Women vary in their feelings about their womb: for some it is nothing but a nuisance, while for others it is the organ that defines them as women or is essential for sexual enjoyment. The latter women will avoid hysterectomy. In addition, Kuh and Stirling found that women who leave school without a qualification are 18 times more likely to have a hysterectomy than those who leave with a qualification.²

After publicity in newspapers I was contacted by over 120 women who had either awoken from an operation to find that they had had an unexpected hysterectomy or had been coerced into consenting to one. Many had been told "You will not feel any less of a woman afterwards," only to find that they did. It is difficult to tell a doctor that you feel something that he or she has said you will not feel. Many of the women were even more dismayed to be told after the operation that their wombs were normal. A study needs to be carried out into the psychosexual consequences of hysterectomy that the patient knows was unnecessary, that was unexpected, or that was done on a reluctant patient.

Twenty years ago women with breast lumps had to sign away their breast before exploratory surgery. Now, thanks to a battle fought and won by women and aided by some good and kind surgeons, they go for a biopsy knowing that they will still have that breast when they awake, that a histological diagnosis will be made, and that they can then discuss treatment options with their surgeon. No such courtesy is afforded to women with benign uterine disease, who are treated by hysterectomy and often castrated with little prior investigation.

A psychosexual study of women who have undergone essential hysterectomy for endometrial or invasive cervical cancer would give a better idea of how women feel about losing their wombs when the operation is life preserving.

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Genetic liability to osteoarthritis may be greater in women than men

EDITOR,—Tim D Spector and colleagues report a study of genetic influences on osteoarthritis in monozygotic and dizygotic female twins.¹ They conclude that their results show a clear genetic effect for radiographic osteoarthritis of the hand and knee in women (ranging from 39% to 65%) independent of known environmental or demographic confounders. In his commentary on the paper John Hopper states that these figures must be considered to be an upper bound of the genetic influence.¹

In a questionnaire sent to the Finnish twin cohort in 1990 we asked about the prevalence of osteoarthritis (in any joint) diagnosed by a physician.² Self reports tend to be specific but not very sensitive. In the age group 33-60, 1757 male twin pairs (577 monozygotic and 1180 dizygotic) and 2338 female twin pairs (836 monozygotic and 1502 dizygotic) responded. Both twins reported osteoarthritis in eight (1.4%) monozygotic and 24 (2.0%) dizygotic male pairs and in 26 (3.1%) monozygotic and 43 (2.9%) dizygotic female pairs. Only one twin reported osteoarthritis in 66 (11.4%) monozygotic and 155 (13.1%) dizygotic male pairs and in 85 (10.2%) monozygotic and 219 (14.6%) dizygotic female pairs.

In male pairs the tetrachoric correlation coefficient for liability to osteoarthritis was 0.34 (SE 0.12) in monozygotic pairs and 0.38 (0.07) in dizygotic pairs. Model fitting indicated that the data were adequately accounted for ($\chi^2 = 2.09$, $P = 0.72$) by a model with shared (non-genetic) familial effects accounting for 37% of the total variance in liability to the disease and unshared familial effects accounting for 63%. Models that included genetic effects fitted significantly worse.

In female pairs the tetrachoric correlation coefficient for liability to osteoarthritis was 0.62 (0.07) in monozygotic pairs and 0.42 (0.06) in dizygotic pairs. Model fitting indicated that the data were best accounted for ($\chi^2 = 3.82$, $P = 0.28$) by a model with additive gene effects explaining 44% of the variance in liability to the disease, shared familial effects accounting for 20%, and unshared familial effects accounting for 36%. Models that did not include genetic effects fitted significantly worse.

Our data for women are consistent with those of Spector and colleagues,¹ who report that 41-46% of the variance in the narrowing trait score for all joints in their subjects was accounted for by additive genetic effects. In contrast, our results for men indicate that genetic effects may be modulated by sex or by environmental factors distributed differently between men and women. We are currently investigating our affected pairs. Until now, however, investigators have held genetic defects responsible for only a small fraction of cases of osteoarthritis.

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Hospital at home

Research is needed into organisation and development as well as cost effectiveness

EDITOR,—I agree with Sasha Shepperd and Steve Iliffe that the cost effectiveness of hospital at home services is still in question.¹ I am less sure that we will know the answer in 1997 when the results of randomised trials become available. The heterogeneity of the schemes will greatly reduce the generalisability of the randomised trials, which, to be feasible, have had to be restricted to looking at patients with selected conditions.

I have been involved in the evaluation of both hospital at home and other, similar schemes. They varied greatly in their organisation and aims. The following observations arise out of my contact with these services.

The secret of success seems to be, firstly, that such services are run from a hospital base or at least with strong commitment from hospitals.

This is essential both to ensure that patients are enrolled into the service and to effect the reduction in hospital beds that should result from the schemes. Secondly, the schemes need to be firmly embedded in existing mainstream services. This will permit more flexibility in the use of staff and less duplication and may resolve problems of recruitment. A full time coordinator is essential to ensure communication across primary and secondary care. Thirdly, we need to develop the technological side of home care. Until now, schemes in Britain have not been very revolutionary or innovative. In the Netherlands the Institute for Applied Home Care Technology has used technological developments to the maximum to keep people at home.² In France and Sweden home care services also have a high degree of technological input. In Britain, however, a survey found that only 15% of schemes provided or were planning to provide specific technological services.¹

Several issues have resulted in the inability of hospital at home schemes to achieve their potential. These include the difficulty of coordination across the social and health sectors as well as the community and acute trusts. I believe that we are evaluating these schemes too early and are in danger of dismissing them too fast. If we put our energy into researching only cost effectiveness and exclude research on organisation and development then we are at risk of throwing the baby out with the bath water.

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- 1 Shepperd S, Iliffe S. Hospital at home. *BMJ* 1996;312:923-4. (13 April.)
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Scheme in Peterborough is expanding

EDITOR,—Sasha Shepperd and Steve Iliffe raise some important questions about hospital at home services and suggest that their future is uncertain.¹ We confess a personal interest. One of us (JKA) was involved in the early phase of the scheme in Peterborough, another (JWM) was responsible for its extension to early postoperative care of patients with fractured hips,² and GAP and MP continue to use the scheme for their (orthopaedic) patients. A study of early discharge of patients with hip fracture was carefully constructed to avoid bias.³ The catchment area of the orthopaedic service is considerably larger than the area for which the hospital at home scheme is available. This geographical factor eliminates the possibility of a conscious or unconscious bias in the selection of patients for early discharge into the scheme. The groups of patients studied were found to have identical physical and mental characteristics. Hollingworth *et al* examined the two populations as a whole to assess the effect of the availability of a hospital at home service.⁴ They concluded that selection bias did not occur.

The initial scheme in Peterborough was a tangled procedure and unpopular with general practitioners. The current model, fully incorporated in primary care,⁵ has endured since 1979 and continues to expand. At a time when health services all over Britain are susceptible to rationalisation, the health of the Peterborough model has ensured its survival. It should be remembered, however, that a small scheme with patients scattered over a large geographical area will have higher costs per patient than a large scheme with a heavier geographical concentra-

tion. Secondly, inadequate resources will be as detrimental to outcomes as they are in an institutional setting. Finally, the local clinicians should determine the operational policies according to local needs; the special interests of the general practitioners; and the availability of specialist nurses, physiotherapists, and occupational therapists.

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Use of placebo in studies of postoperative vomiting is unethical

EDITOR,—Minerva refers to a paper quoting an incidence of postoperative vomiting of 22% despite the use of an antiemetic drug and describes this as "a depressingly high figure."^{1 2} I agree, but I find even more depressing the fact that researchers in 1996, with the consent of ethics committees, should think it justifiable to use placebos in a study of this nature.

The incidence of postoperative nausea and vomiting, when no measures are taken to prevent them, has been known for at least 30 years and confirmed in numerous studies, as summarised in a fairly recent review article.³ It averages about 30% but may be up to 80% or more in susceptible patients after certain operations. The paper to which Minerva refers confirmed this yet again. Similarly, ways of reducing the incidence of this complication are well known, the use of antiemetic drugs being only one. Scores of publications have shown small differences in the effectiveness of various drugs.

Significance may be increased by use of placebos, although an acceptable alternative is to use a known standard rather than a placebo for comparison. Statistically the study in question is flawed, as neither the anaesthetic techniques nor the operations were rigorously standardised. The study was carried out in 35 departments in eight countries—something that is hard to organise and impossible to supervise. Perhaps it is time for all journals to follow the example of the *BMJ* in requiring authors to describe their sources of funding and any conflict of interest.

I am sure that I am not the only anaesthetist (I retired 12 years ago) who has been able to reduce the incidence of postoperative vomiting to about 15%. This is still too high, although the figure includes many patients who vomit once during recovery of consciousness and then have no further trouble.

Sound statistics is one aspect, good patient care is another. What justification is there for the authors of this and similar papers to subject a predictably high proportion of their patients to a distressing complication to show, yet again, that a

new drug is slightly better—or maybe, as these authors admit, slightly worse—than preceding ones? This is what I find depressing.

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Efficacy of colchicine in familial Mediterranean fever is well established

EDITOR,—In their grand round on reactive systemic amyloidosis A R Allen and colleagues state that, while colchicine prevents the development of renal amyloidosis in familial Mediterranean fever, it does not abolish febrile attacks.¹ The latter remark is not true since three double blind clinical trials performed in the early 1970s showed that daily administration of 1-2 mg colchicine prevents febrile attacks in familial Mediterranean fever.²⁻⁴ In a series of 350 children colchicine induced complete remission of the attacks in 64% of the patients and partial remission (a significant decrease in the frequency and severity of attacks) in 31%.⁵ Moreover, colchicine can consistently abort attacks of familial Mediterranean fever if taken at the onset of symptoms.⁶ Thus the efficacy of colchicine in familial Mediterranean fever is well established.

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Proposed academy of medicine

Proposal is a fudge between academy of medicine and of health

EDITOR,—I agree with Richard Smith that an academy of medicine is required¹; I argued constantly for such an academy throughout my six years on the council of the Royal College of Obstetricians and Gynaecologists. What I am not certain about is whether it should be an academy of health (to include a broader constituency) or an academy of medicine. If it was an academy of medicine then it would, and should, be the official (non-trade union) voice of the medical profession. In such a scenario doctors should make decisions and resolutions, albeit with the benefit of advice from non-medical people, whether sociologists, journalists,

or lawyers. Alternatively, an academy of health could be invoked, which would require a much broader constituency at the decision making level. The proposal in Smith's editorial seems something of a fudge between the two. As such, I would not support it; it would neither be a medical representative body nor (with three quarters of the members being doctors) satisfy legitimate aspirations of non-medical professionals.

The other issue on which Smith's editorial is silent is the principles governing membership of the academy and any governing body (council). In my opinion, at least half the posts would have to be filled by election so that a feeling of ownership could develop. The central body could include the presidents (and perhaps secretaries) of all the medical royal colleges. In this way the new body would sit above the royal colleges and act as a conduit by which the profession could have a dialogue with the government.

It would be interesting to know of any experience that other countries have had with such a body.

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Academy of Medical Royal Colleges has its own, independent secretariat

EDITOR,—Richard Smith's editorial on the proposed academy of medicine for Britain contains a misstatement of fact that may lead to a misunderstanding of the role of the Academy of Medical Royal Colleges and its relations to the Royal Society of Medicine.¹ Furthermore, the Academy of Medical Royal Colleges was not party to the consultation paper on a possible academy of medicine in Britain.

During the past few years the Academy of Medical Royal Colleges (formerly the Conference of Medical Royal Colleges, founded in 1976) has been evolving to become more cohesive and effectively organised so that it can speak with a clear voice for the medical royal colleges and their faculties. More than two years ago the decision was made to take the steps necessary for this body to become a limited company and thereby acquire the legal status to rent premises and employ staff. In the course of this process the decision was made to change its name to the Academy of Medical Royal Colleges, to reflect more closely its roles in maintaining and enhancing postgraduate and continuing medical and dental education in the reformed NHS.

Another noteworthy development was our decision, in April, to invite the presidents of the Royal College of Physicians of Ireland and the Royal College of Surgeons in Ireland to join us as full members instead of having observer status. This recognises the longstanding collaboration between the colleges of the United Kingdom and Ireland on a number of issues and strengthens our voice in European medicine.

The chief executive of the Royal Society of Medicine does not "supply the conference with a home and a secretariat." The Academy of Medical Royal Colleges rents space from the Royal Society of Medicine and employs its own, independent secretariat. Nor can it be claimed that the chief executive has been involved in any way in the foundation of the Academy of Medical Royal Colleges.

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