Letters

Data are not now collected by ethnic group in South Africa

EDITOR—Claire Hilton notes that since April 1995 British hospitals have had to collect data on each patient's ethnic group.[1] In the United States this is standard practice. In the *Morbidity and Mortality Weekly Report* data on numerous variables relating to the health and ill health of black and white people are regularly given[2]—as also, on occasion, are data on Hispanics, American Indians, Inuits, and Pacific Islanders. In South Africa precisely the converse has occurred. Ethnic subdivisions have stopped appearing in reports from the Central Statistics Services, and thus mortalities are no longer given for the constituent populations (black, Asian, coloured (Eur-African-Malay), and white), only for the combined population. Additionally, in our big cities annual reports from public health departments provide only combined data on morbidity and mortality.

Disadvantages are many. Recently, colleagues and I reviewed changes in the total death rate and the death rate from coronary heart disease in South African populations.[3] We found that, from 1978 to 1989, mortality from coronary heart disease fell by 56% in the white population and by 36% in each of the Indian and coloured populations. Because changes in mortality from this foremost "killer" are continuing we intended to update our information. But this is now precluded. Currently, desegregation does not apply to our national cancer registry. But if it did how, for example, could benefits from Papanicolaou smear tests be assessed? The incidences of cervical cancer in the black and coloured populations are among the highest in the world. The constituent populations of South Africa have a wider divergence in the occurrence of a variety of diseases than occurs in any other country: coronary heart disease, appendicitis, colon cancer, and hip fractures. Interethnic differences in the occurrence of disease, reactivity, and metabolism are well known elsewhere.[4] In South Africa the change to using combined data will make it harder to identify and quantify the target populations who are in most need of help, as well as severely stultifying research on the occurrence of disease and on combating it.

Hilton is right, of course, that the level of treatment is affected by ethnic group. Recently, in the United States the effect of race and income on mortality and the use of services among beneficiaries of Medicare was investigated; both race and income were shown as having substantial prejudicial effects, that of race being much more pronounced than that of income.[5] This is a matter of universal shame.

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1 Hilton C. Collecting ethnic group data for inpatients: is it useful? *BMJ* 1996;313:923-5. (12 October.)
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Dealing with patients with HIV infection

Isolation is impractical and unnecessary

EDITOR—After all that has been written about AIDS, I am surprised that the *BMJ* should publish Bernard Rabinowitz's personal view.[1] In portraying current ethical standards as the result of an unprincipled cave-in to some homosexual lobby Rabinowitz distorts both those standards and the history of public health medicine. For example, as early as the first world war a British royal commission concluded that the control of sexually transmitted diseases was better served by securing the willing cooperation of those at risk—through education and voluntary, confidential testing and treatment—than by sanctions.[2]

While isolation may work for diseases such as smallpox or diphtheria, which lead to death or recovery within weeks, for HIV infection it is impractical and burdensome because people remain infected but asymptomatic for many years; in addition, it is unnecessary because the virus is not spread by casual contact. The traditionalist view of infectious disease control as being focused solely on identification, isolation, and treatment of infected subjects is questionable in relation not just to HIV infection but also to more conventional diseases.[3]

Obligatory premarital HIV testing has been tried and found wanting.[4] People can have sex before they marry, and a medical test may be a powerful disincentive to matrimony. The idea of compulsory testing before pregnancy is even more bizarre. How could it be enforced?

Doctors working for insurance companies have always been bound by the same rules of confidentiality as the rest of the profession. They may recommend refusal of insurance or a raised premium for someone with hypertension or HIV infection, but they risk disciplinary action if they disclose personal health information without good reason.

HIV testing is already routine for donors of blood, organs, and semen. Current guidance allows a doctor to warn a sexual partner of someone with HIV infection if, after discussion, the person has refused consent for this.[5] Similarly, it requires doctors who have a condition that could affect their judgment or performance, be it HIV infection or dependence on alcohol, to seek medical advice and testing if appropriate and to modify their practice if this is necessary to protect patients. A doctor who knows that a colleague is continuing to practise in a way that puts patients at risk must report him or her to the proper authorities.

There can be valid differences of opinion, but serious debate is scarcely furthered by appeals to a vision of professional "heritage and duty" that is based more on mythology than fact.

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1 Rabinowitz B. The great hijack. *BMJ* 1996;313:826. (28 September.)
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Such bigotry should not have been published

EDITOR—I feel compelled to respond to a few of the many inaccuracies and prejudices in Bernard Rabinowitz's personal view about HIV infection and AIDS.[1]

Firstly, there is still much debate among experts about the origins of HIV. In addition, there was a period of years between the first cases of HIV infection in the United States and a diagnostic test becoming available. By the time that diagnosis was possible people across the world were already infected. The author's suppositions drawn from historical inaccuracies are wrong.

When can any patient be forced to have any test against his or her wishes? How would an HIV test performed before emergency surgery influence the decision to operate? The author clearly does not appreciate that the difference between performing a preoperative HIV test and a preoperative chest *x* ray examination is that one is supposedly for the benefit of the surgeon and the other is clearly for the benefit of the patient.

The comparison between HIV infection and the infectious diseases mentioned in the article—polio and tuberculosis, for example—is astounding. The method of spread of HIV is different, there is no treatment to stop infectivity, there is no cure, and HIV infection cannot be prevented by vaccination. How is HIV infection like a standard infectious disease?

Finally, what exactly is the author suggesting that we do with HIV positive people in order to stop the spread of the virus? Do we tattoo people with the letters AIDS, isolate them in a ghetto, or eventually transport all those infected into separate camps? What would be the next step? A person who is HIV positive is not putting others at risk by living in the same community. If over the past many years society has decided that individuals can make their own choices as to whether to risk exposure to HIV then I think that society is correct and Rabinowitz is wrong. If individuals are not aware of risk behaviour then education, not segregation, is needed.

The rank, but subtle, homophobia inherent in this article and the inference that it was the articulate but morally corrupt homosexuals who are to blame for the HIV epidemic sickens me. I am equally sickened that the *BMJ* should choose to publish this bigotry. The words "personal view" would not justify racial or gender prejudices, so why do they justify this?

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1 Rabinowitz B. The great hijack. *BMJ* 1996;313:826. (28 September.)

Eliminating poverty and ignorance is the solution

EDITOR—We have sympathy for our surgical colleagues who risk HIV infection daily through their work. But Bernard Rabinowitz's suggestion that HIV testing should be obligatory before surgery[1] will do nothing to reduce his risk unless he refuses surgery to people found to be infected; this view is considered to be unethical by the General Medical Council in Britain.[2]

Rabinowitz states that HIV testing could be vital in an emergency. We work in one of the largest HIV treatment centres in Britain, and we cannot think of one instance in which emergency HIV testing would have been essential for patient care.

Rabinowitz's conviction that HIV testing should be obligatory for the entire population is even more misguided. His comparison of HIV infection to diseases such as tuberculosis, diphtheria, and smallpox is flawed for several reasons. Firstly, HIV infection has a prolonged, asymptomatic, incubation phase, and therefore standard isolation procedures (which are applied to the other diseases mentioned above) are impractical. Secondly, as HIV is spread almost entirely through sexual intercourse, patients pose little risk to anyone except their sexual partners. Therefore the only "isolation" needed is sexual. We would be interested to know how he proposes to isolate sexually up to a quarter of the adults in Africa for 10 years while they are in perfect health and able to contribute to the economy. We believe that the only practical way to isolate a population sexually is to promote universal use of condoms. A policy of obligatory HIV testing would give individuals a false sense of security and provide a disincentive to safer sex.

The cost of a programme of obligatory testing and the policing of such a programme is inhibitory, especially as it would need repeating, regularly. Forged results and certificates would become a major problem, as evidenced by one of our patients from Africa, who was infected after her marriage to a man with false medical certificates. The money might be better spent providing adequate services for diagnosing and treating other sexually transmitted diseases (which have been shown to reduce the incidence of HIV infection[3]), a supply of affordable condoms, basic education, and legislation to empower women.

HIV infection is a disease of poverty and ignorance, both of which factors are driving the epidemic in developing countries. These are the battles that need fighting, rather than the "ethical nonsense" in the United States to which Rabinowitz refers.

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1 Rabinowitz R. The great hijack. *BMJ* 1996;313:826. (28 September.)
2 General Medical Council. *HIV and AIDS: the ethical considerations.* London: GMC, 1995.
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Few authors have the courage to stand up and be counted

EDITOR—The *BMJ* should ask Bernard Rabinowitz to write an editorial on AIDS and its hijacking by politicians.[1] His personal view will, I think, be shared by a substantial number, if not most, of the profession. When historians look back on the 20th century his article will be cited as one of the few speaking out against the vocal minority. There will be incredulity that professional organisations did not represent the views of so many of their members and that so few of us had the courage to stand up and be counted.

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1 Rabinowitz B. The great hijack. *BMJ* 1996;313:826. (28 September.)

Activists in India are promoting good health of babies through breast feeding

EDITOR—I was shocked to read the unkind comments made by John Dobbing in his letter[1] about the editorial by R K Anand.[2]

I was the organiser of the international workshop in Bombay on the prevention of mental handicap in developing countries under the aegis of the Commonwealth Association for Mental Handicap and Developmental Disabilities, to which Dobbing refers. The entire proceedings were published in Bombay and later in Britain. I was present throughout the workshop in which Anand questioned Dobbing about the validity of his statement that "exclusive breastfeeding in communities as indigent as this will often adequately support good growth for two or three months."[3] Anand quoted Gopalan, a former director of the National Institute of Nutrition, India,[4] and others about the remarkable ability of poor women to breast feed and the dangers of the early introduction of complementary food. I strongly support the view that the baby food industry takes advantage of statements such as the above from Dobbing and promotes the introduction of cereals from an inappropriately early age.

Anand's editorial related to the unethical exploitation of health professionals by the baby food industry for the purpose of promoting the sales of its products. The seemingly noble gestures that the industry makes in the form of grants or financial help to doctors to support their research, training, and education programmes have none other than this selfish motive behind them. It is a matter for rejoicing that the World Health Assembly has acted to end the conflict of interests and to bring about the unhindered promotion of breast feeding. The assembly's resolution[1] aims at discouraging financial and material help given to health workers.

It is unfortunate that Dobbing has sidetracked the issue raised in Anand's editorial and has made a personal attack on him. He should note that the people to whom he referred as "a fanatical band of activists" are people who have at heart nothing more than the wellbeing and good health of babies the world over.

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1 Dobbing J. Health workers and the baby food industry. *BMJ* 1996;313:1398-9. (30 November.)
2 Anand RK. Health workers and the baby food industry. *BMJ* 1996;312:1556-7. (22 June.)
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4 Gopalan C. Studies on lactation in poor Indian communities. *J Trop Pediatr* 1958;4:87-95.

Having some lifesaving skills must be better than having none

EDITOR—I welcome C L Morgan and colleagues' paper on the effectiveness of the BBC's *999* training roadshows on cardiopulmonary resuscitation and recognise the importance of learning from its findings.[1] As the authors state, the research was carried out after the roadshows in 1994 and provided valuable information to allow the content of the course to be altered and the training to be improved for the roadshows in 1995 and 1996. Several facts omitted from the paper, however, may have led to unnecessary concern among first aid trainers in Britain about the value of their work in training more than 30 000 people in basic lifesaving skills at *999* roadshows over the past four years.

The BBC's *999* lifesaver roadshows were not set up to provide definitive training in cardiopulmonary resuscitation. Their aim was to give people the confidence to act calmly in any emergency. The event has always been called a lifesaver roadshow, not a cardiopulmonary resuscitation roadshow as described in the paper. The course lasts 2.5 hours and includes 1 hour 20 minutes of cardiopulmonary resuscitation; other elements include information on choking, bleeding, burns, and how to dial 999. In fact, one first aid tip picked up by two viewers who watched a safety video at different roadshows helped them each to save a life by using credit cards to seal chest wounds.

Although the instruction at the *999* lifesaver roadshows follows the guidelines of the European Resuscitation Council, it was never expected—and is certainly never stated—that after 80 minutes of tuition people will be able to perform perfect cardiopulmonary resuscitation, especially when measured against the gold standard of the guidelines. It is impressive, however, that 94% of the people tested remembered to do compressions and 97% attempted ventilation.

I would point out that at the end of the course people received a certificate of attendance, not of competence. We have no idea of what level of skill the people acquired at the roadshows and so are unable to compare competence at the end of the roadshows with that six months later. Whatever the people's competence, however, most health professionals would surely agree that in an emergency it is better to do something than nothing at all. It would also seem logical that someone who has had hands on training with a manikin is better able to be talked through resuscitation procedures by ambulance control operators over the telephone.

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1 Morgan CL, Donnelly PD, Lester CA, Assar DH. Effectiveness of the BBC's 999 training roadshows on cardiopulmonary resuscitation: video performance of cohort of unforewarned participants at home six months afterwards. *BMJ* 1996;313:912-6. (12 October.)

Clinical effects of anticoagulants in suspected acute myocardial infarction

Adding heparin seems justified

EDITOR—Rory Collins and colleagues cannot justify the conclusions of their systematic overview of the clinical effects of anticoagulant treatment in suspected acute myocardial infarction; at best the conclusions can be put forward only as a personal view.[1] Heparin, even when added to aspirin, showed a small but (given the large numbers) significant benefit, and without significant harm. A reduction in mortality of five cases per 1000 is of the order of benefit gained from accelerated tissue plasminogen activator in the global utilisation of strategies to open occluded coronary arteries (GUSTO) trial,[2] and, given the huge numbers of patients treated, the total number of lives saved will be clinically important. That this benefit is achieved without significant harm is a most important observation. What treatment that saves five lives per 1000 without significant harm at so little cost could be rejected?

The study has severe limitations. The authors start with the premise that because patients now receive aspirin the results of studies of heparin without aspirin are meaningless. In the studies that they used to analyse this, however, patients were receiving aspirin for the first time. Currently, most patients admitted to hospital with myocardial infarctions are already receiving aspirin and therefore cannot benefit from a new prescription; thus this review is equally meaningless. A patient with infarction who is taking aspirin represents a failure of aspirin; clearly this is a different population. By Bayesian theory we can predict that patients in whom aspirin fails are more likely, rather than less likely, to derive benefit from alternative treatment, and because the treatment seems to do no harm there seems to be every justification in giving it. A prospective randomised controlled trial is needed to provide the answer.

Furthermore, the mode of administration of heparin in the large trials reviewed has always been criticised because there is no evidence that subcutaneous heparin is beneficial in acute coronary syndromes. Indeed, the clinical imperative to use intravenous heparin was so strong that the heparin arms of these large trials were generally invalidated by the large number of protocol violations that were included. As the authors have pointed out, there are few data on the use of intravenous heparin after thrombolysis from large trials apart from GUSTO. We can therefore rely only on the data obtained from the much earlier and smaller trials and the GUSTO substudy that included coronary angiography and showed that heparin helped to maintain arterial patency after thrombolysis. The GUSTO trials have, of course, confirmed the overall safety of intravenous heparin.[3]

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1 Collins R, MacMahon S, Flather M, Baigent C, Remvig L, Mortensen S, *et al.* Clinical effects of anticoagulant therapy in suspected acute myocardial infarction: systemic overview of randomised trials. *BMJ* 1996;313:652-9. (14 September.)
2 GUSTO Investigators. An international trial comparing four thrombolytic strategies for acute myocardial infarction. *N Engl J Med* 1993;329:673-82.
3 Bassand J. Gusto: logic wins at last (EDIT). *Eur Heart J* 1994;5:2-4.

Reduced intensity of anticoagulation may be indicated

EDITOR—In their systematic overview of use of heparin in acute myocardial infarction Rory Collins and colleagues assert that the routine addition of heparin to aspirin "is not justified."[1] I would challenge this. Results from the global utilisation of streptokinase and tissue plasminogen activator for occluded coronary arteries (GUSTO-I) trial show that the lowest 30 day mortality among patients treated with intravenous heparin was in those in whom the activated partial thromboplastin time at 12 hours after thrombolysis was in the range 50-70 s.[2] When taken in conjunction with the favourable mortality figures for accelerated tissue plasminogen activator plus immediate intravenous heparin in that trial,[3] this suggests that it would be premature to exclude the routine use of heparin, particularly if tissue plasminogen activator is the chosen thrombolytic agent. Rather, it seems more appropriate to control the intensity of anticoagulation until a properly designed study has proved otherwise.

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2 Granger CB, Hirsh J, Califf RM, Col J, White HD, Betriu A, *et al.* Activated partial thromboplastin time and outcome after thrombolytic therapy for acute myocardial infarction. Results from the GUSTO-1 trial. *Circulation* 1996;93:870-8.
3 GUSTO Investigators. An international randomized trial comparing four thrombolytic strategies for acute myocardial infarction. *N Engl J Med* 1993;329:673-82.

Authors' reply

EDITOR—Both aspirin and fibrinolytic treatment are of substantial value in acute myocardial infarction.[1] But the randomised trials that have directly addressed the effects of adding heparin to aspirin (with most patients also receiving fibrinolytic treatment) indicate that any further reductions in death or other major vascular events are small, are not statistically definite (as the lower limits of the confidence intervals extend to about zero), and may be offset by a small increase in major bleeds.[2] Most of this evidence has concerned the addition of high dose subcutaneous heparin to aspirin and fibrinolytic treatment, where, despite the randomisation of 62 000 patients in the third international study of infarct survival (ISIS-3) and the second study by Gruppo Italiano per lo Studio della Sopravvivenze nell'Infarto Miocardico (GISSI-2), heparin produced no significant

Table 1 Effect in main trials of adding subcutaneous or intravenous heparin to aspirin and fibrinolytic treatment in acute myocardial infarction. Values are No of cases (SE)/1000 patients treated

Outcome	(*a*) GISSI-2 and ISIS-3 Effect in patients allocated high dose subcutaneous heparin	(*b*) GUSTO-I Effect in patients allocated intravenous heparin instead of subcutaneous heparin	(*c*) Indirect assessment of effect of adding intravenous heparin to aspirin (estimated as (*a*)+(*b*))
One month mortality	2.2 (2.4) less, P=0.4	0.9 (3.8) more, P=0.8	1 (4) less, P=0.8
Reinfarction in hospital	2.7 (1.4) less, P=0.06	6.7 (2.5) more, P<0.01	4 (3) more, P=0.2
Any stroke in hospital	0.6 (0.9) more, P=0.5	2.0 (1.6) more, P=0.2	3 (2) more, P=0.2
Haemorrhagic stroke in hospital	1.0 (0.5) more, P=0.07	1.1 (1.0) more, P=0.3	2 (1) more, P=0.08
Major bleed in hospital	3.2 (0.7) more, P<0.001	2.6 (1.6) more, P=0.10	6 (2) more, P<0.001

(*a*): GISSI-2 and ISIS-3 randomised 62 000 patients between subcutaneous heparin plus aspirin plus fibrinolytic treatment versus aspirin plus fibrinolytic treatment only.(*b*): GUSTO-I randomised 20 000 patients between intravenous heparin plus aspirin plus fibrinolytic treatment versus subcutaneous heparin plus aspirin plus fibrinolytic treatment.

difference in one month survival (table 1, (*a*)).

Few patients have been studied in trials of intravenous heparin plus aspirin versus aspirin alone, and the results from just those trials are unpromising but inconclusive.[2] More definite evidence about the effects of adding intravenous heparin to aspirin is, however, provided by the large global utilisation of streptokinase and tissue plasminogen activator for occluded coronary arteries (GUSTO-I) trial, in which—among 20 000 patients allocated to receive streptokinase—aspirin plus high dose subcutaneous heparin was directly compared with aspirin plus intravenous heparin.[3] Despite the large size of GUSTO-I, however, intravenous heparin was not associated with any apparent reduction in mortality or other major clinical events (table 1, (*b*)). So, although intravenous heparin seems to produce small improvements in coronary artery patency when added to adequate doses of aspirin after streptokinase[3] or tissue plasminogen activator,[4] the regimen of intravenous heparin studied in GUSTO-I did not seem to confer any clinical advantage over high dose subcutaneous heparin—or, indirectly, over aspirin alone (table 1, (*c*))—but it did seem to be associated with a small increase in serious bleeds. Moreover, although the global use of strategies to open occluded coronary arteries (GUSTO-II) trial initially studied a somewhat more intensive regimen of intravenous heparin than did GUSTO-I (as higher activated partial thromboplastin times had been associated with higher rates of coronary artery patency), GUSTO-II was stopped prematurely because of intracerebral haemorrhage and other major bleeds.[5]

Hence the available evidence from clinical trials does not justify the routine addition of either intravenous or subcutaneous heparin to aspirin in the treatment of acute myocardial infarction, irrespective of the type of fibrinolytic treatment used—or, indeed, of whether any fibrinolytic treatment has been used.

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1 Collins R, Peto R, Baigent C, Sleight P. Aspirin, heparin and fibrinolytic therapy in suspected acute myocardial infarction. *N Engl J Med* (in press).
2 Collins R, MacMahon S, Flather M, Baigent C, Remvig L, Mortensen S, *et al.* Clinical effects of anticoagulant therapy in suspected acute myocardial infarction: systematic overview of randomised trials. *BMJ* 1996;313:652-9. (14 September.)
3 GUSTO Investigators. An international randomized trial comparing four thrombolytic strategies for acute myocardial infarction. *N Engl J Med* 1993;329:673-82.
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5 Global Use of Strategies to Open Occluded Coronary Arteries (GUSTO) IIa Investigators. Randomized trial of intravenous heparin versus recombinant hirudin for acute coronary syndromes. *Circulation* 1994;90:1631-2.

Use of anticoagulants in suspected acute myocardial infarction in Europe varies

EDITOR—The conclusion of Rory Collins and colleagues' systematic overview of the use of anticoagulants in suspected acute myocardial infarction—that heparin should not be routinely given with aspirin—prompts an examination of routine clinical practice.[1] A retrospective review of 4035 representative patients admitted to hospital with acute myocardial infarction during January 1993 to June 1994 in regions of 11 European countries has produced a unique picture of typical practice.[2]

Over two thirds (68%) of the sample received heparin, mainly intravenously, and most patients (83%) received aspirin during their admission. Heparin treatment was more common among patients given aspirin than among those not given aspirin (odds ratio 2.15 (95% confidence interval 1.81 to 2.54), $P<0.0001$). There was substantial regional variation, however, in the use of heparin (any regimen) (range 18-95%, median 83%) and in the choice of regimen (range 8-100% for intravenous administration, median 64%), indicating a lack of consensus.

Routine practice in Europe thus does not seem to reflect the published evidence on efficacy. In addition, prescribing practice varies widely despite a shared knowledge base on the use of antithrombotic treatment in acute myocardial infarction. Local practice and policies on use of heparin in suspected acute myocardial infarction need to be reviewed in the light of the results of the systematic overview.[1]

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2 European Secondary Prevention Study Group. Translation of clinical trials into practice: a European population-based study of the use of thrombolysis for acute myocardial infarction. *Lancet* 1996;347:1203-7.

Metabolic effects of antihypertensive treatment should not be overstated

EDITOR—Ola Samuelsson and colleagues conclude that neither evolving diabetes nor increasing triglyceridaemia during antihypertensive treatment has a significant impact on coronary heart disease.[1] Their method of analysis leaves much room for doubt.

Firstly, their definition of diabetes mellitus does not agree with that in the stated reference,[2] in which fasting whole blood glucose concentrations of >6.7 mmol/l are categorised as indicating diabetes. The American Diabetes Association has recommended a further downward revision of fasting diagnostic values. Miscategorisation could have underestimated the impact of diabetes, and it important to examine whether different cut offs for fasting values affect the authors' conclusions.

Secondly, examining the relative risk of coronary heart disease with 1 mmol/l increments of both cholesterol and triglyceride concentrations seems naive, as does the use of non-log transformed triglyceride concentrations, since the data are clearly skewed. Again, it would be interesting to examine whether lesser increments in triglyceride concentration (for example, of 0.4-0.5 mmol/l) were related to coronary heart disease. The lack of data on triglyceride concentrations for the last half of the study is another deficiency, which the authors fail to discuss; so is the ostensible fall in cholesterol concentration over 15 years of observation, since most cohort studies have previously reported either a rise or no change. This raises the issue of whether any dietary manipulation occurred during the follow up period.

The uncertainties of the metabolic impact of antihypertensive agents on cardiovascular disease should be acknowledged. Overstating the case from retrospective data seems to fly in the face of extensive prospective epidemiological data that define the risk of coronary heart disease from the combination of multiple risk factors; it could inhibit the move towards the global assessment of cardiovascular risk.

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1 Samuelsson O, Pennert K, Andersson O, Berglund G, Hedner T, Persson B, *et al*. Diabetes mellitus and raised serum triglyceride concentration in treated hypertension—are they of prognostic importance? Observational study. *BMJ* 1996;313:660-3. (14 September.)
2 World Health Organisation Expert Committee on Diabetes Mellitus. *Second report*. Geneva: WHO, 1980. (WHO technical report No 646.)

Authors' arguments about infertility services were based on a misunderstanding

EDITOR—Kamal K Ahuja and colleagues confirm that sperm may be used posthumously when proper consent has been obtained according to the Human Fertilisation and Embryology Act.[1] However, they misinterpret paragraph 5.4 of the Human Fertilisation and Embryology Authority's code of practice. This paragraph states: "Centres may examine or treat people without first obtaining their consent only in exceptional circumstances. The only circumstances likely to arise in the course of infertility treatment services are where the procedure is necessary to save the patient's life, and she is unconscious and cannot indicate her wishes." Ahuja and colleagues' arguments are therefore based on a misunderstanding of the paragraph.

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1 Ahuja KK, Emerson G, Seaton A, Mamiso J, Simons EG. Widow's attempt to use her dead husband's sperm. *BMJ* 1996;314:143. (11 January.)

Investigations to diagnose cause of dizziness in elderly people

Clinical assessment in the surgery is not adequate

EDITOR—We do not agree with Nicki R Colledge and colleagues' recommendations to general practitioners that their study "should herald a shift away from protracted investigation programmes" for dizzy elderly patients or that a diagnosis can be made "without recourse to hospital referral."[1] Dizziness was provoked by standing in 63% of patients, and 39% had fallen, but the only specialist investigations undertaken were posturography, vestibular testing, and magnetic resonance imaging. The authors attributed dizziness to cervical spondylosis in 65% and to poor vision, anxiety, or hyperventilation in many others. Cervical spondylosis was diagnosed on clinical grounds only. It is common in elderly people, and the authors do not give the incidence among controls. Similarly, poor vision in a dizzy subject does not imply that visual impairment causes dizziness.

The study does not include appropriate cardiovascular investigations. This is important when twice as many of the dizzy patients as controls had ischaemic heart disease and almost three times as many were taking vasodilator drugs. Orthostatic hypotension was diagnosed after only one minute's standing time. In elderly subjects an early temporary increase in arterial blood pressure when standing is common, and the usual recommended test period is at least two minutes. It is advised that measurements should be continued for even longer and that frequent readings should be taken if orthostatic hypotension is suspected.[2]

The authors used supine carotid sinus massage as a provocation test for dizziness, and it is not surprising that none of the dizzy group were positive, because symptoms of the carotid sinus syndrome are usually provoked only when the patient is upright. The vasodepressor response was not measured, and patients with vasodepressor carotid sinus syndrome may present with dizziness. The head up tilt test, which is one of the most useful tests for diagnosing central and cardiovascular dizziness, was not used either.[3]

We would advise general practitioners that a clinical assessment in the surgery is not adequate for dizzy elderly patients. Indeed, if such patients have had falls, if they drive, or if their quality of life is affected they should be referred for outpatient cardiovascular investigations. These tests have a high diagnostic yield in elderly people and greatly alter clinical management.

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1 Colledge NR, Barr-Hamilton RM, Lewis SJ, Sellar RJ, Wilson JA. Evaluation of investigations to diagnose the cause of dizziness in elderly people: a community based controlled study. *BMJ* 1996;313:718-92. (28 September.)
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Algorithm should ask whether patient is taking drugs that may cause dizziness

EDITOR—We agree with Nicki R Colledge and colleagues that dizziness is a difficult diagnostic problem in elderly people.[1] It may often, however, have a simple and remediable cause that has not been brought out in the paper: in our experience iatrogenic causes of dizziness are common in elderly people.

Table 1 of the authors' paper shows significant differences between the cases and the controls with regard to treatment with diuretics and calcium antagonists. No further breakdown of these drug categories is provided, but it can be safely assumed that at most 57 (38%) patients in the dizzy group were taking drugs liable to cause dizziness in elderly people. In the discussion the authors concentrate on the possible pathology present. They do not give an analysis of the frequency of observed postural hypotension in those patients receiving vasodilator or diuretic drugs.

We are therefore surprised that the management algorithm does not include a question asking whether the patient is taking drugs that may cause dizziness. We see this as an important oversight: the evaluation algorithm is incomplete without this simple yet profoundly important question.

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Authors' reply

EDITOR—Some of the points that Sarah Caine and Margaret MacMahon make about our paper are the result of careless reading, some are incorrect, and some we concede. They have missed the fact that central vascular disease was the underlying diagnosis in 70% of our subjects and that, while anxiety and poor vision often accompanied dizziness, poor vision was never the sole cause, and anxiety was the sole cause in only three cases. Cervical spondylosis is a clinical diagnosis and was defined as dizziness reproduced by neck movement that was limited in range. These figures were given for controls in tables 3 and 4 of our paper, with only 6 (6%) fulfilling the criteria.

We accept that standing blood pressure was not measured at 2 minutes. Postural hypotension was defined as a symptomatic drop of >20 mm Hg on standing, but patients with symptoms after one minute had their blood pressure measured again. The results included all those with a drop in blood pressure on standing (table 3), but only those who were symptomatic had their symptoms attributed to this (table 6).

We stated in our discussion that upright carotid sinus massage was not performed, and we would point out that only 10% of subjects are reported as having a positive response only when upright.[1] Very high rates of the carotid sinus syndrome have been reported in secondary and tertiary referrals[2] but not community based subjects such as those we studied. Our discussion also acknowledged that we did not perform prolonged head up tilt testing, which is the recognised test for vasovagal syncope. Even in a highly selected series of 65 patients, however, only seven had positive results of tilt tests.[2]

Our community based series is the most substantial and comprehensive to date, and the results clearly support the initial management of the vast majority of dizzy elderly patients by general practitioners. Our algorithm states that those with falls and blackouts should be referred for further investigation, and we have no argument against the referral of those whose quality of life remains impaired despite their initial management. We are interested that Philip Evans and colleagues think that a formal statement about drug treatment should have been included in the algorithm, because we took this essential step for granted.

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1 Kenny RA. Introduction. In: Kenny RA, ed. *Syncope in the older patient*. London: Chapman and Hall, 1996.
2 McIntosh S, Da Costa D, Kenny RA. Outcome of an integrated approach to the investigation of dizziness, falls and syncope in elderly patients referred to a "syncope" clinic. *Age Ageing* 1993;22:53-8.

Implementing guidance on hepatitis B for applicants to medical school is time consuming

EDITOR—Gordon Parker and Susan Jenkins report medical schools' policy on hepatitis B status for applicants to the schools,[1] and R J C Gilson comments on their findings in an editorial.[2] We have noted problems in implementing the requirement that successful applicants to medical school should produce evidence of immunisation against, or non-carriage of, hepatitis B before starting their course.[3]

Our medical school's prospectus contains information about hepatitis B. Last year, students who were offered a place were asked to return a health questionnaire, which contained a specific question about their hepatitis B status. A letter was then sent to each applicant to take to his or her general practitioner. This contained detailed instructions about the applicant's immunisation and blood test requirements.

When the A level results were announced in mid-August 41 of the 107 students who subsequently started training at the school had not provided the necessary evidence of their hepatitis B status. Much time was spent contacting these students, several of whom were overseas. Twelve entrants could not organise the blood test and had to attend our unit for venepuncture. Sixteen applicants who had complied with the new guidelines did not start training here.

Routine immunisation and subsequent testing for an immune response take a minimum of seven months. Ideally the first dose of vaccine needs to be given before the student has applied for medical school. The occupational health nurses here spent a lot of time explaining the new requirements to general practitioners. When a course of immunisation had not been completed some general practitioners were reluctant to measure hepatitis B surface antigen in healthy applicants without apparent risk factors. Others tested for hepatitis B surface antigen without explaining the consequences of a positive result. And, despite the explanatory letter, one applicant was given hepatitis A vaccine. None of the applicants was found to be a carrier of hepatitis B.

Our previous immunisation programme, run during the preclinical course, was relatively easy to organise. It allowed accurate records of dates of immunisation to be entered into our computerised hepatitis B programme. Students who were carriers of hepatitis B could be counselled and investigated further before embarking on the clinical course.

Whatever the arguments for and against the new requirement for immunisation or evidence of hepatitis B status, or both, before entry to medical school, any scheme for its implementation has to be thorough. Our experience suggests that this is time consuming and diverts limited resources away from other, arguably more important, activities.

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1 Parker G, Jenkins S. Hepatitis B and admission to medical school: an audit of British medical school policy. *BMJ* 1996;313:856-7. (5 October.)
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Predicting adverse cardiac events after myocardial infarction and thrombolysis

External validity of authors' conclusions is doubted

EDITOR—The logic of risk stratification in acute myocardial infarction is that it allows treatment (and finite resources) to be targeted on the group in whom the potential benefits are greatest. Yet Sumit Basu and colleagues seem to be recommending cardiac catheterisation in at least 68 of their 100 patients with myocardial infarction on the basis of abnormal scintigraphic findings, while reporting a positive predictive value of scintigraphy of only 49%.[1] Given that this was a highly selected group of low risk patients, which excluded elderly patients, those with severe heart failure, and those with left bundle branch block, applying the authors' recommendations to all patients with acute myocardial infarction would inevitably lead to invasive investigation of the large majority. While this strategy is possible, it seems to turn the logic of risk stratification on its head.

A fundamental requirement of risk stratification is that it is done early enough after myocardial infarction to anticipate the period of greatest risk. Our own database of 1225 patients shows that, by delaying their studies for five to seven weeks, Basu and colleagues would have failed to stratify the 230-247 patients (18.8-20.2%) who died, had a reinfarction, or were admitted to hospital with unstable angina before that time. Even if analysis was restricted to 800 hospital survivors treated with thrombolysis, death or reinfarction during this early period of heightened risk occurred in 50-60 cases (6.3-7.5%).

The choice of end points in the authors' study is an additional limitation. Several end points were combined for the statistical analysis. Although this is common practice, it can be justified only if the end points reflect similar pathological mechanisms. Combining death, reinfarction, and unstable angina into one end point is acceptable because these are all (with the occasional exception of death) ischaemic events. Heart failure, which accounted for a third of the adverse cardiac events in the study, is less acceptable because its development late after myocardial infarction is usually a function of the ventricular remodelling process rather than a new ischaemic event.

The delay that occurred before investigation, the use of a subgroup of patients at exceptionally low risk, and the choice of end points cast doubt on the external validity of the conclusions as they apply to the real world of the management of myocardial infarction. Thallium-201 imaging may well be more useful than exercise electrocardiography for risk stratification, but this study does not prove the point.

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1 Basu S, Senior R, Dore C, Lahiri A. Value of thallium-201 imaging in detecting adverse cardiac events after myocardial infarction and thrombolysis: a follow up of 100 consecutive patients. *BMJ* 1996;313:844-8. (5 October.)

Study does not justify replacement of exercise testing with radionuclide imaging

EDITOR—The study by Sumit Basu and colleagues addresses the important issue of risk stratification after myocardial infarction in the era of thrombolysis.[1] Its conclusions are that exercise testing does not discriminate between those patients who will have cardiac events and those who will not, whereas planar exercise thallium-201 radionuclide imaging does, predicting 89% of events. Given the cost of radionuclide imaging, this has considerable implications, but if it is true it suggests that a major change in current practice with regard to this common and important condition is both warranted and necessary. Unfortunately, the analysis presented does not justify these conclusions

The main limitation of the study is the paucity of information we are given about the patients and about the exercise tests; we are told only that the results of tests are positive or negative on the basis of the presence of ≥ 1 mm of horizontal or downsloping ST depression at the J point plus 1.5 mm. Patients are not currently managed solely on the basis of the presence or absence of ST depression on exercise testing. This information is combined with the presence and severity of symptoms in daily life and the level of exercise at which symptoms and ST changes occur on the treadmill. Current practice would not regard as reassuring an exercise test that was negative for ST changes in a

patient with poor effort tolerance or severe symptoms. This additional information was almost certainly available to the authors, and they have not done their study justice by excluding it from the analysis.

A further, more minor consideration is the nature of the perfusion imaging used. This could also have been improved. It is well established that tomographic imaging is superior to planar imaging; had this been combined with pharmacological stress, thus eliminating the proportion of patients inadequately stressed by exercise, then the predictive value of the radionuclide studies might have been even higher.

More information is required before this study can be used to justify widespread replacement of exercise testing with radionuclide imaging after myocardial infarction.

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Authors' reply

Editor—Andrew Archbold and colleagues comment that the positive predictive value of thallium-201 imaging for cardiac events was only 49%. This was due to the limited length of the follow up and was fully discussed in our paper. The point to note is that the presence of reversible defects is a marker of increased risk.

We did indeed exclude some high risk patients, including those who had events before imaging. All patients deemed clinically to be at high risk, however, would have been investigated anyway. Additionally, although the studied patients were clinically stable, they can hardly be called low risk, having a 37% incidence of events. Thallium-201 imaging should ideally, however, be performed before discharge.

End points requiring admission—both "ischaemic" and others that may seem to be non-ischaemic—represent adverse outcomes to be averted. Moreover, although heart failure may be due to left ventricular remodelling, patients with ischaemic hibernating myocardium may also develop heart failure unless an intervention is carried out[1]; additionally, myocardial ischaemia itself can precipitate heart failure.[2] Hence the ability to detect diverse cardiac events is an advantage rather than a limitation.

We agree with Simon Grant and colleagues that patients are not judged solely on the basis of ST segment changes. However, patients who had appreciable symptoms were excluded because they would already have undergone invasive investigations. The patients we studied were clinically stable, and symptoms during daily activities were uncommon.

We assessed objective markers of jeopardised myocardium and hence concentrated on ST segment changes and reversible perfusion abnormalities. A short exercise time may reflect ischaemia, poor left ventricular function, or just poor effort by the patient and is subjective. Admittedly, chest pain without ST segment change was not considered to be a positive response. This occurred in just seven patients, three of whom had events. Adding these to the 33 patients with positive results by ST segment criteria still would not have made the exercise test predictive.

We acknowledge that we did not use thallium-201 single photon emission computed tomography. Interestingly, however, most previous prognostic studies have used planar thallium-201 imaging.[3] A salient feature that may have been overlooked is that we used a new imaging protocol—imaging during stress and during rest (with nitrate enhancement)—which detects regions of inducible ischaemia and hibernating myocardium[1] and thus expands the usefulness of this technique.

Overall the main message remains unchanged: it is time to stop relying on exercise electrocardiography and to look towards objective imaging techniques to assess risk after acute myocardial infarction.

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1 Senior R, Glenville B, Basu S, Sridhara BS, Anagnostou E, Stanbridge R, *et al.* Dobutamine echocardiography and TI-201 imaging predict functional improvement after revascularisation in severe ischaemic left ventricular dysfunction. *Br Heart J* 1995;74:358-43.
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Varicella vaccine in pregnancy

Testing should be offered to women without a history of chickenpox

Editor—Daniel S Seidman and colleagues' editorial on giving varicella vaccine to non-immune women to prevent infection during pregnancy addresses an area of growing clinical concern.[1] The potential consequences to the fetus resulting from chickenpox in pregnancy are serious.[2] The problem stems from a rise in the incidence of primary chickenpox in adults, probably as a result of increased personal living standards delaying exposure to the virus.[3] In Northern Ireland over the period 1 January 1995 to 31 August 1996 we established the laboratory diagnosis of acute chickenpox in 28 patients (average age 23 years). The clinical presentation was atypical in several patients, including a 35 year old woman with secondary chickenpox, in whom varicella zoster virus was recovered from an isolated abdominal maculopapule. Eight of the patients (average age 25) were women in the first 20 weeks of pregnancy.

Table 1 Seroprevalence of varicella zoster virus among female health care workers aged 16-45 without a history of chickenpox, Northern Ireland, 1 January 1995 to 31 August 1996

Age (years)	No tested	No (%) not immune to virus
16-25	938	59 (6.3)
26-35	1365	100 (7.3)
36-45	581	15 (2.6)
Total	2884	174 (6.0)

The results of a cost-benefit analysis of screening for varicella zoster virus IgG versus blanket vaccination of women wishing to become pregnant will depend on the local seroprevalence of the virus. We undertake pre-employment screening of healthcare workers in Northern Ireland, which has a population of roughly 1.5 million and 24 000 births a year; we screen those without a history of chickenpox (currently 30%) for immunity to varicella zoster virus. Extrapolation from this group has allowed us to estimate the seroprevalence of varicella zoster virus among antenatal patients. Table 1 shows our results for female healthcare workers over 20 months. The proportion of male healthcare workers lacking evidence of immunity to varicella zoster virus is similar, and for both sexes the proportion lacking evidence of immunity would be expected to be less than 5% among those with a history of chickenpox.

We have taken the cost of varicella zoster virus IgG assay during the period to be £6 and have assumed that the cost of the vaccine when licensed would be £24. On the basis of these figures, if a policy of testing women without a history of chickenpox was introduced it would cost about £43 200 to screen 24 000 pregnancies a year. For now it seems reasonable to ask women at their first antenatal visit whether they have a history of chickenpox and to offer a varicella zoster virus IgG test to those without such a history. This would allow those found not to be immune to be counselled on the risks of exposure to patients with active chickenpox and would avoid the difficulties of managing contacts after the event.

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1 Seidman DS, Stevenson DK, Arvin AM. Varicella vaccine in pregnancy. *BMJ* 1996;313:701-2. (21 September.)
2 Enders G, Miller E, Cradock-Watson J, Bolley I, Rindhalgh M. Consequences of varicella and herpes zoster in pregnancy: prospective study of 1739 cases. *Lancet* 1994;343:1548-51.
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Varicella zoster immunoglobulin should be given after exposure to the virus

Editor—Daniel S Seidman and colleagues' editorial on varicella vaccine in pregnancy contains an important inaccuracy: it states

that passive immunisation within 96 hours of exposure has not been shown to prevent intrauterine transmission or to alleviate fetal infection.[1] In a large prospective study of chickenpox in pregnancy, however, no cases of congenital varicella syndrome or chickenpox in infancy occurred among the 97 pregnancies in which maternal chickenpox occurred after post-exposure prophylaxis with varicella zoster immunoglobulin.[2] Specific IgM antibody was found in one (1%) of 89 samples of cord blood tested, compared with 76 (12%) of 615 samples from asymptomatic infants whose mothers did not receive prophylaxis ($P = 0.003$, χ^2 test with Yates's correction).

On the basis of this evidence, pregnant women who are exposed to varicella zoster virus should be encouraged to seek medical advice. Their immune status should then be ascertained and varicella zoster immunoglobulin given to those found not to be immune to the virus; this will attenuate the attack of maternal chickenpox and, for women in the first 20 weeks of pregnancy, decrease the risk of fetal infection.

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1 Seidman DS, Stevenson DK, Arvin AM. Varicella vaccine in pregnancy. *BMJ* 1996;313:701-2. (21 September.)
2 Enders G, Miller E, Cradock-Watson J, Bolley J, Ridehalgh M. Consequences of varicella and herpes zoster in pregnancy: prospective study of 1739 cases. *Lancet* 1994;343:1547-50.

Methods of surveying patients' satisfaction

Patients should help decide the wording and design of questionnaires

EDITOR—Geoff Cohen and colleagues report their study of the consistency of methods of surveying patients' satisfaction that are used in the evaluation of services.[1] The authors suggest that choice of wording was one possible reason for the underestimation of dissatisfaction with certain elements of the service. They suggest cross calibration of satisfaction surveys as a method of improving the reliability of results.

We recommend that patients should help decide on the wording and design of the questionnaire itself.[2] In a recent survey of patients' satisfaction in this hospital, after consultation with our patient advisory council the proposed questionnaire was modified to make it more easily understandable. It is also important that patients, and not research staff or those who provide services, should determine the dimensions used to assess quality of services.[2]

One page questionnaires were circulated to the patient advisory council and a small cohort of patients on the wards; the recipients were asked to list the most important issues for them in the maternity service and their requirements for a good service. Feedback from this initial research allowed the final questionnaire circulated in the hospital to give priority to dimensions of the quality of the service determined by patients. In a similar fashion a focus group of staff and a small cohort of partners of patients at the hospital were consulted. The final questionnaires circulated to patients, partners, and staff were similar, although some minor modifications were necessary. Comparing dimensions defined by patients and patients' satisfaction scores with dimensions defined by staff and families and with staff and families' satisfaction scores may be useful for evaluating the quality of services and improvement.

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1 Cohen G, Forbes J, Garraway M. Can different patient satisfaction survey methods yield consistent results? Comparison of three surveys. *BMJ* 1996;313:841-4. (5 October.)
2 Gavin KT, Conway J, Glynn WJ, Turner MJ, Brannick T. Designing a measurement instrument for evaluating quality in the maternity services: a composite view involving internal and external customers. *QUIS 5, advancing service quality: a global perspective*. St John's University, New York: International Service Quality Association, 1996:143-53.

Patients' satisfaction is based firmly on their expectations

EDITOR—Geoff Cohen and colleagues suggest that statements in surveys on patients' satisfaction yield different results if they are worded positively rather than negatively.[1] This is entirely feasible. One way to avoid the distinctions between positive and negative statements and all the biases that these might pose, however, is to present the item as a statement of the type: "The amount of information the doctor gave me was...." The patient then selects one of a range of responses, such as from "Very satisfactory" to "Very unsatisfactory." We have used this method to develop an instrument for measuring patients' satisfaction with specialist outpatient departments among Cantonese speaking Chinese patients in Hong Kong.

Using a bottom up approach of first identifying from patients their areas of concern and then carrying out factor and regression analyses, we derived a 20 item instrument and a shorter, nine item version. We found that the domains identified as being most influential in overall satisfaction were the quality of relationship and communications, firstly with doctors and secondly with nurses. Key items in these domains address how the doctor gives information, the doctor's manner and the respect that he or she shows, the doctor's involvement, and the doctor's overall attitude. This finding concurs well with those of studies in Britain as far back as the 1970s in which communication and relationship domains were reported as most important.[2 3] The fact that these same domains are found in an Asian culture with use of a method known to minimise cultural bias emphasises the universal role that good communications and a caring relationship have in achieving patient satisfaction. We also found that the demographic composition of the samples used to test the instrument had a disproportionate effect on the satisfaction reported; we needed to calculate a scaled score to take into account several factors, including first or subsequent visit, age, sex, type of specialist outpatient department clinic, and even time of visit.

One clear conclusion that emerges from this is that satisfaction is based firmly on expectations and that expectations differ. Hence the differences that Cohen and colleagues found according to whether positive or negative wording was used in the surveys may reflect different impacts of the two question forms on expectations.

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1 Cohen G, Forbes J, Garraway M. Can different patient satisfaction survey methods yield consistent results? Comparison of three surveys. *BMJ* 1996;313:841-4. (5 October.)
2 Ley P. Psychological studies of doctor-patient communication. In: Rachman S, ed. *Contributions to medical psychology*. Vol 1. London: Pergamon, 1977.
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Deprivation payments to general practitioners

Scores are calculated relative to national average

EDITOR—I agree with F Azeem Majeed and colleagues that a shift to a system of payments based on enumeration districts targets deprivation payments more sensitively and, from the point of view of implementation, stabilises total practice payments.[1 2] Changing the areal unit on which the score is calculated (that is, enumeration district versus ward) does not, however, address the important underlying issue of change in social composition over time and its impact on general practitioners' workload and deprivation payments.

A fundamental reason for change in the underprivileged area score for a ward stems from the method of calculation, whereby the score is relative to the national average. Thus for a particular ward, even if the sociodemographic profile of its residents and its physical boundaries remain exactly the same from one census to the next, whether it qualifies for payment will depend on the position of that ward with respect to all other wards in Britain. Since the score is not linked to a baseline level of entitlement (such as the 1981 national means of the eight factors in the score), a practice that qualified for additional payments for its patients living in a deprived ward in 1981 might cease to qualify in 1991 not because the ward has, say, fewer lone parents or unemployed people but because a rise in the national percentages of these factors means

that the ward scores lower on the underprivileged area index. Thus while the score takes into account the relative change in the distribution of deprivation between wards, it makes no allowance for the absolute change in the social factors that contribute to increasing workload in general practice.

Using the means and standard deviations of the eight factors in England and Wales in 1981 as a baseline, colleagues and I have shown that in 1991 the national score was 5.6 points higher than that in 1981—that is, relative to 1981 the distribution of scores had shifted towards higher scores.[3] Overall, general practitioners had to be in a more deprived area in 1991 to get the same level of payment as in 1981.

Furthermore, if the total general medical services allocation for deprivation payments remained frozen at the current level the redistributive nature of the proposal to lower the cut off point for payment would inevitably reduce the amount received by practices in the most deprived areas.[4] It remains questionable whether even the current levels of payment are adequate in compensating for the higher morbidity and excess workload in such areas.

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1 Majeed FA, Martin D, Crayford T. Deprivation payments to general practitioners: limitations of census data. *BMJ* 1996;313:669-70. (14 September.)
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4 Hobbs R, Cole T. Deprivation payments revisited (again). *BMJ* 1996;313:641-2. (14 September.)

Payments bear little relation to practices' actual level of deprivation

EDITOR—We support Richard Hobbs and Tim Cole's plea that the methods of determining deprivation payments for general practice should be revised.[1] Clearly, basing payments on ward data will produce a situation whereby there is a large degree of inequity in the distribution of deprivation awards to general practitioners. In our district the relation between the average level of deprivation in general practices and deprivation payments is extremely low.

We have developed a practice profiling approach, whereby each practice's share of every enumeration district is worked out by use of individual postcodes. A practice deprivation score can be estimated by a weighted averaging of the deprivation scores across enumeration districts based on 1991 census data. We tend to use Townsend scores rather than the Jarman index as an indicator of deprivation, but the choice of indicator should not affect the relation between deprivation and deprivation payments to any extent.

We have examined the relation between the average Townsend score for each of the 61 practices in the area during 1994-5 and the proportion of patients for whom the practices receive deprivation payments. Twenty six practices receive no deprivation payments, and the highest payments are made to one practice in which 45% of patients attract deprivation fees. Analysis of the correlation between the Townsend score and the proportion of patients attracting payments showed little or no relation (Spearman's rank correlation = 0.08, P = 0.8). When practices were divided into fifths of deprivation the following percentages of patients attracted deprivation payments: 0.05%, 0.06%, 3.5%, 8.2%, and 7.9%. It might be expected that the five practices with the highest proportion of patients attracting deprivation payments would be in the most deprived group, but two (numbers 1 and 4) were in the fourth most deprived group and one (number 3) was in the middle group.

We conclude that deprivation payments to general practices as currently constituted bear little relation to the actual level of deprivation in the practices, and thus we add our voices to the calls for a revision of the scheme.

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1 Hobbs R, Cole T. Deprivation payments revisited (again). *BMJ* 1996;313:641-2. (14 September.)

Standard of service provided by practice should also be taken into account

EDITOR—We share Richard Hobbs and Tim Cole's view that the system of deprivation payments to general practitioners is long overdue for an overhaul.[1] We also agree with F Azeem Majeed and colleagues that calculation of the payments on the basis of enumeration districts rather than electoral wards would produce a more equitable distribution of funds.[2]

The persisting lack of clarity about the purpose of deprivation payments is unhelpful. It remains a matter of debate whether deprivation payments are a personal reward for general practitioners or for the development of better services for deprived populations with greater health needs. The sums involved can be large: in Lambeth, Southwark and Lewisham Health Authority 11% of money for general medical services (£3.8m) is accounted for by deprivation payments. Some worrying indicators suggest that in some circumstances deprivation payments may act as a perverse incentive against the interests of better primary care services.

In Lambeth, Southwark and Lewisham Health Authority there is an association between higher deprivation payments and larger list sizes. Of 13 general practitioners each receiving more than £20 000 in deprivation payments, nine had list sizes of over 3000 and only one had a list of under 2000. Patients in more deprived areas are likely to have poorer health and more complex social problems; having a shorter amount of general practitioner's time per patient in such areas seems unlikely to be conducive to better health.

We believe that deprivation payments can have an enormous impact on the development of good practice but can at the same time act as an incentive to build up a large list without any increase in the range or quality of services offered to patients. At present, a surgery in a "lock up shop" offering a bare minimum of general medical services could receive deprivation payments identical with those received by a well developed practice in the same area offering an extended range of services to patients and achieving targets for immunisation and screening in difficult circumstances. Payments for achieving targets for smear tests and vaccination may not be seen as a great incentive when deprivation payments far exceed the payment for top targets.

Several actions would increase the beneficial impact of deprivation payments on health: calculating entitlement to payments on the basis of the smaller population unit of the enumeration district, replacing threshold steps of deprivation by a graduated system of entitlement, and allocating a proportion of payments by the local health authority on the basis of the standard of service achieved by the practice.

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1 Hobbs R, Cole T. Deprivation payments revisited (again). *BMJ* 1996;313:641-2. (14 September.)
2 Majeed FA, Martin D, Crayford T. Deprivation payments to general practitioners: limitations of census data. *BMJ* 1996;313:669-70. (14 September.)

Scores should be based on enumeration districts, and payments should be phased in gradually

EDITOR—Richard Hobbs and Tim Cole and F Azeem Majeed and colleagues discuss the inequalities in deprivation payments.[1 2] There will inevitably be a discontinuity in the payments when the census data used for calculating the underprivileged area score, on which the payments are based, change from one census to the next—for example, from 1981 to 1991. The relative effect of these changes would be reduced, however, if, firstly, the payments were based on enumeration district scores (enumeration districts contain about 500 people) rather than ward underprivileged area scores (wards having about 5000 people)[3] and, secondly, they were phased in gradually rather than abruptly at a cut off of 30 for the ward underprivileged area score, which is the current practice.

Patients registered with general practitioners are located to their ward or enumeration district of residence by means of their postcode. At the time of the 1981 census there was about a 50% error in doing this, but at the 1991 census the ability to locate addresses to enumeration districts

was almost 100% accurate. A problem in using enumeration district scores is that, because the population is smaller than in wards, the confidence intervals for the unskilled variable (which is based on a 10% sample of occupations coded at the time of the census) are wide. This problem could be overcome either by substituting the ward value for the enumeration district or by the Office of National Statistics coding all occupations (as happens in Northern Ireland).

Payments could be phased in by use of narrower bands of underprivileged area score for payments. The starting point for payments, based on ward underprivileged area scores, should be between 10 (the point at which more than half of a sample of general practitioners considered themselves to be in a deprived area)[4] and 16 (1 SD)[5] or 20 (the cut off point in the General Medical Services Committee's policy). This would also be a good opportunity to increase deprivation payments above the existing levels. The Department of Health might require general practitioners receiving additional payments to show evidence of realistic plans for the provision of additional services targeted at deprived patients.

One "political" problem that might arise if underprivileged area payments were extended is that a higher proportion of the population would be identified as living in deprived areas and of course not everyone in these areas is necessarily deprived. It therefore might be an advantage to change the name used for deprivation payments; one suggestion has been "social area payments."

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1 Hobbs R, Cole T. Deprivation payments revisited (again). *BMJ* 1996;313:641-2. (14 September.)
2 Majeed FA, Martin D, Crayford T. Deprivation payments to general practitioners: limitations of census data. *BMJ* 1996;313:669-70. (14 September.)
3 Crayford T, Shanks J, Bajekal M, Langford S. Analysis from inner London of deprivation payments based on enumeration districts rather than wards. *BMJ* 1995;311:787-8.
4 Lorentzon M, Jarman B, Bajekal M. *Royal College of General Practitioners inner city taskforce report*. London: Royal College of General Practitioners, 1994. (Occasional paper 66.)
5 Jarman B. Identification of underprivileged areas. *BMJ* 1983;286:1705-9.

New group has been formed to try to bring about change

EDITOR—Richard Hobbs and Tim Cole's editorial on deprivation payments draws attention to the inadequacies of the system.[1] General practitioners from all over Britain have recently formed the group INDEX (Insist—No to Deprivation Excesses) to try to bring about change. We believe that several things are wrong with the system as it stands.

- The banding arrangements are too crude, especially at the cut off between low payments and nothing. A general practitioner with 2000 patients scoring 30 points would earn £11 300 a year more than one scoring 29.99 points
- Doctors working in pockets of deprivation within affluent wards do not receive payments, and vice versa
- Since the new census figures came into effect some general practitioners have lost payments while experiencing an absolute increase in deprivation, due to the averaging out effect.

Having consulted Professor Brian Jarman, we propose the following action to correct these anomalies, which have caused real hardship to some doctors and have created a lot of bitterness.

- Payments should start at 15 points, with banding levels of 5 points. The difference between the lowest payments and none at all would then be relatively small
- Because this would then include a large proportion of the population it would be illogical to describe all those people as deprived. Instead the payments should be called "social payments," in recognition of the differences in workload caused by social factors
- Enumeration districts should be used instead of wards for calculation of the payments as they are much smaller. This would make the payments more sensitive and fairer
- Funding should be made available for further research
- The transitional payments should be continued until any reforms come into effect.

It was encouraging to see local medical committees vote in favour of change, but we do not detect a great sense of urgency on the part of the General Medical Services Committee to bring this about; it has taken five years to get this far. We therefore plan to lobby the committee and would be interested to hear from anyone who would like to join us in this—and, equally, from anyone with other points of view. Though many of our group have lost out in some way through this system, we would not necessarily all benefit from any changes; we are more interested in finding a solution that the whole profession will feel reasonably happy with.

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1 Hobbs R, Cole T. Deprivation payments revisited (again). *BMJ* 1996;313:641-2. (14 September.)

Microbiologists are available 24 hours a day to give advice

EDITOR—I am sure that no microbiologist would dissent from Peter Wenham's view that patients infected with methicillin resistant *Staphylococcus aureus* should be treated with every consideration and a holistic approach, but it is wrong of him to support his view with incorrect assertions.[1] I am informed that the advice given to the man in the case that Wenham discusses was not from the infection control team of Wenham's hospital; I cannot conceive that any infection control team would think it appropriate to try to prevent a husband from seeing his wife on the grounds that he had been infected with methicillin resistant *S aureus*. Wenham was aware at the time that he wrote his Personal View that this was not the advice of the local team.

It would have been advisable for Wenham to check on the availability of a microbiological opinion on a Sunday before stating that one is hard to find. As in most NHS hospitals in Britain, the microbiologists at his hospital have a published on call rota through which a consultant or senior registrar (with consultant cover) is available via the hospital switchboard 24 hours a day, 365 days a year. It is precisely because problems with infection control, with their wide implications for the wellbeing of patients, staff, and the public at large, can occur at any time that microbiologists take their on call commitment extremely seriously. This is apart from the need to provide clinical advice on individual patients.

Wenham highlights the problem of needing to strike a balance between the good of individual patients and the good of the community when making and implementing infection control policy, but he should have ensured that his facts were correct.

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1 Wenham P. Bureaucracy gone mad. *BMJ* 1996;313:949. (12 October.)

⁂We received four other letters from microbiologists taking exception to Peter Wenham's comment that "microbiologists are hard to find on Sundays" and suggesting that the advice of the infection control team was misinterpreted or misunderstood.—Editor

Decision to resect an adrenal mass depends on size of mass and age of patient

EDITOR—We agree with Joohi Nasir and Christopher Walton that all patients with adrenal masses should undergo complete endocrine evaluation before adrenalectomy is considered.[1] Although after full biochemical evaluation has been carried out computed tomography provides additional valuable anatomical information, it should be remembered that other radiological investigations supply both anatomical and functional data. These investigations include not only selective venous sampling, as suggested by the authors, but also magnetic resonance imaging and scintigraphy; scintigraphy allows the hormonal activity of the mass to be assessed. Most hormonally active adrenal masses should be surgically resected,[2 3] but the case described by the authors shows the potential pitfalls of inadequate assessment.

The risk of malignancy in an adrenal mass increases with the size of the lesion. As 90% of adrenocortical carcinomas are greater than 6 cm in size and adenomas of this size are extremely rare, adrenal masses of 6 cm or more should be resected.[2] The issue of how to manage a functionally inactive adrenal mass of less than 6 cm remains controversial. Various cut off sizes for surgical resection down to 2.5 cm have

been recommended,[3] but primary adrenal malignancies are rarely less than 5 cm.[4] Size is a pivotal factor in the decision to carry out surgery, but age is also a consideration as the frequency of adrenal nodules and the ratio of benign to malignant masses increase with age.[2] Furthermore, pursuing a conservative policy in younger patients requires prolonged observation and multiple laboratory and radiological investigations.[2] Thus a policy of resecting adrenal masses of 3-6 cm in patients under the age of 50 and observing those in patients over this age would seem reasonable, especially in this era of minimally invasive laparoscopic adrenalectomy.[5]

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1 Nasir J, Walton C. Adrenal mass with virilisation: importance of endocrine investigation. *BMJ* 1996; 313:872-3. (5 October.)
2 Staren ED, Prinz RA. Selection of patients with adrenal incidentalomas for operation. *Surg Clin North Am* 1995;75:499-510.
3 Gajraj H, Young AK. Adrenal incidentaloma. *Br J Surg* 1993;80:422-6.
4 Herrera MF, Grant CS, van Heerden J, Sheedy PF, Ilstrup DM. Incidentally discovered adrenal tumors: an institutional perspective. *Surgery* 1991;110:1014-21.
5 Gagner M, Lacroix A, Prinz RA, Bolte E, Albala D, Potvin C, *et al.* Early experience with laparoscopic approach for adrenalectomy. *Surgery* 1993;114:1120-5.

Pessimistic views of the NHS

Doctors should stop seeing themselves as victims

EDITOR—The two Personal Views in the issue of 16 November represent the apotheosis of the victim mentality.[1 2] As past president of the Royal College of Surgeons, Norman Browse could have done something worth while about his view that clinicians rather than purchasers should lead in the NHS. He did not. Now he complains. Stephen Eisenstein's provincial reactionary diatribe against the patient's charter is no less encouraging.[2] Indeed, he has capitulated. He is already a victim, he writes.

By deciding to publish these tales of doom and despondency the *BMJ* is moving into the wasteland that is the impoverished mental environment of the victim mentality. Lacking leadership and inspiration from within the profession, and unable to provide and maintain high professional and ethical standards, we are now witnessing attempts to control it from without. The same is happening in the United States. None of this should be surprising, but apparently it is to most doctors, because they simply cannot see it this way. This would never have happened if the profession had ensured that there were no instances of malpractice, no instances of overcharging for services, no instances of procedures and investigations undertaken for fiscal rather than clinical reasons, no instances of consultants spending most of their time in private practice while they were receiving ever increasing salaries from the NHS, no instances of arrogant godlike behaviour by doctors and especially by consultants, no instances of empire building and pathetic in fighting, no instances of expenditure on unproved equipment and treatments, no secret merits awards or worthless research.

The good news—and there is plenty of it if you only care to look—is that the NHS would long since have collapsed if it were not for the stalwart efforts of the majority who believe in it and work their backsides off to keep it going. These people do not have the victim mentality. They are movers, shakers, and doers. I ask the *BMJ* to cut out the whingeing. It should use its influential position to help take the medical profession forward into the next century in a spirit of optimism and joy. It should encourage and publish quality and optimism, not examples of that peculiarly English disease called gloom and despondency. Let's stop blaming other people and behaving like victims and take the lead by example and positive thinking. There is a choice.

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1 Browse N. Clinicians must lead. *BMJ* 1996;313:1268. (16 November.)
2 Eisenstein S. The chartered patient and the damaged doctor. *BMJ* 1996;313:1268-9. (16 November.)

Joint commissioning is the way forward

EDITOR—I could not disagree more with the Personal View of Norman Browse, in which he says that consultants rather than purchasers should lead in the NHS.[1] Consultants are far too close to what they do to lead the hospital service and are far too narrowly based. The instinct of any half decent consultant in a cash starved service such as the NHS must be to fight for more money so that he or she can provide as high quality a service as possible. This instinct should be encouraged and rewarded; it is an important element in extracting as much money as possible from the small pot available. Purchasing health care for a community, however, must be done by generalists who can see the wider picture.

In the days before the reforms, when consultants were less constrained, I watched in disbelief at the way in which limited NHS money was spent. In one hospital a bright, talented, and aggressive young surgeon pushed to get money to start a liver transplant service; meanwhile, the geriatric unit was desperately in need of long stay beds, and the occupational therapy service was virtually non-existent. In another hospital a consultant with skill and interest in neonatal intensive care managed to squeeze money from other budgets as well as from the public to open several intensive care cots. In a unit with two consultants, four junior senior house officers, no middle grade staff, and a big teaching hospital 16 km away was this the best use of resources?

I am not saying that innovation and research should be stifled, but, in an NHS that is so short of money, planning must be at a level where more strategic issues can be addressed. I am not an apologist for fundholding, although I think that those of us who are general practitioners have something to contribute, if only an intimate knowledge of the needs of our microenvironment. Joint commissioning with general practitioners, public health physicians, managers, and specialist purchasers seems to me to be the way forward, with innovation and research funded separately in specialist teaching units. Rationing in the NHS is a reality; priorities must be openly discussed and debated if a truly equitable service is to be provided.

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1 Browse N. Clinicians must lead. *BMJ* 1996;313:1268. (16 November.)

First patient was airlifted from highlands of Scotland in 1933, not 1953

EDITOR—The first page of the Medicine and Books section in the issue of 2 November (p 1154) contains a photograph taken from the book *Air Ambulance*. The legend to this photograph states that "The first patient, with a perforated peptic ulcer, was airlifted to Glasgow in 1953."

I was medical officer for the parish of Kildalton and Oa, Isle of Islay, from 1928 until the outbreak of the second world war. I sent a patient with a perforated peptic ulcer by air to Glasgow in 1933; I accompanied the patient because neither his wife nor anyone else was able to do so at the time. I suggest that the date given in the legend is wrong and should have been 1933.

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Advice to authors

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

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

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

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

Letters will be edited and may be shortened.