Reviewer’s report

Title: Prospective Study of Toric IOL Outcomes based on Dual Zone Automated Keratometry

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Reviewer: Paul-Rolf Preußner

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Major Compulsory Revisions:
1. The number of subjects (eyes) is not well defined at the beginning. The numbers are inconsistent in the abstract (Methods: 50, Results: 43) and the explanation of this inconsistency is found only later in the text. I propose to omit the 3 eyes with clinical complications as well as the 4 eyes with a different calculation method and restrict the whole paper to the 43 cases.

2. The wording “dual zone automated keratometry” used in the heading is not well-defined and may be misleading (even if used in other literature references), as well as the wording “manual keratometry”. I propose to use the specific name(s), e.g. Haag-Streit-Lenstar. The details of the extraction algorithm of 2 central vertex radii and their axis from the 32 points in 2 rings is proprietary knowledge of Haag-Streit, and another algorithm might produce different results based on the same dual zone data. Similarly, different types of manual keratometers with different patterns of the projected pictures (e.g. points, crossbars etc. projected at different distances to the corneal center) can produce different results. Therefore, if compared, the comparison should be restricted to the compared devices, not “manual” versus “dual zone automated”. But in the presented paper, no such comparison is performed. Thus, the results are “as obtained from the Haag-Streit Lenstar”.

3. The “standard deviation of the angle” in the abstract is not understandable. Later in the text I could find that the standard deviation of repetitive measurements of the angle was calculated.

4. Comparisons of pre- and postoperative astigmatism after toric IOL implantation should be based on vector difference calculations, and
the results should be presented in corresponding dual-angle plots. The same calculation method should be applied to calculate the SIA.

5. The axis marking method was changed during the study. This needs additional verification or at least discussion, that both methods are equivalent.

6. The standard deviation of the axis of the astigmatism obtained from repetitive measurements is physically misleading. In the limit of the astigmatism $\neq 0$, the angle is a random number. Instead of the used standard deviation, an error propagation discussion would be needed showing the error of the result as a function of the errors of the input data. If this seems oversized, also the standard deviation of the axis from repetitive measurements ($>5$ or not) should be omitted.