

## CORRESPONDENCE

### To the Editor: Treatment of Convergence Insufficiency in Childhood: A Current Perspective

I read with great interest the article, *Treatment of Convergence Insufficiency in Childhood: A Current Perspective*.<sup>1</sup> As a site principal investigator in the most recent Convergence Insufficiency Treatment Trial (CITT) study, I think the article is aptly titled because the same data can certainly be evaluated differently; thus, this is truly an issue of perspective.

A plethora of issues surround the disorder of convergence insufficiency. The current studies do not answer many of them. We do not have a true understanding of the epidemiology of this problem. For example, the studies did not look at the incidence of symptomatic convergence insufficiency (CI) within the universe of those with clinical signs. The authors have side stepped this non-trivial issue that permeates the interpretation of the CITT data. Why do some with signs have no symptoms? Why do some with symptoms have no signs?

For example, the CI symptom survey (CISS) has not been specifically tested in a group of poor readers or those with a specific learning disability. Given the possibility that poor reading could lead to diminished near task skills or certainly account for the symptoms on the survey, this immediately raises further questions. This is reinforced by the data reported that the “placebo” treatment improved symptoms despite not proportionally improving signs, and the computerized home treatment improves signs without improving symptoms in proportion. The obvious question then becomes, “why?” Further, if the CISS is given to a random population, more children who are normal will score over the 16 cutoff than will that have CI.

The foregoing leads to the concern that the placebo (motivational?) group did not compare “apples to apples.” The in-office group being treated with vergence and accommodative intervention also had true home reinforcement. The placebo group did not. One could argue

looking at the data that the improvement in the placebo group, had it been combined with that of true home treatment, would have resulted in a synergy about equal to the office therapy. This leap, although plausible, needed to be demonstrated or disproven. Otherwise the question of whether the positive reinforcement of the therapist (termed placebo in the study) was simply added to the extra time training and not related to any other in office factor. Further, positive reinforcement is a more powerful tool when the patient is also getting true reinforcement. All this would have significant implications as to the conclusions of the studies.

The scientific question of what caused the difference between the groups is not unimportant. In fact, clinical behavior in practice could have been easily affected if the time in true treatment had been equalized. Not including phone calls or review, but rather time spent training, it would be of interest to see the results at the same time points. If the data were analyzed at the 12-week mark for the home groups and the equivalent time point of training for the office group, the results would be less compelling. Would more weeks of home therapy, equalizing total time training, have ended up with similar results on the CISS? The entire effect shown in these reports could be due to the large differences spent in time training alone.

Compliance with therapy cannot be underestimated. There is little doubt that working with a trainer might make a difference in performing a repetitive task. Even the increased cost of in-office treatment might ensure that parents are enforcing home treatment to get their “money’s worth.” With a limited health care dollar, we ought to be able to answer the above and many other questions. Some of these include the role of simple motivation in CI, the effect of increased reading alone, better validation of the CISS with reading problems, time per session combined with duration of treatment, whether office treatment adds more than motivation combined with increased time of

treatment. Other questions were well enumerated at the conclusion of the author’s article. These questions present the opportunity to design a cost-effective best treatment with the elements known to equate with success.

Until then it still seems reasonable to outline to patients and families that there are various approaches to the treatment of CI. Families should know that the office based treatment—as restrictively studied—ended up with more successes. They should also be told about the problems with the study and the aspects not studied. The choices of treatment in the study represented consensus compromises and not the actual techniques used in a practitioners office necessarily. Currently, many expensive treatments are not covered in medicine and the luxury of an office trainer may find itself in the same position. But, I agree with the authors that the family should have that choice, once truly informed.

I would like to congratulate the CITT group on some very important points. Perspective is a difficult concept to wrap one’s mind around. The effort of the group to use careful science as the lens with which to view this multifactorial and complex issue should be applauded. Moreover, the effort to use terminology that is carefully descriptive and thus not subject to confusion, misuse, or bias when describing the in-office treatment represents a milestone for both the fields of ophthalmology and optometry. Finally, the author’s have carefully not extrapolated success in a limited application to a general one. I hope we all are careful to follow their lead.

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### REFERENCE

1. Scheiman M, Rouse M, Kulp MT, Cotter S, Hertle R, Mitchell GL. Treatment of convergence insufficiency in childhood: a current perspective. *Optom Vis Sci* 2009;86:420–8.