Letters

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JAMA's editor sacked

World medical journal editors should establish an award for editorial integrity in Lundberg's name

EDITOR—The sacking of Lundberg as *JAMA*'s editor[1] is a very sad event for editorial freedom in general and for medical journals world wide in particular. This disgraceful incident has shown that neither size nor success gives any guarantee for editorial freedom—not even the two combined. The fight for integrity must go on continuously in every editorial office every day. Lundberg has been an example for other editors through his editorial integrity, scientific judgment, and his personal strength and warmth.

I suggest that the global community of medical journal editors, through its organisations, establish an award for editorial integrity and name it the George D Lundberg Award.

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1 Tanne JH. JAMA's editor fired over sex article. *BMJ* 1999;318:213. (23 January.)

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World medical journal editors should draw up email protest petition

EDITOR—As every editor knows, editors sometimes time the publication of a piece of research so as to add new information to a current community or political debate, an upcoming major report, or an important anniversary. If editors are supposed to make a virtue out of being indifferent to topicality and relevant world events, heaven help medical publishing. Lundberg's "crime" was that he scheduled a study to be published that has information that many would find germane to one of the central issues in President Clinton's impeachment.[1] Among the many things to which Clinton's conduct and its sequelae have drawn world attention are issues about changing sexual standards. While many found it incredible that he played word games about whether fellatio was actually sex, a key finding in the new study shows that 59% of young people share the same opinion.

At a recent dinner with friends we were talking about our teenage children's sexuality. One parent who had recently hosted a teenage party had been advised by other parents to leave the garden lights on to discourage an epidemic of fellatio in the bushes. All those present who were aged 45 or more were stunned by the very thought that this is what young people are up to. But later when I discussed it with my doing-well-at-school, considerate, and civilised teenagers, they were all "Gee, Dad, you haven't a clue have you?" While I didn't sense that they themselves were up to such tricks, I sensed it was hardly exotic. I felt very old.

The pecksniffs at the American Medical Association, who may be motivated by Republican sentiments, want the medical community to accept that changing community standards are immaterial to the debate and that Lundberg could only have been exercising some naked political agenda. Numbered among *JAMA* readers would be many health researchers and clinicians interested in adolescent and sexual health. The idea that *JAMA* readers ought not to be subjected to papers mapping such social changes is plainly comical.

The timing of the publication, at a time when the whole world is being force fed this subject, is relevant and highly appropriate. I urge that an email petition of protest be drawn up among the world's medical journal editors.

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1 Tanne JH. JAMA's editor fired over sex article. *BMJ* 1999;318:213. (23 January.)

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Dark cloud of sanctimonious cliché settles on *BMJ*'s website

EDITOR—The outpouring of support for Lundberg on the *BMJ*'s website in response to his sacking[1] is gratifying in its promptness; mom and apple pie seem universally desired. It makes me particularly warm inside to see citizens of the former Soviet Union chastising their American counterparts (justifiably) on the issue of free speech. It's good that people care for journalists, but let's not forget that, so far, Lundberg hasn't been shot or jailed and that he was coming up to retirement age.

It is naive to imagine that publications exist for any reason other than to serve the interests of their owners. The editorial latitude allowed by tolerant Victorians such as the BMA merely reflects the security of its establishment and, perhaps, a belief that to do anything else would result in a publication of such stupendous tediousness that we all might as well go home.

JAMA's hideous typography meant that I for one would never read it for pleasure; but if Lundberg was such a great guy no doubt the American Medical Association will live to regret its decision as all his friends and allies in the office resign or strike in protest and America's doctors cancel their subscriptions in great numbers.

It's a useful maxim that you ought to try to do something every day that should get you fired. (Yes, this is today's attempt for me.) Lundberg has finally succeeded. Well done, George, and a happy retirement. Congratulations to *JAMA* for a hot start to the public relations campaign on the "paper that turned the impeachment hearings," and the best of luck with the redesign.

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The sacking of George Lundberg, *JAMA*'s editor, caused a strong reaction in the medical world. Within 10 days of the *BMJ* posting an editorial comment we received 67 rapid responses to our website: 55 of the respondents (82%) were outraged by the dismissal of Lundberg, about the half of this group were editors themselves. Only two responses approved AMA's action; five took a more neutral view, even though three of them disagreed with AMA's action. The remaining responses did not offer a judgment.

Teachers should aim to be stimulating rather than entertaining

EDITOR—Donald's article on "effective teaching"[1] left us with the feeling that this was an opportunity missed. To begin with the analogy of good teaching sessions being akin to parties gives the impression that the prime aim of teachers is to entertain whereas what we need is teachers who are trained to be effective and stimulating.

To illustrate a different approach, in our department all academic members of staff do a formal induction programme on how to teach.[2] This programme is planned with the staff concerned and is geared to their particular needs. Initially, they have a detailed analysis of their consulting skills as this gives many

relevant insights into their likely attitudes to, and behaviour in, teaching.

The teacher then learns how to encourage students to adopt a deep approach to learning by creating an environment in which, among others, the following characteristics are modelled and valued by the tutor: an intention to understand material for yourself, interacting vigorously and critically with content, relating ideas to previous knowledge or experience, using organising principles to integrate ideas, relating evidence to conclusions, and examining the logic of an argument.

The novice teacher progresses by first observing experienced teachers in action and then analysing video recordings of their seminars. In this way they learn how to recognise particular teaching strengths and weaknesses in other teachers and themselves and then how to overcome weaknesses and enhance strengths.

Eventually, the novice teacher progresses to teaching in concert with an experienced teacher, receiving feedback on his or her performance. Self assessment and reflection on the experience gained are encouraged as these are essential for continuing development. To assist the teacher training process we have developed assessment instruments.[3 4]

This induction programme ensures that new teachers have a repertoire of teaching skills from which they can choose the most appropriate for the particular teaching task. Their key aims are to ensure that students can see the relevance of their learning and that they can apply it effectively in practice. Kit Kats are gimmicks which will not succeed if the teacher's basic skills are inadequate.

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Clinicians and epidemiologists view crude death rates differently

Editor—We agree with Tunstall-Pedoe that crude death rates are misleading in comparisons.[1] However, clinicians and epidemiologists have different attitudes towards these health indicators.

Mortality from ischaemic heart disease has been low in Japan.[2] None the less, its recent trends were interpreted differently by clinicians and epidemiologists. On the basis of their experience, clinicians believed that mortality from ischaemic heart disease was on the rise. Trends in crude rates were compatible with their belief. Epidemiologists argued that to see the secular trend required age adjustment, which reduced the resultant rates. There was no simple answer about the validity of the two interpretations.[3] Which was true?

If age adjusted mortality is higher in one population than in another, discovering the cause is a concern of public health. However, age adjusted mortality differs from the crude mortality that directly reflects the real number of deaths because it is a hypothetical value. Crude rates may be used to estimate the extent of the needs for health services. The difference between these two indices may be seen in a community with a high proportion of elderly people and a low age adjusted mortality.

Mortality from ischaemic heart disease has been increasing in Japan, while age adjusted mortality has been decreasing. Therefore, the assumption that an individual is exposed to a higher risk of dying from the disease cannot be validated. On the other hand, the need for improving medical facilities to serve a growing patient population is real because the actual number of patients is increasing, as clinicians noted. A former president of the national cancer centre in Japan described his impression as a clinician: "Age adjusted mortality is overused; an estimated incidence is more important for health care planning."[4] In Japan the population in 1985 replaced that in 1935 as the reference for age adjustment in 1990. Because the aged population was small in 1935, the age adjusted mortality on this basis tends to be unrealistically small.[5] The age adjusted mortality may be misleading if it is used to plan for present and future health care because it differs noticeably from the actual situation.

Measuring the frequency of disease in an aged society is important not only for estimating health risks but for allocating health services. Both age adjusted and crude rates, with actual numbers, are important. With the collaboration of clinicians, epidemiologists must convey the meaning of these indices and use them as appropriate.

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Misusers may receive methadone from drug centres and GPs simultaneously

Editor—Heroin use continues to rise in England and Wales,[1] and the government has recently outlined steps to combat this increase.[2] The misuse of drugs (supply to addicts) regulations of 1997 revoked the requirement for all doctors to send to the Home Office details of drug addicts. Previously, the regulations of 1973 required that "any doctor who attends a person who the doctor considers or has reasonable grounds to suspect, is addicted to any of the 14 notifiable drugs shall, within seven days of the attendance, furnish in writing particulars of that person to the chief medical officer."[3] The index of addicts has now been closed, although doctors are expected to continue to report drug misusers and their treatment to the appropriate regional or national drug misuse database.

During June to July 1998 we became aware of several cases of simultaneous prescription of methadone and other misused drugs by both our centre and local general practitioners. This is worrying for three reasons. Firstly, it may increase the risk of methadone overdose, the subject of a recent study in Manchester.[4] Secondly, the opportunity for diversion of prescribed drugs to the illicit market is increased, thus undermining the harm minimisation strategy of prescribing drugs of misuse. Finally, it undermines effective management plans in both primary and secondary care and contributes to the prescribing problems highlighted in other studies.[5]

When the notification system was in place it was in principle a simple matter for any doctor to obtain information about a potential drug misuser's treatment. However, with the abolition of compulsory notification, information held in local drug misuse databases can no longer be said to be reliable and is usually inaccessible due to anonymous recording techniques. We are aware that staff at the local West Midlands database have noticed that some subjects have been entered by two different sources and may be receiving two or more prescriptions simultaneously. Currently, these staff have no way of alerting medical staff to their findings.

Ultimately, the risk of dual prescribing could lead to increasing vulnerability for general practitioners, who may be liable to criticism in the case of overdoses or accidental deaths from prescribed drugs. The ensuing reluctance to consider prescribing for drug misusers can lead only to pressure on

specialist drug services and so inhibit the government's desire to broaden the scope of medical management of drug problems.

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1 Harling R. Heroin use among young people is increasing in England and Wales. *BMJ* 1998;317:431. (15 August.)
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[Published as a rapid response on our website 26 August 1998]

BMA's gold medal award to the president of the South African Medical Association

Is this a travesty?

EDITOR—We were surprised and disquieted to hear that the BMA awarded its highest honour—the gold medal for distinguished merit—to Bernard Mandell, president of the South African Medical Association, at its annual general meeting last year. Apparently the award was made for Mandell's "distinguished contribution to the understanding of the relationship of medicine and human rights in the national and international spheres."

Mandell was chairman of the Medical Association of South Africa's federal council and board of trustees from 1987 to 1998, and from 1975 he was a member of the Federal Council, the highest decision making body of the organisation. For most of the time before 1990 the Medical Association of South Africa was notable in its slavish support of the apartheid government's policies and was condemned both locally and internationally for failing to speak out against human rights abuses in the health sector and in the country in general.[1–3]

From 1990, after the unbanning of the African National Congress, the Medical Association of South Africa discovered human rights and repositioned itself within the new order to maintain its influence and power. At the health sector hearings of the Truth and Reconciliation Commission in June 1997, the association begrudgingly admitted its complicity in applying apartheid policies, but many people believe that it has yet to come entirely clean on its activities during that time.[4] Furthermore, although Mandell may have played an important part in steering the association through its restructuring into the South African Medical Association, the new organisation has yet to show an active commitment to human rights in health.

The nature of Mandell's "distinguished contribution" to human rights in health is not at all clear. Indeed, the award becomes a travesty when so many who fought against apartheid in health care, often at great personal cost, remain unrecognised. We call on the BMA to set the record straight on Mandell's record at the helm of the Medical Association of South Africa and to bestow its award on someone more deserving.

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1 Rayner M. *Turning a blind eye? Medical accountability and the prevention of torture in South Africa.* Washington, DC: American Association for the Advancement of Science, 1987.
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Response of the BMA's secretary

EDITOR—There will be many who will be surprised to find in the pages of the *BMJ*, published by the BMA, a letter so heavily critical of the BMA. This underlines the freedom enjoyed by the editor of the *BMJ*.

I welcome the opportunity to respond vigorously on behalf of the BMA and, in particular, to reassure the South African Medical Association (SAMA) of our continuing support. The award signals a recognition of the part played by Mandell in ensuring that the medical profession in South Africa owned up to its past sins and the roots of those attitudes. The Medical Association of South Africa was open and explicit in accepting its share of blame before the Truth and Reconciliation Commission.

Its leaders, such as Mandell, willingly accepted considerable personal risk in this process. The BMA's message to doctors and to medical associations in *Medicine Betrayed*,[1] and in our forthcoming human rights report, is that owning up to past errors and trying to ensure that they are not repeated will earn the respect and trust of colleagues. The suggestion that Mandell and the new South African Medical Association are unforgiven will be a highly negative message to current transgressors.

Lewin and de Gruchy may sincerely believe what they are saying, but they are misguided and do not reflect the real processes of change in South Africa. The South African Medical Association represents the majority of doctors in the new South Africa. Refusing to accept the process of reconciliation and the courageous part played by people like Mandell in the process plays into the hands of reactionary forces still within the country.

In a letter to me of 12 November Mandell wrote: "In acclaiming the birth of SAMA and in acknowledging the tortuous and difficult route the medical profession in South Africa had to travel through towards the recognition of human rights and the protection of those rights, the award of the Gold Medal is an endorsement of the role that SAMA intends playing to prevent the abuse of those rights which its predecessor, the Medical Association of South Africa, certainly did not do."

The refusal to acknowledge that "tortuous and difficult route," as Lewin and de Gruchy would have us do, is the road to the dark days of the past, not the bright dawn of the future which we all in the BMA sincerely pray awaits our colleagues and the people of South Africa.

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1 British Medical Association Working Party on Torture. *Medicine betrayed: the participation of doctors in human rights abuses.* London: Zed Books, 1992.

More extensive analysis is needed when assessing facial structure in SIDS

EDITOR—In their short report on facial structure in infants who died of the sudden infant death syndrome Rees et al evaluated the role of retroposition of maxillae and mandibles in predisposing to narrowing and occlusion of the upper airway in the sudden infant death syndrome.[1]

Their hypothesis assumes that maxillary, mandibular, or bimaxillary retroposition reduces the nasopharyngeal airway. This may seem logical, but a range of congenital syndromes exhibit maxillary hypoplasia, such as Binder's syndrome (maxillonasal dysplasia), in which a reduced nasopharyngeal airway has been measured; patients with Binder's syndrome are not prone to obstructive apnoeas.[2] In adult obstructive sleep apnoea the patency of the nasopharyngeal airway is partly determined by environmental factors such as obesity, allergy, and infections and the distribution of submucosal fat in addition to facial form.

The authors' previous cephalometric work was based on four adults with obstructive sleep apnoea whose families had a history of the sudden infant death syndrome. This work suggests a common pathophysiology for obstructive sleep apnoea and sudden infant death syndrome through a familial tendency towards maxillary retroposition. As neonates are obligate nasal breathers, however, fundamental differences exist between the sudden infant death syndrome and obstructive sleep apnoea. Investigation of the effect of facial form by use of recognised bony and soft tissue lateral cephalometric landmarks would be valuable in the evaluation of nasopharyngeal airway patency.

Methodological errors may result from the use of lateral cephalography at necropsy; these include difficulty in ensuring a consistent mandibular position and the

subsequent unreliability of the recording of cephalometric landmarks, resulting in systematic and random methodological errors.[3] In the assessment of facial form, modern geometric morphometrics is more rigorous and takes into account both shape and size difference.[4]

Maxillary and mandibular retroposition may depend on deficient orthocephalisation of the cranial base.[5] The sella-nasion-subspinale and sella-nasion-supramentale angles are highly correlated, so that the finding of a reduction in one means that there is more likely to be a reduction in the other, as in this study. Rees et al's study suggests a lower sella-nasion-subspinale angle in the sudden infant death syndrome, although this finding must be interpreted with caution as the confidence intervals in the group who died of the syndrome and the control group overlap.

This paper indicates the need for more extensive analysis when testing the association between facial hard tissue morphology and the related soft tissues in the sudden infant death syndrome. Further study of craniofacial morphometrics and dysmorphology in the syndrome would benefit from collaboration.

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Alcohol consumption is not related to fertility in Italian women

Editor—Jensen et al observed decreased fecundability among women who drank alcohol compared with those who did not.[1] The decrease was found even among women reporting a weekly intake of five or fewer drinks. The authors call for further corroboration of their findings.

We analysed the relation between alcohol intake and difficulties in conception, using data collected on women in the control group of a case-control study of risk factors for spontaneous abortion.[2] The present analysis is based on 1769 women (median age 31 years, range 14-45) who gave birth on randomly selected days at the Clinica Luigi Mangiagalli (the largest obstetric hospital in Milan) and a network of obstetric departments in the greater Milan area. During their stay in hospital the women were interviewed by trained interviewers. Information was collected about general sociodemographic characteristics and habits (including lifetime alcohol drinking) and gynaecological and obstetric history. Difficulty in conception was defined as taking two or more years to conceive or receiving medical treatment for infertility.

Of the 1769 women interviewed, 135 (median age 32 years, range 22-43) reported difficulty in conception. These women were compared with the 1634 women reporting no such difficulty. We found no relation between alcohol drinking and risk of difficulty in conception. In comparison with never drinkers, the multivariate odds ratios for difficulty in conceiving after age, education, history of spontaneous abortion, and smoking were adjusted for were 0.9 (95% confidence interval 0.6 to 1.3) for women reporting fewer than two drinks a day while trying to conceive and 1.0 (0.6 to 1.7) for those reporting two or more drinks a day (table).

These data agree with the results of previous studies.[3 4] They should, however, be considered cautiously. In particular, we included only women who had difficulty in conception but had a successful pregnancy. Despite this limitation, these findings do not support an increased risk of difficulty in conception among moderate alcohol drinkers.

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Number (percentage) of women having difficulty in conception according to alcohol consumption, Milan, Italy

Alcohol consumption (drinks/day)*	Difficulty in conception		
	No	Yes	Odds ratio (95% CI)†
0	73 (54.1)	911 (55.8)	1‡
1-2	40 (29.6)	499 (30.5)	0.9 (0.6 to 1.3)
3	22 (16.3)	224 (13.7)	1.0 (0.6 to 1.7)

*Sum of glasses of wine, beer, or spirits drunk each day.
†Adjusted for age, education, history of spontaneous abortion, and smoking.
‡Reference group.

NHS breast screening programme

Both extended age range and reduced screening interval are needed

Editor—"The cure for breast cancer," said the American breast surgeon Susan Love, "is political action."[1] There could not have been a clearer illustration of this point than the recent papers about breast cancer screening.[2–4] Unfortunately, the authors do not estimate the cost of implementing both strategies for improving the breast screening service—that is, both extending the age range and reducing the screening interval to two years—but it seems that both policies will save lives and cost relatively little. Interval cancers tend to be faster growing and more life threatening,[5] and older women do respond to screening invitations.[4] The question should not be "Which is the best policy?"[3] but "How soon can we implement both policies?"

In Australia, where free screening is provided from age 50 upwards at two yearly intervals and the life time risk of breast cancer is 1 in 11, mortality is only 28% of the current rate of incidence. In the United Kingdom, which has a similar life time risk, mortality is still 43% despite recent improvements. Though other factors undoubtedly contribute to poor survival rates in the United Kingdom, from an international perspective it is not "early days"[2] as far as screening is concerned, and we should recognise that our screening programme needs updating.

The costs of health care cannot be seen in isolation from other social costs. Though breast screening may not be "the best way of obtaining health benefit per billion pounds,"[1] it is one way of saving the lives of women who not only deserve to survive in their own right but also, to take a purely utilitarian view, contribute crucially to our economy, either by doing paid work or as grandmothers offering free child care to working parents. "Retired" women also often look after elderly relatives and generally fill in the gaps in the health and social services by running hospital tea bars, Leagues of Friends, meals on wheels, youth clubs, support groups, and so on.

The UK Breast Cancer Coalition, representing people with breast cancer from all over the United Kingdom, campaigns for everyone to have access to state of the art treatment. The screening service is only the first step in bringing our breast cancer services up to internationally accepted standards. We owe it not only to

these women themselves but also to the many other people, young and old, who depend on them.

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Money may be better spent on symptomatic women

EDITOR—All the hype, promotional material, and leaflets inviting women to be screened carry the false promise of a 25% reduction in mortality from breast cancer. Boer et al explode this myth without explicitly stating so.[1] Their computer simulation study suggests that the current programme might achieve a 12.8% reduction in mortality, half that promised by the NHS breast screening programme when it was initiated. Even extending the age up to 69 or reducing the three year interval to two would come nowhere near matching the promises on which the whole infrastructure of this programme was based.

To extend the age to 69 or shorten the interval would cost an extra £10m a year. However, it is the human resources which are most precious. A recent report from the Royal College of Radiologists described the parlous state of the radiological support for the existing programme.[2] Morale is at an all time low, recruitment of radiologists to provide the current service cannot be sustained, and therefore any extension to the programme at present is completely impractical.

Yet to do nothing is not an option, as Werneke and McPherson point out.[3] However, they don't go far enough. We need to consider the resource implications and potential opportunity costs applying not only to an expansion of the programme but to the continuation of the programme as it is. Although no doubt politically unacceptable, serious consideration has to be given to dismantling the programme in favour of improving the services for women with symptoms or restricting invitations to women judged to be at high risk of developing the disease.

This problem is not going to go away, and to do nothing is to condone squandering scarce resources and perpetuate the subtle deception encrypted within the invitations for mammographic screening received by the innocent and trusting female population.[4]

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1 Boer R, de Koning H, Threlfall A, Warmerdam P, Street A, Friedman E, et al. Cost effectiveness of shortening screening interval or extending age range of NHS breast screening programme: computer simulation study. *BMJ* 1998;317:376-9. (8 August.)
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Growth rate is more important than size

EDITOR—Werneke and McPherson[1] comment that the model for cost effectiveness adopted by Boer et al[2] did not use a current population to make comparisons. We also note that the model required manipulation to fit the real incidence of small tumours in the North West region. It was fortunate that alteration of the time spent in the diagnostic window worked since the adjustment was based on a fallacy. The time that any tumour spends between two selected sizes depends on its rate of growth (which is exponential) and, within the small range under discussion, is almost certainly independent of size.

We have addressed these issues in relation to predicting the numbers of interval cancers expected in the NHS Breast Screening Service.[3] Our model was based on data from an unselected series of new, primary breast cancers and used the range of diameters at clinical presentation together with the distribution of rates of shrinkage in response to primary medical treatment. We assumed that shrinkage rates might be used as surrogates for growth rates. Our predictions closely matched the incidence of interval cancers reported in the North West.[4]

As well as baseline data on clinical and screen detection sizes, recognition of the range of growth rates in breast cancer is needed. Size alone is meaningless; tumour behaviour is most closely related to histological grade, which we have shown is related to rates of shrinkage in response to treatment.[5]

Boer et al also suggested that reducing the screening interval would gain more life years. Paradoxically, this may not be so. The current NHS screening programme favours detection of the more slowly growing tumours. Effective, but non-curative treatment will set back their metastases for lengthy periods while the more rapidly growing tumours become interval cancers. When interval cancers are included by shortening the time between screens, the delay imposed by their earlier treatment will be proportionately less.

Mortality figures are meaningless without some knowledge of tumour growth rate. Equally effective treatment will produce a long delay in regrowth in a well differentiated slowly growing tumour but a relatively short delay in a rapidly growing one. Although time is important to individuals, it is not a stand alone measurement of the effectiveness of treatment. Whether earlier diagnosis will eliminate metastasis remains problematical. The reductions in diameter at diagnosis so far achieved represent small proportions of tumour life span.

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1 Werneke U, McPherson K. Extending the benefits of breast cancer screening. *BMJ* 1998;317:360-1. (8 August.)
2 Boer R, de Koning H, Threlfall A, Warmerdam P, Street A, Friedman E, et al. Cost effectiveness of shortening screening interval or extending age range of NHS breast screening programme: computer simulation study. *BMJ* 1998;317:376-9. (8 August.)
3 Johnson AE, Shekhdar J. Interval cancers in the NHS Breast Screening Service. *Br J Radiol* 1995;68:862-9.
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Modelling is suspect, and results lack confidence intervals

EDITOR—Boer et al present results of a simulation comparing the cost effectiveness of different screening intervals in the national breast screening programme.[1] Their results should not pass without comment.

Firstly, their simulation model uses data taken from the Utrecht and Nijmegen programmes, which started in the mid-1970s. The relevance for breast screening in the United Kingdom in the late 1990s is unclear, given the considerable variation in basic screening variables between programmes, notably the interval cancer rates and screening detection rates of small invasive cancers.[2]

Secondly, their model shows a bad fit with the results of the second screening round in the north west region. It predicts that at the second screening test at three years over 60% of invasive cancers will be ≤ 10 mm in diameter and 9% >20 mm; the corresponding observed frequencies are 40% and 19%. The effect on predicted mortality of poorly modelling the stage distribution of cancers detected at the second or later screen is likely to be substantial, and even greater with a two year interval than a three year interval since cancers detected on screening are of relatively greater importance. Their estimates of the marginal effect of reducing the screening interval will then be unreliable to an unknown extent.

Thirdly, no uncertainty is attached to the various estimates. The authors claim that the marginal cost per life year gained of shortening the screening interval from three to two years is £3545—a spuriously precise figure. This estimate is likely to be highly misleading. With uncertainties over the data and the poor fit of the model, sensitivity analyses are essential. Alternative data could generate marginal costs several times greater than the quoted estimate. The *BMJ* usually insists on uncertainty estimates, often as confidence intervals. For an article intended to influence policy, omission of uncertainty bounds renders it almost valueless.

Future policy decisions for the national breast screening programme should be based on evidence directly related to the United Kingdom's programme itself. Such evidence will shortly be available from the results of the multicentre randomised trial of different

breast screening frequencies, undertaken in the United Kingdom under the auspices of the United Kingdom Coordinating Committee for Cancer Research.

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1 Boer R, de Koning H, Threlfall A, Warmerdam P, Street A, Friedman E, et al. Cost effectiveness of shortening screening interval or extending age range of NHS breast screening programme: computer simulation study. *BMJ* 1998;317:376-9. (8 August.)
2 Day N, McCann J, Camilleri-Ferrante C, Britton B, Hurst G, Cush S, et al. Monitoring interval cancers in breast screening programmes: the East Anglian experience. *J Med Screen* 1995;2:180-5.

Authors' reply

EDITOR—We agree with Day that policy for the national breast screening programme should be evidence based and should ideally be based on the results of randomised controlled trials. We await with interest the results of the trial being conducted in the United Kingdom. When our study was commissioned in 1995 the frequency trial had just started.[1] In the same year the first reports of unexpectedly high rates of third year interval cancers were published[2] and a House of Commons health committee urged the extension of the screening programme to women aged 65-69.

Day's letter indicates that our paper gave rise to some confusion about uncertainty estimates. The paper made clear that a sensitivity analysis was performed, and this is referenced. We did not provide confidence intervals around the final cost estimates because we believed that they would be misleading: much of the uncertainty in the model follows from choices about the data used, for which uncertainty cannot be readily quantified. We also made it clear that we could have used plausible alternative data that would have altered the final cost estimates. The emphasis, however, was on comparing the cost of two policy options, and it is therefore wrong to be unduly concerned about absolute values.

We acknowledge that the model is imperfect, and we draw readers' attention to its inability to fit accurately the stage distribution at repeat screens. But our conclusions are unlikely to be substantially affected, because the reduction in mortality from screening in the model is not derived solely from the modelled stage distribution; it uses the reduction in mortality observed in the Swedish randomised trials to attribute additional benefits to cancers detected by screening.[3]

The computer simulation package that we used is continually being refined as new data become available. Initially data from the Utrecht and Nijmegen screening programmes were used. The model is now based on more current data, including those from the Swedish randomised trials and the Dutch national screening programme. The NHS breast screening programme's experience was simulated with data from the north west health region as these were the most complete data in the United Kingdom at the time.

We maintain that modelling is the best available method to extrapolate current knowledge on breast cancer screening and that our model performs well (at least better than others) when answering questions about screening policy.

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1 Day N, McCann J, Camilleri-Ferrante C, Britton B, Hurst G, Cush S, et al. Monitoring interval cancers in breast screening programmes: the East Anglian experience. *J Med Screen* 1995;2:180-5.
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Some American driving licences double as organ donor cards

EDITOR—Davidson's idea of credit cards doubling as organ donor cards[1] is good but not new: for years "Organ donor = yes" has been stated on the back of my driving licence in Minnesota. American driving licences are small plastic cards just like credit cards. The system works, yet it is most regrettable that not more people give the affirmative yes to the question of organ donation when they are asked every few years at renewal of their licences.

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1 Davidson N. Credit cards could be used to indicate availability of cadaver organs for transplantation. *BMJ* 1998;317:478. (15 August.)

Opt out registers for organ donation have existed in Belgium since 1987

EDITOR—I'm surprised to learn from Dorozynski's news article that only one other European country, Portugal, keeps a list of people who have opted out of organ donation.[1] Belgium, which approved a law on presumed consent in 1986, was the first country in the world to establish in 1987 a computerised network—unique at that time—that allows citizens to register their objections against or explicit consent to donating their organs and tissues after death.[2]

When the non-donor registry was set up in 1987, a computerised network already linked all Belgian municipalities with the state registry of the ministry of internal affairs, containing, for example, all administrative data on each individual. Citizens can register their unwillingness to donate organs or tissue in their local town hall. A file extract containing only the individual's will about donation is downloaded daily into a smaller database at the health ministry's information centre. All procurement transplant coordinators in Belgium have individual, password protected access to this computer. Consultation of this database, to check whether a referred donor had stated unwillingness to donate during his or her lifetime, is mandatory before surgeons can proceed with the removal of any organ or tissue. At the end of 1995, the cumulative registered objections totalled 160 425 Belgians (1.75% of the native Belgian population) and 29 757 foreigners (3.23% of the foreigners living in Belgium).[3] Non-donor registries have also existed in Austria since 1 January 1995 and have recently been established in Sweden and the Netherlands.

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Rate of carotid endarterectomy in Wessex might already be higher than necessary

EDITOR—The epidemiological study from the Wessex region concludes that there is still an unmet need for carotid endarterectomy, particularly in elderly and female patients.[1] Central to the authors' argument is a claimed 20% prevalence of severe (70-99%) carotid stenosis in patients with carotid territory transient ischaemic attacks and a 25% prevalence after minor stroke. The cited reference describes a cohort of patients referred to a university department of neurosciences with transient ischaemic attacks or eye symptoms.[2] None had a stroke on presentation, but computed tomography showed cortical loss in a quarter of patients. The prevalence of severe carotid stenosis in those selected for angiography was only 15%, even though more than half had presented with either amaurosis fugax or retinal infarction.

Our own audit data for all patients having carotid duplex sonography shows prevalences of severe (70-99%) carotid stenosis of 6% for transient ischaemic attack, 7% after stroke, and 16% for amaurosis fugax.[3] The Manchester pilot study of carotid surgery in acute stroke found an 8% prevalence of severe carotid stenosis after cerebral infarcts in the carotid territory.[4] The recent final results of the European carotid

surgery trial show that there is no definite benefit from carotid endarterectomy in women of any age unless the carotid stenosis is greater than 85%.[5] We conclude that the rate of carotid endarterectomy in Wessex might already exceed that required to produce the confirmed benefits. In younger women there may be substantial overprovision of surgical prophylaxis against stroke.

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1 Ferris G, Roderick P, Smithies A, George S, Gabbay J, Couper N, et al. An epidemiological needs assessment of carotid endarterectomy in an English health region. Is the need being met? *BMJ* 1998;317:447-51. (15 August.)
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Link between breast cancer and ATM gene is strong

EDITOR—We welcome Lavin's recognition of the important role that the ataxia-telangiectasia (ATM) gene plays in breast cancer despite the attention given to two other genes, BRCA1/BRCA2, which are numerically less important.[1] It is important to emphasise that the evidence linking the ataxia-telangiectasia gene to breast cancer is more than suggestive. As well as the two smaller studies referenced by Lavin, two large epidemiological studies have clearly indicated the increased risk of breast cancer among blood relatives of patients with ataxia-telengiectasia.[2,3]

In 1996 a study using genotyping technology and the statistically powerful unbiased index test method for testing gene-disease associations provided compelling evidence that carriers of the ataxia-telangiectasia gene have a 3.8-fold increased risk of breast cancer compared with non-carriers.[4] With a P value of 0.0001, it is unlikely that this result arose by chance. More precise estimates of the magnitude of this risk will become available as more breast cancer cases in families with ataxia-telangiectasia are genotyped.

There is no conflict between these studies and the study by Fitzgerald et al,[5] as Lavin suggests. The apparent lack of association found by Fitzgerald et al is probably because they studied only women who developed breast cancer before the age of 40, based on a speculation by Easton that is not supported by any published data. Furthermore, women with BRCA1/BRCA2 mutations accounted for a substantial proportion (13%) of Fitzgerald's study sample. An unmatched comparison group was used; it is highly unlikely that the distribution of the most important factors that affect gene frequency—ethnicity and social class—in their blood donor "control group" resembled that in the group of breast cancer patients.

Such studies also have low statistical power when the population frequency of heterozygotes is about 1-2%. Additionally, they sought ATM mutations through the protein truncation test, which typically fails to detect about half of all such mutations.

In conclusion, the evidence supporting the association between mutations at the ataxia-telangiectasia locus and breast cancer is strong. No study with sound methods has refuted this association.

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Minerva should not review retrospective audits

EDITOR—The back page of the *BMJ* is one of the most widely read of the entire journal. I was therefore disappointed to read Minerva's review[1] of a paper in *Archives of Surgery* about the management of oesophageal cancer.[2] The statements that she made could not be justified on the basis of the results of this work.

The study in question was a retrospective audit of neoadjuvant chemoradiation for operable oesophageal cancer of various histologies. It was non-randomised, non-standardised radiotherapy doses were given, and data were not collected prospectively. Quality of life was not measured. The main conclusion was that median survival time was not improved by the chemoradiation when compared with results from other studies.

This report has major methodological flaws. The dangers of overinterpreting such results should be obvious to all who try to practise evidence based medicine, which relies on only best available evidence. This generally means prospective randomised trials, of which at least three exist; all show measureable benefits for neoadjuvant chemoradiation.[3–5] Clearly, further randomised studies need to be performed to determine the precise role of combined modality treatment in this disease, but such negative and misinformed reporting will not encourage recruitment.

Making dogmatic statements about end points that were not even measured (in this case quality of life) does a great disservice to the *BMJ*. I would implore Minerva to be more careful in future when reviewing retrospective audits, which clearly do not have the scientific robustness of randomised trials.

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Corrections

Resistance to antibiotics

An error occurred in this letter by Lindbaek and Hjortdahl (28 November 1998). The first full sentence on p 1522 should have read: "Only symptomatic treatment should be given during the first week of symptoms, as the disease in many cases is self limiting."

Elective caesarean on request

An error occurred in this cluster of letters (9 January, pp 120-2). The last two letters, by Adam Rosenthal and Richard J Howard, concentrate on the second part of the argument by Amu et al, not, as incorrectly stated, on the first part of the argument by Paterson-Brown. Both letters should have started "Amu et al," rather than "Paterson-Brown et al" as published.