Reviewer's report

**Title:** Impact of surgical technique (open vs. laparoscopic) on pathological and long term functional outcomes following radical prostatectomy

**Version:** 2 **Date:** 14 March 2013

**Reviewer:** Phillip M Pierorazio

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**Summary:** The authors randomly review >300 patients who underwent RRP and LRP, comparing surgical and functional outcomes after each surgery. Major findings include similar rates of positive surgical margins, similar functional outcomes are achieved in each modality and outcomes that only appear to vary based on surgical technique.

**Review:** There are a number of serious methodological flaws that make this paper unpublishable in its current form. These flaws seriously contend the conclusions. However, if remedied, this paper may be suitable for publication.

**Specific Comments/Critiques:** MAJOR COMPULSORY REVISIONS

1) The study lacks a power calculation. Is there a reason 330 patients were randomly selected? Why not all patients and then a matched analysis? The choice of analysis needs to be further discussed.

2) Table 1 demonstrates a number of important variables that are similar among these randomly selected groups. However, a number of important data points are missing:
   a. How many patients were selected from each surgeon’s experience?
   b. What was the experience level of each surgeon while performing the operation? (ie. What was the average case number of selected RRP and LRP patients?)

3) In general there are more than a handful of typos that make the manuscript difficult to read. For instance, there is a typo on page 4, table 2 is also misreferenced and the formatting of the tables makes them hard to interpret.

4) I would not consider 0-1 pads as No Incontinence. Patients who wear even 1 pad per day because of occasional leakage are not continent. This should be clarified.

5) Overall positive surgical margin rate is high, but within limits of the reported literature. What is the PSM rate in pT2 disease? This is an easily obtainable outcome measure than may speak to quality of surgery and nerve sparing?

6) While the chi-squared test for IIEF is not significant there certainly seems to be a clinical difference between IIEF categories among RRP and LRP. Nearly double the proportion of patients undergoing RRP had mild-moderate ED while 50% of patients undergoing LRP had severe ED. If the p-value is accurate this
study may certainly be underpowered to detect a difference in ED among groups. If so, the authors should consider adding more data points or at least addressing this issue in the discussion.

7) One of the major differences in the groups that is not well addressed in the results is the much higher proportion of non-nerve sparing using in LRP despite similar pathological outcomes. It is unclear from the manuscript why more patients undergoing minimally invasive surgery have less nerve-sparing if they present with the same preoperative characteristics and have similar pathological outcomes. It certainly introduces a bias that the LRP surgeon(s) have toward these patients.

8) The conclusions regarding surgeon experience cannot be drawn given the data that is presented. While it is certainly provocative that the surgeon with less RRP experience had worse outcomes, this data is not granular and care needs to be taken when drawing conclusions from it. It is not enough to say surgeon 1 has X experience and surgeon 2 had Y experience (with X>Y). It should be easy to chronologically number the cases and determine at exactly what level was each surgeon when RRP and LRP were performed. Otherwise there is potentially huge bias that needs to be addressed because surgeon 200 may have performed all of the RRP early in his career and all the LRP later (as with many surgeons) and therefore the conclusions are not valid.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

None