Reviewer's report

Title: Prediction of postoperative pain after radical prostatectomy

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Reviewer: David R. Urbach

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Prediction of postoperative pain after radical prostatectomy

1. Is the question posed by the authors well defined?

The authors defined 2 separate questions. The first was to evaluate the relationship between previously reported preoperative factors associated with post operative pain and the incidence of post operative pain in their population of 155 men undergoing radical prostatectomy. The second question was to assess if pain scores in the immediate post operative period (4 hours post op) predicted ongoing poor pain control over a period of 3 postoperative days. The second question was not clearly described.

2. Are the methods appropriate and well described?

The authors completed a prospective observational study. The instruments were well described, however, the methods could better describe the timeline of data collection. The statistical analysis was not described in sufficient detail (see comments below).

3. Are the data sound?

Yes.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

The way that results of regression models was reported is unconventional. Please see detailed comments below. Results tables should present effect estimates (eg ORs) and 95% confidence intervals, not just P values.

In the results section, under “perceived pain control, anxiety and depression” the authors report a “tendency” between preoperative anxiety and postoperative pain. However because the p-value was 0.073, this should be reported as a non-significant finding in this study.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

Yes, all except the report of the link between anxiety and post operative pain which was not found in this study. The discussion, which in the end tries to focus
on the need for better nursing assessment could focus more clearly on the conclusion from the multivariable logistic regression model that the previous VAS score was always predictive of a high VAS score in the next 24 hour period.

6. Are limitations of the work clearly stated?

There could be more discussion on the limitations of the practice change at the centre, because during the study period, the main pain control treatment changed from EDA to ITA or SOA, which may be a confounder.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

They do refer to their previous studies.

8. Do the title and abstract accurately convey what has been found?

Yes

9. Is the writing acceptable?

There are some stylistic issues with the writing of the paper. VAS which is mentioned as an acronym in both the abstract and the background should be written as Visual Analogue Scale.

Please make your review as constructive and detailed as possible in your comments so that authors have the opportunity to overcome any serious deficiencies that you find and please also divide your comments into the following categories:

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

1. Clarify the link to nursing by referring to the results of the multivariable logistic regression analysis, which suggest that once a patient experiences severe pain, they are more likely to continue to experience it for the next 24 hour period which suggests pain assessment/treatment is not ideal.

2. Use of “ITA” (intrathecal anesthesia?) is unfamiliar. Is this spinal anesthesia?

3. I found it very difficult to follow the last paragraph on page 9 (results related to perceived control, anxiety and depression) – the text is choppy and seems to jump around from concept to concept. It should follow a logical order.

4. Reported mean PACU time (14 +/- 7 hours) seems VERY long for time is a PACU, which is usually less than 3 hours. Please confirm.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. Tables (eg Table 2) should not be referenced in the methods; all data must be presented in the results section. The information presented in Table 2 should be
presented before that from Table 1 in the current manuscript

2. Why was “baseline” (pre-operative) VAS pain score not measured? One might expect correlation between baseline pain perception and pain perception after surgery. The authors should discuss why this was not measured/analysed.

3. Some references are not cited numerically (eg bottom page 12)

4. Figure 1, 1st graph in top left – why does this curve appear to be a step function rather than a smooth function?

5. Why did you analyze relationships using regression models instead of correlation coefficients? While either approach would be OK, the paper should explain why the authors’ preferred approach was taken.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. There is insufficient description of methods related to statistical analysis (eg page 8). Since important conclusions of this study, for example LACK of association between some predictors and response variables, it is critical to know exactly what analyses were done. Please describe the modeling approach. What comparisons were made? How were the analyses done that led to the results described in Figure 1?

2. Results of regression analyses must be presented with estimates of effect and 95% confidence intervals, not solely P values which are not as informative.

3. I do not think that the reported “tendency” for preop anxiety to relate with postop pain should be listed as a finding, especially since it appears to be based primarily on a P value result (see previous comment about reporting results of regression models), with a value (0.073) that would not be considered “statistically significant”.

4. Why are the findings about correlation between pain at 4h, day 1 and day 2 important, particularly in the absence of information on “baseline” (pre-morbid) pain levels?

5. Presentation of the results of the regression models on page 11 (top half of page) is unconventional. It gives the impression of a ‘meandering’ analysis that was not specified a priori, and does not appear to be hypothesis-driven. If a limited number of hypotheses were being tested, these should be described explicitly in the methods.

6. The conclusion on page 11 that “the only factor that could predict pain was the previous VAS score…” may not be correct. It all depends what predictors were included in the regression model, and how this analysis was done. Without this information, and a complete table of results of the regression analysis, it is not possible to tell whether the data support this statement.

7. To what extent is there confounding between choice of anesthesia (spinal,
epidural, IV bolus) and VAS pain etc? I suspect that assignment to treatment modality (eg epidural vs bolus IV, or epidural vs spinal) depends on patient factors which are highly correlated with pain perception. This makes it difficult to interpret comparisons such as those in Table 4, which demonstrate differences in pain perception according to pain management method.

David R Urbach MD

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests