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Should We Require Evidence About the Etiology of A-Pattern Strabismus?

Joseph L Demer, M.D., Ph.D.

Department of Ophthalmology and Jules Stein Eye Institute, Department of Neurology, Neuroscience and Bioengineering Interdepartmental Programs, University of California Los Angeles, USA

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In this issue of the *Journal*, Donahue and Itharat¹ suggest that A-pattern strabismus with overdepression in adduction may be a bilateral skew deviation. Is this novel proposition credible?

Donahue and Itharat report 13 patients with A-pattern strabismus averaging 27Δ, and at least +2 overdepression in adduction, equating this to “superior oblique overaction.” In 11 of the 13 patients, vertical binocular alignment did not change during head tilt, and 12 had some unspecified degree of bilateral fundus incyclotorsion. Most strabismologists recognize such patients.

The authors then speculate that central otolith-ocular reflex pathology causes the A patterns. Recall that each labyrinth has two otolith organs—the utricle and the saccule—that sense linear acceleration including gravity. Donahue and Itharat speculate that their patients have bilateral lesions of otolith ocular reflex connections corresponding to the anterior semicircular canal pathways. They postulate a bilateral brain lesion diminishing input from the otolith equivalent of the anterior semicircular canal, allowing dominance of analogous input from the otolith equivalent of the posterior semicircular canal. This putatively activates the inferior rectus and superior oblique muscles and inhibits the inferior oblique and superior rectus muscles. Donahue and Itharat propose that this vestibular drive for conjugate deorsumversion does not “sunset” the eyes because visual “fixation” input overrides it. Innervation to the superior oblique muscles putatively remains maximal, and the inferior oblique is putatively inhibited. The “tonus” hypothesis requires that these oblique commands generate maximum possible intorsion, rendering the obliques unresponsive to supranuclear input from “fixation” to cancel the intorsion, and from otolith input to explain the negative head tilt test. What is the

Address for correspondence and reprints: Joseph L Demer M.D., Ph.D., Jules Stein Eye Institute, UCLA, 100 Stein Plaza, Los Angeles, CA 90095-7002, Phone: 310-825-5931, Fax: 310-2067826, jld@ucla.edu.

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intellectual attraction to this intricate, speculative proposition? The motivation seems to be strabismologists' traditional thinking about oblique muscles.

Strabismologists have historically *defined* oblique muscle function by observable version patterns.² The study entry required overdepression in adduction, which Donahue and Itharat conventionally term “superior oblique overaction.” Donahue and Itharat infer that in “primary superior oblique overaction,” the superior oblique overcontracts in response to all supranuclear inputs, so that the head tilt response from the otoliths should also be enhanced. Since the head-tilt response was not enhanced with the A patterns, Donahue and Itharat alternatively suggest a skew deviation.

Donahue and Itharat's theory has fundamental problems. While semicircular canal–ocular reflexes evoke vigorous eye movements compensating for dynamic head rotations,³ otolith–ocular reflexes are weak, particularly the tonic responses.⁴ Canal–ocular reflexes compensate nearly perfectly for head rotations³; otolith–ocular reflexes are vestigial, compensating for only a tiny fraction of geometric requirements.⁵ Furthermore, the otolith–ocular reflex depends profoundly in direction and magnitude on visual target location and distance.⁴ Even immediately after unilateral labyrinthectomy, a maximally asymmetric otolith lesion, patients rarely report vertical or torsional diplopia, and the resulting small heterotropia resolves in days.⁶

Donahue and Itharat's proposed equivalence of central processing of semicircular canal and otolith inputs is scientifically unsupported. Major known differences abound, but most otolith processing is unknown in specifics. Unlike uniplanar semicircular canals, otoliths sense motion in numerous directions; otolithic pathways are highly decussated, resulting in a substantial symmetry of the reflex despite asymmetrical peripheral lesions. Otolith mediated ocular responses to tonic inputs such as gravity differ from transient inputs such as linear translation. Neuroscience does not support an otolith origin of A patterns, and Donahue and Itharat provide no direct evidence of vestibular dysfunction in their patients.

Moreover, the otolith theory of A patterns is biomechanically impossible. The proposal hinges on intorsion from maximal superior oblique activation. But, the authors also propose maximum inferior rectus activation, evidently not considering that the extorting effect of the inferior rectus would cancel the intorsion of the superior oblique. Correspondingly, inferior oblique inhibition might facilitate intorsion, but that would be canceled by extorsion allowed by superior rectus inhibition. Furthermore, simultaneous activation of both the superior oblique and inferior rectus during superior rectus and inferior oblique inhibition would cancel any A pattern. I quantified these complicated mechanical phenomena by computational modeling using the Orbit 1.8 Gaze Mechanics Simulation.⁷ Increasing superior oblique strength by 15-fold can produce a 27^Δ A pattern, and a striking alternating hypertropia, but only a barely detectable 5° of incyclotorsion in each eye. Simulation of symmetric 50% superior rectus and inferior oblique weakness, along with symmetric 10-fold inferior rectus and superior oblique overcontraction, does not produce an A pattern or any torsion.

So what might actually cause A-patterns? Rectus pulley heterotopy can. Computer simulation shows that heterotopy, corresponding to incyclorotation of the entire rectus pulley array, produces marked A pattern, incyclotorsion, and alternating hypertropia.⁸ This obvious mechanical cause of A pattern, not excluded by orbital imaging Donahue and Itharat's study, seems particularly likely in their patients who had spina bifida, hydrocephalus, and other conditions associated with plagiocephaly and orbital dystopia.

While evidence does not support skew as a cause of A-pattern strabismus, and serious theoretical objections exist, the current paper highlights hazards of common clinical reasoning. The practice of making conclusions about oblique muscle function from clinical versions is

risky.² Overdepression or overelevation in adduction do not reliably imply superior oblique or inferior oblique hypercontractility, respectively. These may be possibilities, but, as we now know from orbital imaging, rectus pulley heterotopy is a common alternative mechanism that can be ruled in or out by direct clinical evidence. No similar evidence can test alternative theories of “primary oblique dysfunction” or central otolith imbalance, making these theories scientifically useless. Useful theories of strabismus pathogenesis must involve objectively testable phenomena, not chains of abstraction. Strabismologists should familiarize themselves with modern understanding of extraocular muscle function and neural control.⁹ Strabismology should embrace and extend the application of rigorous science and be willing to discard scientifically implausible concepts.

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