesterol concentration over five years in association with changes in fatty acid composition of cooking oil in Mauritius: cross sectional survey. *BMJ* 1996;313:1044-6. (26 October)
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Controlling genital chlamydial infection

Figures are underestimates because men are not tested

EDITOR—Anne M Johnson and colleagues are right to say in their editorial on controlling genital chlamydial infection that screening, diagnosis, and treatment remain inconsistent.¹ Returns from genitourinary medicine clinics cannot give any useful estimate of the true incidence of chlamydial infection while so many clinics do not routinely screen male patients; the figure of 39 000 treated cases in England and Wales in 1995 must be far short of the true figure.

In some clinics screening for chlamydia is rationed because of cost. These clinics usually accept that men will not be given a chlamydia test because they are, in any case, going to receive antibiotic treatment effective against chlamydia. This includes men whose symptoms and signs, together with the presence of pus cells on urethral microscopy, are sufficient to indicate non-gonococcal urethritis and those who are given antibiotic treatment because their partners have proved chlamydia, or in conjunction with treatment for proved gonorrhoea.

Thus those men who would be most likely to have a positive result of a chlamydia test, while being adequately treated, will not be included as having a proved chlamydial infection in returns from genitourinary medicine clinics. This underreporting will lower the national figures.

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Integrated approach is needed

EDITOR—Jonathan D C Ross and colleagues report that the attendance rate at genitourinary medicine clinics of patients with genital infection with *Chlamydia trachomatis* diagnosed in general practice in Lothian is low (13%).¹ Genital chlamydial infection, however, is not just a problem in general practice: it is seen in family planning clinics, gynaecology outpatient clinics, antenatal clinics, and ophthalmology clinics (a recent study from the Netherlands showed that most adults with chlamydial conjunctivitis have concomitant genital chlamydial infection²). Unless patients with the infection identified in each of these settings are treated and their sexual contacts traced, a control programme for genital chlamydial infection will be ineffective.³ Effective surveillance of chlamydial infection at district level

Table 1 Proportion of patients who yielded positive result on testing for chlamydial infection, and proportion of those who attended genitourinary medicine clinic in Leicester, 1995. Figures are numbers (percentages)

Source	No of tests	Positive result	Patient seen at genitourinary medicine clinic
General practice	1286	79 (6)	29 (36)
Gynaecology	816	27 (3)	3 (10)
Family planning clinic	649	38 (6)	29 (76)
Eye department	203	22 (11)	7 (32)
Antenatal clinic/maternity	97	2 (2)	0
Other/unknown	103	5 (5)	1 (20)
Total	3154	173 (6)	69 (40)

therefore requires information about positivity rates and referrals to genitourinary medicine clinics from all other sources.

We have obtained figures for chlamydial infection diagnosed outside genitourinary medicine clinics for 1995 (table 1) from our local microbiology laboratory, which serves the whole of Leicestershire (population 927 000). The usual method of testing was by enzyme immunoassay. Confirmatory testing with another non-culture test (direct fluorescence antibody) of all samples that were positive on enzyme immunoassay was performed to increase the specificity of the test. Chlamydial infection diagnosed outside a genitourinary medicine clinic accounted for a quarter (173/729) of all positive results in 1995. Figures for the first half of 1996 for gynaecology showed a considerable increase in rates of referral of positive cases to genitourinary medicine clinics: from 3/27 (10%) to 15/47 (32%).

Our data for Leicestershire highlight two important points. Firstly, chlamydial infection diagnosed in settings other than genitourinary medicine clinics accounts for a sizeable proportion of all patients known to have chlamydial infection. Secondly, the attendance rate of such patients at genitourinary medicine clinics is not uniformly low. We attribute the high attendance rate of patients from family planning clinics (76%) to close collaboration with genitourinary medicine clinics in terms of treatment and follow up. Other family planning clinics with similar arrangements have also achieved high rates of referral.⁴

The challenge facing us in Leicestershire is to obtain uniformly high rates of referral to genitourinary medicine clinics of cases of chlamydial infection diagnosed elsewhere. The departments of genitourinary medicine and public health are currently working with gynaecology, family planning, and interested general practitioners to develop an integrated approach to the management and control of genital chlamydial infection.

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Allergic patients do not comply with doctors' advice to stop owning pets

EDITOR—Doctors often suggest lifestyle changes to their patients as part of their treatment programme. Such changes are particularly important in certain conditions—for example, the control of exposure to known allergens. Doctors know, however, that many patients fail to comply with their instructions. Reduced compliance can be expected when there are emotional consequences of making lifestyle changes. An interesting example of this involves pet ownership by people with allergies to substances such as animal dander.

During a larger study on the consequences of lifestyle on health I isolated 341 adults (mean age 38.4) who had been diagnosed as being allergic to dogs or cats. Each of those in the sample had been specifically advised by their doctor to stop sharing their living quarters with their pets. The proportion of people who complied with these instructions was extremely low: only 73 (21%) rid themselves of their pets or removed them from the inside of their homes. Such low compliance might be expected, since there is a large emotional investment in a pet, which may be viewed as equivalent to a family member. Perhaps a more striking finding results from analysis of data on a subset of 122 of these people, for whom allergy had been diagnosed sufficiently long ago that the animal they were living with at the time had died. In this group, despite the presence of allergies to their pet and the advice of their doctor, 86 (70%) had replaced the animal with a new dog or cat.

Apparently many people find pets sufficiently important to their lifestyle that they are willing to ignore both chronic allergic

symptoms and specific medical advice in order to continue living with them. The emotional gain from the companionship associated with owning a pet is clearly sufficient to offset the physical discomfort caused by continued allergic reactions. As Byron wrote in *Don Juan* of dog ownership (though it could also be endorsed by cat owners):

"Tis sweet to hear the watch-dog's honest bark
Bay deep-mouth'd welcome as we draw near
home;
'Tis sweet to know there is an eye will mark
Our coming, and look brighter when we come."

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Doctors maintain patients' dependence, whereas counsellors give them control

EDITOR—In her news item on counselling in general practice Alison Boulton quotes remarks by Dr Bonnie Sibbald about patient confidentiality and outcome.¹ These remarks exemplify the lack of understanding of many medical professionals about counselling and make me wonder whether primary care is the best place for counsellors to work.

Counselling and medicine are of different cultures. Medicine is expert centred, whereas counselling is client centred. Medical culture tends to maintain patients' dependence and childlike compliance by means of power and control, labelling, and treating, which often distance the patient from the expert. Medical culture often maintains mystique by using jargon and withholding information.² Counselling, on the other hand, seeks to give more power and control to clients by enabling them to make choices and decisions about their lives. It seeks to share information and insights so as to reduce dependency and increase autonomy; counsellors are constantly working towards their own redundancy.

Counselling is also a "special relationship,"³ which has tight boundaries of confidentiality between the counsellor and client and between the counsellor and his or her supervisor. The power for change lies in this safe relationship (E S Bordin, American Psychological Association, Washington, DC, September 1976), and, contrary to Sibbald's suggestions, the outcome is likely to be affected adversely, rather than enhanced, by breaches in the boundary of confidentiality.

My impression is that medical colleagues become anxious at the thought of patients becoming autonomous and thus less compliant and easy to control. The process of empowering clients through counselling threatens to rock the foundations of the medical power base, and I am not surprised that there is now evidence of a backlash. General practitioners should come clean about their motives. Interference in the relationship between counsellor and client is not really about patient benefit

and outcome; it is about reducing anxiety in doctors. But when the experts start to regard their patients as autonomous adults, instead of helpless children, they will benefit from the relief of not having to be a parent to everybody.

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Reduced risk of wheezing in children using feather pillows is confirmed

EDITOR—T J Kemp and colleagues' study of levels of mite allergen in pillows¹ was prompted by the unexpected findings of our case-control study in which feather pillows were associated with a reduced risk of severe childhood asthma.² The cases and controls in our analysis were drawn from a larger survey, and a different subsample of the survey respondents underwent skin prick testing with extracts of *Der p* I, mixed grass pollen, and cat fur, as described elsewhere.³ The subsample included groups at high and low risk of hay fever, as predicted from family size and structure.³ The questionnaire used in our case-control study was posted to the parents of the 727 children who were skin tested, and replies were received for 617 (85%). We present here the results for 523 children, whose parents denied having made alterations to the child's bedroom (as defined previously²).

Table 1 shows the proportion of children sleeping on a feather pillow by history of wheezing and reaction to *Der p* I on skin prick testing. Only 30 children with a weal <1 mm in diameter to *Der p* I had a visible weal (≥1 mm) to either grass pollen or cat fur, so the results are similar if categorised by any positive skin prick reaction.

Use of a feather pillow was similar among the 353 children with no history of wheezing and 73 children with inactive wheeze in 1991 but was significantly reduced among the 97 children with wheeze

in the past year in 1991. The odds ratio comparing all current wheezers to never wheezers, and feather pillows to non-feather pillows, was 0.46 (95% confidence interval 0.26 to 0.81) (P=0.006). When 26 more severe wheezers were excluded the odds ratio was 0.56 (0.30 to 1.04) (P=0.07). The inverse association of use of a feather pillow with current wheezing was of similar strength among children with and without skin prick reactions to *Der p* I.

These observations on essentially new data confirm our earlier findings of a reduced risk of wheezing, and particularly severe wheezing, in children using feather pillows.² Differences in nocturnal exposure to mite allergen, as proposed by Kemp and colleagues, are unlikely to explain the effect of pillows, which is of similar magnitude in children with and without sensitisation to house dust mite. The observation that levels of mite allergen are no higher in feather pillows than in synthetic substitutes, however, contributes to the equipoise necessary before a more rigorous evaluation of different types of pillow by controlled clinical trial is contemplated.

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Problems with guidelines

Hypertension guidelines should not reflect suboptimal practice

EDITOR—T J Scanlon and colleagues point out that blood pressures taken in normal practice may differ from those obtained in clinical trials.¹ Their evidence is based on the fact that clinicians in West Sussex used Korotkoff phase IV to measure diastolic blood pressure, while recommended guidelines are based on phase V and are therefore "artificially high." There has been controversy over whether to use Korotkoff phase IV or phase V since 1912, but current

recommendations based on sound epidemiological, therapeutic, and clinicopathological evidence strongly favour the use of phase V.²

Colleagues and I have recently published evidence³ (which was highlighted in an editorial by Peter Rubin⁴) that phase IV cannot be adequately reproduced between observers. Although our paper primarily concerned pregnancy (for which the use of phase IV has been recommended), we showed particularly poor reproducibility of phase IV in our non-pregnant controls. A range of clinicians was used to mimic clinical practice, and two blinded observers listened simultaneously to each measurement, noting both phase IV and phase V. Neither observer identified phase IV in 68% of 540 measurements, and both observers identified phase IV simultaneously in only 7% of subjects. In practice this means that the average difference between phase IV and phase V becomes negligible because in most patients those clinicians who say that they measure phase IV are likely to be measuring the same as those who use phase V. I suggest that the average difference between the diastolic blood pressures measured in West Sussex and those throughout England is due to a reason other than use of phase IV.

When the practice of medicine is sub-optimal, evidence based best practice should be used. There is good evidence for using phase V to detect diastolic blood pressure. Thus clinicians should change their practice; the guidelines should not reflect the practice of those clinicians who use Korotkoff phase IV.

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Clinical practice must be taken into account when guidelines are drawn up

EDITOR—T J Scanlon and colleagues point out problems that may arise after guidelines are drawn up.¹ Many methods of developing guidelines for clinical practice have been proposed.² These methods essentially differ according to whether the guidelines are based on expert opinions or on a literature review, or both. None of the methods is perfect, and their impact on clinical practice is controversial.^{3 4} One explanation for problems with the methods could be the gap between the conception of guidelines and action to draw them up, and between research trials and clinical practice. This gap could be reduced if practitioners' views and surveys of practice were included as guidelines were being developed. We have attempted to integrate clinical data and

Table 1 Number (percentage) of children who used feather pillows by history of wheezing and sensitisation to house dust mite*

Wheezing history (1991 questionnaire)†	Skin prick reaction (≥1 mm) to <i>Der p</i> I		Total
	No	Yes	
Never wheezed	92/250 (37)	35/103 (34)	127/353 (36)
Wheezed but not in past year	15/39 (38)	11/34 (32)	26/73 (36)
Wheezed in past year:			
Not severe	6/26 (23)	11/45 (24)	17/71 (24)
Severe‡	1/10 (10)	2/16 (13)	3/26 (12)

*Excluding children whose parents reported "alterations or special arrangements in the child's bedroom because of allergy, asthma, or other chest problems" and also (for comparability with reference 2) 42 children with wheeze reported in indoor environment questionnaire (1993) but not in original survey questionnaire (1991).

†See reference 2 for details of questionnaire and definitions of severe wheeze.

‡These 26 subjects were included in previous case-control study.²

practitioners into the process used to draw up the national guidelines on thyroid nodules, which were based on a combination of expert opinions and literature review.

To ensure that the recommendations were appropriate we studied practitioners' expectations and practice at the beginning of the process, using, firstly, a questionnaire about the attitudes of general practitioners and specialists (n=700); secondly, questions asked by a group of doctors working in private practice (n=120); and, thirdly, a debate among specialists at a conference on thyroid surgery. Finally, 152 general practitioners and specialists were recruited to test the accuracy and appropriateness of the recommendations. Each doctor had to make a prospective analysis when consulting patients with a thyroid nodule to establish whether the patients' situation had been taken into account, whether a recommendation on the subject existed, and the utility and applicability of the recommendation.

The analysis of the doctors' expectations and practice showed the questions to which practitioners needed a clear response and also highlighted differences between knowledge and practice. The test in which the guidelines were applied had marginal influence on their content, because practitioners had participated in the process of drawing them up, especially in work groups and a review group.

We believe that the development of national guidelines in medicine must take account of clinical practice as well as experimental studies, literature, and expert opinions. This approach also allows the guidelines to be adapted to the medical and sociocultural context of the country in which they are developed and used.

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Old, frail people are marginalised in Japan

EDITOR—Noritoshi Tanida's paper on the neglect of elderly people after the Hanshin earthquake reflects a much deeper problem in the provision of care for older people in Japan.¹ An international symposium on the care of older people, hosted in the cities of Kanazawa and Fukui last October by the Sasakawa Health Sciences Foundation,

showed the extent to which old, frail people are marginalised in Japan.

Traditional care of older people by family members consists largely of prolonged bedrest, with consequent dramatic functional loss, contractures, and the development of pressure sores when home care is insufficient. With high levels of employment and an increasing proportion of married women working out of the home, traditional methods of caring for frail older people are no longer sustainable, but the necessary infrastructure of community services required to support family care and avoid the promotion of disability do not yet exist. Pioneering efforts to identify bedbound older people show that the consequence is a high rate of institutionalisation of people whose disabilities are so great that rehabilitation at home is not practicable.

With the prediction that a quarter of the population will be aged 65 or over by 2010, Japan needs to develop community based services that will permit early intervention to reduce disability and maximise functional ability among older people. Although there are some examples of impressive primary care, as a whole the general practitioner workforce is ageing and small and is unable to adopt a proactive role. Community nursing is not seen as an attractive career option, and there is a considerable shortage of staff in many nursing disciplines. Recruitment of migrants to provide staff for community based care is not yet seen as a possible solution; the preferred solution is the development of voluntary organisations that will support older people in the community.

The development of primary care is now a priority in Japan, which is technologically advanced but socially underdeveloped. British general practitioners have many lessons to teach their Japanese colleagues.

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Treatment for trigeminal neuralgia

Pathophysiological mechanisms of trigeminal neuralgia need to be explored

EDITOR—R Walchenbach and J H C Voormolen highlight the difficulties inherent in choosing treatment for patients with trigeminal neuralgia and suggest a prospective randomised comparison between percutaneous ablation of the gasserian ganglion and microvascular decompression.¹ We believe that the problem is greater than simply the treatment of pain: it is our limited understanding of the pathophysiology of trigeminal neuralgia and the lack of a diagnostic gold standard.

Several hypotheses exist to explain the characteristics of the disorder, ranging from peripheral neural ectopic pacemakers to

central disinhibition.² Although unproved, they can be used to justify a variety of treatments, which usually result in excellent early improvement. Logically, any prospective randomised trial should therefore have an arm for pharmacological treatment and an arm for peripheral procedures (for example, cryotherapy), while the outcome measures should assess not only pain and its recurrence but also quality of life.

In our opinion, the authors err in their suggestion that surgery is warranted only after medical treatment has failed. We have shown that abnormal vascular contact can be detected in most patients with trigeminal neuralgia by use of a new technique, magnetic resonance tomography, and that in these cases microvascular decompression leads to excellent results.³ Why should people have to fail a drug trial to qualify for surgery that not only is safe and effective but also results in the restitution of pre-operative sensory status?⁴ Similarly, how would one justify disregarding evidence of vascular compression from magnetic resonance tomography, or deciding against performing this simple imaging, and proceeding to produce a lesion in the nerve that might well result in permanent dysaesthesia?⁵

Advances in research to unravel the aetiologies of trigeminal neuralgia have not resulted in better understanding of the genesis of pain. There are probably several pathophysiological mechanisms leading to different variants of trigeminal neuralgia and requiring different treatments. Good quality research is needed to explore these mechanisms and to develop reliable methods for diagnosis. There may be a case for a randomised controlled trial in the future, but we would expect it to be much more focused than a blunt comparison between two totally different modes of treatment. Meanwhile, patients with trigeminal neuralgia deserve an accurate and dispassionate explanation of the merits and drawbacks of all methods of treatment from the outset.

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Choice of procedures is wide

EDITOR—Although randomised trials are the cornerstone of evidence based medicine, I believe that R Walchenbach and J H C Voormolen are overenthusiastic in their desire

for such trials in trigeminal neuralgia.¹ I also think that they are looking at the treatment of this condition in a rather narrow way, for three reasons.

Firstly, when drug treatment fails to control trigeminal neuralgia it is not always necessary to resort to invasive treatments. In my practice the condition will settle with the addition of acupuncture in at least half of patients, to the extent that it may be possible to reduce their tablets while maintaining adequate control of the pain. Few treatments can be argued to be safer.

Secondly, the authors do not mention glycerol block of the gasserian ganglion.² This may not be the most popular procedure, but it is performed in several centres in Britain, including mine. It is quick to perform and requires no specialised equipment other than x ray screening. General anaesthesia is not necessary, and the mortality, so far as I am aware, is zero. The results are comparable to those of thermo-coagulation, and although some sensory loss may occur, it often recovers, and troublesome dysaesthesia is rare.

Thirdly, the various techniques available differ widely, ranging from percutaneous injection to neurosurgery. When patients are fully informed of the options most have considerable personal preferences for one particular treatment and would resist randomisation. It seems unlikely that randomised trials based on informed consent would be easy to carry out.

Although the pain of trigeminal neuralgia is severe, patients who have it are, in one way, fortunate among those with pain syndromes. The chances of those with the most severe pain obtaining good relief are over 80%, and the patients have a choice of different types of procedures with a relatively low incidence of troublesome side effects. Differences between the outcomes of these various techniques are probably small, even if they turn out to be significant. Do we really need to compare them any more precisely than that?

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Controlled long term study of all surgical options is being planned

EDITOR—I am glad that others are also now calling for randomised controlled trials in the surgical management of trigeminal neuralgia.¹ Thousands of surgical procedures have been reported,² but only a handful have been meticulously evaluated by independent observers, and only one of these studies has attempted to assess outcome after three different procedures from the patient's point of view.³ When I asked neurosurgeons to randomise patients to different procedures they refused for the same reason that R Walchenbach and J H C Voormolen give in their

editorial¹—that is, their belief that only their technique was valid.

On the basis of my experience with over 600 patients I would not randomise patients, but for very different reasons. The morbidity and mortality associated with posterior fossa surgery are very different from those associated with surgery at the level of the gasserian ganglion, and patients' perception of these risks varies. Some of my patients will take the risk that a procedure has a mortality of up to 1%, whereas others will not and are prepared to accept a numb face and a higher recurrence rate.² The only way forward, which a group of British neurosurgeons is currently considering, is for a controlled multicentre, prospective, longitudinal, long term study of all surgical options, with an independent observer. Strict diagnostic criteria will be used, and the level of pain and its effects will be objectively measured. Outcome measures will include recurrence rates, physical and psychological morbidity, and an assessment of the quality of life. For too long the views of patients with trigeminal neuralgia have been ignored, and it is time for them to be taken into account. Only then will patients be able to give well informed consent to surgical treatments for their pain.

It is also noteworthy that there is a scarcity of well designed randomised placebo controlled double blind trials in medical management. Newer antiepileptic drugs are being developed that may be of use in trigeminal neuralgia, and the first such trial since 1987, of lamotrigine, has been reported (JMZ *et al*, 8th world congress on pain, 1996).

I hope that evidence based medicine will find its advocates among the relatively small group of clinicians who are devoted to the care of patients with trigeminal neuralgia, which has devastating effects on the quality of the patients' lives.

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Editorial did not consider a multidisciplinary team approach

EDITOR—R Walchenbach and J H C Voormolen omit several points from their editorial on surgical treatment for trigeminal neuralgia.¹ In an elderly population the management of this disabling condition by central procedures alone carries an associated mortality (up to 1%²) and morbidity (11% of patients have eighth cranial nerve problems postoperatively²), and the authors seem to accept these. More acceptable options exist, which can be carried out under local analgesia³; these include cryotherapy⁴ and surgical neurectomy of the

peripheral branch of the trigeminal nerve affected. Although these peripheral procedures do not have the same long term cure rates as the central procedures that the authors mention, they are associated with negligible morbidity and mortality and are easily repeatable. Walchenbach and Voormolen, as neurosurgeons, discuss in their editorial only those techniques commonly performed for this condition by their specialty and do not consider a multidisciplinary team approach in conjunction with maxillofacial surgeons and pain specialists.

We agree with the authors' conclusions that the least invasive technique should have preference, but peripheral procedures are less invasive than either of the techniques that the authors discuss. It is important for a general medical population to be aware of all the options for treating this condition.³

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- 1 Walchenbach R, Voormolen JHC. Surgical treatment for trigeminal neuralgia. *BMJ* 1996;313:1027-8. (26 October.)
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Telemedicine

Pilot study of telephone follow up in rheumatology has just been completed

EDITOR—Having read the two recent articles on telemedicine,^{1 2} I wish to share my own experience. As there were no published accounts of outpatients' attitudes towards telephone follow up care, I undertook a survey among outpatients about a proposed telephone follow up service.^{3 4}

A detailed questionnaire was given to 275 patients attending various outpatient clinics at Withington Hospital. A random sample of 20 consultants providing services at the hospital were also asked to give their overall opinion. Altogether 201 questionnaires were returned (73% response rate). One hundred and sixty patients indicated that they would be interested in telephone follow up. The reasons given included that they would save time (80 patients) and expense (60), avoid the journey to the hospital (88), and require no time off work (120). Many patients (86) thought that the hospital would also save money. Over half the patients (108) said that they would not necessarily find it easier to ask the doctor questions over the telephone, and a similar number (104) expressed some anxiety that telephone follow up would not be as safe or reliable as attending the clinic. Several expressed concerns about security (62) or confidentiality (91). Most (178), however, had access to a telephone either at work or

at home, and most (160) generally welcomed this new concept as a useful development. There were no significant differences in any of the opinions between rheumatology and other outpatients. Of the 20 consultants, 17 considered telephone follow up to be a good concept and would welcome it.

Telephone follow up may be especially suitable in certain specialties, such as mine (rheumatology). I performed a pilot telephone follow up clinic throughout 1996. This involved discussing patients' progress, informing patients of the results of any new investigations or changes in management, and reinforcing lifestyle or other general advice. The content of each telephone interview was relayed to the general practitioner in a follow up letter, but a copy was also sent to the patient for clarification. I believe that this should allay fears about lack of written records in telemedicine, as expressed in the two recent articles. Now that my project has ended I intend to analyse my findings. Certainly, the hospital managers are interested in the concept of telemedicine and are considering it in our next business plan. I expect that there will be overall support from general practitioners, as indicated by the Primary Care Rheumatology Society.⁵

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Proved acceptable in pilot study in oncology in Scotland

EDITOR—Richard Wootton is rightly cautious about the benefits of telemedicine, because its application and evaluation in Britain is limited.¹ In Scotland telemedicine has potential attractions to accelerate the assessment of patients referred from geographically remote cancer units to cancer centres.

A consultant clinical oncologist in Edinburgh provides a weekly clinic for the assessment of new and follow up patients in Dumfries and Galloway Royal Infirmary, which is 120 km away. A pilot study of videoconferencing was carried out from August 1994 to February 1995. A videoconferencing unit (VCR 7000, British Telecom) was linked to a camera mounted on a gantry to enable radiographs and computed tomograms to be reviewed. Of 18 consultations carried out, four were with patients referred for assessment of palliative radiotherapy for lung cancer. The patients were accompanied by the respiratory physician or the oncology liaison sister. A decision was made on each patient on the basis of their symptoms and general medical condition.

An audit of both patients and staff found the videoconferencing consultation satisfac-

tory and simple. Telemedicine provided a more prompt assessment of these patients than could have been provided by a routine clinic appointment. Experience in the United States suggests that telemedicine in oncology can be cost effective.² Larger studies are warranted to assess the value of telemedicine in providing urgent oncological advice to remote hospitals to complement existing outpatient facilities.

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Value of magnetic resonance imaging of the breast as a screening tool remains uncertain

EDITOR—We welcome Minerva's enthusiasm for magnetic resonance imaging of the breast but believe that she is premature in her assertion that it is a sensitive and reliable screening test for breast cancer.¹ We do not think that her source article was meant to convey that impression.² Gadolinium enhanced magnetic resonance imaging has been proved to be more sensitive than mammography for detecting cancer only in highly selected groups of patients.³ There are as yet no large scale studies evaluating magnetic resonance imaging of the breast in asymptomatic women.

Furthermore, the sensitivity of magnetic resonance imaging of the breast leads to problems when lesions that are clinically, mammographically, and ultrasonically occult are detected. Biopsy devices guided by magnetic resonance imaging are not yet widely available, which limits the current value of such imaging as a screening tool.

Although magnetic resonance imaging does not require compression of the breast, it may be associated with adverse affects. In one survey of women attending for magnetic resonance imaging of the breast 4% described pain, 14% moderate or severe discomfort, and 25% moderate to severe anxiety during the examination.⁴ Specific adverse comments about the procedure included claustrophobia (11%), the noise of the scanner (26%), and the duration of the examination (17%).

Currently, magnetic resonance imaging is recognised to be a useful adjunct to conventional breast imaging in selected groups (for example, those with breast implants and those requiring assessment after surgery). Its role as a screening tool

should become clearer within the next few years. Minerva's interpretation of the present status of magnetic resonance imaging of the breast remains unfounded.

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Doctors who are dependent on alcohol can recover

EDITOR—In 1990 I reported the results of a survey I had carried out in 1988 into the recovery from alcohol dependence of 100 doctors.¹ At the time of the survey I had lost contact with two of the doctors. Contact has now been re-established, and both have sustained sobriety. Another nine doctors did not respond to a questionnaire and were excluded from the results of the survey. Further inquiry among these shows that one has sustained sobriety, which brings the recovery rate to 80%. The eight others did not stop drinking, and all have now died, the last death occurring in June 1996.

The message seems to be a simple one: accept help or die.

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