in our hospital between 1990 and 1993 and obtained similar findings; in addition we found that the satisfaction was maintained during over three years of follow up.

A total of 223 women underwent endometrial resection. Five required an intraoperative hysterectomy, and the remaining 217 women were sent a questionnaire last September. Altogether 204 (94%) women replied. The mean follow up time was 25.1 months (range 9-44 months). Sixty six women had been amenorrhoeic since surgery, and a further 94 described their menstrual loss as light.

We found a high degree of satisfaction associated with this operation, with 154 women describing it as having been a lot of help and a further 29 women as having been of some help. Only 21 women thought that the operation had been of no help. This benefit seemed to be sustained with prolonged follow up. Among the 31 women followed up for between 31 and 36 months 27 described the operation as having been a lot of help. Even among the 21 women followed up for between 37 and 42 months 16 described the operation as having been a great help. Twenty women subsequently had a hysterectomy.

Like Pinion and colleagues, we found that preoperative dysmenorrhoea did not seem to affect subsequent satisfaction. We found, however, that younger women seemed to be less satisfied than older women. The proportion of women who said that the operation had been a lot of help was seven (41%) of the 17 aged under 34; 35 (57%) of the 61 aged 39 or under; 63 (83%) of the 76 aged 40-44; 42 (76%) of the 55 aged 45-49; and all of the 12 aged 50 and over. The proportion of women under 40 who subsequently had a hysterectomy was three times that of women aged 40 and over (11 (18%) of 61 v 9 (6%) of 143).

We believe that younger women should be warned that they may find endometrial resection less helpful than older women, although, given the well documented morbidity associated with hysterectomy, even in this group endometrial resection may have many advantages. Additionally, many younger women thought that the opportunity to choose their surgical treatment was important. Four fifths of our patients were in full or part time employment at the time of their operation, and many commented that they were too busy to consider a hysterectomy. Women are increasingly demanding alternatives to hysterectomy in the surgical treatment of menorrhagia, and, like Pinion and colleagues, we think that endometrial resection can be offered to women with dysfunctional uterine bleeding as a first line surgical treatment. They can be reassured that four out of five women find the operation helpful and that this high level of satisfaction is sustained, even with prolonged follow up.

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Authors' reply

EDITOR,—Anup Sharma and colleagues point out that serious complications may occur during hysteroscopic surgery. It was because of such anecdotal reports that we initiated our randomised trial. Pauli Kajanoja states that we disregarded the fact that 17 women underwent hysterectomy and 11 had a repeat hysteroscopic operation. On the contrary: analysis in randomised trials is based on

"intention to treat," so the recovery times reported include those for women having further treatment. The levels of satisfaction are those reported by the women themselves at 12 months, again independent of whether they had further treatment. A recent nationwide Scottish audit of hysteroscopic surgery reported similar low rates of complications and high levels of satisfaction.¹ There have been anecdotal reports of serious complications during laparoscopic hysterectomy. If this procedure is to replace traditional abdominal or vaginal hysterectomy for menorrhagia it must be assessed in a similarly rigorous manner.

The prevalence of urinary symptoms is known to be high. With regard to Julie Hotchkiss's comments, the preoperative symptom questionnaire was completed in the presence of a doctor whereas the postoperative questionnaires were completed by the patients on their own at home, and this accounts for the difference in definition of nocturia. Does C J Kelleher seriously think that we should screen the general population for urinary symptoms and advise surgical treatment, which may itself have serious complications? This makes no more sense than screening for menstrual problems, to which half of women admit.²

Hugh O'Connor and colleagues state that the title of our paper was misleading. The women had a clinical diagnosis of dysfunctional uterine bleeding. Many women at hysterectomy or hysteroscopy were found to have small fibroids, but in most cases this was a histological diagnosis that would not have altered their clinical management. The trial was designed to compare a new treatment with standard management, and the method of hysterectomy was not specified. At the time of the trial 80% of hysterectomies in Britain (not just Scotland) were performed abdominally. Since then, principally because of the new alternatives and trials such as ours, the best method of performing hysterectomy has been much debated and there has been a move towards vaginal hysterectomy, although to our knowledge no randomised trial of the two operations has been published.

The high failure rate in the group who had hysteroscopic surgery included women who could not be treated successfully by hysteroscopic surgery as a one step procedure as well as true failures of treatment. The rate would undoubtedly have been lower if the women had been preselected for hysteroscopic surgery as in previous reported series. Our trial more genuinely reflects the place of hysteroscopic surgery in clinical practice. We now have follow up data at two years, and the assumption that the failure rate continues to be high is wrong. In addition, the trial was carried out fairly soon after the introduction of hysteroscopic operations. This is reflected in current improved success rates in Aberdeen.

We stand by our recommendation that women should be offered hysteroscopic surgery as an alternative to hysterectomy for dysfunctional uterine bleeding, but we agree with O'Connor and colleagues that much more can be learnt about new surgical techniques from appropriate randomised trials.

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Clonality in Langerhans' cell histiocytosis

EDITOR,—Finbarr E Cotter and Jon Pritchard rightly state that the finding of clonality in Langerhans' cell histiocytosis might provide "the first solid clue to defining the aetiology of the condition in 100 years." As Langerhans' cell histiocytosis is probably a clonal neoplastic disorder² the occurrence of both localised (meaning strictly localised to the bone) and disseminated forms as well as both non-familial and familial cases suggests that it may arise by a two step mutational process, similar to that in the development of retinoblastoma. The two mutation hypothesis was first formulated in 1971 by Knudson, who postulated that two independent events are necessary for a cell to acquire the potential to become neoplastic.³ This notion has now been supported by molecular genetic data in both retinoblastoma and Wilms' tumour. Certain paediatric tumours, whether hereditary or non-hereditary, arise as a consequence of the inactivation of both alleles of one tumour suppressor gene.

Data on 223 patients gathered from four review articles⁴⁻⁷ suggest that Langerhans' cell histiocytosis might also develop as a consequence of two rate limiting events (figure). χ^2 Analysis shows

Distribution of localised (n=75) and disseminated (n=148) forms of Langerhans' cell histiocytosis by age, according to four major review articles⁴⁻⁷

a significant difference in the age at diagnosis between the 75 patients with single bone lesions and the 148 patients with multiorgan disease ($P < 0.001$). This observation is consistent with the earlier age at onset of bilateral retinoblastoma. Also in accord with the "two hit" hypothesis is the fact that in 16 familial cases of Langerhans' cell histiocytosis (five twins, three siblings; literature available on request) all the affected members had multiorgan disease. Although the number of hereditary cases of Langerhans' cell histiocytosis is too small to permit analysis of the type that is possible for retinoblastoma, the data strongly point towards a two step process.

Remarkably, congenital Langerhans' cell histiocytosis occurs solely as the cutaneous or disseminated form and never as the localised form. The cutaneous form should be considered to be disseminated, however, as it seems to be different; the patients are primarily infants and have an excellent prognosis, and the lesions often regress spontaneously. The cutaneous form may represent a special germline form in which the second event is atypical, perhaps like in neuroblastoma IV-S.

Put together, these data predict the existence of a tumour suppressor gene for Langerhans' cell histiocytosis. Genetic linkage analyses in disseminated Langerhans' cell histiocytosis should allow the gene to be localised. Similarly, evaluation of loss of polymorphic markers (studies of loss of heterozygosity) in localised and disseminated Langerhans' cell histiocytosis will help to define the region of interest. Finally, careful karyotyping of patients with the disease might show the

presence of a specific deletion that would include the tumour suppressor gene.

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Palliative care in terminal cardiac failure

EDITOR,—In their review of recent advances in cardiology John McMurray and Andrew Rankin have nothing to report on the neglected subject of terminal cardiac failure.¹ There is little research on this and apparently little perception of the need for palliative care. This is exemplified by a survey I performed of 10 hospices in north east London and Essex. All responded either to a questionnaire or to telephone contact. One hospice had no inpatient beds, and another was a specialist AIDS unit. Of the remainder, four would consider admitting patients with a primary diagnosis of end stage cardiac failure. Only two of these had admitted patients with this condition in the past 12 months: one had admitted one and the other "less than five" in the past year. This seemed to be matched, however, by the demand for hospice beds for patients with cardiac failure. The maximum number of requests was "less than five." Three other hospices had only one or two inquiries a year. The others had no applications at all.

There seems to be virtually no demand for palliative care beds for patients with cardiac failure in the area surveyed. This may be because these patients receive treatment to control their symptoms from their general practitioners, cardiologists, or general physicians. It may also be related, however, to a lack of appreciation of the poor prognosis of this condition in patients unsuitable for transplantation. Research is needed to ascertain whether palliative care in a hospice setting improves the quality of these patients' last days.

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Nutrition and lung health

EDITOR,—We agree with M K Sridhar that antioxidants may ameliorate lung disease caused by exogenous oxidants such as cigarette smoke but would contend that the focus needs to be shifted from individual antioxidants to a more global measure of antioxidant defences.¹ There are several antioxidant vitamins, and many other

compounds such as urate, bilirubin, albumin, and non-protein thiols, whose contributions to the body's antioxidant defences may be neglected by measurement of either the dietary intake or the plasma concentration of an individual antioxidant.²

The ability to measure the body's antioxidant defences directly rather than the contributions of some of its individual components may provide a more accurate picture, one example being the assessment of the total plasma antioxidant potential, which measures the ability of plasma to retard in vivo a reaction mediated by oxidants.² Thus while several studies have shown reductions in plasma ascorbate concentrations in smokers, these may be due to either dietary effects or consumption of the vitamin by the increased oxidant stress of smoking.³

We have directly measured both plasma ascorbate concentration and plasma antioxidant potential in smokers.⁴ While both were reduced in smokers compared with non-smokers, there was no correlation between the plasma antioxidant potential and ascorbate concentration in individual subjects.⁵ Measurement of a single antioxidant molecule in plasma would therefore seem unlikely to provide an accurate assessment of total plasma antioxidant defences. Similarly, the effect of a single dietary intervention (increased dietary intake of ascorbate, α tocopherol, or β carotene) may be masked by other, unidentified dietary changes with opposite effects. Furthermore, substances such as ascorbate may be pro-oxidant in high doses, at least in some groups of patients.⁶

Under such circumstances it would not be surprising, as Sridhar notes, if a given dietary intervention produced no benefit or was shown to be harmful. We urge that antioxidant defences should be considered as an integrated whole rather than as an incomplete sum of partially measured, fragmented parts.

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Care in a midwife managed delivery unit

May not be the best option

EDITOR,—V A Hundley and colleagues state that their objective was "to examine whether intrapartum care and delivery of low risk women in a midwife managed delivery unit differs from that in a consultant led labour ward."¹ They conclude that "midwife managed intrapartum care for low risk women results in more mobility and less intervention." Since 38% of the midwifery group became at high risk antenatally and had their entire intrapartum care in the consultant unit, however,

this conclusion cannot be drawn with confidence and it is fair to state that the objective was not achieved. To compare correctly intrapartum care and delivery of low risk women it would be necessary to exclude all those women who became at high risk antenatally and concentrate solely on the remaining low risk women admitted to the respective delivery units.

The finding of greater mobility in the midwifery group was hardly surprising since the 46% who delivered in the midwives unit had no access to fetal cardiotocography or electrocardiographic monitoring or epidural analgesia. Furthermore, to state that there was "less intervention" in this group is meaningless since intervention is not defined and rates of induction, augmentation, and operative delivery were identical.

We were intrigued by the finding of a reduced incidence of fetal distress in the midwifery group: no attempt is made to define this term, and the Apgar score, cord pH, and rate of operative delivery were identical in the two groups. In fact, the authors state that more babies required resuscitation in the midwifery group, which they attribute to greater use of naloxone. Use of opiates in both groups was identical. These findings are inconsistent.

Also highlighted is a lower rate of episiotomy in the midwifery group, while an increased incidence of third degree tears is ignored. A similar association was recently commented on by Henriksen *et al.*² Of 31 outcome variables analysed, only two are identified as significant: episiotomy ($P=0.04$) and neonatal resuscitation ($P=0.05$). Simple statistics implies that at least one of these was probably a chance finding.

We would be interested to hear from the authors about the frequency of problems in the third stage in the low risk group who required transfer to the consultant unit.

On the basis of the data presented we question the authors' conclusion that a midwife managed delivery unit is "the more effective option."

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Analysis is invalid

EDITOR,—V A Hundley and colleagues have missed the opportunity to contribute usefully to the debate about the safety of non-consultant intrapartum care because their analysis of their work is flawed.¹ They state that their objective was "to examine whether intrapartum care and delivery of low risk women in a midwife managed delivery unit differs from that in a consultant led labour ward." To achieve this they randomised low risk women at booking, provided all the women with the same standard antenatal care, and compared various maternal and fetal outcomes between the two groups on an "intention to book" basis. But such a comparison is invalid because the two groups were no longer comparable (that is, no longer consisted of only low risk women) at the time (the onset of labour) of the intervention.

An analogy might help. Say one wanted to investigate the antihypertensive effect of a new drug in women with newly diagnosed hypertension. Women are screened (a single measurement of blood pressure), classified as hypertensive on the basis of the result, and immediately randomised to receive a new drug or placebo three