Letters

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A practice that changed my patience

EDITOR—As a busy consultant paediatrician at a district general hospital on call for a bank holiday weekend I had cause to reflect on enjoyable times past and an uncertain future. The medical world is under permanent criticism by the media, much change has occurred in the structure of junior staffing, and my job is becoming much more just a job (as viewed by my employers) than the vocation I joined.

Despite their current media image and the open ended nature of their contracts, most NHS consultants provide a good service above their employment contracts. Current negotiation of a new contract means that a resident consultant on call is inevitable to provide senior care and maintain a safety net in the absence of sufficient middle grade staff.

On that weekend I started my on call duty at 9 am on Saturday and finished at 9 am on Tuesday, immediately followed by a normal busy Tuesday and home at 7 pm—a total continuous period of duty of 82 hours, to be followed by the remaining three days of a normal working week (106 hours in seven days). This work pattern is clearly illegal by junior doctor and European definitions. Although I am on call from home, even with a junior registrar (who can only do 24 hours at a stretch) I spent 23 of the 72 hours in the hospital (10 hours between 9 am and 5 pm, and 13 hours between 5 pm and 9 am) and took numerous phone calls for advice.

The following is a conservative estimate based on an average 10 hour working day and an on call frequency appropriate to each post. It does not take account of increased rates of on call to cover colleagues leave but does allow for my annual leave, not all of which has ever been taken.

I worked as a junior doctor for 13 years before applying to be appointed as a consultant in the NHS, clocking up a total of 57 552 hours, equivalent to an average working week of 92 hours. My 13 years of training were equivalent to 21.5 years on the basis of the current 56 hour maximum for juniors.

I have worked as a consultant for 12 years, a total of 42 228 hours. My total time after 25 years of service is 99 780 hours, equivalent to 38.7 years on the basis of a 56 hour working week and a 46 week working year (53 years on the basis of a 40 hour week).

I believe that I and most of my colleagues have done our bit for the NHS. I am willing to continue, but not if our professionalism continues to be ignored and certainly not if our lot is to be more back at the coal face than we are already. I wonder if many of my colleagues will be willing to work the above hours resident on call—I think not—and if we work a new contract of 40-48 hours we will be able to complete less than half of our current 9 am to 5 pm commitment (ratio of 9 am to 5 pm : 5 pm to 9 am = 1:2).

The goodwill of consultants is being rapidly lost; most of us are looking to early retirement (and would be gone now if we had our entitlement on the basis of hours rather than years worked). This sea change in morale has occurred over the past five years, occasioned by government policies, managerial attitudes, and college rearrangements of junior training.

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Health effects of prisons

Many injectors stop injecting while imprisoned

EDITOR—In their study of bloodborne viral infection in Irish prisons, Allwright et al found that infection with hepatitis C was associated with continued drug use by injecting in that setting.[1] A study by Stark in Germany has also confirmed this finding.[2] The authors of both studies have highlighted the discrepancy between the existence of well developed harm reduction programmes in the community, which include needle exchange and methadone maintenance, and the absence of such services in prisons.

I support the principle that imprisonment should not deprive an individual of access to services that are proved to reduce harm. Examination of the currently available research evidence, however, indicates that provision of needle exchange could possibly cause an increase in transmission of bloodborne viral infection in prisons. The findings reported by Allwright and Stark actually support this concern as they indicate that many injectors stop injecting while imprisoned.

In the Irish prison study, 51% of injecting drug users had not injected in the month before interview.[1] In the German study, 53% of injectors had never injected while in prison.[2] An Australian study, examining incidence of hepatitis C among prisoners, found that longer stay in prison (with no access to needle exchange) protected injectors against infection.[3] One plausible interpretation of this research evidence is the following: injectors who inject in prison tend to do so unsafely, but as so many injectors cease injecting during their sentence, the incidence of infection (and other adverse events such as accidental overdose) drops among the total population of imprisoned injectors.

There has been insufficient examination of the reasons why so many injectors cease or curtail injecting while in prison. There are many possible explanations for this finding, but the absence of available sterile injecting equipment could be an important factor. Although there is no evidence that provision of needle exchange encourages individuals to start injecting in the community, implementation of such a service could cause many more of these established injectors to opt to continue injecting while in prison. The introduction of needle exchange in prison could ultimately be shown to have a beneficial effect in reducing harm, but its introduction now would be premature while we have a poor understanding of the factors that mediate the observed reduction of injecting in this setting.

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Risks of syringe exchange programmes in prisons prevail

EDITOR—Since 1998, 203 366 prisoners in Bavaria have been tested for HIV when placed under detention; 1379 prisoners were diagnosed for the first time as being infected with HIV. During the course of their detention around 35 000 inmates have been tested, predominantly drug addicts; only one serum conversion has been found.

An inquiry last year by the doctors in the largest of the 37 Bavarian prisons (12 300 inmates) did not find any case of acute clinical hepatitis C during the course of detention. A survey in four prisons containing 3710 prisoners found that between 11.9% and 22.2% of all prisoners and between 61% and 75% of intravenous drug users were positive for antibodies to hepatitis C virus on entry to prison—lower than in the Irish prison survey.[1] In two prisons 213 prisoners were systematically examined on their release, and one case of serum conversion was found. Examination of the case files on 130 inmates at Nuremberg's prison who were positive for hepatitis C virus showed that two prisoners may have been infected during the course of their detention, one of them in a "blood brother" ritual.

Many studies show that drug users are most likely to become infected with hepatitis C virus at the beginning of their addiction.[2] In Germany, this phenomenon may clearly be seen among young immigrants of German background from parts of the former Soviet Union. Most of them have lived in Germany for only a few years. Having begun misusing drugs intravenously, they become infected with hepatitis C virus before their first prison sentence in an alarming number of cases.

Detention protects against infection according to the results of a study of serum conversion in Maryland.[3] Evaluation of a syringe exchange programme in a prison in Hamburg found that many prison inmates who had stopped misusing drugs started misusing them again. Also, many inmates went from inhaling drugs back to intravenous drug misuse while sharing needles regularly.[4] The decisive factor in the incidence of hepatitis C in prisons has been the availability of heroin. In Bavarian prisons a strict zero tolerance policy is followed in relation to drugs. Under these circumstances a syringe exchange programme would be misunderstood as accepting drugs. Prisons would be flooded with heroin immediately. The situation would be out of control and infection rates would rise considerably.

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Properly executed vaccination programme might minimise harm

Editor—Allwright et al have produced a highly commendable insight into bloodborne infections among prisoners in the Irish Republic, highlighting in particular the high prevalence of infection with hepatitis C virus in that group.[1] Data for England and Wales suggest a similarly high prevalence in the same population.[2]

In the United Kingdom it is recommended that prisoners be vaccinated against hepatitis B infection, particularly injecting drug users and people testing positive for hepatitis C virus.[3] With this in mind, there is a paucity of information in Allwright et al's paper relating to uptake of vaccination against hepatitis B virus among Irish inmates who are positive for hepatitis C virus and HIV.

In 1999 we carried out an unselected prospective study of a proportion (132/550 patients positive for hepatitis C virus) of the Sheffield hepatitis C virus cohort (M L Schmid et al, sixth meeting of the Federation of Infection Societies, Manchester, December 1999). Most of the 132 were injecting drug users or former injecting drug users (>80%), and a significant proportion of these had previously been incarcerated in prison (over 40% admitted to prison sentence). Serological testing showed 60% had no evidence of previous exposure to hepatitis B virus. Only 20% of the 132 had protective antibody levels against hepatitis B virus. Minimisation of harm should start with a properly executed vaccination programme targeting all prisoners, thus minimising the risk of acquiring or disseminating hepatitis B virus and reducing the risk of more aggressive liver disease.[4] Furthermore, vaccination for hepatitis A may also be worth considering for similar reasons.[4]

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Legitimacy of punishment systems should be addressed

Editor—Allwright et al are to be congratulated for obtaining and presenting further solid evidence of the unacceptable health effects of prisons.[1] They also emphasise in their closing statement what is well known—that imprisonment adds to the health risks of an already disadvantaged population. This evidence from Ireland extends similar earlier findings available relating to Scotland.[2] The *BMJ* has a good record of publishing studies describing the health damage wrought by European punishment systems,[1–4] including robust editorial comment by researchers on the lack of evidence based health protection measures in British prisons. But the journal does not go further to address editorially the legitimacy of these punishment systems from a health point of view.

It is salutary to contrast our silent assent to health damage caused by of our own punishment regimens with our willingness to criticise other cultures. A well reasoned piece from Médecins Sans Frontières described the difficulties in expressing dissent against the Sharia punishment system in Afghanistan.[5] Given findings that 21% of drug using prisoners started injecting in prison and a dose-response relation between time in prison and risk of hepatitis C infection,[1] can we really say that punishment systems in the British Isles are less barbaric than those that amputate a hand? The editor's choice article in the *BMJ* that accompanies the articles on Sharia punishment describes judicial amputation as a challenge to the ethics of humanitarian organisations, but the journal is silent on the ethics of judicial elevation of the risk of drug addiction and hepatitis C infection.

This highlights real challenges for the medical profession. Should the medical profession support widespread punishment by imprisonment in our society? Should the profession take the lead in conducting an assessment of the health impact of imprisonment? Also, given that many of the factors predicting poor health and other disadvantage also predict imprisonment, an assessment of the impact of health inequalities is needed. It seems safe to assume that no large political party will make this debate a priority in the near future. If the medical profession in the United Kingdom, and the *BMJ* as its most representative journal, has a duty to the health of the worst off in our society then they must take a lead in this area.

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Studies in meta-analysis of treatment of stable angina had methodological flaws

EDITOR—Bucher et al's meta-analysis of the treatment of stable angina with percutaneous transluminal coronary angioplasty or medical treatment is based on a small number of trials with methodological flaws.[1 2] The conclusions could have considerable adverse effects on the provision of revascularisation services, and we wish to draw attention to problems with the meta-analysis. Its results should not be used to guide clinical practice or decision making in public health.

Sievers et al recruited asymptomatic patients to randomisation between angioplasty and medical treatment.[3] The atorvastatin versus revascularisation treatment (AVERT) study recruited patients who were asymptomatic or had only minimal symptoms, many of whom had only moderate coronary lesions.[4] The medicine, angioplasty, or surgery study (MASS) recruited only patients with angina and a severe, very proximal, stenosis of the left anterior descending artery, who were randomised to surgery, angioplasty, or medical treatment, with an improvement in outcome after surgery.[5]

It is difficult to argue that the data of Sievers et al and the atorvastatin versus revascularisation treatment study have any relevance to the treatment of angina that limits lifestyle. The patients enrolled in the medicine, angioplasty, or surgery study represent a small subgroup of patients with angina who may be best treated with surgery.

The remaining three trials enrolled patients in the early 1990s. These studies were diverse in population size and inclusion criteria, had low recruitment rates from screening processes, and did not have comparable medical regimens. The operative technique used for angioplasty in these patients is now obsolete.

Bucher et al suggest that angioplasty may significantly increase myocardial infarction and death rates even though the stated confidence intervals are compatible with no adverse effect. Additionally, recent studies show that implantation of a coronary stent and use of an intravenous IIb/IIIa receptor blocker significantly reduce periprocedural complication rates and improve prognosis after angioplasty.

The methodological problems that we have outlined above preclude the use of the data from these six trials in a meta-analysis. A more appropriate approach would have been to discuss the data in a comprehensive review article. The data suggest that angioplasty improves quality of life in patients with stable angina, with a small (and diminishing) risk of a complication related to the procedure. To limit access to angioplasty because of the small risk of a complication implies that access to elective surgical procedures for non-life-threatening conditions (such as hernia repair or hip replacement) should also be restricted. These data support informed consent, not limited access.

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Management of women with early breast cancer

Affluence seems to affect management of breast cancer

EDITOR—Macleod et al in their article present a wealth of data on the management of women with early breast cancer from affluent and deprived areas in Glasgow.[1] It would have been informative to examine also a wider range of indicators of quality of care, such as those identified by the Clinical Outcomes Group[2] and the British Association of Surgical Oncology,[3] including access to specialist teams dealing with more than 100 new cases per year, access to diagnostic testing by triple assessment on the same day, and participation in clinical trials.

The lower rates of axillary sampling found in the deprived group may not be, as Macleod et al impute, solely an artefact due to unusual practice in a single hospital. In our work on monitoring the quality of care for breast cancer in North Thames health region,[4] we have found that surgeons use the terms "sampling" and "clearance" rather loosely when recording surgical procedures in the axilla. It is more informative to examine the number of nodes excised—poor practice being excision of too few nodes—to decide on the management of the patient. The comparison of the number of nodes sampled avoids possible bias due to association between hospital terminology and socioeconomic status of the patient.

Although the median wait from referral by the general practitioner to first visit to the clinic was only one day longer for deprived women, there was a distributional shift. Among deprived women, the 25% who waited longest waited 20 days or more. The corresponding figure in the affluent group was only 13 days. Like the one day difference in median waiting time, a one week difference in the 75th centile of waiting time may in itself be of limited relevance to the clinical outcome. If these differences are, however, indicative of other aspects of the quality of care, this may potentially explain part of the known socioeconomic gradient in survival.[5] Compared with the national standard of a maximum two week waiting time, 25% of affluent women, and some 35% of deprived women exceed this standard. Perhaps a closer look at these data might reveal that all is not as equitable as Macleod et al suggest?

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Definitions, outcomes, and analysis need clarifying

EDITOR—In their article on the management of early breast cancer in women with different socioeconomic status, Macleod et al raised some useful points, but we think that the situation is not as straightforward as suggested.[1]

The aim of the study outlined in the abstract was to investigate whether poorer survival of deprived women with breast cancer is related to NHS care. But survival estimates were not attempted, even though the cohort used was treated in 1992-3. We must also agree with Robinson that the outcome measures used to indicate quality of care are not necessarily the most relevant.[2] Waiting times from referral to treatment are perhaps less useful in the context of deprivation than delay in presentation to the general practitioner and delay in the decision to refer women with breast cancer, but this was not investigated. The study concentrated on differences in axillary surgery, yet nodal surgery is often inaccurately described as either sampling or clearance. No data are provided on the number of nodes taken, a better indicator of adequate surgery.

There was no definition of the size or stage of the early cancer group. If patients with locally advanced or metastatic cancer are included, analysis shows a statistically significant increased risk of being diagnosed with advanced cancer for those from deprived areas (relative risk 2.4; 95% confidence interval 1.2 to 4.7; $P = 0.006$).

Finally, the paper does not make clear whether screen detected cases were included

in the analysis. The inclusion of screened cases can mask differences in socioeconomic status with respect to breast cancer survival, and separating screened cases from symptomatic ones may show important differences between the deprived and affluent groups.

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New strategies are needed to address inequalities

EDITOR—With the increasing pressure of health service funding to focus on rewarding performance indices, the paper by Macleod et al is particularly salutary in its finding that the difference in outcome was not due to inequality of access to care.[1] Further support for this view is provided by comparing the outcome for breast and prostate cancer in east London and neighbouring areas from which patients are referred.

Previous reports from the Thames Cancer registry data showed a substantial difference in 5 year survival in patients with breast cancer treated in 1986-7 in east London compared with those treated in other areas of the Thames region.[2] Review of records held by the registry on patients with breast and prostate cancer treated by the Royal London and St Bartholomew's Hospital during the same period and comparison with the total Thames Registry data showed a similar disparity in survival (table).

Such differences do not preclude clinician related factors in outcome, but they make funding on the basis of outcome extremely inequitable. In the context of the current debate on reform of the NHS, there is clearly a need to identify strategies that address these inequalities. Whereas recent data provide doubt about the value of vitamin supplementation in well nourished people, some data show a bell shaped curve of effect, with the benefit of supplementation seen only in people with low values.[3] This highlights the need for more attention to be paid to developing assays of poor nutritional status that could be applied easily by general practitioners to patients attending for routine minor healthcare problems or in schools. Given the high incidence of tuberculosis in the same areas, these approaches could result in further gains. An alternative might be to re-establish population based supplementation such as providing milk and orange juice in schools regardless of whether health indices are poor because it might also benefit educational development.

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Authors' reply

EDITOR—We agree with Robinson et al that it would have been useful to examine a wider range of quality indicators. The design of our study, however, predated the publication of the two reports that they cite. As these indicators are now the basis of prospective audit being carried out throughout Scotland by the Scottish Cancer Therapy Network, it will be possible to relate such indicators to deprivation in future studies.

We examined the data regarding the use of "clearance" and "sampling" closely and found that in only one out of five Glasgow hospitals was the term "sampling" used to any significant extent. This led us to conclude that there was no evidence to suggest that the use of these terms or the procedure was influenced by the socioeconomic status of the patient. Pathology records in that particular hospital were also incomplete and did not always document the number of nodes.

Robinson's reference to the national standard of two weeks' waiting time again relates to current policy rather than practice at the time when participants in our study were managed. During 1992 and 1993 one Glasgow hospital had two new patient clinics each week while the other four had one. The hospital with two clinics serves an area in which there are several affluent post codes. That is why we argue that evidence for differences in the management of women from affluent and deprived areas resulting from by our data can all be explained by differences in hospital policy.

Williams and Fielder highlight the importance of data on patient delay before presentation. For our study we were not able to obtain these data but included delays occurring after the first presentation to the general practitioner. The size and stage of the early cancer group have been described elsewhere. No difference in pathological criteria was found between the women living in affluent and deprived areas.[1] Screened cases are included in this study, but analysis of these cases did not alter the final conclusions.

We agree with Oliver on the importance of new strategies being developed to address these inequalities. Our view is that better outcomes from breast cancer in women living in deprived areas are most likely to come from better understanding of the role of comorbidity and related host and environmental factors.

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Treatment of breast and prostate cancer at Royal Hospitals Trust, 1991. Values are percentages (base numbers)

	Breast cancer		Prostate cancer: Royal Hospitals Trust 2 year survival*
District of residence	Royal Hospitals Trust 2 year survival*	Thames Cancer Registry 1986-7, 5 year survival	
Inner east London	77 (163)	62 (83)	47 (29)
Outer east London	82 (151)	69 (83)	58 (28)
West London	87 (53)	72 (83)	25 (8)
Outside M25 motorway	90 (17)	83 (83)	75 (4)

*J Bell, personal communication.

Women's attitudes to false positive mammography results

A formerly clueless patient responds

EDITOR—I am a patient who received a diagnosis of low grade ductal carcinoma in situ in 1997, on my 43rd birthday, after obtaining a routine screening mammogram showing a cluster of indeterminate microcalcifications. Although I consider myself informed about women's health, I was ambushed by this news. Like the patients in the study by Schwartz et al,[1] I had never heard of ductal carcinoma in situ until it became a terrifying issue that put my life on hold.

Surveying the literature written for patients makes it easy to understand why someone like me could have missed this. I ransacked it, starting with the copy of *Our Bodies, Our Selves*[2] that I grabbed from my bookshelf on the day I came home to an ominous message on my answering machine from the radiology clinic. In the 30 pages about breast cancer, the only comment about suspicious mammograms was buried in a sidebar that had apparently been added in a recent revision and had no refer-

ring text. I learnt that most books and pamphlets written for patients assume that a woman's entry into the breast cancer culture starts with the discovery of a lump.

Many have long revision histories that predate the widespread use of mammography. Discussions of screen-detected disease are often meagre and carelessly patched in. On the day I received my diagnosis my surgeon dutifully educated me with a pamphlet entitled *Breast Lumps*.[3] It covered the normal breast, benign and malignant lumps, the simple procedure of self examination of breasts, and what happens after the discovery of a lump. Of course, little of this applied to me. I had what was finally described in a small inset on page 11 as an area of abnormality on a mammogram. The rest of the pamphlet contained a list of treatment options ranging from modified radical mastectomy to hormonal therapy. But this information did not help because it did not tell me which treatment was appropriate for my diagnosis.

My discussions with doctors were also an exercise in frustration. I was routinely told, often in the same appointment, that I have cancer and I do not have cancer. Perhaps the subtleties of ductal carcinoma in situ cannot be adequately conveyed in a typical 15 minute consultation, but the cryptic, garbled, and sometimes alarmist information that I got from my doctors was not good enough to make decisions about treatments or to make peace with myself. The only reliable source of information for me was the world wide web, where I located gateways to the medical literature and discovered that the message about ductal carcinoma in situ is far more hopeful and coherent than anything I had read in the literature for patients or heard from my doctors. Given the web's current state of chaos, this is a time consuming enterprise, but an overwhelmed and frightened patient is highly motivated, especially one whose professional research specialty is information retrieval on the web. There is a critical need for better patient education on this subject. The study by Schwartz et al study supports the conclusion that my experience is, unfortunately, a common one.

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1 Schwartz LM, Woloshin S, Sox HC, Fischhoff B, Gilbert Welch H. US women's attitudes to false positive mammography results and detection of ductal carcinoma in situ: cross sectional survey. *BMJ* 2000;320:1635-40. (17 June.)
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People in the United States may ignore harms of screening

Editor—I was excited to see the article by Schwartz et al, but I disagree with the authors in their belief that they have shown that support for breast cancer screening does not depend on unrealistic beliefs about the benefits of mammography.[1]

They gave the respondents a choice in completing the sentence "All things being equal, if this 60 year old woman got yearly mammograms for the next 10 years, she would have …" between the following answers: "A higher or unchanged chance of dying of breast cancer," or: "A lower chance of dying of breast cancer: By one fifth to one tenth—By one third—By a half—Reduced to zero." I think the question inadvertently gave away too much information about the range of possible correct answers. To this question only the most naive woman would answer that chances were reduced to nil; and no woman answered, "Reduced to zero," whereas 55% answered "Reduced by a half." Since promotions of screening in the United States have at times made it sound like the risks are thereby reduced to almost nil, I suspect that had respondents been given a choice of "reduced by 10%, 20%, up to 90%, 100%," many would have supplied a much higher guess than 50%, maybe as high as 90%, which would indeed point to a vast overestimation of benefit. I don't have evidence and wish the question had been asked differently.

Screening harms, including the risks of undergoing non-beneficial treatment, are a serious matter. The consensus (which carries almost moral force, sometimes arousing indignation if questioned) in the United States that harms of screening (for prostate cancer, breast cancer, or whatever) should be ignored and cannot be substantial, is extremely peculiar (even though I'm a native Texan) and worthy of inquiry; I would like to understand this cultural imperative better.

People really believe that screening and early treatment must be beneficial, I think more as a matter of logic than evidence. I would like to know if the public in other countries views this differently.

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Findings may not apply to United Kingdom

Editor—The paper by Schwartz et al was all the more interesting because of the availability of comments from peer reviewers.[1] They questioned the relevance of its findings to an international audience. Approaches to breast screening in the United States are different from those in, for example, the United Kingdom, so does this paper add anything useful?

Screening mammography in the United States is mainly recommended annually or biannually from the age of 40 years.[1] A United States website with a link to the American Cancer Society recommends regular screening from age 20 and a clinical breast examination every one to three years for those aged between 20 and 39. Those aged between 40 and 49 years should have a clinical breast examination every year; those aged 50 or older should have a mammogram and clinical breast examination every year.[2]

The British NHS breast screening programme is for women aged 50 years and over, who are offered screening by mammography every three years until they are 65 years old; they can then continue screening if they self refer. The website of the British Cancer Research Campaign mentions screening only in the context of the national screening programme, but explains how women can be "breast aware" and advises them to see their doctor if they notice any of the changes listed on the website.[3] The chief medical officer in England has written to general practitioners and others that there is no evidence to support the efficacy of breast examination by health professionals of the well woman and that palpation of the breast either by medical or by nursing staff should not be included as part of routine health screening for women.[4]

Given the very different approaches to screening, are the results of the paper by Schwartz et al relevant to the United Kingdom? Could the differences be influenced by the way in which health care is funded in these two countries?

Also, can conclusions be drawn for women in general when some groups were excluded from the study? The subjects of the study by Schwartz et al were wealthier and better educated than the general population in the United States, and almost all were white.[1] But general conclusions are made about what education is needed. This point applies to much research that is carried out. One way in which people are marginalised is that most research methods systematically exclude some groups as subjects, and so their needs or knowledge are not known.

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Authors' reply

Editor—We appreciate Godby's comments and wish her all the best. Good data about ductal carcinoma in situ are difficult to find, both for doctors and for patients. Since the natural history of ductal carcinoma in situ is not well understood, discussing prognosis or selecting the best options for treatment is extremely difficult. None the less, the available data are reassuring. Mortality from breast cancer for patients diagnosed with ductal carcinoma in situ is low. Moreover, in the 10 years after diagnosis, women with this disease actually had a lower risk of death

from any cause than women in the general population.[1]

We also appreciate Peticolas's criticism that our response options do not preclude the possibility that women have unrealistic expectations about the mortality benefit of screening. We wish we had used a broader set of responses. Writing good questions is challenging, and we have learned a valuable lesson. Our belief that women have generally realistic beliefs about mammography, however, comes from responses to several questions: our respondents knew that mammography misses some cancers; they accurately estimated the chance of experiencing a false positive result; and they appreciated that other behaviours such as not smoking conferred a much larger health benefit than screening.

Carter and Ghebrehewet are concerned that our results may not apply to poor, minority, or British women. Whether our findings can be generalised is an open question that we were careful to acknowledge. The take home message of our paper stands. The American women most likely to undergo screening appreciate and accept the risk of experiencing a false positive mammogram. On the other hand, they want more education about the possibility of being diagnosed with ductal carcinoma in situ and what such a diagnosis might mean. This last point warrants special emphasis. As new diagnostic methods increase our capacity for early detection, the issues (and questions) raised by ductal carcinoma in situ will become increasingly relevant to many cancers besides breast cancer.[2]

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Baby food industry lobbies WHO

WHO seems to be lobbying against World Health Assembly decisions

Editor—I am writing to offer clarification regarding comments made by Yamey in his news article on the baby food industry lobbying the World Health Organization on advice on breast feeding.[1] Dr David Nabarro of the WHO commented on the structured process of scientific analysis that the WHO is expected to pursue in drawing up its policy on the recommended age of exclusive breast feeding.

I am sure all of your readers would agree with this scientific approach to setting policy. It would, however, be interesting to ask Dr Nabarro why the WHO plans to spend its resources in conducting a literature review on this subject, given the fact that it has already had such a review carried out by globally respected experts in infant nutrition. The WHO published a thorough review of complementary feeding studies including the recommended length of exclusive breast feeding and the appropriate age of introduction of complementary foods in 1998.[2] The primary authors of this review conclude that full term infants should be exclusively breast fed until they are about six months of age. Is the WHO undertaking another review in the hopes that the conclusion will support the secretariat's policy of four to six months?

The second clarification is in response to James Akré's comment that a resolution by the World Health Assembly in 2001 would be a distraction from the cyclical mandate to go to the assembly every two years. A resolution on the nutrition of infants and young children was tabled at the 2000 assembly in response to the mandated WHO report on this issue. After a long debate the assembly requested the 2001 WHO executive board to establish a drafting group, open to all members of the organization, to prepare a resolution based on the one tabled, including proposed amendments. This resolution would then be recommended for adoption by the 2001 World Health Assembly. Mr Akré's comment is inappropriate, given the decision making process of the assembly.

The resolution and amendments tabled at the 2000 assembly deal not only with exclusive breast feeding but also with other important infant health issues, such as the implementation of the international code of marketing of breast milk substitutes. No doubt it is also for this reason that the baby food manufactures are lobbying against the resolution.

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2 World Health Organization. *Complementary feeding of young children in developing countries: a review of the scientific knowledge.* Geneva: WHO, 1998. (WHO/NUT 98.1.)

International network is impatient with those who disregard resolutions

Editor—Yamey's article gave insight into the food industry's tactics[1] but also suggests that the secretariat of the World Health Organization ignores decisions taken by the World Health Assembly, the highest health policy setting body in the world. According to article 18 of the WHO constitution the resolutions passed at the assembly, not WHO staff, determine WHO policy.

As a result of the incomplete and confusing messages issued by the WHO, Yamey's article is misleading in that it does not state that WHO policy, as set out in the World Health Assembly's resolution 47.5 passed in 1994, already recommends complementary feeding of infants from about 6 months**.**

More than 61 countries have adopted this age recommendation as national policy, and this year the Brazilian delegation proposed a resolution that reaffirmed this important health principle. The WHO's secretariat lobbied against the resolution and, in the event, member states referred it for further drafting and consideration for adoption at the 2001 assembly (World Health Assembly 53 agenda item 12.4).

Instead of respecting the wishes of the assembly, Mr James Akré, a technical officer at the WHO, describes the referral as a distraction from the WHO's cyclical mandate. The International Baby Food Action Network links 150 national bodies which monitor industry performance and see the harm that irresponsible marketing has on infant health. We can, as Dr David Nabarro, the WHO's executive director, says, sometimes be impatient with bureaucracies. This impatience, or frustration, is, however, not with scientific analysis (which has already established what the public health advice should be) but with those who disregard the resolutions. We have raised this issue again with the WHO in an effort to clarify its position and await a reply.

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Association supports scientific process

Editor—I refer to the article by Yamey about the baby food industry, which does not reflect reality and delivers information out of its complete context.[1] The International Association of Infant Food Manufacturers supports the scientific process launched by the World Health Organization concerning the nutrition of infants and young children, which is scheduled to be completed in 2002.

The association believes that January 2001 is too soon for the executive board to work on a resolution that raises the topic of the age of introduction of complementary foods because the scientific studies being undertaken by the WHO in this field will not yet have been completed. Precipitating decisions would result in a waste of time; it would also mean taking decisions without the scientific basis that the issue of feeding infants and young children needs and deserves.

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Distinguishing mental illness in primary care

Mental illness or mental distress?

EDITOR—In their editorial Middleton and Shaw set up a false dichotomy between mental illness, which in primary care should be treated with drugs and psychological therapy, and generalised distress, which needs to be treated with empathy, social support, and understanding.[1] Only generalised distress, they assert, represents a failure to respond adaptively to social challenge. If only it were that simple.

The 1995 survey of the Office of Population Censuses and Surveys and numerous surveys in primary care have used the general health questionnaire and the clinical interview schedule to detect mental disorders as they are defined by both the International Classification of Disease and the Diagnostic and Statistical Manual of the American Psychiatric Association.[2,3] But all such patients require empathy, support, and understanding, and most common mental disorders are at least in part reactive to social circumstances. The doctor must first detect that the patient is emotionally distressed and then respond appropriately. Not all those satisfying research criteria for a mental disorder will either wish to have, or benefit from, a medical treatment, but many will benefit from social support.[4] The statement that the best treatment for depression in primary care is antidepressants and drugs is true, provided that the depression is severe enough.[5]

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Nature of psychological illness in primary care needs to be defined

EDITOR—In their editorial Middleton and Shaw navigate between the poles of a debate that has vexed primary care and psychiatry for the past 25 years.[1] They cite Kessler et al as an extreme example of the modernist psychiatric stance that seeks to understand why general practitioners do not detect depression.[2] In counterpoint are studies suggesting that non-identification does not really matter anyway.[3]

To paraphrase the conclusion of the editorial: a greater effort should be made to define the comparatively small number of cases identified as positive through the general health questionnaire in primary care who have a "proper" psychiatric diagnosis so that they can be targeted for drugs and psychiatric interventions, whereas the ill defined majority can be left to muddle through with counsellors and well meant social interventions orchestrated by their vaguely skilled, empathic general practitioners.

There is a need to view psychological distress in primary care in new ways, but I suggest a more polemical approach. This should begin with the dismantling of cherished beliefs about the general health questionnaire.[4] First is the erroneous equation of being a case with depression or anxiety (caseness), or indeed any formal diagnosis. This is acknowledged in the editorial. Up to a quarter of cases will be depressed as defined by psychiatrists using standardised interview schedules, and half of all cases have no definable condition by these criteria.[4] These figures depend on the target population and the threshold used. The threshold is a tradeoff between specificity and sensitivity and commonly produces proportions of cases in primary care samples of up to 50%. Studies rarely conduct their own validation exercise but use thresholds cited by other authors working in similar settings. Caseness by the general health questionnaire is therefore not a diagnosis but a useful construct for psychological distress developed from and validated against dimensional models of psychiatric illness. Furthermore, the prevalence of caseness by the general health questionnaire is as much a function of mathematics as psychopathology.

The editorial's call to evaluate the problems of the many "mentally ill" (sic) in primary care by considering their problems in greater detail and to identify specific disorders corresponds with prevalent psychiatric models of diagnosis. This may be at the expense of models used by most general practitioners who have different relationships with their patients and a different perspective on their distress.[5]

The editorial moves us towards a debate about the nature of psychological illness in primary care. We should, however, resist the predilection of psychiatric tradition to define and categorise as this is potentially at the expense of those who remain "undiagnosed." This group includes devalued health professionals whose task it will be to "detect" while being left to get on with the "leftovers."

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1 Middleton H, Shaw I. Distinguishing mental illness in primary care. *BMJ* 2000;1420-1. (27 May.)
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Psychiatric classification is not a problem

EDITOR—Middleton and Shaw in their editorial have raised some important issues in the debate about common mental disorder.[1] We are repeatedly told that this is a common condition that requires greater recognition and effective treatment. Undoubtedly so, but what common mental disorder? Reported prevalence figures are high, and most of this is undifferentiated anxiety and depression.

In a small study in primary care in Merthyr we found the prevalence of cases defined by the clinical interview schedule-R to be 45%. What other medical condition or group of conditions occurs with this frequency? If this is taken at face value, as representing a condition requiring medical or at least clinical intervention, this is bound to overwhelm current primary health care and leave general practitioners dispirited, exhausted, and even depressed. General practitioners are generally much better at recognising the absence of common mental disorder than its presence but are repeatedly criticised for their failure at recognition.

In our study we found that four case finding instruments were equally effective at identifying cases as the clinical interview schedule-R. These were the general health questionnaire,[2] the mental health index,[3] the self report questionnaire,[4] and the Shona symptom questionnaire, an indigenous Zimbabwean instrument.[5] The most striking feature was the dissimilarity in the structure of these instruments. There were few common items, and the most frequently reported symptom in South Wales was "thinking too much," a translation of a Zimbabwean expression, *kufungisisa*.

How do such different instruments perform so well? Do they register a universal experience of distress, which in turn has a strong but not inevitable association with common mental disorder and treatable mental disorder? As suggested by Middleton and Shaw I believe that we have confused presentation and cause. Many people may have significant emotional or psychological distress, which may well be accurately captured and characterised by formal instruments, but not all apparent common mental disorder will be amenable to clinical intervention.

We do our patients a disservice if we restrict their distress to a clinical condition. Personal, relational, social, economic, political, and spiritual factors have significant impact on people's circumstances, feelings, and future development. Medical assessment often effectively minimises or dismisses these issues, to the detriment of ourselves and our patients. The medicalisation of modern society has already deskilled many other professions and led patients into an unhealthy dependence on medical care. We should be careful not to reinforce this further.

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Definitions are not facts

EDITOR—Middleton and Shaw in their editorial indicate how important it is to develop not only a consistent and reproducible classification of disease, but also to produce a system whose terms correspond to experience and reality.[1] Kendell pointed out more than 10 years ago that although the new consensus "flora" of the Diagnostic and Statistical Manual and the International Classification of Diseases were suitable for attempts to achieve some homogeneity, and hence comparability, in patient groupings for research purposes, they did nothing to establish the validity of the entities so defined.[2]

Various taxonomic techniques such as cluster or discriminant function analysis failed to confirm such groupings as distinct sets in unselected populations. Nor did the syndromes seem to segregate in the manner prescribed when studied for inheritance or response to treatment. These unpalatable truths have not been challenged or disproved but have certainly been ignored, despite or perhaps because of their importance. The situation is now worse as the criteria provided by the Diagnostic and Statistical Manual and the International Classification of Diseases have achieved the status of holy writ: if a patient can tick off the right collection of symptoms to fit a "diagnosis" then that is what he or she "has." The disclaimers in the prefaces to these volumes about the difficulties of distinguishing between normal and pathological states, or between the latter, are seldom quoted. Having a diagnostic label confers several very acceptable short term benefits, to both the patient and the therapist. The sick role excuses the sufferer of any responsibility for their plight, or the need to do anything about it. The therapist achieves power and control.

Ivan Illich accused the therapists of becoming disabling professionals[3] but now the public also should also be arraigned for being too ready to adopt the sick role authorised by the expert's diagnosis of "disease." What A N Whitehead in *Adventures of Ideas* (1933) called the fallacy of misplaced concreteness has beset the understanding and management of distress and discontent. Shakespeare in *Hamlet* (act II, scene 2) and *Macbeth* recognised that it was difficult to define true madness: we, too, should be more cautious in our tendencies to medicalise, say, unhappiness as "depression."

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2 Kendell RE. Clinical validity. *Psychol Med* 1989;19:45-55.
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Authors' reply

EDITOR—It is a pleasure to be invited to respond to such largely agreeable comments. The editorial was intended primarily to develop out of the work of Sir David Goldberg et al, and it is good to read his comments. It is certainly true that the general health questionnaire and the clinical interview schedule are effective in detecting mental disorders as defined by both the International Classification of Disease and the Diagnostic and Statistical Manual. It is also true that not all of those who satisfy such research criteria for a mental disorder will either wish to have or benefit from a medical treatment, but many will benefit from social support. Our intent is to draw attention to the controversial value of ascribing the status of "illness" to distressed people whose condition might fulfil the research criteria for a "subthreshold" mental disorder, but is not one where an evidence based medical or psychological treatment is clearly indicated.

If Earnshaw has gained the impression that our intention was to leave the ill defined majority of psychologically distressed patients presenting in primary care to muddle through with counsellors and well meant social interventions orchestrated by their vaguely skilled, emphatic general practitioners, then we apologise. Our true intention was to emphasise the fact that the needs of these many people are primarily for appropriate social support of one sort or another. Greater emphasis should be put on finding ways of providing it, rather than the present arrangements, which are strongly conducive of a medical style of response. We advocate research that will identify more clearly the social problems and needs that bring such patients to primary care settings and services that can provide for them more appropriately. This acknowledges that social needs and difficulties are a primary cause of concern among these patients, and not merely the unfortunate consequences of an illness. Winston apparently has the same thoughts in mind when he writes that we do our patients a disservice if we restrict their distress to a clinical condition.

Hall refers to Ivan Illich and in doing so indirectly to medical hegemony. We hesitate to expand on this further in the pages of the *BMJ*. It might, however, be worth drawing attention to the interests of the pharmaceutical industry in maintaining as broad a definition of pharmacologically treatable mental disorder as possible, and the power and efficacy of their related publicity.

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US has placed tobacco imports to China high on priority list for liberalisation

EDITOR—The *BMJ* issue on smoking (5 August 2000) is a wonderful resource for teaching about the smoking epidemic and how doctors can work to treat their patients who smoke. Several articles show that control requires wider social involvement, not just medical efforts, and mention that the president and surgeon general in the United States are assisting in these measures.

Yet in the same week that the issue was published, a news report described how the United States secretary of state for agriculture welcomed the opening of the Chinese market for American tobacco[1] after negotiations by the World Trade Organisation between the United States and China.

The effects of this will be massive. Yang et al have shown how smoking is increasing in China,[2] and the widely advertised prestige brands are leading the way. The effects of economic imperialism, pushing the tobacco

Correction

Drug use has declined among teenagers in United Kingdom

The data for use of heroin among girls and among boys shown in the two tables in this letter by Martin Plant and Patrick Miller (3 June, pp 1536-7) were wrong. The correct data are shown in the table below.

Proportions (percentages; 95% confidence intervals) of girls and boys aged 15 and 16 using heroin. Percentages for all regions are weighted

	England	Northern Ireland	Scotland	Wales	All regions in 1999	All regions in 1995
Girls	8/319 (2.5; 0.8 to 4.2)	10/408 (2.5; 0.9 to 4.1)	12/519 (2.3; 0.5 to 4.1)	3/107 (2.8; 0 to 5.7)	33/1353 (2.5; 1.2 to 3.8)	61/4007 (1.5; 1.0 to 2.0)
Boys	10/334 (3.0; 1.3 to 4.7)	7/309 (2.3; 0.4 to 4.2)	15/508 (3.0; 1.2 to 4.7)	2/121 (1.7; 0 to 3.8)	34/1272 (2.9; 1.5 to 4.3)	62/3566 (1.7; 1.2 to 2.2)

that is increasingly being rejected in the West, will be far greater than those of the opium wars. While "trade restrictions may impose other costs"[3] it is hypocritical for the United States to put tobacco high on the list of trade items.

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GPs should take hold of mental health agenda

EDITOR—The national service framework for mental health was published 12 months ago.[1] It acknowledged the extent of the primary care contribution to the mental health of the nation. How has the framework been implemented from a primary care perspective?

The local implementation teams have typically been constituted around the boundaries of health authorities. This has made effective engagement of these teams with primary care groups, and their mental health leads, difficult. Furthermore, the prominence accorded to primary care has been slipping. The Department of Health has prioritised three "must do's" for local implementation teams: assertive outreach, 24 hour access for patients on the care programme approach, and secure places. Important issues within secondary care services are targeted, but the central concerns of primary care, which has no responsibility for their delivery, are not addressed.

The NHS plan seems to re-emphasise the idea that mental health services are different from other community health and social care services.[2] Mental health services are given the option of being provided within combined mental health and social care trusts. There are real dangers in primary care becoming further distanced from secondary care mental health services—including the danger of limited future investment—if these services are routinely excluded from care trusts. Constant structural changes to primary care agencies, and the challenge of implementing other national service frameworks, have also muted the impact of the mental health framework.

The importance of effective mental health services to general practitioners and their colleagues, however, suggests that they take hold of the agenda. There are several ways in which the Department of Health could enable them to do this.

Firstly, the mental health modernisation fund could provide resources and support for mental health leaders in primary care groups and trusts. Furthermore, their role could be more formally defined with the structures of primary care organisations, and include membership of the local implementation team.

Secondly, the new modernisation action team for mental health might reflect on those aspects of the national service framework for coronary heart disease that have facilitated its adoption by primary care.

Thirdly, care trusts should be endorsed by the Department of Health as an appropriate model for the commissioning and providing of local services in mental health and social care.

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2 Department of Health. *The NHS plan: a plan for investment; a plan for reform*. London: DoH, 2000.

Hospital ethics committees may discourage staff from making own decisions

EDITOR—I am sceptical about the value of clinical ethics committees.[1]

Firstly, having a hospital ethicist or ethics committee can allow the ward staff to let others take the responsibility for hard decisions: "The ethicists said it was OK. What do you want of me?"

Secondly, the existence of ethicists and their committees can be an excuse for ward staff not to read biomedical ethics literature themselves and not to think deeply about bioethical questions.

Thirdly, ethicists and members of ethics committees who are hired or appointed by hospitals, national health services, or sick fund managements may naturally be selected to serve the interests of management. This can range from not interfering with experimentation that management wants to pursue to having liberal "do not resuscitate" policies to save resources.

Fourthly, there is no reason why every hospital in a country, or every ward in a hospital, should have the same policy about non-resuscitation and other ethically sensitive issues. Life and death are too complicated for ethical uniformity, let alone universalisation. Hospital ethicists and ethics committees tend to push for uniform policies for an entire hospital, discouraging creative, sensitive case by case thinking by the ward staff concerned.

As a philosopher-bioethicist (not a doctor) in a medical school, I deplore efforts by my colleagues to encourage the existence of clinical ethics committees and hospital clinical ethicists. Instead I think our function should be educational, helping present and future doctors and nurses to learn to think deeply, systematically, and for themselves about life, death, and their ethics. Rather than passing the responsibility on to others, they should make ethics decisions ward decisions: to be made not by individuals but in ward staff meetings that include the nurses and social workers—and the patient and family whenever possible.

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1 Slowther A-M, Hope T. Clinical ethics committees. *BMJ* 2000;321:649-50. (16 September.)

Can heart failure be diagnosed in primary care?

Chest radiography is still useful

EDITOR—In his editorial on diagnosing heart failure in primary care Hobbs fails to mention the value of chest radiography.[1] As every medical student knows, a chest *x* ray film provides information on cardiac size and the major cardiac structures, and more fundamentally on any respiratory cause of breathlessness. It is even more extraordinary that Caruana et al do not mention chest radiography in the diagnostic process in their paper, although their study focused particularly on excluding other causes of the patients' symptoms.[2]

The current obsession with open access echocardiography is tending to unnerve general practitioners, who feel that they cannot now diagnose heart failure without showing abnormal left ventricular function. In fact, in most patients the disease can be diagnosed and treated with a knowledge of the history, clinical findings, chest *x* ray film, and electrocardiography.[3]

Experience of open access echocardiography at this centre over two years was that it added little to the diagnostic process for those in primary care, except in patients who had a heart murmur.[4] Given the rising demands for specialist technicians and medical time, it is neither realistic nor desirable to use echocardiography as a screening test for heart failure. Assay of brain natriuretic peptide may prove better,[3] but surely the first test in primary care for a breathless patient should be chest radiography.

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1 Hobbs R. Can heart failure be diagnosed in primary care? *BMJ* 2000;321:188-9. (22 July.)
2 Caruana L, Petrie MC, Davie AP, McMurray JJV. Do patients with suspected heart failure and preserved left ventricular function suffer from "diastolic heart failure" or from misdiagnosis? [With commentary by A Berger.] *BMJ* 2000;321:215-8. (22 July.)
3 Landray MJ, Lehman R, Arnold I. Measuring brain natriuretic peptide in suspected left ventricular systolic function in general practice. *BMJ* 2000;320:985-6.
4 Wong PS, Doshi S. Service is valuable for cardiac murmurs too. *BMJ* 1995; 311:326.

Author's reply

EDITOR—As Davidson points out, the chest *x* ray film is viewed by many as having an important role in the routine investigation of suspected heart failure. But as far as I am aware there are no reliable data on the performance of chest radiography in diagnosing heart failure,[1] or indeed in diagnosing respiratory causes of breathlessness. To be certain of the validity of chest radiography in this role would require empirical testing.

If heart failure is sufficiently established, cardiac enlargement (cardiothoracic ratio >50%) may be present. But the correlation between the cardiothoracic ratio and left ventricular function is poor. Cardiomegaly depends on both the severity of haemodynamic disturbance and its duration. It will not be present in early left ventricular systolic dysfunction, which is worth identifying since treatment delays progression.[2] In decompensated heart failure radiographic features other than cardiomegaly may be present, such as pulmonary congestion or pulmonary oedema. But again, such features occur when disease is well established.

Echocardiography is therefore required to distinguish reliably between different causes of heart failure as different treatments are indicated, and to detect abnormalities at an earlier stage than chest radiography. Furthermore, echocardiography is less invasive, with no ionising dose to the patient or environment. As far as cost goes, if there was sufficient access to echocardiography it would be unlikely to cost much more than chest radiography since both need skilled operators and similar timing (and presumably capital costs for chest radiography are much higher).

Davidson reinforces the importance of considering respiratory disease in patients presenting with breathlessness when heart failure is a possibility. But chest radiography is not necessarily reliable in diagnosing respiratory causes of breathlessness: for example, it would not exclude asthma or early stages of chronic obstructive airway disease. Indeed, spirometry rather than chest radiography is an appropriate alternative test. Naturally, the probability of respiratory disease is greater if the patient has no history of cardiovascular disease.

In most circumstances in the United Kingdom any patient suspected of having heart failure should be offered cardiac imaging. As well as reliably diagnosing left ventricular dysfunction and valve disease, the reasonably objective information provided by echocardiography should reduce the diagnostic uncertainty that so often results in undertreatment and underdosing. Relying on less specific tests, such as chest radiography, is likely to perpetuate the current undermanagement of this complex problem. Patients with heart failure deserve evidence based treatments, which in turn require the diagnosis to be as certain as possible.[3]

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1 Agency for Health Care Policy and Research. *Heart failure: evaluation and care of patients with left-ventricular systolic dysfunction.* Rockville, MD: AHCPR, 1994, (Clinical practice guideline 11.)
2 SOLVD Investigators. Effect of enalapril on mortality and the development of heart failure in asymptomatic patients with reduced left ventricular ejection fractions. *N Engl J Med* 1992;327 685-91.
3 Task Force on Heart Failure of the European Society of Cardiology. Guidelines for the diagnosis of heart failure. *Eur Heart J* 1995;16:741-51.

Shouldn't people be responsible for their own actions?

EDITOR—I was disappointed in the *BMJ* issue on smoking (5 August 2000).[1] Authors have taken the American position of blaming everyone but the smokers themselves for the problems created by their smoking habit. I had thought that the British were sensible enough to reject our distorted sense of justice that awards billions of dollars in personal injury fees and enriches plaintiff lawyers by millions for contending that some poor innocent victim has been damaged by a defective product. No matter that the injury was the result of misuse or that common sense and general knowledge should have made the user aware of the dangers of his actions.

Does anyone seriously believe that people smoked because some cigarette executive claimed that smoking was not deleterious to health or was not addictive? I for one am doubtful that the majority of smokers had any idea what the executives were saying. For those who did know, I presume that they knew well enough that the producer of a product is always going to exaggerate its benefits and minimise its dangers. Would they believe a used car salesman?

The dangers of smoking have been known for 100 years. On a recent visit to a Toronto museum I saw a poster from 1908 that had been displayed in a school, with a healthy smiling child on one side and a sickly emaciated child on the other. The difference, as the text explained, was that one of the children was a smoker.

As children 50 years ago my friends and I would caution those among us who smoked that smoking would stunt their growth. Since then we have been bombarded with innumerable articles in the lay press and medical journals on the expected health consequences of continuing to smoke. Anyone who has smoked in the past 25 years could not possibly have escaped this barrage of warnings. If they smoked it was not because they heard that some cigarette manufacturer denied the dangers of cigarettes but because they wanted to smoke despite that danger.

Despite the pleadings of the plaintiff lawyers and the pretentious pronouncements of some American academicians, it is time to resurrect the concept that people are responsible for their own actions. Unfortunately, the issue of 5 August gives aid and comfort to the enemy. How about an issue on personal responsibility?

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1 Towards a smoke free world. *BMJ* 2000;321(7257). (5 August.)

Corrections

Lung cancer and passive smoking

An editorial error occurred in the second letter of this cluster by Allan Hackshaw et al (11 November, pp 1221-2). The word "not" was omitted from the third paragraph, which should have read: "There is further evidence against material publication bias in that 32 of the 39 studies reported non-significant results and in 16 (41%) the authors had either concluded that there was no effect[13] or that the evidence was inconclusive[3], suggesting that the passive smoking literature is **not** one with a strong tendency for positive results to be published while negative results remain unpublished."

Sentinel node biopsy for malignant melanoma

An editorial error occurred in the first letter of this cluster by S Rayatt and S Hettiaratchy (8 November, p 1285). A misunderstanding resulted in the omission of two authors and the wrong address being given for where the work was done. The authors should have been: S S Rayatt, registrar, S Hettiaratchy, senior house officer, A Key, research registrar (psychiatry), and B W E M Powell, consultant plastic surgeon, all from the Melanoma Unit, St George's Hospital, London SW17 0QT.

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