Letters to the Editor

Long-term efficacy of ETV and shunt surgery for management of hydrocephalus


We commend the authors for undertaking this study, with one of the longest follow-up periods in a pediatric population with hydrocephalus. The etiologies of hydrocephalus that may impact the long-term efficacy of the treatment and complications of shunt placement have been systematically classified and recorded. However, we would like to bring forth a few issues concerning the article that need further consideration. We observe that this study was undertaken with the aim to understand the preferable treatment modality for hydrocephalus. The two primary modalities studied here were endoscopic third ventriculostomy (ETV) and shunt surgery. In both these treatment groups, the authors noted that there is significant contribution of the etiology of hydrocephalus to determining the long-term efficacy of surgical treatment. We observed in the article’s survival curves that different etiologies responded better to the two treatments. Thus, it would be clinically relevant to know which treatment works better for a particular etiology. And consequently, the comparison of the two treatment modalities for each of the common etiologies must have been done to contribute to the clinical decision making. We also note that the ETV and shunt groups differ systematically in terms of the etiology of hydrocephalus. Thus, for an overall comparison of ETV and shunt surgery, a multivariate analysis must have been performed, eliminating the impact of etiology difference.

The classification of etiologies is an elaborate and comprehensive task. However, we find that there are some instances that might be clinically erroneous in spite of being pathologically correct. As an example, cases of postinfectious and posttraumatic hydrocephalus have been clubbed together in the inflammatory group, although the two groups may have a systematic difference in the long-term outcomes in lieu of the etiology. Thus, we suggest that they should have been grouped and analyzed separately and compared with each other. Also, the tumoral group has not been further classified into posterior fossa and supratentorial tumors, and all patients who underwent ETV had posterior fossa tumors, which might also have an impact on efficacy. These errors in classification have resulted in missing out on a few clinically important findings that this study would have contributed.

It is also notable that in the Orbis Sigma Valve (OSV) shunt group the best outcome occurred in those belonging to the “others” group of etiology. It would have been very informative if the authors had mentioned the specific etiologies comprising this group.

The authors have taken note of the fact that there was a systematic difference with regard to the surgeons performing OSV and differential-pressure valve shunt surgeries and also the small number of patients belonging to the differential-pressure valve group, which might be contributing to the differences observed in the long-term outcomes between the two shunts.

In their review of the literature the authors noted a much lower success rate for shunt surgery in other studies compared to their study.2,3 However there was no attempt by the authors to explain the probable reasons for these observed differences in the outcome measures.

One of the merits of this study is that it had one of the longest follow-up durations. This merit has not been used to the full extent. The authors noted that several patients required repeat procedures after failure of the first surgery. In understanding the long-term outcomes of the patients with hydrocephalus, it is important to understand not only the rate of failure and need for second surgery but also the outcome of the second surgery. And thus we believe that it would have been beneficial if an analysis of the outcomes of redo ETVs and secondary shunt surgeries after initial ETV or shunt surgery had also been reported.

Additionally, comparison between the efficacy of primary and secondary surgery and comparison between ETV and shunt surgery when performed as the secondary surgery should have been done.
In conclusion, we would like to say that although this study has undoubtedly highlighted many clinically important findings, it fails to shed light on several important aspects of the topic.

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References

Disclosures
The authors report no conflict of interest.

Response
No response was received from the authors of the original article.

Biomechanical vulnerability with second concussion

TO THE EDITOR: I read with interest the article by Post et al. demonstrating no increase in biomechanical vulnerability among patients who have experienced a repeat concussion compared to those with their first concussion (Post A, Hoshizaki TB, Gilchrist MD, et al: A comparison in a youth population between those with and without a history of concussion using biomechanical reconstruction. J Neurosurg Pediatr 19:502–510, April 2017). They do not give any data on the time between the first and second concussions among the study patients. A article published by my group demonstrated that the time between concussions could be critical. We found that patients who had a second concussion within 1 year recovered more quickly than patients with a first concussion or those who had a second concussion more than 1 year after their first. We hypothesized that patients who experience head trauma within 1 year of a concussion did have increased vulnerability to a second concussion. Furthermore, patients concussed with less biomechanical force recovered more quickly.

Post et al. could examine the validity of our hypothesis by comparing 3 groups within those patients with repeat concussions: those occurring within 6 months, 6 months to 1 year, and more than a year. I urge them to do so. If on further analysis of their data, they find that patients who suffer a concussion are more vulnerable to a second one within the 1st year, it would be an important factor in deciding when to allow patients to return to contact sports or other activities in which there is an increased risk of head trauma.

One caveat to consider is that all the patients in their study were recruited from emergency visits. None of our patients with a recurrent concussion within a year presented to an emergency department (ED). They were first seen in our primary care pediatric office. In many of these patients, the chief complaint, unlike most concussed patients, was not head trauma but headache or dizziness. Therefore, if our hypothesis is validated, the physician caring for a patient who has had a concussive episode within a year will need to look for evidence of head trauma and a second concussion even if the chief complaint is not head trauma.

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References

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The author reports no conflict of interest.

Response
We thank Dr. Taubman for his interest in this research paper. We read with interest the paper that his group published examining the recovery time for patients with a history, or no history, of concussion. The differences in the results between the Dr. Taubman’s paper and this work may be reflected in differences in methods, specifically the subject sample and the dependent variables. The data set from our paper included a pediatric cohort that presented to tertiary pediatric EDs, whereas in the article by Taubman et al., the study included participants who presented to primary care clinics. This difference in the populations might affect a comparison of results given the possibility that the ED population may have presented with greater initial symptom burden or more severe mechanism of injury. Additionally, Taubman et al. focused on the recovery of the patients, using neurocognitive testing, and how previous concussions can affect this outcome. In our study we focused on dependent variables that were entirely biomechanical in nature, with no quantification of recovery time based on previous head injury history. We concur that it would be valuable in the future to examine...