Letters

Prophylaxis after occupational exposure to HIV

Portsmouth has 24 hour hotline staffed by nurse specialists

EDITOR—Easterbrook and Ippolito discuss prophylaxis after occupational exposure to HIV.[1] Ignorance about this problem among the medical profession is still considerable. Although the risk of occupational exposure to HIV is rare, every hospital should have a written policy on how to manage healthcare workers after needlestick injury or exposure to body fluids.

In Portsmouth, many years ago, a healthcare worker acquired HIV infection after a needlestick injury despite receiving zidovudine. Since then the department of genitourinary medicine has developed an efficient service, providing a 24 hour hotline for all healthcare workers employed by the trust and community and also for members of the general public who have sustained needlestick injuries. The hotline is staffed by nurse specialists experienced in HIV infection, who are overseen by the genitourinary physicians. This enables the person who sustained the injury to access care immediately and to discuss with experienced counsellors not only HIV infection but also infection with hepatitis B and hepatitis C viruses, in a confidential manner. Appropriate treatment can also be started by the nurse specialist without delay. Since all patients with HIV infection in Portsmouth are treated by the genitourinary physicians, the nurse specialists are fully aware of their antiretroviral treatment and the stage of their disease. The consultants hold weekly updates about all the patients and also discuss all new developments in the field of HIV infection with the nurse specialists.

This model of care is superior to the one described in the editorial, which suggests that the assessment and treatment should be initiated in the accident and emergency department. There is a rapid turnover of junior medical staff in accident and emergency departments, who will find it difficult to keep up to date with all the advances in antiretroviral treatment. Though a written protocol gives an overall prescription pattern, one should consider the antiretroviral treatment of the source patient before deciding on the appropriate treatment. Risk assessment and counselling are also difficult for inexperienced staff.

In Portsmouth, genitourinary medicine staff provide an on call service for HIV positive patients with the help of the community staff. The hotline is thus an additional service provided by the on call team. Although this service would be superior to one provided by the accident and emergency department, the cost benefit of setting up such a service when there is no dedicated on call service already in place should be evaluated.

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1 Easterbrook P, Ippolito G. Prophylaxis after occupational exposure to HIV. *BMJ* 1997;315:557-8. (6 September.)

Follow up may have to be for longer than six months

EDITOR—Easterbrook and Ippolito recommend that staff who have been exposed to HIV should be followed up for at least six months after receiving post-exposure prophylaxis.[1] Ridzon et al recently reported on a nurse who declined post-exposure prophylaxis after a needlestick injury but subsequently seroconverted after an interval of between eight and nine and a half months.[2] The general implications of this with regard to testing for HIV antibody are far reaching, but seroconversion would probably be delayed if post-exposure prophylaxis was not successful.

Thus people given post-exposure prophylaxis should probably be followed up for much longer than the authors recommend and, in addition, advised about practising safe sex throughout this time. Presumably healthcare workers who have been occupationally exposed should also consider avoiding exposure prone procedures.

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1 Easterbrook P, Ippolito G. Prophylaxis after occupational exposure to HIV. *BMJ* 1997;315:557-8. (6 September.)
2 Ridzon R, Gallagher K, Ciesielski C, Mast EE, Ginsberg MB, Robertson BJ, et al. Simultaneous transmission of human immunodeficiency virus and hepatitis C virus from a needle-stick injury. *N Engl J Med* 1997;336:919-22.

Register of cases of occupational exposure exists

EDITOR—Contrary to what Easterbrook and Ippolito suggested in their editorial on prophylaxis after occupational exposure to HIV,[1] the Public Health Laboratory Services' Communicable Disease Surveillance Centre and the Scottish Centre for Infection and Environmental Health are undertaking surveillance of healthcare workers occupationally exposed to blood-borne viruses.

Guidelines on Post-Exposure Prophylaxis for Health Care Workers Exposed Occupationally to HIV, published by the Department of Health last year, details this reporting system and recommends that all significant occupational exposures are reported to the Communicable Disease Surveillance Centre and the Scottish Centre for Infection and Environmental Health.[2] Our register was developed further when these guidelines were published. The aim is to follow up significant occupational exposures to HIV and hepatitis B and C viruses to examine the circumstances in which they occurred, what the post-exposure management was, and, in the case of HIV, what the side effects and outcomes of any post-exposure prophylaxis were. The Communicable Disease Surveillance Centre has enlisted the help of occupational health departments, but anyone wanting more information should contact Juliet Baker at the Communicable Disease Surveillance Centre (0181 200 6868 ext 4573) or Fiona Raeside at the Scottish

Advice to authors

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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, whether typed or sent by email, should give each author's current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

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Letters will be edited and may be shortened.

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1 Easterbrook P, Ippolito G. Prophylaxis after occupational exposure to HIV. *BMJ* 1997;315:557-8. (6 September.)
2 Expert Advisory Group on AIDS. *Post-exposure prophylaxis for health care workers exposed occupationally to HIV.* London: Department of Health, 1997.

"Source testing" should be allowed

EDITOR—Management after occupational exposure to HIV has ranged from no action to the use of single agent zidovudine, and now the Department of Health has recommended triple therapy.[1] In their editorial Easterbrook and Ippolito[2] raise the issues of recommendations based on indirect evidence such as a retrospective case-control study,[3] animal models, biological plausibility, and the use of zidovudine to reduce the risk of vertical transmission. All this work is based on the use of zidovudine as a single agent. In the light of current practice this has been extrapolated to recommendations based on triple drug regimens.

Easterbrook and Ippolito sound a note of caution regarding the use of toxic drug regimens and point out that the American guidelines advocate triple therapy only for high risk exposures or when drug resistance is suspected while the British guidelines suggest it for all significant exposures. This divergent advice makes it even harder to offer consistent advice to healthcare workers who are confused by the debate. Anyone who has had personal experience of a needlestick injury, or has had to deal with such situations, knows how difficult a time this is to take in any information, let alone conflicting information, and come to a rational decision.

One issue that in my view has not been satisfactorily resolved by the guidelines is the issue of "source testing." The inability to determine the HIV status of the source patient without obtaining informed consent wastes time in the delivery of prophylaxis, adds uncertainty to the counselling process, and encourages the (possibly unnecessary) use of toxic and expensive drugs.

The time has come for a nationally coordinated helpline to be made available by the Department of Health. This should provide 24 hour advice for people after occupational exposure to HIV, give consistent advice, and document exposures so that further information can be gathered to inform future action. Furthermore, as knowledge of the HIV status of the "source" of the needlestick injury is crucial in determining drug treatment it is vital that the General Medical Council urgently reviews the policy regarding consent to test in this special situation.

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1 Expert Advisory Group on AIDS. *Post-exposure prophylaxis for health care workers exposed occupationally to HIV.* London: Department of Health, 1997.
2 Easterbrook P, Ippolito G. Prophylaxis after occupational exposure to HIV. *BMJ* 1997;315:557-8. (6 September.)
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Universal precautions should be used during all surgical procedures

EDITOR—Recent guidelines on the management of occupational exposure to HIV are important because of the frequency of needlestick injuries and contact with blood during surgical procedures.[1 2] At present the use of "universal precautions" is recommended during all surgical procedures,[3 4] but anecdotal evidence suggests that most surgeons use such measures only if the patient is known to be HIV positive. We investigated how common it was for operations to be carried out on HIV positive patients in Leeds before their HIV status had been determined.

A retrospective case note review was carried out for all 260 patients with HIV infection who were regularly followed up in our department between 1984 and 1997. Operations performed in the three years before the diagnosis of HIV infection were scrutinised and documented only if the patient was likely to have been HIV positive at the time of surgery, taking into consideration the CD4 count at diagnosis. We found that 24 patients had undergone a total of 28 operations under general anaesthesia (table). Twenty two of the procedures were elective and six were emergencies. Surgeons had thus been operating on patients who, unknown to them, were HIV positive. Admittedly, these cases represented only a small proportion of all operations performed, but they included major procedures such as thoracotomy, laparotomy, and hysterectomy. None of the patients had been recognised at the time of operation as being at high risk of HIV infection.

Some surgeons believe that routine preoperative HIV testing of patients would reduce the risks to staff, but this approach has several practical problems. Preoperative HIV testing is clearly impractical before emergency procedures, while for elective surgery a negative result of a test could be falsely reassuring because of the delay to the appearance of antibodies to HIV in the blood. Routine testing would also be expensive. More important still are the ethical issues raised. Would patients who refused an HIV test be denied surgery? Would HIV positive patients be refused non-essential operations? How long would it be before patients in turn insisted on regular HIV testing of medical and nursing staff?

Our survey emphasises the need for all patients to be regarded as potential carriers of HIV infection, and hence universal precautions should be used during all surgical procedures. Such precautions would provide protection not only against HIV but also against other bloodborne viruses such as hepatitis B and C viruses, both of which are more prevalent and more likely to be transmitted by percutaneous injury than is HIV.[5]

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1 Easterbrook P, Ippolito G. Prophylaxis after occupational exposure to HIV. *BMJ* 1997;315:557-8. (6 September.)
2 Expert Advisory Group on AIDS. *Post-exposure prophylaxis for health care workers exposed occupationally to HIV* London: Department of Health, 1997.
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4 Centers for Disease Control. Recommendations for prevention of HIV transmission in healthcare settings. *MMWR* 1987;36(suppl 2S):15.
5 Gerberding JL. Management of occupational exposures to blood-borne viruses. *N Engl J Med* 1995;332:444-51.

Substitution of another opioid for morphine may be useful for pain control

EDITOR—In their article on the principles of palliative care and pain control O'Neill and Fallon rightly emphasise the primacy of morphine among strong opioid analgesics in the management of cancer pain.[1] Although morphine is generally well tolerated and effective, some patients have pain that is relatively resistant to it. Increasing the dose in these circumstances may lead to the syndrome of opioid toxicity, which they describe.

Substitution of another opioid for morphine, rather than dose reduction as the authors suggest, can be an effective strategy in this situation and is commonly used in

Operations performed on HIV positive patients under general anaesthesia in three years before HIV infection was diagnosed

Procedure	No	Procedure	No
Cystoscopy	5	Thoracotomy and lung biopsy	1
Termination of pregnancy	3	Laparotomy for perforated colon*	1
Lymph node excision	2	Appendicectomy*	1
Hysterectomy and oophorectomy	1	Sphincterotomy for anal fissure	1
Cone biopsy	1	Correction of malformation of foot	1
Caesarean section*	1	Fixation of pelvic fracture*	1
Ovarian cystectomy*	1	Excision of oral ulcers and biopsies	1
Removal of testicular seminoma	1	Oesophagoscopy and laryngoscopy	1
Torsion of testis*	1	Sinus washout and drainage of tonsillar abscess	1
Circumcision	1	Eyelid correction	1
Groin dissection	1		

*Emergency operation.

North American practice.[2 3] Pharmacokinetic differences between opioids can be exploited to the benefit of patients, particularly when metabolic and excretory capacity is impaired. Experience suggests that fentanyl and hydromorphone are better tolerated by patients with limited renal function, although this has yet to be shown in a formal study. Some opioids have additional analgesic effects mediated by non-opioid receptors or mechanisms—for example, methadone at the *N*-methyl D-aspartate receptor and tramadol via effects on neuronal reuptake of serotonin and noradrenaline.

Neuropathic pain (described in the second article in the ABC series) is often relatively resistant to opioid analgesics, perhaps reflecting mediation through non-opioid pathways. Although a proportion of patients will benefit from the listed adjuvant analgesics, the dose response is unpredictable, and it may take days or weeks to reach an effective dose. A recent informal survey of doctors working in pain clinics in the United Kingdom showed widespread use of ketamine in the management of neuropathic pain. Ketamine, an antagonist of *N*-methyl D-aspartate, has pronounced analgesic effects and may be given effectively both orally and parenterally[4]; prudent estimates of the dose will usually achieve analgesia without excessive sedation or associated side effects.[5]

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1 O'Neill B, Fallon M. ABC of palliative care: Principles of palliative care and pain control. *BMJ* 1997;315:801-4. (27 September.)
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Studies of drugs in epilepsy cited by author are not evidence based

EDITOR—We were surprised to read Brodie's letter claiming the existence of "hard empirical evidence" to support a mechanistic approach in the management of epilepsy.[1] He refers to an unpublished study in which patients with partial seizures resistant to monotherapy with carbamazepine (a sodium channel blocker) were randomised to take additional valproate or vigabatrin (drugs with GABA-ergic mechanisms).[2] In those patients who responded to dual therapy, withdrawal of carbamazepine was attempted, with a view to achieving monotherapy with either valproate or vigabatrin. Altogether 7% became seizure free with valproate or vigabatrin alone, while a further 14% became seizure free with dual therapy. Because this study did not contain a placebo group or a group not receiving treatment, the results cannot provide reliable evidence that patients not responding to a sodium channel drug may respond to a drug with a GABA-ergic mechanism. Similarly, because the study did not contain a group allocated to take a second sodium channel drug (for example, lamotrigine), it cannot provide evidence that patients failing to respond to a sodium channel blocker are more likely to respond to a GABA-ergic drug than to a second sodium channel blocker. For the same reasons, it cannot provide evidence to support the hypothesis of "rational polytherapy," which deems it more rational to treat patients requiring polytherapy with drugs with differing mechanisms of action.

Brodie refers to a second open uncontrolled study, in which lamotrigine was added to carbamazepine, phenytoin, or valproate.[3] Patients taking a combination of lamotrigine and valproate seemed to respond best. This study, however, is confounded by too many factors to allow any inference with respect to a synergistic action between these two drugs.

We agree with Brodie that the evidence underpinning the management of epilepsy must be clinically relevant and scientifically credible. Currently, however, no scientifically robust data are available to allow epilepsy specialists to adopt a mechanistic approach. At best, the evidence cited by Brodie generates hypotheses that need testing in large pragmatic studies.

If we are to adopt an evidence based approach to the management of epilepsy we need to have a clear understanding of what constitutes good and reliable evidence. Epilepsy specialists will also need ready access to that evidence, and systematic reviews produced by the Cochrane Epilepsy Group should provide a valuable up to date resource for those wishing to apply an evidence based approach to the management of epilepsy.

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1 Brodie MJ. Data on results of using different antiepileptic drugs do exist. *BMJ* 1997;315:885. (4 October.)
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Search for evidence of effective health promotion

Quantitative outcome evaluation with qualitative process evaluation is best

EDITOR—Non-randomised studies are currently regarded as inferior, if not worthless, and Speller et al are right to question whether randomised controlled trials are always the best or most appropriate method of evaluating health promotion.[1] Evaluation entails quantifying worth: wellbeing is an important asset but is difficult to quantify and hence to evaluate. It is important to distinguish between "not effective" and "not evaluable."

Attribution of the effects of an intervention (and the relative costs involved) is the goal of evaluation. An insistence on randomised controlled trials ignores some of the unique features of health promotion: interventions often take place at a community or national level, the expected proportional benefits to individuals are small, and beneficial outcomes are delayed.[2] Potential contamination and confounding mean that attribution can rarely be a certainty, and even when it can be, replication is limited.

The external validity of randomised controlled trials of preventive interventions is questionable. Patients who agree to participate in such trials tend to be affluent and better educated and to adopt a healthier lifestyle than those who do not participate (A R Britton et al, unpublished systematic review). Moreover, most health promotion interventions involve individual behaviour change, so the use of blinding techniques may be impossible. This has implications for the effect of patient preference on the result.[3] The value of randomisation in ensuring internal validity is unquestionable, but such trials are not always an appropriate design in health promotion. Therefore, lack of evidence from randomised controlled trials should not be viewed as a failure in the quality of research; rather, more attention should be given to refining and strengthening other trial methodologies (community trials, before and after trials) and incorporating these appropriately into the evidence base.

We do not dispute that, where possible, randomised controlled trials should be conducted and incorporated into systematic reviews. Nor do we dispute that systematic reviews are an important tool for judging evidence. We cannot support Speller et al's argument that reviewers should exclude interventions simply if they consider them poorly conceived—this would mean a return to the bad old days of the "expert review."

An increased use of qualitative methods is needed to complement quantitative research, but, more than that, there should be a new integration of both methodologies within dynamic multicausal models.[4] The focus on effective outcomes too often ignores the process of an intervention. Quantitative outcome evaluation combined with qualitative process evaluation may be a way forward in understanding the interrelation between people's behaviour and the social structure within which they live.

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1 Speller V, Learmonth A, Harrison D. The search for evidence of effective health promotion. *BMJ* 1997;315:361-3. (9 August.)
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Systematic reviews include studies other than randomised controlled trials

EDITOR—Health promotion specialists seem threatened by rigorous systematic review methods that attempt to judge the value of theories and practice in the light of the data rather than by what is fashionable.[1] The approach sometimes used by the International Union for Health Promotion and Education (discussed by Speller et al), involving the selection of about 10 favourite studies, is interesting but provides insufficient basis for policy. Systematic reviews are an improvement on the casual way in which health promotion has been assessed. Health promotion practitioners often base their practice on opinion, received wisdom, or a favoured theory, occasionally supported by selective reference to a few studies of variable quality which rarely assess health outcomes. To deny the centrality of examining the effect of health promotion on health related outcomes at a personal or community level (regardless of whether one is using a traditional medical or more holistic definition of health) is to raise serious questions about the legitimacy of some health promotion activity.

The authors are correct to emphasise the importance of looking at how interventions are delivered (even though Speller et al's own review on childhood accidents gives no details about process),[2] but they wrongly assert that this is ignored in our reviews. Our guidelines highlight the importance of a qualitative approach in assessing the effectiveness of interventions.[3]

Speller et al mistakenly assume that systematic reviews include only randomised controlled trials and that these exclude the use of qualitative methods. The guidelines from the NHS Centre for Reviews and Dissemination do not prescribe the research designs to be included in a review. Several of the cited reviews include studies that have used other designs appropriately. The authors' reflex rejection of a key role for experimental designs ignores a growing appreciation that well designed experiments can be conducted in the community and provide less biased estimates of the impact of programmes than traditional, poorly controlled approaches.[4 5] Speller et al's article makes little contribution to our understanding about which sorts of study designs and methods are best for different purposes.

The authors accuse those who conduct systematic reviews of health promotion of drawing false conclusions which may "lead to the long term detriment of public health." However, not one example is given of a finding from any of the cited reviews that is misleading. Their critique is even more difficult to take seriously given that Speller recently undertook a paid commission from the NHS Centre for Reviews and Dissemination to disseminate the results of one of the reviews that she criticises.

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1 Speller V, Learmonth A, Harrison D. The search for evidence of effective health promotion. *BMJ* 1997;315:361-3. (9 August.)
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Debate is needed over who provides drug treatment in attention deficit hyperactivity disorder

EDITOR—Levy's editorial on attention deficit hyperactivity disorder focuses on treatment issues in Australia and the United States, which only partially generalise to the current situation in Britain.[1] Although the diagnostic criteria given in the fourth edition of the *Diagnostic and Statistical Manual of Mental Disorders* for attention deficit hyperactivity disorder and in the 10th revision of the *International Statistical Classification of Diseases and Related Health Problems* for hyperkinetic disorder are much more comparable than their predecessors were, they are not "almost identical." Hyperkinetic disorder has a much more stringent definition than attention deficit hyperactivity disorder: it is characterised by persistent traits of severe and pervasive inattentiveness, overactivity, and impulsiveness, beginning in the first five years of life.[2] The prevalence varies according to the diagnostic system used, and this influences prescription rates.

Information on prescribing of stimulant drugs in Britain is fairly sparse. A recent survey of experts in child and adolescent psychiatry, however, showed considerable agreement that methylphenidate is useful in those with hyperkinetic disorder and helpful for some children with attention deficit hyperactivity disorder who do not meet criteria for hyperkinetic disorder.[3] These views are likely to become more widespread in clinical practice, with a resultant increase in prescription rates.

Levy expresses concern about paediatricians and general practitioners initiating prescribing. These issues were also examined in the above survey. Opinion was divided on whether methylphenidate should be initiated only by child psychiatrists or whether paediatricians should do this too. Some respondents emphasised that prescribers should also be able to address psychological, educational, and family issues if necessary. The role of general practitioners in initiating prescribing was not examined, but there was a majority view that they should be able to continue prescribing and monitoring until the next specialist review.

Since parental demand for treatment exceeds mental health services' resources, it is important to obtain interested paediatricians' views about initiating treatment and developing consensus guidelines for clinical practice. Currently specialists are more likely to be working in parallel rather than jointly, and services may be duplicated in some regions, with general practitioners being uncertain whether to refer patients to psychiatrists or paediatricians. It may be that child psychiatrists, paediatricians, and general practitioners have complementary roles in the management of attention deficit hyperactivity disorder, but this requires debate among the disciplines involved.

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1 Levy F. Attention deficit hyperactivity disorder. *BMJ* 1997;315:894-5. (11 October.)
2 Taylor E, Hemsley R. Treating hyperkinetic disorders in childhood. *BMJ* 1995;310:1617-8.
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Community based heart health promotion project in England

Self reporting overestimates smoking cessation rates

EDITOR—Baxter et al state that their community intervention led to a 6.9% difference in smoking rates.[1] Quitting was defined by self report in questionnaires. In previous studies, reported smoking cessation has been reduced when self reporting has been checked against biochemical markers. For example, in the OXCHECK study 30% of those who reported having stopped smoking were classified as smokers by cotinine estimations.[2] Self reported dietary change was similarly greater than actual change in serum lipid concentrations.

The effects of this intervention are likely to be considerably less than the authors claim. The authors are not justified in concluding that their intervention is superior to other forms of health promotion that have been subjected to more rigorous evaluation.[2]

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1 Baxter T, Milner P, Wilson K, Leaf M, Nicholl J, Freeman J, et al. A cost effective, community based heart health promotion project in England: prospective comparative study. *BMJ* 1997;315:582-5. (6 September.)
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Authors' conclusions are unjustified and misleading

EDITOR—Baxter et al conclude that Rotherham's heart health promotion project was so successful that "the estimated cost per life year gained was £31."[1] If this were true it would be a stunning finding and the rapid redirection of a considerable bulk of health service resources should follow. Unfortunately, the evidence for their claim is thin.

The evaluation was based on a before-after comparison in two intervention communities and one control community. These communities were not well matched, with the control community being more deprived and having a higher unemployment rate and higher mortality from coronary heart disease at baseline. The clear evidence of widening socioeconomic differentials in smoking would, therefore, be expected to produce an apparent beneficial effect of the project on smoking. Indeed, social class differences in smoking data from the 1991 and 1995 health surveys for England show that the prevalence of current smoking did not fall in social classes IV and V, whereas in social classes I and II combined, relative percentage declines in the prevalence of current smoking of 10% and 18% occurred among men and women respectively (table). The differential trends in smoking habits found between the intervention and control communities may be explained by confounding by social circumstances.

The eagerness of the authors to claim an effect of their project on health related behaviours is, unsurprisingly, not applied to the unemployment rate. In their discussion they state that control and intervention areas experienced similar declines in unemployment. In fact there was a 29% greater decline in the intervention areas than in the control area—an even better effect than with smoking. What is being observed is probably simple drift associated with different characteristics of the areas. If the intervention and control areas had been switched the intervention would probably have apparently produced a 24.5% relative increase in smoking and a 29% relative increase in unemployment. The authors would have been unlikely to conclude that the cost effectiveness of the project was £31 per life year lost.

Well designed and properly evaluated health promotion projects that deployed considerably greater resources than the project under consideration show limited signs of effectiveness.[2] Baxter et al's interpretations are another example of the King Canute principle in health promotion[3]: insist that the tide will go out after it has turned and then accept the credit. Rather than see this study as a proof of the effectiveness of health promotion we should take it as another demonstration of the importance of socioeconomic factors in determining health status. Perhaps it is to this domain that the focus of interventions should be turned.

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1 Baxter T, Milner P, Wilson K, Leaf M, Nicholl J, Freeman J, et al. A cost effective, community based heart health promotion project in England: prospective comparative study. *BMJ* 1997;315:582-5. (6 September.)
2 Ebrahim S, Davey Smith G. A systematic review and meta-analysis of randomised controlled trials of health promotion for prevention of coronary heart disease in adults. *BMJ* 1997;314:1666-74.
3 Davey Smith G, Ströbele SA, Egger M. Smoking and health promotion in Nazi Germany. *J Epidemiol Community Health* 1994;48:220-3.

Authors' reply

EDITOR—Muir et al suggest that the difference in smoking rates between the control and intervention communities after the study period was less than the 6.9% that we reported because self reporting underestimates quit rates. This may be true, but the Action Heart study measured the prevalence of reported smoking and not of stopping smoking. Validation exercises on self reported data have generally confirmed that people tell the truth in population studies[1 2] unless there is a reason for them not to do so.[3] We have no reason to believe that those who reported that they were current smokers were not telling the truth.

Davey Smith and Ebrahim are concerned that the control and intervention communities were not well matched in terms of socioeconomic variables and mortality from coronary heart disease. The differences they point to, however, are minuscule and could not explain a 6.9% difference in smoking rates. It is true that unemployment fell from 11.1% to 9.6% in the intervention area and from 11.4% to 10.2% in the control area, a relative risk reduction of 25% in the intervention areas compared with the control area (not 29% as Davey Smith and Ebrahim state). Analysis of absolute numbers, however, shows a different story. If the intervention community experienced the same decline in unemployment as the control community over the study period (a 1.2% fall rather than the 1.5% actual fall) then there would only be 44 more unemployed adults in the intervention communities. Even if all 44 were smokers, probably only four would have been sampled in the Action Heart survey. This would have a negligible impact on our results overall.

The real objection is that the areas were not identical. This criticism can be applied to all community intervention trials and is as helpful as pointing out that randomised controlled trials can never offer any evidence about effectiveness in types of patients who will not consent to be randomised. Nevertheless, in the 10 randomised controlled trials involving 15 subgroups that Davey Smith and Ebrahim reviewed[4] they found a net reduction of −4.2% (fixed effects) or −2.8% (random effects) in smoking rates in health promotion intervention groups compared with controls. Winkleby et al found a non-significant reduction equivalent to −1.2% in a synthesised analysis of three large community intervention trials.[5] The evidence is that health promotion campaigns can have an effect on smoking rates and that the approximate halving of smoking rates in men in Britain in the past 25 years has not occurred as a result of "simple drift."

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1 Cartwright A. *Health surveys in practice and potential.* London: King's Fund, 1983.
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4 Ebrahim S, Davey Smith G. A systematic review and meta-analysis of randomised controlled trials of health promotion for prevention of coronary heart disease in adults. *BMJ* 1997;314:1666-74.
5 Winkleby MA, Feldman HA, Murray DM. Joint analysis of three US community intervention trials for reduction of cardiovascular disease risk. *J Clin Epidemiol* 1997;50:645-58.

Changes in prevalence of current smoking and percentage decline by social class in 1991 and 1995*

	Current prevalence of smoking (%)				Decline in current smoking (%)	
	1991		1995		1991-5	
Social class	Men	Women	Men	Women	Men	Women
I and II	22	22	20	18	10	18
III non-manual	27	29	27	26	0	10
III manual	35	35	33	30	6	17
IV and V	38	35	38	35	0	0

*Sources: Office of Population Censuses and Surveys. *Health survey for England 1991.* London: OPCS, 1993; Joint Health Surveys Unit, Social and Community Planning Research. *Health survey for England 1995.* London: Stationery Office, 1997.

Bus shelters in photograph, showing drug adverts, were replaced long ago

EDITOR—I was disappointed to see a two year old photograph of the bus shelters on the Queen Elizabeth Hospital site in the *BMJ*'s Photofinish.[1] A national voluntary agreement between tobacco companies and the advertising industry states that there should be no advertisements for tobacco products on any bus shelters anywhere in the United Kingdom. This agreement was reached shortly after last year's general election. The bus shelters are on a public road running past the hospital. Responsibility for advertising on the shelters rests with the local passenger transport authority, not the hospital.

Not only have the bus shelters not carried advertising for tobacco companies for many months but the photographs showed hospital signs that were replaced before April 1996. Therefore there is no possibility that Douglas Salmon could have taken the photographs in April 1997.

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1 Salmon D. Minerva: photofinish. *BMJ* 1997;315:1722. (20-29 December.)

⁂The photograph was sent to Minerva on 18 April 1997, and it then joined the long queue of photographs awaiting publication. Minerva had no way of knowing when it had been taken. She wrote the caption to the photograph and apologises for her error.—EDITOR

Consultants could give patients a letter summarising their consultation

EDITOR—Burkey et al state that patients in outpatient clinics value a clear message when they are being discharged, being given written and verbal information about their condition, and knowing that their general practitioner has this information.[1] I suggest that one way to address these is to write to the patient or the parents.

After every consultation I write only one letter, to the parent(s), with a copy to other parties. This includes (examples in brackets) whether I have discharged the patient ("I will see X in three months' time" or "I have not arranged to see X again"); information about the condition ("This was not caused at the time of birth, but while the baby was being formed in the womb. It was not due to anything you or anyone else did or did not do during pregnancy"); and what to do subsequently ("He needs his blood pressure checking every year because …"). With parental agreement I send a copy to the child's school if appropriate ("X can participate in all activities to the level of his abilities"); there has never been difficulty with confidentiality when I suggest this—in fact, parents welcome it. It is clear what information has been given and to whom.

I am currently conducting a survey using a modified patient satisfaction questionnaire[2] to discover, among other things, whether parents find these letters helpful. Anecdotally the letters are popular with parents and professionals as they clarify what has been said, avoid confusion, and lessen the risk of conflicting advice (though I do not know whether others support or contradict my advice).

Burkey et al did not examine whether patients' dissatisfaction with their doctors was legitimate. Communication is an essential part of every doctor's skills. When a patient is dissatisfied it may be because of poor communication or performance on the part of the doctor, but one must interpret patient dissatisfaction with caution. To assume that the fault lies with the doctor is fallacious. Most readers will have memories of holding clinics with too little experience or support and too many patients to be able to offer a quality service—a fault of the system, not the staff.

Parents may have unrealistic expectations about what I can do. The letter to parents can address that and other issues (such as, for example, that they are hostile, aggressive, or insulting) and give advice on how to maximise the efficiency of their consultations with doctors.

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1 Burkey Y, Black M, Reeve H. Patients' views on their discharge from follow up in outpatient clinics: qualitative study. *BMJ* 1997;315:1138-41. (1 November.)
2 Nguyen TO, Attkisson CC, Stegner BL. Assessment of patient satisfaction: development and refinement of a service evaluation questionnaire. *Eval Program Planning* 1983;6:299-314.

Self regulation is necessary in war on drugs

EDITOR—In their editorial on why Britain's drug czar must not wage war on drugs Strang et al strongly advocate a pragmatic approach by the government to the national and international drug problem.[1] In doing so they seem keen to protect the relative medical autonomy in treatment of addicts afforded by the "British system."[2] They call for a maintained emphasis on evidence based treatment, rehabilitation, and preventive strategies and quote the success of the needle and syringe exchanges, as well as the tolerance of injectable heroin and methadone prescribing, as examples of this pragmatic approach.

As well as advantages, however, there are surely disadvantages afforded by a system largely devoid of regulation. The most frequently expressed concern is that of widespread diversion of prescribed drugs to the black market.[3] As a profession, we seem to be doing little to rebuff such criticism. The recently published results from a survey of community pharmacies showed prescribing of injectable methadone to be as prevalent in the non-specialist as in the specialist field.[4] Indeed, relatively simple controls, such as prescription facilities for daily collection of drugs, were shown to be underused in general, and in particular in the private sector. Doses prescribed were also larger in the private sector than the NHS sector.

As prescribers, we must recall the authors' message to reduce harm to individuals and society.[1] If we are to protect our British system from legislation that is considered the norm in other countries, we must, firstly, make proper use of the simple controls available to us. Secondly, we must develop an array of biochemical tools, both qualitative and quantitative, which will allow us to monitor both use of non-prescribed drugs ("use on top") by individuals and diversion of prescribed drugs to the black market. The need for such measures has been recognised for several decades,[5] but little progress has been made. For those receiving prescriptions for long term methadone maintenance and for the minority who receive prescriptions for oral amphetamines or injectable opiates, we must ask questions such as "How much?" and "When?" as well as distinguishing the illicit from the pharmaceutical preparations.

To protect society from the diversion of prescribed substances, as well as to help the individual addict to battle the loss of control that is central to his or her death, we must have self regulation before exposing ourselves to the whims and prejudices of the new drugs czar.

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Vulval Pain Society provides information on vulval symptoms

EDITOR—Nunneley's Personal View will have struck a chord with women with chronic vulval symptoms.[1] Her story is typical of that of the many women with burning, soreness, and itching of the vulval area who fail to get recognition and appropriate treatment. By the time such women reach a knowledgeable specialist their symptoms have often lasted for months or even years without their having even received a diagnosis.[2] As Nunneley says, symptoms are often wrongly attributed to monilial infections.

The greatest failure among clinicians when it comes to vulval complaints is to trivialise symptoms. Vulval diseases are not simple skin diseases; after all, this is vulval skin that is symptomatic. The condition has far reaching implications for the woman's self esteem, sexuality, and lifestyle. Chronic pain and itching of the vulva, particularly when the woman does not understand her diagnosis and has been given inappropriate treatment, will lead to isolation, fear, and self treatment.

In 1996 we established the Vulval Pain Society, an information service for women with vulval symptoms to give unbiased, accurate information on all aspects of vulval symptoms, particularly pain. We do not encourage self diagnosis but do encourage women to work with a clinician knowledgeable in vulval diseases. Three groups of women contact us: those with longstanding symptoms who have been under clinical supervision by a specialist; women who have had vulval symptoms for many months or years but have received little or no clinical input by a clinician and have heard of our

organisation by chance; and health professionals themselves—consultant gynaecologists, dermatologists, genitourinary physicians, nurses, and health advisers, who all see these women but have run out of answers for them and write to us for information.

We provide women with a complete understanding of their condition, including ideas on treatments, ways of coping with pain, and dealing with sexuality and vulval pain; and we allow them to share their experiences through a newsletter. Several support groups exist. In essence, we provide basic information and a forum for women to become more involved with their condition. The address of the Vulval Pain Society is PO Box 514, Slough, Berkshire SL1 2BP.

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1 Nunneley I. I stopped asking doctors for help. *BMJ* 1997;315:890. (4 October.)
2 Nunns D. *A clinico-pathological study of vulval vestibulitis.* Manchester: University of Manchester, 1997. (MD thesis.)

Weight loss will be much faster in lean than in obese hunger strikers

EDITOR—Peel's editorial article on hunger strikers draws attention to an area of pathophysiology that few doctors will be familiar with.[1] There is an extensive literature on the normal physiological response to fasting and pathological events that may occur during prolonged therapeutic starvation, a treatment for morbid obesity that has largely been abandoned because of lack of long term success and pressure on hospital beds.

When monitoring adverse events, and especially the time to the appearance of such events, it is essential to take into consideration the weight of the starving subjects before the fast. Studies of starvation that colleagues and I have carried out have highlighted important differences in metabolic adaptive responses between subjects who were obese and those who were of normal weight, especially in terms of protein metabolism.[2 3] After just 60 hours of fasting, lean subjects showed active protein breakdown whereas obese subjects did not. In addition, the rate of weight loss was greater in lean than obese subjects, lean subjects having lost 3.9% of their initial body weight after 60 hours whereas obese subjects had lost 2.4%. Peel suggests independent medical monitoring after a weight loss of 10% in lean healthy subjects. This weight loss is likely to be arrived at much sooner in lean than obese subjects: our obese subjects lost only 9.3% of their initial body weight after fasting for two weeks.

Peel is right to warn of the dangers of refeeding. Colleagues and I described one patient who developed recurrent ventricular tachycardia when feeding restarted after total therapeutic starvation. He was successfully resuscitated[4] but subsequently developed a moderately severe proximal myopathy, which recovered.[5] Doctors caring for people on prolonged hunger strikes or supervising prolonged therapeutic starvation need to be aware of the many dangers of this unusual metabolic situation.

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1 Peel M. Hunger strikers. *BMJ* 1997;315:829-30. (4 October.)
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5 Scobie IN, Durward WF, MacCuish AC. Proximal myopathy after prolonged total therapeutic starvation. *BMJ* 1980;280:1212-3.

Several factors were not considered in study of increase in hay fever and eczema

EDITOR—Butland et al show that the observed doubling in the prevalence of hay fever and eczema in cohorts of British adolescents between 1974 and 1986 cannot be accounted for purely by differences between the cohorts in terms of sex, birth weight, birth order, maternal age, breast feeding, maternal smoking in pregnancy, or father's social class.[1] While these results are important, several factors were not considered in the analysis.

Firstly, an increase in public awareness of atopic diseases over the past two decades may have contributed to the apparent increased reporting of allergic rhinitis and eczema. The authors dismiss this possibility by referring to circumstantial evidence of increased positive results of skin prick testing in London (between 1974 and 1988) and a study of specific antibodies in Japanese schoolchildren. These data cannot be extrapolated to account for the differences observed here. Skin prick testing of a sample from each cohort would have provided stronger evidence of an increase in atopic disease.

Secondly, the response relied on accounts of symptoms from parents, rather than from the 16 year olds themselves. Parents of 16 year olds may not know such details of their children's health. Furthermore, parents in the study may have misunderstood the medical terminology used. For example, "eczematous rashes" is a broad (though technical) term, which could be interpreted in several ways, such that conditions other than atopic disease were reported. If explanations of the terms were requested the use of different interviewers between the two cohorts may have introduced bias, which is not discussed.

Thirdly, there is a genetic predisposition to the development of atopic disease,[2] yet differences in family history of atopy between the two cohorts were not considered in the analysis. The overall response rates were poor (62% and 54%), and there was no reported follow up of non-respondents to see if they differed significantly in terms of a family history of atopy. Furthermore, families with experience of atopic disease may have been more likely to respond positively to these specific questions, which could have led to bias.

The observations of the study are important in developing understanding of atopic disease, but the factors we have highlighted need to be taken into account before it is concluded that there has been a true doubling in the prevalence of atopy in Britain over the past 20 years.

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1 Butland BK, Strachan DP, Lewis S, Bynner J, Butler N, Britton J. Investigation into the increase in hay fever and eczema at age 16 observed between the 1958 and 1970 British birth cohorts. *BMJ* 1997;315:717-21. (20 September.)
2 Coleman R, Trembath RC, Harper JI. Genetic studies of atopy and atopic dermatitis. *Br J Dermatol* 1997;136:1-5.

Royal colleges need modernisation

EDITOR—I was pleased to read of Professor Alberti's intention to modernise the Royal College of Physicians.[1] The royal colleges generally are regarded by many of their fellows and members as too remote and run by elite councils heavily weighted with academics. There is an urgent need for the colleges to be run by councils composed of democratically elected councillors who have clearly defined medical parliamentary constituencies to which they are directly responsible. Fellows and members need to feel more directly involved in their colleges by having a meaningful vote in elections to their governing bodies.

Unless the colleges truly represent the views of all their fellows and members in the United Kingdom their important role in deciding the direction of the country's health care will inevitably be weakened. It may be further weakened in the future if specialist registrars, having completed their training and been recognised as specialists by the European Union, no longer see the need to join a college or contribute to its (generally) well filled coffers.

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1 Abbasi K. A crusader with a sense of humour. *BMJ* 1998;316:252. (24 January.)

Correction

Survival is better indicator than mortality in geographic comparisons of health

An editorial error occurred in this letter by P A West (14 February, p 556). The wrong address was given for Dr West, who does not work for Berkshire Health Authority; his correct address is the Department of Public Health Medicine, Division of Public Health Sciences, UMPS, St Thomas's Hospital, London SE1 7EH.