a repeat in another year.

On the occasions on which two consecutive smears are reported as showing 'no endocervical cells' I am left wondering whether timing in the cycle makes a difference, since there are undoubtedly some times when the cervix appears dry and the quantity and quality of the sample seems poor. Our local histopathologist does not agree with this suggestion but then he is not taking the sample. It would be interesting to know what other general practitioners do in this situation.

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Reference

1. Mutch F. If the cytology report from a cervical smear states 'no endocervical cells seen' should the test be repeated? *Br Med J* 1988; **297**: 787.

Women's experiences of miscarriage

Sir,

Trevor Friedman (November *Journal*, p.456) raises some important points in his paper on women's experience of miscarriage. Unfortunately I believe his paper is inherently flawed.

His study only includes women who were admitted to hospital. As most spontaneous miscarriages are managed by general practitioners in the community, a question central to a study of this kind must be, 'Why was the woman admitted?'. Although medical indications might make up a substantial proportion there is no doubt that many women are admitted because the general practitioner feels the woman or family cannot cope in the community with primary health care. Reasons for this will be numerous but will include dissatisfaction with the treatment offered by the general practitioner.

Does the mental state of women being admitted to hospital differ from those who are not? Does the psychiatric morbidity of women after a miscarriage depend upon whether they were admitted or not?

Is not the very title of the paper misleading as it deals with women whose general practitioner did not manage their miscarriage?

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Health care in deprived areas

Sir,

Dr Bedford's excellent editorial (October *Journal*, p.398) raises once again the important issue about the link between socioeconomic deprivation and poor health. The main problem in the white paper *Promoting better health*, of increasing the proportion of income from capitation fees, has been highlighted by Douglas Black[1] and re-emphasized by Michael Drury.[2] The recent Consumers' Association report[3] has shown once again that what patients really want is more time with their general practitioners, not less. The capitation fee change will have the opposite effect.

Both Alastair Donald[4] and Michael Pringle[5] have recently suggested that the capitation issue will have major adverse implications for patients in areas of deprivation. This, and the imposition of high targets for preventive procedures, are likely to widen the health and quality divide in primary care. At the recent RCGP spring general meeting the chairman of council accepted as a reference to council the following resolution from the Wessex faculty 'This meeting asks council to note with concern the findings of the recent publication *The nation's health*, in particular that the social class gap in mortality and morbidity has shown no improvement since the Black report, and in many aspects has widened. This meeting further asks council to urge the government to take these findings into account in its future health care planning and social policy'. We would endorse this and submit that this issue is a major priority for the government, the medical profession and this College.

As Dr Bedford remarks, we should not talk about the inner cities, but rather areas of deprivation. It is important that we do not overlook the 'forgotten' areas of deprivation, which are our large peripheral council estates, where unemployment, morbidity, poor housing, and numbers of pre-school children are high. A commitment to these areas does not just require the general practice deprivation supplement, but also more targetted resources for nursing, health visitor, midwifery and community psychiatric services.

It is now nine years since the Black report was published and it appears that this major issue is still largely neglected.

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References

1. Black D. General practice observed. *Br Med J* 1989; **299**: 119.
2. Drury M. General practice observed. *Br Med J* 1989; **299**: 518.
3. You and your GP. *Which?* 1989; October.
4. Donald A. Time and targets. *Medical Monitor* 1989; **2**: 11.
5. Pringle M. The quality divide in primary care. *Br Med J* 1989; **299**: 470.

Effect of small group education on the outcome of chronic asthma

Sir,

In their letter (November *Journal*, p.479) responding to our observations on their paper, White and colleagues imply that we are advocating the use of a more complex methodology in the search for statistically significant results. This was not our intention, indeed rather than putting forward an overly elaborate analysis we were proposing a more closely focused approach.

We take this opportunity to make some more general comments which we hope will be of use to other researchers.

The aim of any clinical trial is to uncover the real effect of the intervention being evaluated. To aid this process and the reporting of such studies in the medical journals a set of fundamental recommendations suggested by Professor Pocock have been generally accepted by medical statisticians and epidemiologists.[1] The paper by White and colleagues contravened several of these recommendations.

1. The study should identify a small set of patient responses (primary endpoints) in advance of carrying out the study and to be used in the evaluation of the trial. The asthma study had nine measures of morbidity but no indication of their relative importance. Thus it is hard to know what conclusions could have been reached had only some of the measures shown a consistently significant difference between the intervention and control groups.

In their letter the authors say that they 'confirmed the null hypothesis so uniformly ..' Apart from the fact that one cannot confirm an hypothesis only attempt to refute it, their failure to demonstrate a significant difference between the groups of general practitioners (not the patients as stated) has two interpretations. Either the intervention does not have the clinical effect that the researchers were looking to detect or the study lacked the power to detect the true clinical effect at that level of significance owing to a small study size.

However, neither the reduction in morbidity the authors considered clinically

important enough to detect nor the power of the trial to detect that reduction are reported. If the power of the study is low (which is likely given the small numbers in each group) then lack of significance is not proof of lack of effect. If the size of the effects found and their confidence intervals had been reported we would have been able to make a sensible judgement.[2]

Finally, the authors in their reply state that 'significant differences are not in themselves enough to reject the null hypothesis'. It is not clear what they mean. If they mean that in a study with 54 significance tests we should interpret the odd significant results with caution, we could not agree more. We oppose the slavish use of *P*-values.[3]

It would have been better if (say) three endpoints had been used, for example a measure of morbidity, asthma remedy use and health service resource use. These three endpoints would have had a clear interpretation and would have been measuring different outcomes.

2. Trials with repeated measurements of response over time require a prespecified policy for statistical analysis, aimed at a single specific hypothesis of interest-repeated significance tests at each time point should be avoided. White and colleagues report nine significance tests at half-yearly intervals, 54 in all. The analysis of variance for each morbidity measure at the end of each time period tests a different hypothesis. Each relates to a different clinical effect and a different time after which one expects these effects to be observed.

If one is interested in the effect on morbidity over time, then the presence of a time trend can be tested using a multivariate analysis (as we suggested) which allows for the correlation between general practitioners' scores over time and which more efficiently uses the repeated measures.

An alternative approach is to decide *a priori* how long it is likely to take for the intervention to produce a clinically meaningful effect. Once that time has been decided an appropriate test of the difference in outcome between the general practitioner groups at that time point should be performed with the confidence intervals reported.

3. The magnitude of the clinical effects for the primary endpoints should be stated along with the confidence limits. Nowhere in White and colleagues' paper are the general practitioner scores reported for the intervention and control groups separately. They test the significance of the differences in the scores achieved by the general practitioner groups but do not present the size of difference or clinical effect being tested. Thus we have no idea of the clinical importance of the effects being tested.

This is all the more surprising since they state that 'one has to interpret the importance of results from a clinical point of view'. We could not agree more.

4. The intended size of the trial and the power calculations should be specified in advance. When a study is being planned the researchers need to decide what is the smallest size of clinical effect they consider to be worth detecting and at what level of statistical significance. The appropriate size of the study (in this case the number of general practitioners) is determined by the decision as to the power of the study to detect such an effect at that level of significance if indeed the intervention does produce that effect.

Results must be evaluated in the context of prior knowledge, corroborative studies, dose–response relationships and their reproducibility.[4] However, taking the above into account the finding of a statistically significant result is, as far as we are aware, the only basis for rejecting a study's null hypothesis.

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References

1. Pocock S, Hughes M, Lee R, *et al.* Statistical problems in the reporting of clinical trials. *N Engl J Med* 1987; 317: 426-432.
2. Rose G. Beta blockers in the treatment of myocardial infarction. *Br Med J* 1980; 2: 259.
3. Doll R, Peto R. When is significant not significant? *Br Med J* 1977; 2: 259.
4. Gore S, Altman D. *Statistics in practice.* London: British Medical Association, 1982: 71.

Workload of full-time women GPs

Sir,

The paper by Judith Cooper and colleagues on workload and remuneration for part-time women in general practice (October *Journal,* p.400) has failed to emphasize that the most important factor in deciding the profit sharing ratio for part-time partners is the amount of out of hours and weekend work that they do.

Although the study shows how much on-call work is done by the two groups as a whole, this is not related individually to their daytime commitment. As a wife, mother and full-time general practitioner, I am in no doubt at all that the nights and weekends on call are by far the most stressful, tiring and intrusive part of a general practitioner's workload. It is also the most dangerous. The review body's figure of 13.5% of gross remuneration for out of hours work bears no resemblance at all to a realistic payment for the degree of disruption, wear and tear and fatigue that a full part in the average general practitioner's rota brings. It is not only the hours of night work either, but a busy night on call makes the following day's work far harder to cope with.

The amount of money earned for the number of nights on call should not be in a linear relationship because the more nights done the harder the load is to bear. The reward should accordingly be increased or decreased exponentially.

Therefore, full-time general practitioners, working at nights and weekends, may feel that their part-time partners, who do not share this load fully, should share significantly less of the profits regardless of their daytime working hours.

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Trainee collaborative research in the Essex faculty

Sir,

The paper by Timmins and colleagues (October *Journal,* p.423) demonstrates that a trainee collaborative study provides a healthy symbiosis: trainees participate in audit and encounter the College at a local level while the College benefits from a novel research tool. Clearly, as the authors point out, other faculties might consider involving local trainees in similar studies, to mutual benefit.

Given this pioneering approach, it seems ironic that the decision of the Joint Committee on Postgraduate Training for General Practice, in February 1988, to withdraw its recognition for training from the North East Thames region would have affected the trainees cooperating with the Essex faculty during the period of this study. Presumably, the training environment was so poor as to warrant withdrawal of recognition, rendering trainees ineligible for the College examination, yet adequate enough to stimulate the 'high standard' of work described by the authors which provided the basis of a paper deemed suitable for publication in the College *Journal.* It will be interesting to see how rapidly and capably trainees in less blighted regions are encouraged to