A total of 67 people (54%) felt that partner notification would have been acceptable, the reasons given being to seek earlier treatment (51; 76%), to prevent infecting others (47; 70%), to plan their lives better if test results were positive (41; 61%), and to live more healthily (34; 51%). Fifty eight men felt that they would prefer not to have been notified, because it might cause unnecessary anxiety (36; 62%), it risks breaking confidentiality (43; 74%), and it can affect being able to obtain insurance and mortgages (20; 34%).

In a separate study of 50 women positive for HIV, 40 women had had a sexual partner subsequent to diagnosis. Of these 40, 24 (60%) had informed their partners and the remaining 16 (40%) had not.

Despite these figures, when the men in our study were asked whether a pilot scheme based on the Swedish model for voluntary partner notification ought to be instituted, 90 (72%) agreed and only 35 (28%) said no. The survey highlighted particular worries among patients such as anxiety and lack of confidentiality, which might be assuaged by appropriate counselling before and after testing, with assurances of confidentiality at all stages. Partner notification has been shown to be efficacious in Sweden[6] and South Carolina,[7] and pilot studies to assess efficacy, with careful audit, are now overdue in this country. This study shows that the majority of patients who have gone through the experience of testing positive for HIV would support this.

DAVID G DANIELS
KATE SWEENEY
PHILLIP D KELL
NICK HULME
SIMON E BARTON

John Hunter Clinic,
London SW10 9TH

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Condoms as primary method of contraception

SIR,—We applaud the ethos behind the new fertility control unit as established in Leeds,[1] but, though we do not dispute the data that Messrs David R Bromham and Richard S V Cartmill provide, we wonder whether there is another explanation for the apparently high failure rate of the condom.

They say that of an admittedly small group of 25 women seeking terminations of pregnancy, 12 said that their partner had been using a condom. Of these, two had used the condom incorrectly and in 10 cases the condom had failed. Before criticising the condom and the way it had been misused we must ask: How many women who claimed that their partner had been wearing a condom were being honest?

In the course of clinical practice we have encountered several women who, after a termination of pregnancy, admitted that they had actually had unprotected intercourse but had subsequently claimed that their unwanted pregnancy was the result of a condom failing. They had done this because they had thought that they would be looked on more favourably by doctors if their unwanted pregnancy was a result of failure of contraception rather than the result of unprotected intercourse. This cohort is hard to quantify, but it should be borne in mind when data on contraceptive failure are considered.

These women have clearly been failed by those who provide contraceptive services. Efforts must therefore be made to improve health education and practical provision of all forms of contraception, especially emergency contraception.

In addition, all those involved in offering terminations of pregnancy must try to ensure that they work in an environment where a woman can be honest about the cause of her unwanted pregnancy. Her future use of contraceptives can then be planned more appropriately with her to reduce the chances of the harrowing experience of a repeat termination of pregnancy.

ELLIS DOWNES
MOHAMAD ANWAR

Department of Obstetrics and Gynaecology,
University of Leicester,
Leicester Royal Infirmary,
PO Box 65,
Leicester LE2 7LX

1 Bromham DR, Cartmill RSV. Condoms as primary method of contraception. *BMJ* 1991;**302**:1150. (11 May.)

Antenatal testing for HIV

SIR,—We read with concern Professor Geoffrey Chamberlain's paper on normal antenatal management, in which he stated that venous blood is checked for HIV antibodies "if the woman is at risk of infection through intravenous drug abuse, having received contaminated blood transfusions, or having a partner who is HIV positive or may be so."[1]

We take issue with this on two counts: the specific recommendation of selective testing in antenatal clinics and the absence of any reference to the need to obtain informed consent.

The question of whether to offer HIV antibody testing to all or to selected women attending antenatal clinics is still a matter of debate. It is not helpful to imply, as the paper does by omission, that there is universal agreement on this. We argue that selective testing leads women to underestimate their risk of infection if they are not in recognised risk groups.

There is strong evidence that a considerable proportion of women attending antenatal clinics who are positive for HIV are not aware of or are unwilling to disclose risk behaviour. A study in New York reported that selective testing failed to detect 86% of HIV infected mothers[2]; two other reports found that only 58%[3] and 45%[4] of those identified as being seropositive had self identified risk factors.

An alternative approach is to offer the test to all women and involve them in the decision making process. This would avoid possible stigmatisation of those designated at risk and lead to HIV testing becoming part of the routine discussion with attenders. A study in Baltimore showed that selective testing would have detected only 57% of seropositive pregnant women; when counselling was offered to all the rate increased to 87%.[5]

This approach is now offered in various clinics in the United Kingdom. Our own study in Riverside Health Authority indicates that this is the preferred choice of both the women and their midwives. Only 10% of attenders and 6% of midwives were in favour of the test being offered selectively.

We believe that the possibility of HIV antibody testing should be discussed with every parturient woman. To reduce the stigmatisation of testing this discussion should be part of the routine booking history. As knowing a patient's serostatus should not make any difference to control of infection (universal precautions should be used in all cases) the only change a positive result produces is in the level of care a woman receives. We strongly recommend that the decision to have an HIV antibody test should be made by the woman after adequate counselling, and not by selective persuasion.

JOSE CATALAN
JEAN MEADOWS

Academic Department of Psychiatry,
Westminster Hospital,
London SW1

YVONNE STONE

West London Hospital,
London W6

SIMON BARTON
NICK HULME

St Stephen's Clinic,
London SW10

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Vaginal bleeding in early pregnancy

SIR,—Professor Geoffrey Chamberlain's article on vaginal bleeding in early pregnancy unfortunately goes a long way to explain why there is so much dissatisfaction expressed by women as to the management of their miscarriage.[1] In an article reminiscent of the textbooks I used to read as a medical student there was virtually no acknowledgment of the problems that I and my colleagues face regularly in dealing with this difficult, emotive subject. There was no mention, for instance, of the use of ultrasonography by general practitioners, the difference between six week and nine week miscarriages, the use of rhesus antibody, or management of subsequent psychological morbidity.

The article states in confident, didactic terms that complete abortion is unusual and implies that all women should be admitted for a dilatation and curettage. Some women feel so alienated from the system that they refuse admission and are managed in the community. These women are rarely seen in hospital, let alone by consultants, which might explain why Professor Chamberlain's view may not be shared so strongly by his general practitioner colleagues.

Despite the fact that at least a fifth of all pregnancies miscarry there is little consensus between general practitioners and gynaecologists as to the most appropriate management guidelines. It was disappointing that Professor Chamberlain did not use the opportunity in his article to explore future management strategies that might be more relevant to the needs of general practitioners and their patients.

D J PAYNTON

Bitterne Health Centre,
Southampton SO9 2DA

1 Chamberlain G. ABC of antenatal care. Vaginal bleeding in early pregnancy—I. *BMJ* 1991;**302**:1141-3. (11 May.)

Management of threatened miscarriage in early pregnancy

SIR,—I was disappointed that Professor Geoffrey Chamberlain suggested taking an outdated approach to the common problem of threatened miscarriage in early pregnancy.[1] I believe that a "wait and see" policy is unjustifiable when ultrasound scanning is widely available.

Our practice is to arrange a scan as soon as

possible—within a few days for light bleeding, or the same day for heavier bleeding—to see whether the fetus is still alive. If it is the woman may be reassured that she has only a one in 10 chance of subsequently losing the pregnancy.[2] If the fetus is dead and there are retained products of conception she may be admitted electively for evacuation within a few days to allow calm, daytime surgery, although if the bleeding is heavy she should be admitted immediately. This reduces the risk of septic abortion and other complications compared with that associated with emergency surgery later. If there is doubt about viability in a pregnancy of only a few weeks' gestation scanning may be repeated a week or 10 days later before proceeding to evacuation if appropriate. When she first presents we also check the woman's blood group and give anti-D immunoglobulin if she is Rh negative.

In general practice the woman's beliefs about the bleeding must be considered. It is worth explaining that the bleeding comes from the mother, not the baby; that in over 95% of cases the outcome is determined before the bleeding starts (as shown by scanning on the first day of bleeding); and that the bleeding does not itself harm the baby. It is dreadful for a woman to wait and see because she may be unwilling to commit herself to the pregnancy if she thinks that she is likely to lose it. The fact that a scan showing a viable fetus means that she has a better chance of having a successful pregnancy (nine out of 10) than she started the pregnancy with (four out of five) is reassuring. A pregnancy full of anxiety can lead to an over-anxious, overprotective approach lasting long into the baby's childhood.

It must be pointed out to the patient that there is no evidence at all that rest influences the outcome. Otherwise she may think that she is to blame if she cannot or does not rest and subsequently miscarries.

Finally, aftercare needs attention. The woman needs to know what to expect after evacuation of retained products of conception so that signs of retained products or infection are acted on early. She and her partner should be encouraged to allow themselves time to recover emotionally from the loss. Many women lose their libido and their interest in trying again for a baby after a miscarriage and should know that this is a normal grief-defence reaction, which usually passes quite quickly. They should be invited to come back to talk about it if they are not getting over their loss in a couple of months.

R C H LYLE

Lower Clapton Health Centre,
London E5

1 Chamberlain G. Vaginal bleeding in early pregnancy—I. *BMJ* 1991;**302**:1141-3. (11 May.)
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Prognosis of breast cancer associated with pregnancy

SIR,—Minerva states that many women and some doctors continue to believe that breast cancer has a poor prognosis if it is diagnosed during pregnancy or within one year of delivery.[1] She then quotes an article from the Memorial Sloane-Kettering Cancer Center, New York, suggesting that cancers associated with pregnancy are no more or less aggressive than others.[2] It should be recognised that this was a review of 56 patients, of whom only 12 were diagnosed and treated before delivery, and 80% of the 12 did not have spread to the lymph nodes. The numbers are small and the proportion of patients without spread to the lymph nodes surprisingly high.

In contrast, a series from the Princess Margaret Hospital, Toronto, which included 154 patients whose tumours were coincident with pregnancy and 96 whose tumours arose during the 12 months after parturition (conventionally termed "the lactation period"), showed a serious reduction in survival for these patients.[3] Patients whose tumours were coincident with pregnancy had a poor survival of 32% at five years and 25% at 10. Relapse free survival was 24% at five years and a dismal 18% at 10 years. Patients whose tumours arose during the lactation period fared a little better, with a five year survival of 39% and a 10 year survival of 35%. Among those whose tumours were coincident with pregnancy there was no difference in survival between patients with and without spread to the lymph nodes, and only 10% of these patients had tumours less than 2 cm in diameter.

The Sloane-Kettering series matched breast cancers that were associated with pregnancy with breast cancers that were not and were of the same stage and compared survival. This does not take into account, however, that patients with breast cancers associated with pregnancy have a strong tendency to present with more advanced disease, presumably because of the pregnancy itself. Such a comparison therefore becomes irrelevant as it is the effect of the pregnancy that is the dominant factor.

R M CLARK

Ontario Cancer Institute,
Princess Margaret Hospital,
Ontario,
Canada M4X 1K9

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Breast cancer screening: the current position

SIR,—Dr J A Muir Gray and colleagues[1] refuse to accept the failure of the British randomised controlled trial of mammography and physical examination to show statistically sound evidence of benefit[2] and accuse unbelievers of "inappropriate use of epidemiology." Their argument, however, is based on the inappropriate use of meta-analysis.

As shown by Professor Nicholas Wald and colleagues,[3] three of the four randomised controlled trials of mammography failed to reach statistically significant benefit for women aged 50 and over. Professor Wald and colleagues suggest that the reason for the failure to show a clear benefit in the Malmö and Edinburgh trials was that both were small studies. If studies of 16 000 women in Malmö and 14 000 women in Edinburgh who accepted screening and were followed up for 10 and seven years, respectively, are "small" then the clinical benefits of such screening, if any, must also be small. As Dr Muir Gray and colleagues rightly point out, "doctors need to know the clinical benefits of such screening and not to be confused by statistical red herrings."

One of the red herrings is the combination of randomised controlled studies and case-control studies in an attempt to show the non-significant as significant. Gullberg *et al* showed that applying case-control methodology to their data from the Malmö randomised controlled trial "improved" the clinical benefit from a relative risk of 0·96 (not significant) to 0·42 (highly significant).[4] Case-control studies should thus not be combined in meta-analyses with prospective randomised trials. Dr Muir Gray and colleagues do not attempt to correct their meta-analysis for such biases.

Dr Muir Gray and colleagues express the opinion that "on the basis of the experience obtained in the early days of the NHS screening programme . . . the quality being achieved was adequate to ensure that this benefit was attainable across the country." In stark contrast, the authors of the report on the Edinburgh trial concluded with the statement that if the defects encountered in their trial "were to persist we would be only spending resources recklessly and to little or no effect."[2]

PETR SKRABANEK
JAMES McCORMICK

Department of Community Health,
Trinity College,
Dublin 2

1 Muir Gray JA, Vessey MP, Patnick J. Breast cancer screening: the current position. *BMJ* 1991;**302**:1084. (4 May.)
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SIR,—Dr J A Muir Gray and colleagues have taken issue with the statement that there is no statistically sound evidence that breast screening has ever saved a life in the United Kingdom.[1] This is difficult to reconcile with the fact that the only trials of screening in this country have been reported as statistically non-significant.[2 3] I suggest that it is an inappropriate use of epidemiology to discount these disappointing results by supporting unjustified meta-analyses of foreign trials which are, to a greater or lesser extent, unreliable or irrelevant, or both. For example, there is good evidence to suggest that the benefits indicated by case-control studies are partly[2] or even totally[4] factitious. Furthermore, the relevance of the more reliable randomised trials must be questioned—for example, there are several reasons why the results from the Swedish two counties study might not be reproduced here,[5] not least of which are the far superior attendance rates in Sweden.

Moreover, Dr Muir Gray and colleagues seem to be selective in their criticism of epidemiological integrity. They cite uncritically an overview by Professor Nicholas Wald and colleagues,[6] which (*a*) claims to describe the current position without any acknowledgment of the detractions of screening; (*b*) includes the breast cancer detection demonstration project, which, as its title admits, was no more than a demonstration project, on the same graph as the randomised trials; and (*c*) claims that randomised trials somehow underestimate benefit because they eliminate selection bias by including non-attenders in the study group.

I do not believe it is acceptable to say that screening saves lives when so many different factors—such as attendance rates; the method and frequency of screening; and input from radiologists, surgeons, pathologists, and community physicians—all contribute to the success of any individual programme. The fact remains that two of the finest centres in the United Kingdom failed to produce a statistically sound reduction in mortality at seven years using annual clinical examination and biennial mammography, and we have embarked on a national programme of triennial mammography alone. I agree that the weight of evidence suggests that screening probably will save some lives (and we could fill the letters column with arguments over how many), but I fear that the number will not be enough to outweigh the damage to the women traumatised in the process or the opportunity costs of the scheme. My original letter was a plea to keep screening in the age group that enjoys the maximum cost to benefit ratio,[7] and I stand by it.

ANTHONY RODGERS

Southmead Hospital,
Bristol BS10 5NB

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