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objective and subjective outcome criteria for pelvic support, incontinence, and sexual function, is a plus. Not surprisingly, the anatomic outcomes at a mean of 2 years are excellent and symptomatic improvements have followed suit.

While randomized comparisons confirming solid anatomic and subjective outcomes are welcome with open arms, analyses such as this one inevitably raise several questions going forward. First, can the results of this study be applied to a typical, tertiary referral population with recurrent prolapse, especially since the current cohort is a highly selective one (ie, thin, healthy, and having little previous pelvic surgery)? Second, does uterine preservation in 30% of the current cohort represent an unaccounted confounding factor in this study where the authors have done a terrific job of recruiting a relatively “pure” population? Third, do surgical nuances, which admittedly vary from study to study, influence long-term outcomes? In particular, does anchoring mesh to the sacral periosteum rather than the anterior longitudinal ligament increase the possibility of osteomyelitis? Likewise, does performing an extended dissection between bladder and vagina to the level of the bladder neck increase the chance for unrecognized, or recognized, organ injuries in the hands of a less-experienced surgeon? Finally, and this is more an observation rather than a question, the handling of overt preoperative stress urinary incontinence (SUI) is somewhat puzzling to me. As it appears, none of the women in this study was offered a concomitant anti-incontinence procedure, which has the potential benefit of addressing preoperative SUI in the same setting without significant attendant increase in morbidity. Fortunately, in this population, the rate of SUI resolution after the prolapse procedure alone was high and the appearance of new-onset, overt SUI was quite low.

With emerging outcomes such as those in the current study, several conclusions can be drawn. In experienced hands, robotic- and laparoscopic-assisted sacral colpopexies are associated with similarly solid anatomic outcomes. Robotic procedures are associated with greater costs and may take longer to perform (at least initially), while laparoscopic procedures require specialized expertise. Robotic procedures are also useful because surgeons trained in the open approach can often transition to robotic surgery directly without undergoing specialized laparoscopic training. Finally, as the outcomes are relatively immature, durability should be monitored closely.

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<https://doi.org/10.1016/j.urology.2019.07.046>
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EDITORIAL COMMENT



There is a lot to like about this well-designed, prospective RCT comparing robot-assisted and laparoscopic sacral colpopexy by a high-volume, experienced surgeon. Compared to 2 previous similar RCTs,^{1,2} the current paper offers a longer mean follow-up period and a higher number of recruited patients. The use of standardized questionnaires, as well as the utilization of both

AUTHOR REPLY

We would like to thank you for your positive critique and the opportunity to further discuss the implications of our results.



As you have pointed out the main conclusion drawn from our data is that, for the time being, the choice between laparoscopic and robot-assisted surgery can only be based on cost, time, and training requirements, since there is consistently no difference in efficacy and safety in available trials. When longer follow-up becomes available, durability of results could be compared and further guide our choice.

Randomized trials are essential in giving high quality information but also have inherent flaws. The need to have 2 balanced and comparable arms at randomization imposes certain exclusion criteria such as obesity. Very severe obesity is not common in our referral area and the inclusion of such patients randomly in one of the arms would have made our groups not balanced. As far as excluding patients with comorbidities is concerned, it did not significantly change the characteristics of our sample: in our practice we advise patients with comorbidities, especially respiratory ones, to undergo vaginal and not abdominal surgery. The results of this study can be applied to a typical, tertiary population with recurrent prolapse. Without a doubt, the factor that makes the difference is the surgical expertise that is expected, but not always guaranteed, in tertiary centers as you have pointed out.

As far as dissection of tissues before mesh placement is concerned, it goes without saying that the wider the dissection the more the adjacent organ injuries, especially in inexperienced surgeon's hands. This issue probably calls for an improvement in the quality and supervision of young surgeons' training more than for a modification of the surgical technique: we have associated this particular step in prolapse repairs with a significant improvement in outcomes such as degree and duration of anatomical correction and resolution of preexisting stress incontinence,¹ and we feel that a slightly increased risk of minor intraoperative complications is justified. The issue of adding or not a concomitant anti-incontinence procedure is still not resolved in the literature, but the widely accepted idea to adopt a 2-step approach is further supported by the resolution of stress incontinence in our series.

Uterus preservation during prolapse surgery has been a significant trend in recent years as an increasing number of women, especially the younger ones, consider the uterus an integral part of their self/body image and a testament to their femininity. This is particularly relevant in our referral area and is supported by

evidence of very good prolapse-repair results, both anatomic and functional, with uterous preservation.²⁻⁴ Consequently a high percentage of women with the uterus in situ in our population is not so much the result of selection.

Anchoring of the sacral end of the mesh on the sacral is a very commonly adopted approach⁵ and has never been a relative safety issue.

We believe that long-term follow-up will confirm our initial ideas and observations.

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