Corneal hysteresis changes in diabetic eyes

In their recent article,[1] Goldich et al. found that corneal hysteresis (CH), corneal resistance factor (CRF), and central corneal thickness were significantly higher in diabetic eyes than in healthy eyes and CH was claimed to be related to the corneal stiffness. However, we found significantly lower CH values in diabetic eyes than in eyes of healthy subjects.[2] We believe that the statements that equate a higher CH with increased corneal stiffness are at best speculative in this context. Corneal hysteresis can increase or decrease with stiffening depending on the behavior of the viscous material element, so the change in CH alone has too many undefined degrees of freedom to say anything more than that CH is increased.

Hysteresis has been shown to decrease during aging, when the cornea is known to stiffen, as well as after the cornea has been stiffened by crosslinking techniques (Noguera GE, et al. IOVS 2007;48:ARVO E-Abstract 1860). Since diabetes is a well-known cause of crosslinking, it may be that CH decreases in diabetes, as in aging. A recent paper by Glass et al.[3] addresses this complexity. The output of the 3-element viscoelastic model demonstrated behavior consistent with clinical data of increasing or decreasing hysteresis with stiffening of the cornea and low hysteresis with low elastic modulus (as in keratoconus) or high elastic modulus (as in advanced age). This model illustrates how changing viscosity and elasticity affect the hysteresis measurement in various ways.

We think issues surrounding the samples are driving the findings in both papers; that is, how similar the groups are in terms of possible confounders of CH and CRF. In the Goldich et al. study, the diabetic patients were not described in terms of severity. We think it essential to investigate the role of HbA1c and disease duration on CH and CRF as it may help explain the discrepancies. From this point of view, we believe that larger cohorts studying the role of severity of diabetes, disease duration, and HbA1c are needed to explain the inconsistencies.

Afsun Şahin, MD
Atilla Bayer, MD
Eskisehir, Turkey

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REPLY: We agree that CH is more a measurement parameter specific to the Ocular Response Analyzer (ORA; Reichert, Inc.) than a well-defined physical property of the cornea and that much remains to be understood about the relationship between parameters measured by the ORA and their relative contribution to corneal elasticity and rigidity. We also agree that further studies of larger cohorts are needed to identify CH's role in the diagnosis of corneal disorders. For example, while some studies show decreased CH with increasing age,[1] others report the opposite.[2] The same ambiguity exists in our study and the study by Şahin et al. concerning CH and diabetes. Regarding these 2 studies, different results may be the result of specific differences. For example, in our study, patients were significantly older. We included only one eye per patient, whereas Şahin et al. included both eyes without statistically accounting for between-eye correlation (we think this is a methodological flaw).[3,4]

The IOVS abstract by Noguera et al. cited by Şahin et al. described ex vivo porcine eyes that had experimental ultraviolet-A (UVA) riboflavin collagen crosslinking (CXL) and were evaluated by ORA. These treated eyes showed an increase in CH. However, the eyes that were weakened by microkeratome flap creation in this and other studies showed a decrease.[1,5] Our in vivo human study of keratoconic eyes treated with UVA– riboflavin corneal CXL showed no statistically significant change in CH or CRF.[6]

We agree that in a future study of a large cohort of diabetics, it would be beneficial to know the severity and duration of diabetes, as may be assessed by the presence of diabetic neuropathy, retinopathy, nephropathy, or by measurements of HbA1c.—*Yakov Goldich, MD, Yaniv Barkana, MD, Isaac Avni, MD, David Zadok, MD*

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Chopper and side-port incision leakage

Liyanage et al.[1] should be commended for attempting to study incisional leakage and potential anterior chamber stability during cataract surgery, although the latter was not formally studied. Unfortunately, the study has several methodological issues, including a very rudimentary method to measure irrigation and aspiration volume (graduations on irrigation bottle and aspiration bag, respectively). Measuring volume differences using a strain gauge might have been more precise.

Our biggest concern is in the second part of the study in which the maintenance of a chopping instrument within the side port and its impact on incision leakage was evaluated. Fluid loss between 2 different surgeons (one who removed the chopper after chopping and one who did not) was compared. Although incision sizes were apparently similar, numerous confounding variables probably made the results less meaningful. Confounders that were not reported or controlled include specific machine settings (bottle height, flow/vacuum settings, power modulation), bed height, surgical styles (handling of chopper, maneuvers), hand positioning, cataract density, phaco times, intraocular lens model and size, and surgeon skill. While we assume neither surgeon used continuous irrigation, one surgeon might have been in foot position 2 between chopping/lens manipulations, while the other surgeon might have preferred foot position 1; this can dramatically alter the measured incision leakage. Furthermore, with such a small number of cases assessed (11 in one group and 16 in the other), a spurious result is highly possible. The scatterplot presented is misleading because data appear to be presented as paired data when, in fact, they are not.

Although a percentage score of incision leakage was used to negate variable surgical times, other factors as discussed above could have exerted a confounding effect on the results. Furthermore, it might have been better to measure fluid inflow and outflow during only the phacoemulsification portion of the procedure to more accurately assess differences in side-port instrumentation techniques.

Although the authors raised some important considerations in phaco technique, we would encourage a more robust and controlled method of analyzing the impact of side-port instrumentation. A second instrument can provide stability for the eye during topical surgery, and many surgeons find it helpful in carouseling nuclear fragments at the phaco tip. This permits greater efficiency, less turbulence, and reduced case times, as well as improved fluidic and phacoemulsification metrics. To reduce the potential for excessive leakage from side-port incisions, we advocate a smaller side-port incision with a trapezoidal design (1.0 mm external incision and 0.5 mm internal incision) to prevent oar locking but with less leakage and using the incision as fulcrum to reduce tissue trauma.

Devesh Varma, MD
Iqbal Ike K. Ahmed, MD
Toronto, Ontario

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REPLY: We thank Varma and Ahmed for their helpful comments. While anterior chamber stability was not formally studied in this paper, the closed-system model of phacoemulsification implies that incisional leakage is related to anterior chamber instability.[1]

We acknowledge that this is a small study that took place in a clinical setting dedicated to high-volume cataract surgery. It was not practically feasible to use a strain gauge to measure volume differences and to specifically measure inflow and outflow during phacoemulsification; these are more suited to a controlled laboratory environment.

Varma and Ahmed correctly highlight confounding variables inherent in this study, some of which were controlled (same machine settings and intraocular lenses used by the 2 surgeons). It is notoriously difficult to quantify and qualify surgical skill, but both consultants have performed more than 1000 phacoemulsification procedures. It is also difficult to control for hand position, foot position (in the absence of continuous irrigation), cataract density, and phacoemulsification times in a real-world setting.

We apologize for the scatterplot misleading Varma and Ahmed, but the key and legend make no reference to paired data and clearly identify the techniques used. While we agree with the potential benefits of a trapezoidal side-port incision design and the use of the second instrument in manipulating nuclear fragments, our experience has encouraged us to question whether the use of a second instrument confers significant additional ocular stability when the phacoemulsification probe is in situ.