Letters to the Editor

Nuclear Lens Opacities and Visual Acuity

To the Editor:

I am writing to you concerning the article entitled “Use of Photographic Techniques to Grade Nuclear Cataracts” by Sheila K. West, Grank Rosenthal, Henry S. Newland, and Hugh R. Taylor that appeared in the January 1988 issue of *Investigative Ophthalmology and Visual Science*.

The study, performed by Dr. West and her co-workers, reports on the grading of nuclear lens opacities by clinical means, as well as with the use of a photo slit lamp, and a camera built on the “Scheimpflug” principle. In the section, *Materials and Methods*, as well as in Table 1, titled “Nuclear Opacity Grading Definitions,” descriptions of varying degrees of opacification are described as being “consistent with” a visual acuity in specific ranges. However, no data are provided to indicate what were the actual visual acuities in the 24 subjects who were studied.

The authors should be aware that cameras employing the Scheimpflug principle are now being marketed to ophthalmologists, some of whom are in turn using these instruments to document and justify cataract surgery in some of their patients. However, it is recognized that the appearance of lens opacities, particularly those associated with nuclear sclerosis, may be at variance with best corrected visual acuity. Most ophthalmologists have had patients whose nuclear opacities compare to “standard photograph 4” which appears in Figure 2 of the article by Dr. West and her colleagues, and who retain excellent visual acuity. Conversely, some patients with minimal nuclear changes, such as those illustrated in “standard photograph 1”, may show significant degradation of visual acuity.

Unless the authors of this report have data which support their assumption that nuclear opacities are consistent with given levels of visual acuity, they should clearly disavow this aspect of their study. Otherwise, they are likely to find themselves quoted by colleagues they never knew they had and wish they didn’t.

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Reply

To the Editor:

We appreciate Dr. Weinstein’s thoughtful comments on our article and are grateful for the opportunity to respond.

We wish to clarify the use of the standard photographs and definitions, which were developed explicitly for use in an epidemiological study to characterize the severity of lens opacities. The original standard photographs were selected from hundreds of photographs of nuclear opacities. The visual acuity associated with each of the standards was within the range specified by the definition for that particular standard. Visual acuity criteria had been used in earlier studies and we used it as a starting point in the selection of our standard photographs.

However, once the photographs had been selected, we moved beyond the consideration of visual acuity and characterized lens opacities by comparing them with the standard photographs. We recognize the well-known variations of visual acuity within the same degree of nuclear change, and we recommend...
the use of the standards strictly to grade the degree of
apalescence 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. Wein-
stein for pointing out this potential for misunder-
standing.

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 pho-
tograph 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) ratio-
nale 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 nor-
mal people who have only esophorias and exophorias
on the alternate cover test. Almost 1/3 of the individ-
uals they studied were so classified (“latent stra-
bismus”). If, in fact, we classify all individuals who
have any type of a phoria as having (latent) stra-
bismus, then 100% (or virtually 100%) of the entire
population of the world has strabismus, either mani-
fest 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 patho-
logical 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 ge-
neric name of strabismus, or squint”.5

My Stedman’s Medical Dictionary, Williams &
1345, describes strabismus as “a constant lack of par-
allelism of the visual axes of the eyes.” Dorland’s
1256, defines strabismus as a “deviation of the eye
which the patient cannot overcome”. Both definitions
prohibit heterophorias.

It was, therefore, disturbing that the subjects were
35 patients with “manifest strabismus” and 16 pa-
tients with “latent strabismus” (esophoria/exo-
phoria), “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 con-
clusions.

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 mo-
tility or fusion status of subjects. Did they have com-
plete examinations or only an alternate cover test?
This last test does not discriminate heterophoric nor-
malst from heterotropic abnormals (pathologic stra-
bismus).

Four pages into the paper, one also finds that ¼ 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 fac-
tor.