Neurosurgical Forum LETTERS TO THE EDITOR

Hindsight vision and admission after spinal growing implants

TO THE EDITOR: I read with great interest the article by Shaw et al.⁴ (Shaw KA, Fletcher ND, Devito DP, et al: Complications following lengthening of spinal growing implants: is postoperative admission necessary? *J Neurosurg Pediatr* 22:102–107, July 2018). The authors performed a retrospective study on 796 children undergoing growing spinal implants and concluded that postoperative admission status did not affect the rate of 30-day perioperative complications, readmission, or rate of unplanned operations following lengthening of growing spinal instrumentation. The authors should be congratulated for performing a well-designed study on an important topic (e.g., hospital admission) in pediatric patients.^{2,5} In addition, the current emphasis on the need to reduce health-care costs makes the topic very relevant in perioperative medicine.³

Although the study of Shaw et al. was well conducted, there are questions regarding the study that need to be clarified by the authors. First, the authors did not account for the impact from the immediate postoperative complications on the decision to admit the patients.¹ It is plausible that patients who were not doing well in the postanesthesia care unit were admitted. This fact significantly limits the disposition planning of these patients. Moreover, the authors did not account for specific factors that could lead to higher rates of admission (e.g., patients with chronic pain). Last, the authors should present 95% confidence intervals for the outcomes so that clinical practitioners can make more informed decisions. Although complications were similar between the groups, patients who were admitted and develop complications had better support and care than those who developed complications at home.

I would welcome comments by the authors, as this would further support the findings of this important clinical trial.

Mark C. Kendall, MD

Rhode Island Hospital, Warren Alpert Medical School
of Brown University, Providence, RI

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Disclosures

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Correspondence

Mark C. Kendall: mark.kendall@lifespan.org.

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Response

We thank Dr. Kendall for his interest and comments regarding our recently published article investigating postoperative admission status and its influence on 30-day perioperative complications for children undergoing lengthening of growth-friendly spinal instrumentation. As Dr. Kendall is well aware, children requiring growing spinal instrumentation for spinal deformity are a unique patient population that requires special attention.^{1,2}

Dr. Kendall brings forth an important point in his critique of our article, pertaining to the influence of post-anesthesia monitoring and the decision to admit children postoperatively. This is an important factor emphasized in the American Academy of Pediatrics guidelines for perioperative anesthesia management and the need for extended, inpatient postoperative monitoring for children who develop any complications in the perioperative period.³ Unfortunately, this point was unable to be accounted

for within the confines of the NSQIP-P (National Surgical Quality Improvement Program–Pediatric) database, which does not contain a variable within the defined, 120 data variables collected by the participating institutions on the reason for inpatient or outpatient admission. Given the high incidence of comorbidities in this at-risk patient population,^{1,2} it is quite plausible that several patients underwent unplanned postoperative admission from the postanesthesia care unit due to unforeseen complications. This point highlights the error with a dogmatic approach in the admission decision process for postsurgical patients, and the need for individualized patient treatment plans, especially with regard to postoperative admission.

Regarding the request for confidence intervals of clinical interpretation of the data, I would highlight Table 3 in our publication, summarizing the univariate and multivariate analysis of risk factors for postoperative admission. The analysis found that only preexisting ventilator dependence (which included use of continuous or bilevel positive airway pressure support) as the only statistical and clinical independent risk factor for postoperative admission, with an odds ratio of 4.2 (95% CI 1.44–12.29).

With the known deficiencies of the NSQIP-P database, further clinical research is needed to investigate the influence of patient factors on the need for postoperative admission after lengthening of growth-friendly spinal instrumentation.

K. Aaron Shaw, DO

Dwight D. Eisenhower Army Medical Center, Fort Gordon, GA

Nicholas D. Fletcher, MD

Emory University, Atlanta, GA

Dennis P. Devito, MD

Joshua S. Murphy, MD

Children's Healthcare of Atlanta, GA

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Correspondence

K. Aaron Shaw: kenneth.a.shaw34.mil@mail.mil.

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Ligamentum nuchae as a graft material for duraplasty in patients with Chiari malformation type I

TO THE EDITOR: We read with avid interest the paper by Cools et al.³ (Cools MJ, Quinsey CS, Elton SW: Chiari decompression outcomes using ligamentum nuchae harvest and duraplasty in pediatric patients with Chiari malformation type I. *J Neurosurg Pediatr* 22:47–51, July 2018). In their single-center, retrospective cohort study of 25 pediatric patients with Chiari malformation type I, the authors have not only presented their surgical outcomes following Chiari decompression but also eloquently described the technique of harvesting ligamentum nuchae (LN) for duraplasty, which is very commendable. From the results of their study, they infer that LN “provides a more robust autologous graft than the commonly used pericranial graft.” However, we disagree, in part, with their opinion.

Ligamentum nuchae could be more robust than pericranium, but this statement needs to be considered with caution and the understanding that this may not be true for all age groups. Pericranium in children tends to be thin and fragile. Its quality and thickness improve with maturity.^{1,2} In a recent literature review of studies comparing outcomes with two dural graft types during Chiari decompression surgery, Ablat et al.¹ concluded that the inherent difficulty in harvesting thin pediatric pericranium could have influenced the results of a pediatric study that was included in the review. Moreover, LN can undergo ossification in adults. The incidence of this is about 20%–30% in the Asian population, and it increases with age.⁵ Improvement in the quality of the pericranium with maturity and the loss of elasticity of the LN with aging may render the pericranium a better choice over LN as a graft material in adults.

Citing the literature review by Ablat et al.,¹ Cools et al. stated that there was a 10% reoperation rate “due to CSF-related complications” when pericranial grafts were used. However, a thorough analysis of this review and its included studies reveals that this was the rate of “revision hindbrain decompression” and that the actual rate of reoperation due to CSF-related complications would be lower. Also, in the present-day scenario, pericranium can be harvested in a simplified manner without extending the skin incision beyond the standard approach. Stevens et al.⁸ described how the use of a handheld retractor, retracting dorsally at the apex of the wound, could avoid injury to paired occipital arteries and occipital nerves and how a judicious subgaleal dissection using Metzenbaum scissors could yield a sufficiently sized pericranial graft without extending the skin incision.

Lastly, the statement “it is possible to use midline collagenous scar, in a repeat operation, as further graft material” appears to be controversial. Scar tissue may be detrimental as it can further shrink and cause cerebellar compression and obstruction of CSF flow within the posterior fossa.⁷

In light of the scarce literature available on the use of LN as a graft material, it is safe to say that pericranium should be utilized when it is available and of good quality.^{1,4,9} Nevertheless, the limited literature suggests similar surgical outcomes with the use of LN.⁶ It is a simple, low-cost, robust alternative to pericranium especially in the pediatric age group. But is it superior? Randomized controlled trials comparing these two autologous graft materials are warranted to arrive at a definitive conclusion in this regard.

Nitish Agarwal, MBBS

Mohit Agrawal, MBBS

Dattaraj P. Sawarkar, MCh

All India Institute of Medical Sciences, New Delhi, India

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Disclosures

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Correspondence

Dattaraj Sawarkar: dattaraja@gmail.com.

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Response

We thank Agarwal and colleagues for their comments

on our paper. We would like to address a few of their concerns. As they astutely pointed out, Alba et al. noted that pericranium in children may be fragile and more difficult to harvest, thus affecting the incidence of complications in the pediatric population.¹ They also pointed out that ossification of the LN may occur in adults, especially in Asian populations.³ In our study we looked only at pediatric patients, and though the LN has been used with anecdotal success in adults, it requires more intensive study.

Pericranium is clearly a popular² and satisfactory graft material in many circumstances, with a low complication rate.¹ The goal of our limited retrospective study was not to prove the superiority of LN, but instead to illustrate an alternative that should be considered when designing future studies. We agree that there is a scarcity of literature on the use of LN and that a minority of pediatric neurosurgeons use this material for Chiari decompressions.² However, this does not indicate that it is inferior. We agree that further study is needed for a true comparison of graft materials.

Michael J. Cools, MD

Carolyn S. Quinsey, MD

Scott W. Elton, MD

University of North Carolina, Chapel Hill, NC

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Altered intracranial venous physiology

TO THE EDITOR: We read with keen interest the article by Cornelissen et al.¹ describing the role of intracranial venous physiology in intracranial hypertension in craniosynostosis (Cornelissen MJ, de Goederen R, Doerga P, et al: Pilot study of intracranial venous physiology in craniosynostosis. **J Neurosurg Pediatr** 21:626–631, June 2018).

We commend the authors for attempting to study the role of altered intracranial venous physiology with the use of Doppler and the possible role in raised intracranial pressure (ICP), especially in patients with nonsyndromic craniosynostosis. However, we would like to seek clarifications on some of the shortcomings that we found in the study.

Papilledema was used as a measure of intracranial hypertension in this study. Although it is highly specific, the sensitivity of papilledema in assessing intracranial hypertension is only 22% in children less than 8 years of age.⁵ Use of ICP monitoring would have been a better measure to identify intracranial hypertension.

It was not described whether the study population was evaluated for variables like Chiari I, hydrocephalus, obstructive sleep apnea (OSA), altered cranial venous outflow, and craniocerebral disproportion (CCD); these factors are known to contribute to raised ICP in patients with syndromic craniosynostosis,³ and thus may confound the results.

Doppler assessment is highly operator dependent and is affected by various factors like level of activity and head end elevation.⁴ In this study, measurements were taken at various degrees of head end elevation (0°–30°) and also could not be performed in a few patients in the postoperative period due to lack of a radiological window. The use of noninvasive techniques like MR venography could overcome these limitations.

The study population was heterogeneous (syndromic and nonsyndromic) and consisted of patients who underwent different procedures, which probably would have had varied effects, and the subjects were evaluated at different points of time, which would have led to bias. The authors also fail to describe the mechanism by which superior sagittal sinus velocity increases postoperatively in patients with nonsagittal craniosynostosis like metopic craniosynostosis if it is hypothesized that a synostotic sagittal suture is responsible for venous outflow obstruction.²

Finally, we would like to congratulate the authors for reporting on an alternative cause for raised ICP in patients with craniosynostosis and for considering possible therapeutic interventions in the future. This article paves the way for further such studies with a larger and more homogeneous population.

Surya Sri Krishna Gour, MS
Mohit Agrawal, MCh
Dattaraj Sawarkar, MCh

All India Institute of Medical Sciences, New Delhi, India

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Correspondence

Dattaraj Sawarkar: dattaraja@gmail.com.

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Response

We thank Gour et al. for the letter about our pilot work on intracranial venous physiology in patients with craniosynostosis. Our report is the first to analyze intracranial venous outflow and its relation to intracranial hypertension. In so doing, we have used the data for generating new hypotheses that could help in rational therapies in our patients.

To be clear, we entirely accept the criticisms raised by Gour et al.: the operator dependence in Doppler ultrasound, validity of papilledema, and heterogeneity in causes of intracranial hypertension. We, too, are concerned about such limitations when studying the in vivo pathophysiology of craniosynostosis, and value the opportunity to discuss our thoughts on these matters.

First, in regard to measurements with Doppler ultrasound. Such measurements are influenced by the angle of insonation and the patient's position, which may lead to bias unless a standardized technique and protocol is used, as was done in our study.

Second, the debate about using papilledema goes far beyond the scope of a brief letter. We acknowledge that in the diagnosis of intracranial hypertension in craniosynostosis, Tuite et al. showed that the sensitivity is low (approximately 0.22) in young children.⁸ Recent reports, however, have shown variation in occurrence of papilledema according to craniosynostosis subtype. For example, at our center we have identified papilledema in 10%, 2%, and up to 50% of cases with sagittal,^{5,9} metopic,¹ and syndromic craniosynostosis,² respectively. This diagnosis-related prevalence of papilledema corresponds with the previously reported occurrence of intracranial hypertension. Therefore, in our practice, we remain convinced of the value of funduscopy.

Third, there is no doubt that we are dealing with a complex pathophysiological problem that may include interactions between OSA, Chiari I, hydrocephalus, altered cranial venous outflow, and CCD. Of these factors, OSA, hydrocephalus, CCD, and Chiari I are found in Apert and Crouzon syndrome.^{3,4,6,7} Our study only included 2 cases with Crouzon syndrome. In one case OSA, hydrocephalus, and Chiari I malformation were present. In theory, the OSA or the Chiari I malformation could have caused intracranial hypertension instead of abnormal cranial venous outflow. In the other patient, there was no evidence of OSA, hydrocephalus, or Chiari I malformation. In the nonsyndromic craniosynostosis cases we only review CT scans and occipitofrontal circumference (OFC) measurements routinely because OSA, hydrocephalus, altered cranial venous outflow, and Chiari I malformation are rarely present.

Taking all of the above together, readers should consider our study as the first step to improving what is known

about cranial venous outflow in patients with craniosynostosis. Our pilot study indicates an alteration in cerebral venous outflow that appears to be associated with the occurrence of intracranial hypertension. In the future, our aim is to clarify this matter further, and we thank Gour et al. for their invaluable contribution to the discussion.

Martijn J. Cornelissen, MD, PhD

Robbin de Goederen, MD

Irene M. J. Mathijssen, MD, PhD, MBA-H

Sophia Children's Hospital, Erasmus MC, University Medical Center,
Rotterdam, The Netherlands

Robert C. Tasker, MA, MD, FRCP(UK)

Harvard Medical School, Boston Children's Hospital, Boston, MA

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