To the Editor,

Dear Sir,

Dr Chou and his colleagues should be complimented on their commitment to the development of minimally invasive surgery in gynaecology.1 While their study aim was answered i.e. 'to confirm the feasibility of the shortened postoperative stay' some of their conclusions are not supported by their results and their manuscript raises a number of questions.

1 While 30 patients were recruited for the study over a 7-month period, how many patients were excluded or were ineligible for this study and what were the reasons?

ii Comment is made on the cost effectiveness of the procedure and compared to a group of patients having open abdominal hysterectomy performed. In a dedicated laparoscopic unit where open surgery is presumably restricted to high-risk patients, is it appropriate to use this group as a comparative group?

iii The assumption that reducing length of stay is cost effective is not necessarily correct. As a consequence of early discharge 2 possible scenarios result: there is an increased availability of beds for utilisation so that more surgery can be performed, with a resultant increased cost. The second more likely scenario of reduced post operative stay is that as most surgeons have only 1 or 2 operating sessions per week in the public sector and as these cases take, on average, twice as long as an open hysterectomy, fewer patients are able to be accommodated in the restricted operating time irrespective of their length of stay. The result is an increase in surgical waiting lists.

iv Comment is made on patient satisfaction with the procedure and follow-up, but no account is given in the methodology of how this was performed.

v The authors report no complications in their study group. This would be expected for such a highly selected low risk group of patients. A much larger study is needed before one could confidently assure the public of the safety of an outpatient hysterectomy in an Australian setting. Based upon the data presented, do the authors recommend then this approach be incorporated into general gynaecological practise?

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1 Chou DCY, Rosen DMB, Carlo GM, Carlton MA et al. Home within 24 hours of Laparoscopic Hysterectomy. Aust N Z J Obstet Gynaecol 1999: 39: 234-238

To the Editor,

Dear Sir,

I believe Ross Pagano is mistaken in his recent report in your Journal3 in declaring vulvar vestibulitis syndrome (VVS) is not primarily a psychosexual disorder. He observed a 21% spontaneous resolution or improvement rate in keeping with functional disease. If he believes it is a definite neuronal dysfunction, this remains to be proven.

Clearly this condition exists, but it is only a syndrome. However, I am on record as saying I believe it is a non-disease as also is another cause (ie not result) of introital dyspareunia under yet another guise, the Focal Vulvitis Syndrome as described by Peckham et al.3

Peckham, like Pagano, took a history. The latter called his version careful, the former in-depth and in addition he took a detailed sexual history. Peckham's paper contained a text box listing the sexual questions asked. However none pertain to arousal and orgasm, rather vital questions especially when investigating introital dyspareunia, thus placing the validity of the conclusions of that study in doubt. What resulted was that the American Journal of Obstetrics and Gynecology created a new disease. Dr Pagano may define his word careful for us and in that case, I wonder how many of his cohort of 230 women were capable of sexual arousal and/or orgasm with their partners?

The repeated mention of prolonged, recurrent candidiasis and often only temporary improvement gained through the use of prolonged topical and or systemic fungicides makes me strongly suspicious...
that the women were not fully investigated about aspects of personal hygiene.\textsuperscript{5} I am certain many in this series were in fact reinfecting themselves. Candidiasis often proves the trigger which will generate secondary vaginismus resulting in even more introital dyspareunia and local abrasions. This is why it is not so surprising that surgical management in this series was not so successful. I have seen numerous patients where these lesions have been excised, one patient presenting to me after 4 separate such operations, but who then also had secondary vaginismus.

Whereas I am convinced that were we to colposcope every woman after sexual intercourse we would find focal areas of erythema and abrasions (lesions), we do not get to see them. They do not complain. I venture to say that even where a woman has had very vigorous, very pleasurable, even transiently painful, prolonged intercourse she too will have macroscopic lesions.

If a woman is penetrated when incompletely aroused, the physiological self-widening and lubricating mechanisms of the vagina and vulva are not activated. Such forceful entry of an unwelcoming, dry, narrow orifice will cause mini-abrasions around the introitus. They will take some 36-48 hours to heal over. In the interim, every time urine is passed, her nerve endings will smart since the urine is both acidic and at deep body temperature. It has been well described as the same sensation of sweat falling into one's eyes during vigorous physical activity or in high external temperature. From my perspective, teaching the preorgasmic preparations or components in my conventional armamentarium. The risk of subsequent infertility alone is somewhere of the order of 30% following an open metroplasty.\textsuperscript{3}

I would also take issue with the authors in describing a bicornuate uterus as a 'minor uterine abnormality'. The surgical repair of a truly bicornuate uterus is a very major operation and by no means the sort of operation that the average specialist gynaecologist can have in his/her armamentarium. The risk of subsequent infertility alone is somewhere of the order of 30% following an open metroplasty.\textsuperscript{3}

I trust that none of our Junior staff reading this article will believe that a bicornuate uterus is either common or a minor abnormality.

\textbf{Professor M J Bennett}
\textbf{Head of School}
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\textsuperscript{1} Soh EBS, Lim JMH. An unusual case of antepartum haemorrhage \textit{Aust N Z J Obstet Gynaecol}. 1999; 39: 389-390.
\textsuperscript{2} Buttram VC, Gibbons WE. Müllerian anomalies: a proposed classification (An analysis of 144 cases). \textit{Fertil and Steril} 1979; 32: 40-46.
\textsuperscript{3} Bennett MJ. Congenital abnormalities of the fundus. In: Bennett MJ and Edmunds DK (editors.) \textit{Spontaneous Abortion}. Oxford: Blackwell Scientific, 1987.

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To the Editor,

Dear Sir,

I read the case report by Drs Soh and Lim\textsuperscript{1} with some interest. Certainly the case itself was a very rare clinical situation but I was disconcerted to read some of their comments under their heading ‘Discussion’. The statement made by them that ‘the most common Müllerian defect is bicornuate uterus’ is one that is unsubstantiated in the literature. Buttram and Gibbons\textsuperscript{2} in a seminal paper published in 1979 indicated how unusual a truly bicornuate uterus was. They revealed that of 39 patients diagnosed as having a bicornuate uterus on hysterography, 38 had no laparoscopic evidence of an external fundal abnormality and had their diagnosis amended to that of separate uterus. In my personal practice of large numbers of women with recurrent miscarriages I find congenital abnormalities of the uterus in approximately 14% and have not come across a bicornuate uterus in over 6 years.

I would also take issue with the authors in describing a bicornuate uterus as a ‘minor uterine abnormality’. The surgical repair of a truly bicornuate uterus is a very major operation and by no means the sort of operation that the average specialist gynaecologist has in his/her armamentarium. The risk of subsequent infertility alone is somewhere of the order of 30% following an open metroplasty.\textsuperscript{3}

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1. Bennett MJ and Edmunds DK (editors.) \textit{Spontaneous Abortion}. Oxford: Blackwell Scientific, 1987.
To the Editor,

Dear Sir,

Re: The integrity of surgical gloves during gynaecological operations.

I would like to thank authors Khoo and Isbester for their recent article.¹

May I offer the practical addition that surgeons consider double gloving (mandatory in my view – both to protect surgeon and patient) with their usual glove size as the ‘outer’ glove and the next 1/2 size up as the ‘inner’ glove.

In my experience this minimises discomfort associated with pressure and I can assure current single glove surgeons that in time they will feel less than protected when they do not double glove.

With respect to the oft-claimed lack of sensation and dexterity, this is a feeling that diminishes with the passage of time when the surgeon double gloves.

Surgeons are well advised to minimise/eliminate use of powdered gloves as latex allergy is a very real risk to all exposed chronically to (especially) powdered latex gloves.

Suggested additional reading may include the RACS policy document ‘Infection control in surgery’ July 1998 and the excellent article by Gani et al Aust N Z J Surg 1996; 60: 171-175.

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¹ Khoo SK, Isbester A. The integrity of surgical gloves during gynaecological operations. Aust N Z J Obstet Gynaecol 1999; 39: 357-359.