Neurosurgical Forum LETTERS TO THE EDITOR

Arterial spin labeling in evaluation of venous drainage pattern in brain arteriovenous malformations

TO THE EDITOR: I read with interest the article by Blauwblomme et al.¹ (Blauwblomme T, Naggara O, Brunelle F, et al: Arterial spin labeling magnetic resonance imaging: toward noninvasive diagnosis and follow-up of pediatric brain arteriovenous malformations. *J Neurosurg Pediatr* 15:451–458, April 2015). In this article, the authors used arterial spin labeling (ASL) in 21 patients with brain arteriovenous malformations (AVMs) and used cerebral blood flow (CBF) values to determine the nidus

location and patency of AVMs after treatment. They also recommended that absolute quantification of CBF values could be relevant in the follow-up of pediatric brain AVMs after partial treatment.

One of the most important factors in the appearance of the ASL perfusion map is the use of additional crusher gradients, which can be used prior to image acquisition to null the signal from intravascular spins. By nulling the residual intravascular tagged signal, the blood-flow maps will be reflective of the protons deposited in the tissues, which allows more accurate quantification of CBF; however image acquisition without crusher gradients allows direct visualization of arteriovenous (AV) shunting and the draining veins,⁴ which can be very helpful in the diagnosis of new or residual/recurrent AVMs (Fig. 1). Le et al. demonstrated

FIG. 1. **A:** Axial T2-weighted image obtained in an 11-year-old patient demonstrating hemorrhage in the medial right temporal lobe associated with multiple tiny flow voids (arrow). **B and C:** Contrast-enhanced T1-weighted MR image (B) and source image of time-of-flight MR angiography (C) demonstrating early subacute blood products and tiny foci of flow-related enhancement (B and C, arrow). **D and E:** ASL images demonstrating increased CBF at the AVM (D, arrow) and a linear hyperintense structure coursing posteriorly from the AVM to the sinus confluence and right transverse sinus (E, arrows) indicative of AV shunting with deep venous drainage. **F:** Conventional angiogram confirming the ASL findings of an AVM (F, black arrow) with deep venous drainage via a prominent posterior mesencephalic vein (F, white arrow).

that identifying venous ASL signal intensity improved detection of dural arteriovenous fistulas and small AVMs.² Our group also evaluated 19 patients with AV shunting and demonstrated the added value of ASL imaging without crusher gradients when compared to conventional sequences for detection of AV shunting.³ Some studies even used ASL imaging without crusher gradients to quantitate the AV shunting.⁵ In conclusion, ASL imaging without crusher gradients can provide direct evidence of AV shunting and venous drainage, which can have an important role in the initial diagnosis and follow-up of AVMs.

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Disclosures

The author reports no conflict of interest.

Response

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

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Intrathecal baclofen trials: complications and positive yield in a pediatric cohort

TO THE EDITOR: We read with interest the article by Sayer and colleagues⁵ (Sayer C, Lumsden DE, Perides

S, et al: Intrathecal baclofen trials: complications and positive yield in a pediatric cohort. *J Neurosurg Pediatr* 17:240–245, February 2016). The authors performed a retrospective case review to examine the outcomes and complications in intrathecal baclofen (ITB) trials for childhood dystonia. They found that the rate of positive trials in this study was 98%, of which 21% did not progress to pump implantation. Although still advocating for ITB trials prior to ITB pump insertion to aid parental decision making, Sayer et al. believe that this response rate suggests that with good patient selection, ITB pumps could be placed without a preceding trial.

However, there was no mention of the pre-trial oral baclofen doses in the patients prior to the initiation of ITB trials. In general, ITB infusion is considered only when oral baclofen causes intolerable side effects or systemic toxicity at the dose level required to achieve therapeutic response.¹ We believe that the pre-trial oral baclofen dosage information is critical, especially when considering a very high positive trial rate. We wonder if the reported positive response rate of 98% was partially contributed to by prior systemic baclofen doses that had not yet been excreted.

Furthermore, in patients who were taking high doses of baclofen for severe spasticity, completely tapering off oral baclofen could be extremely difficult to achieve. In some cases, weaning off their oral baclofen could predispose them to complications such as skin breakdown and serious withdrawal effects.² We wonder if any of those patients who went through tapering off of oral baclofen encountered any significant clinical problems. In our practice, we have encountered a case of severe spasticity due to multiple sclerosis,⁴ that was minimally responsive to baclofen (60 mg 3 times daily) and tizanidine (4 mg 3 times daily), in which completely tapering off oral baclofen was deemed unfeasible. The patient had previously experienced severe spinal headache following lumbar puncture and therefore she firmly refused an ITB trial. We performed an outpatient epidural infusion trial that lasted 12 days, in which her oral baclofen was slowly tapered off while the epidural baclofen rate was gradually titrated up to efficacy.⁴

Another point we would like to comment on is the trial protocol (i.e., “Patients were encouraged to lie flat in bed for a minimum of 12 hours after LP [lumbar puncture] and following catheter removal, as well as with the catheter in situ.”). We believe a strong focus during the trial should be placed on evaluating how patients are doing during their usual activities of daily living. There were reported cases of worsening ambulation or transfers following permanent ITB pump placement following ITB bolus trial, because some of the tonicidity was used by patients to perform pivot transfer or assist ambulation, and when their lower extremities became totally flaccid with ITB infusion, they lost their ambulating abilities.³

This underscores the importance of performing a thorough evaluation, including functional improvement during a patient’s activities of daily living, rather than a mere reduction of Modified Ashworth Scale scores. In this regard, we believe that the conclusion of the study by Sayer et al., that ITB pumps could be placed without a preceding trial, is not advisable.

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The authors report no conflict of interest.

Response

We are grateful for the valuable comments on our article by Drs. Ruan and Kaye and for a voice in this important discussion.

As per our inclusion criteria, an ITB trial is offered after other treatment modalities—therapy, botulinum toxin injections when applicable, and oral/enteral medications including baclofen—were tried and provided insufficient clinical benefit or were not tolerated. The patients would have usually tried baclofen and one or more other tone-managing medications. However, we have not collected and analyzed data related to enteral medications and their doses for the purpose of this paper, which could be an interesting subject indeed.

During the trial we endeavored to document the benefit of ITB in addition to existing enteral medications. We did not routinely stop or taper off existing medications, including enteral baclofen, before the trial. We agree with Ruan and Kaye that the process can be very difficult or even unsuccessful. We had patients in whom one or two doses were omitted after the ITB test dose administration, if the effect was particularly powerful or signs of overdose occurred. In our cohort the patients were gradually weaned off enteral baclofen after implantation of the ITB pump and alongside titration of the infusion rate to efficacy, in order to avoid withdrawal symptoms.

We encourage patients to lie in bed for 12 hours following lumbar puncture and with an intrathecal catheter in situ to reduce the risk and severity of spinal tap syndrome. In our study we report a severely affected group of pediatric patients, mostly nonambulant; 38 of 41 were classified as Gross Motor Function Classification System (GMFCS)

Level IV or V (Level IV in 5, Level V in 33).¹ For these patients the goal areas were mostly related to comfort and ease of care. Care activities such as dressing or nappy change are usually performed by caregivers with the child lying on a bed. Position can be changed on a bed or a patient can be seated in a chair briefly to document reduction of difficulty, returning promptly to lie in bed. We acknowledge that in ambulant patients, assessment of function such as walking or transfers will have to be undertaken, and therefore bed rest will be more difficult to execute.

The suggestion that an ITB pump could be implanted without a trial in patients who are carefully selected by experienced professionals was made in regard to patients with hypertonia in the most severely affected groups (GMFCS equivalent Level IV and V), where efficacy is well documented in relation to comfort, positioning, and in alleviating difficulties in care delivery by caregivers. In our paper we agree that the outcomes of ITB therapy in more functional ambulant patients are less well reported and more difficult to predict.² In these cases and in all others, in which the benefits of ITB therapy are less certain or an ITB trial is essential in decision making by a patient and/or family, the test dose should remain an important step in patient selection.

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Endoscope-assisted hemispherotomy and corpus callostomy

TO THE EDITOR: We read the article by Sood et al.⁴ with interest (Sood S, Marupudi NI, Asano E, et al: Endoscopic corpus callosotomy and hemispherotomy. *J Neurosurg Pediatr* **16**:681–686, December 2015).

We have are very happy to note that the concept of the technique (which has been described by us) of performing endoscopy-assisted hemispherotomy and corpus callostomy has been reproduced in this study, demonstrating its efficacy. However, we wish to point out certain things.

In *Discussion*, the authors state, “Since the submission of this paper, Chandra et al. have described their experience with endoscopic hemispherotomy.”²⁴ We do not understand how the authors claim their originality on this technique when our paper was already published¹ before their own paper (date of acceptance of our paper: January 4, 2015, vs date of acceptance of Sood et al.’s paper: May 11, 2015). Even the PubMed publication shows our paper to have been published April 2015, while Sood and colleagues’ paper was published September 2015. We request that the authors acknowledge and correct this in the proper perspective. Following this paper, we also published a fairly large series on endoscopy-assisted corpus callosotomy with commisurotomy (n = 16) and then subsequently also a review article.^{2,3}

The authors claim a variation of using a bimanual technique. We wish to state that, currently, we now have experience with performing more than 25 hemispherotomies using a free-hand technique. When we devised the procedure, we went through the learning curve of using a single port versus a bimanual technique and finally settled on a 2-handed technique of suction and bipolar with the endoscope held rigidly by a holder. We find this technique most suitable for all types of pathologies, including hemimegalencephaly and nonatrophic pathologies. A bimanual technique may be adequate for atrophic pathologies but

becomes challenging for pathologies like hemimegalencephaly and Rasmussen’s and cortical dysplasias. In our series of 25 cases, 15 cases belong to this category (Fig. 1). All of our procedures were performed using neuronavigation with the use of intraoperative MRI. Postoperative MRI in all cases confirmed a complete disconnection. We are now closely following up our cases and will publish on them after an adequate long-term follow-up. Currently, we are performing all our hemispherotomies using our described technique.

We feel that the evolution of the endoscope-assisted hemispherotomy would be the same as that for pituitary surgery. This started with the initial use of use of endoscopy through cannulas. However over a period of time, the best method was found to be a free-hand technique, which provides much more flexibility and maneuverability. Having said this, it does depend a great deal on the surgeon’s level of comfort.

Sood et al. thought that the size of the craniotomy would be larger with our described technique. The size of our craniotomy was the same as that described by them. Furthermore, we recommend that they, like us, should use the term “endoscope-assisted hemispherotomy” rather than endoscopic corpus callosotomy and hemispherotomy. “Endoscopic procedure” essentially refers to a surgical technique through a bur hole.

FIG. 1. An endoscopic technique in a nonatrophic pathology. A 6-year-old boy presented with seizures that began when he was 2 years old. **A and B:** MR images showing evidence of large areas of cortical dysplasia in the frontal, insular, and parietal lobes with polymicrogyria on the left side. **C:** Magnetoencephalography (left) showed a tight cluster over the left (L) frontal lobe (red area). The child underwent an endoscope-assisted left-sided hemispherotomy. **D–G:** Postoperative MR images obtained on Day 6 showing the hemispheric disconnection. The track of access (D), disconnection at the level of the ventricles on axial section (E), disconnection as seen on the coronal section at the level of the temporal horn (F), and disconnection at the posterior part at the level of the thalamus (G) are shown. The MR images clearly show the efficacy of this technique where there is evidence of a clean hemispheric disconnection without any apparent damage to the surrounding area. In addition, it can be seen that the endoscopic technique has been used effectively even in a nonatrophic pathology. Figure is available in color online only.

Finally, the authors stated that we use the sphenoid ridge and planum as landmarks. As stated in our paper in great detail, we use the junction of the genu with the caudate head as the landmark for the beginning of the anterior disconnection at the level of the anterior cerebral artery (ACA). Using this landmark, under neuronavigation, we proceed downward until we reach the posterior part of sphenoid ridge. Essentially, our internal landmark is also the ACA as later described by Sood et al.

While congratulating Sood et al. on performing the technique of endoscope-assisted hemispherotomy and corpus callosotomy, we wish to reiterate that this is not an original technique described by them, but it has helped the literature in changing the paradigms of management for this complex pathology.

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Disclosures

The authors report no conflict of interest.

Response

We want to thank Drs. Chandra and Tripathi for their constructive comments. We would like to bring to their attention that our paper was originally submitted to *Journal of Neurosurgery: Pediatrics* in January 2015. We became aware of their work months after, when their paper was published online.¹ Indeed, we added their paper to the reference list of our final article and stated that there were critical differences in the methodologies as highlighted in our paper and their letter. In this regard we feel we have appropriately cited and acknowledged their work. The evolution of our work was independent of theirs and is a continuation of our description of using an endoscope with mounted suction along the interhemispheric fissure for transcalsallosal resection of intraventricular and thalamic tumors.³

Both papers¹ describe the application of an endoscope in somewhat different ways to perform a slight variation on vertical hemispherotomy originally described by Delalande et al.² While Chandra et al. stated the use of a holder/assistant to hold and maneuver the endoscope,¹ we

have used a suction with the endoscope attached, which the surgeon can maneuver using his/her left hand to the required field of surgery and, at the same time, use the attached suction to bimanually perform the procedure. This is conceptually different from their method,¹ which is traditionally used in transsphenoidal procedures, where the position of the endoscope does not need to be frequently changed since the field of surgery is small and is essentially the same as the field of vision through the endoscope. However, for hemispherotomy, where the field of surgery is far greater than the view through one position of the endoscope, frequent repositioning of the endoscope can be frustrating. Furthermore, it may result in crowding of instruments and necessitate a larger opening, as it appears to be on the coronal MRI submitted by Chandra et al. with their letter (Fig. 1G) and Fig. 7 of their article.¹ Finally, with a fixed endoscope, reintroducing an instrument into the field is done blindly until it reaches beyond the tip of the endoscope where it can be seen. Alternatively, the endoscope has to be unmounted and remounted, which is somewhat tedious even with use of a robotic arm. We fully acknowledge that Chandra et al.¹ have developed considerable experience with their technique. On the other hand, we have found the use of an endoscope with mounted suction far more convenient. In addition, the ability to follow the ACA laterally to the internal carotid artery bifurcation and middle cerebral artery, in continuity to the medially explored ACA, rather than identifying the sphenoid ridge on neuronavigation, seems more definitive and desirable to us. Over the past year, we have added another dimension to the procedure with the use of 3D Visionsense Endoscope (Visionsense Ltd.). The improved depth perception is a clear advantage over 2D and neuronavigation. Rather than using the traditional transverse incision used by Chandra et al.,¹ we favor a vertical incision just lateral to the midline starting behind the hairline. The 3-cm incision allows us to perform a 2.5- to 3-cm craniotomy as seen in Figs. 1 and 4 of our article. This incision tends to be less painful and does not cause numbness from sectioning of supraorbital nerve branches as may occur in a traditional transverse incision.

Finally, the use of terms such as “minimally invasive surgery,” “endoscope-assisted surgery,” and “endoscopic surgery” as alluded to by Chandra et al. in their letter are somewhat confusing. To our knowledge, “minimally invasive surgery” does not truly describe how the surgery was done and appears most apt for spine surgery, where small incisions are used for instrumentation done under navigation guidance. The concept that “endoscopic surgery” needs to be done through a bur hole is antiquated. In our opinion, “endoscope-assisted surgery” refers to surgery primarily performed with a microscope while an endoscope is used intermittently during the procedure. Conversely, “endoscopic surgery” refers to the use of an endoscope as the major surgical tool; therefore, we prefer to refer to our procedure as such. In reality, since retractors are used during the procedure, one could also refer to it as retractor-assisted endoscopic surgery. This is really a question of semantics rather than scientific endeavor.

We would like to commend Chandra et al.¹ for their effort in the development and improvement of their surgical

techniques. It is heartening to see how similar ideas can develop in different institutions and how exchange of these ideas through publications further advances the field.

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