Letters

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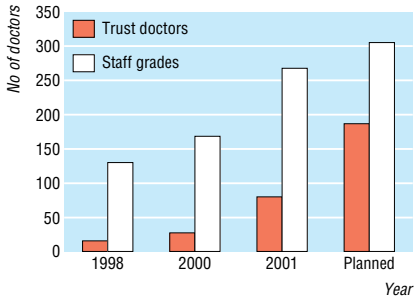
Are trust doctors the new lost tribe?

Editor—The number of trust doctors (doctors in non-training grades) in the United Kingdom is rising as trusts try to meet both service and hours requirements and are unable to employ more doctors in training. The Department of Health has no code for trust doctors so its annual census cannot show how many there are.

We surveyed the human resource departments in each acute trust in Yorkshire by postal questionnaire; nine out of 10 replied. We found that the number of trust doctor posts has increased at least fivefold over the past four years, and the current number will more than double in the near future on the basis of trusts' stated plans (figure).

We sent separate questionnaires to 63 of a sample of 81 trust doctors in the region; 36 (57%) replied. They were asked questions about educational provision and supervision, reasons for being in the post, previous clinical experience, career aspirations, work patterns, and pay. With regard to education and training, 16 trust doctors reported that they had an educational supervisor and only six had appraisals. Although 32 trust doctors had been granted study leave, only 25 received funding for this. Surprisingly, 15 trust doctors rated their work as equivalent to that of a senior house officer and 21 as equivalent to a specialist registrar.

Most of the doctors were United Kingdom graduates (16), with eight being from the Indian subcontinent, five from Africa and the Middle East, and one from the European Economic Area. The most common career aim was a consultant post in the United Kingdom (21 doctors), and the main reasons for taking a post as a trust doctor were as a "stop gap" or to gain experience in a particular specialty. Trust doctors come under the legal requirements of the European working time directive, but only 12 doctors in our sample currently worked 48 hours a week or less. Most did on-call rotas—another interesting finding.



Increase in numbers of doctors in non-training grades in Yorkshire

Our survey shows that the number of trust doctors is increasing dramatically.[1] These are junior doctors with educational needs, yet they do not receive the same educational supervision as their training grade equivalents. Delivery of acute care in the NHS is going to increasingly depend on doctors who are receiving little educational supervision. Trusts say they cannot meet the requirements of the new deal or the European working time directive without employing trust doctors. This survey has put figures to a quietly growing problem that must now be tackled.

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1 Department of Postgraduate Medical Education. *A survey of non-training grade doctors in Yorkshire.* Leeds: DPME, 2002. www.yorkshiredeanery.com (accessed 13 August 2002).

Peer review of statistics in medical research

Journal reviewers are even more baffled by sample size issues than grant proposal reviewers

Editor—With reference to the article by Bacchetti,[1] the confusion surrounding sample size estimates in research protocols elicits quite strange responses from reviewers when they are faced with the completed research in a report submitted to a journal for publication. One of our submissions was rejected because the planned sample size was not attained. But the effect size was greater in the study than we had anticipated, and thus the difference was of clinical and statistical significance. Another submission met the same fate for a similar reason—it was an equivalence trial—and even though the difference in effect between intervention and control arms (and both sides of the confidence interval around this difference) lay completely within the equivalence interval, the fact that the planned sample size was not attained in some way invalidated the result in the mind of the reviewer.

Although sample size estimation is useful in considering the feasibility of conducting a study (and protocol reviewers should discourage funding for studies that are plainly too small to be meaningful) attainment of the planned sample size does not seem to me to be a useful indicator by which journal reviewers should assess the validity of a completed research report in which clinically and statistically meaningful results have been obtained.

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My competing interest in relation to this question is my desire to publish research in the face of overoptimistic sample size estimates in my grant proposals.

1 Bacchetti P. Peer review of statistics in medical research: the other problem. *BMJ* 2002;324:1271-1273. (25 May.)

Rationale for requiring power calculations is needed

Editor—The article by Bacchetti with its comments about uncertainties surrounding power calculations prompted me to seek advice about an issue that has implications for clinical research.[1] The company I work

Advice to authors

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Letters will be edited and may be shortened.

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for, Laxdale Limited, often conducts pilot studies on new entities. Our usual practice is to state in the protocol that there is no reasonable basis for a power calculation. In order to collect information about an effect size (if any) and the variance of that effect size, we state that we plan to randomise a modestly sized group of patients to two or three doses of the active drug and to placebo. Depending on indication and on advice from experienced clinicians, such studies might include 15-30 patients per group. Results in hand, we can then plan further studies with evidence on which to base a sensible power calculation.

Recently a multicentre regional ethics committee insisted on a formal power calculation as an ethical requirement for a pilot study. We were told to base power calculations on results obtained with other products with different mechanisms of action and on the minimum useful clinical improvement that might be expected. In vain we pointed out that this procedure had no scientific basis and that, by basing the power calculation for a pilot study on the minimum useful benefit, group sizes would have to be large and many patients might unnecessarily be exposed to placebo, or an ineffective drug, or even a drug that might be toxic in this new patient population.

Little literature is available on power calculations in pilot studies. We have not found any study that, for a consecutive series of trials of any type, compared prestudy power calculations with the results obtained. There are studies of power calculations in published papers, but that is different from prospectively evaluating whether power calculations have validity or whether they require so many assumptions that they are of limited practical use. There is a theoretical basis for power calculations, but the absence of any prospective evaluation raises suspicions. What other procedure in clinical research has become standard with so little evidence from real world studies?

What is the rationale and where is the evidence base for requiring power calculations in pilot studies of new entities? More generally, does the use of power calculations for any studies have a strong experimental basis? Or are the assumptions so flawed that calculated power frequently bears little relation to actual power?

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Reporting power calculations is important

Editor—In response to the article by Bacchetti, Zwarenstein (letter above) recounted how he had papers rejected because the trials failed to reach the planned sample size.[1] Bacchetti responded on bmj.com, observing that, unfortunately, many published standards for presenting studies' results, as well as scales for rating article quality, insist that power calculations are necessary even after the results are known and speculation about power is no longer needed (either P was <0.05 or not).[2] We wish to explain why the CONSORT statement includes the recommendation that reports of randomised controlled trials say how the sample size was determined, including details of a prior power calculation if done.[3 4]

There is little merit in calculating the statistical power once the results of the trial are known; the power is then appropriately indicated by confidence intervals. We agree that failing to reach the planned sample size is not a reason to reject a paper. But power calculations are still of importance to readers, both directly and indirectly.

Firstly, if the achieved smaller size differs from the planned sample size the reader will wish to know why: was this just because of an overestimate of the likely recruitment rate or because the trial stopped early because of a statistically significant result (perhaps after multiple looks at the data, and, if so, was a formal stopping rule or guideline used)?

Secondly, a power calculation indicates strongly what is or should be the principal outcome measure for the trial (although it may not indicate how the analysis will be performed). This is a safeguard against changing horses in midstream and claiming a big effect on an outcome that was not a primary outcome or even not prespecified. Also, a power calculation is explicit evidence that the trial was properly planned and that some thought was given to the size of effect that would be clinically important (even though we all know the values used are often rather optimistic in order to keep the sample size down).

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Author's thoughts on power calculations

Editor—I am delighted to see Altman et al reaffirm that confidence intervals are preferable to post hoc power calculations, but I disagree with their reasons for nevertheless presenting prior power calculations for completed studies. Just as confidence intervals more directly and clearly address uncertainty than power calculations, so too the information that they see flowing from prior power calculations can instead be presented more directly. Papers can provide information about early stopping, recruitment rates, and relevant departures from expectations without giving power calculations. The importance of prespecification can be debated, but it is easy enough to simply state the planned primary outcome without reference to power calculations. The clinical importance of a given effect size similarly does not rely on power calculations.

The remaining point Altman et al cite is that prior power calculations provide assurance that the study was properly planned. The scientific relevance of this is unclear to me. If a study finds important information by blind luck instead of good planning, I still want to know the results. I discussed in the paper the frequent difficulty of conforming to common notions of "proper" sample size planning, and Horrobin has expanded on this. Even when investigators can approximate the ideal, assumptions often turn out to be inaccurate. Does this mean that the studies were poorly planned and should be disregarded? Or should we learn from them what we can? Requiring prior power calculations as back end enforcement of the sort of ethics based sample size review that Horrobin describes seems particularly misguided to me. If a study provides information important enough to warrant publication it seems reasonable to assume that it did not have an unethically small sample size.

An important reason not to present prior power calculations is that doing so contributes to the very problem it is supposed to mitigate: misinterpreting $P>0.05$ as providing strong support for the null hypothesis. Authors, reviewers, and readers may reasonably interpret the presence of a power calculation as having some direct relevance for interpreting the results. Given the oblique (I have argued) reasons for presenting power calculations, they may understandably assume that the calculation's purpose is to assure us that the sample size is adequate for the classic statistical hypothesis testing approach of either accepting or rejecting the null hypothesis. Despite subtleties that statistical theorists may try to convey, "accepting" the null hypothesis is often described as, "There is no difference." The needed corrective for this type of erroneous interpretation is to encourage investigators and readers to pay attention to estimated effects and the confidence intervals around them, particularly when interpreting "negative" results. I contend that presenting power calculations works against this. In addition, presenting power calculations opens the study to a second round of unhelpful sample size criticisms, of which Zwarenstein's experience provides an extreme example.

There is certainly room for disagreement about what practices and recommendations will improve the use of statistical methods in medical research. I thank Altman et al for contributing their perspec-

tive and hope that this clarifies where and why I disagree. I also thank the other correspondents for the important information and thoughts they have provided.

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Reviewers' contributions should be thoughtful, constructive, and encouraging

EDITOR—Congratulations to Bacchetti for his paper on some of the difficulties inherent in our present peer review system for publications and grant applications.[1] May I add to his examples the following opening gambit from one of our reviewers: "There is a great need regarding virtually all aspects of life for [patient group] across their life spans in those countries in which this population at least has been attended to." The project team has to respond to this (and the 70 questions that follow) within a week. As we are unable to discern its meaning, or even whether it is a positive or negative comment, writing to the *BMJ* seems a more useful way to spend 30 minutes of the deadline.

In our role as a resource for clinicians attempting to get published or applying for funding, and as researchers in our own right, staff at the research and development support unit see more reviews than most. Bachetti says that the number of criticisms in a review is taken to be a measure of its quality. Reviewers attempting to achieve this "quality" all too often and obviously stray into areas about which they know little, and statistics is the most obvious example (the English language is another). They nevertheless feel empowered to make damning comments. May I add that it is the sneering attitude with which they feel obliged to do so which makes the process so profoundly disheartening for inexperienced researchers. We all review scientific papers and grant applications in the research and development support unit as well as provide a peer review system for local trusts under the new research governance arrangements.

When these are blind I am embarrassed to find my own reviews described by applicants or authors as "the supportive" or "the encouraging" referee. It takes time and effort to put a funding bid together, and applicants are usually to be congratulated on doing so within a strict deadline. Inexperienced authors may still have an important message to convey and should be encouraged to do so. The honour roll is an attractive idea and could be linked to, say, the research assessment exercise for heads of academic departments to be enthusiastic. This elevated status should be reserved for reviewers whose contribution is thoughtful, constructive, and encouraging.

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1 Bacchetti P. Peer review of statistics in medical research: the other problem. *BMJ* 2002;324:1271-1273. (25 May.)

Suggested solution may partly solve other problem

EDITOR—I was delighted to read Bacchetti's article on the flaws of the peer review system.[1] The power of ill informed or undermotivated reviewers is disproportionate to their gatekeeping role. One of the main frustrations with the current system is the lack of ability of researchers to respond to spurious criticism.

However, a recently submitted proposal to a medical research charity has given some hope in this area as we were able to respond to reviewers' comments on the proposal before it went to committee for adjudication. Some of the comments were valid, but most showed that the reviewers had not read the application properly. In either case we were able to amend or clarify the situation.

This process added another two weeks before hearing the outcome. This is a small amount of delay considering the many months it takes to put an application together, and it gives some confidence that when articles or proposals are rejected that enormous amounts of effort have not been wasted for trivial reasons. I recommend that this process becomes part and parcel of the peer review system

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1 Bacchetti P. Peer review of statistics in medical research: the other problem. *BMJ* 2002;324:1271-1273. (25 May.)

Cases of polio in Nigeria are dropping

EDITOR—Raufu reports that the number of cases of poliomyelitis has risen in Nigeria,[1] but evidence shows that the number of confirmed cases of poliomyelitis has fallen since the start of the polio eradication initiative. It is true that both routine immunisation and the initiative suffered a setback in northern Nigeria as a result of propaganda fuelled by a cassette recording by an American organisation that was widely circulated among the Muslim community. The recording claimed that immunisation was a ploy by Western governments to promote family planning and infect children with HIV. As a result, many parents refused immunisation for their children in 2000 and 2001.

However, Nigeria's government mobilised traditional and religious leaders (the Ulama), who campaigned assiduously to counter the propaganda, with tremendous success. Both routine immunisation and the polio eradication initiative are now fully accepted except in a few areas (minor pockets of resistance) in Niger, Jigawa, and Kano States, as reported by Raufu. Immediate intervention by traditional and religious leaders in those areas resulted in the parents inviting the immunisation teams to return and immunise the children, which was done.

The polio eradication initiative took off in Nigeria only in 1998. In 1999 hundreds of cases of acute flaccid paralysis were reported, with 98 confirmed to be due to wild poliovirus. Last year there were 57 confirmed wild poliovirus cases, whereas this year only 29 cases were confirmed by early June. The number of cases of wild poliovirus has actually been falling, which makes the title of Raufu's news item highly misleading.

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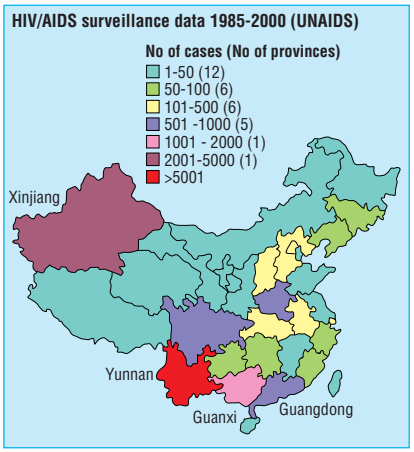
1 Raufu A. Polio cases in Nigeria rise as vaccine is shunned for fear of AIDS. *BMJ* 2002;324:1414. (15 June.)

Editorial on epidemiology of HIV in China was misleading



EDITOR—Zhang and Ma's editorial paints a somewhat misleading and incomplete picture of the epidemiology of HIV in China.[1]

Firstly, the numbers of HIV infections need to be seen in terms of percentages. Even if the highest current estimate of 1 million infections nationwide is used, this converts to an overall rate of around 0.07%. The male: female ratio is 4:1 and unlike almost any other country most of the people affected (80%) are residents of rural areas. At present four provinces (out of 31) account for 77% of all cases: Yunnan, Xinjiang, Guanxi, and Guangdong. This is because of the predominance among drug misusers (www.ns.unchina.org/unaids). The breakdown of mode of transmission is given in the table.

Secondly, although we completely agree that drug misusers, commercial sex workers, and to a much lesser extent migrant workers are current drivers of the epidemic, there is an important omission: commercial blood donors.[2] The government itself admits that 30 000 to 50 000 plasma donors have become infected through faulty blood collection practices, and many local reports suggest that these figures are a gross underestimate. And this does not consider the sexual partners of the donors and the recipients of infected blood products. Although measures were introduced in 1997-8 to outlaw commercial blood donation, and the scale of the problem has reduced, they have succeeded in driving the

Mode of transmission of HIV in China: official figures

	No of cases
Heterosexual	407
Homosexual	12
Intravenous drug misuse	3460
Blood transfusion	107
Blood products	8
Vertical	10
No detail	1197
Total	5201

Source: Ministry of Health sentinel surveillance data (www.ns.unchina.org/unaids).

practice underground with potentially greater risks to donors.

Thirdly, Zhang and Ma portray the surveillance system as extensive and efficient. But this is far from the case. One of the reasons that so much about HIV prevalence is speculation is because the surveillance system is totally inadequate. In fact, HIV is rarely tested for outside the official HIV surveillance system. This system targets high risk groups, provides poor guidance about sampling at a local level, and the number of samples tested and the sites themselves vary from year to year, threatening any credibility to detect time trends. There is no true population based surveillance being carried out.

The government has indeed made a strong commitment to prevention. But the general philosophy is still one of eradicating the risk behaviour, rather than educating about risk reduction: so drug misusers are incarcerated and sex workers imprisoned, as Zhang and Ma note. This of course makes it very hard to effectively target prevention measures. This is compounded by the fact that local officials are frequently afraid to admit to an HIV problem in their area and cases go missed and unreported. Greater openness and more tolerance towards HIV sufferers and those in high risk groups are an essential prerequisite for effective control of the epidemic.

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1 Zhang KL, Ma SJ. Epidemiology of HIV in China. *BMJ* 2002;324:803-4. (6 April.)
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Trends in HIV, gonorrhoea, and syphilis

Screening for neurosyphilis is recommended

Editor—Nicoll and Hamers report the prevalence of sexually transmitted diseases, such as syphilis, gonorrhoea, and HIV.[1] Despite the frequent invasion of the central nervous system by *Treponema pallidum*, most infected subjects will not develop neurosyphilis. Several studies have suggested an increased incidence of neurosyphilis, particularly in patients infected with HIV.[2]

We measured the prevalence of neurosyphilis by screening all serum and cerebrospinal fluid received at the neuropathology laboratory between November 1989 and April 2000 using the venereal disease research laboratory assay and *T pallidum* haemagglutination assay with fluorescent absorbed treponema antibody. The only exclusion criterion was being positive for HIV.

A total of 9410 samples was screened, of which 195 had positive serology with the fluorescent assay. Of the 195 samples, 67 fulfilled criteria for neurosyphilis.[3] Nineteen samples tested negative for fluorescent treponema antibody in cerebrospinal fluid, and neurosyphilis was considered improbable. Twenty one samples had probable neurosyphilis (negative results in research laboratory assay for cerebrospinal fluid) and 27 samples had definite neurosyphilis (positive results in research laboratory assay for cerebrospinal fluid).[4]

The 48 subjects with probable or definite neurosyphilis were directly evaluated or charts reviewed. Forty four were men, the mean age was 59.2 years, and three patients had been referred with primary infection. Eleven of the subjects presented with meningovascular symptoms, eight with general paresis or mental deterioration, two with tabes dorsalis, two with acute meningitis, two with ocular symptoms; six were asymptomatic, and 15 presented with atypical symptoms (four with meningoradicolytis, four with cranial nerve involvement, two with myelopathy, three with seizure, two with parkinson-like syndrome).

In cerebrospinal fluid, cell count was $>5/mm^3$ in 27 subjects, proteins were >0.515 g/l in 28, and the immunoglobulin index was >0.8 in 27; an oligoclonal band was present in 26 out of 36. IgG index and oligoclonal bands were significantly different in patients with positive and negative results in the venereal disease research laboratory assay for cerebrospinal fluid. All patients with such positive results had reactive serum with the laboratory assay compared with 13 out of 21 patients with negative results in cerebrospinal fluid.

Over 10 years 195 (2%) samples of serum and cerebrospinal fluid gave positive results in the fluorescent treponema antibody assay, which means a primary infection of *T pallidum*, and 48 (0.5%) fulfilled the criteria for neurosyphilis. The clinical presentation has shifted from general paresis and tabes dorsalis to meningovascular and atypical forms.

Our data suggest that screening for syphilis in neurological populations may be appropriate in all patients because the clinical situation alone is not helpful in determining when to screen and results may quite often be positive.

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1 Nicoll A, Hamers F. Are trends in HIV, gonorrhoea, and syphilis worsening in western Europe? *BMJ* 2002;324:1324-7. (1 June.)
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Sexual health services in general practice can be improved

Editor—Nicoll and Hamers discussed trends in sexually transmitted diseases in Europe.[1] I have been involved in a pilot in a new service to deliver improved sexual health services in general practice. In practical terms general practice is the only hope to improve services as it is the only provider with the capacity needed. Also it is important to recognise that the stigma associated with services in genitourinary medicine will only be broken when this becomes "mainstream" service provision in general practice. Disappointingly, no mention of sexual health provision is made in the new contract, and there is no national primary care strategy.

Our pilot is different, and we will report by the end of 2002 when we hope to have more than 300 patient contacts. What we offer is "a relationship consultation" that patients can access at any time, but it is specifically focused at patients who are entering a new relationship. We focus equally on men and women, unlike other services in the past. We have had a good uptake by men. The consultation is entirely confidential, with no records being kept in the case notes. This practice alone makes a great deal of difference to the issues patients are willing to discuss. A nurse led model may be the ideal.[2 3]

We offer an integrated approach and consider sexual health and contraceptive provision together. We give out free condoms, often with the "morning after pill" given for back up to take home.[4] We are considering providing the "morning after pill" to men who request condoms. We are experimenting with computer consultation aids for contraception in MS PowerPoint. These ensure that consistent information and sexual health promotion are given.

HIV testing and counselling is offered without referral to specialist clinics.[5] Tremendous anxiety about HIV prevails, much is generated through the media, but very few places will test. These have protracted counselling and are generally hospital based. This "HIV anxiety," therefore, can go on for many years without the possibility of resolution. When offered, some 50% of our patients want to be tested for HIV. Follow up is

over the phone like any other standard investigation.

If we want patients to come forward for sexual health testing then we need to stop examining them. The new technologies virtually negate the need for examinations, and urine testing by polymerase chain reaction or ligase chain reaction is as effective as conventional testing. Currently we could test for gonorrhoea, chlamydia, and trichomonas in this way if the NHS would pay.

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Intermediate care is ageist

Editor—With reference to the editorial by Pencheon, intermediate care is logical if you do not want people to go to hospital.[1] Since there is no other avenue to obtain rapid assessment of patients and access to diagnostic facilities, hospital remains the safest option, provided that hospital is proactive with a rapid discharge policy. The problems general practitioners have in getting patients admitted, even as arranged admissions, will not be ameliorated by the small sums each primary care trust spends on intermediate care.

When discussing which patients are suitable for intermediate care services, the inherent ageism within the NHS becomes apparent. A 75 year old patient who is unsteady and has a chest infection is usually regarded as an ideal patient to manage in their own home environment. A 35 year old patient with pneumonia is regarded as an obvious hospital case.

It is the older patient, however, who is likely to have multiple pathologies, who is to be directed away from the "hi tech" hospital environment. It is quite difficult to even make the diagnosis of pneumonia in elderly people, which will simply present as a fall (a fall in a younger person is called a collapse). The fever, if any, will be slight, and signs are not obvious. Only after admission and full assessment and a chest radiograph at the very least, will the diagnosis become clear. Other pathology will be found, from mild Parkinson's through to hyponatraemia from the general practitioner's medication.

Intermediate care can work only if we have rapid access to full, enthusiastic, consultant led assessment 24 hours a day. This requires the full resources of the hospital team. Intermediate care cannot relieve hospital resources. If it attempts to do so, the NHS acknowledges its wish to introduce a two tier service, with "lo tech" services for elderly people.

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1 Pencheon D. Intermediate care. *BMJ* 2002;324:1347-8. (8 June.)

Selection of medical students

How can medical schools produce good doctors if political dogma restricts them?

Editor—Tutton and Price raise several points about the selection of medical students that need to be clarified.[1] They rightly say that scholastic achievement, aptitude tests, and selection interviews can all be faulted as means of selecting students for a career in medicine, but they seem to agree that general intelligence, allied with emotional stability and social integration, is a good predictor of achievement.

The main thrust of their editorial, however, is to advocate affirmative action to increase the intake of students from lower socioeconomic groups. The justification for this is to "redress inequities from the past" and admit students who have "genuine, rather than apparent, merit." To further this end at Witwatersrand University in South Africa, interviews have been abandoned because those in low socioeconomic groups scored badly in the criteria of teamwork, leadership, and social involvement. I assume that prior scholastic achievement is also ignored. Selection can then be made only on the basis of social class and perhaps some kind of personal statement.

This attitude is now prevalent in the United Kingdom, where the Higher Education Funding Council for England is putting pressure (including financial) on universities to admit students on the basis of several "access indicators," including the postcode and the social class of their parents (www.hefce.ac.uk).

Is it necessary or desirable to use affirmative action in the United Kingdom? The experience of my colleagues whose families came from the Indian subcontinent and were classified in the lowest socioeconomic groups on arrival is pertinent. Without affirmative action they have come through the system to become articulate, intelligent doctors. Affirmative action requires medical schools to preferentially take students with lower academic achievement and communication skills. Do British medical schools have the resources to turn these students into well rounded and competent doctors? The experience in the United States is that "many of the preferentially admitted students from minority groups could not pass their licensing examinations, despite greater resources being directed towards helping them than other students."[2]

This is social engineering. Can and should medical schools be expected to reverse the deficiencies of the school and social system? We have been harangued by the politicians about the problems of supposedly inadequate doctors. How can medical schools produce good doctors if political dogma restricts their freedom to select those they feel are most able?

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1 Tutton P, Price M. Selection of medical students. *BMJ* 2002;324:1170-1. (18 May.)
2 Charatan F. Minorities get preferential admission to US medical schools. *BMJ* 2001;322:1563.

Sheffield University has developed an outreach programme

Editor—Tutton and Price have written about the selection of medical students.[1] We believe that the best way to help those from disadvantaged backgrounds who wish to apply for medicine is to adopt a scheme that will bring them up to a level that enables them to compete equally with other applicants. To this end, and with government backing, we have developed Sheffield's outreach and access to medicine scheme (SOAMS); this is an extension of the University of Sheffield's successful outreach and compact schemes, which have both been running for nearly 12 years.

For entry into the scheme students must be in the first generation of their family to go to university, come from a family with a low income, and have personal or family circumstances that may affect their aspirations, expectations, and potential academic achievements. Students are targeted at year 9 (phase 1, 13/14 to 16 years), and the aim is to involve 100 students a year at that stage. Information is provided for students, parents, and teachers, and a series of lectures explains what is involved in studying medicine. Provided students successfully complete phase 1, they proceed to phase 2, having been given advice on suitable A levels.

In phase 2 we provide community service projects, work experience, medical conferences, and a residential summer school. In addition, we provide advice on how to apply to medical school through the Universities and Colleges Admissions Service (UCAS) and practice for interviews. We hope by the end of phase 2 to have produced around 25 suitable candidates out of the original 100. Those who are not successful will be advised and fully supported for other career options. Those who are successful will be formally interviewed in the usual way. Financial support is provided during the course.

We believe that a scheme such as Sheffield's is the correct way forward. We believe that our progression scheme is sensitive and welcoming and is designed to bring out the best in those from underrepresented groups who would never have seriously considered medicine as an option.

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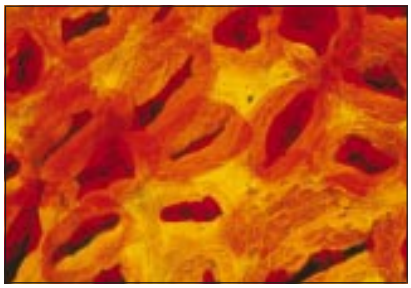
1 Tutton P, Price M. Selection of medical students. *BMJ* 2002;324:1170-1. (18 May.)

Acid, burns, and feeding tubes



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Patients should not be allowed to lie in their own vomit: gastric acid burns

EDITOR—I dispute the recommendation of Lapsia and Maguire in Minerva that "patients fed via a nasogastric tube should take a gastric protecting agent" on the basis of their finding of chemical burns in a patient who vomited after nasogastric feeding on a neurological ward.[1]

Anyone without achlorhydria secretes hydrogen ions from gastric mucosa whether fed orally, by a nasogastric tube, or parenterally. Thus during vomiting, acid is likely to come into contact with the skin. If we follow the authors' advice anyone capable of vomiting acid would be prescribed a gastric protecting agent.

The authors note that nurses reported that the patient in their case had vomited previously and that during the morning ward round "there was evidence of fresh vomit on her sheets." This implies that she had been in contact with her vomit for some time.

I suggest that a more logical conclusion for the authors to have drawn is that patients should not be allowed to lie in their own vomit.

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1 Lapsia S, Maguire S. Minerva. *BMJ* 2002;324:1404. (8 June.)

bmj.com

Summary of rapid responses

We published six electronic responses making similar points about this picture report in Minerva, as well as an apology from Minerva for allowing the report to be published without the corroboration of peer review.[1]

Correspondents offered pithy conclusions, including "patients being fed via a nasogastric tube should be properly nursed" (Kevin Booth) and "drugs are not a substitute for quality nursing care" (Michael Terry).

Tim Palfreman and Nicholas Lavies reiterated that the burns had nothing to do with acid suppression or nasogastric feeding: "To suggest otherwise is at best an elementary failure to think straight." They gave the last word to J B S Haldane, who said in 1939: "We are told the most fantastic biological tales. For example, that it is dangerous to have acid in your stomach."

Sharon Davies *letters editor*
BMJ

1 Electronic responses. Minerva. bmj.com 2002. bmj.com/cgi/eletters/324/7350/1404 (accessed 20 August 2002).

On Hippocrates

Hippocratic ideals are alive and well in 21st century

EDITOR—Loefler regards the Hippocratic ethics as taking no account of modern medicine and that craving for the simplicity of Hippocratic medicine is pointless nostalgia.[1] We argue that his article takes a simplified view of the application of the Hippocratic ideals to modern medicine and that these are as relevant today as they were in the 4th century BC.

At the very centre of the Hippocratic ideals are the principles of "doing no harm" and "absolute regard for life." Loefler argues that harm is an inevitable consequence of the powerful array of interventions available to the modern doctor. Although many investigations and interventions have potential adverse effects and complications, it is the balance of potential benefit and harm of each intervention that is the most important consideration. If overall benefits outweigh potential complications, this Hippocratic ideal is still respected.

Harm—often in the form of death—can also be incurred by failing to intervene. Guidelines regarding consent for examination or treatment state that to give valid consent a patient needs to understand the nature and purpose of the procedure and that it is advisable to inform the patient of any risks in the proposed treatment, any alternatives to it, and the risks incurred by doing nothing.[2] By fully involving patients and their next of kin in such decisions, we respect the individual and uphold another tenet of Hippocratic medicine.

Loefler says that the principle of absolute respect for life is no longer held in regard concerning orders not to resuscitate. In doing so, he addresses the wrong Hippocratic ideal, for when weighing up a patient's wishes, prognosis, coexistent medical problems, and the likely success of an attempt to resuscitate, the principle of doing no harm often takes precedence over absolute regard for life. Loefler finds piety and serenity lacking in modern medicine and yet in making an order not to resuscitate, the aim is often to facilitate a pious and serene passing while maintaining a patient's dignity.

In an age when technological advancement has inevitably amplified the potential risk to patients and has increased longevity sometimes at the expense of quality of life, we believe that the Hippocratic ideals are more important than ever and remain the cornerstones of ethics in modern medicine.

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1 Loefler I. Why the Hippocratic ideals are dead. *BMJ* 2002;324:1463. (15 June.)
2 Department of Health. *Reference guide to consent for examination or treatment.* London: DoH, 2001.

Either help or do not harm the patient

EDITOR—Loefler, like many others, believes (incorrectly) that the concept, "First, do no harm" has its origin in the Hippocratic oath.[1] The Latin phrase, "Primum non nocere" (above all else, do no harm) is cited often and believed to be a major component of the oath. The phrase does not actually appear in the oath. Further, one must wonder why Hippocrates, a Greek, would, for centuries, continue to be quoted in Latin.

The confusion may have arisen from the fact that during the time of Hippocrates, doctors were used to administer (for a price) fatal potions to dispatch unwanted individuals to their heavenly reward. Hippocrates strongly disapproved of these Hellenic hitmen and did include in the oath the phrase, "I will neither give a deadly drug to anybody if asked for it, nor will I make a suggestion to this effect."[2]

Further, it seems as if "do no harm" is a distortion, a phrase taken out of context. According to John Morrison, a doctor and scholar of Greek antiquity, the phrase is not from the oath but from the Hippocratic corpus, "Of the Epidemics," Book I, section 11, 5 which states: "Practise two things in your dealings with disease: either help or do not harm the patient" (personal communication).

Obviously, what Hippocrates had in mind was that doctors are there to help the patients, but if they are unable to help, the doctor should take care not to harm. The significance of the distinction cannot be overlooked.

If a modern practitioner believes that treatments are ethically acceptable so long as they "do no harm" to the patient, then it follows that ineffective treatment is morally permissible. Overtreatment, superfluous tests, and unneeded procedures would also be justified as long as no harm results. Since a profession that foolishly clings to such a concept will ultimately face a justified storm of public indignation, doctors are urged to take care to avoid such practices.

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1 Loefler I. Why the Hippocratic ideals are dead. *BMJ* 2002;324:1463. (15 June.)
2 Ancient medicine: selected papers of Ludwig Edelstein. In: Temkin O, Temkin CL, eds. "The Hippocratic oath: text, translation and interpretation." Baltimore: Johns Hopkins Press, 1967:4.