Neurosurgical Forum LETTERS TO THE EDITOR

Acquisition of quantitative Flow 800 vascular mapping from qualitative intraoperative ICG angiography

TO THE EDITOR: I read with interest the article by Marbacher et al.,⁴ who have confirmed the accuracy of intraoperative indocyanine green (ICG) angiography comparable to that of the gold-standard intraoperative digital subtraction angiography (Marbacher S, Mendelowitsch I, Grüter BE, et al: Comparison of 3D intraoperative digital subtraction angiography and intraoperative indocyanine green video angiography during intracranial aneurysm surgery. *J Neurosurg* [epub ahead of print July 13, 2018. DOI: 10.3171/2018.1.JNS172253]). This ensures the acquisition of equivalent results, avoiding the limitations of intraoperative digital subtraction angiography (DSA), such as technical complexities and prolonged operative time as well as minimizing associated risks of radiation hazards and neurological complications.^{3,5} The addition of a Flow 800 study that can be auto-generated from the ICG study can provide quantitative assessment with regard to the pattern of flow velocities owing to simple analysis of the results of the average absorption intensity (AI) and time lag in the appearance of the dye.¹

This assessment can further safeguard us in identifying patients who are at risk for developing postoperative vasospasm, especially those in whom there is significant discrepancy in AI and time lag values between the parent and the branching vessels. Prophylactic as well as rescue management can be initiated in a timely manner with added measures to minimize complications such as the placement of a Swan-Ganz catheter in preventing pulmonary edema during augmented normovolemic hypertensive therapy.²

This simple additive measure of the acquisition of Flow 800 quantitative data from quality images of the ICG study can provide both diagnostic and therapeutic benefits, thereby ensuring better patient safety.

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Disclosures

The author reports no conflict of interest.

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INCLUDE WHEN CITING

Published online October 12, 2018; DOI: 10.3171/2018.7.JNS182083.

Response

We thank Dr. Munakomi for his interest in our findings and appreciate the opportunity to further discuss the implications of our study related to his comments. We agree that intraoperative ICG video angiography (ICGVA) can be an extraordinary tool in cerebrovascular cases, specifically in intracranial aneurysm (IA) surgery.⁷ In this regard, we recognize the potential benefit that Dr. Munakomi pointed out when generating blood flow dynamics data from ICGVA via the semiquantitative software analyzing tool Flow 800 (ZEISS), which can expand our diagnostic armamentarium during surgical IA occlusion.^{2,3,6}

Dr. Munakomi ascertained equal results with ICGVA that could avoid the limitations of intraoperative DSA (iDSA). Despite the undisputed utility of ICGVA, our study detected a 6% rate of false-negative findings, as determined by 3D-iDSA, which necessitated immediate surgical intervention. Such results confirm iDSA as the gold standard for the assessment of surgical outcome.

FIG. 1. In multiplanar 3D-iDSA, virtual rotation of the clipped aneurysm allows visualization in any direction, and therefore enables detection of even the smallest aneurysm remnant (*white arrow*) hidden behind the clipped aneurysm complex. The hybrid operating room delivering high-quality 3D-iDSA eliminates the need for postoperative DSA and warrants unselected use by supplanting postoperative angiography as the standard of care in surgical IA treatment.

However, the value of ICGVA and iDSA cannot be evaluated separately because they influence each other. Each vascular monitoring tool (i.e., visual inspection, Doppler ultrasonography, ICGVA, iDSA, endoscopy, electrophysiological monitoring) offers a distinct advantage, but when combined they can be further optimized for each patient's safety.⁴

To date, 3D-DSA is considered the most accurate technique to assess the clipped IA; in fact, some authors insist on its routine postoperative use.^{1,5} In the hybrid operating room, however, validation of the surgical result can be achieved intraoperatively with 3D-iDSA, thus maximizing patient safety. High-quality 3D-iDSA eliminates the need for postoperative DSA, warrants unselected control of all IAs undergoing surgical obliteration, and thus holds the potential to become the standard of care in surgical IA treatment. In our recent analysis of 32 clipped IAs in 26 patients who underwent both intraoperative and postoperative 3D-DSA, imaging quality was equally excellent for both modalities, and no discord occurred in our assessment of the surgical result in any of these aneurysms (Fig. 1).

At our institution, unselected routine use of iDSA, including 3D visualization of the clipped IA, has replaced postoperative 3D-DSA as our quality control; it also serves as a baseline to further monitor all surgically treated IA patients. In this era when surgeons have less exposure to and experience with open IA surgery, 3D-iDSA has im-

proved intraoperative understanding of the angioarchitecture of the vascular complex that consists of the clip, the aneurysm, and the parent arteries. As recognized by Dr. Munakomi, 3D-iDSA can facilitate decision-making and strategies that can immediately improve the surgical result or manage intraoperative and early postoperative hemodynamics if needed.

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INCLUDE WHEN CITING

Published online October 12, 2018; DOI: 10.3171/2018.9.JNS182145.

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Glasgow Coma Scale–Pupils Score: opening the eyes to new ways of predicting outcomes in TBI

TO THE EDITOR: We have read with great attention the articles by Brennan et al.¹ and Murray et al.⁴ (Brennan PM, Murray GD, Teasdale GM: Simplifying the use of prognostic information in traumatic brain injury. Part 1: The GCS-Pupils score: an extended index of clinical severity. *J Neurosurg* 128:1612–1620, June 2018; Murray GD, Brennan PM, Teasdale GM: Simplifying the use of prognostic information in traumatic brain injury. Part 2: Graphical presentation of probabilities. *J Neurosurg* 128:1621–1634, June 2018). First, we would like to congratulate the authors. These are indeed fascinating papers on traumatic brain injury (TBI). We agree with many of their statements.^{3,4}

The authors performed a study analyzing available data from 9045 and 6855 patients, respectively, from the CRASH (Corticosteroid Randomisation After Significant Head Injury) and IMPACT (International Mission for Prognosis and Clinical Trials in TBI) clinical trials. Information such as Glasgow Coma Scale (GCS) score, pupil response, patient age, CT findings, and Glasgow Outcome Scale (GOS) score were combined into a pooled data set for the main analysis.^{1,3,5,6}

However, we think some analytical issues are worth discussing. First, over the years, more than 100 methods have been tried to make outcome predictions in TBI. The challenge is to transform simple measurements into information for clinical practice. The authors performed an excellent analysis of a large data set, associating the relationship of the GCS-Pupils (GCS-P) score with mortality. They showed an association between a patient's loss of pupil reactivity and a worse outcome. Compared to methods that use other variables, the pupillary response is simpler to evaluate with quite reproducible results.^{3,7,8} However, the type of pupil response may compromise method applicability. Patient sedation is a factor because it may alter the pupillary response.

Theirs was a retrospective study based on two large and heterogeneous trials. We believe that although the statistical evaluation is adequate, it may harbor bias because of free data and different protocol evaluations and popula-

tions. A prospective study may be an alternative to these problems, although they are long and expensive.

The loss of pupil reactivity was more common in patients with GCS scores at or below 8 (35.7%) than in the patients with GCS scores 13 and higher (2.1%), precisely the subgroup that needs information on mortality. Therefore, there is a differential loss, which causes bias and may impact the final results. Conversely, as acknowledged by the authors, patients with a GCS-P score of 4 had higher rates of mortality and unfavorable outcomes than individuals with a GCS-P score of 3. Additionally, a GCS-P score of 1 or 2 slightly underestimated the likelihood of an adverse outcome. These facts may be related to selection bias in the original trials.

Nonetheless, when it comes to consciousness level scales, a pertinent question is whether we should test more or less. The Simplified Motor Score (SMS)² is an interesting alternative to the GCS that has been proven to be a good predictor of outcome. The verbal response may sometimes be misleading, as focal verbal deficit lowers the score on the GCS without any impairment in consciousness level. It would be interesting to see how the GCS-P performs compared to the SMS.

The critical point is to use the adequate score in clinical practice to predict mortality in brain injury. The GCS-P seems to be a useful tool but needs further investigation to compare it with existing scales and apply it in clinical practice. The inclusion of pupillary response in the GCS is a thoughtful refinement, as we all know the strong clinical meaning of anisocoria or fixed, dilated pupils.^{8,9}

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Disclosures

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INCLUDE WHEN CITING

Published online May 24, 2019; DOI: 10.3171/2019.2.JNS19296.

Response

We are grateful to the authors for their supportive comments on our development of a simple tool that combines the GCS and pupil reactivity data to provide a clinical index of the early severity of TBI. We wish to reassure them that their concerns about “bias” are unnecessary. The data used in our analysis were collected prospectively, not retrospectively as they assert, and it is precisely the large size and heterogeneous nature of the data that makes the results so likely to be widely applicable. Moreover, although the loss of pupil reactivity was more frequent in patients with more severe head injuries—unsurprisingly, given the physiological mechanism by which it arises—we demonstrated that the adverse effect of the loss of pupil reactivity was not restricted to such patients. Instead, it applied across the spectrum of GCS scores from 3 to 15 (Fig. 2 in Brennan et al.) and thus was not subject to bias from the composition of the series.

We report that the pupil reactivity score, derived by subtracting 1 or 2 from the GCS score according to the number of nonreactive pupils, was developed to be simple and that it slightly underestimates the likelihood of poor outcomes at lower GCS scores. This is also not bias from the data, but rather a choice of modeling for clinical utility, informed by statistical analysis.

In the studies from which the data in our analysis were taken (CRASH and IMPACT), pupil reactivity was assessed as a simple pupil response to light. No caveats are described as to how to interpret this simple assessment. When the patient has self-administered or been given drugs that affect pupil reactivity, it will not be possible to make the full assessment necessary to determine the GCS-P score. Similarly, the apparent lower mortality with a GCS score of 3 versus 4 is likely to result from allocating a score to patients whose responsiveness was depressed pharmacologically (“pseudothrees”), and the smoothing out of the relationship is a further advantage of the GCS-P score.²

Like the authors, we would welcome assessment of the GCS-P and the associated GCS-PA chart (GCS-P plus age) in other large, relevant neurotrauma data sets and against other systems for the assessment of TBI severity.

However, we do not expect the shortened scales to match the discrimination in stratifying patients for early severity, for monitoring changes in the individual, or in relating to prognosis for different late outcomes provided by the 15-point GCS-P. The SMS the authors refer to was actually developed for use by technicians in making binary decisions in prehospital care, and extensive studies have already shown that it has no benefit over the basic GCS in predicting early mortality.^{1,3} We do agree with Nicollas Rabelo and his colleagues that there is a need to strike a balance between simplification and complexity in estimating a patient’s likely outcome. We believe that the combination of clinical, demographic, and imaging information in the GCS-PA charts³ provides this in a way that will be easy to understand and use in clinical practice.

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Published online May 24, 2019; DOI: 10.3171/2019.2.JNS19358.

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Impaired glymphatic pathway as potential contributing factor in amyloid beta accumulation and enhanced neuroinflammation after IVH

TO THE EDITOR: We read with great interest the article by Jiang and colleagues⁹ (Jiang C, Zou X, Zhu R, et al: The correlation between accumulation of amyloid beta with enhanced neuroinflammation and cognitive impairment after intraventricular hemorrhage.

J Neurosurg [epub ahead of print July 20, 2018. DOI: 10.3171/2018.1.JNS172938]) regarding the correlation between amyloid beta (A β) accumulation with enhanced neuroinflammation and cognitive impairment in a rat model of intraventricular hemorrhage (IVH). The authors performed an in vivo study examining biomarkers of A β accumulation and neuroinflammation and evaluating the cognitive function in a rat model of IVH from autologous arterial blood injection. They demonstrated that elevated levels of A β and neuroinflammation biomarkers, including microgliosis, astrogliosis, tumor necrosis factor- α , and interleukin-1, were present in the rats with IVH, and these animals also showed impairment of cognitive function, correlating with the A β accumulation and enhanced neuroinflammation. IVH, as a common complication in intracerebral hemorrhage (ICH), is a proven negative prognostic factor.⁶ Mechanism of secondary brain injury in IVH is of utmost importance, while, as highlighted by the authors, literature in this area is poor. We therefore would like to congratulate the authors on their study.

We agree with the idea in general that IVH could induce A β accumulation and enhanced neuroinflammation and in turn may lead to neurocognitive dysfunction of the rats, which is in accord with our data. However, we intend to offer some constructive criticism on the hypothesis of the mechanism of A β accumulation and accompanying neuroinflammation. The authors attributed the A β accumulation and neuroinflammation to iron accumulation based on the co-localization of A β and ferritin. It is well-accepted that iron overload has a key role in IVH-induced brain damage.² Ferritin, an iron storage protein, results, in this rat model of IVH, from the blood extended into the ventricles; therefore, we believe that the amounts of ferritin should correlate with the blood volume in the ventricles. However, the expression of A β and the biomarkers of neuroinflammation such as IBA-1 and GFAP present as a trend of progressive elevation over time. We thus speculate that there might be other factors contributing to the accumulation of A β and neuroinflammatory factors. The glymphatic system (GS), a specialized brain-wide paravascular pathway, is essential for the clearance of interstitial waste via cerebrospinal fluid (CSF).⁸ GS dysfunction could contribute to the accumulation of A β and tau in neurodegenerative disease and traumatic brain injury.^{7,8} Gaberel et al. assessed the movement of gadolinium in CSF in order to investigate the role of GS clearance in stroke and found that impaired glymphatic perfusion and clearance could be observed in ischemic stroke and subarachnoid hemorrhage (SAH) but was absent in ICH.³ More recently, Luo et al. demonstrated in an animal model that blood components rapidly enter the paravascular space after SAH and cause widespread perivascular neuroinflammation.¹⁰

Additionally, hydrocephalus, a common complication in IVH, is considered to be an important reason for neurocognitive dysfunction.¹ Previous animal studies have demonstrated that IVH induced via intraventricular autologous blood injection could lead to hydrocephalus, which might also contribute to the neurocognitive outcome.^{1,4,5} Therefore, we believe that the influence of ventricular dilation on neurocognitive outcome should be taken into consideration in interpreting the findings of Jiang et al.

After all, the study is potentially novel, and addresses a clinical question of substantial potential interest, which might trigger further investigations of IVH.

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Disclosures

The authors report no conflict of interest.

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INCLUDE WHEN CITING

Published online November 9, 2018; DOI: 10.3171/2018.8.JNS182194.

Response

We thank Drs. Zhang and You for their interest in and comments on our article regarding the correlation between accumulation of amyloid beta with enhanced neuroinflammation and cognitive impairment after IVH. They made

the very good suggestion that glymphatic pathway impairment may contribute to the accumulation of A β and enhanced neuroinflammation in IVH. We agree and genuinely appreciate this idea.

As we found the aberrant accumulation of A β after IVH in our research, it is natural to consider the possible reasons, which can be categorized as either aberrant production or impaired clearance of A β . On the one hand, amyloid precursor protein (APP) can be cleaved by beta- and gamma-secretase to generate A β . We have noticed increased activity of BACE1, the major beta-secretase in the brain, but not aberrant accumulation or distribution of APP in our research. On the other hand, various mechanisms exist to clear the A β , including intra- and extra-enzymatic degradation, transport across the blood-brain barrier, CSF absorption, and interstitial fluid (ISF) bulk flow through glymphatic system.⁵ The GS is a brain-wide paravascular pathway that facilitates ISF bulk flow and CSF recirculation into the brain parenchyma, thus helping clearance of neurotoxic proteins like A β . Aquaporin-4 (AQP4), the astroglial water channel, contributes to perivascular exchange of ISF and CSF in GS.⁴ In AQP4 knockout mice, A β clearance was reduced by 55%-65% compared to wild-type mice.^{3,4} Therefore, AQP4-dependent GS plays a significant role in A β clearance and deserves more attention.

However, there are still some doubts to be resolved. First, as mentioned in the comments, impaired GS was observed in Alzheimer's disease (AD), stroke, and subarachnoid hemorrhage, but not in ICH or IVH. Additionally, the temporal change of GS function is still unknown. New emerging noninvasive techniques like diffusion tensor MRI may make it easier to examine the function of GS in ICH or IVH.² Second, iron overload not only contributes to the secondary injury of IVH, but also correlates with A β accumulation in IVH and AD. To our knowledge, no previous research has investigated the relationship and mechanism of iron overload and glymphatic system impairment. Last, the effort to improve GS function has been made through activation of AQP4 channels to promote glymphatic efflux of A β and many other approaches.¹ But as a matter of fact, none of them has been proven clinically effective. We believe that a major breakthrough depends on a better understanding of the mechanism of GS.

Altogether, GS plays an important role in A β clearance, and impaired GS is potentially a contributory factor to aberrant A β accumulation. The GS status after IVH, the possible mechanism, and potential therapeutic targets require further investigation.

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INCLUDE WHEN CITING

Published online November 9, 2018; DOI: 10.3171/2018.9.JNS182281.

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Endoscopic endonasal surgery and the superior hypophyseal artery: further studies remain mandatory

TO THE EDITOR: We have read with great attention the article by Truong and colleagues⁷ (Truong HQ, Najera E, Zanabria-Ortiz R, et al: Surgical anatomy of the superior hypophyseal artery and its relevance for endoscopic endonasal surgery. *J Neurosurg* [epub ahead of print July 13, 2018. DOI: 10.3171/2018.2.JNS172959]). In their index study, the authors referred to some of our publications,^{1,2,4–6} and as we have been involved in studying the surgical anatomy of the superior hypophyseal artery (SHA) along with the clinical implications,^{1–6} our lead author (A.N.) would like to discuss specific surgical nuances.

First, the concepts of the infundibular arteries as a complementary blood supply to the infundibulum and optic apparatus, arrangements of the SHAs, and the origin of the SHA and its branching patterns are not new. In 1981, Professor Rhoton's group introduced such anatomical data from the cranial prospective using 50 cadaveric adult cerebral hemispheres.³ Interestingly, Rhoton's results³ are quite similar to those in the index study.⁷

Second, in Fig. 5B in their article, the authors stated that “the anastomosis between the [primary SHA] and [secondary SHA] of the same side and that between the posterior SHA and perforators from the [posterior communicating artery] help to reinforce the preinfundibular anastomosis and comprise the circuminfundibular anastomosis.” It is well known that the circuminfundibular plexus³ gives rise to descending (short-stalk penetrating and superficial infundibular arteries) and ascending (supply the inferior surface of the chiasm, proximal portions of the optic nerves, tuber cinereum, and median eminence) branches. What is the quantifiable impact of the suggested “preinfundibular anastomosis” that still needs help from other contributing arteries?

Third, what is the clinical significance of the “no-optic type” of primary SHA? Is it to be sacrificed safely (when needed) regardless of the hierarchy protocol?⁷ In Fig. 2A in their article, although there is evidence of a right no-optic SHA, we can appreciate that the related optic apparatus

is reinforced with a significant artery that runs parallel to the no-optic SHA along with perforating branches from the anterior communicating complex and anastomoses close to the midline. This might lower the risk of postoperative visual deficits following occlusion of such a unilateral (very rare, 1.7%)⁷ no-optic SHA. Even in Fig. 6D in their article, the optic nerve is reinforced by the ascending infundibular artery regardless of “early optic branching.”

Fourth, although the authors⁷ described the surgical relevance of intraoperative manipulation of the SHA via meticulous dissection and scarifying the descending branch (Videos 1 and 2 in their article), we missed the surgical implication of each type on the visual outcome.

Finally, in our experience, the anatomy of the SHA is highly variable, and the unique anastomosis around the chiasm/infundibulum plays a vital role in the postoperative visual outcome. For a better visual outcome, we believe that under the guidance of visual evoked potential (VEP) monitoring during temporary occlusion of the SHA, the ipsilateral SHA can be safely sacrificed (if needed; never bilateral scarification)⁵ to avoid hazardous surgical manipulation around the optic nerve/chiasm. Perhaps, in cases with complex skull base tumors treated with extended endoscopic endonasal surgeries, direct endoscopic temporary clipping of the SHA under VEP monitoring will give a real-time indicator of whether to sacrifice or to preserve the targeted SHA in order to avoid more aggressive surgical manipulations.

We appreciate the authors' contribution to neurosurgery, and we are looking forward to further studies, which remain mandatory.

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Disclosures

The authors report no conflict of interest.

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INCLUDE WHEN CITING

Published online November 30, 2018; DOI: 10.3171/2018.9.JNS182513.

Response

We are appreciative of the attention that Nagm et al. have given our work on the SHA. However, we disagree with several discussion points and believe most are addressed in the paper. Certainly, the study of the SHA is not new, and many authors' works are selectively cited in our paper. Their findings were carefully referenced before and during the process of our investigation. Existing nomenclatures were adopted as much as was scientifically correct, and novel nomenclatures were limited for necessary clarity. The fact that we used the same nomenclature should be seen as our respect and recognition of the work of previous authors but should not deny the novelty of our findings.

The definition of “infundibular artery,” as well as the concept of circuminfundibular anastomosis, was introduced by Dawson in 1958¹ and was again presented by Gibo et al. in the collaborative work with Professor Rhoton in 1981.³ However, the anatomy was more richly described by Gibo and colleagues in a later publication (1988).² Until that time, the origin of the SHA was believed to be in the ophthalmic segment of the internal carotid artery (ICA) (C4 level), i.e., distal to the origin of the ophthalmic artery. The findings of Kobayashi et al. about carotid cave aneurysms suggested that the aneurysm may develop in relation to the SHA and that the SHA may originate proximal to the ophthalmic artery.⁴ Our findings confirm the suggestion of Professor Kobayashi in that the SHA originates more frequently in the paraclinoid segment than the ophthalmic segment of the ICA. Additionally, in both of Gibo's works, the SHA was seen as a group of arteries without further classification. The identification and classification of the primary SHA (pSHA) is a key difference of our paper compared to previous authors with respect to optic nerve perfusion.

The proposed concept of preinfundibular anastomosis, as clearly described and presented in our paper, is part of the circuminfundibular anastomosis, with the former being more constantly present and the major part of the latter. The impact of the preinfundibular anastomosis is not limited to the perfusion of ascending and descending terminal branches but also the “safety” feature of contralateral supplement in case of unilateral occlusion whether iatrogenic or not. Moreover, the concept of arterial anastomosis does not mean one artery helping the other, but rather a mutual relationship with actual blood flow to be determined by the hemodynamic gradient at a given time. From the developmental standpoint, arteriogenesis

is guided by hypoxia or the oxygen demand of the tissues. The fact that there is an artery, or an anastomosis, proves the need for blood flow by local tissues, which is not to be defined by anatomists.

The no-optic type of SHA branching pattern is a rare variant with no available clinical data or experience. It would be reckless to assure the clinical safety of its occlusion. If a no-optic type pSHA is the dominant one of the two sides, the clinical consequence of its occlusion may also affect the opposite side. Regarding Fig. 6D, the presence of the ascending branch from the preinfundibular anastomosis to the optic chiasm does not lessen the role of the optic branch (early optic) in perfusion of the optic nerve in this case because the artery can be seen not yet branching, which means its perfusion territory is more distal on the course of the optic nerve and outside the field of this view.

The clinical application of temporary occlusion of the SHA and intraoperative VEP monitoring in order to predict the visual outcome of SHA sacrifice is beyond the scope of our paper. However, from an anatomical standpoint, it is known that the optic chiasm and intracranial optic nerve are supplied from several sources, including the SHA, the superior chiasmatic arteries from the anterior cerebral artery, and the prechiasmatic artery from the ICA.¹ Therefore, a negative VEP during SHA occlusion, theoretically, cannot rule out a partial loss of visual field, hence wrongly assuring the visual outcome of the occlusion. Moreover, as already discussed in the paper, in the data presented in the authors' series of intraoperative VEP application, no details regarding the specific branch of SHA were provided. An occlusion of the secondary SHA (sSHA) is more likely to have a minimal effect on VEP and visual deficit. This is the key clinical relevance of our proposed classification of pSHA and sSHA.

We appreciate the comments of Nagm et al. and hope our paper adds to and works with their significant body of work and experience. Any discord in these results is surely a reflection of the need for further understanding of the clinical implications of this critical vascular network.

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INCLUDE WHEN CITING

Published online November 30, 2018; DOI: 10.3171/2018.10.JNS182590.

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Egress of CSF during surgery: a thin line separating transition from a friend to a foe

TO THE EDITOR: We read with great interest the article by Czorlich et al.¹ focusing on the context of a rare but potentially devastating complication of visual loss associated with various positioning options in neurosurgery and the potential preventive role of loss of CSF on visual loss (Czorlich P, Krätzig T, Kluge N, et al: Intraocular pressure during neurosurgical procedures in context of head position and loss of cerebrospinal fluid. *J Neurosurg* [epub ahead of print August 24, 2018. DOI: 10.3171/2018.3.JNS173098]). However, compared to supine and lateral positioning, prone positioning has various associated risk factors that confer added risk of visual loss, such as direct ocular compression, increased abdominal pressure, and sometimes inadvertent compression of the neck veins.

So, it is a more rational approach to properly dichotomize the patient on the basis of “equipose” by ensuring similar precautionary measures such as head pin fixation to avoid direct eye pressure, keeping an arterial line for hemodynamic augmentation, ensuring a free abdomen, and elevating the head to a 10° reverse Trendelenberg position. Thereafter, one must study the role of CSF loss selectively in those patients undergoing spine surgery in the prone position and record variability in intraocular pressure (IOP) before and after opening the dura mater.^{2,3}

Furthermore, although loss of CSF certainly decreases intracranial pressure and thereby the IOP, a point of caution should always be taken regarding significant egress of CSF. Such egress can sometimes lead to intracranial hypotension-related venous congestion with resultant brain edema, and thereby paradoxically lead to raised IOP irrespective of the patient's head positioning.^{4,5}

Further research on these aspects will certainly help provide newer insights regarding this critical interplay between the changes in CSF volume and their resultant effect on the ocular circulatory dynamics.

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Disclosures

The author reports no conflict of interest.

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INCLUDE WHEN CITING

Published online November 9, 2018; DOI: 10.3171/2018.9.JNS182519.

Response

We thank Dr. Munakomi for his interest in our article and appreciate his comments that enable us to clarify the conclusions drawn from our research.

It is well known that risk factors like direct mechanical ocular compression, increased abdominal pressure due to prone positioning, compression of the neck veins, and others have an impact on the IOP and/or the microvascularization of the eye. This may lead to an ischemic optic neuropathy and ultimately increase the risk for the occurrence of a perioperative visual loss (POVL).^{2,3,5} It is undisputed that prophylactic strategies to avoid those risk factors—such as careful patient positioning without direct ocular pressure and a free abdomen with at least 10° elevation of the head in reverse Trendelenburg position as well as keeping an arterial line for hemodynamic monitoring—should be addressed first.²

Nevertheless, it is an important aspect of research to question common theories and procedures and to think outside the box. Therefore, from our point of view, it is highly important to discuss other possible influencing factors on the IOP, which might then be addressed as well in the future to reduce the overall risk of POVL in surgical procedures. If our study and subsequent research could show that prophylactic insertion of a lumbar drain to reduce CSF pressure actually leads to an optimized microvascularization of the eye, a new and additional aspect

to prevent or reduce the risk of POVL might have been found. Insertion of a prophylactic CSF drain is an established procedure in patients undergoing aortic aneurysm surgery, and is associated with a significantly lower risk for developing an infarction of the anterior spinal artery due to decreased perfusion pressure despite the correctly addressed, well known, and severe complications associated with a lumbar drain, which include intracranial hematoma, brain edema, etc.^{1,4}

Summarizing all these aspects, we have to emphasize again that the aim of our study was not to evaluate a potential preventive role in the loss of CSF in neurosurgical procedures nor to claim a preventive insertion of a lumbar drain, but rather to demonstrate the correlation between the loss of CSF and the course of the IOP. To evaluate the absolute reduction of the IOP for different diagnoses and surgical procedures, we agree with Dr. Munakomi that further studies will help to find out if a perioperative CSF drain in spinal surgery becomes a friend rather than a foe.

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Published online November 9, 2018; DOI: 10.3171/2018.10.JNS182592.

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