Letters

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Racial discrimination in distinction awards

Discrimination is probably indirect

EDITOR—Esmail et al attribute disparity between white and non-white award holders to discrimination.[1] Existence of direct discrimination in some spheres of the NHS does not imply it also affects distinction awards. The regional and central advisory committees on distinction awards are beyond reproach. Rubin's suggestion that several other factors may explain the skewed distributions is more plausible.[2] Disparity between groups of consultants is inevitable because of differences in abilities, training, and opportunities.

The Commission for Racial Equality held that the criteria laid down for distinction awards could, however, result in indirect discrimination, not necessarily with discriminatory intent. For example, the weight given to work of national and international significance may make the awards less accessible to those in smaller district general hospitals or specialties, where ethnic minority consultants may be concentrated. The Department of Health has therefore issued criteria placing less emphasis on national and international recognition and making A awards available to consultants delivering "outstanding and sustained service to the NHS in an exceptionally hard pressed post" provided it is not the sole ground for an award. Introduced for the 1998 awards round, the criteria are fairer and more widely applicable, but will they change attitudes?

Consultants work under varied conditions. However, the criteria do not seem to allow for this. The uniform expectations across the board lead to a skewed distribution of awards with a bias towards academics, consultants in teaching hospitals and larger district general hospitals, and those in the high profile specialties.

Esmail et al quite rightly highlight the disparity. Their campaign should, however, be directed towards rectifying the situation which places not only ethnic minority consultants but also many other consultants at a significant disadvantage. Eliminating prejudice in the system, if it exists, is more difficult than establishing a fairer assessment of achievements based on individual circumstances.

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1 Esmail A, Everington S, Doyle H. Racial discrimination in the allocation of distinction awards? Analysis of list of award holders by type of award, specialty, and region. *BMJ* 1998;316:193-5. (17 January.)
2 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)

Awards system is fair as possible

EDITOR—The conclusion reached by Esmail et al[1] regarding the uneven allocation of distinction awards is flawed because it assumes that the proportions of white and ethnic minority consultants showing exceptional performance is equal, and this may not be true. Despite the emergence of accusations of prejudice from time to time, the system has operated for half a century with few changes being needed.

As a former chairman of regional C and higher awards committees I had confidence that the system was as fair as possible, given its confidentiality. I also found the chairmen of the central advisory committee to whom I reported willing to consider ideas for modification. Members of C awards committees are elected and trusted by their peers to represent both the health districts and the specialties, and it is important that they do their homework diligently by consulting widely among their colleagues. Only C award holders are considered for higher awards, with rare exceptions where injustice is seen to have been done.

Rubin emphasises the need for all consultants to understand the system.[2] Esmail et al admit that they had no information about the composition of regional awards committees; had they obtained this, they might have modified their conclusions. During my service I prepared a detailed description and appraisal of the system, which was distributed to every consultant in my district. There is no reason why this practice should not be widely adopted.

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2 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)

NHS monitoring of discrimination should be more transparent

EDITOR—Peter Rubin's editorial[1] highlights some important shortcomings of Esmail et al's paper on racial discrimination and allocation of distinction awards.[2] Esmail and colleagues have previously highlighted concerns about racial and ethnic disadvantage among applicants to medical school and in shortlisting of junior hospital posts.[3 4] The criticism of their research methods in these two studies raised similar arguments to Rubin's.

The fundamental question seems to be why do researchers have to use covert measures to gain this information, or, indeed, use indirect measures for ethnic background (such as surnames as a proxy for ethnicity).[2] Reports such as Esmail et al's have a demoralising effect on the significant proportion of the NHS who belong to a minority ethnic community. This was echoed by the secretary of state for health in a recent Department of Health press release.[5]

The challenge for the NHS as a whole, and the NHS Executive especially, is to become more transparent in its monitoring and release of information with regard to disadvantage in its workforce. In doing so it will avoid researchers drawing wrong or inappropriate conclusions on discrimination and underline its commitment to bring about change and equity.

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1 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)
2 Esmail A, Everington S, Doyle H. Racial discrimination in the allocation of distinction awards? Analysis of list of

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award holders by type of award, specialty, and region. *BMJ* 1998;316:193-5. (17 January.)
3 Esmail A, Nelson P, Primarolo D, Toma T. Acceptance into medical school and racial discrimination. *BMJ* 1995;310:501-2.
4 Esmail A, Everington S. Racial discrimination against doctors from ethnic minorities. *BMJ* 1993;306:691-2.
5 Department of Health. *Frank Dobson spells out action on race equality in the NHS.* London: DoH, 1997. (Press release 97/262.)

Analysis should be standardised for age

EDITOR—Esmail et al's paper may be fundamentally flawed in that it does not take any account of the likely differences in age structure between white and non-white consultants in the NHS.[1]

Gaining a merit award is obviously associated with advancing age, and if non-white consultants in the NHS were younger than white consultants on average this would be enough to explain the difference in the prevalence of merit awards without any discrimination. It is essential, and not difficult, to age standardise the rates of award of merit awards to see if there is racial discrimination in their award or not.

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Authors' reply

EDITOR—To suggest, as Joseph and White do, that regional and the central advisory committees on distinction awards are beyond reproach is ignoring the huge amount of evidence that we have collected on the racism of the medical profession.[1] No one is exempt, and the senior members of the profession, because they are in a position of power, must bear the brunt of the responsibility. It is scandalous that an organisation that disburses £185 million of public funds does so in a climate of secrecy with little regard to issues of fairness and equality which affect all doctors and not just the 20% of ethnic minority doctors that are highlighted in our report.

We need better information, which as Bedi and Williams point out is essential to our understanding of what is going on. But more information will not resolve the issue until the profession accepts that racism is a problem and develops mechanisms to deal with it. As we have pointed out previously, racism affects students when they apply for medical schools,[2] in career progress,[3] in complaints,[4] and now in remuneration.

We believe that a major reform of the merit awards system is required. General practitioners and other senior doctors such as staff graders, who provide a huge and often unrecognised service to the NHS, should be considered for merit payments. Explicit criteria for merit need to be developed. Patient care as well as research needs to be taken into account, as should total earnings. For example, why should consultants who have a lucrative private practice be rewarded when a lot of senior doctors who devote all their time to the NHS and patient care may be disadvantaged. The awards should be genuinely time limited (no awards were withdrawn in 1996 or 1997) and non-pensionable so that they can be more widely distributed. The widespread perception that merit awards go with certain jobs needs to be challenged. Most deans of medical schools receive A or A+ awards. They may be deserving, but should the awards continue when their tenure as dean ends?

Distinction awards were created in 1948. At a time when Britain is reviewing many of its postwar structures, surely we can come up with a better system than one which rewards outdated perceptions of specialists as the highest order in the medical hierarchy.

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1 Esmail A, Carnall D. Tackling racism in the NHS. *BMJ* 1997;314:618-9.
2 Esmail A, Nelson P, Everington S. Ethnic differences in applications to United Kingdom medical schools between 1990-1992. *New Community* 1996;22(3):495-506.
3 Esmail A, Everington S. Racial discrimination against doctors from ethnic minorities. *BMJ* 1993;306:691-2.
4 Esmail A, Everington S. Complaints may reflect racism. *BMJ* 1994;308:1374.

Disparity is due to institutional discrimination

EDITOR—The imbalance in both employment of and consultant posts held by non-whites and women compared with the majority peer group continues to be reflected in the awarding of merit awards.[1]

In institutional discrimination, which has produced these results, the minority categories usually obtain employment in the so called Cinderella specialties or where there are few applicants for the posts. In popular specialties people in minority groups have to prove themselves clearly superior in their attainment and potential to their majority group peers. Once they have achieved consultant status the need to demonstrate superior performance over the majority groups becomes more pronounced and hence more difficult. This is a major reason why even highly competitive black or women doctors are unable to achieve comparable merit awards with their majority group peers.

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1 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)

Vested interest prevents true debate

EDITOR—As a recently retired consultant (with an award), I would like to comment on some of the points raised by the paper and editorial on distinction awards.[1 2]

Firstly, it is only now that I am no longer in a position to benefit from further awards that I feel able to speak freely on this subject. There is a real danger that fear of losing an award may inhibit free speech and criticism among serving consultants.

Secondly, I agree with Esmail et al that there may be racial bias (probably unintentional) in the allocation of awards because of the composition of the awarding committees, on which ethnic minorities are likely to be under-represented. This may correct itself in time, but that is small consolation to those now in the system who may lose tens of thousands of pounds in salary, lump sums, and pension rights.

Thirdly, Rubin seems to imply that holders of the new discretionary points should not be categorised as award holders. As these points were a direct replacement for C awards I think that this is totally wrong. It is a patronising assumption which I would like to see corrected.

Fourthly, the fact that merit awards have always been a feature of the NHS does not necessarily make them a good idea. Sensibly, general practice never adopted them. The awards have been perpetuated by the vested interest of that portion of the consultant body whose voices are in the position to be best heard. In my view they have served to reduce the overall level of consultant salaries. Whenever a consultant's salary is quoted in the press a merit award is almost invariably attached to it, with misleading consequences.

The award system was always bad and open to potential abuse, including racial discrimination. It should be abolished as soon as possible and the money released should be visibly and clearly attached to individual posts or used to raise the basic level of consultants' salaries.

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2 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)

Awards perpetuate bias against generalism

EDITOR—Esmail et al show that distinction awards are yet another mechanism through which minority ethnic groups experience discrimination within the medical profession.[1] Peter Rubin's editorial unwittingly reveals another prejudice inherent in this outdated system of patronage.[2] He states that the awards were originally thought necessary to attract the best possible recruits to specialist practice and supports this as their main continued purpose. In so doing he exemplifies the bias against generalism which permeates British medicine, echoing Lord Moran's famous 1966 portrayal of general practice as a career for those who "fall off the ladder" of hospital specialism.[3]

Distinction awards amount to a system in which many of the medical profession's most unsavoury values are institutionalised. Not least, they perpetuate the myth that specialist practice is somehow more important

and more worthy of reward than generalism. This is hardly congruent with the aspirations of a primary care led NHS, nor more broadly with the principles of equality on which the NHS was founded.

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1 Esmail A, Everington S, Doyle H. Racial discrimination in the allocation of distinction awards? Analysis of list of award holders by type of award, specialty, and region. *BMJ* 1998;316:193-5. (17 January.)
2 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)
3 Tudor Hart J. *A new kind of doctor: the general practitioner's part in the health of the community.* London: Merlin Press, 1988.

GPs should get awards too

EDITOR—Now that the discussion around the issue of distinction awards in the NHS has started,[1] perhaps it is time to reassess the awards system in the light of the new primary care led NHS. Indeed this might be a point that the forthcoming Department of Health inquiry could address.

Should general practitioners be excluded from these awards when many will have contributed "more than ordinary ability and effort" while working in the health service? I am sure that there are many general practitioners (and others) who would relish the chance of doubling their salaries and demonstrating that the system rewards endeavour and excellence, albeit of generalists not specialists. The existing system could be broadened relatively easily by making merit awards part of non-pensionable income and sharing out the resulting savings more equitably.

I should, however, declare a conflict of interest. As a white, male, public school educated general practitioner I fear I would be three times more likely to receive a merit award than my non-white colleagues.

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Money could be better spent on more consultant posts

EDITOR—Rubin[1] has failed to evaluate objectively the data presented by Esmail et al.[2] The data show clearly that racial discrimination exists in the distinction award system.

The figures for neurology, a specialty in which the majority of consultants are based in teaching rather than district general hospitals, leap from the page. Merit awards are received by 61 of 221 (27.6%) white consultants compared with one of 18 (0.06%) non-white consultants. I challenge Rubin to explain such an obvious discrepancy on grounds other than race.

He is correct to question whether such a system should exist at all in today's NHS. It was clearly needed as a sweetener to win over some consultants at the birth of the NHS. However, the argument that it still needs to be retained to attract the best possible recruits to specialist practice is false and undervalues many consultants in district general hospitals working in hard pressed, poorly supported posts. The argument is really an excuse touted by those who benefit, or will benefit, from the system. It is an issue on which the whole consultant body should be entitled to vote.

Should the £74.8 million A+, A, and B merit award monies be distributed between a relatively small group of often already privileged individuals, or should the system be axed and a further 1324 consultant posts created and spread throughout the NHS?

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2 Rubin P. Distinction awards and racial discrimination. *BMJ* 1998;316:165. (17 January.)

Author's reply

EDITOR—The criticisms of my editorial fall into three broad areas. The first is that distinction awards shouldn't exist at all, and I have nothing to add to the observations which I made in the editorial. The second is that distinction awards should be available to general practitioners. Neither the paper by Esmail et al nor my editorial addressed this issue, and it would be impossible to do it justice through the correspondence columns. The third is that I have missed the point and racial bias does exist in the allocation of distinction awards. The paper by Esmail and Everington has produced data which need an explanation. They have not confirmed their hypothesis because to do so will require the analysis of awards by age, specialty, and type of hospital.

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Only one quarter of women with learning disability in Exeter have cervical screening

EDITOR—With health authorities and trusts having just completed performance reviews of their local screening programmes (required by executive letter EL(97)67), issues surrounding access to health services have been highlighted. As part of local interest in the health needs of men and women with learning disability, we looked at the validated screening history of women with learning disability known to the Exeter and District Community Health Services NHS Trust. Of 62 women eligible for cervical screening, 15 (24%) had had a smear test in the previous five years (district average 82%). Of 12 women eligible for breast screening, seven (58%) had had breast screening within the last three years (district average 66%).

We then surveyed the practices of the 43 women for whom recall had been postponed by the general practitioner without clinical grounds. The table shows the reasons given.

Reasons that general practitioners gave for postponing recall for screening of 43 patients with learning disability

Reason given	No
"Smear not required"	13
"Disabled/mentally challenged"	8
"Learning disabilities"	5
"Down's syndrome"	3
"Not sexually active"	2
No response	12
Total	43

Recent advice from the Department of Health notes that "concerns are often raised about the inclusion of people with learning disabilities in routine programmes, particularly when consent and co-operation are an issue. The staff of screening services, such as those for breast and cervical screening, require training on the special needs and problems of people with learning disabilities."[1]

We are concerned about the difference between uptake of breast screening and uptake of cervical screening, particularly in the light of the responses to our letter asking for reasons why women had not been invited by local practices. We believe that value judgments may be made about women with learning disability that affect their access to cervical screening. We have been disappointed in the level of interest in the health of this client group, particularly as a local initiative to increase awareness and interest among local general practitioners (a free study day organised by Exeter and District Community Health Services NHS Trust, which is approved for the postgraduate education allowance) has attracted minimal interest (nine (3%) of 270 general practitioners invited).

We would be interested to know whether other people have had a similar experience in this area.

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1 Lindsey M. *Signposts for success in commissioning and providing health services for people with learning disabilities.* Leeds: NHS Executive, 1998.

Screening for breast cancer is necessary in patients with learning disability

EDITOR—We recently conducted a survey of health screening in people with Down's syndrome and found that only two of the 20 female respondents had had breast screening in the previous three years. The group was aged 18-57, and the two who had been screened were both in their mid-30s. We

assume that this rate of screening is typical for people with learning disability.

Breast cancer is the second most common cancer in women, and nulliparity increases the risk for women over 40.[1] Older women with Down's syndrome and with learning disability in general are a vulnerable group. A study of the association between cancers and mental handicap showed an increase in deaths from cancer over the past five decades; of 34 women in that study who died of cancer, five had breast cancer.[2]

The importance of health education for carers and for people with learning disability and of good health screening programmes has recently been highlighted by the Department of Health.[3]

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1 Kelsey J, Gammon M, John E. Reproductive factors and breast cancer. *Epidemiol Rev* 1993;15:36-47.
2 Jancar J. Cancer and mental handicap; a further study. *Br J Psychiatry* 1990;156:531-3.
3 Lindsey M. *Signposts for success. Commissioning and providing health services for people with learning disabilities.* Leeds: NHS Executive, 1998.

Co-proxamol is effective in chronic pain

EDITOR—Li Wan Po and Zhang's review substantiates the widely held view that paracetamol is as effective as co-proxamol in single dose for acute pain.[1] We agree with their message; indeed, another recent systematic review comes to the same conclusion.[2]

The authors dismiss comments we have made about the use of co-proxamol.[3] However, our remarks related to the treatment of chronic pain, which cannot and should not be managed with single doses of analgesics. Co-proxamol is recommended and has been extensively used at step 2 of the World Health Organisation analgesic ladder for managing chronic pain in cancer, when it is used in repeated doses. We are concerned that Li Wan Po and Zhang fail to make sufficiently clear that their analysis was of single dose studies only. It may therefore be taken out of context and dissuade doctors from implementing this good practice.

As we and others have suggested, the analgesic efficacy of single and repeated doses of co-proxamol is likely to differ, a lesson learnt 20 years ago with other morphine-like opioid analgesics.[4] Because of the extensive first pass metabolism of dextropropoxyphene, which is dose dependent,[5] plasma concentrations after a single dose may be four times lower than those found in steady state after regular six hourly administration. In addition, the active metabolite, norpropoxyphene, has a longer elimination half life than the parent compound and will accumulate to some extent on regular dosing. Thus there is a strong pharmacokinetic basis for believing that repeated doses of co-proxamol are likely to be more effective than single doses.

Li Wan Po and Zhang's systematic review is therefore not directly relevant to the discussion of the efficacy of co-proxamol as it is usually used—in repeated doses. We agree that there is a lack of data from randomised controlled trials relevant to this situation. Such trials are required to settle the argument but are difficult to accomplish. In the absence of this evidence we abide by our view that extensive anecdotal experience cannot be disregarded. It is worth listening to patients when they report that pain not controlled by regular paracetamol alone is relieved by repeated doses of co-proxamol.

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1 Li Wan Po A, Zhang WY. Systematic overview of co-proxamol to assess analgesic effects of addition of dextropropoxyphene to paracetamol. *BMJ* 1997;315:1565-71. (13 December.)
2 Moore A, Collins S. Carroll D, McQuay H. Paracetamol with and without codeine in acute pain: a quantitative systematic review. *Pain* 1997;70:193-201.
3 Sykes JV, Hanks GW, Forbes K. Co-proxamol revisited. *Lancet* 1996;348:408.
4 Hanks G W, Hoskin PJ, Aherne GW, Turner P, Poulain P. Explanation for potency of repeated oral doses of morphine? *Lancet* 1987;ii:723-5.
5 Perrier D, Gibaldi M. Influence of first pass effect on the systemic availability of propoxyphene. *J Clin Pharmacol* 1972;12:449-52.

Mefloquine to prevent malaria

Interpretation of study was not based on evidence

EDITOR—Croft and Garner's interpretation of their systemic review of controlled studies involving mefloquine cannot qualify as evidence based.[1] They have selected two outcome measures, non-compliance and withdrawal, as proxy markers for drug tolerance without any evidence of correlation of this behaviour to tolerability. In Orht's study,[2] which makes up 43% of the mefloquine group withdrawals, four of the seven withdrawals were because soldiers were redeployed and the remaining three were because of protocol failures[3] and concurrent fever, none of which was obviously related to tolerability. In clinical studies where subjects were at no risk of malaria the threshold and reason for withdrawal from prophylaxis may be different from those of travellers, whom the authors claim will behave similarly despite a different risk of malaria.

The author's argument that a symptom based outcome is less objective than a withdrawal based outcome when measuring tolerability is subjective. The claim that poor compliance or withdrawal from mefloquine is more likely to leave travellers incompletely protected compared with other regimens is unsupported and selectively used to discredit mefloquine. Their study and others[3] confirm that mefloquine is an effective antimalarial prophylactic drug. To restrict its use to the fittest and healthiest travellers on the basis of differences in undefined withdrawal rates in non-travellers because no current studies define its true tolerability is naive.

An important issue, which the authors have failed to acknowledge, is that the tolerability of any prophylactic regimen needs to be counterbalanced by the risks of morbidity and mortality associated with the disease it is used against. Under the banner of an evidence based analysis the authors have used a limited dataset, which is arguably inadequate for meta-analysis, to reflect their personal preferences on the indications for mefloquine prophylaxis.

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1 Croft A, Garner P. Mefloquine to prevent malaria: a systematic review of trials. *BMJ* 1997;315:1412-6. (29 November.)
2 Ohrt C, Richie TL, Widjaja H, Shanks GD, Fitriadi J, Fryauff DJ, et al. Mefloquine compared with doxycycline for the prophylaxis of malaria in Indonesian soldiers. A randomised, double-blind, placebo-controlled trial. *Ann Intern Med* 1997;126:963-72.
3 Behrens RH, Bradley DJ, Snow RW, Marsh KM. The impact of UK malaria prophylaxis policy on imported malaria. *Lancet* 1996;348:344-5.

Withdrawal rates are misleading measure of tolerability

EDITOR—Croft and Garner[1] draw conclusions about the tolerability of mefloquine for malaria prophylaxis that are not justified. Based on a meta-analysis of "withdrawal rates, presumably from side effects" from randomised trials they conclude that "mefloquine prophylaxis may be less effective than alternative chemoprophylaxis that is better tolerated."

Firstly, higher withdrawal rates with mefloquine occurred only in comparison to placebo, not in comparison to alternative chemoprophylaxis such as doxycycline or chloroquine and proguanil. Secondly, the use of withdrawal rates is a problematic measure of tolerability because the reasons for withdrawal from controlled trials are often unrelated to the study drug. For example, in Ohrt et al's trial, which contributed a considerable proportion of the weight in the analysis, 10 out of 16 withdrawals were due to travel from the study area. The authors clearly stated that "no participant was withdrawn from the study because of intolerance to the study drugs or adverse effects that seemed to be related to the study drugs."[2] It is misleading to use withdrawal rates as a direct measure of tolerability without considering the reasons that led participants to abandon the study.

Non-immune visitors to malarious areas need to know about the risk associated with malaria. Only with this in mind will they be able to make informed choices when they experience adverse events from prophylactic drugs. Adequate advice before travel on malaria prophylaxis should also include the following messages:

1. All the drugs available today for malaria prophylaxis have adverse effects—the seriousness of malaria warrants tolerating certain temporary adverse effects.

2. If more severe adverse effects occur travellers need to seek medical advice and, if justified, need another drug prescribed so that they don't remain without protection for the rest of their journey.

If people taking prophylactic drugs follow these instructions, together with the general guidelines on malaria prophylaxis, it should be possible to improve compliance and efficiency of prophylactic schemes, both for individual drugs and for prevention schemes as a whole. Croft and Garner's meta-analysis is inconclusive. The interpretation of this inconclusive body of evidence by Croft and Garner is, however, clearly biased. Alarmist reports on adverse effects that are based on weak grounds must be challenged by evidence based recommendations otherwise travellers' lives will be put at risk unnecessarily through underuse of effective prophylactic drugs.

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1 Croft A, Garner P. Mefloquine to prevent malaria: a systematic review of trials. *BMJ* 1997;315:1412-6. (29 November.)
2 Ohrt C, Richie TL, Widjaja H, Shanks GD, Fitriadi J, Fryauff DJ, et al. Mefloquine compared with doxycycline for the prophylaxis of malaria in Indonesian soldiers. A randomised, double-blind, placebo-controlled trial. *Ann Intern Med* 1997;126:963-72.

Mefloquine remains best drug

Editor—The results of the systematic review of mefloquine to prevent malaria[1] are largely in line with other reviews and published opinion on mefloquine prophylaxis, showing a small difference in risk of withdrawal but no difference in specific adverse events between mefloquine and placebo and no difference in risk of withdrawal when mefloquine was compared with other prophylaxis. However, there was a striking contrast between the objective findings of the analysis and the highly subjective opinions expressed in the discussion.

The study estimate of withdrawal rates of 3.3% and 0.95% (in placebo and drug controlled studies respectively) represents very high acceptance compared with other commonly prescribed drugs. This implies that only a small proportion of individuals would need to switch to an alternative prophylactic regimen.

The authors speculate that the stresses of travel in unselected travellers might act as a substrate for mefloquine associated neurotoxicity. A more likely explanation may be the large overlap between symptoms associated with jet lag, culture shock, and other stresses of travel and those attributed to mefloquine, especially after the extensive media coverage of adverse effects attributed to the drug. The discussion also confuses intolerance, which is addressed by the review, with safety, which is not.

The authors hypothesise that mefloquine prophylaxis may be less effective than alternative chemoprophylaxis that is better tolerated. They neglect to mention that no other effective prophylaxis has been shown to be better tolerated[2] and that no other prophylaxis has been shown to be equally effective.

The statement "not one randomised controlled trial has assessed the tolerability of mefloquine chemoprophylaxis in a heterogeneous study population of non-immune tourists and business travellers" is refuted by the study by MacPherson et al,[2] which is cited in the review.

Finally the authors propose an evidence based, expert review process for making recommendations about malaria prophylaxis. A process which largely fits their description has been in place for several years in Canada. Current Canadian guidelines on malaria prevention and treatment are the product of this process and are widely available to both practising physicians and the public in Canada.[3]

The clinical bottom line is that mefloquine remains the drug of choice in most travellers for whom chemoprophylaxis is indicated after careful assessment of the risks.

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1 Croft A, Garner P. Mefloquine to prevent malaria: a systematic review of trials. *BMJ* 1997;315:1412-6. (29 November.)
2 MacPherson D, Gamble K, Tessier D, Keystone I, Streiner D. Mefloquine tolerance: a randomised, double-blinded, placebo-controlled study using a loading dose of mefloquine in pre-exposed travellers [abstract No 220]. Fifth International Conference on Travel Medicine, Geneva, March 1997.
3 Committee to Advise on Tropical Medicine and Travel. 1997 Canadian recommendations for the prevention and treatment of malaria among international travellers. *Can Communicable Dis Rep* 1997;23S5:1-23.

Authors' reply

Editor—While we welcome constructive feedback to help update our review,[1] there is little in these letters that helps us.

Both Behrens and Erny and Maradit question whether withdrawal is a relevant primary outcome measure for tolerability. Common sense tells us that higher discontinuation rates in the experimental arm of a randomised trial than in the control arm provide good evidence that some adverse events are occurring more often with the intervention. We therefore adopted this as a useful proxy for tolerability as it measures a decision to discontinue the drug.[2] We also summarised reported symptoms in those patients continuing to take prophylaxis, although we believe this to be a less reliable indicator.[1]

The correspondents focus on one study to criticise us for not taking into account post hoc explanations for withdrawal. The logical conclusion of their argument is that we should alter the data from trials to exclude drop outs if post hoc explanations for withdrawal seem unrelated to drug reactions. However, as the studies we analysed were all randomised, such chance effects are distributed between the two arms and taken into account in the statistical analysis. A post hoc exclusion of one participant subgroup in the analysis of data from a trial is methodologically unsound and potentially dangerous.

Professor Houston and colleagues welcome the objective findings of our analysis but proclaim that mefloquine is the drug of choice for most travellers. Far from being the "clinical bottom line" as they assert, this is simply a clinical opinion founded on a wholly inadequate evidence base. The fact is that there has never been a randomised controlled field trial of mefloquine prophylaxis in non-immune tourists and business travellers.[1 3] As we have pointed out,[1] the study by MacPherson et al that they cited was a clinical toxicity study carried out in volunteers before travel during 1996-7 and not a field trial. Until appropriate field trials of mefloquine prophylaxis have taken place national malaria advisory committees have a duty to clinicians and travellers to make explicit the insufficiency of the scientific evidence in favour of this drug.[4]

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1 Croft AMJ, Garner P. Mefloquine malaria prophylaxis. In: Garner P, Gelbland H, Olliaro P, Salinas R, Volmink J, Wilkinson D, eds. Infectious diseases module. *Cochrane Library* [database on disk and CD ROM]. Oxford: Update Software, January 1998.
2 Greenhalgh T. Papers that report drug trials. *BMJ* 1997;315:480-3.
3 Mefloquine and malaria prophylaxis. *Drug Ther Bull* 1998;36:20-2.
4 Croft AM, Garner P, Squire SB. Malaria prevention for travellers. *JAMA* 1998;279:990.

Passive smoking in pregnancy

Editor—In the issue of 18 October several original papers and an editorial by Davis[1] highlighted the importance of passive smoking—an issue that deserves to receive widespread publicity. The problem of passive smoking in pregnancy remains underappreciated by both healthcare workers and the public.

Smoking in pregnancy is associated with numerous complications for both mother and baby. Effects start in utero, resulting in increased perinatal mortality and morbidity and the sudden infant death syndrome.[2] Many pregnant women appreciate that their own cigarette smoking may have ill effects on the fetus, but limited information is available to pregnant women on the potential harm of their inhaling the cigarette smoke of others. Meconium analysis indicates that nicotine metabolite concentrations in infants of passive smokers are not significantly different from those in infants of active light smokers.[3] Fetal exposure to tobacco smoke may therefore be substantial even as a result of maternal passive smoking.

We recently conducted a small study to determine the prevalence of passive smoking among pregnant women at home and in the workplace and how many women received advice against smoking.[4] Data were collected prospectively by anonymous self administered questionnaire from 113 women attending a public antenatal clinic. The response rate was 100%. Sixty women were primiparous, 48 were single, and 62 were in employment outside the home during their pregnancy. Forty seven women smoked, 26 were ex-smokers, and 40 had never smoked. Nine of the 26 ex-smokers had stopped just before or during the current pregnancy. Overall, 81 women were exposed to passive smoking during pregnancy (72%), 41 being exposed at home only, 18 at work only, and 22 at home and at work. Forty of the 62 women who were employed were exposed to passive smoking. Most smokers were also exposed to passive smoking (38 out of 47). Advice against smoking had been given to 56 women during their current pregnancy and to 28 at some stage in the past, but 29 women had never received such advice.

Our results indicate that the prevalence of passive smoking in pregnancy is high. This study relied on a questionnaire as a measure of maternal passive smoking and may have underestimated the extent of the exposure. Although the number studied was small, this report indicates that passive smoking in pregnancy may be a bigger problem than is generally appreciated. The need to highlight the risk of environmental tobacco smoke should not be restricted to smokers, and we recommend that it should be discussed with all pregnant women at booking.

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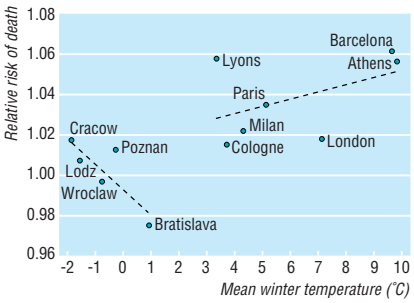
1 Davis RM. Passive smoking: history repeats itself. *BMJ* 1997;315:961-2. (18 October.)
2 Haglund B, Cnattingius S. Cigarette smoking as a risk factor for sudden infant death syndrome: a population based study. *Am J Public Health* 1990;80:29-32.
3 Ostrea E Jr, Knapp D, Romero A, Montes M, Ostrea A. Meconium analysis to assess fetal exposure to nicotine by active and passive maternal smoking. *J Paediatr* 1994;124:471-6.
4 Geary M, Crowley D, Boylan P. Passive cigarette smoking in pregnancy. *J Obstet Gynaecol* 1997;17:264-5.

Causes of regional differences in air pollution effects are being studied further

Editor—In their letter about the APHEA project's results on the effects of particulate matter and sulphur dioxide on mortality,[1] Bobak and Roberts proposed that heterogeneity in the observed effects between eastern and western European cities could be explained by differences in average winter temperature.[2] They have calculated Spearman rank correlation coefficients to illustrate this association.



City specific risk of death per 50 μg increase in sulphur dioxide/mm^3 by mean winter temperature; regression lines were estimated separately for eastern and western European cities

As described in our paper, we investigated the role of temperature as an effect modifier, along with other variables.[3] In our analyses we noticed that other variables explained the heterogeneity in the effect estimates as well as or even better than indicators of the local temperature. For example, the Spearman correlation coefficient between age standardised mortality and the effect estimates was −0.92 and between the proportion of old people (>65 years) and the effect estimates it was 0.66, whereas between average winter temperature and the estimated effects it was 0.70. Furthermore, the correlation coefficient between the average winter temperature and the estimated effects in eastern European cities was −0.70 while in the western and southern European cities it was 0.29 (figure). In addition, in weighted regression models in which the inverse variance of the city-specific effect estimates was used as weight, when temperature, age standardised mortality, and geographical area (east-west) were introduced as explanatory variables simultaneously all three remained significant (that is, they independently explained part of the observed heterogeneity in the effect variables).

In the light of these results we thought it premature to report positive associations with possible effect modifiers, beyond the east-west difference, before these are investigated further. One of the main targets of the current APHEA2 project ("short term effects of air pollution on health: a European approach to methodology, dose-response assessment, and evaluation of public health significance") is the study of effect modifiers as causes of regional differences.

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1 Bobak M, Roberts A. Heterogeneity of air pollution effects is related to average temperature. *BMJ* 1997;315:1161. (1 November.)
2 Katsouyanni I, Touloumi G, Spix C, Schwartz J, Balducci F, Medina S, et al. Short term effects of ambient sulphur dioxide and particulate matter on mortality in 12 European cities: results from time-series data from the APHEA project. *BMJ* 1997;314:1658-63.
3 Katsouyanni K, Zmirou D, Spix C, Sunyer J, Schouten J, Ponka A, et al. Short-term effects of air pollution on health: a European approach using epidemiological time-series data. The APHEA project: background objectives, design. *Eur Respir J* 1995;8:1030-8.

Six months' follow up after occupational exposure to HIV is usually long enough

Editor—The chief medical officers' Expert Advisory Group on AIDS has recommended that at least six months should elapse after the cessation of post-exposure prophylaxis before a negative result of an HIV antibody test is used to reassure the exposed worker that infection has not occurred.[1] Willcox has expressed the view that the follow up period should exceed six months to allow for the possibility of late seroconversion, and that exposed healthcare workers should practise safer sex and consider avoiding exposure prone procedures throughout the follow up period.[2]

The expert advisory group recommended a six month period because this would include all but the most exceptional HIV seroconversions; it recognised that there may be unusual circumstances that prompt a more extended follow up, including the circumstances of the exposure and the wishes of the exposed worker. Routinely extended follow up may not only reduce the compliance of exposed workers but also prolong the intense anxiety that they will experience, as well as being unnecessary in all but exceptional cases.

The advisory group also advises that it is unnecessary to avoid exposure prone procedures during follow up after occupational exposure to HIV. The risk of an exposed worker acquiring HIV infection is so low that such a restriction is not justified; the risk of HIV infection then being transmitted by the exposed worker to a patient is even lower. Advice should, however, be given about safer sex and avoidance of blood donation during the follow up period. If a worker has a positive result of an HIV antibody test at any time, then he or she must stop performing exposure prone procedures in accordance with the expert advisory group's recommendations.[3]

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1 Expert Advisory Group on AIDS. *Guidelines on post-exposure prophylaxis for health care workers occupationally exposed to HIV*. UK Health Departments, 1997.
2 Willcox JR. Prophylaxis after occupational exposure to HIV. *BMJ* 1998;316:701. (28 February.)
3 Expert Advisory Group on AIDS. *AIDS/HIV infected health care workers: guidance on the management of infected health care workers*. London: Department of Health, 1994.

Acute pancreatitis

Normal serum amylase does not exclude severe acute pancreatitis

Editor—Mergener and Baillie highlight the problem of diagnosing acute pancreatitis if too much reliance is placed on the serum amylase level, drawing attention to the relatively low specificity of the test.[1] They also state that amylase is rapidly cleared from the kidneys, and this, along with other factors, may lead to a normal serum amylase

level even in the presence of necrotising pancreatitis.[2]

We have recently seen two patients with severe necrotising pancreatitis and normal serum amylase levels (one of whom also had a normal result on abdominal ultrasonography), which led to a delay in establishing the correct diagnosis. Computed tomography showed pancreatic necrosis in both cases, and both patients required management in intensive care; one died subsequently from sepsis and multiorgan failure.

Clinicians need to be aware not only of alternative causes of raised serum amylase but also of the fact that a normal serum amylase does not exclude severe forms of acute pancreatitis, which are associated with a high morbidity and mortality.

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1 Mergener K, Baillie J. Acute pancreatitis. *BMJ* 1998;316:44-8. (3 January.)
2 Misiewicz JJ, Pounder RE, Venables CW, eds. *Diseases of the gut and pancreas.* Oxford: Blackwell Scientific, 1994.

Major haemorrhage may be a late complication

EDITOR—In their clinical review of acute pancreatitis Mergener and Baillie point out that patients often get worse before they get better and so outcome may depend on careful monitoring for complications.[1] Such a review should also raise awareness of potential life threatening complications even if they are rare. One such is delayed major haemorrhage due to large vessel erosion associated with pancreatitis, which we believe is an underrated phenomenon because of its perceived infrequency.

Major haemorrhage complicating both acute and chronic pancreatitis is well described and of formidable severity. Flati et al reported eight cases and reviewed a further 389 cases in the literature, finding a mortality of 60.4% in haemorrhage associated with acute pancreatitis.[2] The condition is potentially treatable: embolisation under radiological guidance is an established technique,[3 4] while surgical treatment by pancreatic resection or vessel ligation has been reported in 15 patients with pancreatic disease, six with acute pancreatitis.[5] A comprehensive search of the computerised patient database at our own intensive care unit has shown that of the 36 patients admitted to the unit with acute pancreatitis during 1990-8, five developed acute major haemorrhage. Two had pseudoaneurysms of the splenic artery embolised by angiography and survived the haemorrhagic episode (one died later), two died of uncontrollable bleeding from lumbar vessels despite laparotomy, and one died suddenly of haemorrhage after discharge from the unit. In four of these five cases the haemorrhage occurred as a late event in patients who were recovering from the most severe stage of their pancreatitis. This is an important incidence in a clearly defined group of patients and suggests that haemorrhage in patients with acute pancreatitis that requires support in an intensive care unit is underreported.

Thus we found that although major vessel haemorrhage complicating acute pancreatitis is uncommon, it has a significant rate of occurrence, particularly after severe disease requiring support in an intensive care unit. Its onset may be rapid and catastrophic, and it is potentially treatable by radiological and surgical measures once identified. We believe that this makes it worthy of listing in even the most generalised discussion of acute pancreatitis, as awareness is essential for rapid recognition and intervention.

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1 Mergener K, Baillie J. Acute pancreatitis. *BMJ* 1998;316:44-8. (3 January.)
2 Flati G, Salvatori F, Porowska B, Talarico C, Flati D, Proposito D, et al. Severe haemorrhagic complications in pancreatitis. *Ann Ital Chir* 1995;66:233-7.
3 Sawlani V, Phadke RV, Baijal SS, Kumar S, Roy S, Gupta P, et al. Arterial complications of pancreatitis and their radiological management. *Australas Radiol* 1996;40:381-6.
4 Owen TD, Davies DG. Massive haemorrhage in pancreatitis. *Br J Clin Pract* 1991;45:33-4.
5 Shankar S, Russel RCG. Haemorrhage in pancreatic disease. *Br J Surg* 1989;76:863-6.

Alexander the Great may have died of acute pancreatitis

EDITOR—Mergener and Baillie's clinical review of acute pancreatitis prompted me to remember the circumstances surrounding the death of Alexander the Great.[1] Alexander was 32 when he died on 10 June 323 BC in Babylon after an illness lasting 12 days. The exact cause of his death has never been established, but the descriptions are consistent with acute pancreatitis as a consequence of an extended period of excessive alcohol intake.[2]

Sources agree that Alexander became ill unexpectedly on 29 May, when he attended a party dining and drinking into the night, but the descriptions vary about what followed. One account claims that Alexander suddenly felt ill while drinking; another that he shouted with pain as if struck through the liver with an arrow; and a further source indicates that he did not sicken until just after leaving the party.

All accounts agree that Alexander gradually declined after the party but remained rational, discussing the next campaign. After a day he was moved to another palace, which was cooler. By 7 June, however, he was very ill and returned to the main palace. Two days later various commanders and officers visited him. The next day he died. There is a story that, realising that he was about to die, he crawled out of his room (he was too weak to walk) to commit suicide by throwing himself into the river, but he was intercepted.

Thus Alexander seems to have had a sudden onset of severe, probably upper, abdominal pain which may have radiated to his back. He then declined over almost two weeks but remained lucid. He spent this time lying down. The likely diagnosis is acute pancreatitis, with a perforated ulcer as the main differential diagnosis. A dissecting aneurysm or malaria is unlikely, as is poisoning, although this last is possible. Finally, he could have suffered a spontaneous pneumothorax at the banquet. He had received a chest wound at the fort of Multan on the return from the Indus.

Alexander had settled in Babylon after his disastrous return from India, when he had lost his army. He was planning new campaigns, initially against Arabia, but he probably considered that he would have difficulty mustering new forces. Also, because he had had India in his hand and lost it, everything else would have seemed trivial. Thus accounts of Alexander holding recurrent drinking parties, sitting up all night, and then sleeping it off the next day are plausible.

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1 Mergener K, Baillie J. Acute pancreatitis. *BMJ* 1998;316:44-8. (3 January.)
2 Fox RL. *Alexander the Great.* London: Penguin, 1986.

Opiate detoxification under anaesthesia

EDITOR—The editorial by Strang et al[1] summarises a previous paper to which I wrote a detailed reply noting several serious errors and omissions.[2] They have ignored three important points.

Firstly, although one commercial provider has seriously misled the media,[3] rapid opiate detoxification under anaesthesia has been regularly and increasingly used by state hospitals in several countries since 1987. Seoane et al described its effectiveness in detoxification and in starting naltrexone maintenance treatment in heroin addicts who had previously been treated without success.[4] Naltrexone's ability to improve rates of abstinence, when properly supervised, has been confirmed in a randomised controlled trial.

Secondly, as to "the hazards of prolonged general anaesthesia," I noted that during the past 15 years the risks have become low. Mortality approaches 1 in 200 000, and as rapid opiate detoxification under anaesthesia does not entail surgery, the comparison is really with intensive care, where patients are commonly and safely sedated for days or weeks. Deaths have been reported[2 5] but not during anaesthesia, when "the sudden pharmacological bombardment" is presumably most intense. I think none would have occurred had patients been monitored (as is our practice) by experienced intensive care staff for 24 hours (surely the safest setting for detecting and managing cardiorespiratory problems). Rapid opiate detoxification under anaesthesia has been safely performed in addicts with severe cardiac and renal disease.[2]

Finally, and perhaps most importantly, rapid opiate detoxification under anaesthesia is a pain relieving technique. As with sur-

gical anaesthesia, some patients wake in pain but most have minimal withdrawal ratings[5] and can return home in 24-48 hours. This is abundantly documented. Pain relief is a traditional medical function, but anaesthesia was introduced in less sophisticated times, without randomised controlled trials of amputation with or without chloroform. If we follow the logic of the editorial, surgery with general anaesthesia should cease pending a randomised controlled trial. Though widely disliked by health professionals, opiate addicts deserve effective pain relief. Not all need anaesthesia, but some find detoxification too agonising to complete or contemplate. The justification for using anaesthesia in detoxification is the same as for any other procedure whose unpleasantness raises barriers to treatment or causes unacceptable suffering. Patients who are phobic about dental treatment request (and usually get) general anaesthesia, despite occasional disasters.

In 1995 I invited Professor Strang to collaborate in the sort of comparative study he now claims is essential. He did not respond.

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1 Strang J, Bearn J, Gossop M. Opiate detoxification under anaesthesia. *BMJ* 1997;315:1249-50. (15 November.)
2 Brewer C. The case for rapid opiate detoxification under anaesthesia (RODA): a reply to Gossop and Strang. *Br J Intensive Care* 1997;7:137-43.
3 Caplehorn J. Ultrarapid opiate detoxification: what's all the fuss about? *Med J Australia* 1997;167:397.
4 Seoane A, Carrasco G, Cabré L, Puiggrós A, Hernández E, Álvarez M, et al. Efficacy and safety of two new methods of rapid intravenous detoxification in heroin addicts previously treated without success. *Br J Psychiatry* 1997;171:340-5.
5 Brewer C. Ultra-rapid, antagonist-precipitated opiate detoxification under general anaesthesia or sedation. *Addict Biol* 1997;2:291-302.

Clinical experience and performance in final examinations

Teaching styles need to be reviewed to help students with inappropriate learning styles

Editor—McManus et al found that their prospective study of two cohorts of medical students at a London medical school showed that the students' learning style, but not their final examination results, was related to the amount of knowledge gained from clinical experience.[1]

Their questionnaire measured some aspects of the students' clinical exposure but did not measure the knowledge gained from such experience.[2] A proportion of the practical procedures and surgical operations selected in the questionnaire was not of central relevance to undergraduates, even after the time when the study was carried out is taken into account. Students should never have performed procedures such as colonoscopy, abdominal paracentesis, ring block, endotracheal intubation, and intramuscular or subcutaneous injection without supervision, even in the early 1990s.

Likewise, having seen operations such as laryngectomy, removal of cerebral tumour, or skin grafting more than four times was less relevant than developing a systematic approach to the diagnosis and management of common symptoms. It is this systematic approach that final examinations assess. Furthermore, even if the authors' measurements of clinical experience were valid, the association of learning style with clinical experience was small (for 1986 cohort, deep learning, $r = 0.26$, $r^2 = 0.07$).

The authors concluded that medical schools could select students by assessing their learning styles. This might lead to profit making organisations offering courses on completing questionnaires on learning styles. The reliability and validity of the assessment of learning style with a questionnaire would decrease if it were carried out under such threatening conditions, as the candidates might respond to the questionnaires according to their perception of what medical schools regard as desirable.

Students adapt their learning strategies to the perceived demands of lecturers and departments.[3] Furthermore, having a match between styles of learning and teaching has been shown to be even more important than the particular learning style adopted by the student.[4 5] A more constructive and effective approach would be for teachers and departments to review their own teaching styles and to identify and help students with an inappropriate learning style.

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1 McManus IC, Richards P, Winder BC, Sproston KA. Clinical experience, performance in final examinations, and learning style in medical students: prospective study. *BMJ* 1998;316:345-50. (31 January.)
2 McManus IC, Richards P, Winder BC, Sproston KA, Vincent CA. The changing clinical experience of British medical students. *Lancet* 1993;341:941-4.
3 Ramsden P. Student learning and perceptions of the academic environment. *Higher Educ* 1979;8:411-28.
4 Pask G. Styles and strategies in learning. *Br J Educ Psychol* 1976;46:128-48.
5 Entwistle N. *Styles of learning and teaching—an integrated outline of educational psychology for students, teachers and lecturers.* London: David Fulton, 1988.

Valid methods are needed to assess students

Editor—McManus et al's paper discusses the lack of correlation between clinical experience and measures of clinical competence in undergraduate medical students.[1] This is almost certainly partly a result of inadequacies in the validity of traditional clinical examinations.[2] The link with learning style, however, is constrained by the fact that learning style may vary with changes in educational environment, so measures taken at entry may not be resilient to changes of course and other pressures.[3]

There are other explanations. McManus et al measured experience in terms of the number of acute conditions, practical procedures, and surgical operations encountered by the cohort. This may not be an adequate measure of students' experience unless it includes other features—for example, the number of patients seen, the length of time spent with each patient, the feedback that students have had from their teachers on their clinical activity, and the many other attributes of the clinical learning environment. One problem is that we are still not clear about what the term "clinical experience" actually means. Moreover, experiences that students value highly—such as ward rounds, positive as well as correctional feedback on their clinical practice, a feeling of being welcomed on to clinical rotations, a high degree of responsibility for the initial contact with and management of patients, and regular teaching and learning events—are provided by medical teachers to varying degrees.[4]

Unpublished data from a recent study[5] suggest that where a valid method is used to assess students and when students have both the opportunity to gain experience and the time to reflect on that with close supervision, the correlation between clinical experience and an assessment shortly afterwards increases to around 0.3-0.4. More research of this type must be funded if we are to make progress in understanding what aspects of clinical experience contribute to clinical competence.

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1 McManus IC, Richards P, Winder BC, Sproston KA. Clinical experience, performance in final examinations, and learning style in medical students: prospective study. *BMJ* 1998;316:345-50. (31 January.)
2 Newble DI, Jolly BC, Wakeford RE. *The certification and re-certification of doctors.* Cambridge: Cambridge University Press, 1994.
3 Coles CR. Is problem-based learning the only way? In: Boud D, Feletti G, eds. *The challenge of problem-based learning.* London: Kogan Page, 1991:295-307.
4 Jolly BC. Training versus experience in the acquisition of bedside manners. In: Jolly BC, ed. *Bedside manners: teaching and learning in the hospital setting.* Maastricht: University of Maastricht Press, 1994:102-20.
5 Murray E, Jolly BC, Model M. Can students learn clinical method in general practice? A randomised crossover trial based on objective structured clinical examinations. *BMJ* 1997; 315:920-3.

Authors' reply

Editor—In response to Leung we emphasise that it was our aim to be "constructive and effective"—to help improve medical education, for patients, teachers, and students. More specifically:

(1) We did not discuss whether students should be doing various procedures unsupervised, but 46% of the 1986 cohort had given an intramuscular injection unsupervised, 34% a subcutaneous injection, and 23% a ring block, and 9% had done an endotracheal intubation and 2% an abdominal paracentesis.[1] If, as Leung implies, students should never do anything unsupervised, that partly explains the decline in experience during the past decade,[2] although it does not explain the reduced experience of acute medical conditions.

(2) Assessing the quality of repeated theatre experience is difficult. We suspect that surface learners are uninterested the

first time they see an operation, bored the second time, and do not turn up the third; that deep learners compare and contrast first with second and second with third, and that strategic learners ask what is going on in the theatre next door. That, however, is speculation; it is time someone measured these things properly.

(3) Of course our r^2 values are not terribly high. If they were then statistical analysis would be unnecessary as mere observation would be sufficient. Scientific studies are interesting for multifactorial human conditions influenced by 10 or 20 factors, and mathematical necessity means that none alone can determine more than 5-10% of accountable variance, and r is rarely more than 0.3.

(4) When one of us (ICM) was asked once if questionnaires on learning style could be used for selection he emphasised the problem of "faking good." [3] At present questionnaires are a research instrument, not a practical selection method. Practical measures that cannot be faked could probably be developed with some ingenuity, effort, and money and might involve real problem solving at an assessment centre.

Jolly and Murray are correct in describing the limitations of our measure of experience, but it had to be simple and questionnaire based. We would like better measures that incorporate the features they discuss, but meanwhile we have to make do with simple inventories that at least have face validity (and have been used previously by Jolly[4]).

We agree entirely that final assessments with objective structured clinical examinations may well have better reasonable correlations with clinical experience, as Jolly et al have suggested elsewhere.[5] Ultimately, as Jolly and Murray emphasise, we know little about what makes doctors competent, how they become competent, and how we assess competence. Work is indeed urgently needed.

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2 McManus IC, Richards P, Winder BC. Clinical experience of UK medical students. *Lancet* 1998;351:802-3.
3 McManus IC. From selection to qualification: how and why medical students change. In: Allen I, Brown P, Hughes P, eds. *Choosing tomorrow's doctors*. London: Policy Studies Institute, 1997:60-79.
4 Jolly BC, Macdonald MM. Education for practice: the role of practical experience in undergraduate and general clinical training. *Med Educ* 1989;23:189-95.
5 Jolly BC, Jones A, Dacre JE, Elzubeir M, Kopelman P, Hitman G. Relationships between students' clinical experiences in introductory clinical courses and their performances on an objective structured clinical examination (OSCE). *Acad Med* 1996;71:909-16.

Earlier study of effect on healthcare costs of preventing fatal diseases yielded similar results

EDITOR—I was surprised that Bonneux et al[1] did not refer to my work of almost a decade earlier[2] in their paper showing that preventing fatal diseases increases healthcare costs.[2] To have done so would have strengthened their findings.

I posed the same question as Bonneux et al and used similar lifetable analyses. The most important differences between the two studies arise from the costings, the population referred to, and the time period. I could not find readily available monetary costs. I therefore used condition specific bed use abstracted from the hospital inpatient enquiry for 1980 and 1981 as a proxy measure for acute hospital costs.[3] The analyses were based on age specific and cause specific mortality in England and Wales in 1980-1.

Qualitatively Bonneux et al's findings agree with mine. For instance, I found that eliminating cancers would increase population acute care costs by 3% and eliminating ischaemic heart disease would increase them by 5%. Eliminating deaths from injuries and poisoning reduced costs by 7%.

It is reassuring when studies that use slightly different methods and relate to different populations and time periods have such similar findings. The attempt to replicate others' findings is undervalued; it is of as much importance to the progress of science as the search for novelty.

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1 Bonneux L, Barendregt JJ, Nusselder WJ, Van der Maas PJ. Preventing fatal diseases increases healthcare costs: cause elimination life table approach. *BMJ* 1998;316:26-9. (3 January.)
2 St Leger AS. Would a healthier population consume fewer health service resources? A life-table analysis using hospital in-patient enquiry (HIPE) bed-usage statistics as a proxy for hospital treatment costs. *Int J Epidemiol* 1989;18:227-31.
3 Department of Health and Social Security, Office of Population Censuses and Surveys, and Welsh Office. *Hospital in-patient enquiry, main tables, England and Wales 1980*. London: HMSO, 1980. (Series MB4 No 16.)

Lessons of a hip failure

Registers of joint replacement operations should be set up

EDITOR—Fickleness characterises the fashion industry. The fact that the introduction and use of hip prostheses in the United Kingdom should also be characterised by such whimsy is scandalous.[1] The failure of the 3M Capital hip system and the difficulties in tracing and reviewing patients as a result bear witness to this.[2,3]

While we agree with the points made by Muirhead-Allwood in her editorial, she does not take the next logical step of requiring the establishment of a national hip prosthesis register. On the introduction of new prostheses and monitoring of outcomes, a register would provide a nationally coherent database recording preoperative, perioperative, and postoperative follow up information.

It has been estimated that around 62 different replacement hip joints are available in Britain, manufactured by 19 different companies. Part of the problem in evaluating their effectiveness is the lack of high quality, prospective, comparative studies.[4] A national register would provide the basis for scientifically well designed, statistically robust studies. Registers in Sweden and Norway collect a lot of relevant patient data and use these to allow adjustments for the effects of case mix when comparisons are made.

We were provoked by the failure of the 3M Capital hip into ascertaining the national view of the need to establish a register and sent a UK-wide Epinet message to all directors of public health. The replies showed that several databases, audits, and research projects exist, but they are uncoordinated, geographically restricted, and, for some, strictly time limited. The overwhelming response was for the establishment of a national database to unite this research.

In the light of the importance that the government attaches to clinical effectiveness and the establishment of clinical governance in trusts,[5] we hope that this call for a national register is heard and acted on.

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1 Muirhead-Allwood SK. Lessons of a hip failure. *BMJ* 1998;316:644. (28 February.)
2 Medical Devices Agency. *Hazard notice*. London: MDA, 1998. (MDA9801.)
3 Department of Health. *3M Capital Hip system update*. London: DoH, 1998. (HSC 1998/041.)
4 NHS Centre for Reviews and Dissemination. Total hip replacement. *Effective Health Care* 1996;No 2:7.
5 Secretary of State for Health. *The new NHS*. London: Stationery Office, 1997. (Cm 3807.)

Register exists in Trent region

EDITOR—Muirhead-Allwood calls for better regulation of the replacement hip industry in her editorial on the lessons to be learnt from the unacceptably high failure rate of the 3M Capital hip implant.[1]

In the Trent region a register of all primary and revision hip and knee replacement operations was established in 1990. It has the cooperation of all orthopaedic surgeons in Trent and provides a confidential record that is available to individual surgeons. Capital Hips started to be implanted in the region in 1991, and implantation continued until 1995. Most of the implants were inserted in 1992 and 1993. Altogether 187 operations to implant a Capital hip were registered with the Trent study between 1991 and 1995. An increasing trend in revision operations was noticed in patients in whom the Capital hip had been implanted, with five revision operations in 1993-4 and

10 in 1996-7. Altogether, 16 revision operations for patients with Capital hips are registered with the database, giving an overall revision rate of 8.6% for these implants (95% confidence interval 4.5% to 12.5%); this is very high when it is considered that most of the implants were inserted in 1992 and 1993, and it contrasts with the five year revision rate for Charnley hip replacements implanted in Trent in 1990 of 3.0% (34/1132).

The true effectiveness of a new implant can be assessed only if such databases are established across wide areas. We believe that new implants should not be used in regions unless a database is in place to monitor the results. It is only with the monitoring of such innovations in joint replacements that costly errors will not be made and advances can be judged against the performance of standard joint replacements as implanted by the average surgeon in the average hospital.

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1 Muirhead-Allwood SK. Lessons of a hip failure. *BMJ* 1998;316:644. (28 February.)

Medical Device Regulations 1994 are now mandatory for all medical devices

Editor—Muirhead-Allwood argues that the introduction of a prosthesis must be regulated by a system analogous to that set up for drugs, involving extensive clinical trials, a licensing process, and postmarketing surveillance.[1] This is almost precisely what will occur now, because on 13 June the Medical Device Regulations 1994 became mandatory for all medical devices, not just for prostheses. This new regulatory system will be introduced throughout Europe under directive 93/42/EEC and was described in detail by Ludgate and Potter in 1993.[2]

The question, therefore, is not whether a system should be introduced but whether this new system is appropriately designed and will be appropriately operated and enforced. No doubt the regulators—principally in this case the Medical Devices Agency—and the European Commission, industry, and professionals will be able to consider the new system in the light of the facts of the problems with the 3M Capital hip as they emerge. On the basis of the limited information so far available from the press, I have confidence in the system as designed. I suspect, however, that improvements could be made in details of the postmarketing vigilance system for medical devices, although one should not jump to hasty conclusions from individual cases.

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1 Muirhead-Allwood SK. Lessons of a hip failure. *BMJ* 1998;316:644. (28 February.)
2 Ludgate SM, Potter DC. European directives on medical devices. *BMJ* 1993;307:459-60.

Clinicians need to record name of manufacturer

Editor—The hazard warning issued recently for the 3M Capital hip system by the Medical Devices Agency highlighted not only the need to regulate its use[1] but also the importance for all clinicians who give drugs or implant medical devices of recording the batch number and the manufacturer of the products.

Manufacturers are strictly liable for injuries caused by defects of products they supply if they are unreasonably dangerous to those who have used them.[2 3] However, if the manufacturer has notified the clinicians of potential defects but the clinicians are unable to identify the patients who have received the potentially defective devices and take immediate remedial action, the clinicians may be liable for damages resulting from the delay in corrective action being taken. Furthermore, if the clinicians are unable to identify the manufacturer during litigation—which may occur many years after the event—they stand in the shoes of the manufacturer and may become legally liable for the defective products. Clinicians should audit their records of all drugs given and devices implanted, such as intraocular lenses, hip and knee prostheses, and pacemakers.

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1 Muirhead-Allwood SK. Lessons of a hip failure. *BMJ* 1998;316:644. (28 February.)
2 Product liability directive. *Official Journal of the European Communities* L210 1985 Aug 7. (85/374/EEC.)
3 *Consumer Protection Act 1987*. London: HMSO, 1987.

Cardiac surgical services in Bristol are now of high quality

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Editor—The unfortunate events relating to the General Medical Council's recent inquiry into paediatric cardiac surgery performed at Bristol Royal Infirmary have been widely reported, both in the medical press and in the public media.[1 2] Clearly in the past there was a major problem with this particular aspect of the service. The clinicians who now provide both the paediatric and adult cardiac service in Bristol support the efforts of the General Medical Council and any future public inquiry to resolve the matter fully.

In the mass of reporting, which was often written in a highly emotive style, it is easy to overlook the fact that the last of the events in question occurred some three years ago and the earliest occurred many years before that. Like many other centres, Bristol has been developing an audit programme in recent years, and we currently have a sophisticated audit system in the Bristol Royal Infirmary cardiac unit. This multidisciplinary computer database is completed for each cardiac surgical procedure, with 200 separate data fields entered for each patient. This allows many forms of analysis, including analysis of crude mortality, mortality adjusted for risk, and a wide range of other factors.

The results for the unit in 1997-8 are excellent as well as comprehensive and show that we are functioning as a cardiac surgical unit of the highest quality (table). There is no difference between the performance of any of the current surgical teams. Paediatric cardiac surgery, which has been performed at Bristol Royal Hospital for Sick Children since October 1995, is also carefully audited, and again our most recent figures from 1997-8 show excellent results (table). The data in the table confirm that the results for both adult and paediatric cardiac surgery are among the best that we know of in the United Kingdom. The full audit documents for both adult and paediatric cardiac surgery are in the final stages of preparation and will be made publicly available shortly. Patients, their families, their doctors, and other health professionals can be confident in the quality of care that is currently offered by our cardiac surgical service.

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1 Dyer C. Compensation claims expected to follow GMC's findings. *BMJ* 1998;316:1691. (6 June.)
2 Delamothe T. Who killed Cock Robin? *BMJ* 1998; 316:1757. (6 June.)

Number of cardiac operations done in Bristol on adults (at Bristol Royal Infirmary) and children (at Bristol Royal Hospital for Sick Children) in 1997-8, and mortality in same period

	Total No of procedures in Bristol	Mortality in Bristol (%)	Mortality in UK (1996-7)
Bristol Royal Infirmary:			
Total adult cardiac surgery operations	1184	2.4	5.1
Coronary artery bypass grafting	831	0.8	3.7
Bristol Royal Hospital for Sick Children:			
Total of operations for congenital heart disease	260	3.1	4.1
Open heart operations before age of 1 year	50	8.0	7.5

*Most recent figures from Society of Cardiothoracic Surgeons of Great Britain.