LETTERS

Association of TASS with ointment in the anterior chamber following cataract surgery

In their report of toxic anterior segment syndrome,[1] Werner et al. highlight the importance of a secure wound. They mention eye patching as a possible risk factor for this complication.

After sutureless clear corneal cataract surgery in an adequately pressurized eye, there is good reason to believe a patch (placed just firmly enough to hold the lid closed if the patient looked up) would create a more stable wound than if the eye were free to blink. McDonnell et al.[2] illustrate how a new clear corneal wound will open if the intraocular pressure (IOP) drops. By patching the eye, the low IOP associated with blinking[3] is avoided, thus increasing wound integrity. Patching may also speed re-epithelialization of the wound and help wound edges adhere, both of which maintain wound integrity. Inadvertent eye rubbing by the patient is also avoided.

For years, I have patched sutureless temporal clear corneal wounds after ensuring adequate IOP and wound integrity. In almost 1000 cases, tobramaycin 0.3% and dexamethasone 0.1% ointment (TobraDex) has not entered the eye. However, I am perplexed by how ointment gained access to the anterior chamber in the reported cases. Werner et al. report this complication disappeared after patching was stopped, but the use of ointment was stopped at the same time. I wonder whether these cases illustrate the hazards of a tight patch over a soft eye rather than the hazards of patching in general.

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Reply: In our recently published report of 8 cases of TASS associated with ointment in the anterior chamber, we highlighted the risks possibly associated with the combination of unsutured clear corneal incisions, ointment, and eye patches. We believe the association of the first 2 factors was the most significant in these cases; the role of the firm eye patch used by the surgeon was unclear.

Grosser states that after sutureless clear corneal cataract surgery in an adequately pressurized eye, a patch might create a more stable wound by holding the lid closed. We hypothesize that the presence of the eye patch could enhance the contact of the ointment with the anterior corneal surface by keeping the eyelid closed, increasing the risk for ointment penetration into the anterior chamber in case of hypotony. In a retrospective clinical study, Shingleton et al.[1] report that 20.5% of the patients evaluated had an IOP below 5 mm Hg 30 minutes postoperatively. All the patients in the same study had a self-sealing clear corneal wound that was tested to be watertight, and an attempt had been made to leave the IOP at an arbitrary level of 10 mm Hg by tactile measurement at the completion of the procedure. However, tactile IOP measurements can be inconsistent, and the IOP could have been lower than 10 mm Hg. The time between completion of surgery and IOP measurement may not have been long enough to allow the aqueous formation and IOP to normalize. Based on this mechanism, it is unclear that an eye patch would have made any difference in terms of the hypotony observed. We also have to consider that a corneal incision that is hydrated at the end of the procedure may dehydrate relatively fast, compromising its self-sealing characteristics.

Grosser also states that patching may speed re-epithelialization of the wound and help wound edges adhere. To our knowledge, no systematic study to evaluate this has been done. The available literature relates to the management of corneal abrasions with patching, showing no acceleration of re-epithelialization and no alteration in the epithelial wound healing pattern.[2] In a study involving 44 patients having phacoemulsification with intraocular lens implantation, Honda et al.[3] found higher aqueous flare levels in the patched group. They hypothesize that this was because of the lack of cooling. Cooling facilitates aqueous flow, which helps disperse and remove proteins in the anterior chamber. It is also possible that the extended lid closure that occurs with an eye patch causes anterior chamber hypoxia, which influences the flare level.

For these reasons, we still do not recommend tight patching of the eye following cataract surgery, especially in association with ointment and a clear corneal wound.—*Liliana Werner, MD, PhD, Nick Mamalis, MD*

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Q-factor customized ablations

In their article about Q-factor customized ablations with the WaveLight Allegretto laser,[1] Koller et al. conclude that "corneal asphericity was less impaired by the custom-Q treatment up to −5 D myopia." This conclusion does not seem to be fully supported by the data presented. Would the authors please elaborate on some aspects of their methodology to clarify how they arrived at their conclusions?

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1. The authors calculate the asphericity (Q) at 5 different zones (10, 15, 20, 25, and 30 degrees) from the corneal apex in 4 main hemimeridians and then average the opposite hemimeridians to determine the asphericity in the major axes. This would seem to be invalid because asphericity is not symmetric and Q-values cannot be averaged because the function to calculate Q-values is not linear.

In fact, this appears to have led to an increase in coma in the custom-Q group, which did not occur in the wavefront-guided cohort. If a treatment attempts to symmetrically equalize the asphericity of an asymmetric surface, coma will result. This suggests that custom-Q treatments, as presented, are clinically inferior to wavefront-guided treatments (at least for coma). Wavefront-guided treatments target the entire wavefront, not just the shape of the corneal surface. Can the authors reconcile this observation with their conclusions?

2. The authors use a one-size-fits-all approach to set the target asphericity and base the target on a theoretical calculation presented by Manns et al.[2] using the Navarro eye model. That eye model uses 4 aspheric surfaces to represent the cornea and lens to simulate anterior and posterior contributions of the real optical surfaces. Manns et al. present elegant calculations to show the asphericity needed to offset the induced asphericity of myopic ablations through 10.0 diopters in a 6.0 mm ablation. They also graph the relationship of how the preoperative asphericity affects that relationship. On average, for the Navarro eye model, Manns et al. show that the ideal postoperative corneal asphericity to achieve zero spherical aberration is about −0.46.

Koller et al. interpret this literally and do not allow for variations in preoperative asphericity among eyes. Rather than customize the target Q-value to neutralize spherical aberration, they target all eyes to the mean value of −0.46. They then state that Q-adjusted ablations are superior to wavefront-guided because they leave the cornea more prolate, on average, without reference to the starting point. This seems invalid. If an eye has positive preoperative spherical aberration, setting the postoperative asphericity to a more negative value will actually increase spherical aberration. If the ocular spherical aberration is not determined preoperatively, the optimized Q-value cannot be determined. The use of a universal Q-value target of −0.46 will make some eyes worse.

3. The authors compare their visual and refractive outcomes for custom-Q and aberrometer-guided treatments but do not report the spherical aberration results for the 2 cohorts except to say they were similar. This statement seems inconsistent with the differences in asphericity reported for the 2 cohorts. Could the authors please explain this?

4. Results from the recent U.S. Food and Drug Administration study of the Allegretto laser for wavefront-guided laser in situ keratomileusis (LASIK) study, which I administered, show that eyes with 0.2 μm preoperative RMS_H wavefront error or less had no significant change in aberrations after surgery on average (K.G. Stonecipher, MD, "Comparison of Wavefront-Guided and Wavefront-Optimized Treatments with the Allegretto Excimer Laser," presented at the ASCRS Symposium on Cataract, IOL and Refractive Surgery, San Francisco, California, USA, March 2006). Eyes with higher amounts of preoperative aberrations experienced decreased aberrations after surgery. The ability of wavefront-guided LASIK to lower aberrations in some eyes was only apparent when the data were stratified according to preoperative characteristics.

The authors' average preopeative aberration RMS_H was 0.24 μm, and the analysis was not stratified based on preoperative findings. In the absence of stratification, it is not possible to draw meaningful conclusions from the results, as significant differences can be expected to exist among subgroups. I would have expected the data to show significant differences depending on the amount of preoperative aberrations. Was this the case?

Pending the authors' response, I would question the authors' conclusion that the Q-factor cohort outperformed the aberrometer-guided cohort because they had more prolate corneas with great caution. Visual quality is determined by the resultant wavefront, not the shape of the first optic. Prolate corneas may be desired in most eyes, but one size won't fit all.

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Reply: First of all, we want to thank Kezirian for the valuable comments to our presentation of the first and preliminary results of the Q-factor adjusted ablation profiles. We'll discuss the final remarks first. We fully agree that there is not one optimal Q-factor that fits all. In recent years, we have learned that the total spherical aberration in the eye is a balance of the spherical aberrations originating from all refractive elements of the eye such as the 2 corneal surfaces, the lens (anterior and posterior surfaces but also the refractive gradient inside the lens), and even the curvature of the posterior pole. In eyes that had cataract extraction plus intraocular lens (IOL) implantation, there was an abnormal high variance of spherical aberration that is explained only by the loss of compensation due to the spherical IOL.[1] At first glance, one logical consequence of this consideration is to attempt to reproduce the preoperative Q-factor within the optical zone during keratorefractive surgery to maintain the assumed balance of positive and negative spherical aberrations. Another consequence is the question of why cataract surgeons are still implanting (a)spheric IOLs without considering the asphericity of the cornea. From a strategic point of view, we also have to accept that aberrations originating in the lens are changing (as a result of aging and, eventually, cataract surgery) and therefore it is worthwhile to think of the cornea as a suborgan to be optimized separately.

The intention of our study was to prove the feasibility of Q-factor adjustment. Therefore, we had the choice of aiming at a fixed Q-factor that may be the best compromise or at the preservation of the preoperative Q-factor. It was clearly easier to interpret the results following the first strategy, which was why we aimed at a single postoperative Q-factor of −0.46. We failed to achieve this postoperative Q-factor, but we could easily analyze the influence of parameters such as preoperative Q-factor, K-readings, attempted refractive change, and ablation profile. Based on the results of this study, our next step is to begin a study aiming at preserving the preoperative asphericity of the cornea. But even this approach, preserving the preoperative Q-value to achieve the optical performance of the total eye after the refractive