Reviewer's report

**Title:** Rotational stability of the AcrySof SA60TT toric intraocular lenses.

**Version:** 1  **Date:** 6 January 2008

**Reviewer:** David Chang

**Reviewer's report:**

This is a nicely written report of a relatively small study of eyes receiving the Acrysof toric IOL. The study has been carefully performed and would be of interest to the readership.

I would like to see several concerns addressed in the discussion.

First, the alignment measurements are stratified into increments as small as 2 degrees. While such precise measurements may be possible at the slit lamp when using a special reticle, such accuracy does not seem possible intraoperatively at the operating microscope. It would seem that increments of +/- 5 degrees of misalignment would be more practical and useful, since the "limiting" factor is the accuracy of intraoperative measurement and alignment. It may therefore be somewhat misleading to report data broken down into increments of single degrees.

Second, and related is the assumption that any misalignment is due to rotation of the lens. The authors even go so far as to report whether the IOL "rotated" counterclockwise or clockwise. This degree of precision again seems to be based upon assumptions that may not be correct. Would such slight amounts of misalignment be just as likely to be due to imperfect surgical alignment? There are myriad issues, including parallax, that make it somewhat difficult to align toric IOLs perfectly intraoperatively, and the authors should at least acknowledge this as a possible explanation for any "misalignment". I suggest that all the tables be labelled as "misalignment" rather than "rotation". The latter term is making a diagnosis and assumption that I do not see sufficient proof of.

While the tables may remain as formatted, I believe the discussion should caution against assuming that the IOLs are rotating, and that the study can detect rotation of as small an amount as a 2 degrees. The reader should not be led (by the counterclockwise/clockwise data) into trying to determine the likely direction of rotation from this study.

Finally, the authors seem to be questioning why the net drop in refractive cylinder is not larger in the T5 group compared to the T4 group. Using the mean may not be the best measure when the sample size of each group is 20 eyes or less. It would be more interesting to know how many of the T5 group achieved a reduction of at least 1.75 D, compared to how of the T4 group achieved a reduction of at least 1.25 D. The problem with a small sample size is that just 2-3 eyes with aberrant results or inaccurate refractive data can significantly affect the mean. If
we are to compare the T4 and T5 results, then showing us the breakdown of how many (and what % of) patients achieved the targeted reduction would be better than just presenting a mean.

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I am on the Alcon Speakers bureau and have received travel and speaking reimbursement from Alcon. I have no financial interest in their products or IOLs. I am a consultant for AMO and for Visiogen. I have no competing interests at all.