Treatment of Horizontal Nystagmus Using Myectomy Without Reattachment

We read with great interest the article by Lingua et al. titled, “Myectomy of the Extraocular Muscles Without Reattachment as a Surgical Treatment for Horizontal Nystagmus” in the May/June 2016 issue. As specialists with many years of research, we applaud the underlying creative and innovative thinking of the authors but have significant concerns with this report.

1. The introduction ignores a large body of literature on the medical and surgical treatment of nystagmus, and thus may well mislead the reader.

2. The diagnosis used in this report of “horizontal nystagmus” is extremely broad because this could refer to any combination of neurological, neuro-ophthalmological, and ocular motor conditions. There is a clear lack of precision in the terminology, resulting in inconsistently used, overlapping, and poorly defined terms and phrases, such as “vision,” “null point,” and “contort their head.”

3. We wonder why the authors included a figure in the discussion illustrating a previous procedure performed in 4 patients, 2 of whom had significant complications.

4. The hypothesis and rationale for employing two different procedures (eg, Sinskey anterior extirpation procedure [SAEP] and myectomy without reattachment [MWR]) are unexplained and remain unclear. We can only assume that the mechanical purpose of these techniques was to impair and limit extraocular muscle function regardless of the underlying diagnosis or patient characteristics.

5. The description of the SAEP is unclear, seems to be fluid, and its changes are based on the evolving judgement of one surgeon. There are no data to support the changes. No single procedure has been evaluated for its impact on a defined, consistent ocular motor diagnosis.

6. The purpose of the MWR was to reduce complications from the SAEP. The MWR procedure seems to be inconsistently indicated and performed throughout the report (eg, patients older than 10 years, with different acuities, had different procedures). Why was this done? This type of intra-study change of procedure/protocol is unusual in modern interventional research.

7. The outcome measure of visual acuity has no defined protocol. In addition, the authors’ overall report of visual acuity improvement after these procedures was not different from those reported in previous studies cited by the authors.

8. The authors were unsuccessful in using eye movement recordings as an outcome measure, now a standard for measuring nystagmus as part of clinical and basic science research. The individual eye movement outcome measure (eg, amplitude) is a poor measure of nystagmus in most patients with the infantile forms and does not correlate with visual function (although overall intensity is helpful in other forms of eye movement disorders such as acquired pendular nystagmus).

9. The other significant outcome measure was clinical evaluation of ocular motility (eg, primary position deviation and ductions/versions). The use of the common qualitative, clinical, nominal, -4 to +4 duction scale is poorly amenable to statistical (non-parametric) analysis and is plagued with subjective bias.

10. The authors indicated that 20% to 25% of patients needed more eye muscle surgery. There are no data on any of these patients. What were their symptoms and oculomotor findings? How was another procedure done in light of the magnitude of the previous surgery? What was the functional and structural outcome of these procedures? There were no other complications other than alignment changes discussed.

REFERENCE


Richard W. Hertle, MD
Louis F. DellOsso, PhD
David Granet, MD
Larry A. Abel, PhD
Jonathan B. Jacobs, PhD
Akron, Ohio

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Reply

I would like to thank the authors for their comprehensive letter. The article “Myectomy of the Extraocular Muscles Without Reattachment as a Surgical Treatment for Horizontal Nystagmus” was not meant to be a comprehensive review of the literature and therefore included only references I judged to be relevant to the clinical surgical community. It was my intent to provide a clinical demonstration of treatment effect for an innovative surgical approach to the treatment of nystagmus.

We employed a commercially available nystagmograph (Interacoustics Infrared Videonystagmography VO425; Interacoustics A/S, Middelfart, Denmark) used primarily in the evaluation of vestibular function in patients with dizziness and balance problems. It provided us with a measure of nystagmus amplitude otherwise unavailable in the absence of a nystagmus laboratory. We performed a reliability study of this instrument with the assistance of our statisticians. There is an enormous bank of preoperative and postoperative data on our patients documenting the horizontal and vertical nystagmus in central gaze and 30 degrees up, down, right, and left yet to be analyzed. Additionally, waveforms are on record for pursuit and saccade movements. In the future, we look forward to providing the degree of waveform analysis anticipated by the authors in their letter. Going forward, it is my hope that the authors will also undertake an investigation of this procedure with the resources of their nystagmus laboratory.

Figure 1 in our article was included not to demonstrate the prior technique (Sinskey anterior extirpation procedure [SAEP]) but to clarify the anatomic junction referred to as the capsuloseptal adhesion in the article. That photograph was taken from a video of the myectomy without reattachment (MWR) procedure (which was recently shown at the annual meeting of the American Academy of Ophthalmology in Chicago). We again emphasize that a key purpose of the article was to point out the shortcomings of the SAEP and to discourage future readers and surgeons from resurrecting the enucleation snare and snip method as reported by Sinskey, as is stated in the Discussion section of our article. It was the primary aim of our work to define the optimal amount of myectomy that can be performed without risk of orbital bleeding and the limitation of versions noted by Sinskey, but nonetheless achieve improved amplitude reduction.

The procedure was indeed fluid, due to the varying presentation of individual patients. What remains constant for each patient who had MWR is the consistent technique of muscle removal on the horizontal recti, but of a varying number of millimeters based on prior surgery and available muscle at exploration. The length of muscle measured from the insertion to the dense capsuloseptal adhesion varies from patient to patient. Whether the amount of myectomy affected outcomes is the subject of a recently completed 40-member cohort (unpublished data). The addition of additional muscles due to concurrent strabismus, with or without oblique dysfunction, required the composite procedure to change based on clinical presentation rather than on type of nystagmus. For these patients, it was clinically inappropriate to standardize the surgical procedure to one type and ignore the individual aspects of each patient, given the variability in their anomalous head postures, concurrent strabismus, and prior surgeries. We reported on the clinical observations noted when applying this procedure across a varied population.

I believe all anomalous head postures derive from a vision-limiting nystagmus in primary position and, if primary position can be still, then all anomalous head posture can be treated equally when we make primary position the patients’ quietest gaze position. For that reason, the MWR was applied regardless of the horizontal nystagmus waveform because it was our intent to ascertain its impact on all forms of horizontal nystagmus and anomalous head postures.

Regarding visual acuity results in Table 2, we referenced the “best binocular optically corrected acuity” in the footnote. The best binocular optically corrected acuity was described by Hertle et al. Objective assessment of versions is inarguably imprecise and open to subjective bias. I have been unable to identify a more objectively precise clinical method.

I propose that it is of greater benefit to the patient to quiet the eyes in central gaze (given the social implications of nystagmus and the anomalous head posture) in exchange for a modest reduction (-2 or less) in rotation. We hope to evaluate the application of a nystagmus-specific health-related quality-of-life survey in our next group of patients, which will provide illumination on this issue.

Finally, it is important to note that the patients discussed in our publication were operated on prior...
to July 2015. Subsequently, we revised the operative technique, and applied it to a different group of 40 patients operated on after July 2015 (as initially reported in July 2016\textsuperscript{7}). We hope to publish these results in the future.

REFERENCES

Robert W. Lingua, MD
Irvine, California

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