inroads into the possible impact of drug resistant tuberculosis.¹ A central laboratory to which samples could be sent would provide us with an important means of combating the problem.

It would be helpful if specimens from any patient believed to be at risk of drug resistant tuberculosis could be sent to the central laboratory for fast tracking. The polymerase chain reaction could identify the organism quickly once a smear had been found to be positive for acid-alcohol fast bacilli at the local laboratory. Use of restrictive fragment link polymorphism would enable a national register of drug resistant organisms to be established by DNA fingerprinting so that, should subsequent samples arrive with the same print pattern, the clinician could be rapidly alerted to the possibility of drug resistance.

There are currently some 7000 notifications of tuberculosis a year in Britain; in addition, in about 1000 cases in which smears yield a positive result, culture yields mycobacteria that do not result in tuberculosis. It would probably not be feasible to send all 8000 specimens for which smear testing yields a positive result to a central laboratory. It would therefore be appropriate to identify the risk factors in patients who may have drug resistant tuberculosis and fast track these patients alone. I suggest the following categories: patients who have had previous treatment, particularly if they are from abroad; those known to have had drug resistant disease previously; HIV positive patients; patients who may have been infected in the African continent or middle east; contacts of patients known to have drug resistant tuberculosis; and substance misusers and homeless people. I estimate that these six categories of patients make up 10-20% of the patients notified as having tuberculosis in Britain—that is, 700-1400 patients. The total cost of this service would not be excessive and would represent money well spent in the control of a potentially disastrous problem.

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Transmission of tuberculosis in patients with HIV infection

EDITOR,—We agree with the final conclusions drawn by G M Scott and J Holton that even more strict controls should be adopted to control the transmission of tuberculosis in centres caring for patients with HIV infection.¹ Transmission of tuberculosis at their hospital is, however, supported by both our molecular biology data and the epidemiology.

Editorial constraints prevented us from including detailed epidemiology of transmission in our paper.² In Scott and Holton's investigation, apart from the patient in case 1 walking through Ward B, there were other potential exposures. Previous sputum smear tests in the patient had yielded positive results and he did not begin to receive antituberculosis treatment for several hours after bronchoscopy. Bronchoscopy induces coughing and formation of droplet nuclei. At the time of the potential outbreak of tuberculosis the room had an extractor fan venting separately from the ward but no negative pressure ventilation. The bronchoscopy room opens directly on to Ward B. Wards A and B are connected by an atrium, and patients from each ward use facilities on the other.

The probability of a person becoming infected during a one hour exposure to infectious droplet nuclei is estimated to range between one in four and one in 600. Extensive transmission has been observed during exposures as brief as two hours. The American Thoracic Society recommends that if tuberculosis is a diagnostic possibility and bronchoscopy is required then the procedure

should be performed in a room designed to meet acid fast bacilli design specifications; these are stringent.³

We find the epidemiological data adequate to support our view that transmission of tuberculosis occurred in the three patients Scott and Holton described, who originated from different continents. The fact that a research project performed on specimens stored for one year has brought this episode of transmission to light supports the view that fingerprinting of all strains of *Mycobacterium tuberculosis* complex should become routine.

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Length of hip axis and rate of hip fracture

EDITOR,—Ian R Reid and colleagues conclude that increases in the age adjusted rate of hip fracture can be accounted for by changes in the geometry of the proximal femur and that the "epidemic" of hip fractures "cannot be reversed by any single intervention."¹ The assertion that no intervention can reverse the trend is flawed. The epidemic is, after all, one of fractures, not of limb lengthening. Central to the argument that an increase of 5.5 mm in the hip axis, or length of the femoral neck, is associated with an increased rate of fracture is the physics of trauma: the longer the axis the less force is needed to produce a fracture in a bone of given strength. This itself suggests prevention based on (a) the prevention of falls^{2,3} (that is, by identifying those at risk, removing hazards, improving mobility, providing aids, or modifying medical management) or (b) a reduction in the impact of trauma associated with falls (by designing environments to reduce the impact of falls for those at high risk⁴ or improving neuromuscular protective responses and gait⁵ or padding the hip⁶).

In attempts to prevent hip fracture, focusing on the characteristics of bone to the exclusion of other factors (as Reid and colleagues do) is probably mistaken. To reduce the incidence of hip fracture over coming years preventive strategies need to be developed that modify the environment and behaviour of cohorts at risk to prevent falls. Such prevention should reduce both mortality and personal suffering and may also prove cost effective.

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Prophylaxis against malaria

EDITOR,—J H Miller and D A Warrell's observation on British troops' non-compliance with simple preventive measures against malaria is of concern.¹ One of the key issues in prophylaxis against malaria is that the strategies for improving compliance should include both educating the prescribers and counselling the patients.² As a doctor providing medical help to travellers in India, I find the standard advice on prophylaxis against malaria often inadequate, especially among young people travelling for long periods on low budgets. Also, many health advisers overseas have a limited idea about the vastness of the sub-continent and consequent regional variability in advice concerning travel.

Visitors to India are at risk of malaria, which is a problem in many cities.³ The current outbreak in Rajasthan, which is predominantly an outbreak of *Plasmodium falciparum* infection, is causing concern. I did a survey among 452 foreign travellers in Calcutta and found that 21 were taking an inadequate dose of antimalarial drugs and that 111 were not taking any (S Chatterjee, third conference on international travel medicine, Paris, 26-29 April 1993). Inquiry about the reasons for not taking the drugs showed that 45 had been given inadequate information before travelling since they had either not been informed about prophylaxis or had been told that it was not required. A further 33 had decided to stop taking medicines, contrary to medical advice, mainly because of concern about side effects, the complexity of a daily regimen of two drugs, and perceived uselessness. This was based on past travel experience, "friendly" advice to stop the drugs, and confusion because fellow travellers were taking alternative regimens. Twenty five had stopped because of side effects, and the remaining eight comprised a miscellaneous group.

These findings point to areas of conflict concerning compliance with prophylaxis against malaria among travellers. We need to address these facts if we believe that the key to healthy travel lies with improving the behaviour of travellers.

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Travel prophylaxis

Figures in study were incorrect

EDITOR,—Debate about the role of immunoprophylaxis for travellers is to be encouraged, but health care planners should not be lulled into complacency by R H Behrens and J A Roberts's conclusion that vaccination against typhoid and

hepatitis A is not cost effective for society or the person concerned.¹ No such study should group all countries outside Europe, North America, and Australasia into the same category of risk regardless of the itinerary and duration of visit and the lifestyle followed. The authors' observation that 68% of travellers to these areas are not immunised against typhoid is suspect as only the number of vaccine phials for which general practitioners were reimbursed was used to estimate the number of immunised travellers. Many other travellers are already protected, having been immunised within the previous three years (30% at my travel clinic). In addition, the intradermal route allows up to 15 booster doses to be obtained from one phial; many practitioners purchase phials themselves, avoiding unnecessary visits for prescriptions, and increasingly vaccines are administered by NHS hospitals and private clinics. Other travellers are intentionally not immunised because the risk is minimal—for example, in Japan, Singapore, and the West Indies or during brief stays elsewhere in good accommodation. Perhaps only 20%—not 68%—of those at risk may not be immunised.

For hepatitis A the authors considered only prescribed commercial immunoglobulin, not that supplied by the blood transfusion service (10 000 doses annually in the west of Scotland alone). Most trips abroad are brief, for which only 250 mg, half the standard 5 ml dose, is necessary. Also, two doses (more recently one of active vaccine)—not three as used in the calculations of cost—are necessary for fully primary immunisation.

In the calculations of total illness, those episodes occurring abroad are not considered. Confining the total number of cases to notifications confirmed by a laboratory, will yield a poor estimate, especially for hepatitis A: many practitioners avoid non-essential venepuncture, particularly in children. The estimate of 50% underreporting for typhoid and hepatitis A is probably far too small.

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Assumptions were confusing

EDITOR,—We have performed economic evaluations of prophylaxis against hepatitis A in travellers, and two of R H Behrens and J A Roberts's main conclusions¹ are in line with our findings^{2,3}—namely, that hepatitis A vaccination is not cost saving for the average traveller and that vaccination becomes more cost effective than immunisation with human immunoglobulin when the expected frequency of travel exceeds two occasions in the next 10 years. Despite this agreement, we have some difficulties with several assumptions and statements made in the article.

Firstly, the authors state that the cost-benefit ratio of (passive and active) immunisation increases as the seroprevalence reduces to 9.7% (instead of 30%). This seems nonsensical: it can never be the case that the attractiveness of a preventive action decreases when the proportion of susceptible subjects increases since costs remain the same but benefits increase as more infections can be prevented. Secondly, there is substantial confusion regarding the required threshold incidence (or risk) of hepatitis A that would give a cost-benefit ratio of one. The authors state that this

would occur with 1550 cases annually. This is about six times their reported number of cases of 288 but only three times the "corrected" number of 576 in table I. Nevertheless, the authors say that even if the assumed incidence of 0.05% increased sixfold to the incidence of 0.3% per visit reported for Swiss travellers "the costs would still exceed the benefits by a significant amount." How can these two contradictory statements be reconciled? Thirdly, the authors have performed a cost-benefit analysis but use the terminology carelessly when they say that hepatitis A vaccination is not cost effective (in the abstract) or even is inefficient (in the discussion) when they mean not cost saving.

Finally, we think that it is important to specify further the indications for prophylaxis against hepatitis A and agree that it is necessary to design "a suitable protocol which would target high risk non-immune groups." We have derived some recommendations regarding optimal choices in various circumstances based on a cost-effectiveness analysis elsewhere.⁴

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Journals should develop guidelines for economic evaluations

EDITOR,—The cost-benefit analysis of travel prophylaxis reported by R H Behrens and J A Roberts is flawed.¹ Among other things, such an analysis requires a monetary value to be placed on all the benefits resulting from a health care intervention.² The cost-benefit ratios presented by the authors, however, exclude any estimate of the main components of the benefits of the vaccination programmes under consideration—namely, people's willingness to pay to avoid the morbidity associated with the diseases. This means that the cost-benefit ratios reported and all discussion and conclusions based on them are unsafe and could seriously mislead decision makers.

The paper raised the important issue of how the interests of decision makers in health care and of patients can be safeguarded through measures to ensure that economic evaluations are conducted to high methodological standards. Recently, the pharmaceutical industry worked in partnership with the government to produce guidance on good practice in economic evaluations of medicines either sponsored or undertaken by pharmaceutical companies.³ Similar standards should be applied to all researchers. The editors of peer review journals such as the *BMJ* have a potentially important role in upholding such standards. There would be enormous benefit to be gained from journals developing clear policies on the acceptance of economic evaluations, including guidelines for authors.

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**The *BMJ* is currently working with an international group of health economists to develop guidelines for economic evaluations. These should be useful to authors, editors, reviewers, and readers.—ED, *BMJ*

Reporting of air pollution levels

EDITOR,—Two articles in the issue of 22 October reported levels of air pollutants and their possible health effects.^{1,2} Gregor Campbell-Hewson and colleagues refer to ozone levels being "at a two week high" and report a figure of "45 ppb, compared with a daily average of 28.7 ppb over the preceding and following weeks."¹ Was this value an hourly maximum value for that day, the eight hour mean (for which the Expert Panel on Air Quality Standards has recently set a new standard of 50 ppb³), or a 24 hour mean? On how many days over the preceding and following weeks (I presume one week each side) was an equivalent value recorded? The given value is uninterpretable as it stands.

In the other article Alexander Dorozynski falls into an identical trap, reporting that health problems were seen in Paris "as soon as levels of pollution... rise 100 $\mu\text{g}/\text{m}^3$ above the maximum levels said to be acceptable for Europe."² Again it is unclear whether he is referring to hourly or daily measures. He also does not state the form of particle measurement that was used. In Britain particulate matter of $<10 \mu\text{m}$ mass median diameter exceeds 100 $\mu\text{g}/\text{m}^3$ as a daily mean value only during episodes of air pollution. If Dorozynski is using the daily mean guideline in winter (111 $\mu\text{g}/\text{m}^3$) then an increase of 100 $\mu\text{g}/\text{m}^3$ over that value will only very exceptionally be achieved.

With so much inaccurate, and at times scare-mongering, reporting of possible health effects of pollution in the lay press the medical press must ensure that levels of air pollution are reported adequately and accurately. Whenever a measurement of air pollution is recorded the sampling time must be given, and when reference is made to guideline levels these levels should be specified so that the measured value can be compared with them.

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Pigments in breast fluid

What are they?

EDITOR,—The article on assessing symptoms of breast diseases contains a photograph showing that physiological breast secretions, like fluid from breast cysts, range in colour from white to yellow to green to blue-black.¹ The pigmentation is thought to have no diagnostic importance.² But what causes it? About the only truly green compound produced in humans is the bile pigment