LETTERS TO THE EDITOR

Metrics for Keratoconus

To the Editor:

I can appreciate the quest for a better metric for keratoconus. I welcome any metric that improves on current ones. Jafri et al., in their article that appeared in the October 2007 issue of the Journal of Refractive Surgery, believe that the results of their modified I-S accomplishes this goal. I believe their new metric perhaps does not meet the authors’ claims.

The authors define the ‘‘suspected keratoconus’’ group on the asymmetric bowtie skewed radial axes (AB/SRAX) pattern that is shown in Figure 1. Figure 4 is a case of ‘‘suspected keratoconus,’’ but topography is only inferior steepening, which is not an AB/SRAX pattern. By Rabinowitz’s AB/SRAX definition, two lobes are required, yet Figure 4 shows only a single lobe that is not AB/SRAX pattern. This questions which eyes were used for the ‘‘suspected keratoconus’’ group.

On page 777, the authors state ‘‘…we estimated that 95% of normal eyes had a value less than 1.72, a value between 2.1 to 3.2 in 95% of eyes with suspected keratoconus, and a value greater than 2.82 in 95% of eyes with early keratoconus.’’ There is a ‘‘blank spot’’ between 1.72 (normal) and 2.1 (suspected keratoconus) and there is great overlap between 3.2 (upper range of suspected keratoconus) and 2.82 (lower range of early keratoconus). How useful is a new metric that leaves a large gap on one end and has significant overlap on the other end of the spectrum?

On page 780, the authors refer to Figure 10 as proof their new metric can separate the three groups better than inferior-superior (I-S) value or vertical coma alone. Close inspection of the graphs reveals in the top right graph (new metric) and bottom graph (I-S value) that the X axis maximum values are different: 4.5 and 8.75, respectively. Despite this graphical misrepresentation (maximum value should be the same for graphs to allow fair comparison), a similar number of eyes have the same value for both the new metric and I-S as indicated by similar overlaps of the green line (normal group) and yellow line (suspected keratoconus) in both graphs. Similar overlap means that the new metric does not better separate eyes than I-S. The new metric does not seem to help.

Why do I think this study fell short of the claims? The new metric is based on the old I-S value. Inferior-superior is dated—it assumes cones are directly inferior along the 270° meridian. Most cones are not there; most are oblique. We conducted a comparative study of I-S value to L-U value (lower-upper). The L-U individualizes the reference meridian. Figure A shows for an oblique cone, I-S will be calculated with Point 1 being in the superior (presumed flat) area, but this point is actually steep, not flat, which leads to calculation error of the cone. In Figure B, L-U avoids this error by rotating the reference meridian based on each cone. Our comparative study (unpublished) showed L-U was more accurate than I-S.

The authors are to be congratulated for their attempts to improve I-S value. I am confident that such metrics will be available in the future, which include L-U value.

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REFERENCE

Reply:

Dr Boxer Wachler has missed the point of our article, which hopefully has not been lost to the readers of the Journal. The point we tried to communicate in our publication is that if you see a topography map with “suspicious” inferior steepening and you are not sure what to make of it, do wavefront analysis. If wavefront analysis shows this patient has increased vertical coma, this should heighten your suspicion that you might be dealing with a patient at risk for developing keratoconus. This is just an additional tool in your armamentarium for screening patients prior to refractive surgery and should not be relied on solely. Ultimately, your clinical judgment should take precedence over any topographic or wavefront tests.

Dr Boxer Wachler’s critique of the article reflects a general lack of familiarity and understanding of the published literature with regard to the “early” detection of keratoconus and keratoconus “suspects.” Two previous large scientific peer-reviewed published studies have verified the accuracy and sensitivity of the inferior-superior (I-S) value in separating patients with keratoconus from normal controls. In one study by our group employing discriminant analysis using the I-S value alone in 200 normals and 281 keratoconus patients, we were able to distinguish the two groups with 95.8% accuracy.2 In an independent study by Maeda et al3 of 44 keratoconus patients and 132 non-keratoconus pathologies using discriminant analysis, they demonstrated that keratoconus could be distinguished from non-keratoconus pathologies with 96% sensitivity.

In the present study, the addition of vertical coma increased the accuracy by an additional 2.4% for a total accuracy of 98.2% using the product of the I-S value and vertical coma. This is an extremely small yet significant number. Such small differences were not easily demonstrated graphically particularly because they were computed on a logarithmic scale, which we noted several times in the publication. Throughout the publication we clearly stated that the computation of the quantitative measures were different, as such there was no attempt to misrepresent any data.1

Dr Boxer Wachler’s suggestion that I have used different criteria for defining keratoconus “suspect” is also incorrect. In this regard, I would like to refer him to our publication in the British Journal of Ophthalmology in 1996, which clearly states that when computing or interpreting the asymmetric bowtie skewed radial axes (AB/SRAX) pattern the upper and lower lobes used are the ones with the highest power. They do not need to be the same color on the topographic map.4

We are familiar with shortcomings of the I-S value and have published extensively on this. This is the main reason we came up with the KISA% index, which has a much higher degree of accuracy.2 Despite its shortcomings, the I-S value is still one of the most widely used and studied indices with regard to the “early” detection of keratoconus and which most clinicians to this day find very valuable. If you type in the words “I-S value keratoconus” while performing a MEDLINE search, 18 citations in the peer-reviewed literature appear. Its simplicity is what gives it popular appeal. Even without a sophisticated computer program it can easily be computed from a corneal topographic map in an accurate and reproducible way. It is also the only index that I am aware of for which there is a comparative database of approximately 200 normal controls, making it easy for a clinician to determine whether this index falls beyond two or three standard deviations of normal thus allowing him to confirm his suspicion of abnormal topography.4

What about the Lower-Upper (L-U) ratio? It seems to me that lower and upper are just different words for inferior and superior and that ratio is just another word for value. Also you will note that five data points are calculated above and below a meridian just as is the case with the I-S value. It appears that what Dr Boxer Wachler is describing is merely a variant of the I-S value. The problem with the way this metric is computed, however, is that for each eye you have to compute the steepest axis to determine the horizontal meridian—this is not easily done in an accurate and reproducible way either manually or with a computer program. Additionally, there is no published literature with normative values or data for this index to be compared, thus diminishing its value for use in the “early” detection of keratoconus. A MEDLINE search of the L-U ratio reveals one publication on this ratio by Dr Boxer Wachler himself. In this article,5 he reports that you can demonstrate more change in effect on the cornea after implanting INTACS (Addition Technology, Des Plaines, Ill) in patients with keratoconus. I do not need a metric to tell me whether INTACS has worked. My patients will tell me whether they have noticed any benefit from the surgery.

If Dr Boxer Wachler truly believes that his variation of the I-S value, the L-U ratio, is a better metric for the detection of keratoconus, I would encourage him to conduct a rigorous statistical analysis comparing these two indices in a cohort of keratoconus and normal control patients, as others have done, and publish it in the peer-reviewed scientific literature. Unfortunately, merely citing “unpublished data” makes it difficult to attach any credence to his claim.

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What About LASEK?
To the Editor:

I recently received a book commemorating the International Society of Refractive Surgery of the American Academy of Ophthalmology (ISRS/AAO) and global refractive surgery.1 I was surprised to see that no mention was made of laser epithelial keratomileusis (LASEK). I am well aware that photorefractive keratectomy (PRK) and LASIK are more common procedures, but I believe LASEK has the merit of having restored faith in surface refractive procedures.

Since 1998, when I first developed the procedure, I have treated thousands of patients with all degrees of myopia3 and hyperopia, with no significant side effects in either of these conditions. Over the years, I have read many interesting laboratory articles demonstrating the efficacy of the basal membrane in protecting the stroma from apoptosis and haze. I have also read articles that have tried to demonstrate the poor efficacy of the technique, but they only analyzed few cases or low myopia. Moreover, it seems the authors were interested in pain control, not haze control.

Laser epithelial keratomileusis is an easy technique, but it has its rules. Because many authors did not follow the recommendations provided, it is difficult to draw scientifically correct and reliable conclusions. However, the technique works well and many surgeons worldwide are satisfied with the high level of safety it offers.

I recently developed a new variation, epi-LASEK4 (LASEK with an epikeratome), and results in flap making are excellent. This technique has, in my opinion, overcome all drawbacks of LASEK (difficult flaps in some cases) and epi-LASIK (broken hinges or incomplete separation).

I hope the next time the world’s leading ophthalmologists write a new book about global refractive surgery, they will reserve at least a little space for a technique that has significantly improved my practice and the life of so many patients worldwide.

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REFERENCES