Neurosurgical Forum

LETTERS TO THE EDITOR

Risk factors for shunt dependency after aneurysmal subarachnoid hemorrhage

TO THE EDITOR: We read with great interest the article by Wilson et al.²¹ (Wilson CD, Safavi-Abbasi S, Sun H, et al: Meta-analysis and systematic review of risk factors for shunt dependency after aneurysmal subarachnoid hemorrhage. *J Neurosurg* [pub ahead of print April 1, 2016. DOI: 10.3171/2015.11.JNS152094]), in which a comprehensive meta-analysis had been conducted and several risk factors for shunt dependency in patients with aneurysmal subarachnoid hemorrhage (aSAH) were identified. Their findings were helpful for risk stratification in the

assessment of patients with aSAH. However, some more issues should be addressed.

Firstly, the criteria for shunt dependency were not specified in this meta-analysis. According to the American Heart Association/American Stroke Association guidelines, chronic symptomatic hydrocephalus requires shunt placement.⁴ However, 2 of the studies^{8,16} included in the meta-analysis focused on risk factors associated with hydrocephalus instead of shunt dependency. Therefore, we believe that these studies should not be included. Secondly, 2 included studies focused on the aSAH patients with external ventricular drain (EVD) placement.^{2,3} Since some studies reported EVD placement as a risk factor for shunt dependency,^{18,19,23,24} we recommend performing a subgroup analysis (general patients group and EVD placement group). Thirdly, though the study from Lai and Mor-

FIG. 1. Forest plot for female sex as a risk factor for shunt dependency after aSAH. Figure is available in color online only.

gan had the largest population, the selection bias was not a negligible issue. Since the Australian National Hospital Morbidity Database was unable to track the data on aSAH patients beyond discharge from acute-care hospitals, data on patients with delayed or chronic hydrocephalus might not be captured.¹³ This bias may explain the reason that study reported the lowest shunt rate (6.5%) among all the included studies. Therefore, we recommend performing a sensitivity analysis by excluding that study to verify whether the results remain robust.

Fourthly, female sex did not reach statistical significance as a risk factor in this meta-analysis. However, according to the study inclusion criteria, some more studies should be considered eligible.^{1,7,10,11,18,25} When adding data from these studies, we detected a significantly increased risk of shunt dependency in female aSAH patients (Fig. 1; OR 1.28, 95% CI 1.12–1.46).

Fifthly, it was suggested that neurosurgical clipping might improve cerebrospinal fluid circulation and reduce the risk of shunt dependency by early evacuation of cisternal clot and blood products.⁹ However, Wilson et al. did not explore whether treatment modalities could influence the risk of shunt dependency.

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Disclosures

The authors report no conflict of interest.

Response

We thank Dr. You and colleagues for their interest in our article. Indeed, there are many variables to be considered and we appreciate their suggestions.

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Evoked potentials and Chiari malformation Type 1

TO THE EDITOR: We read with interest the study by Moncho et al.⁹ (Moncho D, Poca MA, Minoves T, et al: Are evoked potentials clinically useful in the study of patients with Chiari malformation Type 1? *J Neurosurg* [epub ahead of print April 15, 2016. DOI: 10.3171/2015.11.JNS151764]). In their retrospective analysis of prospectively collected data, the authors suggest that evoked potential aberrations in preoperative workup of patients with Chiari malformation Type 1 (CM-1) do not contribute to establishment of treatment algorithms, yet somatosensory evoked potential (SSEP) and brainstem auditory evoked potential (BAEP) changes may help to establish evidence of subclinical dysfunctions.

In the accompanying editorial,¹ Dr. Adelson describes the complexity of decision making in these cases and the inability of electrophysiological testing to indicate either dysfunction or disease progression, which also emphasizes the unestablished indications for preoperative nerve monitoring evaluation.

The role of intraoperative evoked potential monitoring in CM-1, traditionally using SSEP and BAER modalities (auditory brainstem response), remains, in our opinion, unclear. In our recent study² we addressed this issue in addition to questioning the potential benefit of adding transcranial motor evoked potential (TcMEP) monitoring to the equation in an attempt to improve sensitivity and specificity to detect postoperative deficits. We presented data that demonstrate that the use of multimodality intraoperative

neurophysiological monitoring (INM), including TcMEP, could be beneficial in detecting and avoiding iatrogenic injury stemming from inappropriate patient positioning and could possibly contribute to the decision-making process of deciding when adequate decompression has been achieved on an individual-patient basis.

As was the case for image-guided neuronavigation that began as a useful tool in neurosurgical procedures and today is widespread and often considered standard of care, INM is rapidly becoming a “must” in various surgeries, including treatment of brainstem and spinal cord tumors,^{6,7,10} tethered cord syndrome,⁴ scoliosis and other spinal deformities,^{3,8} syringomyelia,¹³ and degenerative cervical spine disorders,⁵ as well as supratentorial surgery.^{11,12}

In the modern era of neurosurgery, and when taking into account factors of “defensive medicine,” prioritization in the use of INM as a surgical adjunct must be addressed. In this light, we see Moncho and colleagues’ study as an opportunity to call attention to this dilemma and urge further large-scale studies regarding the role of electrophysiological testing in CM-1 both preoperatively and intraoperatively.

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Disclosures

The authors report no conflict of interest.

Response

We would like to thank Dr. Barzilai and colleagues for their letter regarding our recent paper on the BAEP and SSEP alterations found in patients with CM-1. We are familiar with Dr. Barzilai's studies in the field of INM in treating pediatric CM-1,⁴ and therefore his interest and comments on our work are greatly appreciated. At the same time, his letter gives us the opportunity to broaden the discussion and address the still-controversial topic of the usefulness and clinical relevance of intraoperative evoked potential monitoring in patients with CM-1.

The application of INM has expanded rapidly over the past 2 decades, as seen in the large number of studies published in different disciplines, including neurosurgery, neurophysiology, orthopedic and vascular surgery, otolaryngology, and neurology. The main objective of INM is to protect the vulnerable neural structures by allowing early detection of reversible neurophysiological dysfunction during surgery, thus preventing permanent neurological damage. INM is a very interesting field, not only from a clinical point of view, but also because it is an attractive instrument for research.¹⁰ At present, however, there is still much to learn and do in order to demonstrate the real benefit of these techniques in some pathologies. Given the current low volume of evidence, we do not agree with Dr. Barzilai and colleagues' opinion that INM may be used as an instrument of "defensive" medicine. As far as we know, no studies have shown the sensitivity, specificity, and predictive values (either positive or negative) of INM in CM-1 patients. This is a requirement for any diagnostic tool to be routinely used in the operating room. In addition, INM increases anesthesia, surgical time, and monetary costs and, when applied to CM-1 surgery, can lead to erroneous, or at least questionable, decisions.

Some authors have proposed INM during surgery in patients with CM-1, mainly in the following 3 scenarios: 1) during placement of the patient before surgery, 2) to determine when adequate decompression has been achieved

and therefore to design surgery on an individualized basis, and 3) to detect intraoperative evoked potential worsening.

For surgical positioning in patients with CM-1, the rationale for intraoperative evoked potential monitoring is that it can diminish the risk of neurological injury at a time when it can be reversed. Anderson et al. reported the case of a 14-year-old patient with CM-1 and a holocord syrinx who underwent suboccipital decompressive craniectomy and in whom surgical positioning had to be modified after a dramatic deterioration of baseline SSEPs.² After the patient's neck was repositioned, the left median nerve potential improved but did not return to baseline. Postoperatively, the patient had decreased proprioception of the left arm that persisted for 2 weeks.² In that case report, the first figure shows that the patient had severe CM, with the obex below the foramen magnum, significant tonsillar descent, and possible basilar impression with anterior compression. Using the new classification, this patient would have been included in the category of CM Type 1.5 (CM-1.5), or even been diagnosed as having a complex craniocervical junction malformation. Other studies have reported that the patients with impaired evoked potentials during surgical positioning were those with complex craniovertebral junction abnormalities.⁹

At our center, surgery is always performed with the patient in the prone position, with cranial flexion and discrete cervical distraction, and the head fixed in the Mayfield headholder. In all patients, a tolerance test is always performed before surgery: the patient is asked to maintain cervical hyperflexion—similar to that used during surgery—for at least 2.5 hours while they carry out a routine activity, such as reading. The few patients reporting neurological symptoms during this test are placed with their neck in a neutral position. After treating more than 300 patients with CM-1, we have not detected any neurological complications related to head positioning. However, we agree with Dr. Barzilai and colleagues that evoked potential monitoring during head positioning may be useful in some CM patients, especially those with more severe malformations who are at risk for neck flexion.

The most controversial aspect of this topic is, in our opinion, the use of INM to limit the degree of posterior fossa decompression (PFD) and guide the decision of whether or not to open the dura mater. Some authors have argued that an extradural approach—that is, suboccipital craniectomy with a C-1 laminectomy and resection of the fibrous band at the level of foramen magnum, with eventual serial incisions of the outer layer of the dura without opening it—is enough to relieve the pressure gradient at the craniocervical junction and improve clinical symptoms.⁵ Intraoperative ultrasonography and/or BAEP monitoring have been used by several authors to decide whether to open the dura in CM patients, especially children. Zamel et al. reported that PFD involving bone removal alone significantly improved conduction time in BAEPs in most pediatric patients with CM-1¹² and that the use of duraplasty allowed for a greater improvement in conduction time in only 20% of patients when compared to surgery involving bone decompression alone.¹² Anderson et al. reported improved BAEP conduction times in most patients who had undergone PFD with duraplasty, but the authors stated that

the majority of improvements were also observed after bony decompression.³ These results suggest that opening the dura mater is not necessary in the treatment of CM, especially in patients without syringomyelia. However, in the Zamel series, postoperative brain MRI studies were only available for 55 of the 80 patients treated and showed normalization of the position of the cerebellar tonsils in only 54.5% of patients in whom a pure extradural approach was taken; this rate increased to 84% in patients who underwent duraplasty.¹² Similar negative findings were observed in the position of the cerebellar tonsils on the postoperative MRI studies of most of the 30 pediatric patients with CM-1 treated by Caldarelli and colleagues using a purely extradural procedure.⁵ Taking into account these results, we cannot consider that optimal treatment was applied based on evoked potential findings.

It is well known that opening the dura mater and carrying out a dural graft increase the risk of CSF leakage, pseudomeningocele, and aseptic meningitis. However, restoring “normal” CSF circulation through the foramen magnum is essential in the surgical treatment of CM. It is well established that PFD and duraplasty achieve the best results in the treatment of CM, regardless of whether it is associated with syringomyelia or not,^{1,7,11} in both adults and children. This has also been the case in most CM-1 patients admitted to our department for a second-look surgery after an extradural procedure (see example in Fig. 1).

It is also important to note the potent osteogenic effect of the dura mater in the process of calvarial regeneration,⁶ which can explain the partial posterior fossa reossification observed in some pediatric patients with CM-1 treated without duraplasty (Fig. 2). In terms of security, we want to emphasize that when surgery involves wide suboccipital craniectomy, resection of the posterior arch at C-1 and opening the dura with preservation of the arachnoid membrane (a surgical technique we named “posterior fossa reconstruction” in 1994)⁸ produces excellent morphological results, with cranial ascent of the hindbrain, good clinical outcomes, and minimal complications. This technique has been used in most patients who undergo this type of surgery at our institution for the last 20 years.

With regard to the use of evoked potentials for monitoring neurological worsening during surgery, Zamel and co-workers’ study reported that none of the 80 children with CM-1 in whom BAEPs were monitored showed any significant worsening during surgery that would have prompted the surgical team to modify their surgical strategy.¹² Similarly, of the 22 patients of the Barzilai et al. study, none had evoked potential alterations during surgery, although 3 patients displayed significant SSEP attenuation concomitant with patient positioning.⁴ These findings confirm that the incidence of neurological complications once the patient is positioned is very low⁹ and therefore raise doubt about the cost-effectiveness of this procedure.

FIG. 1. Presurgical MR image (A) and sagittal CT scan (B) in an 11-year-old boy with CM-1.5 and a moderate basilar impression, in whom a PFD was performed at another institution without opening the dura mater (B). The postoperative control images 4 years after surgery (C and D) did not show any change in the cerebellar tonsil position or in the size and extension of the syringomyelia. The patient underwent a second surgery at the age of 16 years, involving an increased occipital bone resection, opening of the dura mater, and a wide duraplasty. The postoperative MR images (E and F) showed cerebellar remodeling with a large pseudocisterna magna, a significant tonsillar repositioning, and a small residual syrinx.

FIG. 2. A 6-year-old girl with a treated syndromic craniosynostosis who, when she was 6 months old, underwent PFD without opening the dura mater. Follow-up MR image (**A**) obtained when she was referred to our institution at the age of 4. At the age of 6, a follow-up CT scan showed a partial reossification of the occipital bone (**B and C**). Figure is available in color online only.

We completely agree with Dr. Barzilai and colleagues' opinion that there is a need to establish an optimal, cost-effective monitoring protocol for posterior fossa surgery in patients with CM-1.⁴ However, the evidence to date shows that the only potential benefit of evoked potential monitoring in CM-1 is during patient positioning and only in a small number of patients who could most likely be identified before surgery, making the routine use of INM difficult to justify.

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Whole-brain CT perfusion and subarachnoid hemorrhage

TO THE EDITOR: We read with keen interest the article by Malinova et al.³ (Malinova V, Dolatowski K, Schramm P, et al: Early whole-brain CT perfusion for de-

tection of patients at risk for delayed cerebral ischemia after subarachnoid hemorrhage. *J Neurosurg* 125:128–136, July 2016). The authors have described the utility of whole-brain CT perfusion (CTP) in predicting the occurrence of delayed ischemic neurological deficits and delayed cerebral infarction in patients with acute subarachnoid hemorrhage (aSAH). We commend the authors for their study, which sheds light on the enigma of delayed ischemic neurological deficits and delayed cerebral infarction following aSAH. In days to come, CTP may become part of routine study in patients with aSAH.

Global cerebral edema (GCE) occurs after aSAH and is associated with functional and cognitive dysfunction. It usually occurs during the early phase (0–3 days) after aSAH. GCE is identified on CT scan by the following characteristics: 1) effacement of hemispheric sulci and basal cisterns, and 2) bilateral and extensive disruption of the cerebral gray-white matter junction at the level of the centrum semiovale.^{1,2} Patients with GCE have global perfusion deficits when compared to patients without GCE.¹ This may affect early CT perfusion studies, and it probably was not considered by the authors. The number of such patients and the CTP findings in such patients should be addressed.

Acute hydrocephalus after aSAH is known to be associated with reduced cerebral blood flow in the deep gray matter and periventricular white matter.⁴ This could also have affected the results, and interpretation of this study and the number of patients with hydrocephalus have not been mentioned.

The authors' study has very few patients with Fisher Grade 2 SAH and no patient with Fisher Grade 1 SAH, and it may be very difficult to generalize the result of this study to patients in these two groups. Thus, a study with a larger patient population and one composed of these two groups needs to be done, considering the radiation and the use of contrast agents for CTP studies.

We once again commend the authors for this study and concur with them that a larger study group is required to compare various subgroups and to analyze the effects of CTP on outcome of patients with aSAH.

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Disclosures

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Response

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

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Recurrent or residual craniopharyngioma: management options

TO THE EDITOR: I congratulate Dhandapani et al.¹ for their excellent article (Dhandapani S, Singh H, Negm HM, et al: Endonasal endoscopic reoperation for residual or recurrent craniopharyngiomas. *J Neurosurg* [epub ahead of print May 6, 2016. DOI: 10.3171/2016.1.JNS152238]).

I want to comment on certain aspects of their study. The authors concluded that the endonasal endoscopic transphenoidal approach for residual or recurrent craniopharyngioma results in resection and visual outcomes similar to those following first-time operations.¹ As the extent of resection is influenced by prior radiation, they suggested that endonasal reoperations are preferred over radiation. However, the overall rates of gross-total resection (GTR) and GTR + near-total resection (NTR) in their study were 80% (28/35) and 86% (30/35) among primary tumors compared to 55% (12/22) and 68% (15/22) among reoperations. These overall outcomes may be more relevant than the rates calculated based on the 46 cases intended for GTR, especially since radiation is the standard option applicable for all residual or recurrent cases.² As the endonasal reoperations involved patients both with and without prior radiation, the impacts of reoperation and radiation had some overlap. Hence, this differential impact would probably become clearer if the authors had presented the resection rates and visual outcomes among reoperations without prior radiation and reoperations following radiation separately in comparison to primary surgery.

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References

1. Dhandapani S, Singh H, Negm HM, Cohen S, Souweidane MM, Greenfield JP, et al: Endonasal endoscopic reoperation for residual or recurrent craniopharyngiomas. *J Neurosurg* [epub ahead of print May 6, 2016. DOI: 10.3171/2016.1.JNS152238]
2. PDQ Pediatric Treatment Editorial Board: Childhood craniopharyngioma treatment (PDQ®): Health professional version. **PDQ Cancer Information Summaries.** (<http://www.ncbi.nlm.nih.gov/books/NBK65891/>) [Accessed October 27, 2016]

Disclosures

The author reports no conflict of interest.

Response

We very much appreciate the thoughtful comments of Professor Srinivasan. Two questions were raised. The first is whether it would have been more accurate to report rates of GTR and GTR+NTR for all reoperations rather than just those in which GTR was the goal of surgery. The second question is whether the results should have been presented separately for those who received prior radiation versus those who had not received prior radiation therapy.

To address the first question, we wish to highlight that our conclusions only support reoperation over radiation therapy for those craniopharyngiomas for which the surgeon estimates that GTR can be achieved. If GTR cannot be achieved, and there are no symptoms of mass effect, then radiation is often the more reasonable treatment option. However, in the subset of patients in whom GTR *can* be achieved, our results show that success is as likely as in a first-time operation. For this reason, we separately reported the results of patients in whom GTR was the goal of surgery. To report rates of GTR in patients undergoing palliative debulking does not accurately reflect the success or failure of the intended goals of surgery.

The second question was more of a suggestion to show the results of reoperation in patients who received prior radiation therapy compared with those who did not. We presented our data in just this manner and found that prior radiation reduced the chances of achieving GTR. This result further supports our contention that reoperation should be offered before radiation therapy for recurrent craniopharyngiomas if GTR can be achieved since waiting until after radiation therapy will reduce the chances of success.

We would also like to challenge Prof. Srinivasan's statement that radiation is the "standard option" and the implication that attempts to pursue any other treatment course should not be undertaken. Such assumptions are based on reviews of the existing literature. However, the scientific literature is a dynamic phenomenon, and the precise goal of our article is to challenge the current literature by presenting updated results.

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