Peer Review File

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Review Comments

Reviewer A

Comment 1: I applaud the authors for attempting to describe an intriguing theory and potential treatment for pelvic organ prolapse. However, I think the video and manuscript are not of sufficient quality to warrant publication in their present form. The video is poor quality and would benefit from repeated iterations to improve the flow and clarity of the verbal script. There are mistakes when describing cure vs failure in Table 1.

Abbreviations (POP and OAB) are used without orienting the viewer first to what they mean, and there are also phonetic errors in using the abbreviations.
I recommend providing more introduction and rationale for the commentary, and also spending time to improve and "polish" the video from a production perspective (remove the yellow pointer, practice the script before recording, etc).
Reply: We agree it is not a film quality video. However, the main points, poor surgical results in post-menopausal women both in the Prospect and Shkarupa trial are well made.
Regarding the abbreviations (POP and OAB), our audience are urologists, gynecologists, urogynecologist who surely are familiar with these descriptions.

Comment 2: The weaknesses of the manuscript also outweigh the strengths. Again, the topic is interesting and potentially relevant to many urologists and urogynecologists around the world, but the manuscript is poorly written
There is not sufficient rationale or background for how or why the authors chose the particular papers they discussed in the commentary.
Reply: It is disappointing to see the reviewer on the one hand applauding the content, then proceeding step by step to demolish the paper on matters of style. It begs the question, " Is science about facts, or style?"
The importance facts of this paper is that the Shkarupa et al. prove convincingly that it is futile performing native tissue surgery on post-menopausal women, whatever the method employed. As such, they provide an important validation for the Lancet PROSPECT trial. Not many surgeons know this. This point is made in different ways several times in the paper.

Comment 3: Typically, one would describe in a commentary or review paper how the articles were chosen.
There is no description of strengths or weaknesses of the studies, nor comment on future research or directions. Also, the section of commentary from the guest editor is

repetitive. Much of what is written in this section is just repeating the sections from above and does not add or expand ideas that are already stated.
Reply: Agree all this is appropriate for a standard review.
All the 19 papers of this special ATM issue, follow a special template, less than 1600 words, an emphasis on the core message, as can be seen by use of "Highlights".

Comment 4: I was also very confused when the authors were describing "vaginal excision" surgeries.

I'm not sure if they were referring to obliterative procedures or reconstructive, and I'm also not sure which compartments these repairs are referring to.

Reply: Reconstructive. Vaginal repairs which excise tracts of the vagina. The consequent scarring can create a condition called "The tethered vagina syndrome". The scar at bladder neck "tethers" the more powerful posterior pelvic muscle forces to the anterior forces and forcibly pull open the posterior wall of the urethra, resulting in massive loss of urine.

Comment 5: Lastly, there needs to be more comment on the idea of collagen breakdown after menopause. The authors need to cite other papers (perhaps basic science) papers that prove this is a valid theory, and there also needs to be more discussion of mechanism of action and rationale for the "collagen-creating TFS minisling."
Reply: This is a valid and very important comment. There are 19 papers in this ATM issue and collagen was dealt with in detail in another paper.

Comment 6: And there also needs to be more discussion of mechanism of action and rationale for the "collagen-creating TFS minisling."
Reply: Again a valid comment. Again this is dealt with elsewhere. The basic science is detailed in https://obgyn.onlinelibrary.wiley.com/toc/16000412/1990/69/S153
See
The Autogenic Ligament Procedure: A Technique For Planned Formation Of An Artificial Neo-Ligament
Peter E. Papa Petros, Ulf I. Ulmsten, John Papadimitriou

Comment 7: Both the video and manuscript are very poorly done. Regarding the video, it is overall poorly produced and seems like an informal talk rather than a formal video to be published. There is a distracting yellow pointer which is not great quality.

The script has many mistakes, including when describing failure vs success in Table 1. Abbreviations are not defined at the beginning of the video, and there are mistakes when using the abbreviations that could easily have been edited out. Overall, the video is poorly done, not rehearsed or polished, not worthy of publication in a formal journal.

Reply: The video was done on commercial video software "Screen Pal". I have reviewed the video and I agree it could be improved. The video is clear, however, and the message, is clear: native surgery on post-menopausal women is basically futile.

Comment 8: I have similar comments regarding the manuscript. The manuscript is poorly written. It lacks flow or appropriate background/rationale. The authors pick 2 papers to describe in detail (one trial is written by a co-author of the current manuscript) but there are no inclusion or exclusion criteria, or description of how these papers were chosen.

Reply: The authors were constrained by the template which all 19 papers in the ATM issue followed. All these details requested, background/rationale, inclusion or exclusion criteria are available at
https://www.ncbi.nlm.nih.gov/pmc/articles/PMC8552928/
I regard the Shkarupa paper as one of the most important papers on pelvic floor surgery ever written, as it proves convincingly, at least to me, that it is futile performing native tissue surgery on post-menopausal women, whatever the method employed. As such, they provide an important validation for the Lancet PROSPECT trial.

Comment 9: It does not seem that a formal literature review was undertaken.

Reply: Agree. See original Shkarupa paper. The authors were constrained by the template

Comment 10: There is no basic science rationale for the theory about collagen "wasting away" from the postmenopausal uterosacral ligaments, nor any rational for using the "collagen-creating TFS mini-sling."

Reply: The whole Integral Theory paradigm can be distilled into one word, "collagen",and collagen is dealt with in other papers in the issue. If you have time, please read some or all of the 1990 Theory
https://www.ncbi.nlm.nih.gov/pmc/articles/PMC8552928/.

Comment 11: There is no comment about strengths, limitations or implications for these papers, nor any discussion of future research or directions.

Reply: Agree, but it was not an original paper. All of this was implied in Table1, and brought together in the video and the conclusions to the paper and dealt with in the original Shkarupa paper.

Conclusions (repeated) from the ATM-23-1781(ATM-2023-ITP-18) paper

The key message from the Shakarupa *et al*. native cardinal/uterosacral (CL/USL) ligament study was that in premenopausal women, ligament repair alone provides good cure rates for pelvic organ prolapse (POP), and urgency at 12 months. However,

catastrophically low cure rates were noted for both conditions in post- menopausal women. They advised that post-menopausal women required collagen-creating slings. These comments were validated by Inoue *et al.* (11). Using the collagen-creating TFS minisling, high 5-year surgical cure rates for POP and urge were reported in 70-year-old Japanese women. The final question by Shakarupa *et al*. was "What happens to the successful POP and urge group results after the menopause?"

Reviewer B

Comment: POP and repair surgery are themes that are challenging and more relevant than ever nowadays. The paper is sound and I would suggest a minor revision of the english language throughout the manuscript (eg. native tissue repair remained to native tissue repair still remains").
Another thing that came to my attention was the ban of mesh sheets by regulatory bodies in some western countries, is there a particular reason for this? If possible, please include this in the text.

Reply: Dear Reviewer B
We really appreciated the time and effort you have spent in reviewing our review. Thank you for the suggestions. We will deal with them.

Reviewer C

Comment: The authors presented the detailed results of recently published researches, namely: Shakarupa et al and Inoue et al;
The manuscript is not an original resource and nor systematic review, I don't see the reason to publish this rewritten work, when we can reach the original resources.

Reply: Dear Reviewer C
We really appreciated the time and effort you have spent in reviewing our review. The importance of this paper is that the Shkarupa paper proves convincingly that it is futile performing native tissue surgery on post-menopausal women, whatever the method employed. As such, they provide an important validation for the Lancet PROSPECT trial. Not many surgeons know this. This point is made in different ways several times in the paper.