

Peer Review File

Article information: <https://dx.doi.org/10.21037/tau-23-534>

Reviewer A

After reading the introduction, I concluded this text is somewhat unlogically and unfluently written.

Therefore I used a free webbased Artificial Intelligence detector to check for the use of artificial intelligence in the introduction section. <https://sapling.ai/ai-content-detector>

Since the number of words is limited in this free application, I first checked the first two paragraphs of the Introduction section. The AI detector classifies this as 99.9% fake. Then I checked the last paragraph of the Introduction section. The AI detector classifies this as 100% fake.

Since the use of AI, according to the TAU Author Guidelines, should have been mentioned in the cover letter, Materials and Methods and Acknowledgements, but is currently not mentioned anywhere in the manuscript, I recommend to reject the manuscript.

Reply: Thank you for taking the time to review my manuscript. But it's hard to agree with the reviewer. In particular, I don't know much about the AI detector that the reviewer uses. I wrote the manuscript based on references and facts, and as you can see if you check for plagiarism, this is a paper I wrote myself. As for the last paragraph of the introduction, I think that parts presented at other academic conference could be detected in that way, but as I mentioned earlier, I am not familiar with the AI detector used by the reviewer, so it is difficult to give a clear answer. It may be inconvenient, but please review my manuscript closely again. thank you

Reviewer B

1) Strange terminology such as "gas out day" (to refer to return of bowel function),

Reply: We appreciate the reviewer's comments and agree with your opinion. We have revised as followed "passing gas".

Changes in text: Table2, Table4, Abstract: on line 9 in page 2 & on line 1 in page 3, Methods section: on line 21 in page 6, Results section: on line 11 in page 10, Discussion section: on line 13 in page 1.

2) None of the reported results in the body of the manuscript include units of measure.

Reply: Thank you for your comment and we apologized for our carelessness. We have added units of measure in Results section.

3) The authors should make sure they write out all abbreviations in full before resorting to their use.

Reply: Thank you for your comment. We have added abbreviations in Table 1 & 3.

4) Authors describe a handful of Clavien 3-4 complications for both SP and MP groups; they should include what those complications were. Any bowel related?

Reply: Thanks for the good point. Most complications were grade 3a due to urine leakage requiring endoscopic intervention. One patient with rectal injury had a grade 4 complication, and the patient was excluded when matching as he was an advanced stage patient who underwent mpRARP. We hope for the reviewer's generous understanding.

5) Why are authors using a PCA after minimally invasive surgery?

Reply: Thank you for your comment. In the case of cancer patients under our insurance system, the use of PCA after surgery is very inexpensive and is routinely used to control the patients' pain.

6) LN dissection *can* be performed during extraperitoneal prostatectomy and should not be considered a limitation of the SP platform or surgical approach. This is contrary to what is written in discussion.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have added follow sentence in discussion section; "However, this is a limitation due to the extraperitoneal approach and is not a problem with the SP platform itself" (on line 16 in page 12).

7) Need to have longer term functional and oncological outcomes than 3 months.

Reply: We appreciate the reviewer's comments and agree with your opinion. Our study aims to examine the feasibility of spRARP by comparing the initial results of spRARP, which has not yet been widely implemented, with mpRARP, for which sufficient experience has been accumulated. Based on these results, we plan to conduct a study to compare long-term f/u results. We hope for the reviewer's generous understanding.

Reviewer C

The article is not well designed to add new information or evidence on this topic. Comparing an extraperitoneal approach with a transperitoneal approach is a great bias. With this premise it is not possible to draw clear conclusions on post-operative pain.

Reply: We appreciate the reviewer's comments and agree with your opinion. Our study aims to examine the feasibility of spRARP by comparing the initial results of spRARP, which has not yet been widely implemented, with mpRARP, for which sufficient experience has been accumulated. Based on these results, we plan to conduct a study to compare long-term f/u results. We hope for the reviewer's

generous understanding.

Reviewer D

The authors want to evaluate the feasibility of spRARP at a single center, evaluating standard intra and post operative outcomes as well as early continence recovery. The paper is well written despite some mis-spelling. The statistical analysis is correctly run and the PSM is appreciated in this cohort since the low number of patients included in the study.

Here some of my concerns:

- First, I would like to know if the experienced surgeon accomplished the learning curve with the spRARP procedures since this can influence the results. This should be declared in the manuscript.

Reply: Thanks for the good point. We analyzed the surgical results of one senior surgeon (S.H Cheon). This experienced surgeon had sufficient mpRARP experience before performing spRARP and did not show a learning curve, so we performed a comparison between surgical methods through propensity score matching.

- in the "surgical procedure" paragraph the authors did not list the Santorini plexus ligation as step of surgery. Do they normally spare this anatomical landmark?

Reply: Yes, it is. Dr S.H.Cheon does not ligate the Dorsal Venous Complex (DVC)/Santorini's plexus during prostatectomy. In the case of laparotomy, ligation is necessary to ensure visibility, but in the case of robot, even without ligation, there is not much bleeding due to intra-abdominal pressure, and visibility is good, so it does not seem necessary.

- In the discussion section the authors cite the less Trendelenburg inclination as a factor improving anesthesiological complications. Can they provide a reference of the literature to this sentence?

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have added reference "20. Kilic OF, Borgers A, Kohne W, Musch M, Kropfl D, Groeben H. Effects of steep Trendelenburg position for robotic-assisted prostatectomies on intra- and extrathoracic airways in patients with or without chronic obstructive pulmonary disease. Br J Anaesth. 2015;114(1):70-6."

- How can the authors explain the higher NRS scale in spRARP?

Reply: Thank you for your comment. Several factors and variables may contribute to this observation. Firstly, discerning the difference between the two groups can be challenging because mpRARP is known for its relatively low pain levels when compared with traditional open or laparoscopic surgery. No differences were observed immediately after surgery or on POD day 1. Notably, the presence or absence of gas inflation and the use of pressure maintenance system (AirSeal®) can be a significant factor. This instrument is currently used not only for robotic

surgery but also for laparoscopy and various surgeries. However, during the initial extraperitoneal spRARP operation at our hospital, it was difficult to maintain abdominal pressure due to the absence of this equipment. Additionally, subcutaneous air leakage occurred, and some patients complained of pain. However, the introduction of AirSeal® resulted in significant reduction in subcutaneous emphysema. We have mentioned above information in Discussion section (on 21 in page 11).

- The authors conclude that the spRARP "had a short surgical time" but having a look to tab 2 and tab 4 the mean total surgical time and the mean console time did not reach any significance. They should change their conclusion

Reply: Thanks for the good point. However, although there was no statistical difference, the spRARP surgery time was short both before and after matching. We hope for the reviewer's generous understanding.

Reviewer E

In this study, the authors evaluated the feasibility of extra peritoneal SP RARP , and compared the results to the gold standard (MP_RARP).

The study is well written; however, it adds nothing new to the current literature.

I would encourage the authors to consider the following:

1- Add a figure to depict the operative time for the extraperitoneal SP RARP overtime and perform trend analysis. How many extraperitoneal SP RARP cases an experienced surgeon has to perform to achieve low operative time?

Reply: Thanks for the good point. We analyzed the surgical results of one senior surgeon (S.H Cheon). This experienced surgeon had sufficient mpRARP experience before performing spRARP and did not show a learning curve, so we performed a comparison between surgical methods through propensity score matching. We hope for the reviewer's generous understanding.

2-What's the rational to shift to extraperitoneal ? Elaborate in the introduction.

Reply: We appreciate the reviewer's comments. Among the different surgical approaches, the extraperitoneal approach offers several advantages when compared with the transperitoneal approach. It results in less bowel irritation, facilitating early postoperative dietary intake and contributes to quicker patients recovery. Additionally, adopting a less steep Trendelenburg position is associated with fewer respiratory and cardiovascular complications. Since the introduction of extraperitoneal spRARP by Kaouk et al. We have mentioned above information in Introduction section (on line 22 in page 4).

3- How is this pertinent to your population? And how you build on this for the future? for example shifting into all extraperitoneal?

Reply: Thanks for the good point. As answered in comment 2, the extraeritoneal approach has many advantages, and the sp robot flatom is very helpful in

carrying out this. However, it may be difficult to perform if the prostate is large, node dissection is required, or in advanced stages. However, on the contrary, for patients with a history of previous intra-abdominal surgery, the extraperitoneal approach is a necessary option for the patient. Therefore, it is believed that the surgical method needs to be selected depending on the patient.

4-Discuss the results in context of this new metanalysis
<https://doi.org/10.1016/j.pnrl.2023.04.002>

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have added above reference.

Change in text: on line 13 in page 11 (Reference 21).

Reviewer F

The authors report their initial experience in extraperitoneal single-port radical prostatectomy using the Da Vinci SP system, also they compared this approach with the multiport transperitoneal multiport robot-assisted radical prostatectomy. I congratulate the authors for the technical expertise and experience they provide with this manuscript. However, the feasibility of extraperitoneal single-port radical prostatectomy using SP system has already been assessed from various teams. Furthermore, a meta-analysis of studies comparing extraperitoneal and transperitoneal SP RARP has been recently published.

Jiang Y, Liu Y, Qin S, Zhong S, Huang X. Perioperative, function, and positive surgical margin in extraperitoneal versus transperitoneal single port robot-assisted radical prostatectomy: a systematic review and meta-analysis. *World J Surg Oncol.* 2023 Dec 12;21(1):383. doi: 10.1186/s12957-023-03272-7. PMID: 38087327; PMCID: PMC10714462.

Reply: Thanks for the good point. Although there are many reports and good meta-analyses, I thought that comparing surgeries performed on a single surgeon would also have some value, so I conducted an analysis and wrote a paper. We hope for the reviewer's generous understanding.

In general, this manuscript is adequately presented and includes an interesting comparison between two different approaches with different robotic systems. I would like some comments on the points below and revisions in order this paper to be published.

Title: Please avoid using abbreviations in the title. Revise the title according the comment below.

I think that the objective of this study should be the comparison of the two approaches as feasibility has already been described.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have changed title as "Comparative study of extraperitoneal single-port robot-assisted radical prostatectomy and transperitoneal multiport robot-assisted radical prostatectomy using propensity score matching."

Change in text: on line 1 in page 1.

Methods. The dissection is extraperitoneal and not retroperitoneal.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have revised.

Change in text: on line 7 in page 7.

Please add some figures for your single port plus one port placement and intraoperative images for better understanding of your technique and configuration of the "arms" of SP during the procedure.

Reply: Thanks for the good point. Since many cases have already been implemented and reported using the same method, reference have been added.

Change in text: on line 12 in page 7 (Reference 15).

Results: please describe in detail the major complications.

Reply: Thanks for the good point. Most complications were grade 3a due to urine leakage requiring endoscopic intervention. One patient with rectal injury had a grade 4 complication, and the patient was excluded when matching as he was an advanced stage patient who underwent mpRARP. We hope for the reviewer's generous understanding.

I noticed a high rate of positive margin (>40%) despite that more than half of patients had a How could you explain the longtime of hospitalization of 9 and 10 days! RARP has the advantage of short hospitalization of a few days and has also been described as an outpatient procedure.

Reply: Thanks for the good point. In our insurance system, cancer patients have many treatment fee reduction benefits, so the burden of hospitalization costs is low. Therefore, patients may be hospitalized before surgery to perform necessary examinations, which may result in a longer hospital stay. Also, if the patient wishes, we check urination and general condition after foley removal and then discharge the patient. Therefore, compared to other studies, the hospitalization period was found to be longer, but looking at the date of foley removal, you can see that there is no significant difference.

Discussion: Extraperitoneal access through subumbilical incision and not supraumbilical.

Reply: We apologize for our carelessness. It has been revised (on line 10 in page 11).

Please revise the parts discussing about feasibility and add more literature about extraperitoneal SP RARP.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have added followed reference. "21. Tuan TN, Ryan WD, Huy GV, Khoa Q, Hanh TTN, Anh TM, et al. Single-port and multiport robot-assisted radical

prostatectomy: A meta-analysis. Prostate Int. 2023;11:187-94.”

Change in text: on line 13 in page 11.

Reviewer G

This is a well-written report presenting a single surgeon experience of EP spRALRP vs mpRALRP, utilizing propensity score matching to achieve balanced cohorts. The manuscript is concisely and clearly written with high-quality data tables and appropriate discussions. I hope the authors can address the following queries

- Could you please describe the lead surgeon's learning experience of single port RALRP? How many SP has the surgeon performed independently prior to this retrospective cohort analysis?

Reply: Thanks for the good point. We analyzed the surgical results of one senior surgeon (S.H Cheon). This experienced surgeon had sufficient mpRARP experience before performing spRARP and did not show a learning curve, so we performed a comparison between surgical methods through propensity score matching. We hope for the reviewer's generous understanding.

- It is unclear to me whether in the spRALRP cohort the surgeon performed any pelvic lymph node dissection? The result table just showed 0 for lymph node involvement but the manuscript doesn't have clear description of whether lymph node dissection was routinely performed for either spRALRP or mpRALRP, and if performed for selective cases, what were the criteria and how many patients had lymph node dissections performed?

Reply: We appreciate the reviewer's comments and agree with your opinion. We did not routinely perform node dissection, but only performed it in cases where high stage or node invasion was suspected.

- The authors include in the operative outcome table 'Neurovascular bundle save' as a result outcome. I am not sure if this outcome in isolation is that meaningful. Was sparing of neurovascular bundle not done because of likely cancer involvement or because of technical challenges of the case? Delineating the reason why neurovascular bundle sparing was not performed is more meaningful.

Reply: Thanks for the good point. We perform it routinely because we believe that preservation of NVB is helpful for the patient's functional outcome. However, there are cases where it is difficult to perform due to suspected tumor invasion or adhesion of the capsule.

- I am very surprised by the long post-op hospitalization for both cohorts. Could the authors please elaborate on why patients stay for more than a week post-op and what is the typical post-operative surgery recovery protocol? At least in the US, in the era of ERAS protocols, at most high-volume centers most patients

typically get discharged on post-op day 1 after RALRP, and some even on the day of surgery at select institutions. It is very surprising to see that after robotic surgery, especially single port, that patients still end up staying in the hospital for 9-10 days on average.

Reply: Thanks for the good point. In our insurance system, cancer patients have many treatment fee reduction benefits, so the burden of hospitalization costs is low. Therefore, patients may be hospitalized before surgery to perform necessary examinations, which may result in a longer hospital stay. Also, if the patient wishes, we check urination and general condition after foley removal and then discharge the patient. Therefore, compared to other studies, the hospitalization period was found to be longer, but looking at the date of foley removal, you can see that there is no significant difference.

- The authors should highlight that a major limitation of the study is that results were based on the experience of a single surgeon and therefore may not be generalizable to diverse settings. Further, patients in the series appear to be quite healthy, with low BMI, no prior surgeries or radiation. Highlighting further the highly selective nature of the patients is also important

Reply: Thanks for the good point. Although there are many reports and good meta-analyses, I thought that comparing surgeries performed on a single surgeon would also have some value, so I conducted an analysis and wrote a paper. We hope for the reviewer's generous understanding.

- Lastly, I encourage the authors to discuss further what they think are the patient selection criteria for single port platform. Based on the lead surgeon's experience of the single port platform, which has its limitations and can provide benefit for some but not all patients in my opinion, which are the patients that can benefit the most from single port, and which patients are better served with multi port, which after all is already associated with a low level of invasiveness and has such an overall excellent performance and safety profile.

Reply: Thanks for the good point. The extraeritoneal approach has many advantages, and the sp robot platform is very helpful in carrying out this. However, it may be difficult to perform if the prostate is large, node dissection is required, or in advanced stages. However, on the contrary, for patients with a history of previous intra-abdominal surgery, the extraperitoneal approach is a necessary option for the patient. Therefore, it is believed that the surgical method needs to be selected depending on the patient. We have mentioned above opinion in discussion section (on line 6 in page 13).

Reviewer H

Although there are already comparative studies on this topic such as for example "Single-Port vs Multiport Robot-Assisted Radical Prostatectomy: A Propensity Score Matching Comparative Study" of Tae Il Noh et al. on J Endourol (2022 May),

I think that this topic is important and not yet well and fully explored.

Major revisions:

-Abstract:

You wrote in the abstract, but also in the manuscript, in the section results, that there are not significant differences between the groups in the operative time and then in the Conclusion section both in the abstract and in the manuscript that spRARP had a short Surgical time. So It seems that mpRARP have longer operative time than spRARP in your study. Therefore it is better to explain that spRARP have not statistically significant differences in the operating time compared to mpRARP also in your study.

Reply: Thanks for the good point. However, although there was no statistical difference, the spRARP surgery time was short both before and after matching. We hope for the reviewer's generous understanding.

-Methods and Results:

If you want to report statistical differences on urinary continence and erectile function, you should add data regarding the percentage of patients in both groups subjected to bladder neck preservation, any posterior and anterior reconstruction techniques and nerve-sparing techniques because these techniques significantly influence the results.

Alternatively you must also include among the limitations of the study that you do not have such data and that this can significantly influence the results on urinary continence and erectile function.

Reply: We appreciate the reviewer's comments and agree with your opinion. We perform it routinely because we believe that preservation of NVB is helpful for the patient's functional outcome. However, there are cases where it is difficult to perform due to suspected tumor invasion or adhesion of the capsule. Therefore, functional outcome was analyzed according to surgical approach (extraperitoneal or transperitoneal). We hope for the reviewer's generous understanding.

-Conclusion:

As you rightly explain in the limitations section, the spRARP procedure for the reasons you also mentioned, was applied on small prostates therefore it is not possible to extend the conclusions to any type of prostate until studies are carried out that demonstrate its real effectiveness even in those cases, so I would specify this in the conclusions and also in the limitations section.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have added your opinion in conclusion section

Change in text: on line 9 in page 14.

Minor revisions:

-The conclusion of the abstract and of the manuscript are written with the exact same words. It would be better to change the words of the conclusion of the

manuscript.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have revised conclusion section

Change in text: on line 3 in page 14.

Reviewer I

Multiport robot-assisted radical prostatectomy (mpRARP) is the standard treatment for localized prostate cancer.

In this retrospective study, the authors compared initial experiences of extraperitoneal single-port robot-assisted radical prostatectomy (spRARP) with transperitoneal mpRARP demonstrating that extraperitoneal spRARP is a feasible and promising procedure.

Congratulations to the authors for this study. The manuscript is well structured and it could be suitable for publication after some minor revisions.

Reply: Thank you for your positive evaluation and comments on the paper.

- How were patients who underwent spRARP selected? Inclusion and exclusion criteria, as well as prostate size, should be specified in materials and methods.

Reply: We appreciate the reviewer's comments and agree with your opinion. This study has limitations as it applied the SP robot system to a specific, small group of patients. The retrospective design also presents an important limitation, as it involved the selective inclusion of appropriate cases based on an initial experience with spRARP. To overcome this limitation, we conducted a PSM analysis; however, larger, well-designed comparative studies are needed. We have mentioned these limitations in discussion section (on line 1 in page 13). We hope for the reviewer's generous understanding.

- Is the trocar for the assistant used in spRARP?

Reply: We appreciate the reviewer's comments and agree with your opinion. We used trocar for the assistant in spRARP. Since many cases have already been implemented and reported using the same method, reference have been added (on line 12 in page 7).

- Why was the extraperitoneal mpRARP compared to the transperitoneal mpRARP instead of the extraperitoneal mpRARP? In the literature, several studies have compared extraperitoneal mpRARP with transperitoneal mpRARP (doi: 10.3389/fsurg.2023.1157528 ; doi: 10.1097/MD.000000000011176) . The authors should describe more about the advantages and disadvantages between the two approaches.

Reply: Thanks for the good point. Among the different surgical approaches, the extraperitoneal approach offers several advantages when compared with the transperitoneal approach. It results in less bowel irritation, facilitating early

postoperative dietary intake and contributes to quicker patients recovery. Additionally, adopting a less steep Trendelenburg position is associated with fewer respiratory and cardiovascular complications. Likewise, the extraeritoneal approach has many advantages, and the sp robot platform is very helpful in carrying out this. So, although there are many reports and good meta-analyses, I thought that comparing surgeries performed on a single surgeon would also have some value, so I conducted an analysis and wrote a paper.

- There is a difference in PSA value and prostate volume between the two groups. Could these differences have influenced the results?

Reply: We appreciate the reviewer's comments and agree with your opinion. These limitations are relevant to spRARP initial patient selection. So, we conducted propensity score matching to overcome this, and found similar results after matching. Of course, we think more research and well-designed large cohort studies are needed. Based on these results, we plan to conduct a study to compare long-term f/u results. We hope for the reviewer's generous understanding.

- Describe what were the grade 3 and 4 complications according to the Clavien-Dindo classification. In some cases, was it necessary to convert from an sp approach to an mp or open approach?

Reply: Thanks for the good point. Most complications were grade 3a due to urine leakage requiring endoscopic intervention. One patient with rectal injury had a grade 4 complication, and the patient was excluded when matching as he was an advanced stage patient who underwent mpRARP.

- The number of lymph node dissections in this series of patients is small. Was a validated nomogram used to decide whether to perform lymphadenectomy?

Reply: We appreciate the reviewer's comments. We did not routinely perform node dissection, but only performed it in cases where high stage or node invasion was suspected.

- There are some spelling mistakes that should be corrected.

Reply: We apologize for our carelessness. We will review it closely again and revise it. Thank you again for the review and good comments.

Reviewer J

In this paper, 64 patients who underwent spRARP and 120 patients who underwent mpRARP were compared in terms of postoperative outcomes using propensity score matching. To date, various studies have suggested that spRARP reduces postoperative pain and hospital stay, but this paper does not present anything new compared to these previous findings. Additionally, the comparison groups are not equivalent, as they did not use the same extraperitoneal approach, making it unclear whether the differences observed are due to SP vs MP or extra

vs transperitoneal approaches.

Reply: We appreciate the reviewer's comments and agree with your opinion. Among the different surgical approaches, the extraperitoneal approach offers several advantages when compared with the transperitoneal approach. It results in less bowel irritation, facilitating early postoperative dietary intake and contributes to quicker patients recovery. Additionally, adopting a less steep Trendelenburg position is associated with fewer respiratory and cardiovascular complications. Likewise, the extraeritoneal approach has many advantages, and the sp robot flatform is very helpful in carrying out this. So, although there are many reports and good meta-analyses, I thought that comparing surgeries performed on a single surgeon would also have some value, so I conducted an analysis and wrote a paper. We hope for the reviewer's generous understanding.

Minor points of concern include the Gleason Score (GS) not being categorized into five Gleason grades, and the need to verify why 80% of cases have a PI-RAD score of 4-5 while 70% are in the GS 6-7 at biopsy. Also, the pathological T stage distinction of T2a and T2c seems unnecessary. Modifications are needed in Table 2 and 4 regarding the use of analgesics, expressed in N%.

Reply: Thanks for the good point. Unfortunately, the difference between PI-RADS score and pathologic outcome is thought to be influenced by the fact that MRI was performed after biopsy. Tables 2 and 4 were confirmed and modified.

Further, the paper should clarify the indications for choosing SP over MP, and explain why the hospital stay is longer compared to other studies.

Reply: We appreciate the reviewer's comments and agree with your opinion. This study has limitations as it applied the SP robot system to a specific, small group of patients. The retrospective design also presents an important limitation, as it involved the selective inclusion of appropriate cases based on an initial experience with spRARP. To overcome this limitation, we conducted a PSM analysis; however, larger, well-designed comparative studies are needed. We have mentioned these limitations in discussion section (on line 1 in page 13).

In our insurance system, cancer patients have many treatment fee reduction benefits, so the burden of hospitalization costs is low. Therefore, patients may be hospitalized before surgery to perform necessary examinations, which may result in a longer hospital stay. Also, if the patient wishes, we check urination and general condition after foley removal and then discharge the patient. Therefore, compared to other studies, the hospitalization period was found to be longer, but looking at the date of foley removal, you can see that there is no significant difference.

Reviewer K

The authors compared the single-port extraperitoneal approach with the multi-port transperitoneal approach.

There are some comments and questions:

1. The title should be preferably without abbreviations.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have changed title as "Comparative study of extraperitoneal single-port robot-assisted radical prostatectomy and transperitoneal multiport robot-assisted radical prostatectomy using propensity score matching."

2. It should be explained what Gas-out(day) exactly means.

Reply: We appreciate the reviewer's comments and agree with your opinion. We have revised as followed "passing gas".

Changes in text: Table2, Table4, Abstract: on line 9 in page 2 & on line 1 in page 3, Methods section: on line 21 in page 6, Results section: on line 11 in page 10, Discussion section: on line 13 in page 1.

3. Why all patients received CT, skeletal scintigraphy and prostate MRI? Didn't they depend on pathological findings?

Reply: Thanks for the good point. In our insurance system, the above imaging test fees are reduced for cancer patients, so examinations are performed without any restrictions. Therefore, bone scan to check for bone metastasis, CT scan to check for abnormalities in other organs and metastasis, and prostate MRI to provide anatomical assistance in prostate surgery are routinely performed after diagnosis of prostate cancer.

4. To what criteria or indications was the decision regarding each approach (single-port extraperitoneal vs. multi-port transperitoneal)?

Reply: We appreciate the reviewer's comments and agree with your opinion. This study has limitations as it applied the SP robot system to a specific, small group of patients. The retrospective design also presents an important limitation, as it involved the selective inclusion of appropriate cases based on an initial experience with spRARP. To overcome this limitation, we conducted a PSM analysis; however, larger, well-designed comparative studies are needed. We have mentioned these limitations in discussion section (on line 1 in page 13). We hope for the reviewer's generous understanding.

5. The methods part should also include the definition of each approach.

Reply: We appreciate the reviewer's comments and agree with your opinion. So, we have revised methods section as followed.

Change in text: In spRARP, patients were positioned supine with 10–15° Trendelenburg tilt. A 3-cm subumbilical incision was made to create extraperitoneal access and space using fingers and a balloon dissector (on 6 in page 7). In mpRARP, Patients were placed in the lithotomy and 30° Trendelenburg positions. Four 10-mm incisions along the umbilicus for the robot arms and two 12-mm incisions for assist were made for assistance. After transperitoneal access, the bladder was dissected. Afterwards, prostatectomy was performed as in

spRARP (on 14 in page 7)