Neurosurgical Forum LETTERS TO THE EDITOR

Brain herniation with surrounding CSF into the skull

TO THE EDITOR: I read the article by Valci et al.⁷ with great interest (Valci L, Dalolio M, Kuhlen D, et al: Intradiploic encephalocele of the primary motor cortex in an adult patient: electrophysiological implications during surgery. *J Neurosurg* [epub ahead of print April 28, 2017. DOI: 10.3171/2016.11.JNS162426]). The authors reported a case of spontaneous right frontal lobe parenchyma herniation with surrounding CSF into the right parietal bone in a 70-year-old man who was experiencing a spastic progressive paresis of his left lower limb. The authors described this entity as a spontaneous intradiploic encephalocele.⁷ As I have a special interest in herniations of brain parenchyma into calvaria and/or dural venous sinuses (DVS),¹⁻³ I would like to contribute regarding naming, imaging, and clinical features of this recently recognized entity to prevent confusion that may occur in readers.

In their paper, Valci et al.⁷ reported that there was a bone defect on the right parietal bone with herniation of right precentral gyrus parenchyma through this osseous defect into the intradiploic area, without extension to the outside of the calvaria. Consequently, the authors noted that this entity was consistent with a spontaneous intradiploic encephalocele. However, in the relevant case there was lysis of the internal table and extension into the diploic region of the parietal bone, and only thinning of the external table without any total defect as seen on the presented MR and CT images and mentioned by the authors in the operation section of the case report.⁷ Herniations of brain parenchyma with surrounding CSF into the calvaria or DVS without apparent complete bone defects in the adjacent skull were recently described on MRI and CT.¹⁻⁵ Initially the descriptions, possible etiologies, and clinical significances of this entity were determined differently in various reports. Chan et al.⁴ described this entity as focal cerebellar hemisphere herniation into a giant arachnoid granulation located in occipital bone. Çoban et al.,⁵ in a similar case of left temporal lobe parenchyma herniation (surrounded with CSF) into the left transverse sinus without an apparent bone defect, described it as an occult temporal lobe encephalocele. Battal and Castillo¹ reported 5 cases in their report and described this entity as brain parenchyma herniation with the surrounding CSF into the calvaria or DVS. They reported that this entity was probably an incidental finding that

might be more common than previously recognized.¹ This entity was described as brain herniation with surrounding CSF into the calvaria or DVS in the most comprehensive research about this topic reported by Battal et al.³ I believe that brain herniations into the skull have different features from classic encephaloceles. Central skull base encephaloceles remain occult unless there is a CSF leak, meningitis, seizure, or headache. Encephaloceles are documented as masses composed of meninges and brain extruding outside the skull.¹⁻³ Conversely, the brain herniation that was described by Valci et al.⁷ did not occur through a complete calvarial defect. Instead, it occurred through a dural defect into calvaria and thus does not completely meet the criteria for encephaloceles. Moreover, as mentioned by the authors in the discussion section, the herniated brain parenchyma was not covered by the dura, and there was not a history of trauma, infection, or previous cranial operations as an underlying cause for acquired encephaloceles. Because of these reasons, this entity should be described as brain herniation with surrounding CSF into the calvaria instead of an encephalocele.

Arachnoid granulations are protrusions of arachnoid through the dura mater. A neck of arachnoid penetrates an aperture in the dura and expands to form the core of the granulation. Arachnoid granulations may enlarge with age or in response to increased CSF pressure.¹⁻³ Although the features of brain parenchyma herniations into calvaria are also different from classic arachnoid granulations and arachnoid pits, it appears reasonable to postulate that the presence of preexisting arachnoid granulations or pits may facilitate the formation of brain herniation into the adjacent calvaria or DVS.¹⁻³ Therefore, I believe these structures are not covered by the dura, as mentioned by Valci et al.⁷, and are a different entity from the classic acquired encephalocele.

Battal et al.³ reported that brain parenchyma herniations with surrounding CSF into the calvaria and/or DVS are more common than previously assumed, with a prevalence rate of 0.32%. These herniations are encountered more frequently in posterior-inferior parts of the intracranial cavity and the most common locations were the transverse sinus and occipital bones.³ Recently, a very similar case was reported by Rodrigues and Santos⁶ in the parasagittal convexity area. They presented a case of parietal lobe parenchyma herniation with surrounding CSF into the incomplete parietal bone defect, with a narrow neck and atrophy, partial strangulation, and hyperintensity of

herniated brain parenchyma on MR images.⁶ Consequently, brain herniation into the calvaria has been reported to be very rare and its etiology, symptomatology, and clinical significance are not well-known. I believe that this entity is more common than expected and should be considered in the differential diagnosis of encephaloceles and lytic skull lesions. The continuity of the external table of the calvaria (although it may be very thin) and no brain tissue extruding outside the skull can be distinguishing features of this entity from encephaloceles.

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Disclosures

The author reports no conflict of interest.

Response

We read with interest the letter to the editor from Dr. Battal and are happy to provide an extensive response. The details provided by Dr. Battal certainly help to define the characteristics of anatomical “peculiarities,” which are hardly classifiable because of their rarity. Battal also suggests interesting ideas about the mechanisms of their formation.

Encephaloceles can present in various ways and, as noted in our article, they are not well classified yet. In the literature there are many classifications according to distinct aspects of the encephalocele, such as content, location, and pathogenesis.⁷ The extradiploic encephalocele—when brain, meninges, and CSF protrude out of the skull, whether occult or not—is an intuitive and defined entity. In contrast, the intradiploic encephalocele, which is

a herniation into the calvaria through an incomplete bone defect with or without dural defect, remains complex and difficult to classify. Nevertheless, we believe that our case should be defined as an encephalocele for two main reasons.

First, the etymology of the term itself helps. The term “encephalocele” comes from ancient Greek, “enkefalé” and “kele,” meaning brain herniation. A brain herniation is a displacement of cerebral mass, with or without CSF and meninges, from the usual position to a different one, in most cases through a narrow passage (i.e., tonsillar, uncus, or transtentorial herniation). So, according to the actual classification^{4,6} and in our opinion, the encephalocele is a type of brain herniation in which brain protrudes out of an osseous defect within or outside the diploe. We can argue that in the case of brain herniation into the calvaria (so called intradiploic), the partial osseous defect plays an effective role in pathogenesis. The brain herniation in DVS, as described by Battal et al.,^{1–3} appears to be a different entity in which the osseous defect has no role in its pathogenesis.

Second, the reason why we did not find any dura layer covering the herniated brain could correlate with the presence of meningioma discovered during pathological examination of the bone margins. The tumor, therefore, could have induced a reabsorption of the dura layers. An acquired encephalocele can be the consequence of trauma, infection, or previous cranial operations, but also because of tumors. In the literature, another case of an intradiploic encephalocele not covered by the dura layer has been reported.⁵ In that case, the patient experienced a previous trauma that over time induced the protrusion of the brain parenchyma through an osseous-dural defect.

In conclusion, the main focus of our article was to emphasize that the herniated parenchyma was functional. We also supposed that the meningioma caused a progressive reabsorption of both bone and dura, inducing the parenchyma herniation by intermittent physiological pulsation of the brain. Despite the long duration of this process, a reorganization of the primary motor area, with exclusion of the herniated brain parenchyma, did not occur. This is certainly rare, considering the cases of encephalocele already published, as we discuss in our article. Beyond definitions and classifications, the clinical features, involvement of the primary motor area in the herniated parenchyma, and possibility of mapping it and thus preserving it, represent the peculiar and surprising aspects of our case.

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Usefulness of 5-ALA in resection of intracranial meningiomas

TO THE EDITOR: Millesi et al.⁵ should be commended for their study published in your esteemed journal (Millesi M, Kiesel B, Mischkulnig M, et al: Analysis of the surgical benefits of 5-ALA-induced fluorescence in intracranial meningiomas: experience in 204 meningiomas. *J Neurosurg* 125:1408–1419, December 2016). The authors aimed to investigate the possible surgical benefits of the use of 5-aminolevulinic acid (5-ALA) in 190 consecutive patients with intracranial meningiomas over a 5-year period. 5-ALA was administered preoperatively to 190 patients undergoing resection of 204 intracranial meningiomas. The meningiomas' protoporphyrin IX (PpIX) fluorescence status, fluorescence quality (strong or vague), and intratumoral fluorescence homogeneity were investigated during surgery. Additionally, specific sites were analyzed, including the dural tail, tumor-infiltrated bone flap, adjacent cortex, and potential satellite lesions. PpIX fluorescence was observed in 185 (91%) of 204 meningiomas. Typically, most meningiomas demonstrated homogeneous fluorescence (75% of cases). No PpIX fluorescence was observed in any of the investigated 89 dural tails. Tumor-infiltrated bone flaps could be visualized by PpIX fluorescence in all 13 cases. The authors concluded that PpIX fluorescence enables intraoperative visualization of most intracranial meningiomas and allows identification of residual tumor tissue at specific sites, and this intraoperative detection of residual meningioma tissue by PpIX fluorescence might reduce the risk of recurrence in the future.⁵

First and foremost, meningiomas are well-defined, usually lobulated tumors that can be easily differentiated from the surrounding parenchyma, in contrast to gliomas that are infiltrative and are on many occasions not separately

discernible from the adjacent brain parenchyma. Hence, the utility of 5-ALA assumes much more importance in gliomas than in meningiomas.^{1–3,7–9}

The authors had 37 (18%) cases of recurrent meningiomas in their study. What were the locations of those tumors? Also, what were the patterns of immunofluorescence in primary as compared with recurrent meningiomas? A recent systematic review by Motekallemini et al.⁶ concluded that the use of 5-ALA as a tool to guide resection of intracranial meningiomas remains experimental, especially in cases with tumor recurrence. In addition, it has been stated that the superiority of 5-ALA-assisted resection of intracranial meningiomas regarding progression-free survival needs to be investigated in prospective cohort studies.

Many studies have noted the presence of tumor cells in the dural tails. The authors repeatedly mention that they did not find 5-ALA uptake in any of the 89 cases with dural tails on imaging. Does that mean that dural margin resection is not warranted? This is especially important in convexity meningiomas in which removing the tumor is easy but the difficult part is assessing to what extent the dura is involved and what needs to be resected. Kinjo et al.⁴ advocated Simpson Grade 0 resection (resection of the adjacent 2-cm of normal dura mater), which has been advocated as a means of reducing the recurrence rates (close to 0%) in convexity meningiomas. Also, were any Grade 0 resections performed in the present series in cases of convexity meningiomas?

With regard to bone flap infiltration, as correctly stated by the authors at the end of the paper (the *Limitations* section), to have bone involvement the ability of this dye to detect apparently normal-looking bone (on imaging) to show fluorescence and indirectly the presence of tumor tissue remains to be validated.

Satellite lesions were noted in 7 cases at sites distant from the primary tumor. What were the grades of the primary tumor and of the satellite lesions in those patients? Also, what was the mean distance from the primary tumor? This assumes importance because with recent widespread use of neuronavigation in meningioma resections, the sizes of bone flaps have been customized to the lesion, especially in convexity and parasagittal locations. Because this study has shown the presence of satellite lesions at a distant site, does that mean bone flaps need to be much bigger than the usual ones?

The authors mention that they observed a significantly higher proportion of PpIX fluorescence within the adjacent cortex in cases with a disrupted arachnoid layer than in cases with an intact arachnoid layer. I feel it is simple logic that a disrupted arachnoid layer leads to increased uptake of the dye. In 5 cases of intact arachnoid layer, fluorescence was observed. Did this observation change the management in any way, such as resection of that cortex?

Lastly, similar to the authors' comments of the study's limitations, recurrences have not been studied. This takes on a significant importance, especially to validate this technique in reducing recurrences and in the wake of the authors studying the correlation of fluorescence with dural tail, adjacent cortex, and satellite lesions. Therefore, if this technique does not show any reduction in the recurrence

rates as compared with the existing world literature (using standard microsurgical methods), then the usefulness, efficiency, and cost-effectiveness of this PpIX fluorescence technique will remain debatable.

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Disclosures

The authors report no conflict of interest.

Response

We greatly appreciate Dr. Prasad's interest in our study and his comments on our recent article analyzing the surgical benefits of 5-ALA–induced fluorescence in the largest series of intracranial meningiomas to date. We completely agree that there is no doubting his statement on the importance of 5-ALA in gliomas that are characterized by their infiltrative growth pattern. In this sense, 5-ALA fluorescence-guided surgery demonstrated the ability to increase the portion of complete resections of the contrast-enhancing tumor in malignant gliomas as compared with white-light procedures.⁸ Although meningiomas are usu-

ally well defined, in our study we found specific surgical benefits of 5-ALA–induced fluorescence in intracranial meningiomas as well. To this end, we observed that 5-ALA–induced fluorescence is also capable of visualizing the majority of these tumors (91% of cases) and identifying residual meningioma tissue at specific surgical sites, such as infiltrated bone flaps or satellite lesions in proximity to the meningioma and the dural tail. Consequently, the 5-ALA fluorescence technique might also be useful in intracranial meningiomas to maximize the extent of resection and thus eventually reduce local recurrence.

In our study cohort of 204 intracranial meningiomas, 37 recurrent tumors (18%) were included. The most common localization of the recurrent meningiomas was the convexity (n = 11 cases), followed by parasagittal/falx (n = 9 cases) and sphenoorbital meningiomas (n = 3 cases). Visible fluorescence was present in the majority of these recurrent meningiomas (87%). No visible fluorescence could be detected in only 5 recurrent meningiomas (13%). In the study period, we found 5 patients with high-grade meningiomas who underwent more than 1 surgery with intraoperative application of the 5-ALA fluorescence technique because of tumor recurrence. In all 5 of these patients with multiple 5-ALA procedures in the study period, strong fluorescence of the resected meningioma was observed in the first and also in the following surgeries. Consequently, in our study we did not observe a change in the 5-ALA fluorescence status in the recurrent tumor as compared with the primary meningioma of the first surgery in the same patient.

Furthermore, we agree with the mentioned systematic literature review from 2015 by Motekalleli et al. that considered the use of 5-ALA in meningiomas “experimental” (especially in recurrent cases) according to the few available literature reports at that time.⁶ In this sense, the largest study in this review consisted of a total of 33 meningiomas.^{3,6} Exactly this lack of sufficient data with regard to 5-ALA in intracranial meningiomas has encouraged us to investigate the surgical benefits of this technique in a large patient cohort of 204 intracranial meningiomas. According to these recent data, 5-ALA–induced fluorescence is considered useful in newly diagnosed as well as in recurrent intracranial meningiomas. Moreover, we also agree with Dr. Prasad's comment that the potential superiority of 5-ALA resections with regard to a prolonged progression-free survival has to be analyzed as the next step in future prospective studies.

Our strategy is to surgically remove the dural tail of meningiomas whenever it is feasible. Unfortunately, the dural tail of intracranial meningiomas cannot be visualized according to our experience, and thus this technique is not able to support the neurosurgeon to better delineate the extent of the dural tail. Consequently, this represents a major shortcoming of this technique in intracranial meningiomas. In 1986, Borovich et al. reported on the existence of tumor nodules distant from the resected meningioma and suggested a more extensive resection termed Simpson Grade 0, including up to 4 cm of surrounding dura.^{1,2} This use of a more extensive resection was emphasized in a later study by Kinjo et al., who resected a 2-cm margin around the resected meningioma, if feasible,

in patients with convexity meningiomas.⁵ However, based on our clinical practice, we do not usually perform such extensive Simpson Grade 0 resections, not even in cases of convexity meningiomas at our neurosurgical center that would require significantly larger craniotomies.

The 5-ALA fluorescence technique was able to identify 7 satellite lesions distant to the surgically treated meningioma (6 WHO Grade I and 1 WHO Grade II meningiomas, respectively) in our study. In all of these cases, the same histopathological WHO grade was observed in the meningioma and the corresponding satellite lesion. We did not systematically analyze the precise distance of these satellite lesions to the meningioma in this study. However, the distance of the observed satellite lesions to the meningioma did not exceed more than 2 cm because we usually performed standard craniotomies customized to the lesion in this study, including the dural tail. Consequently, such a standard craniotomy appears to be sufficient to identify such satellite lesions with the assistance of 5-ALA-induced fluorescence. Nevertheless, we cannot exclude the possibility that additional satellite lesions can be identified with 5-ALA-induced fluorescence at greater distances to the meningioma if larger craniotomies than the usual ones are performed.

In our study, we also observed visible fluorescence of the adjacent cortex in 25% of the analyzed meningiomas. Interestingly, we found a significantly higher portion of such fluorescence of the adjacent cortex in meningiomas with a disrupted arachnoid layer than in tumors with an intact arachnoid layer, as mentioned by Dr. Prasad. This would also support our results on peritumoral brain edema formation and the arachnoid layer/angiogenetic factors in meningiomas.⁷ However, we also found 5 meningiomas that demonstrated visible fluorescence of the adjacent cortex despite an intact arachnoid layer. Thus, additional factors other than the status of the arachnoid layer appear to be responsible for the fluorescence in these cases. As previously noted in our article we did not systematically collect tissue specimens from the adjacent cortex in this study based only on the fluorescence status, to clarify if this fluorescence is caused by tumor infiltration. Consequently, this interesting observation did not result in a change of our surgical strategy with additional removal of fluorescing adjacent cortex in our study. As noted in our paper, Cornelius et al. described similar observations with visible fluorescence in the adjacent cortex in a subgroup of patients with high-grade meningiomas, and in this study biopsy samples were also collected in some cases.⁴ In their histological workup, tumor cells were found in all of the biopsied cases of a fluorescing adjacent cortex.⁴ However, future studies are needed to draw a final conclusion with regard to the morphological correlate, the factors involved, and the surgical relevance of visible fluorescence within the adjacent cortex of some meningiomas.

Finally, we fully agree that it is crucial to analyze the rate of recurrence following meningioma resection with assistance of 5-ALA-induced fluorescence. However, as already outlined in our article, as a first step the aim of our study was to analyze the surgical benefits of 5-ALA-induced fluorescence in a large series of intracranial meningiomas. In the next step, the rate of recurrence (and as

mentioned above, progression-free survival) should be investigated in future prospective studies with a sufficiently long postoperative follow-up period.

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Outcome prediction in brain tumor surgery

TO THE EDITOR: We have read with great interest the article by Sagberg et al.⁶ (Sagberg LM, Drewes C, Jakola AS, et al: Accuracy of operating neurosurgeons' prediction of functional levels after intracranial tumor surgery. *J Neurosurg* **126**:1173–1180, April 2017). This creative and important study posed the following question: How precise are neurosurgeons in predicting the outcomes of intracranial tumor resection? Using the Karnofsky Performance Status (KPS) score as a measure of outcome, the authors compared the operating neurosurgeons' predicted

KPS score at the time of surgery with the KPS score 30 days after intracranial tumor resection in 299 patients. The results of their study clearly showed a significant discrepancy between the neurosurgeons' estimation of outcome and the actual result of the surgical intervention. Notably, predictions appeared to be overly optimistic (that is, the KPS score at 30 days was worse than what had been expected by the operating surgeon) in 62% of the patients studied. There was no significant difference between residents and consultants in overestimating outcomes.

Our interest in this somewhat startling and revealing result is heightened because our own neurosurgical group has been committed to finding an evidence-based methodology that might lay the foundation for outcome prediction of any neurosurgical intervention on more solid, scientifically based ground. Like the Norwegian group, we have been acutely aware of the influence that many "human factor" variables have on a neurosurgeon's prediction of surgical outcome.⁴

In a study recently published in *Neurosurgical Focus*,³ we compared the pre- and postoperative (at discharge) KPS scores in a cohort of 746 patients with brain tumors who had undergone maximal safe resection (biopsies excluded) at our institution, and we tried to identify some reliable prognostic factors for clinical worsening after intracranial oncological surgery. Predictably, most neurosurgeons base many of their expectations about outcome on the anticipated surgical complexity of any single case. Factors such as tumor volume and its relationship to important structures such as vessels, nerves, and eloquent brain parenchyma significantly burden this prediction process. Intuitively, the greater the surgical complexity, the harder the expected postoperative course. While the Norwegian group carefully examined the roles that optimism and pessimism play in the predictive process, we investigated the role that complexity plays on the actual surgical outcome. Despite the evident methodological limitations in our retrospective monocentric study, statistics did confirm what experience and intuition suggest—that is, complexity matters!

We applaud Sagberg and coauthors for their effort to expose the neurosurgeons' predictive process to scientific scrutiny and invite more members of our neurosurgical community to examine the factors contributing to their own predictive process. We owe this exercise in self-awareness as much to ourselves as clinicians, as we do to our patients, whose lives and existential well-being rest so heavily on the expectations we share with them. We are well aware that neurosurgery has a long way to go in establishing reliable prognostic factors for intracranial surgery. Nevertheless, a number of factors seem to weigh heavily on the clinical outcome. In our study on the predictive value of complexity,³ we found that at discharge, 523 patients (70.1%) had improved or remained unchanged and 223 (29.9%) had worsened. A logistic regression model was used to identify possible predictors of clinical worsening after surgery in terms of changes between the preoperative and discharge KPS scores. Statistically significant predictors were rated based on their odds ratios. For each patient, a corresponding total score was calculated, and ANOVA was performed to compare the mean total scores

between the improved/unchanged and worsened patients. Relative risk and chi-square statistics were used to provide the risk of worsening after surgery for each total score. The following 5 factors—what we call the "Big Five"—were found to be statistically significant predictors of the change in KPS scores: tumor size larger than 4 cm, cranial nerve manipulation, major brain vessel manipulation, surgery in the posterior fossa location, and eloquent area involvement (Nagelkerke $R^2 = 0.286$). A grading scale, the Milan Complexity Scale (MCS),³ was then created with scores ranging between 0 and 8. Patients with a worsened KPS score showed mean total MCS scores that were significantly higher than those in patients with improved/unchanged KPS scores (3.24 ± 1.55 vs 1.47 ± 1.58 , $p < 0.001$). Finally, a grid was developed to show the risk of worsening after surgery for each total score: scores higher than 3 were suggestive of a worse clinical outcome. Oncological cases, as intuitively evident, can be divided into low risk (MCS score < 4) and high risk (MCS score ≥ 4) allowing for more precise prediction of the expected outcome and resources needed to manage each case (Fig. 1).

We could not agree more with Sagberg and coauthors' conclusion that our profession needs "practical and reliable prognostic tools in intracranial tumor surgery."⁶ Indeed, the primary reason we developed the MCS was to promote it as "a new practical grading scale designed to estimate the risk of neurological clinical worsening after performing surgery for tumor removal."³ For too long, we neurosurgeons have relied on predictive processes that bear little or no statistical scientific relationship to the outcomes we expect or, more importantly, to the hopes and expectations that we share with our patients. We congratulate the Norwegian surgeons for summoning the courage and the scientific acumen to expose the tradition of subjective predictions to scientific investigation. At the same time, we hope that more neurosurgeons will take advantage of the work we have done in developing the MCS. We recognize that our scale is far from providing a complete answer to the many complexities that play a role in predicting surgical outcomes, but as we stated in our paper, the MCS may enable "neurosurgeons to estimate the risk of a negative clinical course after brain tumor surgery and share these data with the patient."³ In this respect, we wonder how the Norwegian surgeons would have rated their patients' KPS scores at 30 days after surgery if they had applied the MCS to their cases immediately after surgery. Would their predictions have deviated so much from the outcome they achieved? Would their unjustified optimism have been so excessive?

We would like to address a second issue and that concerns complication reporting. When dealing with health management and quality assessment issues, the question of reporting "complications" takes on crucial significance. At the Zurich University Hospital, for example, all neurosurgical activity is monitored by way of a dedicated high-profile registry for complications.⁷ We believe that complication classification systems based on the kind of treatment required (as in the Clavien-Dindo system¹ or Clavien-Dindo–derived Landriel Ibañez classification⁵) offer the most appropriate and effective means of establishing the resources needed to treat a complication. Interestingly,

FIG. 1. Role of complexity in outcome prediction. Following intracranial tumor resection, patients with an MCS score < 4 were found to have a lower risk of worsening at the 3-month follow-up than the patients with an MCS score ≥ 4 ($p < 0.05$). Values on the y-axis represent the percentage of patients with a worsened KPS score.

however, as comprehensive as this strategy may appear, it neglects to account for the seriousness of the complication itself.² In fact, a hemiplegic patient with an anterior choroidal artery infarction typically receives a low score (for example, Clavien-Dindo Grade 1 or 2 and Landriel Ibañez Grade 1) despite the permanent invalidity, whereas a patient who requires single needle drainage of a subgaleal cerebrospinal fluid (CSF) collection receives a higher score (for example, Clavien-Dindo Grade 3a and Landriel Ibañez Grade 2a) even though restitutio ad integrum is the usual outcome of this complication. This kind of grading, which ignores the gravity of a chronic disabling complication like hemiplegia, is misleading in terms of its correlation with both the long-term prognosis and the impact on the patient's life. Because of this, we believe that it is better to understand and to pay serious attention to the occurrence of complications and their consequences. It is for this reason that in our registry, we score complications according to both the Clavien-Dindo grade and an etiological classification that we devised (infectious, hemorrhagic, CSF related, medical, and so forth). We did this to identify the weak points that require maximal consideration to improve the outcome in terms of what really matters, that is, quality of life. It would be interesting to know more details about the complications that were found in those 90 patients in the Sagberg series, the most frequent causes of those complications, and their long-term outcomes.

Despite the many limitations of our own study,³ we believe we are in complete alignment with the philosophical thrust of the Sagberg et al. report.⁶ Our profession needs to more carefully address the role of several factors, especially those arising from surgeon sentiments, that weigh heavily on the predictive process. Along this very same line, we believe that the MCS can serve as a template for future studies helping neurosurgeons to make predictions on a sounder, more objective scientific basis. The Norwegian

group should be congratulated for their elegant contribution to the Socratic goal of “Neurosurgeon, know thyself.”

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Disclosures

The authors report no conflict of interest.

Response

We would like to thank Drs. Ferroli and Broggi for their thoughtful and positive commentary about our article. There is clearly a need for valid, reliable prognostic tools in brain tumor surgery. Today, assessments of operability and risk are highly subjective, and as seen in our study, surgeons seem to be overly optimistic with respect to the results of their work. A risk scoring system like the MCS¹ may be a step in the right direction. However, validation studies to confirm the findings of the Milan group are still required. If a scoring system had been used in our study, the surgeons might have been able to rate their patients' postoperative functional levels more precisely. However, potential prognostic tools need to prove that they are better than clinical judgment or “gut feeling” because this is not always the case. For example, clinical gut feeling was recently found to be comparable to several prognostic tools developed for predicting mortality in critically ill intensive care unit (ICU) patients.⁴

In agreement with Ferroli and Broggi, we believe that using a classification system for complications is preferable to avoid assessment bias, and we have used the Landriel Ibañez classification system in recent years.² However, even with the use of a standardized classification system,

complication rates should be interpreted with caution, given the influence of center-related factors such as surgical aggressiveness, case mix, postoperative surveillance, treatment and follow-up routines, quality of documentation, and discrepancy in what is regarded as a postoperative complication. As pointed out by Ferroli and Broggi, the Landriel Ibañez classification is based on the severity of treatment rather than the severity of the complication per se, and this can be misleading. However, scoring the severity of individual complications is not straightforward. For example, a patient with postoperative pneumonia, postoperative delirium, acquired paresis, and a small cavity hematoma who ends up with a prolonged stay in the ICU is, by definition, graded as having suffered a severe complication. However, judging which complication has led to the ICU stay can prove difficult. Further, complications that do not require treatment or surveillance (for example, cerebral infarctions) are graded as mild complications but can still have severe consequences for function. In our recent paper, we chose not to give a more detailed presentation of the complications since doing so was outside the main scope of the paper and there are some obstacles in attempting to relate complications to functions. In using the Landriel Ibañez classification, only the most severe complication in each patient is to be used for grading, although many patients have multiple complications. This makes it difficult and rather subjective to assess which particular complication may have most affected the functional levels of a patient at 30 days.

Many of the predictive factors that are summed in the MCS relate to the location and size of the tumor. These are probably factors that (hopefully) are integrated into the ill-defined, experience-based clinical gut feeling that influences decision making today. Instead of dichotomous measures of tumor size, tumor location, brain eloquence, and more, one could speculate that the integration of more detailed knowledge of the functional anatomy (for example, in maps) might be a way to advance. Probabilistic brain maps of the extent of resection have been published³ and were later supplied with information about functional anatomy and location.^{5,6} Yet, unpublished maps of functional outcomes might be able to predict risks and benefits of surgery even more precisely. Experience-based clinical judgment will probably still have a role, but evidence-based tools to guide and support decision making are needed to avoid both systematic over- and undertreatment and decision outliers.

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Tumors of the fourth ventricle

TO THE EDITOR: I read with interest the recent article by Ferguson et al.¹ (Ferguson SD, Levine NB, Suki D, et al: The surgical treatment of tumors of the fourth ventricle: a single-institution experience. *J Neurosurg* [epub ahead of print April 14, 2017. DOI: 10.3171/2016.11.JNS161167]). I would like to commend the authors on this series of cases involving fourth ventricle lesions, which is the largest so far in the literature, particularly because of the extensive comparison between the 2 key surgical approaches to these lesions. As one would expect, overall results generally appear to demonstrate superiority of the telovelar approach over the transvermian approach, especially in terms of complication profile. Interestingly, however, the authors report that their use of MRI-guided intraoperative navigation, ultrasonography, and neurophysiological monitoring did not significantly affect neurological outcome in this 18-year retrospective review of 55 patients, despite previous documentation in the literature on the usefulness of these adjuncts.^{2–4} This is quite a remarkable finding from this review because it then raises the question as to the extent of impact of the intraoperative use of these adjuncts in reducing unwanted neurological sequelae during the resection of fourth ventricle tumors. Perhaps this may be suggesting that the management strategy for surgical treatment should be based on other more important factors such as the experience of the surgeon, more meticulous microsurgical dissection, and better understanding of the anatomy of each lesion on the one hand, and less dependence on these adjuncts on the other hand, for optimal neurological outcomes. A high-quality comparative observational cohort and randomized controlled study may be required to objectively prove this.

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Disclosures

The author reports no conflict of interest.

Response

We thank Dr. Onyia for his comments. We agree that this finding was somewhat surprising. Because this study spans more than 18 years, neuromonitoring and imaging techniques are likely to have substantially improved over time, and the impact of this may not be captured in this retrospective report. Additionally, our study may highlight the notion that no level of monitoring will match the necessity of anatomical knowledge and careful surgical technique, particularly in critical locations such as this.

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Rendering unto Caesar: mini-pterional and mini-orbitozygomatic approaches

TO THE EDITOR: We have read with great attention the article by Yağmurlu and colleagues⁹ (Yağmurlu K, Safavi-Abbasi S, Belykh E, et al: Quantitative anatomical analysis and clinical experience with mini-pterional and mini-orbitozygomatic approaches for intracranial aneurysm surgery. *J Neurosurg* **127**:646–659, September 2017). It is indeed a very interesting paper, and we agree with many of the authors' statements, including the fact that these two approaches have become standard cranial exposures for many surgeons who perform cerebrovas-

cular and neuro-oncological procedures.^{1–8} However, we also noticed many methodological incongruencies that we think are worth discussing.

First, the concept of surgical freedom as adopted by the authors is different from that previously defined.^{7,8} Surgical freedom is classically described as a measure of the maneuverability of surgical instruments in the superficial area of exposure provided by a particular approach.⁷ The authors innovated and utilized this concept in a different manner. However, they do not explicitly describe how surgical freedom was measured. Why should surgical freedom depend on anatomical targets as an intrinsic variable of the approach? This point needs further explanation. A figure illustrating the authors' idea would surely clarify their points.

Second, in studies like this, a source of error is the degree of retraction that is applied when the measurements are taken. It is necessary to maintain a similar degree of retraction when the measurements are performed. This is achieved by maintaining the same extent of retraction while converting the approaches (usually using miniplates and screws) in order to prevent bias in the measurements. Nonetheless, it seems that this strategy was not used in their study. The extent to which this fact may have biased the results was not discussed.

Third, the results clearly showed that the mini-pterional approach was significantly superior to the mini-orbitozygomatic (OZ) approach in many parameters, including surgical freedom for access to the internal carotid artery (ICA), middle cerebral artery (MCA) bifurcations, ipsilateral posterior cerebral artery (PCA), and the basilar artery (BA) tip. No differences were noticed for access to other anatomical targets. In addition, when the angular exposures were analyzed, the mini-pterional approach was statistically superior to the OZ approach for horizontal (ipsilateral MCA bifurcation) and vertical (ipsilateral ICA, posterior clinoid process, ipsilateral MCA bifurcation, ipsilateral PCA, ipsilateral superior cerebellar artery, BA tip) angles. In fact, the mini-OZ approach was not statistically superior to the mini-pterional approach in any of the evaluations. Thus, discussion seems to be biased in favor of the mini-OZ approach, despite the eloquence of the results to the contrary. Real statistical differences that favor the mini-pterional approach have not been emphasized while the authors claim that there are "trends" in some measurements that may favor the mini-OZ approach. However, as we all know, such trends may only represent statistical artifacts that reflect a small sample size.

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Disclosures

The authors report no conflict of interest.

Response

We appreciate the items of discussion regarding our paper. The mini-OZ and mini-pterional approaches we described are remarkably different from the standard mini-OZ and mini-pterional techniques detailed previously^{5,10} since our approaches reduce the tissue trauma from the skin to the lesion. Additionally, we included a quantitative anatomical analysis of the new mini-OZ and mini-pterional approaches and a retrospective review of 396 cases at our institution in which these two approaches were used to assess which pathologies were most amenable to access via these surgical routes. We described both surgical techniques in a stepwise manner and included detailed anatomical dissection figures, intraoperative photographs, and descriptions of pertinent anatomy.

First, we are aware of the initial description of methods to measure surgical instrument maneuverability in the superficial area of exposure and acknowledge the development of methods for quantitative assessment of neurosurgical approaches.^{8,9} Specifically, we reviewed the articles referenced by Figueiredo et al. as examples of the “classic” definition of surgical freedom. In 2006, Figueiredo and colleagues published an explanation of surgical freedom⁸

and rightfully identified the method as a “superficial area of exposure” developed in our laboratory. However, we could not find information regarding surgical freedom in the publication by Welling et al.⁹ While the initial description of surgical freedom as a superficial area of exposure is a valuable method among several assessment techniques, it does not account for the fact that maneuverability of the surgical instruments is highly dependent not only on restrictions created by the surgical corridor but also on the location of the anatomical target that is being approached. To more accurately reflect these aspects, surgeons have refined the concept of surgical freedom to include the anatomical target for which surgical freedom is being defined.^{3,5} Readers are referred to Elhadi et al. for an illustration of the concept of surgical freedom to the anatomical target.⁵ Although this definition is still not perfect since it does not account for the variations in angled instruments or the number and profile of instruments used simultaneously in the surgical corridor, we do consider this new definition to be the best available. Moreover, interpretation of surgical freedom using methods similar to those in our paper has been reported in a number of peer-reviewed articles from our institution^{3–5,10} and others.^{1,2}

Second, we used the technique of bone conversion to avoid an error in the degree of retraction during our measurements, as mentioned in the authors’ letter; however, references were not cited.^{6–9} We thank the authors for noting this point of discussion.

Third, the authors’ letter states that the “mini-OZ was not statistically superior to the mini-pterional approach in any of the evaluations.” In our investigation, the mini-OZ approach was superior in the horizontal angle of attack compared to the mini-pterional for access to the anterior communicating artery (ACoA), the contralateral ICA bifurcation, and the contralateral MCA bifurcation, as reported in the *Results* of the text and the abstract. However, as noted, these differences did not reach the threshold of $p < 0.05$, so exact p values were given. For the vertical angle of attack, the mini-pterional was not superior to the mini-OZ approach for access to the ACoA and contralateral ICA bifurcation. The mini-OZ approach also provided exposure of a longer vessel length, as we described in the *Results*. We believe that exact p values should speak for themselves. Near-significant p values could represent either “statistical artifacts” (as mentioned by Figueiredo et al.) or a true finding for which the power of the study was not enough to reach the significance threshold ($p < 0.05$). This study included a limited number of cadaver specimens, thus the power was limited and did not result in p values of significance. Reported p values for the comparison of the mini-OZ and the mini-pterional were not statistically significant at the selected 0.05 threshold; thus, comment on the relevant anatomical observation without mentioning near-significance would have been better.

Finally, neurosurgical anatomical science is a dynamic field of study. The knowledge gained from what is investigated and published in one paper may in fact change as we develop new methods of assessment and inquiry. As shown by our exchange in these letters, procedures and methods of assessment may change in subtle ways that cause us to rethink and reevaluate our previous studies and concepts.

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