Cognitive assessment in glioma patients

To the Editor: I read with great interest the article by Wu et al.19 (Wu AS, Witgert ME, Lang FF, et al: Neuropsychological function before and after surgery for insular gliomas. Clinical article. J Neurosurg 115:1115–1125, December 2011), in which the authors demonstrated the existence of common neuropsychological impairment in insular as well as noninsular gliomas, nonetheless with few statistically significant differences in both groups at either the pre- or postoperative evaluation.

Interestingly, recent advances in neuroimaging have allowed earlier diagnosis of gliomas, in patients with few symptoms (seizures) or even in asymptomatic patients (incidental discovery), especially diffuse low-grade gliomas (LGGs).12 As a consequence, as stated by Wu et al., a standard neurological examination is not accurate enough to objectively assess these patients. Thus, an extensive neuropsychological examination should be performed in a more systematic way. Indeed, in patients with LGGs, it was shown that more than 90% experienced at least some neuropsychological deficits (for example, working memory disorders) prior to any treatment, whatever the location of the glioma (insular or noninsular).15 In addition, a postoperative neuropsychological assessment is also crucial to better evaluate the possible impact of glioma resection on high-order functions. Therefore, the authors have to be congratulated for their original data, in particular regarding insular gliomas.

However, such extensive cognitive examination should be more actively used to modulate therapeutic management. First of all, on the basis of the presurgical assessment, intraoperative tasks must be adapted to optimize the reliability of functional mapping in awake patients throughout the resection.2 For example, Wu et al. observed greater postoperative decline in the domains of visuoconstruction in patients with right-sided insular tumors. It is worth noting that awake surgery with mapping of spatial awareness (for example, line bisection task) can be achieved in right gliomas to avoid visuospatial impairments, such as hemineglect.16 Awake surgery with language mapping may also be chosen for right-handed patients with right-hemisphere tumors when the presurgical assessment evidences even slight language disorders, showing the participation of the “right nondominant” hemisphere in this function.17 In addition, preoperative neuropsychological scores should be considered as reflecting only a part of the real quality of life, which must be defined for each patient according to his or her job, habits, hobbies, and projects.2 Therefore, it can be important to map different languages as well as language switching in multilingual patients10 or to map calculation in a school teacher,5 cross-modal judgment in a manager,11 syntax in a writer,18 and so forth.

In the postoperative period, the immediate postsurgical cognitive assessment can also be useful to build a specific rehabilitation program. In their report, Wu et al. did not discuss cognitive rehabilitation; a recent prospective randomized trial demonstrated the significant role of such rehabilitation in brain tumor patients.8 In a surgical series of left insular LGG cases, patients benefited from specific rehabilitation at home following resection, on the basis of postoperative cognitive examination that showed working memory deficit despite the lack of language impairment.7 Interestingly, such neuropsychological scores after surgery, especially assessments of lexical access speed, may represent a good predictive factor of the long-term quality of life, in particular concerning return to work.11 Mechanisms of brain plasticity underlying functional compensation are likely elicited by LGG growth itself as well as by adapted rehabilitation.4

Finally, long-term follow-up is essential (although not detailed by Wu et al.), particularly for LGG patients, with a long median survival. To this end, with the aim of better evaluating the benefit-risk ratio of a therapeutic strategy, it was recently proposed to calculate simultaneously (and not separately) both the functional and oncological gain of a treatment, by plotting time with quality of life (including objective neuropsychological assessment) versus time to malignant transformation in LGG.9 This can be helpful in comparing subgroups of patients, such as those with insular gliomas versus those with noninsular gliomas. Indeed, as supported by the results reported by Wu et al., the same surgical treatment must be considered regardless of whether the tumor is located within the insula or elsewhere in the brain—in agreement with the recent surgical series for insular gliomas that showed a low risk associated with surgery and a significant impact on epilepsy control as well as an increase of median survival.5,14 On the other hand, because the resection cannot be complete in all insular cases, combined therapeutic strategies might be considered, for instance by performing (neo)adjuvant chemotherapy. Of note, a recent study has evaluated both quality of life and neurocognition in patients who were treated with a combination of chemotherapy and surgical resection(s) for an LGG, showing an excellent tolerance of combined therapies.1

In summary, longitudinal neuropsychological assessments—before and after each treatment—should be more widely performed in patients with gliomas, especially LGG, in a more active way, in order to 1) select the best surgical tasks during intraoperative mapping at the individual scale, 2) develop a specific postoperative rehabilitative strategy, and 3) help in the determination of the best personalized therapeutic strategy over years.

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Disclosure

The author reports no conflict of interest.

References


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Closed head injury


Head injury often leads to morbidity and mortality. Therefore, the investigation of a novel therapeutic approach is very important. Zhang et al. initiated an experimental investigation to evaluate the therapeutic effect of Cerebrolysin on an experimental closed head injury (CHI) model. They concluded that early Cerebrolysin administration demonstrated a neuroprotective effect, with improvement of functional recovery in rats after CHI.

However, the time point for delayed administration of Cerebrolysin (after CHI for 1 hour) has not yet been investigated. It is very important that delivery of Cerebrolysin should be considered in patients not enrolled in the 1st hour after CHI, because it often happens that some of the patients with CHI present to the emergency room after 1 hour. Therefore, further investigation of the time course of the administration of Cerebrolysin is warranted before clinical trials.

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Disclosure

The authors report no conflict of interest.

Reference

RESPONSE: We appreciate the comments of Dr. Hueng et al. regarding our paper. In our paper, Cerebrolysin was administered intraperitoneally starting at 1 hour after CHI. The primary aim of this study was to investigate the effect of early treatment of CHI with Cerebrolysin in a clinically relevant rat model. We fully agree with the comment from Dr. Hueng et al., and are cognizant that further investigation of the time course for administration of Cerebrolysin is warranted, as was explicitly indicated in the Conclusions section of our article. The study with delayed Cerebrolysin treatment is ongoing in our laboratory. In addition, further investigation of the optimal dose and long-term effect of Cerebrolysin on functional recovery after traumatic brain injury (TBI) is warranted.

Although neuroprotection is an important strategy for the treatment of TBI, to date no effective neuroprotective agents have been identified from TBI clinical trials. The disappointing clinical trials may be due to the heterogeneity of the population of patients with TBI and the variability in treatment approaches. Another important aspect is that most clinical trial strategies have used drugs that mainly target a single pathophysiological mechanism, although many mechanisms are involved in secondary injury after TBI. Recent preclinical studies performed by us and others have revealed that TBI induces neurogenesis, axonal sprouting, synaptogenesis, and angiogenesis, which may contribute to the spontaneous functional recovery. In addition, agents and treatments that promote these neurorestorative processes have been demonstrated to improve functional recovery after brain injury. However, clinical trials in patients with TBI have primarily targeted neuroprotection, and trials directed specifically at neurorestoration have not been conducted. Therapeutic strategies for those who miss the early neuroprotection time window should be based on neurorestoration rather than acute neuroprotection. Cerebrolysin is a multifunctional neuropeptide mixture and may provide neuroprotection and neurorestoration after TBI. Therefore, it is important to conduct further investigation of the efficacy of delayed treatment and associated mechanisms underlying the functional recovery in clinically relevant animal models of TBI, which will facilitate translation into treatments for patients with TBI.

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References


Shaving

To The Editor: I would like to weigh in on the debate over the shaving of hair as continued by Dr. McKalip in the March 2013 issue (McKalip D: Shaving. J Neurosurg 118:701–702, March 2013 [Letter]). As an author of a study on this topic, I stopped shaving hair for most cranial procedures in 1991. Since then, I have performed over 1100 cranial procedures without hair removal, including mass resections, trauma surgeries, Chiari procedures, shunt placements, intracranial electrode placements, deep brain stimulator placements, and so forth. Over that time, I have experienced 2 cranial wound infections after elective surgery; one after a subdural grid was in place for almost 2 weeks and one after multiple shunt revisions in a patient with a pseudotumor. I am satisfied that this way of performing surgery is safe. But this is not the reason for writing.

Early in the course of my decision to stop shaving, in addition to obtaining separate consents for this part of the procedure, I requested that my patients and other surgeons’ patients whose hair had been shaved complete a questionnaire at their first postdischarge clinic visit. The questionnaire was mostly full of distracters about the nursing care, hospital food, pain control, and so forth. The key questions had to do with whether the patient’s appearance after surgery hindered their return to work or social activities. While some patients mentioned peri-orbital ecchymoses and facial swelling, the majority of shaved patients referred to loss of hair, the obviousness of their incisions, their embarrassment at having to explain...
having undergone brain surgery, fear of sunburn, and so forth. For those patients whose hair had not been shaved the response was either 1) nothing said about any social stigma or 2) specific comments on being happy that their hair had been preserved. Patients with cognitive or public occupations mentioned that they feared that their coworkers and customers would be skeptical of their abilities upon learning that they had undergone brain surgery. Many wrote unsolicited letters to me or to the hospital administration to comment positively about keeping their hair.

Thus, in my opinion, there is an additional dimension to this debate that has not been mentioned, that of the dignity and privacy of our patients. Shaving their heads diminishes self-esteem and advertises to others that they, the patients, have undergone brain surgery, something many patients would rather not discuss with others. One need only refer to Dr. Schwartz’s paper on his own surgical experience to validate this. After a craniotomy for recurrent chronic subdural hematoma he wrote, “I looked in the mirror and saw a person with half of a bald head covered with staples and the other half with a full head of hair. I nearly fell over. I did not know that I had been shaved. One of the worst parts of the craniotomy for me was being stripped of my hair. It depressed me horribly. It was debasing and dehumanizing to my spirit. It took almost four and a half months to get my hair back completely.” If there is no compelling reason to remove patients’ hair, then we should spare them the indignity of doing so.

In a similar vein, approximately 10 years ago, I began requiring the preoperative nurses to put paper pants on all my patients going to surgery. This also helps to preserve my patients’ dignity. The positive response to this change mirrors that described for the cessation of hair shaving. I suspect that when neurosurgeons go to surgery, they would prefer to keep their hair and their pants on. Let us offer our patients the same consideration.

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Disclosure

The author reports no conflict of interest.

References


RESPONSE: No response was received from the author of the original article.

Outcome prediction in traumatic brain injury

TO THE EDITOR: I read with interest the articles by Shi et al. 2,3 (Shi HY, Hwang SL, Lee KT, et al: Temporal trends and volume-outcome associations after traumatic brain injury: a 12-year study in Taiwan. Clinical article. J Neurosurg 118:732–738, April 2013; and Shi HY, Hwang SL, Lee KT, et al: In-hospital mortality after traumatic brain injury surgery: a nationwide population-based comparison of mortality predictors used in artificial neural network and logistic regression models. Clinical article. J Neurosurg 118:746–752, April 2013). In the first article these investigators studied the impact of factors like hospital volume and surgeon volume on length of stay, hospitalization cost, and in-hospital mortality. They concluded that annual surgical volume is the most important factor, which has a very crucial role in the formulation of national public health policies. In the second article, the authors compared artificial neural network models and logistic regression models for prediction of mortality in patients with traumatic brain injury. They concluded that artificial neural network models were more accurate in outcome prediction than logistic regression models. In connection with this, I would like to state that the Madras Head Injury Prognostic Scale (MHIPS) of Ramesh et al. 1 is also a simple and accurate prognostic model that is very useful for routine bedside outcome prediction, performed using readily available parameters. The MHIPS is regularly used by us, and the accuracy of prediction of outcome in traumatic brain injury is 87.5%.

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Disclosure

The author reports no conflict of interest.

References


RESPONSE: No response was received from the authors of the original article.
**Beta-amyloid oligomer**


Severe head injury often leads to long-term neurological impairment, but the outcome is not always easy to predict. Therefore, the investigation of novel predictors is very important for decision making in the treatment of head injury, especially for patients with poor Glasgow Coma Scale scores.

Gatson et al.1 used Western blotting to assess CSF levels of β-amyloid oligomers. These values were then compared with levels of other CSF proteins, such as neuron-specific enolase (NSE) and β-amyloid 42 (Aβ42), detected by enzyme-linked immunosorbent assay. The authors found an association between good outcomes and an elevation in CSF levels of Aβ42. In contrast, patients with worse outcomes displayed increased levels of CSF NSE and β-amyloid oligomers.

We have one concern regarding the 72-hour period. The paper does not seem to provide any rationale to support this time period for CSF collection, because the authors did not show the time course of detection of CSF β-amyloid oligomers in their Western blot results. Actually, neuronal death occurs within hours after severe head injury, and although the authors mention that amyloid plaques have been found in autopsy specimens as early as 2 hours after injury, we wonder whether findings in specimens from patients who have died of TBI would be different from findings in specimens obtained from living patients. Despite these minor limitations, the authors’ findings provided us with an important candidate marker for the prediction of clinical outcome in patients with severe head injury.

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Disclosure

The authors report no conflict of interest.

Reference


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**Angiogenesis**

To The Editor: We are interested in the study by Yener et al.3 (Yener U, Avsar T, Akgün E, et al: Assessment of antiangiogenic effect of imatinib mesylate on vestibular schwannoma tumors using in vivo corneal angiogenesis assay. Laboratory investigation. J Neurosurg 117:697–704, October 2012). We have two comments on their study. First, although angiogenesis is one of the main characteristics of glioblastoma multiforme (GBM), using GBM as a positive control of angiogenesis for comparison with vestibular schwannoma (VS) was not completely adequate. In fact, the secreted proteins contributing to angiogenesis may not necessarily be the same between VS and GBM. Therefore, the adequate positive control should be proangiogenic protein, such as recombinant protein including platelet-derived growth factor (transpose: PDGF)-A, PDGF-B, or in combination with PDGF-α and PDGF-β receptors. To test the function of PDGF in VS, they could use neutralizing antibody to block PDGF-A and PDGF-B to check if the phenotype of angiogenesis disappeared in a loss of function test. This experimental strategy would be more convincing to suppress the effect of PDGF on the angiogenesis of VS.

Second, according to Fig. 2 in their article, Yener et al. tried to analyze the contributed proangiogenic protein. Therefore, they performed Western blot analysis to identify the protein production of PDGF-A, PDGF-B, PDGF-α and PDGF-β. However, their data just showed an association between VS and GBM or normal brain tissue. They should directly investigate the effect of PDGF-A, PDGF-B, or angiogenesis through knockdown1,2 PDGF-A and PDGF-B to delineate whether the function of PDGF determined the angiogenesis or not. Moreover, they should show the corresponding internal control such as β-actin or α-tubulin.

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Disclosure

The authors report no conflict of interest.

References

**Response:** We are very grateful for the valuable comments of Huang et al. Using GBM as a positive control of angiogenesis is a proven and admitted method for comparing the angiogenesis level. Huang et al. are right in their concerns, but we are aware that using recombinant PDGFs and/or their receptors as positive controls would be more accurate. But the results in our study are the first-line results to prove the level of angiogenesis as well as inhibition via the antiangiogenic effect of imatinib mesylate. For further studies, both recombinant proteins will be used, and the effect of neutralizing antibodies will be evaluated and shown.

Secondly, the knockdown model with PDGFs and their receptors is a great suggestion. But currently we have no such facility, neither for the production nor the maintenance of knockdown animals. Additionally, as regards Fig. 2, we have shown the representative Western blot bands of corresponding proteins. As predicted, we did not totally convince the readers that their method is as good as traditional MER-guided DBS electrode placement. Therefore, future prospective outcome evaluations of image-based DBS electrode placement in patients with medically intractable movement disorders is warranted.

**References**


**Disclosure**

The authors report no conflict of interest.

**Response:** We thank Drs. Yan, Tsai, and Huang for their letter to the editor and interest in our manuscript. As stated in the body of the manuscript, our objective was to evaluate the accuracy of DBS placement using 3-T MR images merged with intraoperative CT images. This manuscript does not constitute an outcome study evaluating the efficacy of this new movement disorder surgery method compared to frame-based MER. Hence, a follow-up study investigating long-term outcomes using this new method is currently underway at Oregon Health & Science University, which should address the concerns expressed by Yan et al. We believe this manuscript provides evidence of a first step in validating the method as an alternative option to current frame-based MER-guided DBS electrode placement. Given advances in neuroimaging and the large body of data accrued from years of knowledge regarding the ideal targets within the deep brain nuclei for specific movement disorder symptoms—ironically derived from MER data—direct targeting with a higher degree of accuracy and precision is viable. We believe that MER might continue to have research or even clinical benefit when newer targets are explored, which would require acquisition of knowledge and experience regarding ideal targeting. Once this new knowledge has been established and tested by rigorous research and increasing experience, direct targeting without the added risk of MER is a very reasonable option.

**References**


**Disclosure**

The authors report no conflict of interest.

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**References**


**Disclosure**

The authors report no conflict of interest.
Diffuse low-grade gliomas

TO THE EDITOR: We would like to congratulate Capelle and coworkers for their important contribution to the literature on low-grade gliomas1 (Capelle L, Fontaine D, Mandonnet E, et al: Spontaneous and therapeutic prognostic factors in adult hemispheric World Health Organization Grade II gliomas: a series of 1097 cases. Clinical article. J Neurosurg 118:1157–1168, June 2013). A noteworthy achievement in their work is the collaboration between academic institutions, a relative rarity when it comes to clinical research in neurosurgery. We hope more colleagues will follow in their path. Based on the findings from our own research we firmly agree that early resection should be the primary strategy.3

In their study they present results from several regions in patients with hemispheric diffuse WHO Grade II gliomas. Patients are selected based on certain inclusion and exclusion criteria. Because of certain similarities to our own findings in a population-based study, we wonder to what extent their patient selection is population based, and whether they think potential selection bias might have influenced the results.

We also found it interesting that the prognostic score presented by Chang and coworkers from the University of California, San Francisco (UCSF) seems to have better discriminating properties than the Pignatti score.1,2,4 We adjusted for the Pignatti score in our own study, so we were curious to see if the findings of improved survival at a center advocating early and aggressive surgery were consistent when using the scoring system introduced by the UCSF group. Based on the available data from our previous study, we find that treatment policy is still a significant risk factor (p = 0.015, HR 1.9) when adjusting for UCSF score.3 In the same Cox multivariable model the score was tested in a population-based setting with heterogeneous surgical management, and the discriminating properties were still very good (p < 0.001, HR 2.0, Fig. 1). The distribution of the score in a population-based setting with solely histopathological inclusion differs markedly from the French study (Table 1).

In conclusion, we agree that surgical management should be the primary treatment in diffuse WHO Grade II gliomas, and surgery probably will have an important role at the time of recurrence/progression. The UCSF score seems promising for grouping patients with respect to survival, and it should be used to adjust for the case mix for meaningful comparative studies.

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Disclosure

The authors report no conflict of interest.

References


TABLE 1: The UCSF score for patients with WHO Grade II gliomas in a population-based Norwegian study compared with the French study*

<table>
<thead>
<tr>
<th>UCSF Score</th>
<th>Norwegian Study</th>
<th>French Study</th>
</tr>
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<tbody>
<tr>
<td>0</td>
<td>10%</td>
<td>50%</td>
</tr>
<tr>
<td>1</td>
<td>43%</td>
<td>34%</td>
</tr>
<tr>
<td>2</td>
<td>25%</td>
<td>16%</td>
</tr>
<tr>
<td>3</td>
<td>14%</td>
<td>0%</td>
</tr>
<tr>
<td>4</td>
<td>8%</td>
<td>0%</td>
</tr>
</tbody>
</table>

* Norwegian study by Jakola et al.; French study by Capelle et al.
Occipital lobe

To The Editor: We took great interest in the article by Alves et al. (Alves RV, Ribas GC, Párraga RG, et al: The occipital lobe convexity sulci and gyri. Laboratory investigation. J Neurosurg 116:1014–1023, May 2012), in which the authors reported the findings from their study of sulci and gyri of the occipital lobe convexity in human brains.

This brain sector is also part of our research study in neuroanatomy. Based on our experience and on the literature, we object to some sections of this work. We hope the following details serve to enhance this interesting work. Additionally, we hope to contribute to this work soon with a similar research study from Colombia (South America) trying to establish a statistically significant pattern of occipital gyri and sulci and to suggest to the Federative International Committee on Anatomical Terminology to adopt a terminology for these brain surfaces that contributes to the morphological field and at the same time allows its application in the field of imaging and neurosurgery.

We have the following objections to the authors’ paper:

1. The authors indicate that there is no description in the literature of the gyri in the occipital convexity. However, Jiménez-Castellanos described the superior gyrus, the middle gyrus, and inferior occipital gyrus; 6 Nehmad and Clement described a pattern of 2 gyri; 7 and Cornide 8 and Testut and Latarjet 9 described a pattern of 3 horizontal superimposed gyri, called the first, second, and third gyri, that are continuous with the parietal and temporal third gyri. Finally Rouvière described a pattern of 4 gyri in the lateral occipital lobe. 10

2. The article assumes that the terms are not included in the International Anatomical Terminology, such as “anterior occipital sulcus” and “intraoccipital sulcus.”

3. In Methods the authors indicate that the removal of arachnoid membranes and superficial vessels is performed using microsurgery; however, the meninges and a large part of the pia mater and vessels can be removed macroscopically. In addition, we note that some sections of the brain presented in the paper show tears in plain view, which demonstrates that the removal of meninges and vessels was done through a gross surgery.

4. The work mentioned that an extensive literature review was made on the subject, but actually only 14 references were consulted.

5. We believe that the study sample is small in regard to the number of cerebral hemispheres (20 hemispheres) and has therefore no statistical significance. However, the work is worthy of being noted for its morphological importance and its application in the clinical area, which has neither been noticed nor been described for years.

6. We propose to make joint efforts in different countries for descriptive studies of the gyri and sulci of the lateral surface of the occipital lobe so that the sum of the samples is statistically significant in order to validate the findings and to propose inclusion in the International Anatomical Terminology.

7. In Figs. 1A and B, 3B, 5A and B, and 7B in the article, we noted a methodological error in defining the occipital lobe. The blue thread used for demarcation was placed ahead of the preoccipital notch so that the angular gyrus was included in this lobe. However, this gyrus belongs to the parietal lobe.

8. The authors use “anastomosis” as an inadequate term for the extension of a gyri or fold of cortex. Actually, the term refers to the communication between 2 vessels or nerves, 11, 12 2 tubular bodies, or a surgical or pathological formation between 2 structures. 13

9. The superior, middle, and inferior gyri of the occipital lobe are not included in the International Anatomical Terminology. Chusid 14 indicated that the occipital lateral sulcus, which extends transversely along the lateral surface, divides the occipital lobe into the superior and inferior gyri, and often they are simply called “lateral occipital gyri.” 15

10. Based on figures the authors state, “when the lunate sulcus is present in humans it does not delineate a functional region.” However, Allen and colleagues 16 state, “...the lunate sulcus in humans is not typically structurally associated with the primary visual cortex.” However, this region has a visual function. Thus, whether the lunate sulcus exists or not, this lateral occipital cortical region is functional with respect to vision. 17

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Disclosure

The authors report no conflict of interest.

References

Regarding the noted tears on the specimens, they oc-
curred, unfortunately, previous to our study. As described
in the publication, the specimens were stored at our insti-
tution for at least 15 years before dissection. The noted
tears were mostly due to the original removal of the en-
cephala from the cadaveric head and from previous ma-
nipulations during this long period.

4) Concerning our literature review, we would like to
clarify that we did indeed consult other sources, but
according to the Journal of Neurorsurgery Publishing
Group’s instructions for authors, we mentioned within the
article only the references that were cited in the text. We
believe that we have listed the main references relative to
this topic.

5) The present study was done with 20 cerebral hemi-
spheres, but we do agree that the study of more specimens
would enrich our research. Nevertheless, we would like to
point out that the majority of articles about neurosurgical
anatomy are based on fewer than 20 specimens, and they
are usually descriptive. The main aim of our study was to
investigate and describe the anatomy of the occipital lobe
convexity and clarify its nomenclature. We believe that
the number of specimens was sufficient for these objec-
tives.

6) We agree and are available to discuss this interesting
topic. We believe that direct contact by email would be
the best channel to exchange observations and informa-
tion.

7) The occipital convexity is not separated from the
temporal and parietal lobes by any clearly defined sulcus,
and the occipital lobe is arbitrarily defined as the lobe
that lies behind the imaginary line joining the preoccipit-
tal notch and the parietooccipital sulcus on the supero-
lateral surface. We connected these two landmarks with
a small blue thread to demarcate the occipital lobe and
to facilitate the identification of the occipital sulci and
gyri. The preoccipital notch was identified as the dural
fold that causes an indentation in the inferolateral border
of the cerebral hemisphere about 5 cm in front of the oc-
cipital pole.3

We believe that due to the magnification provided
by the camera and to the angle of the photograph (high-
lighting the occipital lobe), some figures and illustrations
may give the impression that the preoccipital notch was
marked incorrectly.

8) Although colloquially the term “anastomosis” is
used to describe the connection between two anatomical
structures, we agree that there is a semantic mistake. The
correct use of the term has been explained above in Dr.
Duque Parra and colleagues’ letter. We congratulate our
colleagues for the careful attention to this detail.

9) Again, we emphasize that our study does not state
that any occipital sulcus or gyrus is, or is not, described in
International Anatomical Terminology. We only em-
phasize that the occipital sulci and gyri have been given
different names according to different authors in the
medical literature, and hence that there is not a consensus
about their nomenclature.

10) Our study dealt with morphological findings, and
it was not our aim to relate them to any brain function
delineation. The manuscript does not indicate, based on
anatomical dissections, the presence or absence of any
brain function. We believe that it is clear in the Discus-

RESPONSE: We thank Dr. Duque Parra and colleagues
for their interest in our recent publication in which we
attempted to describe the anatomy of the occipital lobe
convexity and clarify its nomenclature.

First, we would like to emphasize that further studies
regarding the anatomy of the occipital lobe are welcome.
Only by re-studying this complex part of the cerebral con-
vexity we will be able to better understand its anatomy,
better interpret the information provided by imaging, and
perform more “anatomical” surgeries. We look forward to
the publication by Duque Parra et al. in which they will
attempt to establish a statistically significant pattern of
occipital sulci and gyri.

In their comments to our article, Duque Parra and
colleagues raised interesting questions, which we answer
point by point below.

1) Our manuscript does not state that there is no de-
scription in the literature of the gyri in the occipital con-
vexity. Rather, our study covers how the convexity of the
occipital lobe has been described by several authors and
emphasizes that the sulci have received different names
according to different authors. The classic anatomy text-
books do not describe the variations and the different pat-
terns in detail, and there is no consensus in the descrip-
tion of this part of the brain surface.

2) The publication does not state at all that an occipi-
tal sulcus or gyrus is, or is not, mentioned in the Inter-
national Anatomical Terminology. However, we would
like to take this opportunity to highlight that the Inter-
national Anatomical Terminology recognizes only the
following landmarks on the occipital convexity: occipital
pole, lunate sulcus, preoccipital incisure, and transverse
occipital sulcus.

3) We thank Dr. Duque Parra and colleagues for this
important observation. It is true that the arachnoid mem-
branes and superficial vessels of the cerebral hemispheres
may be removed both macroscopically, as seems to be the
authors’ preferred technique, or with the aid of a surgical
microscope, as we did. However, we strongly believe that
dissection with the aid of a surgical microscope allows a
more delicate and gentle dissection of the cerebral cortex.

Regarding the noted tears on the specimens, they oc-

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sion that, according to the literature, the structural and functional reorganization of the occipital lobe during brain evolution has been extensive and complex. This reorganization led to the loss of a "true" lunate sulcus in humans, and, when now present, this sulcus is found in a more posterior position to that observed in the great apes, with no correlation between the lunate sulcus and primary visual cortex.

Finally, we reaffirm our appreciation for Dr. Duque Parra and colleagues’ interest in our study. We encourage them to continue their study and soon present us with another publication concerning this interesting subject.

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Indocyanine green videography and meningioma

To The Editor: We read with great interest the article by Ueba et al. (Ueba T, Okawa M, Abe H, et al: Identification of venous sinus, tumor location, and pial supply during meningioma surgery by transdural indocyanine green videography. Clinical article. J Neurosurg 118:632–636, March 2013). We congratulate the authors on their clinical work and contribution.

We would like to draw the authors’ attention to our previous work on this subject, in which we highlighted the usefulness of indocyanine green (ICG) videoangiography to optimize the dural opening for parasagittal lesions, including meningiomas, by delineating the venous sinuses and cortical draining veins before dural opening. Ueba and colleagues’ article described the primary aim of this technique well, which is to accurately allow the surgeon to strategically plan and protect important parasagittal dural venous drainage during craniotomy. In addition, their group pointed out the importance of control visualization of veins and the dural sinuses as well as assessment of total tumor volume, intratumoral circulation time, and lesion tendency to bleed. In their experience, as in ours, ICG videoangiography proved to be successful in identifying tumor boundaries, transdural location of the cortical vessels, and venous sinuses.

We commend the authors for their excellent account of the additional value of ICG videography.

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Disclosure

The authors report no conflict of interest.

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Response: We appreciate the interest and comments of Drs. Defilho and Nussbaum. Transdural observation of the cortical arteries and veins and the venous sinus was successfully performed, as was visualization of the projection of meningiomas as a shadow. We proposed the naming of this sign as the “eclipse sign.” Transdural ICG videography could delineate tumor location and venous sinus, which enabled precise opening of the dura mater and optimization of the surgical approach. Real-time visualization of veins and the dural sinuses, as well as assessment of total tumor volume, intratumoral circulation time, and tendency of a lesion to bleed, is highly relevant for brain tumor surgery.

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