Letters to the Editor

NEUROSURGICAL FORUM

Ventriculoiliac shunt: a single case experience

TO THE EDITOR: The recent article by Tubbs et al.¹ assessing the feasibility of ventriculoiliac shunts prompted us to share our clinical experience and difficulties using a ventriculoiliac shunt in a pediatric patient (Tubbs RS, Tubbs I, Loukas M, et al: Ventriculoiliac shunt: a cadaveric feasibility study. *J Neurosurg Pediatr* 15:310–312, March 2015). We currently care for a 7-year-old boy who was born premature at 26 weeks and developed hydrocephalus of prematurity. His early years were complicated by multiple distal shunt revisions, largely due to infection and a subsequent abdominal pseudocyst. Subsequent distal sites for the shunt included several attempts in the peritoneum, atrium, pleura, gallbladder, and superior sagittal sinus. Bilateral jugular veins, subclavian veins, and femoral veins were utilized at different times to access the atrium until thrombus occluded all of these veins and the only venous return to the heart was through a dilated azygous system. At 5 years of age, after all intraoperative and interventional radiology attempts to access the atrium were ultimately unsuccessful, we placed the distal catheter in the iliac crest (Fig. 1). Unfortunately, the shunt failed after 10 days due to failure of absorption and leakage from the distal wound, and the shunt had to be placed directly in the atrium via a median sternotomy.

From our limited experience with placement of a ventriculoiliac shunt in a single pediatric patient, two technical considerations are worth noting. First, we believe the shunt failed primarily due to the inability to maintain a tight seal between the distal catheter and the iliac crest. In our patient, a 2.5-cm deep channel into the iliac crest was made using a gear shift and a drill with a small bur. Distal tubing was cut precisely to fit into the channel and then was attached to a straight metal connector at the cortical margin of the iliac crest to be able to secure it tightly with 2-0 silk sutures and seal it with bone wax. Distal flow into the iliac crest was confirmed intraoperatively with a manometer. Despite these efforts, the shunt still failed distally after 10 days. We hypothesize that this was due to small movements of the catheter and loosening of the seal that occurs with clinical activity, with subsequent backflow of fluid along the catheter. Although this anatomical study nicely demonstrates that the ilium has enormous capacity to absorb spinal fluid (up to 30 L/hour), our clinical experience

FIG. 1. Shunt series (anteroposterior and lateral views) showing successful placement of the distal tubing in the right iliac crest.

ience suggests that securing the distal catheter with only bone wax is unlikely to be successful.

Second, we elected to tunnel the distal catheter into and then back out of the peritoneum before securing the end to the iliac crest. This allowed us to leave extra tubing in the peritoneum to allow for subsequent growth of the patient. In the artist's rendition of the ventriculoiliac shunt included in this article, there appears to be no additional tubing to allow for growth and is likely to result in eventual pullout of the catheter and distal failure.

We congratulate the authors of this study for scientifically assessing the feasibility of alternative sites for distal catheters and look forward to further modifications that will help make this technique successful clinically.

Hannah E. Goldstein, MD

Neil A. Feldstein, MD

Richard C. E. Anderson, MD

Columbia University, Morgan Stanley Children's Hospital of NewYork-Presbyterian, New York, NY

DISCLOSURE

The authors report no conflict of interest.

Reference

1. Tubbs RS, Tubbs I, Loukas M, Cohen-Gadol AA: Ventriculoiliac shunt: a cadaveric feasibility study. *J Neurosurg Pediatr* 15:310–312, 2015

Response

We appreciate the comments of Goldstein et al. and their interest in the ventriculoiliac shunt as proposed in our recent cadaveric study. This study has resulted in several groups using the ilium for distal CSF diversion in the clinical setting. One group (personal communication) with a similarly difficult patient has had, to date, several months of success with their patient. We recently placed our first ventriculoiliac shunt in a child in whom other sites (e.g., peritoneal and pleural cavities, right atrium) had failed and who had been in the hospital with extraventricular drainage for months. We placed the distal end of this child's shunt into his ilium and placed a purse string suture around the adjacent abdominal wall soft tissues including the catheter as it entered the ilium. The child was discharged and was well for over a month. However, he presented just last week with a shunt infection and is currently undergoing antibiotic therapy with extraventricular drainage. Regarding the case of Goldstein et al., bone wax was used in our cadaveric feasibility study with success, but long-term clinical use will most likely necessitate other anchoring mechanisms. This is an engineering issue, which can be easily addressed.

R. Shane Tubbs, MS, PA-C, PhD
Children's of Alabama, Birmingham, AL

Aaron A. Cohen-Gadol, MD, MSc
Goodman Campbell Brain and Spine, Indiana University, Indianapolis, IN

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Ethnicity and craniosynostosis

TO THE EDITOR: We read with enthusiasm the recent paper by Anderson et al.¹ (Anderson IA, Goomany A, Bonthron DT, et al: Does patient ethnicity affect site of craniosynostosis? *J Neurosurg Pediatr* 14:682–687, December 2014) regarding the epidemiological pattern of suture involvement in craniosynostosis among different ethnic groups. The authors identified 312 cases of nonsyndromic craniosynostosis over a 5-year period, and compared the distribution of involved sutures between two ethnic groups; namely, a cohort of 222 white patients and a cohort of 56 Asian patients. The authors found a statistically significant difference in the prevalence of the various sutures affected in the nonsyndromic craniosynostosis between the two cohorts.¹

In the Asian patients, there were more cases of complex craniosynostosis and fewer cases of sagittal synostosis than would be expected. The rate of decision for surgery in the Asian cohort was lower than in the white cohort.¹

We would like to report our own experience with a pure population of Asian (Iranian) patients who underwent operation for nonsyndromic craniosynostosis by a single neurosurgeon over a period of 5 years in Children's Hospital Medical Center. This hospital is a referral center for pediatric neurosurgery patients in Iran and has admissions from the entire country. In our surgical cohort of 253 patients, there were 73 sagittal, 63 metopic, 62 unicoronal, 27 bicoronal, 27 multisuture, and 1 lambdoid suture synostosis. These results are compatible with those of the Asian cohort in the series reported by Anderson et al., in terms of the distribution of synostotic sutures among the patients who underwent surgery. Although other studies reported the sagittal subtype as the most common type, with the rate of as high as 50.7%,³ the proportion of surgery for sagittal synostosis was lower than the predicted level in our series. Also, multiple suture involvement in our center (10.2%) was higher than that in the white group in Anderson et al.'s study. The total number of bicoronal and unicoronal craniosynostosis cases surpassed other subtypes in this Iranian population.

The different pattern of suture involvement in Asian populations can be attributed to either genetic or external influences. A positive family history has been proposed as a significant risk factor, which highlights the role of genetics in craniosynostosis.² Cultural aspects may affect the surgical rate as well; the shape of the head in sagittal synostosis (especially in boys) may be acceptable for Asian parents, but most families adopt surgical treatment for what they consider to be more disfiguring deformities, in particular unicoronal and metopic subtypes. Nonetheless, we observed more cases of the sagittal subtype in our outpatient clinic, many of whom were not scheduled for surgery due to their parents' refusal. On the other hand, some people in this area do not assume that scaphocephaly is a disease, especially when there is no prominent frontal bossing. Interestingly, there is a traditional concept, mostly in the east of this country, that scaphocephalus is not a disease and that these children are somewhat more intelligent than others. Our surgical data may therefore underestimate the true prevalence of sagittal synostosis in the community because many individuals with less disfiguring scaphocephaly possibly do not seek medical care.

Farshid Farzaneh, MD

Shariati Hospital, Tehran University of Medical Science, Tehran, Iran

Ehsan Moradi, MD

Zohreh Habibi, MD

Farideh Nejat, MD, MPH

Children's Hospital Medical Center, Tehran University of Medical Science, Tehran, Iran

DISCLOSURE

The authors report no conflict of interest.

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3. Di Rocco F, Arnaud E, Renier D: Evolution in the frequency of nonsyndromic craniosynostosis. **J Neurosurg Pediatr** 4:21–25, 2009

Response

We thank Dr. Nejat and his team from the Children's Hospital Medical Center, Tehran, Iran, for their correspondence. Their results would appear to confirm our suspicion that the distribution of craniosynostosis is different in different ethnic populations. Additionally, the authors also confirm our suspicion of a relatively strong cultural aspect to the diagnosis and uptake of surgery in different populations. This confounding factor in studies on the incidence

of craniosynostosis can only be unravelled by a community-based study. If such a study could be undertaken, it would be important to include some functional outcome measures because this may well add further information to the growing debate over whether nonsyndromic craniosynostosis is purely an aesthetic problem.

Ian A. Anderson, MRCS(Ed)

Paul D. Chumas, FRCS

Leeds General Infirmary, Leeds, United Kingdom

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