Tuberculosis Research Unit for the streptomycin trial,⁴ which subsequently (from 1948) served as a model for randomisation in many later randomised controlled trials? Or is it because the results of the patulin trial were negative and those of the streptomycin trial made medical history?

I have previously quoted Bradford Hill's recollection that his statistical, and my medical, experiences in this field converged when we prepared for the streptomycin project.^{5,6} The patulin trial certainly influenced my thinking. Yet, so far as I am aware, the literature on the modern clinical trial contains no recognition of this trial, despite its many deserving features. Should not this omission be rectified?

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Recent advances in obstetrics

Testing for Down's syndrome carries too much stress

EDITOR,—I would like to highlight some of the points that Philip Steer made about serum screening for Down's syndrome,¹ in the light of personal experience.

My first reaction to the offer of an antenatal appointment to discuss a risk factor of 1 in 165 for Down's syndrome was one of surprise. I thought that this was too low a risk to contemplate amniocentesis. My second reaction was to consider the implications seriously for the first time and to realise my reluctance to go through with the late abortion of a fetus that might have the potential for a reasonably independent and fulfilled life. I thought it important, however, that my non-medical husband should have more information, so we attended the clinic. Ironically, I missed a postgraduate course on antenatal screening in order to do so.

The visit was far from satisfactory. I was persuaded to have more detailed ultrasound scanning, despite having declined amniocentesis. The ultrasound scan showed the presence of chorion villus cysts, a marker for Down's syndrome but present in 3% of normal fetuses. Suddenly the risk of Down's syndrome was increased, but in an unquantifiable way. The consultant now strongly recommended amniocentesis, even if only to prepare or reassure us, and enjoyment of my normal pregnancy was spoiled. I was anxious and confused. What in fact was the risk of Down's syndrome? My husband felt sure that he could not cope with a child with learning disabilities and preferred not to think about it, perhaps hoping that he would feel differently in the event if he had to. He respected my decision not to have a termination, and we both agreed to decline further testing.

Our anxiety persisted to the point of delivery and beyond. I was convinced for a few awful minutes, before reason intervened, that my perfectly normal baby (shocked after a rapid delivery) did have features of Down's syndrome. Now 2 years old, he is generally healthy but has had more than his fair share of medical problems. I wonder whether my experience in pregnancy

had some affect on my ability to copy with his subsequent illnesses.

Serum screening has low sensitivity (0-48% in the studies quoted), causes parents high levels of anxiety, puts normal fetuses at risk from amniocentesis, and is financially expensive. Routine use should be stopped, and adequate counselling of patients is obligatory.

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- 1 Steer P. Recent advances in obstetrics. *BMJ* 1995;311:1209-12. (4 November.)

Figures on screening for Down's syndrome are inaccurate

EDITOR,—Philip Steer's comments about serum screening for Down's syndrome¹ should not go unchallenged. Firstly, it is inappropriate to cite a 48% detection rate. For a 5% false positive rate the estimated detection rate is 59% when dates are used to estimate gestational age and 65% if an estimate based on an ultrasound scan is used.² The Barts Down's syndrome demonstration project, in which 48% of cases were detected, was not designed to estimate the detection rate and was not large enough to do so.

Secondly, a serum screening test carried out on a community basis costs much less than the £80 cited. There are existing NHS programmes which are provided at a quarter of this sum.

Thirdly, contrary to Steer's statement, we know that screening has had a considerable effect on reducing the birth prevalence of Down's syndrome. In the absence of screening and at a time of increasing maternal age, the birth prevalence of Down's syndrome would have increased from 1.41/1000 in 1989 to 1.47/1000 in 1993,³ assuming a natural fetal loss rate of 27% among the women who had terminations. In fact it has decreased from 1.11/1000 in 1989 to 0.92/1000 in 1993. This is equivalent to an increase in the percentage of affected births avoided through screening from about 21% to 37%.

Finally, it is acknowledged that the provision of adequate information before the screening test and appropriate counselling afterwards is important and is not always carried out satisfactorily. This is not the fault of the screening test but is a fault in the way the screening test is delivered to women. It underlines the fact that the test itself is but one component of the screening procedure.

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Epidural analgesia in labour

EDITOR,—The comments on epidural analgesia for labour in Philip Steer's review overlooked important new evidence relating to postpartum backache and a potential hazard of ambulatory epidural analgesia.¹

Steer implies that the use of epidurals in labour

results in increased rates of long term backache. However, the reference supporting this is a retrospective review of only 39% of a population of women who delivered their baby at a given hospital over an eight year period.² Apart from the inherent drawbacks in making causal associations in a retrospective study, recall bias over such a protracted period would tend to render its conclusions unreliable.

On the contrary, a more recent prospective study of 1042 women interviewed during their admission for delivery and again two months later found no difference in the incidence of new postpartum back pain between those who received epidural analgesia for labour (44%) and those who did not (45%).³ The design of this study is more appropriate to assess cause and effect, and the results impressively refute the suggestion that epidural analgesia is a risk factor for "long term backache."

Furthermore, although Steer correctly states that the combined spinal-epidural technique of ambulatory labour analgesia may cause a greater degree of maternal hypotension than conventional epidural analgesia, he does not mention its potential impact on posterior column sensation, particularly proprioception.⁴ This may undermine safe ambulation, although there is no doubt that the ambulatory technique improves maternal satisfaction and that retention of lower limb mobility is an advance, even if actual walking may not be advisable.

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- 1 Steer P. Recent advances in obstetrics. *BMJ* 1995;311:1209-12. (4 November.)
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Transmission of HIV from mother to infant depends on many factors

EDITOR,—I was disturbed at the many unfounded assertions about preventing the transmission of HIV from mother to infant in Philip Steer's review of recent advances in obstetrics.¹ He asserts that transmission rates could be halved if women did not breast feed. This oversimplifies a very complicated reality, in which transmission to infants depends on many other factors as well; women who have HIV related illnesses may be at high risk of transmitting HIV to their infants whether they breast feed or not.

Secondly, Steer asserts that caesarean section reduces transmission by a "further 40%." The evidence for this is far from clear cut, and the study cited does not draw this conclusion. No large controlled trials have ever been done, and no one has ever recommended routine caesarean section for HIV positive pregnant women on the basis of the existing evidence. Nor does Steer even consider that women with HIV might be at higher risk of complications from caesarean section, when there is at least anecdotal evidence that this is so.²

Thirdly, the role of zidovudine during pregnancy to prevent transmission to infants needs to be confirmed. One trial in one country is not enough. Just as the use of zidovudine in most developing countries for this purpose is highly unrealistic, given the cost,³ we might ask whether the NHS would pay for all pregnant women with HIV infection to use it. If not, there is no benefit.

Thus for Steer to reach the conclusion in a few sentences that, with these three (in his eyes) apparently simple precautions, vertical transmis-

sion of HIV can be reduced to less than 5% is almost unbelievable—but he does not stop there. He then wags a righteous finger at women who do not know they have HIV infection for not finding out, by not having themselves tested antenatally, in spite of all the known reasons why women do not get tested antenatally. The Department of Health had a consultation with a large group of women from a range of voluntary organisations involved in HIV/AIDS work in January 1994, in which the problematic issues of antenatal testing were thoroughly aired. It seems that the message—that the supposed benefits to both mother and baby are not at all clear cut—has been lost.

One hospital, Steer says, emphasises to women that “knowing their HIV status may save their baby’s life.” In so many words, he is saying that 95 out of 100 babies of HIV positive women need no longer be at risk of HIV infection. This is patently false. It is ridiculous and dangerous to reduce the situation in this manner, especially since someone might believe him.

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Author's reply

EDITOR,—My review (PS) recommended that pregnant women should be advised to have a test for HIV antibody so that if the results were positive they could opt for interventions to minimise the risk of transmitting the virus to their baby.¹ The discussion of HIV infection was but a small component of the paper, and it was not possible to cite all the documentary evidence in support of this proposal.

We stand by the statement that the evidence for the role of breast feeding in the transmission of HIV is incontrovertible. In our maternity service 85% of mothers breast feed. We know from anonymous testing during 1994 that nine of the HIV positive women who delivered in this unit were not tested and, as far as is known, were unaware of their HIV infection. If eight of these women breast fed it is possible that one or two of their babies became infected with HIV when this might have been prevented by their avoiding breast feeding. In addition, they were denied the opportunity of discussing delivery by caesarean section or treatment with zidovudine.

People have a right to accurate information, and this is important in informing their choice regarding HIV antibody testing. This is an essential aspect of pretest counselling, whether the patient is pregnant or not. When presented with the evidence most mothers we look after who know that they are HIV positive choose both caesarean section and zidovudine. They clearly disagree with Marge Berer's conclusions.

Berer refers to a meeting she attended, together with a variety of representatives from women's groups, hosted by the Department of Health in January 1994. One of us (JRS) attended such a meeting, but after two days of discussions no agreement could be reached between the women's groups' representatives and the medical staff present, with the former being reluctant to accept the value of the scientific evidence being reviewed.

It is not clear to us why Berer takes such issue with our attempts to offer women the choice of methods which we believe reduce vertical transmission. We strongly support further trials of these and other strategies but feel that the current evidence justifies us recommending to women that

they consider interventions to benefit the health of their children.

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- 1 Steer P. Recent advances in obstetrics. *BMJ* 1995;311:1209-12. (4 November.)

Evidence-Based Medicine

Commentaries should be evidence based too

EDITOR,—As enthusiastic supporters of the recent drive towards evidence based health care we eagerly looked forward to the first issue of the journal *Evidence-Based Medicine*, published by the BMJ Publishing Group.¹ When we read it, however, we were disappointed both by the way in which it reviews individual articles rather than focusing on areas of clinical practice and by the lack of objectivity of the commentaries on the articles reviewed.

We were struck by the inclusion of unsubstantiated statements in the commentaries, four of 23 commentaries being unreferenced. The value of such unsubstantiated commentaries has recently been questioned.² Furthermore, no detail is given about the process used to select the authors of commentaries, which leads to concerns about the representativeness of the views expressed and about potential conflict of interest.

There has been a welcome trend towards basing conclusions on evidence from a variety of sources, including randomised controlled trials, meta-analyses, and, particularly, systematic reviews of all published and unpublished studies.³ Selecting individual articles from the world literature moves away from this approach. This could potentially undermine the value of systematic reviews as perceived by the clinical community.

We advocate that the commentaries in *Evidence-Based Medicine* should be substantiated by references, that the process for choosing commentators should be published, and that the findings of systematic reviews should be incorporated in the journal. Finally, we would request that future journals published by the BMJ Publishing Group should be encouraged to include a letters section (*Evidence-Based Medicine* does not do so).

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- 1 Davidoff F, Hayne B, Sackett D, Smith R. Evidence based medicine. *BMJ* 1995;310:1085-6.
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Editors' reply

EDITOR,—Evidence based medicine requires the integration of individual clinical expertise with best available external evidence from systematic research.¹ Good doctors use both. Without the former, practice quickly becomes “evidence tyrannised”; without the latter, practice quickly

becomes out of date. Journals such as *Evidence-Based Medicine* strive to bring both to a readership that can find less than 30 minutes a week to read about subjects relevant to patients. We serve our readers by monitoring both the current literature and other sources (including both the Cochrane and the York reviews) and extracting just the 2% of reports that are both pertinent to front line clinical practice and scientifically rigorous. They are summarised in structured abstracts (as external clinical evidence), and one of our panel of seasoned clinicians (who are required to disqualify themselves if they have any conflicts of interest) places them in clinical context through accompanying commentaries, synthesising the clinical evidence with clinical experience.

The page headed “Purpose and procedure” appears in every issue of the journal and answers the points raised by N J Pearson and colleagues. Because commentators provide the context of personal clinical experience they add references only when they introduce evidence not already cited in the abstracted original. Thus two of the four unreferenced commentaries in our first issue accompanied exhaustive systematic reviews, rendering any further references repetitious. The third commented on placement of a central line under ultrasonographic guidance and provided exactly what was called for: personal clinical experience with the procedure.

Pearson and colleagues' claim that we have ignored systematic reviews is wrong. Our inaugural issue contained a Cochrane review on stroke units and a routinely published systematic review on β agonists, and our next issue contains several more. But good systematic reviews are rare, and they necessarily lag behind important and often definitive trials by several years. Moreover, they are just beginning to address clinically important topics in diagnosis and aetiology.

Although we provide an opportunity for authors to append brief letters to the pages on which their work appears, we have decided not to have a letters section for two reasons. Firstly, authors of the most helpful letters we have received (which are leading to improvements in the journal) do not request publication. Secondly, having a letters section, although it might provide interesting “theatre,” would mean fewer abstracts and commentaries, and we do not think that the trade off would serve our readers.

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Management of blood loss in children of Jehovah's Witnesses

EDITOR,—David Busuttill and Adrian Coppleson's editorial on the management of blood loss in Jehovah's Witnesses touches on, but does not resolve, the clinical and ethical issues of treating children of Jehovah's Witnesses.¹ The authors concentrate on the use of recombinant erythropoietin in the postoperative management of anaemia at the expense of considering other pharmacological agents, such as aprotinin or desmopressin, which can reduce intraoperative blood loss. As paediatric surgeons we are aware of the need to preserve circulating blood volume and have been early to adopt techniques that provide