Letters to the Editor

NEUROSURGICAL FORUM

Physician specialty and endovascular treatment of intracerebral aneurysms

TO THE EDITOR: We read with interest and great concern the paper by Fennell et al.² (Fennell VS, Martirosyan NL, Palejwala SK, et al: Morbidity and mortality of patients with endovascularly treated intracerebral aneurysms: does physician specialty matter? *J Neurosurg* 124:13–17, January 2016). The paper presents a retrospective analysis on morbidity and mortality rates of endovascular embolization of intracranial aneurysms according to the primary specialty of the treating operator. The data source is the University Health System Consortium database, which includes 120 academic centers with more than 300 of their affiliated hospitals. The authors' claim is that the morbidity and mortality of patients with ruptured and unruptured intracranial aneurysms treated with endovascular embolization is higher for those treated by neurologists or radiologists than those treated by neurosurgeons. They further assert that this is related to the less intense training and exposure to neurocritical care of neurologists and radiologists compared with neurosurgeons for whom training includes early exposure to endovascular and open procedures. In addition, they mention a concern about the variability of training in endovascular procedures. We believe this paper is at best a disservice to the specialty and at worst a political statement disguised as science for the following reasons.

The authors' entire paper focuses on morbidity and mortality adjudicated to the treating operator, when in reality the periprocedural care is as important or even more so than the actual procedure, and morbidity and mortality may be unrelated to the procedure. With regard to the issue of training, the authors show no knowledge and fail to mention that for neurologists there is a requirement of 1 year of vascular neurology or neurocritical care after residency and before the endovascular training that can be extended to 2 years. Moreover, their whole discussion not only is against current American Heart Association guide-

lines,^{1,5} which recommend a multidisciplinary approach, but is also contradictory when they state that their neurosurgical "multiphysician model of surgeon, interventionalist, and intensivist" is an unsafe anachronism and must be abandoned.

The authors' analyses are not statistically sound and have several limitations. First, they do not provide confidence intervals with any of their estimates, nor do they provide expected and observed rate of complications to compare with. Second, this database consists of self-reported complications from academic centers "with a focus on quality, safety and excellence," yet it is very well known that self-audits usually result in a lower rate of complications than when the procedural audit is independent.³ Third, one should not accept the validity of any report of operative morbidity and mortality when factors such as volume of cases, case mix, and risk adjustment were not carried out, as it does not allow a fair comparison. Fourth, the authors' data show that the great majority of cases are actually treated by neurosurgeons, followed by radiologists and then neurologists; this is important as the point estimates may be less accurate for those with lower case volume, besides the fact that one cannot exclude the effect of chance,⁴ not to mention the conspicuous absence of the actual absolute numbers and confidence intervals in the data presented. Fifth, the argument of justification of publication on the basis of "statistical significance" without consideration of the points mentioned above is not scientifically sound. Last but not least, there is a major flaw in the design of the study, considering that in October 2009 the *International Classification of Diseases, Ninth Edition* underwent a major revision pertaining to Current Procedural Terminology code 39.72. This actually coincided with the present study and was not accounted for by the authors. The revision included 2 new codes more specific for endovascular coil embolization of intracranial aneurysms, 39.75 and 39.76, to include occlusion of the head or neck vessels using bare coils and bioactive coils, respectively. These new codes leave endovascular treatment of aneurysms using devices other than coils to code 39.72 (Table 1) (see <https://www.cms.gov/Regulations-and-Guidance/Guidance/Transmittals/downloads/r1770cp.pdf>).

TABLE 1. New ICD-9 codes, added in 2009, related to diagnosis and coiling of aneurysms

Code No.	Description
ICD-9 procedure	
39.72	Endovascular embolization or occlusion of head & neck vessels (description revised October 1, 2009)
39.75	Endovascular embolization or occlusion of vessel(s) of head or neck using bare coils (new October 1, 2009)
39.76	Endovascular embolization or occlusion of vessel(s) of head or neck using bioactive coils (new October 1, 2009)
ICD-9 diagnosis	
430	Subarachnoid hemorrhage
437.3	Cerebral aneurysm, nonruptured

We wholeheartedly agree with the idea that interventional procedures need to be audited, which requires knowledge of the expected and acceptable rate of complications as well as risk adjustment. We further agree that training needs to be standardized. However, audits and standardization will require collaborative work for which we would welcome initiatives such as the American College of Surgeon National Surgical Quality Improvement Program (ACS NSQIP; <https://www.facs.org/quality-programs/acs-nsqip>) applied to neuroendovascular procedures. We would also welcome interspecialty discussions targeting standardization of training and the creation of board certification that is inclusive of, and accounts for, the different backgrounds of those performing the procedures, out of fairness to patients, society, and practitioners from all training backgrounds entering the specialty. In the end, the volume of cases and the presence of a multidisciplinary team providing care from admission to discharge may prove to be the key determinants of the level of skill and rate of complications rather than the background specialty of the operator. We also think that claims like those by Fennell et al., are unfair and potentially inflammatory given the substantial limitations mentioned above.

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Disclosures

The authors report no conflict of interest.

Response

We wish to thank Dr. Maud and his colleagues for their comments in regard to our article. Their critique is at times valid, and we would like to address individual points.

The authors comment "... periprocedural care is as important or even more so than the actual procedure, and morbidity and mortality may be unrelated to the procedure." We agree with this point on aneurysm ruptures but disagree with the authors on elective treatment of unruptured aneurysms. Similar findings of complications based on specialty training were noted between both ruptured and unruptured aneurysms. In the senior authors' experience, postoperative treatment is performed by the treating physician, although it is noted that this may not be true at all facilities and may be part of the reason for the difference in patient outcomes.

The authors comment, "The authors' analyses are not statistically sound and have several limitations." While we do not agree that the statistics are unsound, we have provided an updated Table 1 that provides standard error (SE). The statistical error and findings initially reported are unchanged. We have addressed the issue of case volume in our editorial response to Dr. Walter Montanera published with the original article in the January 2016 issue of *Journal of Neurosurgery* and refer the authors and readers to this piece.¹ We acknowledge that the additional codes should have been included in the analysis, but we do not suspect that their inclusion would change the findings in any meaningful way. We acknowledge limitations of the data set but feel the uniformity of the University Health System Consortium database (in that only academic centers are included) allows for some estimation of heterogeneity of cases between practitioners.

We suspect all interventionists treating cerebral aneurysms ultimately want the same thing for their patients: good outcomes. Using large databases such as the University Health System Consortium allows access to a large volume of health care data, which permits the drawing of potentially important clinical conclusions. To ignore such data may be to ignore a potential opportunity for improvement in patient care. We regret that the authors view the article as "potentially inflammatory," and, for the benefit of the reader, we repeat our primary conclusion: "In this study there was a statistically significant finding that neurosurgically trained physicians may demonstrate improved outcomes with respect to endovascular treatment

TABLE 1. Original Table 1 with the addition of SE of the mean

Physician Specialty	Unruptured Intracerebral Aneurysms (SE)		Subarachnoid Hemorrhage (SE)	
	Morbidity	Mortality	Morbidity	Mortality
Neurosurgery	5.4% (0.5%)	1.4% (0.1%)	27% (0.7%)	11.5% (0.5%)
Neurology	11.1% (0.8%)	3.0% (0.4%)	34% (1.2%)	13.5% (0.9%)
Interventional Radiology	6.6% (0.4%)	1.3% (0.2%)	30% (1.0%)	12.1% (0.7%)

of unruptured aneurysms in this cohort. This finding warrants further investigation.” Indeed, we see this as an open question, and this data set should not be construed as high-level evidence. We again welcome higher-level evidence on this interesting topic.

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The Parkland Carotid and Vertebral Artery Injury Survey

TO THE EDITOR: We read with great interest the article by Scott et al.³ regarding the screening, management, and follow-up of Grade 3 and 4 blunt carotid artery injuries (Scott WW, Sharp S, Figueroa SA, et al: Clinical and radiographic outcomes following traumatic Grade 3 and 4 carotid artery injuries: a 10-year retrospective analysis from a Level 1 trauma center. The Parkland Carotid and Vertebral Artery Injury Survey. *J Neurosurg* 122:610–615, March 2015). It is one out of a set of 4 important articles regarding the Parkland Carotid and Vertebral Artery Injury Survey (PCVAIS), undertaken to determine outcome after the treatment of different grades of blunt artery injuries.

For grading the injuries, Scott and colleagues refer to the established grading scale described by Biffi et al.¹ in an article published in 1999, and they invoke the previous publication on the subject as part of the rationale informing their own study. However, the authors seem to have modified the original grading scale without further comment or scientific validation.^{1,3} Originally Biffi et al. described Grade I lesions as luminal irregularity or dissection with < 25% luminal narrowing; Grade II as dissection or intraluminal hematoma with ≥ 25% luminal narrowing, intraluminal thrombus, or raised intimal flap with an intimal flap or thrombus; Grade III as pseudoaneurysm; Grade IV as occlusion; and Grade V as transection with free extravasation.¹ In the PCVAIS study, there is a change in defini-

tion of Grades II and III: Grade II became a dissection or intraluminal hematoma with 25%–50% narrowing, intraluminal thrombus, or raised intimal flap with an intimal flap or thrombus; and Grade III became narrowing > 50% or aneurysm.³ The PCVAIS Grades I, IV, and V seem to have been left untouched, but the authors have divided the original Grade II injuries (luminal narrowing of > 25%) into narrowing of 25%–50% and 50%–99%. The original Grade III (originally a pseudoaneurysm) is not included anymore, and luminal narrowing > 50% without pseudoaneurysm was reassigned from Grade II to Grade III in the PCVAIS grading scale. The comparability of the PCVAIS study to previously published studies is diminished by the modifications of the grading scale, and the modifications may explain discrepancies in outcome between studies using the original Biffi scale and those using the modified scale. Possibly the modifications employed by the PCVAIS authors are valid as there may be a difference in outcome between an injury causing luminal narrowing of 26% and injury causing narrowing of 87%. However, for readers interpreting the results of the PCVAIS study, we feel it is crucial to be aware of the modifications made to the original Biffi grading scale. Consequently, this letter is intended to make these discrepancies explicit.

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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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Electing the surgical approach in craniopharyngioma surgery

TO THE EDITOR: We read with great interest the study authored by Jeswani et al.³ (Jeswani S, Nuño M, Wu A, et al: Comparative analysis of outcomes following craniotomy and expanded endoscopic endonasal transsphenoidal resection of craniopharyngioma and related tumors: a single-institution study. *J Neurosurg* 124:627–628, March 2016).

Despite great advances in surgical techniques and hormone replacement therapies, managing craniopharyngioma has been a source of dilemmas for many neurosurgeons over the past 50 years with respect to both the pediatric and the adult age groups.^{1–4} This is because these tumors are considered malignant on the basis of their location and behavior, although they are histopathologically benign.

We believe it is difficult to make a general conclusion concerning the best surgical approach for this specific type of brain tumor, especially when it comes to craniotomy versus a transsphenoidal approach. Jeswani et al. stated “that a surgeon’s expertise was the strongest factor driving surgical choice in this series.”³ However, we have found that in electing the surgical approach in the case of craniopharyngiomas we need to apply what we can call the concept of a patient-oriented surgical plan, which means that the surgical approach should be designed, redesigned, and adjusted from one patient to another. This patient-oriented surgical plan should always take into consideration the following crucial points: the microsurgical anatomy of the location, tumor extension, the tumor’s relationship to critical structures, directions of tumor growth, the distribution of calcification, the number of cystic formations, and the relationship of these formations to the solid component. During the actual surgery, these points can be practically translated as follows.

First, the surgical approach should allow multiple trajectories. This will give the surgeon the ability to protect critical structures by shifting from one trajectory to another, in such a way as to be able to conduct safe maneuvers around these critical anatomical structures.

Second, the surgical trajectory should allow the surgeon to tackle the solid component first. The cyst capsule should follow without much difficulty.

Third, beware of calcified cyst capsules; usually they are adherent to the great vessels. Also, craniopharyngiomas almost always are adherent to the diaphragma sellae. This will always be a point of resistance during the resection. Transecting the diaphragma sellae circumferentially will facilitate the mobilization of the sellar component of

the craniopharyngioma, and will help facilitate complete resection. Usually this will be the solid component of the craniopharyngioma.

Fourth, if the pituitary stalk is involved with the craniopharyngioma, do not hesitate to transect it and remove it with the tumor, since it can be a source of recurrence.

Fifth, the surgical trajectory must be planned to not go through the hypothalamus. Identification of the tumor’s point of origin and direction of growth will help in determining whether the hypothalamus is pushed upward or downward. For example, when the hypothalamus is pushed upward by a tumor through the lamina terminalis, part of the hypothalamus could appear very thin and transparent and could be mistaken for part of the tumor capsule. Resection of this tissue will lead to a serious hypothalamic dysfunction and to major postoperative complications.

All in all, we believe that in the treatment of patients with craniopharyngiomas, the choice of one surgical approach over another (for example, a craniotomy vs a transsphenoidal approach) should be made on an individual basis, taking into account the above-highlighted technical points and tailoring the approach to the specific case.

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Response

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

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Glioblastoma in the elderly

TO THE EDITOR: We read with great interest the recent article published by Babu and colleagues² (Babu R,

Komisarow JM, Agarwal VJ, et al: Glioblastoma in the elderly: the effect of aggressive and modern therapies on survival. *J Neurosurg* 124:998–1007, April 2016). This is an important and timely contribution to the literature.

The standard of care for glioblastoma multiforme (GBM) involves resection to the maximal extent feasible, followed by radiotherapy plus concomitant and adjuvant temozolomide (TMZ).⁸ Nonetheless, the operative treatment of GBM in the elderly has tended toward more conservative approaches, mostly biopsy, rather than aggressive resection, the rationale being that these patients have a worse overall prognosis and higher operative risk due to poorer physiological reserves, multiple medical comorbidities, and polypharmacy.^{3,5,7,9} More recently, this treatment paradigm has been challenged by a growing body of literature, including the current paper, which has provided evidence of survival benefit in elderly patients with greater extent of resection.

The authors retrospectively reviewed 120 elderly patients (≥ 65 years old) with newly diagnosed GBM and evaluated predictors of survival through univariate and multivariate regression. All patients underwent cytoreductive resection, with a gross-total resection (GTR) being achieved in 63%. Most (97%) patients received postoperative radiotherapy and concurrent TMZ. A majority (59%) of patients received additional agents, including bevacizumab (39%), irinotecan (21%), etoposide (19%), lomustine (11%), and immunotherapy (5%). A GTR was associated with longer survival times (median 14.1 months) compared to subtotal resection (STR, median 9.6 months). Other predictors of survival on univariate analysis included younger age (< 75 years old), female sex, higher preoperative functional status as measured by the Karnofsky Performance Scale (KPS), use of bevacizumab, use of irinotecan, and administration of stereotactic radiosurgery after focal tumor progression. Younger age, higher KPS score, and use of bevacizumab were independent predictors of survival on stepwise multivariate analysis. Although GTR improved survival, its effect did not reach statistical significance (hazard ratio 0.70, $p = 0.10$).

The work by Babu et al.² is limited by its retrospective design and a relatively small sample size of patients from a single institution. The authors excluded patients who underwent only stereotactic biopsy. Certainly, it would have been interesting to see whether and how survival changed with progressively greater extents of cytoreductive surgery (i.e., biopsy vs STR vs GTR). Moreover, a key limitation of the current paper is the lack of a functional or quality of life (QOL) outcome measure, such as postoperative KPS score. It is important to weigh the impact of any intervention on survival against its effect on QOL, because life-prolonging therapies that critically impair QOL may ultimately be considered detrimental rather than beneficial. In a recent systematic review and meta-analysis including 34 studies and 12,607 patients, we found progressively longer median overall survival (OS) times with greater extents of resection: biopsy, 5.7 months; STR, 8.7 months; and GTR, 14.0 months.¹ We also found significant stepwise improvement in postoperative KPS score with biopsy (median 62.5), STR (median 65.2), and GTR (75.9), and similarly for progression-free survival. Importantly,

greater degrees of resection did not result in greater morbidity or mortality.

In addition to treatment-related factors, such as extent of resection, Babu and colleagues² also evaluated the association of several patient- and tumor-related factors with OS, including age, sex, preoperative KPS score, medical comorbidities (as evaluated by the Charlson index), tumor location, and tumor size. The importance of tailoring the therapeutic approach to the individual patient and clinical scenario is increasingly recognized in the context of elderly patients with GBM. These patients are a heterogeneous group, and we cannot expect them to do uniformly poorly with aggressive cytoreductive surgery, and the corollary is that we cannot expect them to do uniformly well either. These types of analyses are hence critical to help us identify which patients benefit most from surgery, and those for whom such efforts may be futile and potentially even harmful. In 2011, Chaichana et al.⁴ found a KPS score of < 80 , chronic obstructive pulmonary disease, motor deficit, language deficit, cognitive deficit, and tumor size > 4 cm to be preoperative factors independently associated with decreased survival among patients > 65 years who underwent aggressive resection of GBM. They proposed a 3-tiered prognostic classification scheme based on the number of these factors present: 0–1 (median OS 9.2 months), 2–3 (median OS 5.5 months), or 4–6 (median OS 4.4 months). Of note, although age > 75 years was associated with decreased survival on univariate analysis, this association was not significant on multivariate analysis controlling for other preoperative factors, suggesting that the influence of age is relative rather than absolute. On the other hand, Babu et al.² found age to be significant on both univariate and multivariate analyses. Indeed, it would be interesting to see how the model proposed by Chaichana et al.⁴ fares in an independent cohort, such as the one studied by Babu and associates.

The incidence of GBM increases with age, peaking between 65 and 84 years.¹⁰ In recent years, there has been a steep rise in the incidence of GBM among the elderly.⁶ This, together with the aging population, makes optimization of the treatment of GBM in the elderly a key priority in the field of neuro-oncology. We thank and congratulate the authors of this paper for their effort toward this goal.

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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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Electric current application for motor tract mapping

TO THE EDITOR: We read with interest the articles by Raabe et al.⁴ (Raabe A, Beck J, Schucht P, et al: Continuous dynamic mapping of the corticospinal tract during surgery of motor eloquent brain tumors: evaluation of a new method. *J Neurosurg* **120**:1015–1024, May 2014) and Shibana et al.⁸ (Shibana E, Krieg SM, Haller B, et al: Intraoperative subcortical motor evoked potential stimulation: how close is the corticospinal tract? *J Neurosurg* **123**:711–720, September 2015).

Stimulation of the corticospinal motor tract has been for a long time a valuable intraoperative technique of localization. Initially performed in awake surgeries with the so-called 60-Hz stimulation technique for cortical mapping,⁵ it is nowadays used under general anesthesia and even with the “trains of five” technique.² The latter does not generate 60 pulses anymore, but 5 stimulating pulses per second, decreasing substantially the electric charge, and thereby reducing the risk of eliciting stimulation-induced seizures during mapping (approximately 720 μC

with 1-msec pulse duration for the 60-Hz stimulation, vs 24 μC with 400- μsec pulse duration for trains of five at, for example, 12 mA).

Beyond localization, mapping techniques are becoming more and more a preventive tool for the anatomical delineation of a safe resection distance to the corticospinal tract.^{6,7} Recently it has been proposed that as an alternative to a dedicated stimulation probe, an aspirator could be used as the stimulation tool.⁴ This avoids the frequent change of tools in the hands of the surgeon that still hampers the process of surgery when using conventional stimulation probes. Stimulation through one of the surgical tools also offers the possibility for continuous mapping. In line with this original idea, stimulation through the resection tool itself (Söring handpiece, Söring GmbH; Cavitron ultrasonic surgical aspirator [CUSA], Integra) was initiated.

Stimulation through the resection tools per se is based on the knowledge of electrical principles: when applying an electrical current to a conducting path, free electrons flow in the wire as soon as the path is closed (i.e., once the tool touches the brain); when a current source stimulator is used, as offered by contemporary neuromonitoring systems, the same amount of electrical charges will flow through the conducting path, whatever its impedance.

As shown in Fig. 1, this path is made of the following: 1) the distal part of the tool (i.e., one part of the spherical or ring-shaped tip of the tool in contact with the brain); 2) the surface of the brain that is in contact with this tool; and 3) the return electrode in contact with the patient (e.g., subdermal electrodes placed contralaterally to the site of surgery). Figure 2 shows 4-mA currents flowing within the conducting path made with, from top to bottom, a stimulation probe (Inomed, stainless steel); an aspirator (Inomed, stainless steel); a CUSA tip (e.g., titanium nitride); and a Söring handpiece (titanium aluminum) applied on the biological tissue, measured across a 1-kOhm resistance, placed in series within the aforementioned current loop.

Again, whatever is used as a stimulation tool—a probe per se, an aspirator, or a resection tool—the current flowing through the brain will be the same, depending only on the amplitude requested to the current source. Measuring the muscle contraction thresholds with these different tools is neglecting these universal electrical rules. The measurements that do not respect the pure linear relation in Fig. 2 of Raabe et al.⁴ can only be attributed to measurement errors, like the reproducibility of the stimulation location. The variance described in Fig. 4 of Shibana et al.⁸ is also due to such measurement errors.

In the 21st century, while so-called smart and con-

FIG. 1. Scheme of the equivalent circuit of cortical or subcortical mapping.

FIG. 2. Voltage generated by a 4-mA current, measured across a 1-kOhm resistance, placed in series within the loop made of the stimulator—either (from top to bottom) a stimulation probe (Inomed), an aspirator (Inomed), a CUSA tip, or a Söring handpiece—and the patient's brain. Irrespective of the probe, the same current flows through the loop. Note that the polarity of the pulses was alternated through the use of biphasic pulses for safety considerations (see Merrill et al.³ and Brummer and Turner¹). Figure is available in color online only.

nected tools are becoming part of our lives, neurosurgical techniques benefit from integrating multidisciplinary teams, including engineers, in their daily work.

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Disclosures

Mrs. Boëx has given lectures during workshops for Medtronic.

Response

No response was received from Shiban et al.

Response

Improvement of mapping strategies is increasingly becoming a focus in supratentorial tumor surgery. Initially only performed on the cortex,^{2,10} mapping was soon also established at the subcortical level^{1,3} to preserve the white matter tracts. Recently, the spatial and temporal limitations of the conventional intermittent mapping technique have been overcome to create the possibility of continuous “dynamic” subcortical stimulation. This can only be achieved by integrating the stimulation probe in a surgical instrument that is used during tumor resection.¹⁴ We favored the integration of mapping into a standard suction device because continuous dynamic mapping should be available during all steps of tumor removal, including hemostasis or subpial dissection, which may be performed with a variety of instruments.

To obtain information about distance from a specific site of resection to the CST, many groups have correlated

the stimulation intensity (in mA) needed to elicit motor evoked potentials with the distance (in mm) to the CST.^{4,5,7–9,11} Until now, no definitive statement on this relationship has been possible, but the rule of thumb “1 mA correlates to 1 mm” is increasingly being used. We know that this is only a rough estimate, but it has turned out to be a reliable and practical one. We have also tried to approach the dilemma from another perspective by correlating the lowest stimulation intensities during subcortical mapping to postoperative short-term and long-term motor outcome.^{6,13}

Taking all these aspects into account, the importance of the statement made in the Letter's original title (“About the electric current applied for motor tract mapping”) becomes evident. Tissue impedance might influence motor thresholds during stimulation, and therefore constant current or current source stimulators are important.¹³ In the letter from Boëx et al., the experiment in Fig. 2 clearly demonstrates that the current will be the same regardless of the applied stimulation device (monopolar probe, suction device, or CUSA). Therefore the small differences in some measurements between suction device and monopolar probe (Fig. 2 of Raabe et al.) as well as between CUSA and monopolar probe (Fig. 4 of Shibani et al.¹⁴) might be explained by measurement errors arising because the stimulation site or depth used are not exactly the same, but they might also be attributable to other confounding factors.

It is particularly important to highlight once more that when discussing correlation of stimulation intensities and distance to the CST, more neurophysiological aspects should be considered, especially when comparing different studies.^{1,12,13,15}

- Stimulation paradigm: Penfield stimulation (50 or 60 Hz) versus short-train/high-frequency/train-of-five stimulation

And, especially when discussing short-train stimulation studies, the following should be taken into account.^{12,13}

- Pulse duration
- Pulse configuration (anodal vs cathodal)
- Number of pulses in a train
- Interstimulus interval between the pulses
- Repetition rate of the individual trains

The choice of the stimulation probe configuration (monopolar vs bipolar) will also influence that correlation independently of the selected stimulation paradigm (Penfield vs short train).^{1,12,13,15}

We believe that the integration of the stimulation probe into the surgical instrument could increase the reliability, acceptance, and clinical handling of subcortical mapping. We thank the reviewers for highlighting this topic once more.

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Disclosures

Dr. Raabe is a consultant for Inomed.

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Contemporary carotid imaging

TO THE EDITOR: We read with interest the authors' timely and thorough review of this important subject¹ (Brinjikji W, Huston J III, Rabinstein AA, et al: Contemporary carotid imaging: from degree of stenosis to plaque vulnerability. *J Neurosurg* 124:27–42, January 2016). However, we wish to point out that the statement made by the authors, “No studies to date have used MDCTA [multidetector-row CT angiography] plaque imaging to select candidates for revascularization therapy” is incorrect. In 2015 our group² published “Plaque morphology (the PLAC Scale) on CT angiography: predicting long-term anatomical success of primary carotid stenting.” We found that the long-term morphological outcome of primary carotid stenting was predicted with considerable accuracy by using a straightforward MDCT carotid plaque grading scale. We continue to use the PLAC (Predicting Long-term outcome with Angioplasty of the Carotid artery) grading scale on a daily basis to assist in decision making with regard to carotid revascularization, particularly the safety and efficacy of primary carotid stenting.

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Disclosures

The authors report no conflict of interest.

Response

We thank Drs. Lownie and Pelz for their interest in our

manuscript “Contemporary carotid imaging: from degree of stenosis to plaque vulnerability.” We read with great interest their article “Plaque morphology (the PLAC Scale) on CT angiography: predicting long-term anatomical success of primary carotid stenting,”¹ which was published after our review was accepted for publication. In their article, the authors presented a straightforward scale that quantifies the extent of calcified and soft plaque in the carotid artery and demonstrated how it can be used to select patients for primary carotid stenting (i.e., without pre-stenting angioplasty) and carotid angioplasty with stenting. The authors found that if the calcification grade of a carotid plaque is low and soft plaque is present, primary carotid stenting without angioplasty can result in good long-term anatomical outcomes. Conversely, if the calcification grade is high and soft plaque is absent, either angioplasty and stenting with an embolic protection device or carotid endarterectomy should be considered. Further studies examining the role of plaque characterization with CT angiography and MR angiography in patient selection for carotid revascularization therapies are needed to determine optimal therapies for patients with carotid artery stenosis.

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