screening would raise, or the costs of providing a service for women who would not consider termination of pregnancy. We may be able to screen, and might potentially be able to do it cost effectively, but should we do it and, if so, how?

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Authors' reply

EDITOR,—Counselling is an important component of screening, but unless an appropriate level is adopted the cost will be unsupportable. Therefore in our analysis we used two levels: a low cost option (basic information in a leaflet, which was reinforced by a midwife or general practitioner) for all people who might be screened, and expensive genetic counselling (by a nurse specialist) for carrier couples. Unlike David J H Brock, Joan Morris, and Richard A Doherty and colleagues, we are not convinced that the expensive option is needed for carrier women whose partners have yet to be tested. Carrier couples have a 1 in 4 chance of having an affected pregnancy, and the next step is to consider having an invasive diagnostic procedure with the possibility of subsequently terminating the pregnancy. In contrast, carrier women have only a 1 in 199 chance of having an affected pregnancy, and the next step is simply to test their partner. Since this step is implicit in the woman's agreement to be screened we costed only repeating the original information to the partner—the approach taken in the Yorkshire pilot study of over 6000 women.¹

Done this way, sequential screening will be more cost effective than couple screening even if, as Morris claims, only 4% of women change partners between pregnancies. Other options short of full genetic counselling are possible, but more research would be needed to determine their cost effectiveness. Our preferred strategy is disclosure couple screening, which costs no more than sequential screening but retains some of the advantages of full couple screening.²

The marginal (or incremental) costs of detecting mutations additional to $\Delta F508$ were included in our results. These are much higher than the average costs of the single mutation test, provided that under 10% more carriers are detected, and so a full analysis was not included. The incremental cost quoted by David J Torgerson is incorrect: 90% detection of carriers and 75% uptake of screening yields 486 affected pregnancies in 1 000 000 women ($400 \times 75\% \times 90\% \times 90\% \times 2$), not 432, and would cost £158 000, not £332 000. Torgerson's suggestion that screening should be restricted to women already undergoing invasive prenatal diagnosis would be relatively cheap but is unattractive to health planners as it would have little impact on birth prevalence.

Sarah Walters seems to confuse cost effectiveness and cost benefit analysis—for example, lifetime medical costs do not affect the cost effectiveness of detecting an affected fetus. As we stated in our discussion, we chose cost effectiveness because the valuation of life is difficult and involves ethical issues, which fall outside the realm of economics. Others can build on our results to develop a more comprehensive decision analytical model incor-

porating the valuation of all outcomes and costs including treatment.

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Clinical trials and rare diseases

Statistical results should be expressed in different ways, depending on circumstances

EDITOR,—Since the *BMJ* requested that, when possible, the statistical analysis of results should give confidence intervals, the relevance of many studies has become clearer. In recommending a bayesian approach to clinical trials in rare diseases Richard J Lilford and colleagues point out that power calculations are based on the probability of the proposed hypothesis being true, even though the frequentist test giving the P value (the possibility that the null hypothesis is true) is almost always used to justify the results.¹ Perhaps the *BMJ* should encourage authors to present results as the likelihood of the hypothesis being true, whenever this is appropriate.

In some situations the appropriate test is to consider the possibility that the conclusion is wrong (here, a low P value indicates significance), while in others it is to consider the probability that the conclusion is right (here, the higher the P value the greater the significance). On the one hand, when a new discovery is made it is appropriate to consider the possibility that the effect has arisen by chance and to test a null hypothesis. On the other hand, when two treatments are known to be effective the relevant statement is the probability that one is superior to the other by a certain amount. Should we not express our findings in this way?

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- 1 Lilford RJ, Thornton JG, Braunholtz D. Clinical trials and rare diseases: a way out of a conundrum. *BMJ* 1995;311:1621-5. (16 December.)

Trials of adequate size are possible with the right organisation

EDITOR,—Richard J Lilford and colleagues have opened the debate on the difficult problem of clinical trials in rare diseases with insight and clarity.¹ We are concerned, however, that some of the messages conveyed in their article may be open to misinterpretation.

Firstly, readers should not accept that a trial that is not powerful enough to provide a definitive answer is as good (that is, clinically useful) as one

that is appropriately sized. There is a hierarchy of evidence, with some forms of evidence carrying more weight than others. A trial that can produce reliable evidence must be better than one that cannot, although we agree that some evidence from a small trial is usually preferable to non-randomised evidence, even if this is based on large numbers.

Secondly, different parties may interpret the word "rare" in different ways. The authors quote the example of a trial of fetal surgery, which, if it was to be capable of producing a definitive answer, would need to recruit from a population of 12 million pregnant women. They imply that this would be impossible, which we do not accept. A trial of this size has not yet been done in this field, but that is not to say that it is impossible. Examples of trials in rare diseases show that widespread international collaboration is possible. A trial of the management of posthaemorrhagic ventricular dilatation in neonates is currently recruiting from 137 centres in 26 countries. This condition is very rare, and, although recruitment will take several years, the size of the trial has been calculated so that it will be capable of providing a definitive answer to the question being posed. If the main barriers to conducting large collaborative trials in rare diseases are organisational should we not be investing our scarce resources in overcoming these barriers to collaboration rather than relying on evidence from trials of inadequate size that may provide misleading evidence?

We agree with the authors that "any randomised evidence is better than none." We are concerned, however, that this approach may encourage researchers and funding bodies to support inadequately sized trials when trials that may provide definitive answers are possible with the right organisation and commitment from participating centres.

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GMSC's advice on intrapartum care is unhelpful

EDITOR,—The General Medical Services Committee recently issued guidance stating, "We think that practitioners who are going to provide intrapartum care should be the relatively few GPs [general practitioners] who are highly skilled and practised in this area . . . these GPs are referred to as GP obstetricians. Only they should undertake home deliveries and deliveries in GP units."^{1,2}

We believe that this advice is unhelpful as it is likely to reduce the number of general practitioners prepared to attend women in labour. Few would be prepared to have their professional skills judged against some hypothetical standard of "general practitioner obstetrician," would describe themselves as "highly skilled and practised in this area," or would ever exercise obstetric skills at home. It might be argued that this advice seeks only to regulate the current position, but such general practitioners have never argued that they are doing anything more than exercising the skills that all general practitioners should have. The advice is counter to that in *Changing Childbirth* and that of the Royal Colleges of Midwives and General Practitioners,^{3,4} which says that those general practitioners keen to provide care to women in

labour should be enabled to do so. If followed, the advice will cause unnecessary hostility between general practitioners and women at this important stage of their life.

The document raises two other points. Firstly, there is no evidence to support the committee's advice. Delivery outside consultant units for women at low risk is safe, and the skills required for safe intrapartum care are well within the scope of the average general practitioner.⁵ Secondly, there is the "slippery slope" argument. If it is accepted that not all general practitioners will be able to undertake every responsibility in the new primary care led NHS, how is the GMSC to decide which skills are core skills and which lie within the domain of subspecialties in general practice that may develop? Will it be the province of every general practitioner to inject joints, give streptokinase, or advise about insulin regimens?

The GMSC's advice may placate those general practitioners who fear that they will be asked to attend home deliveries or supervise midwives. But the committee would answer the needs of pregnant women much better if it were to point out that midwives are professionals in their own right, general practitioners are not accountable for midwives' actions, and general practitioners with ordinary skills are usually capable of performing the tasks likely to be expected of them when attending women in labour.

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Deaths in police custody are being analysed retrospectively

EDITOR,—The Police Complaints Authority has expressed its concern about deaths in police custody to the Home Office,¹ and the Association of Police Surgeons has identified a need for further research to be undertaken into the issues concerned in these deaths. The two organisations are currently conducting a retrospective analysis of those deaths in custody that have already been investigated by the Police Complaints Authority. It is hoped that this study will highlight key issues that can then be considered by a confidential inquiry.

Vinod Patel's support for a prospective confidential inquiry into deaths in police custody is thus welcome, but the arguments that Patel presents in favour of such an inquiry seem misinformed.² Patel states that, according to the national police authorities, 15 such deaths occurred during the past 12 months. There is, however, no national police authority, and it is unclear where Patel obtained this figure. Certainly, it does not seem to accord with those of the Police Complaints Authority, which supervised inquiries into 25 deaths in police stations during the 12 months ending 31 March 1995. More surprising is Patel's

suggestion that there is evidence of a "hitherto unknown clinical syndrome" that predisposes subjects in police custody to forms of natural death. Patel seems to base this hypothesis on the fact that in only one of the 15 cases quoted "is prosecution of the police officer concerned pending." Deaths in police custody are rarely due to criminal activity by the police, and the absence of such criminality hardly lends support to the existence of an unknown clinical syndrome.

In fact, suicide is the most common cause of death in police custody, with deaths from natural causes forming only a small proportion of the overall total. The second most pressing issue of concern, after the suicide rate, is the management of people who have been arrested because they seem incapably drunk. These detainees seem to be at risk of death from alcohol poisoning, concurrent drug overdoses, or masked head injuries (table 1).

Table 1—Cause of deaths in police stations (or in hospital after collapse in police stations) as stated on medical certificate or established at inquest in cases investigated by Police Complaints Authority, 1994

Cause of death	No
Poisoning:	
Due to alcohol	7
Due to drugs	6
Asphyxiation:	
By hanging	12
By other means	1
Cerebral haemorrhage	1
Skull and brain injury	3
Natural causes	2
Total	32

We agree with Patel that the causes of all deaths in police custody should be ascertained and documented. Any confidential inquiry, however, is more likely to reduce the number of these deaths by identifying risk factors for suicide, promoting suicide awareness programmes, and producing guidelines on the management of drunk detainees than by identifying putative unknown clinical syndromes.

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- 1 Police Complaints Authority. *Annual report of the Police Complaints Authority. 1 January 1994-31 March 1995*. London: HMSO, 1995.
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Role of police surgeons is changing

EDITOR,—Graham Moon and colleagues highlight the importance of the further development of Britain's police surgeon service.¹ The work of the forensic physician (this is now the preferred term for a doctor providing medical care and forensic assessment of detainees in police stations) has changed dramatically over recent years. Since the introduction of the Police and Criminal Evidence Act 1984 the traditional role of a doctor (attending the police station to take blood in drink-drive cases) has changed, and further change is likely with increasing specialisation. A clear distinction between the therapeutic and forensic roles is not possible and has already been criticised.²

The management of people detained in police custody because of substance misuse has been

studied by a working party of the Department of Health, and guidelines are now available for doctors.³ Increasingly, the police request the doctor's opinion on whether a detainee is fit to be interviewed, and guidelines for this are being developed by the education and research subcommittee of the Association of Police Surgeons.

The editorial does not mention the role of appropriately trained doctors in the examination of people alleging assault. The examination of adults alleging sexual assault; child victims (usually done in consultation with a paediatrician); and others alleging violent crime, including domestic violence, is a substantial part of the work. The importance of having sympathetic doctors who can provide an accurate interpretation of the examination findings for the medical report and for court purposes cannot be overemphasised.

A group of enthusiastic doctors exists who are interested in the challenging work of clinical forensic medicine. It is our duty to ensure that they are well trained, skilled, and properly qualified to provide independent forensic medical services to anyone who needs them. There are now initial training courses, a national development training programme to update doctors, and specialist advanced tutorial groups to prepare doctors for the examination for the diploma in medical jurisprudence (the South East and London Group and the Forensic Academic Group in the North).

The Association of Police Surgeons is at the forefront of improving the standard of clinical forensic medicine in Britain, and we therefore look forward to the findings of the Home Office working party on the role of police surgeons, set up at the suggestion of the Royal Commission on Criminal Justice.

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Drugs for atrial fibrillation

Measurement of the pulse deficit is unnecessary

EDITOR,—In the article on drugs in atrial fibrillation a chart showing measurements of the pulse deficit is used to show the effect of digoxin.¹ This perpetuates the myth that measurement of the pulse deficit is useful. The measurement is a normal part of practice in Britain, though in North America it seems to have been abandoned (D Pauline, personal communication). It is a time consuming observation for nurses, for they are taught that the apex and radial counts must be made in the same minute and so two nurses are required.² It does not contribute to management, for the important measured effect of digoxin (or any other treatment) in fast atrial fibrillation is the lowering of the ventricular rate, not the radial rate or the apex-radial difference.

The reason that the practice is widespread in Britain is historical: the pulse deficit was a useful sign in diagnosing atrial fibrillation when electrocardiograms were difficult to obtain (the greater the pulse deficit the more likely that the rhythm was atrial fibrillation). Electrocardiograms have