Neurosurgical Forum

LETTERS TO THE EDITOR

Prevention strategies for nonaccidental head trauma in infants

TO THE EDITOR: We read with great interest the article by Thompson and colleagues¹ (Thompson LW, Bass KD, Agyei JO, et al. Incidence of nonaccidental head trauma in infants: a call to revisit prevention strategies. *J Neurosurg Pediatr.* 2019;24(6):689–696). We would like to provide some contextual background and express some concerns we have with the authors' results and conclusions. Since its inception in December 1998, The Upstate New York Shaken Baby Syndrome Prevention Program (currently renamed as Safe Babies New York) was designed as a primary prevention program to educate parents of all newborn infants in Upstate New York, while documenting prospectively the incidence of abusive head trauma (AHT) to evaluate the program's impact. Our initial results, evaluating the first 5.5 years, demonstrated a 47% reduction in AHT incidence compared with a historical control period immediately preceding the intervention.² A second independent study of the program in Downstate New York achieved an even greater reduction.³ However, a virtually identical intervention across Pennsylvania, and another program in North Carolina, unfortunately were not successful.^{4,5}

We have continued, over the last 20 years, to prospectively monitor the impact of our Upstate New York program on AHT admissions to Oishei Children's Hospital of Buffalo (OCHB). The Pediatric Neurosurgery Department provided us prospectively with the number of identified AHT patients admitted to OCHB, identifying 92 such cases between December 1, 1999, and December 1, 2018. Specifically, 82 cases were identified during the time period of Thompson and colleagues' study (between January 1999 and December 2016). This latter number stands in sharp contrast to the 46 AHT cases the authors report from the Safe Babies New York program during this same time period (we did not provide these numbers as we were unaware of this study until after its publication). There are years during which the authors identified cases retrospectively that were not reported to us prospectively, and other years during which they identified fewer cases than were reported to us. Finally, there remains a discrepancy of 25 cases over the time period of this study that were not reported to us prospectively and which, if true AHT cases, we will need to add to our data set.

We also have some concerns about the methodology the authors used to identify cases from retrospective chart reviews. Their selected ICD-9 and -10 codes appear to be different from algorithms previously published and recommended by the Centers for Disease Control and Prevention to track AHT cases.^{6–8} We are therefore uncertain as to the validity of these additional cases. Lastly, although they allude tangentially to the recession of 2007 in their discussion of poverty rates in Upstate New York, the authors do not mention the documented impact of this recession on AHT rates in other studies,^{9–11} something that may well have influenced the AHT rate in Upstate New York as well.

However, we find the results intriguing. We interpret the increase over time as confirming our initial concern that dedication to such prevention efforts may flag over time, especially with changes in floor nurses and nursing leadership. Additionally, the loss of the physician champion (M.S.D.) may have further reduced the regional commitment to continuing the program. We are presently analyzing the results of this program on AHT incidence in Upstate New York over the last 20 years, both compared with preintervention rates and contemporaneous AHT rates from states lacking statewide prevention programs. We invite the authors to collaborate with us on this project.

Mark S. Dias, MDPenn State Health Children's Hospital, Penn State College of Medicine,
Hershey, PA**Kim Smith, RN, BSN****Kathy deGuehery, RN, BSN**Safe Babies New York, Oishei Children's Hospital of Buffalo,
University at Buffalo, NY**References**

1. Thompson LW, Bass KD, Agyei JO, et al. Incidence of nonaccidental head trauma in infants: a call to revisit prevention strategies. *J Neurosurg Pediatr.* 2019;24(6):689–696.
2. Dias MS, Smith K, deGuehery K, et al. Preventing abusive head trauma among infants and young children: a hospital-based parent education program. *Pediatrics.* 2005;115(4):e470–e477.
3. Altman RL, Canter J, Patrick PA, et al. Parent education by maternity nurses and prevention of abusive head trauma. *Pediatrics.* 2011;128(5):e1164–e1172.
4. Dias MS, Rottmund CM, Cappos KM, et al. Association of a postnatal parent education program for abusive head trauma

with subsequent pediatric abusive head trauma hospitalization rates. *JAMA Pediatr.* 2015;171(3):223–229.

5. Zolotor AJ, Runyan DK, Shanahan M, et al. Effectiveness of a statewide abusive head trauma prevention program in North Carolina. *JAMA Pediatr.* 2015;169(12):1126–1131.
6. Parks SE, Kegler SR, Annett JL, Mercy JA. Characteristics of fatal abusive head trauma among children in the USA: 2003–2007: an application of the CDC operational case definition to national vital statistics data. *Injury Prev.* 2011;18(3):193–199.
7. Parks SE, Sugarman D, Xu L, Coronado V. Characteristics of non-fatal abusive head trauma among children in the USA, 2003–2008: application of the CDC operational case definition to national hospital inpatient data. *Injury Prev.* 2012;18(6):392–398.
8. Shanahan ME, Zolotor AJ, Parrish JW, et al. National, regional, and state abusive head trauma: application of the CDC algorithm. *Pediatrics.* 2013;132(6):e1546–e1553.
9. Berger RP, Fromkin JB, Stutz H, et al. Abusive head trauma during a time of increased unemployment: a multicenter analysis. *Pediatrics.* 2011;128:637–643.
10. Huang MI, O’Riordan MA, Fitzenrider E, et al. Increased incidence of nonaccidental head trauma in infants associated with the economic recession. *J Neurosurg Pediatr.* 2011;8(2):171–176.
11. Wood JN, French B, Fromkin J, et al. Association of pediatric abusive head trauma rates with macroeconomic indicators. *Acad Pediatr.* 2015;16(3):224–232.

Disclosures

Dr. Dias reports that the research discussed was supported by grants from New York State.

Correspondence

Mark S. Dias: mdias@pennstatehealth.psu.edu.

INCLUDE WHEN CITING

Published online January 31, 2020; DOI: 10.3171/2019.12.PEDS19713.

Response

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

©AANS 2020, except where prohibited by US copyright law

Extradural posterior odontoidectomy

TO THE EDITOR: We read with great interest the article by Archer et al.¹ on the posterior transdural approach for odontoidectomy in the *Journal of Neurosurgery: Pediatrics* (Archer J, Thatikunta M, Jea A. Posterior transdural approach for odontoidectomy in a child: case report [published online October 11, 2019]. *J Neurosurg Pediatr.* doi:10.3171/2019.7.PEDS19337). In their article, the authors have reported the use of this technique in a 12-year-old child diagnosed with Down syndrome and a prior history of occipitocervical fusion. The child presented with compressive cervical myelopathy due to basilar invagination and os odontoides with an increased atlantodental interval (6 mm). They successfully managed this patient with a suboccipital craniectomy, C2 laminectomy, posterior transdural drilling of the odontoid, and stabilization by occipito-C3–4 fixation. Intraop-

eratively, they also found a dorsal arachnoid cyst, which was fenestrated.

In 2017, we described the posterior odontoid drilling technique simultaneous with bilateral posterior C1–2 joint distraction and stabilization using bilateral posterolateral corridors through a single posterior midline skin incision.² We reported on 4 patients who had been diagnosed with complex atlantoaxial dislocation and treated using this technique with good clinical outcomes. As opposed to the technique described by Archer et al., we utilized a completely extradural approach to drill the odontoid from both posterolateral corridors.

A number of surgical nuances to this approach are important. A contralateral table tilt helps to improve the operative angle and widens the surgical corridor; therefore, the patient should be safely strapped onto the table. Performing a C1–2 joint distraction using spacer placement prior to the posterior odontoid drilling offers a significant technical advantage. It helps to bring down the odontoid tip in cases with high basilar invagination. Also, it aligns a retroflexed odontoid back to its natural vertical position, creating more space for safe drilling between the odontoid and the dura mater. However, the C1–2 screws and rods should not be placed prior to drilling, as they would restrict the posterolateral trajectory to the odontoid.

Given our experience with the technique, we strongly believe that the extradural approach offers the advantage of being able to approach the odontoid from both sides. We do agree that it is relatively easier to drill the odontoid in the setting of a displaced spinal cord; however, the extradural approach offers the advantage of being able to drill more bone laterally to widen the surgical corridor. Therefore, a displaced spinal cord is not a limiting prerequisite. Additionally, it avoids direct retraction over the spinal cord and the need for cutting the dentate ligament. Sometimes, it is possible to preserve the C2 nerve root to avoid loss of sensation in the suboccipital region, which can predispose these patients to develop decubitus ulcers on prolonged bed rest, often required of them due to significant myelopathy.³

We strongly believe in the feasibility, safety, and reproducibility of the posterior approach to drill the odontoid. The transdural approach described by Archer et al. finds its application in dealing with cases in which intradural lesions co-exist with a retroflexed odontoid. We appreciate the work done by the authors. It adds to the existing literature some evidence on the importance of posterior odontoid drilling to manage complex lesions at the craniovertebral junction, where the C1–2 joint distraction and posterior stabilization technique alone fails to relieve the anterior thecal compression. It would be of potential interest to compare these two techniques in a greater number of patients.

Deepak Khatri, MCh

Lenox Hill Hospital, New York, NY

Jaskaran Singh Gosal, MCh, DNB

All India Institute of Medical Sciences (AIIMS), Jodhpur, Rajasthan, India

Kuntal Kanti Das, MCh

Kamlesh Bhaisora, MCh

Arun Kumar Srivastava, MCh

Sanjay Gandhi Post Graduate Institute of Medical Sciences (SGPGIMS),
Lucknow, Uttar Pradesh, India

References

1. Archer J, Thatikunta M, Jea A. Posterior transdural approach for odontoidectomy in a child: case report [published online October 11, 2019]. *J Neurosurg Pediatr*. doi:10.3171/2019.7.PEDS19337
2. Srivastava AK, Behari S, Sardhara J, Das KK. Simultaneous odontoid excision with bilateral posterior C1-2 distraction and stabilization utilizing bilateral posterolateral corridors and a single posterior midline incision. *Neurol India*. 2017;65(5):1068–1075.
3. Salunke P, Karthigeyan M, Futane S. Pros and cons of C2 nerve sectioning/preservation in posterior fusion for congenital atlantoaxial dislocation. *World Neurosurg*. 2018;118:e925–e932.

Disclosures

The authors report no conflict of interest.

Correspondence

Arun Kumar Srivastava: doctorarunsrivastava@gmail.com;
arunks@sgpgi.ac.in.

INCLUDE WHEN CITING

Published online February 7, 2020; DOI: 10.3171/2020.1.PEDS2017.

Response

We greatly appreciate the interest that our article garnered from Khatri et al. It is apparent that the authors have much experience with posterior odontoidectomy, and we look forward to following their work into the future.

Jacob Archer, MD

Indiana University School of Medicine, Indianapolis, IN

Meena Thatikunta, MD

University of Louisville Hospital School of Medicine, Louisville, KY

Andrew Jea, MD, MHA

Indiana University School of Medicine, Indianapolis, IN

INCLUDE WHEN CITING

Published online February 7, 2020; DOI: 10.3171/2020.1.PEDS2024.

©AANS 2020, except where prohibited by US copyright law