inated by amblyopia with very low visual acuity, surely this ceiling effect would not be present. Indeed, the ceiling effect strongly suggests that the A&SQ may perform better in samples of amblyopes with acuity much poorer than that of the sample we tested.

We agree with Drs. Kelderman, Felius, and Passchier that our modest sample size of 102 amblyopes limits the extent of any interpretation of the Rasch analysis that can be made from our results, and we highlighted that point in our paper. We also agree that the Rasch model is not a panacea, as no model is. It does, however, provide scientific measurement properties of data that are otherwise ordinal. The main problem with the A&SQ does not appear to be whether it is unidimensional, but whether it provides a valid measure of the quality of life of patients with strabismus and/or amblyopia. In the original Likert-scaled A&SQ, responses to all items (questions) were weighted equally. Unless all items are equally difficult or important, then simply adding them all up to get a final score does not provide a valid measure. This observation is particularly true of the A&SQ when the responses are arbitrarily given a score of 4 if they are unable to answer a particular question. Our Rasch analysis of the A&SQ highlighted how different some items were from others (see Fig. 2 in our paper), so that responses to item 7, “miss the other person’s hand when shaking hands,” are weighted very differently from those to item 21, “squint or shut one eye in bright sunlight,” for example, and the difference seems logical.

We also believe that the results from the A&SQ are probably not unidimensional. In their letter, Kelderman et al. criticize the validity of the Rasch analysis for assessing the dimensionality of the A&SQ and favor the use of traditional psychometric analysis (i.e., factor analysis). Comparisons between factor analysis results and Rasch analysis should be performed with caution, as the fundamental intent of each method differs. The principal component analysis identifies factors within a correlation matrix (i.e., factor structure underlying the items of the A&SQ), whereas the Rasch analysis determines whether there are other dimensions left once the initial latent trait (i.e., vision-related quality of life) has been extracted. Once subscales have been demonstrated to exist by factor analysis, they should be assessed for unidimensionality. For example, a questionnaire with several subscales (demonstrated using factor analysis) may be considered unidimensional (according to the Rasch analysis), since all the items in the questionnaire measure a single underlying trait. If both are not used, which one should be chosen? Factor analysis assumes that the data being analyzed are linear measures and not the ordinally labeled stochastic observations that are provided by the A&SQ. Studies that have compared factor analysis and the Rasch analysis have concluded that the Rasch analysis is much better at determining the identification of the core construct, particularly when the data are ordinal and factors correlate highly. In our report, not only do the results of the Rasch analysis strongly suggest that there is an important second dimension, but this dimension also includes a relatively large number of items (5 of a total of 23 items) and the inherent qualities of those items (mainly psychosocial) are all similar and seem logically different from most of the remainder, which assess difficulties with functional activities. Thus, we suggest that it may be more appropriate to provide two scores, since the breach of unidimensionality does not allow for appropriate summation of the items within the A&SQ. In turn, our proposed strategy may help to draw meaningful clinical conclusions about the consequences of living with strabismus or amblyopia or both.

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Citation: Invest Ophtalmol Vis Sci. 2010;51:6899–6900. doi:10.1167/iovs.10-6176

GEN Is Not HGN

I am deeply troubled by your publication of “Occurrence of Physiologic Gaze-Evoked Nystagmus at Small Angles of Gaze,” in the May issue. In multiple areas, it falls well below any acceptable standard of peer review and especially is not in keeping with the high standards that are typical of the papers published in this journal. The research and analysis are flawed and the conclusions are not supported by the references or evidence presented.

The authors make no reference to any accepted, peer-reviewed literature that supports the horizontal gaze nystagmus (HGN) test or its underlying principles. Numerous additional research papers and reports, extending back over 50 years, also could be cited.

The authors cite incorrect references in several instances or misstate what they are citing. For example, they list Leigh and Zee as the source of the “facts” stated in the second sentence of the paper: “Studies suggest that [gaze-evoked nystagmus] is present in over 50% of the normal population and is more common in fatigued subjects.” However, this information comes essentially from Abel et al. Also, fatigue nystagmus occurs with maintenance of lateral gaze for an extended time. It has nothing to do with sleep deprivation, as the term “fatigued subjects” suggests.

Other errors include the authors’ count of the subjects used in the research—they claim that data from 56 subjects were
analyzed, but their breakdown of ages totals 63—and the lack of a detailed description of Figure 1.

As to the research itself, the authors used an infrared eye-tracking system with an accuracy of no better than 0.5°, and that varies with gaze angle. Yet, they reported a single mean amplitude of nystagmus of 0.22° across all gaze angles. In any case, a measuring system should be at least twice as sensitive as the effect that one intends to measure. Consequently, one can have no confidence in any reported nystagmus with amplitude of 1° or less. In addition, the authors provide no information about the parameters of the targets that the subjects fixated.

With regard to data analysis, reporting of means and standard deviations implies that the data are normally distributed. The reported mean amplitude of nystagmus is an absolute value, and a negative amplitude has no meaning. Consequently, the SD 0.33° gives a 2.25D range (that should include approximately 2.5% of the sample) of +0.88° to −0.44°. Thus, the data are not normally distributed.

Finally, and most important, is the issue of the authors’ conclusion that their findings somehow relate to the HGN test conducted by police officers. The authors state that they did not visually observe the nystagmus that was being recorded. Yet the HGN test is one of visual observation. Even the most experienced clinician under ideal conditions can recognize an eye movement of no less than approximately 2 prism diopters, or approximately 1.1°. The only conclusion that the authors can make is that they measured small-amplitude physiologic nystagmus using a non-visualy observed method. This method has nothing to do with the HGN test.

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Citation: Invest Ophthalmol Vis Sci. 2010;51:6900–6901. doi:10.1167/iovs.10-5889

Author Response: GEN Is Not HGN

We are sorry that Dr. Citek is “deeply troubled” by the peer review of our paper. The review was conducted by two reviewers, both of whom took time to evaluate four drafts of the paper before the final acceptance.

The title of Dr. Citek’s letter “GEN Is Not HGN” suggests that his difficulties with our paper on physiologic gaze-evoked nystagmus had more to do with his involvement with the HGN test than with our paper itself. There is no physiological difference between horizontal gaze-evoked nystagmus and horizontal gaze nystagmus.

The erroneous number of subjects per decade appeared in the online prepublication version of the article and was corrected in the print version.

This study was performed specifically to evaluate the incidence of horizontal gaze nystagmus in normal subjects of different ages. We found that there was clear recordable nystagmus at smaller gaze angles, with amplitudes well within the recording limits of our system in a significant number of normal subjects. This finding suggested to the reviewers that a more detailed look at physiological gaze-evoked nystagmus at these angles would be important in judging the limitations of the HGN test. We concurred.

Dr. Citek comments that we made no reference to any “accepted peer-reviewed literature that supports the horizontal gaze nystagmus test.” We also did not reference any of the ample literature that documents the high false-positive rate of the test. Had we done either, the reviewers, we suspect, would have rightfully noted that these references should be removed, as they don’t have anything to do with the study itself.

We agree that the nystagmus amplitudes quoted in the paper are small compared with those in other studies. As noted in the paper, the method used to calculate the amplitude would result in smaller values than other methods. The program written to identify the beats of nystagmus looked at velocities of the fast and slow phases. The algorithm clipped the extremes of the movements, to get the best estimate of the velocities of both the slow phases and the quick phases. We did not write an additional program to best determine the true amplitudes of nystagmus beats, as it was not critical to the original purpose of the project.

Clearly, this article does not prove that the HGN test is not valuable. As Dr. Citek noted, we did not evaluate the incidence of visually detectable nystagmus at different gaze angles. Our findings suggest that such a study would be worthwhile.

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Citation: Invest Ophthalmol Vis Sci. 2010;51:6901. doi:10.1167/iovs.10-6537

Eu-estrogenemias and Retinal Blood Flow

Deschênes et al. reported remarkable news in their study, “Postmenopausal Hormone Therapy Increases Retinal Blood Flow and Protects the Retinal Nerve Fiber Layer,” which appeared in the May 2010 issue of the journal. For those women who are using hormone replacement therapy (HRT) for specific indications, such as vasomotor instability, this serendipitous benefit can provide additional reassurance that the holistic effect of HRT improves the quality of life for many women.