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In reply

We thank the authors for their comments and interest in our article.[1] This issue is not new. Commenting on evisceration more than 40 years ago, Wendell Hughes, MD, stated, "The possibility of the development of sympathetic ophthalmia has been a deterrent to the general acceptance of this procedure, in addition to the remote possibility of missing the diagnosis of an intraocular malignancy in a degenerated globe."[2]

We concur that evisceration is a highly successful and safe alternative to enucleation in most instances. Ophthalmic pathologists admittedly have a unique perspective of ophthalmic practice. One of the corresponding author's colleagues frequently reminds him that he "only sees the cases with complications, or the procedures that don't work." On the other hand, we have a vantage point that allows us to recognize trends that may not show up on the radar of individual practitioners.

It is well-known that blind, painful eyes can harbor unrecognized malignant neoplasms. Unfortunately, some of these eyes are being treated by evisceration. These cases continue to show up in our practices, and we feel obliged to report our findings.

Two of the correspondents comment about speculation and flimsy data. Regarding our purportedly flimsy data, it should be noted that 2 of our authors have seen 3 additional cases of eviscerated eyes with melanoma since our article was submitted for publication. In addition, several other cases were not included in our series because we had no access to clinical data or were unable to review the histopathology. Other cases were excluded because there were complicating factors such as prior tube shunt placement. In one instance, melanoma was found in an evisceration specimen from a 36-year-old man whose blind, painful eye was eviscerated after unsuccessful tube shunt surgery for unilateral glaucoma. He developed massive orbital recurrence of an epithelioid melanoma that required exenteration. There is no question that he was harmed by evisceration. Another case of unsuspected melanoma was excluded because the eye was enucleated. The unsuspecting surgeon then harvested the patient's sclera and used it to wrap the implant.

The consequences of inadvertently eviscerating an eye with a previously undiagnosed intraocular tumor are not known. Theoretically, evisceration might not substantially increase the risk for death from metastatic melanoma because it is now believed that many melanomas have already spawned micrometastases before they become symptomatic and are treated. However, it is not unreasonable to expect that the surgeon would still be blamed for an untoward outcome whether she or he were truly responsible or not. Failure to diagnose is a major cause of malpractice litigation. Our article is intended to benefit the ophthalmologist as well as the patient when considering evisceration of a blind, painful eye.

Dr Brown seems to imply that competent, caring ophthalmologists somehow should be absolved of blame if their goal was the palliation of pain or suffering. Unfortunately, the best of intentions generally count for naught in the current medicolegal climate of res ipsa loquitur.

Regarding phthisical eyes with a remote history of trauma, Brown states that one does not "expect to catch an alligator in a barrel of fish." Unfortunately, this is not always the case. Pleomorphic adenocarcinoma of the nonpigmented ciliary epithelium classically occurs in chronically blind, phthisical eyes.[3] The corresponding author recently has seen 2 of these rare malignant tumors in consultation. One was found in a severely phthisical eye with osseous metaplasia that was enucleated 65 years after a knife injury. That case developed massive orbital invasion necessitating exenteration.

We are uncertain how Perry and colleagues concluded that most the tumors in our series were small lesions that would be expected to have a good prognosis. The tumor in case 1 fills half of the globe (Figure B) and is greater than 15 mm in diameter, and a 15-mm fragment of tumor was found histopathologically in case 4, which had a 27-mm diameter orbital mass. Large melanomas are defined as greater than 15 mm in diameter.

We concur with Perry and colleagues' suggestion that "eyes with opaque media should undergo B scan ultrasonography prior to evisceration and that, if the diagnosis is unclear, enucleation should be performed rather than evisceration." Perry et al also convey that at least 4 of the patients we described received substandard care. We agree with this, and continue to see similar cases, thus indicating that our article is warranted.

We stress that the presence of a malignant intraocular neoplasm should be excluded prior to evisceration of any blind or blind and painful eye, particularly with opaque media, as was conveyed in the letters by Perry et al and Rosner. Necrosis-related inflammation can confound clinical diagnosis of occult lesions, as can failure of necrotic tumors to enhance on imaging studies. Evisceration has many proponents, and we agree that it is a safe alternative to enucleation in most cases, if the presence of an occult tumor is excluded by careful preoperative evaluation.

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Response to Editorial About the Convergence Insufficiency Treatment Trial

We thank Dr Wallace for his interest in our article[1] and for pointing out its many strengths including ophthalmology and optometry involvement, randomization, successful masking, and out-

standing (99%) follow-up. While noting that office-based vergence/accommodative therapy was found to be more effective than office-based placebo therapy, home-based pencil push-up therapy (PPT), and home-based computerized therapy plus PPT (computerized therapy), Dr Wallace[2] suggested that the 2 home-based groups should have received the same amount of therapy at home that the office-based groups received in the office, as well as equal therapist contact time.

In response, we would like to emphasize that this was not the intent of the trial.

As stated in our article,[1] treatments for convergence insufficiency differ in several ways including dosage and mode of administration, and the objective of our trial was to compare the effectiveness of 3 commonly prescribed treatments. Effectiveness refers to whether an intervention has benefit as used in clinical practice. In contrast, treatment efficacy denotes whether an intervention is successful when properly implemented under highly controlled conditions. Thus, the home-based PPT and computerized therapy regimens were modeled after those used in clinical practice, ie, 15 to 20 minutes 5 days per week (patients also had weekly phone calls and monthly office visits).

To equalize treatment dosage and face-to-face office contact time with the therapist, children in the home-based groups would also have had to attend 12 weekly, 60-minute, therapist-supervised office therapy sessions. While this protocol might indeed have greater efficacy than the prescribed 15 to 20 minutes of home-based therapy 5 days per week, it is unlikely that weekly 60-minute in-office therapy sessions of therapist-supervised PPT or computerized therapy would be prescribed. Alternatively, an additional 12 minutes of PPT or 7 minutes of computerized therapy each day would equalize the prescribed dosage; however, it would not equalize face-to-face therapist contact time (weekly 60-minute in-office interactions between the child and therapist would be required). We feel these hypothetical treatment approaches are untenable, and unlikely to be prescribed or successfully completed. More importantly, they do not represent clinical practice. Thus, while it may be of scientific interest to equalize therapy dosage and face-to-face contact time, it would have limited clinical utility because it precludes us from evaluating effectiveness. Moreover, equalizing in-office therapist contact time would negate the primary advantages of home treatment: simplicity and low cost.

This brings us to another point. We chose the treatment interventions based on the current practice patterns of members of the American Academy of Ophthalmology, American Optometric Association, and pediatric ophthalmology members of the Pediatric Eye Disease Investigator Group. Because the most commonly prescribed treatment for children with convergence insufficiency is home-based PPT,[3,4] it was necessary to include this as a treatment arm. We also included home-based computerized therapy, recognizing the recently growing trend of prescribing more intensive stepwise home-based therapy using computer software.

Dr Wallace's mention of his experience in treating children with convergence insufficiency reminds us that evidence-based eye care, at times, has and will continue to contradict personal clinical experience and consensus expert opinion.[5-9] We are confident that he agrees that challenges to clinical impressions and prevailing wisdom are necessary endeavors and, in fact, are part of our responsibilities as scientists, researchers, and clinicians.

Lastly, we are aware that incorporating new treatments within a clinical practice is often difficult. We concur that the cost of treatment is important and maintain that it is incumbent on all of us to educate parents regarding the success rates and advantages and disadvantages of all available treatments. This enables parents to give truly informed consent for one treatment over another based on the information provided, their perception regarding their child's symptoms, their personal goals and values, and their financial situation. Ultimately, we all strive for treatments that lead to better care for our patients.

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In reply

The authors emphasize that treatment arms had different dosages and therapist contact times because their study was designed as an effectiveness trial to compare treatments as they are used in clinical practice. I understand this reasoning, but if I am going to recommend to my patients a more expensive and time-consuming treatment as first-line therapy, I want to be confident that there is something about the therapy itself (aside from its dosage or other variables) that sets it apart from less expensive home-based options. My un-

certainty about the superiority of office-based treatment persists because office-based and home-based groups differed in at least 3 important ways: the mode of treatment received, the duration of treatment received, and the amount of therapist contact time.[1] Therefore, we cannot know the relative contribution of each of these factors to the reported improvement in symptom survey scores.

I acknowledge that it would have been difficult to design a practical protocol with equal therapist contact time in each arm. However, I see no reason why treatment dosage could not have been equalized, thus removing this important variable from any discussion of why one treatment may have worked better than another. Treatment was prescribed 135 min/wk for the office-based group, 100 min/wk for the home-based computer group, and 75 min/wk for the home-based pencil push-ups group.[1] It would be interesting to know whether office-based treatment is superior to equally intensive home-based therapies.

My editorial's reference to my experience is not intended, of course, to suggest that my clinical impression should trump randomized trial data. By contrast, it is to provide one clinician's perspective on convergence insufficiency and on how the results of this study will change my practice. In the end, I think that office-based therapy may be worth its additional cost for those patients who do not achieve adequate benefit from less expensive home-based treatments. There may be something about office-based treatment that is intrinsically better than home-based therapies. Alternatively, its effectiveness may be due in large part to the positive reinforcement provided in the office that encourages patients to stick with treatment routines.

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The Steroid Controversy in Bacterial Keratitis

The January 2009 Controversies series of articles highlights a major debate.[1,2] Some corneal specialists use adjunctive corticosteroids to reduce scarring in bacterial keratitis. Others fear that suppressing the immune system is the worst thing you can do. Hindman and Patel et al[1] state that only a single, underpowered trial has addressed this and suggest that a large definitive trial is necessary to end the debate. By the time this Controversies series was published, there had actually been 2 published trials,[3,4] and a third, large trial is well under way.

We recently published the results of a randomized trial of 42 patients that studied the effect of adjunctive topical steroids in bacterial keratitis.[4] Although steroids affected neither acuity nor scar size, they did significantly delay reepithelialization. Dr Cohen[2] discontinued her trial because of a poor outcome in a case of *Pseudomonas* infection. We too had adverse events—all 4 were in the placebo arm. Although this is the largest study published to date, it was clearly underpowered to put the controversy to rest.

We are now in the midst of the Steroids for Corneal Ulcers Trial, which addresses this very question (NEI U10-EY015114). As of June 2009, we have enrolled 417 of 500 cases (including many ulcers caused by *Pseudomonas* bacteria) at the University of California, San Francisco, Dartmouth Medical Center, and in India, with by far the greatest number from the Aravind Eye Care system. With standardized treatment and assessments, we hope our study will provide definitive evidence to guide clinicians through this controversy. In the meantime, doubts remain. When we announced our trial at a national meeting, one researcher told us that our steroid regimen was timid; another told us we had better have a good lawyer.

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