Neurosurgical Forum LETTERS TO THE EDITOR

Preoperative third ventricle shape and ETV

TO THE EDITOR: We read with interest the recent article on endoscopic third ventriculostomy (ETV) failure by Gianaris et al.³ (Gianaris TJ, Nazar R, Middlebrook E, et al: Failure of ETV in patients with the highest ETV success scores. *J Neurosurg Pediatr* 20:225–231, September 2017). This article adds important information regarding further predictors of ETV success in patients with high ETV success scores (ETVSSs). We would like to thank the authors for their significant work on this important issue. The study demonstrated that signs of lethargy and transependymal flow were correlated with success and that patients without these features may be at greater risk of ETV failure despite a high ETVSS (80 or 90). We are concerned, however, that these conclusions may preclude ETV in some patients with hydrocephalus who might be suitable candidates for this treatment.

In their study, Gianaris et al. retrospectively analyzed the clinical course and radiological parameters of 59 patients with ETVSSs of 80 or 90. They propose that the existence of transependymal flow may represent the result of elevated intraventricular pressure in patients with normal brain parenchymal compliance and that resistance forces from brain parenchyma with normal elastic properties can force fluid through the stoma created by the ETV, resulting in treatment success. However, they did not mention the compliance of the third ventricle itself or its preoperative morphology. In our institutional experience, we have found that a high rate of ETV success can be achieved in patients with preoperative bowing of the third ventricle floor—even in the absence of signs of acute intracranial pressure (ICP) elevation, such as lethargy and transependymal flow. Previous studies have also demonstrated an association between preoperative third ventricle bowing and higher ETV success rates. Foroughi et al.² reported an ETV success rate of 96% in patients with preoperative lamina terminalis and third ventricle floor bowing. Dlouhy et al.¹ also found that preoperative third ventricle bowing was a good predictor of ETV success, and they reported that patients with third ventricle bowing had a 3-fold likelihood of success compared with those without such bowing. The existence of third ventricle bowing, especially third ventricle floor bowing, suggests compliant third ventricle walls and a pressure difference between the

third ventricle and the subarachnoid space, which indicate an intraventricular obstructive hydrocephalus and can result in higher ETV success.¹ We believe that in patients with chronic hydrocephalus, even if without signs of acutely elevated ICP and transependymal flow, preoperative third ventricle floor bowing can still reflect good compliance of the third ventricle wall and a pressure difference between the ventricle and the subarachnoid space, and these patients therefore can still be treated successfully with ETV.

In our institution, the preoperative morphology of the third ventricle is an important consideration in selecting candidates for ETV. Currently, an institutional study is underway in our hospital on predictors of ETV success. We believe that analysis of factors that predict ETV success in patients with the highest ETVSSs should also consider preoperative third ventricle morphology, and this variable will be included in that study.

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Disclosures

The authors report no conflict of interest.

Response

We greatly appreciate the insight provided in the response to our article regarding “failure” of ETV in those patients with the highest ETVSSs. In our article, we hoped to study both clinical and radiographic factors that would predict either success or failure in patients who, theoretically, should have the highest chance of success.

Dr. Wang and coauthors highlight the idea that “third ventricle bowing” is a predictor of ETV success and state “bowing can still reflect good compliance of the third ventricle wall and a pressure difference between the ventricle and the subarachnoid space.” We believe our data support this idea. Further, we believe that this pressure difference is the critical factor in ETV success. In our paper, factors that showed higher intraventricular pressure (such as the clinical presence of lethargy or the radiographic appearance of transependymal flow) did predict success. Conversely, we speculate that this lack of a pressure differential may explain the lack of efficacy of ETV without choroid plexus cauterization in newborns, who have soft brains and pliable skulls.

While we did not record “bowing” in quite the manner that Dr. Wang and coauthors mention, we did record the maximum width of the third ventricle both before and after surgery. While an absolute reduction of the third ventricle width did not correlate with outcome, the postoperative width did. We used stepwise variable selection, maximum likelihood estimates, and logistic model fitting for statistical analysis. This method generated odds ratios. For the third ventricle width, the postoperative maximum width did correlate with outcome. For every 1-mm increase in the size of the third ventricle, the chance of “failure” increased by 12.8%. It is reasonable to assume, as Dr. Wang and colleagues suggest, that in patients whose third ventricle walls have normal compliance properties, the third ventricle will be smaller after ETV. Patients with abnormal compliance may continue to have an enlarged third ventricle and thus a higher risk of failure.

Therefore, we believe our findings agree with Dr. Wang and his coauthors’ assertions. We further believe that factors (clinical, anatomical, or radiological) that correlate with a differential in pressure between the ventricles and the subarachnoid space will predict success. We look forward to the results of their study, hope that both our studies encourage collaboration of data, and thank them for their thoughtful and collegial letter.

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Role of dysplastic and genetic mutations during the formation of cerebral aneurysms in infants

TO THE EDITOR: We had the pleasure of reading the article by Lyon et al.¹ in which they discuss the case of a 5-week-old female infant who presented with a large, ruptured middle cerebral artery (MCA) bifurcation aneurysm

that was treated by coiling with a good outcome (Lyon KA, Arrey EN, Haider AS, et al: Endovascular treatment of a large ruptured middle cerebral artery bifurcation aneurysm in a 5-week-old infant: case report. *J Neurosurg Pediatr* 20:357–363, October 2017). They reviewed 26 cases of saccular aneurysms treated in infants < 3 months of age, showing that the MCA was the most common location for such lesions in this age group. The authors comment that although connective tissue disorders are commonly mentioned as predisposing factors for cerebral aneurysms in infants, the specific genetic factors predisposing to this disease remain unclear.

Our group recently published a case of a 6-month-old infant presenting with a large left MCA aneurysm that underwent clipping, but the child developed a new aneurysm at an adjacent segment of the dysplastic MCA, and a second rupture occurred.³ Vessel occlusion with Onyx was performed and the effect was durable. Although the aneurysms in this case were not clearly due to a connective tissue disorder, genetic testing uncovered a mutation in myosin heavy chain 11 (*MYH11*), a contractile protein of smooth muscle cells.⁴ *MYH11* mutation has been associated with thoracic aortic aneurysms and patent ductus arteriosus due to disruption of smooth muscle cells.^{2,5} We subsequently reviewed 189 previous cases of pediatric cerebral aneurysms.

We commend Lyon et al. on their treatment of a challenging case in an infant. Our results suggest that occult genetic mutations may play an important role in the pathophysiology of aneurysms in young children. Formal genetic evaluation of infants with saccular aneurysms, possibly including whole-exome sequencing, may offer more insight into the underlying mechanisms and risk factors in this patient population. In addition, the dysplastic nature of cerebral aneurysms in infants suggests that continued close follow-up, regardless of treatment modality, is likely warranted.

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Disclosures

The authors report no conflict of interest.

Response

We thank Karsy and colleagues for their interesting comments on our paper. Since their publication¹ focused on a 6-month-old infant, it was excluded from our analysis. It is unclear whether cerebral aneurysm formation and rupture in this older age group is etiologically different from that in infants younger than 3 months of age.

We agree that genetic evaluation and testing should be pursued in all infants harboring an intracranial aneurysm. Our patient underwent a formal genetic evaluation, but no genetic abnormalities were identified. Of note, whole-

genome sequencing was not undertaken. We also agree that these children need to be carefully followed up since further cerebrovascular problems may develop.

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