Reviewer's report

Title: Optical Coherence Tomography for an In-Vivo Study of Posterior-Capsule-Opacification Types and their Influence on the Total-Pulse Energy Required for Nd:YAG Capsulotomy: Case Series

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Reviewer: Mostafa Elgohary

Reviewer's report:

Methods

The author had to explain to us how he distinguished cases with capsular distension syndrome, whether any of the PCO types occurred also in these cases and why they chose to look at late ones.

The authors should include a section in the methods on Surgery or Surgical technique to avoid repetition of Phacoemulsification in brackets throughout the manuscript.

In the methods, comments such as "Many Nd:YAG laser capsulotomy techniques have been described in the literature and used in clinical practice. However, agreements about a standardized technique and postoperative management still do not exist [22]." are better left for the discussion.

What was meant by average in the methods (i.e. mean, median, mode) and there is no measure of distribution e.g. SD or interquartile range.

Authors should explain the methods a bit more i.e. by explaining if the measurement of the PC distance from the IOL posterior surgery was made over a selected area and how that area was chosen. if not the inconsistency in the area used for measurement might be a potential source of errors.

The authors did not explain why they used Kruskall-Wallis non-parametric test for comparison, was that because of the lack of normal distribution of their data?

The authors did not elaborate on how they choose their area for capsulotomy i.e. if they had a predefined area for everyone. This is important as larger areas would require more energy regardless of the PCO type. If not, was the area measured afterwards and accounted for in the linear regression analysis? I believe this all must be done to increase the validity of the data before the publication.

Results

There is no need to include the data in a tabulated and graphical form (e.g. Table 1 and Figure 5)
The comments in the results about the use of median as well as the mean to compare the data is confusing as it suggests they give different results when in fact they really should not if there indeed was a difference. The comparison between the mean and median however should relate to how the data is distributed and the authors should be consistent in the use of their statistical method.

I would not accept the results on the total energy used for different types of PCO until I have seen the area treated and whether it was standardised (i.e. authors used a fixed area) or measured post hoc and accounted for statistically.

Discussion
The authors are discussing the use of SD-OCT as a tool for PCO characterisation. The obvious difficulty of capacity and cost notwithstanding, the recommendation fails to explain how this might be useful as it appears there is no harm induced or increased risks portended by using higher energy level for posterior capsulotomy.

The small sample size is not the only explanation for the poor fitting of the model as I believe the lack of other variable that are more relevant such as the area treated is more important for enhancing the fitting of the model.

Statistical review is necessary to check tables, graphs, test results etc!

Some English editing is necessary for the manuscript to read better e.g. Sentences such as in the methods, Patient section: The patients for treatment were chosen according to the reduction...., can be changed to The patients were chose for treatment if they had ....). Also phrases such as "medicamentous mydriasis" is unfamiliar and may be confusing.

Minor Essential Revisions
Tables 1 & 2 contain redundant data i.e. Mean & median, SD and range. Only two of these are required if they are not already mentioned in the text.

Discretionary Revisions
In the abstract: results: the author uses the present tense to describe their results when they should use the past tense (e.g. the highest energy is required should be was required, etc)

The authors should discuss the non-masking design which understandably would have been impossible but this could have introduced bias from the operator towards the type of PCO and use of energy, etc.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published
Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
None to declare!