


We are grateful to Trehan and Trehan for their comments. We did consider suspending the ovaries following laparoscopic surgery for longer than 36 h. However, we had experienced a case of acute small bowel obstruction following a similar suspension procedure which required immediate release of the suspension suture (data on file). This is a very distressing and potentially serious complication and we did not feel that it would have been safe to discharge women home with the ovarian suspension suture in situ. We have decided to adopt a 36 h interval based on a methodologically robust study which used an animal model to show that susceptibility for adhesion formation was significantly reduced or eliminated after the first 36 h following a peritoneal injury. This was a relatively recent study, whilst all the other animal studies quoted by Trehan and Trehan were published almost four decades ago.

We were interested to learn that Trehan and Trehan have introduced post-operative ovarian suspension for 5–7 days into their routine clinical practice. We could not find any publications authored by them or by anyone else describing the efficacy of this approach. In view of that we would urge our colleagues to consider publishing their data in order to help others to learn from their unique experience.

A second look laparoscopy for the outcome evaluation of the trial has its benefits; however, even in their own publication, they did not report on outcomes because a repeat laparoscopy could not be justified for ethical reasons (Trehan, 2002).

Reference

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Uterine artery embolization for severe symptomatic fibroids: effects on fertility and symptoms

Sir,

We read with interest the paper by Torre et al. (2014) but would like to raise a number of concerns. They conclude that uterine artery embolization (UAE) should not be offered to women of reproductive age as it could be detrimental to their fertility prospects. We strongly contend that the only legitimate conclusion they could reach from the data obtained from their study cohort is that UAE does not improve fertility prospects in women whose prospects were, at best, dismal, while the treatment significantly improved their symptoms.

Torre et al. studied a relatively small (for purposes of fertility outcomes) cohort of women with extensive fibroid disease, the majority of whom had had previous surgical interventions, and for whom further, or de novo, surgical intervention was either refused or advised against. These women were symptomatic and for that alone they needed some form of intervention. Implicit in the way the paper is

Reply: Ovarian suspension for longer than 36 h is necessary for temporary ovarian suspension to fulfil its remit

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written is that these women wished to preserve their uterus, even when, as in 50% of the study population, they had given up on their fertility aspirations. The authors’ suggestion would deny these women a treatment modality that could, by improving their symptoms, significantly improve their quality of life.

The authors acknowledge that a significant limitation of their study is the lack of a control group, or comparison to another uterus-sparing procedure, and then go on to arbitrarily divide the cohort into two—women wishing to conceive (50%) and the other 50% who had given up on fertility. This approach alone distorts the denominator, and therefore, the interpretation of the data. In other words the definitive study population should have been only those women wishing to conceive.

Notwithstanding the criticism the authors level at the paper by Frederick et al. the data they present on their own study population in fact support the concept that fertility prospects following a second intervention, such as repeat myomectomy, are very poor. Therefore it is highly likely that the prospects for this cohort would have been no better had it been possible to carry out a surgical intervention.

In Table IV of their paper, Torre et al. compare the pregnancy outcomes of their study to those in previous publications. This is wholly inappropriate, uninformative and misleading. A more meaningful comparison requires a formal systematic review of the literature with pre-defined criteria.

Finally, rightly or wrongly, many busy clinicians keep abreast with developments in their fields by reading only abstracts and/or conclusions from published studies. This places a major obligation on researchers to ensure that the message put across in abstracts and conclusions is as accurate as possible. In this regard, Torre et al.’s conclusions would mislead many a reader. Recently published guidelines by the UK’s Royal College of Obstetrics and Gynaecology/The Royal College of Radiologists acknowledge the absence of robust evidence on the relative merits of UAE versus myomectomy, and encourage the participation in an on-going large prospective multi-centre trial (FEMME) comparing the two uterus-sparing treatments. The guidelines implicitly accept that UAE can be offered to women of reproductive age, and emphasize the paucity of data and poor quality of the studies of the impact on fertility of both UAE and myomectomy. Torre et al.’s study is a classic example of the need for a well-designed RCT to assess the effects of UAE. We agree that women should be carefully counseled prior to undergoing UAE, but that should also be the case when their planned treatment is myomectomy.

Conflicts of interest

F.S., A.-M.B. and I.T.M. are investigators in the FEMME Trial, a prospective randomized multi-centre trial comparing UAE to myomectomy. None of the authors has any financial conflict of interest.

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A Randomised trial of treating Fibroids with either Embolisation or Myomectomy to Measure the Effect on quality of life, among women wishing to avoid hysterectomy: the FEMME study, http://www.birmingham.ac.uk/research/activity/mds/trials/bctu/trials/womens/femme/index.aspx (15 February 2014, date last accessed)