the use of the standards strictly to grade the degree of opalescence in the nucleus for research purposes. This grading system was not developed for, nor did we intend it to be used for, making clinical decisions regarding cataract surgery, and we thank Dr. Weinstein for pointing out this potential for misunderstanding.

We take this opportunity to correct two errors in Table 1. Nuclear grade O: should read “Less dense and less extensive. . . .” Nuclear grade 3: should read “. . . less dense and less extensive than standard photograph 4.”

The point that should be stressed is that we have developed a reproducible and a valid way of grading nuclear opalescence for use in epidemiological studies.

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Stereopsis in Strabismic Subjects

To the Editor:

Congratulations to Kitaoji and Toyama on their important work that seems to provide an additional functional (ie, medical, as opposed to aesthetic) rationale for the aggressive management of all forms of strabismus, regardless of patient age or other factors.1

I must protest, however, their use of the term “strabismic” in describing apparently otherwise normal people who have only esophorias and exophorias on the alternate cover test. Almost ½ of the individuals they studied were so classified (“latent strabismus”). If, in fact, we classify all individuals who have any type of a phoria as having (latent) strabismus, then 100% (or virtually 100%) of the entire population of the world has strabismus, either manifest or latent, since virtually everyone has a phoria of some sort, however small.

Strabismus, however, is not considered a normal physiologic condition. It is considered pathologic. It is defined, for example, as “deviation of the visual axes and by a disturbance of binocular vision”;2 “a pathological situation known as strabismus”;3 “a manifest deviation of the visual axes”.4 “There are two main classes, the class of latent deviations (heterophorias) and the class of manifest deviations (heterotropias). The manifest deviations are also known by the generic name of strabismus, or squint”.5


It was, therefore, disturbing that the subjects were 35 patients with “manifest strabismus” and 16 patients with “latent strabismus” (esophoria/exophoria), “demonstrated by the alternate cover test”. Only in “Materials and Methods, Experimental Subjects” is “manifest strabismus” separated from “latent strabismus.” These two groups are mixed without distinction for the remainder of the study and the paper, including statistical analysis and conclusions.

Nonstrabismic heterophoric subjects are, I believe, represented in Figure 2 by the solitary column of 16 patients at the left. Fifteen of 16 later demonstrated stereopsis on the Titmus fly test. Only four of 15 achieved normal stereoacuity of 40–60 seconds. Are 11 or 12 of these patients “monofixation syndrome?” The paper does not describe the precise ocular motility or fusion status of subjects. Did they have complete examinations or only an alternate cover test? This last test does not discriminate heterophoric normals from heterotropic abnormals (pathologic strabismus).

Four pages into the paper, one also finds that ⅛ of all subjects had amblyopia in one eye! Amblyopia markedly affects binocular vision and stereopsis.6,7 But the authors have not isolated these patients in any way, nor accommodated for this additional factor.
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Through the entire "Results," "Discussion," and "Conclusion," I found NO differentiation between presumably normal patients who have a "latent strabismus" (heterophoria), and those with "manifest strabismus" (heterotropia). Nor were patients with amblyopia separated in any way. It's not surprising that their major conclusion is that the smaller the angle the better the stereopsis, because their patient population is ½ normal people.

If the authors cannot tell me that I am wrong in my interpretation of their paper, then I hope they will rearrange and reexamine their data: Heterophoric normal persons should be placed in a control group separated from heterotropic (strabismic) subjects. Amblyopes with heterotropias should be placed in a separate experimental group since they are doubly handicapped.

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References


Reply

There are three major points in Dr. Romano's argument against our paper concluding that the strabismic subjects are able to have stereopsis, if the present squint angle is reduced to a sufficiently small degree by treatment, regardless of the past strabismic symptoms. The first point is the argument against the fact that the experimental subjects include those with latent strabismus (demonstrated by the alternate cover test) but probably with normal stereopsis in addition to those with manifest strabismus. This will lead to the erroneous conclusion that the strabismic subjects have normal stereopsis.

However, our point is that stereopsis persisted in many patients with manifest strabismus in the past, and which changed to latent strabismus with treatment. Our subjects with latent strabismus include only those whose strabismus had been originally (in the past) manifest, and which became latent and is still latent at present, but not those with original latent strabismus. Inclusion of the subjects with the type of latent strabismus as described above is essential to our analysis of the relevant factors for preservation of stereopsis.

The second point of the argument is concerned with our finding that only five of the 16 subjects with null present squint angle had fine position stereopsis demonstrable by the Titmus fly test (disparity, 40–60 seconds), which seems to be not easily acceptable to Dr. Romano, probably due to a misunderstanding of our research design. However, our interpretation of the results is quite straightforward: binocular fixation in the 11 subjects may not be precise enough to assure the fine position stereopsis, or alternatively, binocular fixation may be sufficiently accurate at present (but not in the past), but the fine position stereopsis may have been impaired by the past misalignment, as has been shown by many studies concerning visual cortical plasticity. At the moment it is impossible to decide which possibility is the case, since we have no way of assessing the performance of binocular fixation in the accuracy of stereopsis, which is measured in the order of seconds.

The last point is his complaint against the fact that about a quarter of our subjects had amblyopia, which could by itself affect stereopsis. We think this also comes from a misunderstanding of our research design. Animal experiments indicate that strabismus and amblyopia are theoretically inseparable. Of course, it would be interesting to know whether amblyopia associated with strabismus could affect stereopsis. This question was in fact answered in our paper by statistical analysis, which indicated that squint...