level of fetal heart rate when this is stable, accelerations and decelerations being absent, whilst decelerations were then defined as departures from the baseline.

Drs Persad and Settatree raised points of technique regarding accelerations and how false positives should be handled. We are intensely interested and would like to have a continuing debate on these issues. However, these were not the main subjects of our paper and the constraints of the letters column do not allow adequate discussion here.

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Perineoplasty compared with vestibuloplasty for severe vestibulitis

Sir,

I read with great interest the article by Bornstein *et al.* (Vol 102, August 1995)[1] which describes the poor outcome of vestibuloplasty compared with perineoplasty in the treatment of women with severe vulvar vestibulitis. These women are extremely difficult to manage, and prospective randomised studies such as this one are urgently needed to help rationalise treatment.

The role of surgery in women with vulvar vestibulitis remains to be defined, as does treatment with interferon. Other reviews of perineoplasty in women with severe vulvar vestibulitis show lower success rates than Bornstein's series. In the series by Mann[2], of 71 women who underwent perineoplasty, 29% ($n = 16$) had 100% improvement in symptoms, and a further 37% ($n = 21$) were much improved. This success rate is only modest. In Bornstein's series follow up is only reported at six months, and the long term relapse rate is not stated.

Vulvar vestibulitis has a multifactorial origin, and women with all grades of vulvar vestibulitis form a heterogeneous group. In some instances a specific cause can be identified and avoided with improvement, in even severe symptoms, e.g. woman with allergic contact dermatitis[3]. In other women no cause can be found. It is not clear from this study whether the women had a full microbiological, dermatological and psychological assessment prior to surgery. Surgery should remain a last resort, as it is not often acceptable to women, who may respond to topical treatments such as steroid creams. Conservative methods should be considered as the first line of treatment. If surgery is carried out, pre-operative psychological assessment and post-operative sex therapy has been shown to improve results[4]. It is well known that the onset of symptoms in women with vulvar vestibulitis can coincide with psychological upset, such as abortion and marital breakdown, and symptoms may possibly arise as a result of vaginismus and altered pain perception, resulting in chronic, severe introital irritation[5]. Surgical treatment of women with vulvar vestibulitis should therefore be complemented by intensive counselling and general measures to improve hygiene, and should address the psychological and psychosexual needs of the woman. It is not clear from this paper whether these issues were addressed.

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Authors' Reply

Sir,

I thank Dr Nunns for his interest in our article. There are two issues regarding vulvar vestibulitis that are very hard to quantify and probably cause confusion, as was expressed in his letter.

The first issue is the definition of the disease and selection of women for surgery. As stated in our article, the diagnosis of vestibulitis was based in all cases on Friedrich's criteria[1]. Furthermore, women were subjected to surgery only if their disease was graded as "severe" (i.e. pain was very strong and prevented intercourse). These criteria do not include women with vaginismus, allergic dermatitis, or those with symptoms resulting from a psychological upset, such as an abortion.

All women in our series had failed previous conservative treatment with anti-fungal preparations, steroidal and oestrogenic ointments, interferon, antibiotics and anti-depressant medications. They underwent a colposcopic, cytologic and microbiological evaluation. Only those with "pure" severe vestibulitis underwent surgery. Both study groups underwent the same pre- and post-operative evaluation and care; therefore, the suggestion that "these issues ... may account for the apparent failure rate (of vestivuloplasty)", is untenable.

The second issue is the variable complete response rates of surgery quoted by the different authors, of 29%[2] to 82%[3]. These authors use different surgical techniques. Apparently a modified perineoplasty as carried out by Marinoff and Turner[3] and used in our study leads to the best response rates, as it removes the anterior vestibule in those cases in which it is affected by the disorder.

Post-operative sex therapy and counselling is reserved in our centre only for the few cases who fail perineoplasty. So far, it has been unnecessary to add these measures to our routine treatment protocol.

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Complications of pregnancy after infertility treatment: awareness and prevention

Sir,

There is an important omission on the causes of miscarriage after assisted conception in the recent review on complications of pregnancy after infertility treatment by Professor Hull (Vol 102, July 1995)[1]. Antiphospholipid antibodies (APA), the two most clinically important of which are the lupus anticoagulant (LA) and the anticardiolipin antibodies (ACA), are a family of autoantibodies which are recognised to be associated with recurrent miscarriage[2]. There is now increasing evidence that these

antibodies play an important role in the pathogenesis of infertility and of pregnancy loss after assisted conception.

APA have been reported to be present significantly more often in women with unexplained infertility than normal controls[3]. Birkeneld *et al.*[4] have reported that women who fail to conceive after IVF–ET (which represents a failure of implantation) are significantly more likely to have APA than those who conceived and had either delivered or had ongoing pregnancies (32% *versus* 0%). The pathogenesis of fetal loss in women with APA has traditionally been ascribed to thrombosis of the utero–placental vasculature. This is not the full story. Evidence suggests that APA also impair trophoblast function via mechanisms unrelated to thrombosis and have a direct effect on the placenta. The binding of APA to surface phospholipids on the trophoblast, resulting in direct cellular injury and inhibition of syncytial formation has been demonstrated[5,6].

Another point of interest is that serum levels of APA are increased after IVF[4,7]. This may be due to the massively increased levels of oestradiol that are seen in women undergoing ovulation induction. Oestrogens are known to influence the development and course of autoimmune disease, perhaps by augmenting $CD5^+$ B cell function[8]. Indeed, Ben-Chetrit and Ben-Chetrit[9] have recently reported a series of three women who developed systemic lupus erythematosus following ovulation induction.

Recognition of the association between infertility, defective implantation and APA opens the way to therapeutic interventions to improve the poor live birth rate following IVF–ET. Sher *et al.*[10] reported that the pregnancy rate following IVF–ET was significantly higher amongst APA-positive women if they were treated with a combination of low dose aspirin and heparin (49%) than if they were untreated (16%). Further study of APA and the elucidation of their mechanism of action in causing pregnancy failure is needed to improve the successful pregnancy rates following both IVF–ET and natural conception.

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The definition of pre-eclampsia

Sir,

Higgins and Byrne (Vol 102, July 1995)[1] suggest that the definition of pre-eclampsia should include an assessment of blood pressure while on the obstetric day care ward. Many definitions of pre-eclampsia have been proposed, but only one has been derived from a single dataset and validated on a second, comparing the group defined as pre-eclamptic with the normal population, in terms of outcome. Any definition now proposed must improve on what has been formulated by Redman and Jeffries[2].

The assertion by the authors is that any new classification of hypertension in pregnancy should include an assessment of mean blood pressure from the obstetric day care ward. However, before a definition is revised it is necessary to ensure that the proposed revision is valid.

The data presented by Higgins and Byrne confirm the effect of "white coat" hypertension in the clinic, which apparently diminishes while on the obstetric day care ward. It is not clear from these figures how a change from the current International Society for the Study of Hypertension in Pregnancy (ISSHP) definition (i.e., two diastolic blood pressure readings equal to or greater than 90 mmHg 4 hours apart, and the proposed revised definition of a mean blood pressure equal to or greater than 90 mmHg over a 4-hour interval at the obstetric day care unit would affect the group selected at risk in their own cohort. A study of outcome in relation to obstetric day care unit blood pressure recordings performed on large numbers is required to set thresholds of abnormality, rather than perpetuating arbitrary levels set in 1955[3].

Using receiver–operator characteristic (ROC) curves relating obstetric day care unit measurements to outcomes such as preterm delivery, birth centile and proteinuria, we have found that a lower threshold of blood pressure may improve sensitivity whilst not compromising specificity. Indeed, it may be dangerous to use conventional cut offs when averaging a number of readings, as women at risk may be missed[4].

While obstetric day care unit measurement is common and intuitively appears to select a high risk population, it has never been subjected to a prospective assessment of outcome versus conventional management strategies. To change the classification of hypertension in pregnancy again, with no evidence to support the change, would be a retrograde step. Before a definition of pre-eclampsia is based on obstetric day care unit parameters, it is essential to relate these measurements to outcome.

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Authors' Reply

Sir,

We welcome the interest shown in our letter by Penny *et al.* We agree that patterns of blood pressure in the day care unit should