Neurosurgical Forum LETTERS TO THE EDITOR

Multiple meningiomas

TO THE EDITOR: We read with great interest the article by Tsermoulas et al.⁶ (Tsermoulas G, Turel MK, Wilcox JT, et al: Management of multiple meningiomas. *J Neurosurg* [epub ahead of print July 21, 2017. DOI: 10.3171/2017.2.JNS162608]) regarding the management of multiple meningiomas. The authors performed a retrospective observational study and described their comprehensive management strategy, providing details on epidemiology, decision making, and treatment options. The concept of multiple meningiomas was first proposed by Cushing and Eisenhardt in 1938,² and researchers began to focus on this field thereafter. The character of multifocality, however, contributes to the complexity and challenge of the management of these cases. Tsermoulas and colleagues should be commended, because their study helps to address these issues by presenting comprehensive evidence.

We agree with the management principles of this study in general as well as with the authors' point that patient- and tumor-specific factors should be considered in the decision-making process. With respect to radiotherapy, we believe that the potential benefits of this treatment should be balanced against the risks, because meningiomas can be both induced and treated by radiation. As described by the authors, in 26 (20%) of the 133 patients included in the study—with 110 (25%) of the total of 448 meningiomas—the tumors were considered to be associated with prior radiation at baseline, and a total of 27 patients with 55 meningiomas underwent radiotherapy modalities (in the form of Gamma Knife stereotactic radiosurgery [SRS] or fractionated radiation therapy).⁶ Radiation-induced meningiomas have been reported to develop decades after the original radiotherapy in a substantial proportion of surviving patients who were treated with brain radiotherapy in childhood.^{1,3,5} According to Strojan et al.,⁴ the latency period between radiation exposure and development of radiation-induced meningioma can range from 2 to 63 years. Given that the potential risks of radiotherapy as well as the finding that metachronous meningiomas were more common in the patients with radiation-induced tumors than in the sporadic meningioma group (62% vs 16%, $p < 0.001$),⁶ the treatment strategy should be chosen with caution, and longer follow-up is necessary for patients with multiple meningiomas associated with prior irradiation.

We note that 79 of the patients in this study were not treated at initial presentation and were followed instead. Of these 79, 31 (39%) required treatment at a later stage. Are there any differences between this group and the other 48 patients? More details may need to be explored in order to detect the clinical and radiological features that could predict a greater likelihood of the need for additional treatment. Patients who have a higher risk of requiring treatment of multiple meningiomas over their lifetime may need more vigorous surveillance.

A related issue that we would like to mention is that there is an interesting phenomenon among patients with multiple meningiomas called “mother-son tumor,” which was reported by Tian et al.⁵ Although Tsermoulas and colleagues did not provide the precise sizes of the tumors in their case series, they provided information about 67 patients with signs or symptoms attributed to one of their tumors, always the largest one. In our clinical practice, the type of multiple meningioma case that we encounter most often is a large meningioma in the presence of one or more small ones. The exact mechanism for this phenomenon is yet to be understood. Further studies are warranted to confirm and elaborate on this finding.

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Disclosures

The authors report no conflict of interest.

Response

We thank Dr. Wang and colleagues for their comments. The management of multiple meningiomas can be challenging, and the strategy we proposed was to treat the symptomatic tumors or the ones likely to become symptomatic. The rest should be observed for a period of time tailored to the individual patient. This is the same philosophy behind the management of single meningiomas as well.

We agree that it may seem paradoxical to treat radiation-induced meningiomas with radiotherapy.² In our cohort, 22 of 26 patients with radiation-induced meningiomas had treatment; 16 were treated with surgery, 5 with Gamma Knife SRS, and 1 with a combination of surgery and SRS. Our treatment strategy for the radiation-induced meningiomas is resection when feasible. We consider SRS in tumors of appropriate size with high surgical risk, because the dose of radiation to the brain is clinically inconsequential and the control rate is satisfactory.¹ None of the patients with radiation-induced meningiomas had fractionated radiotherapy, because of the potential toxicity of re-irradiation and the fact that the long-term control rate of fractionated radiotherapy as primary treatment for meningiomas is not always satisfactory.³

Regarding the 79 patients who were not treated at presentation, 31 had surgery and/or radiotherapy during surveillance and 48 did not require treatment. We did not compare the 2 groups because our study was not powered to examine possible differences in their baseline characteristics, but we accept that it is valuable to be able to predict which patients will need treatment and therefore we analyzed the relevant data. We found that the patients who had treatment during observation were more likely to have radiation-induced meningiomas (55% vs 9%, $p = 0.003$), be younger (52 vs 62 years, $p = 0.007$), and be male (48% vs 14%, $p = 0.03$). Radiation-induced meningiomas are more likely to be treated during observation because they are more aggressive than sporadic meningiomas.⁴ The age difference is self-explanatory, as meningiomas have more time to grow and become symptomatic in young patients and the growth rate is lower in older patients.⁵ There was no statistically significant difference in the number of meningiomas at presentation ($p = 0.9$) or the proportion of patients with large and medium-sized tumors in the 2 groups ($p = 0.7$ and $p = 0.2$, respectively).

Wang et al. mention the “mother-son tumor” phenomenon and report that the cases of multiple meningiomas that they encounter most often are large tumors with one or more small ones, implying a common origin from a single large meningioma. In our study, however, more than

two-thirds of the patients (68%) did not have a tumor with a maximum cross-sectional diameter greater than 4 cm.

Finally, we agree that tumorigenesis of multiple meningiomas is not yet well understood and it is currently the subject of laboratory research in our center.

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Endoscopic approach for skull base malignancies: data sparsity

TO THE EDITOR: We were interested to read the article authored by Kim and colleagues⁵ (Kim YH, Jeon C, Se YB, et al: Clinical outcomes of an endoscopic transclival and transpetrosal approach for primary skull base malignancies involving the clivus. **J Neurosurg** [epub ahead of print June 2, 2017. DOI: 10.3171/2016.12.JNS161920]). The authors' purpose was to retrospectively review the clinical outcomes of patients who underwent an endoscopic endonasal approach to treat primary malignancies involving the clivus and to analyze prognostic factors for gross-total resection (GTR). As one of the important results, they found that the tumor laterality was a significant predictor of GTR in both univariable (OR 6.25, 95% CI 1.51–25.86) and multivariable (OR 41.16, 95% CI 1.12–1512.65) models, which is questionable. It has been reported that a large measure of association such as an OR with a substantially wide CI is yielded in studies with sparse data.^{3,4}

Sparse data means that there is not sufficient data in each combination of independent and dependent variables. The data sparsity would be more severe in the multivariable models because the number of combinations between variables is increased in these models, compared to the corresponding univariable model.⁴ Data sparsity in the study led to an important bias—namely, sparse data bias—which inflates the OR and makes the CI substantially wide.⁴ Based on the data provided by Kim and colleagues' Table 5, minor sparsity exists in the univariable model assessing the association between tumor laterality and GTR ($n_{\text{central GTR}} = 20$; $n_{\text{central STR}} = 4$; $n_{\text{paramedian GTR}} = 8$; $n_{\text{paramedian STR}} = 10$), where STR means subtotal resection, but the more severe data sparsity can be seen in the multivariable model, so that a huge OR with a substantially wide CI has been yielded (OR 41.16, 95% CI 1.12–1512.65). This means that the adjusted OR has been severely affected by a sparse data bias.

There are various methods to decrease the sparse data bias, but penalization via data augmentation is the most effective method; it was introduced in 2016.⁴ Hence, we respectfully suggest that Kim and colleagues apply the penalization method in their multivariable models to obtain a less biased OR with a narrow CI.

A take-home message for the readers is that the sparse data bias is common in medical research,^{1,2} and it should not be considered as a strong association. This bias can be controllable by advanced statistical methods.

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Disclosures

The authors report no conflict of interest.

Response

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

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Fluorescence-aided evaluation of nasoseptal flap perfusion

TO THE EDITOR: We have read with great interest the article by Chabot et al.² (Chabot JD, Patel CR, Hughes MA, et al: Nasoseptal flap necrosis: a rare complication of endoscopic endonasal surgery. *J Neurosurg* [epub ahead of print July 21, 2017. DOI: 10.3171/2017.2.JNS161582]). The authors are to be commended for their praiseworthy focus on a rare but sneaky complication in the use of the nasoseptal flap (NSF). Its introduction in 2006 revolutionized skull base transnasal surgery by expanding our reconstructive abilities as well as by dramatically reducing the rate of fearsome complications, namely CSF fistulas.³ The fact that the use of the NSF is reported to be a non-risk-free procedure, even when performed by the experienced hands of the renowned Pittsburgh group, must draw our attention to the potential etiologies of NSF failure. Apart from an incorrect NSF design and/or concomitant infection, the main cause of NSF necrosis is insufficient vascular patency. Two ischemic problems can arise: one at the time of surgery because of kinking of the flap vessels and one in a subacute or delayed fashion as a result of vasospasm. This latter issue can be addressed by avoiding postoperative hypotension. For dealing with the immediate vessel kinking, we propose a real-time method of checking perfusion of the flap by using vascular fluorophore distribution, a well-known technique in plastic and reconstructive surgery.⁵ Briefly, we have employed a slightly modified endoscopic filtering system for fluorescein detection to evaluate vascular perfusion of nasal mucosa and NSFs.¹ We have already reported on our results in the setting of the identification of lesions in the nasal mucosa related to hereditary hemorrhagic telangiectasia.⁴ After applying a customized yellow filtering system to the endoscope, we switch the emitting light to violet-blue frequencies via a dedicated filter in order to elicit fluorescein detection. A low dose of fluorescein (5 mg/kg) is then administered intravenously, and after about 15 seconds, vivid green staining is progressively visible in the perfused tissues. By comparing the synchronicity and amount of fluorescein staining of the NSF with the residual mucosa, we can infer effective perfusion of the pedicled flap.

The advantage of this technique lies in the real-time visualization of perfusion under viable operative light conditions, thus allowing one to precisely identify where perfusion may be blocked. Moreover, it is a fast (< 1 min) and low-cost procedure (< €5 per dose) with little to no learning curve since it employs instrumentation already available in most skull base centers worldwide. The potential

disadvantage of this technique is related to the interstitial distribution of the fluorescein, which stains the mucosal tissues for many hours, thus limiting repeated evaluations. This could be avoided through the use of indocyanine green (ICG) fluorescence, even if, unfortunately, it does not allow real-time visualization and entails higher operating costs. In our opinion, this easy, fast, low-cost technique may be helpful in detecting NSF hypoperfusion, at least during actual operative time.

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Disclosures

The authors report no conflict of interest.

Response

We thank Dr. Bongetta and colleagues for their interest in and comments on our recent article on the very relevant topic of the diagnosis and treatment of NSF necrosis. This group in Pavia, Italy, is to be commended for their pioneering efforts in the real-time visualization of NSF perfusion and their thoughtful commentary on the possible causes of flap hypoperfusion, particularly those that might be corrected intraoperatively, such as pedicle kinking and vasospasm. We fully agree that intraoperative fluorescence angiography has potential in the evaluation of vascular perfusion of NSFs, and for this reason, we have been conducting a study for the last 2 years to investigate the accuracy of intraoperative ICG fluorescence endoscopy in the prediction of postoperative flap necrosis for various types of pedicled intranasal flaps.

In this study, we administered 12.5–25 mg of intravenous ICG (Akorn) and used a non-FDA-approved endoscopic fluorescence imaging system (0° and 30° endoscopes with the IMAGE1 S H3-Z camera and D-light P Lightsource, Karl Storz) under an IRB-approved protocol.

Our preliminary impressions with this technology were presented at the North American Skull Base Society 2017 Annual Meeting and were recently submitted for publication (Geltzeiler M, et al: Evaluation of intranasal flap perfusion by intraoperative indocyanine-green fluorescence angiography. *Oper Neurosurg*, submitted).¹ The results have been promising thus far. For the first 38 patients enrolled in the study, all NSFs were evaluated at both the pedicle and the body of the flap by 3 blinded observers using 2 possible grades: high or moderate enhancement (“positive enhancement”) and low or no enhancement (“no enhancement”). The flap pedicle demonstrated the same or a higher degree of fluorescence than the flap body in all cases. All flap bodies graded as positive enhancement survived. Among those cases with no body enhancement but positive enhancement of the pedicle, 91.7% (11/12) of the flaps survived. Among the cases with no enhancement of either the body or the pedicle, 66.7% (2/3) of them developed flap necrosis postoperatively.

Therefore, body and pedicle fluorescence was correlated with postoperative flap necrosis ($p = 0.01$). The positive and negative predictive values for pedicle and body enhancement for postoperative flap necrosis were 97% and 67%, and 100% and 20%, respectively. Furthermore, body and pedicle enhancement was related to postoperative MRI enhancement ($p = 0.01$). However, there was no association between the degree of fluorescence intraoperatively and the occurrence of CSF leak postoperatively ($p = 0.90$), a result in line with the findings of Kang et al.² regarding NSF contrast enhancement on postoperative MRI and its correlation with flap failure.

It is key to emphasize that NSF necrosis, as described in our paper, is not an imaging diagnosis but a clinical entity that is characterized by a foul-smelling odor and the onset of acute meningitis, often with no obvious CSF leak, which typically occurs 1–3 weeks after surgery, and findings characteristic for a lack of flap enhancement on postoperative MRI studies. Early diagnosis and recognition are essential to prevent or treat meningitis and to establish the definitive treatment, which entails aggressive debridement (necrotic flap resection), new vascularized reconstruction with alternative flaps, such as the lateral nasal wall/inferior turbinate flap or pericranial flap, along with additional grafting with material such as fascia lata, and lumbar drain placement.

Despite the small size of the sample analyzed thus far, ICG fluorescence has the potential to identify those patients at risk for developing flap necrosis and to guide decision making for healing and the prevention of infectious complications.

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Supplemental Information

Previous Presentations

Part of the results in this letter were presented as an oral presentation at the North American Skull Base Society 2017 Annual Meeting held in New Orleans, LA, on March 3–5, 2017.

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Pullout complications with external ventricular drains

TO THE EDITOR: Velásquez et al.⁴ (Velásquez C, Rivero-Garvía M, Mayorga-Buiza MJ, et al: Avoiding pullout complications in external ventricular drains: technical note. *J Neurosurg* 126:1003–1005, March 2017) described their excellent and innovative technique for avoiding catheter misplacement and pullout complications in external ventricular drains (EVDs), two of the most common complications in children and adult patients after infection and obstruction.³

We agree with the authors that the best option to avoid infections or other complications due to pullout is to improve the technique, as the authors did. Reading their technique, we agree with the authors that the first step is the most critical because the EVD catheter can be damaged when it is being stapled. We suggest that the authors enhance their technique and modify this particular step by using a modified purse-string stitch, which we use in our patients at the General Hospital of Mexico.

This stitch is used to treat round or oval defects¹ in particular and secure the catheter, especially in cases of chest drains, with excellent results. Kim and Cho² proposed a new method using knotless sutures. We suggest that the stitching be performed as usual, using multifilament thread to keep the knot tense for a longer time. The needle is then inserted into the dermis at 3 points (anchored at each side of the catheter and another anchor above it). The knot is cinched down tightly enough to close the exit point but not so tightly that the lumen is closed. It is important to mention that this knot is not a square knot. The ends of the thread are crossed in a simple manner so that the knot can be tightened after catheter removal. We leave approximately 3 cm of free thread, which will secure the catheter as usual (crossing thread end A and thread end B around the catheter). At the end of the procedure, the catheter is secured to the scalp with a simple stitch.

When the EVD needs to be removed, the stitch farthest from the exit point is cut, and the ends are untied from the catheter. The catheter is removed carefully. When the

proximal tip is out of the exit point, the suture is cinched down to close the defect and secure the knot with a square knot over the previous simple crossing (i.e., the knot used when the EVD was placed) to tighten the defect and avoid the need of adding another stitch, which can be very traumatic, especially for kids.

This modified purse-string stitch helps avoid the most critical step in the technique proposed by Velásquez et al., and it is more comfortable for the patient because there is no need for another stitch.

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Disclosures

The authors report no conflict of interest.

Response

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

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Microsurgical anatomy in the reappearance of AVMs after complete resection

TO THE EDITOR: We read with keen interest the article by Aboukais et al.,¹ in which they discussed the distinction between a true recurrence and a missed remnant in the reappearance of arteriovenous malformations (AVMs) after treatment (Aboukais R, Vinchon M, Quidet M, et al: Reappearance of arteriovenous malformations after complete resection of ruptured arteriovenous malformations: true recurrence or false-negative early post-operative imaging result? *J Neurosurg* 126:1088–1093, April 2017). As the authors note, residual edema or hematoma can mask a remnant of a ruptured AVM.² They also point out that “a focal vasospasm or thrombosis could be

the reason for the absence of a visible residual AVM on angiograms taken immediately postoperatively,” noting that a hidden residual arteriovenous shunt could then be reactivated when the vasospasm resolves.

The article included angiographic images obtained in a patient with a Spetzler-Martin grade III ruptured AVM (their case 5) who suffered a recurrence, and we would like to discuss that case from the perspective of microsurgical anatomy. The digital subtraction angiography (DSA) images show that the AVM feeding arteries included the posterior parietal artery and angular artery arising from middle cerebral artery, supplying the nidus laterally, and the parieto-occipital artery arising from posterior communicating artery on the carotid angiography image, supplying the nidus medially. The drainage veins included the vein of Trolard, connecting to the nidus laterally and superiorly and draining into the superior sagittal sinus; the vein of Labbé, connecting to the nidus laterally and inferiorly and draining into the transverse sinus; and the middle temporobasal vein, connecting to the nidus medially and inferiorly and also draining into the transverse sinus. The areas involved by the AVM might have included the angular gyrus and superior occipital gyrus laterally and the cuneus and precuneus medially.

Based on the above anatomical analysis, we postulate that the reappearance of the AVM might be due to a remnant that was left after the surgical procedure. A parietal craniotomy might have been performed, and the resection might have begun with the lateral part of the nidus. Following the occlusion of the parietal artery and angular artery, the dissection of the major nidus would then be achieved. In this scenario, the vein of Trolard and the vein of Labbé, connecting to the nidus laterally, could have been separated successfully, but the medial fragment of the nidus might have remained hidden due to the deeper location in the precuneus. Vasospasm or residual edema might then have masked this remnant on the angiographic images obtained 7 days postoperatively. As the illustration shows, the recurrent AVM was located in the inferior portion of the precuneus and was supplied by the parieto-occipital artery and drained by the middle temporobasal vein, which would both be located in the deeper surgical field.

We believe that microsurgical anatomy plays a dominant role in AVM recurrence. Preoperative and intraoperative anatomical analysis is essential for the complete resection of AVMs and prevention of recurrence.

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residual and recurrent lesions. *Childs Nerv Syst* **23**:31–37, 2007

Disclosures

The authors report no conflict of interest.

Response

We agree with Ke Tang and colleagues' analysis concerning the medial compartment of the nidus, which could have remained hidden during the microsurgical procedure. However, the operative findings were in favor of a total resection of the nidus, and no abnormal vessels or arterialized vein remained at the end of the procedure. Moreover, intraoperative indocyanine green videoangiography did not show abnormal residual vessels. Nevertheless, the hypothesis of a missed remnant cannot be ruled out, as we highlighted in our *Discussion*.

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Prognostic factors in skull base chordoma

TO THE EDITOR: Chordoma is a very rare mesenchymal tumor with a low to intermediate malignant grade. Clinically, chordoma has a high risk of local recurrence and responds poorly to conventional chemotherapy or radiotherapy. Currently, despite the refinement of surgical techniques and adjuvant radiotherapy, prognostication of the clinical outcomes in chordoma is still challenging. Recently, Wang et al.² conducted a retrospective analysis of 238 patients on clinical features and surgical outcomes in skull base chordoma (SBC) (Wang L, Wu Z, Tian K, et al: Clinical features and surgical outcomes of patients with skull base chordoma: a retrospective analysis of 238 patients. *J Neurosurg* **127**:1257–1267, December 2017). The authors found that recurrent tumor and intralesional resection were statistically significant predictors of worse long-term outcome in the patients on univariate analysis.

We commend the authors for performing this interesting study as these helpful results would be useful to customize postsurgical monitoring and allow stratification of patients into prognostic groups. However, we noted that the authors did not perform multivariate adjustment for their statistical analysis in this study, which may have introduced bias to the results, as many factors can affect patient survival in SBC.^{1,4} Furthermore, we found that the

authors did not further address the important issue with regard to which prognostic factors are important for patients with SBCs.

Currently, although many studies have been conducted to correlate clinical and histopathological features, as well as molecular biomarkers, with chordoma prognosis, the results are still inconclusive or controversial. Given this fact, we believe a systematic review on prognostic factors in chordoma would be helpful to make a balanced treatment decision in clinical practice. To date, although previous evidence-based reviews on prognostic factors in spinal chordoma have been published in the literature,^{3,5} a rigorous systematic review assessing the association of such factors with prognosis of SBC is warranted at this time.

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Disclosures

The authors report no conflict of interest.

Response

Regarding the comments from Dr. Zou et al. about our paper on the topic of SBCs, we would like to provide a response, mainly composed of two aspects.

First, the key point of this paper was to explain the issues regarding SBCs and surgery, such as approach selection, postoperative complications and preventive measures, and factors related to extent of tumor resection as well. Due to article length limitations of the current paper, the issues of progression-free survival and overall survival of this cohort, as well as clinicopathological features acting as survival predictors, have been calculated, analyzed, and published as two other individual papers.^{1,2}

Second, we agree with Dr. Zou's comment that a high-quality systematic review will definitely be very welcomed and helpful in dealing with this tough disease. However, in our opinion, at least two realistic limitations impede its completion. One limitation is the various treatment strate-

gies in different medical centers around the world. For instance, using our group as an example, due to the fact that we lack use of proton-beam radiotherapy, we particularly emphasized the role of resection and normally recommended Gamma Knife or CyberKnife radiosurgery as the adjuvant radiotherapy. These discrepancies definitely lead to heterogeneous data and results that are not comparable among different studies from different groups. Another limitation is due to the characteristic of SBC, which is a slow-growing tumor in most cases. The mean follow-up durations in most studies are not long enough to determine the exact prognostic factors. After overcoming these limitations in the future, a high-quality systematic review should become possible.

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Treatment of ruptured AVMs

TO THE EDITOR: We read with great interest the article by Beecher et al.¹ (Beecher JS, Lyon K, Ban VS, et al: Delayed treatment of ruptured brain AVMs: is it ok to wait? *J Neurosurg* 128:999–1005, April 2018). The proper timing of treatment of nonemergent cases of bleeding arteriovenous malformations (AVMs) is debated. Unlike bleeding aneurysms, the risk of rebleeding for AVMs is calculated on an annual range rather than a weekly range. The risk of AVM rerupture is more elevated, from 6% to 15.8%, in the 1st year² after initial rupture and thereafter declines from 2% to 7.9% each subsequent year, equaling the natural history of unruptured AVMs. The authors retrospectively analyze ruptured AVMs treated at least 4 weeks after initial hemorrhage. The choice in timing of treatment is greater than 4 weeks, as can be deduced from the median and mean delays to treatment (3.5 and 6.1 months, respectively). Patients with rupture of unrelated aneurysms and proximal flow-related aneurysms were deemed more dangerous and excluded the study. One hundred and two patients met inclusion criteria. The treatment paradigm failed in 7 patients (6.9%), in 6 (5.9%) of

FIG. 1. This 75-year-old male patient presented with a bleeding posterior fossa AVM (**A, B, and D**). His clinical history was significant for a previous bleeding 4 years earlier (**C**). The AVM bled a third time, 1 month after the second bleeding (**F**). A CT scan shows initial blood reabsorption after the second bleeding (**E**). The examination dates are indicated to better illustrate the time sequence of the hemorrhages.

whom failure was due to repeat hemorrhage. Five of the 6 rebleeding cases had deep venous drainages. Two of these 6 cases had rebleeding on the 1st day after initial rupture, and their prognosis was poor. There were a total of 18,740 patient-days observed, yielding a rehemorrhage rate of 0.032% per day, 0.96% per month, or 11.5% per year. One patient in the rehemorrhage group had associated aneurysms, among the overall 12 associated aneurysms in the 102-patient population. The calculated rehemorrhage risk of 11.4% per patient-month justified the modification of the strategy for the high-risk subgroup of patients with associated aneurysms.

The authors' analysis concludes that a delay in surgery for at least 4 weeks is appropriate since the risk of rebleeding is less than 1% if there are no associated high-risk aneurysms. This in part reflects a change in authors' strategy

FIG. 2. This 22-year-old male patient presented with a bleeding left thalamic AVM (**C and D**). His clinical history was significant for a first bleeding 8 years earlier (**A and B**). The patient underwent 3 CyberKnife treatments and had microbleeds during the 8 years between radiosurgery and presentation. Initial angiography was performed before CyberKnife treatment (**A**). Thirty-six days after the second relevant bleeding, a third hemorrhage occurred (**F**). A CT scan shows initial blood reabsorption after second hemorrhage (**E**). The examination dates are indicated to better illustrate the time sequence of the hemorrhages.

since that the treatment delay of this series was greater in both mean and median durations.

We would like to focus attention on patients with bleed-

ing AVMs that have a history of bleeding (more than 1 year earlier). The bleeding curve in these patients is presumed to equal the natural history of unruptured AVMs. These patients are wisely excluded from these analyses. We believe, based on our experience, that such cases carry a higher risk of acute rebleeding. We provide 2 illustrative cases (Figs. 1 and 2) of patients who presented with bleeding AVMs and who had a history of bleeding (4 and 8 years earlier, respectively) and a third early bleeding (30 and 36 days later, respectively). The risk rates of rebleeding per month in Beecher and colleagues' series do not apply to patients with a history of bleeding AVMs, and this must be considered when dealing with such cases.

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The authors report no conflict of interest.

Response

We appreciate the interest expressed by Dr. Rustemi and colleagues in our recent publication. To encourage discussion, they have presented 2 cases of patients with a ruptured AVM in whom rupture occurred again more than a year later. They appropriately question whether the rupture risk of AVMs that have previously bled truly returns to that of an unruptured lesion. To encourage discussion, the authors briefly describe the cases of 2 patients: one with a small cerebellar AVM and the other with a basal ganglia/thalamus AVM. While unified in terms of being AVMs with recurrent hemorrhage, these lesions would require different treatment considerations that are based more on their location than on their rupture history.

Many studies support the resection of ruptured AVMs when the lesion can be removed with acceptable morbidity.² In asking the question, "Is it ok to wait?" we imply that we are waiting so that a definitive resection can be performed in an appropriate subset of AVMs. By waiting, we assume that we are decreasing the additional surgical morbidity that occurs when operating on a recently ruptured lesion. A delayed approach was not supported in lesions with associated aneurysms. It is interesting that the basal ganglia AVM that Rustemi and colleagues presented appears to have a nidus aneurysm at the time of the second hemorrhage.

Unfortunately, inoperable AVMs do exist, and stereotactic radiosurgery does not provide an infallible alternative.¹ Fortunately, these formidable AVMs seem to be the

exception and not the rule. A study could be designed to examine the rehemorrhage rates of these lesions, but this was not our focus. It is our contention that it is safe to allow patients with ruptured brain AVMs to recover from the initial reaction to the hemorrhage (if not injury) prior to undertaking definitive treatment in the absence of a high-risk feature (for example, a peri- or intranidal aneurysm). If a high-risk feature is identified, treating the aneurysm should allow the AVM to be managed in a standard, delayed fashion without excessive risk.

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Endovascular management of epidural hematomas

TO THE EDITOR: We read the article by Peres et al.⁶ with great interest (Peres CMA, Caldas JGMP, Puglia P Jr, et al: Endovascular management of acute epidural hematomas: clinical experience with 80 cases. *J Neurosurg* [epub ahead of print April 14, 2017. DOI: 10.3171/2016.11.JNS161398]). In this paper, the authors analyzed the safety and efficacy of embolization of the involved middle meningeal artery (MMA) and associated lesions in small- to medium-sized acute epidural hematomas (EDHs). They had treated 80 patients with small- to medium-sized EDHs using embolization of the MMA and its associated lesions. They compared their results with those in a historical cohort of 471 patients. After embolization of the MMA, there was no increase in the size of the EDH in any of the 80 patients in their study. They concluded that embolization is a highly effective and safe method for achieving size stability in nonsurgically treated acute EDHs. We congratulate the authors for considering endovascular treatment for small EDHs and thus avoiding an increase in the size of the lesions, repeated scans, and prolonged hospital stays. However, we would like to draw the reader's attention to a few pertinent points.

In their study, the authors mentioned that the mean and

median durations from admission to angiography were 4.6 and 4 days, respectively. It has been concluded in various studies that an increase in the size of EDH usually occurs within 24 hours.^{8–10} Sakai et al. demonstrated that small EDHs (65%) expanded in the 24 hours after trauma.⁸ Sullivan et al. found that 25% of EDHs enlarged in the first 24 hours.⁹ In a study by Knuckey et al., 22% of patients deteriorated within 24 hours and only 1 patient deteriorated after 10 days.⁴ In a study by Basamh et al., 11.2% patients deteriorated in a mean range of 5–30 hours.¹ Progression of initially nonsurgically treated EDHs mostly occurs within the first 24 hours, is less likely within 48 hours, and rarely occurs beyond that.¹ In the study by Peres et al., endovascular intervention was performed at a mean duration of 4.6 days, after which enlargement of EDH is rare. The authors did not include the time period from the time of injury to admission, which further increases the interval between trauma and intervention. Hence, it would not be appropriate to conclude that the EDH did not increase in size because of the endovascular therapy. Moreover, the utility of embolization in these cases in terms of preventing the need of surgery cannot be concluded until a case controlled trial is done.

Suzuki et al.¹⁰ showed that endovascular intervention is useful in patients with active contrast extravasation and an increase in the size of a hematoma. Only 61 patients had contrast extravasation in the study by Peres et al., but they did not mention an increase in the size of EDH. Although there is a theoretical risk of bleeding from a pseudoaneurysm, the usefulness of embolization in small EDHs cannot be commented on until the natural history of these lesions is known.²

In the study by Peres et al., 6 patients with normal angiographic findings, 8 patients with MMA wall irregularities, and 1 patient with choroidal blush and an internal carotid artery aneurysm each underwent embolization. The authors did not mention the reasons for embolization in these patients.

We agree with the authors that endovascular embolization is a safe and efficient method, but one cannot conclude that it is effective in preventing the morbidity and mortality associated with small EDHs until the natural history of these lesions is known and embolization is performed early and the intervention group is compared with a nonintervention group. We also believe that endovascular intervention is an excellent tool especially for controlling torrential bleeding from the MMA intraoperatively, in severely comorbid patients, and in cases of multiple lesions.^{3,5,7} It can be selectively done in patients with risk factors such as unstable EDH, active contrast extravasation, fracture running across the MMA, and enhancement of hematomas on MRI.¹¹

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Disclosures

The authors report no conflict of interest.

Response

We thank Dr. Samala et al. for their interest in our study of endovascular embolization of acute EDHs. We would like to take this opportunity to clarify some issues concerning our study. The first point to discuss is about the time from admission to angiography. Given features of the Brazilian health care system, our hospital receives patients from low-complexity centers where they were previously admitted. Therefore, the time from admission to angiography is, indeed, the interval between head injury and embolization. Additionally, we should acknowledge that multidisciplinary case-by-case discussion (especially in the first cases of our series), as well as scheduling difficulties in our busy hemodynamic suite, might have prevented earlier treatment in some cases. We fully agree that early angiography in these cases would be the best option.

The acute phase is far more important in the setting of EDH,¹⁰ but there is always the possibility of late rebleeding of MMA pseudoaneurysms, even after 3–30 days.⁸ Even so, there are no guidelines for routine follow-up CT

scanning or CT angiography in traumatic brain injury. The optimal timing for follow-up images in EDH is an unanswered question.¹⁻³ There may be relative stability in the level of consciousness despite marked enlargement of these lesions. Can we discharge to home those patients with a fracture crossing the MMA sulcus and a 15-mm-thick, conservatively treated EDH 36 hours after trauma? A more careful reading of the cited literature may lead us to other interpretations.

In the articles by Sakai et al.⁷ and Knuckey et al.,⁵ it is really impossible to know if there were enlargements after 48 hours because only a second CT was obtained after the initial scan. In the words of Sakai et al.: “In this study only two CT examinations were conducted, so the exact course of hematoma enlargement could not be clarified.”⁷ They also emphasized that “acute extradural hematoma is a progressively enlarging lesion . . . rather than a lesion completed early after the onset of hemorrhage.” Sullivan et al.⁹ also followed up patients with only a single CT scan, and enlargements were observed up to 36 hours. And these authors also remarked on the fact that EDHs may not attain their maximum size in the first hours of formation.

Delayed enlargement of EDH is presumably caused by an unstable vascular injury associated with the lowering of previously high intracranial pressure (in the setting of associated brain swelling) or the resolution of MMA vasospasm. Conservative management of these lesions must take into account the long time for observation (sometimes more than a week), increasing costs of hospitalization and monitoring, and, worst of all, making us lead with uncertainty—a rare but sometimes devastating effect of late enlargement and brain herniation.

The second issue is about the usefulness of occluding MMAs without contrast extravasation. In our cases, all 6 patients with normal angiographic findings had fractures clearly crossing the MMA sulcus. We admitted that the normal aspect could be due to arterial vasospasm—which is very commonly observed in treating other diseases via MMA microcatheterization. So, it seemed to us very safe to occlude this artery via superselective catheterization. The patient with choroidal blush presented with a pseudoaneurysm and active contrast extravasation (as clearly depicted in our Fig. 5). It should be emphasized that a pseudoaneurysm may represent a dangerous unstable lesion, as severe as contrast extravasation.^{6,11} It has been reported that they can rupture as late as 1 month after injury.⁸

We fully agree that the natural history of these lesions is unclear. Nonetheless, there are clear ethical limitations and a lack of clinical equipoise in performing natural history studies on these potentially dreadful lesions. Unfortunately, such a study may never be available for our analysis. Instead, we must retrospectively and prospectively assess the role of embolization in the treatment of small EDHs, as has been done at our institution.

We concur with Dr. Samala and colleagues’ statement that MMA embolization is a safe and efficient method. And we ask ourselves if it should be considered, if it is immediately available, even in selected surgical cases of EDH: identifying the source of bleeding and the foramen spinosum in a surgical field obscured by blood and/or brain swelling is something that many of us have experienced as extremely disturbing and dangerous.⁴

We thank Samala et al. for bringing up for debate these relevant concerns, and we agree that the longer the EDH is small and asymptomatic, the greater the likelihood that it will remain so. However, arterial MMA embolization will give us peace of mind in discharging these patients home early and safely.

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