Neurosurgical forum
Letters to the editor

Chiropractor and dissections

To The Editor: We read with interest the article by Albuquerque and colleagues (Albuquerque FC, Hu YC, Dashti SR, et al: Craniovertebral arterial dissections as sequelae of chiropractic manipulation: patterns of injury and management. Clinical article. J Neurosurg 115:1197–1205, December 2011), in which they describe 13 cases of cervical artery dissections reported to have occurred following a chiropractic manipulation. Throughout their paper, the authors assume that a causal link exists between chiropractic manipulation and cervical artery stroke. However, this assumption is flawed. To support their assumption of causality, the authors cite case series and 2 case-control studies. Case series cannot be used to make causal inferences because they are fraught with selection and information bias. In addition, case series do not include a control group, which is necessary to quantify the association between an exposure and an outcome.3

Surprisingly, Albuquerque et al. did not review the most recent epidemiological evidence on this controversial issue.1–3 Rather, they support their position by using 2 case-control studies. The first study has known methodological flaws,4,6 and the association described in the second one was later shown to be noncausal.2,5 The most recent epidemiological evidence on the relationship between chiropractic care and vertebrobasilar artery (VBA) stroke suggests that the association is noncausal.2 The case-crossover study by Cassidy et al. found that the risk of VBA stroke is similar for patients who consult a family physician or a chiropractor prior to their stroke. This evidence suggests that patients seek health care (chiropractic or medical care) for early symptoms of an evolving stroke (neck pain and headache), and that chiropractic care may be associated with, but is not a risk factor for the stroke.

Simply observing that an exposure precedes an outcome does not prove causation. Although temporality is necessary to establish causality, it is not sufficient. Well-designed epidemiological studies that include a valid control group are necessary to understand complex causal problems. Although case series are helpful to develop etiological hypotheses, they cannot be used to study causality. Case series serve no purpose once an exposure–outcome relationship has been investigated in sound epidemiological studies. We question the motivation to keep publishing hypothesis-generating case series that do not advance knowledge in this area.

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To The Editor: I read with interest the recent article by Albuquerque and coauthors (Albuquerque FC, Hu YC, Dashti SR, et al: Craniovertebral arterial dissections as sequelae of chiropractic manipulation: patterns of injury and management. Clinical article. J Neurosurg 115:1197–1205, December 2011). Their article contains very strong language implicating manipulation as a cause of stroke. However, this paper is essentially an observational case series reporting the clinical vignettes of 13 patients who the authors claim developed craniovertebral arterial dissections as the sequelae of chiropractic manipulation. It is a fundamental principle of epidemiology that observational studies can only determine whether an association exists between an exposure and a disease.

Observational case reports simply cannot be used as evidence to prove causal relationships.5 Nonetheless, the authors begin their article with a definitive statement that chiropractic manipulation of the cervical spine can cause stroke. The 5 references cited to substantiate this statement are all case reports, 1 of which used only survey data from physicians in a specific region in France.2 None of these references provide any convincing evidence that manipulation directly causes stroke.

The clinical information from the 13 cases presented

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in this article was obtained retrospectively from medical records and history as reported by the patient or a family member, with the a priori assumption that there was an association between dissection and chiropractic manipulation. This type of anecdotal information is especially prone to recall bias; the presumed accuracy is based solely on the recollection of the patient or family member. Another assumption by the authors is that all 13 patients had high-velocity, low-amplitude manipulation performed on their cervical spines, but this assumption is unsupported by evidence. Leach published a retrospective case series of 4 chiropractic patients who presented to his office with stroke symptoms—who did not receive manipulation, but were referred for medical evaluation—and who subsequently had strokes. Would Albuquerque and his coauthors have erroneously assumed that these 4 chiropractic patients had received cervical manipulation?

There is a critical epidemiological question that cannot be answered by an observational case series: “Is there an increased risk of stroke associated with visits to a chiropractor?” Cassidy et al. published a large population-based case-control study of all cases of VBA strokes in the province of Ontario over a 10-year period. The paper concluded that there was no increased risk of VBA stroke associated with chiropractic care compared with primary care. It was suggested that the most likely explanation for the perception of increased risk was that some patients with VBA dissections in progress seek chiropractic care before their stroke actually occurs. It is disturbing that Albuquerque et al. would completely ignore any discussion or reference to this landmark study by Cassidy et al., considering that it is arguably the best epidemiological evidence published on this topic to date.

Readers of the Journal of Neurosurgery should be cautioned about the fallacy of making causal inferences from an observational case series. A comprehensive review of the key issues regarding cervical manipulation and stroke can be found in a recent paper by Murphy, which contains a more current list of references on this topic.

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Disclosure

The author reports no conflict of interest.

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RESPONSE: Although we appreciate that the authors of these letters read our manuscript, we disagree with their assertions. Côté and Cassidy argue that we have mistakenly labeled chiropractic cervical manipulation as the cause of the neurological sequelae suffered by our patients. They cite their own manuscripts as the “most recent epidemiological evidence” showing the lack of causality between chiropractic treatment and stroke. In their first study, they analyzed 2 trends: 1) the incidence of VBA stroke in 2 Canadian provinces over a 10-year period; and 2) the utilization rate of chiropractic care during that same period. Because the rate of VBA strokes and chiropractic care did not increase symmetrically in these populations, they concluded that “the increase in the incidence rate of VBA stroke could not be explained by a proportional increase in exposure to chiropractors.” Although this statement may be true, it has nothing to do with whether chiropractic manipulation of the spine can indeed cause arterial dissections. In fact, the majority of our patients presented within hours of chiropractic manipulation, and numerous similar cases have been reported.

The authors argue that this phenomenon is actually the result of chiropractors having the unfortunate luck to encounter patients who are suffering a VBA stroke “in evolution.” Their assertion that headache and neck pain are signs of a preexisting arterial dissection is both inaccurate and worrisome. If patients manifest the symptoms of a dissection, why would a chiropractor proceed with cervical manipulation? Would they truly think that cervical manipulation cannot worsen a dissection and produce a stroke? That the majority of patients present either immediately or within hours of chiropractic manipulation with neurological sequelae or stroke argues against such a notion. Furthermore, such denial is deeply problematic if not dangerous for patients. If chiropractic manipulation cannot worsen or produce a dissection, it is an amazing coincidence that many chiropractors have been at “ground zero” during the onset of a stroke.

Several times in our manuscript we stated that the incidence of manipulation-induced dissection was low and almost impossible to ascertain. Nonetheless, it does occur, and the secondary neurological injuries can be severe and occasionally fatal. Hence, the low incidence of these dissections would be small consolation to affected patients.

The authors of both letters also referred to another publication, again written by Cassidy and Côté, who found that patients with VBA stroke were just as likely to have seen a primary care physician as they were to have seen a chiropractor in the days preceding their stroke. Again, this statement may be true. However, primary care physicians rarely perform cervical manipulations. Patients who eventually die of VBA strokes, whether related
to a dissection or atherosclerosis, are an inherently ill subpopulation that is likely to seek frequent medical care or consultation. Furthermore, clear anatomical evidence, as referenced in our article, delineates that extension and contralateral rotation of the head compromise vertebral arterial flow. Again, the study that the authors reference fails to consider whether chiropractic manipulation can produce arterial dissections and stroke; its actual incidence is irrelevant to that point.

Finally, Côté and Cassidy question our “motivation to keep publishing hypothesis-generating case series that do not advance knowledge in this area.” In our report, we describe the endovascular and surgical techniques that are used to manage what can be complex arterial injuries. We had no motivation, financial or otherwise, in reviewing these cases. Interestingly, both Côté and Cassidy received funding for their work from the Canadian Chiropractic Protective Association, the malpractice insurance carrier representing the majority of Canada’s chiropractors.

The crux of this issue is not how frequently dissections occur after chiropractic manipulation but that they can indeed occur. In neurosurgery, failure to identify and learn from one’s complications produces poor outcomes and even death. The same surely applies to alternative medical fields, including chiropractic care. Our report underscores a thankfully rare but probably underreported clinical entity that can lead to significant disability or death.

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Tic and autonomic symptoms

TO THE EDITOR: We read with great interest the recent paper by Simms and Honey (Simms HN, Honey CR: The importance of autonomic symptoms in trigeminal neuralgia. Clinical article. J Neurosurg 115:210–216, August 2011). The authors reviewed 92 cases of trigeminal neuralgia (TN) and revealed that 67% had at least 1 preoperative autonomic symptom and 14% had 4 or more symptoms associated with their TN attacks. Such preoperative symptoms are related to a significantly worse outcome for TN symptom relief after surgery in these patients. The authors concluded that the presence of autonomic symptoms in TN is a more severe form of TN; therefore, these patients have a poorer response to surgery.

The mechanism of autonomic symptoms is related to the activation of what Simms and Honey call the “trigeminal autonomic reflex,” initiated by the neurovascular conflict at the nerve root entry zone, triggering the functional brainstem connection between the trigeminal nucleus and superior salivatory nucleus, which leads to activation of parasympathetic efferents. Intraoperatively, such brainstem connections are attributed to the trigemino-cardiac reflex (TCR), first introduced by Schaller in 1999, which uses the similar autonomic reflex arc, but with different efferents.

We retrospectively reviewed the cases of TN surgically treated between January 2007 and August 2011 in the Department of Neurosurgery, Tokuda Hospital, Sofia, Bulgaria (Table 1), and present the findings here together with our previously published results. All in all, 97% of the patients underwent microvascular decompression and a 3% alcohol injection.

The TCR is, for example, a relatively common event during Janetta operations. Different retrospective studies have shown a TCR incidence ranging from 8% to 18% using all of the same inclusion criteria as defined by us earlier. The significance of the intraoperative occurrence of TCR lies in the fact that the appearance of such a reflex has already been shown to be a negative prognostic factor and associated with a significantly worse postoperative outcome in various microsurgical skull base operations. In our present study we demonstrate a similar line of results in the Janetta operation: persistent pain at the 3-month follow-up in 2 (40%) of 5 patients with preoperative autonomic symptoms and intraoperative TCR occurrence. Therefore it seems that preoperative autonomic symptoms might be a negative prognostic factor for an intraoperative TCR event.

To date, several preoperative risk factors for the in-

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<td>2 (40)</td>
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<tr>
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<td>preop autonomic symptoms</td>
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* Values represent the number of patients.
† Definition of TCR according to Schaller et al.³,⁴
‡ At 3 months after surgery.
traoperative occurrence of TCR have been identified, but their relevance toward a personalized treatment plan remains a matter of debate. Whether the results of Simms and Honey are in line with these findings or whether they represent a completely different entity must be clarified by additional larger and more detailed studies.

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Disclosure
The authors report no conflict of interest.

References

RESPONSE: We thank Drs. Spiriev, Sandu, Kondoff, Tzekov, and Schaller for their interest in our paper and their thoughtful comments and important observations. There are some differences (and potential similarities) between our report of preoperative autonomic symptoms in TN and their reports of intraoperative TCR. First, our data were obtained preoperatively and could therefore provide a surgeon with information that could be used to counsel a patient to adjust their surgical expectations. Second, autonomic symptoms accompanying TN are clearly a part of the process of this disease, whereas the TCR may be an epiphenomenon of iatrogenic manipulation of the nerve. We stress “may” because patients with a bout of TN do not comment on their heart rate (unlike glossopharyngeal neuralgia). Our unpublished experience with over a thousand percutaneous radiofrequency rhizotomies is that the TCR is common during puncture of the foramen ovale, whereas is it rare during microvascular decompression. This may speak to the etiology of this reflex but not its predictive val-ue. Third, although the numbers are quite small and some of the follow-up is quite short (3 months), the presence of an intraoperative TCR appeared to be a more ominous factor than preoperative autonomic symptoms. Dr. Spiriev and colleagues reported that 96% of the patients were pain free after treatment if they did not have an intraoperative TCR as compared with 66% if they did, whereas 100% of the patients were pain free after treatment if they had not had a preoperative autonomic symptom as compared with 86% if they had.

Dr. Spiriev and colleagues’ letter brings to attention the autonomic features associated with TN and calls for additional research to tease out the underpinnings of this phenomenon. We echo their request. It is interesting to speculate whether the TCR might be the only aspect of an autonomic response that can be detected intraoperatively (since the facial swelling, tearing, flushing, and salivation are obscured from vision) or whether it represents a separate entity. As other centers look into this phenomenon, we ask that the work be done prospectively and with clear end points (for example, “pain free” means no TN while off medications at 1 year).

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Tuberculum sellae

TO THE EDITOR: I read with great interest the article by de Notaris and coauthors (de Notaris M, Solari D, Cavallo LM, et al: The “suprasellar notch,” or the tuberculum sellae as seen from below: definition, features, and clinical implications from an endoscopic endonasal perspective. Laboratory investigation. J Neurosurg 116:622–629, March 2012) on the tuberculum sellae’s anatomical appearance as seen during transsphenoidal approaches to suprasellar areas. Given that we have performed a preliminary study on the same subject (Kunicki J, Bakoč L, Cisek B, Bonicki W, presented at the 5th International Symposium on Microneurosurgical Anatomy in Istanbul, Turkey, on November 10, 2010), we sincerely appreciate the efforts of these authors.

They performed a CT scanning–based analysis of the tuberculum sellae (TS) area in relation to the endoscopic transsphenoidal appearance of the corresponding sphenoid sinus recess. As a result of their observations, they presented the concept of the “suprasellar notch” (SSN) by renaming and dividing the sphenoid sinus indentation corresponding to the TS and the chiasmatic sulcus.

However, anatomical tradition and nomenclature reserve the term “notch” for indentations or cuts in the margins of structures (for example, jugular notch, suprascapular notch, sciotic notch, mandibular notch, and genoid notch). Moreover, the term “notch” could, to some degree, be confused with the tentorial notch, which is closely related to the same area.

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Recently, Wang et al.\textsuperscript{4} presented a modern division of
the sphenoid sinus recesses and introduced the term
“tuberculum recess” for the indentation corresponding to
the TS complex. The TS and chiasmatic sulcus are char-
acterized by anatomical variability, which was recently
well described by Guthikonda et al.\textsuperscript{2} on a significant
number of skull specimens. In a recent study on the neuro-
surgical anatomy of the sphenoid sinus, Zada et al.\textsuperscript{5} de-
described and measured the tuberculum angle, sellar angle,
and sellar prominence, characterizing the sella and area
of the tuberculum recess. Our observations showed that
the tuberculum recess indentation is variable and a rather
ill-defined area. Its endoscopic appearance depends on a
number of factors, with the most important being the de-
gree of sphenoid sinus pneumatization, bone thickness,
dimensions, morphology, and interrelations between the
sellar floor and the sphenoid roof.

As the authors concentrated on the variability of the
intracranial surface of the TS–chiasmatic sulcus area
without taking into account other factors influencing the
differences in the endoscopic appearance of the region,
their results and conclusions seem to be misinterpreted.

However, their article title refers to the definition and
features of the TS as seen from below, and they stated that
they performed an anatomical comparison between the
endoscopic view and the radiological data, although they
did not show results for this part of the study.

The authors did not focus on the intrasphenoidal
morphology and the variation in the tuberculum recess,
as they have not proposed morphometric or even a subjec-
tive distinction in the endoscopic views of the tubercular
recess, which might have been correlated with different
types of TS.

The pictures of reformatted reconstructions and the
virtual endoscopic views presented as illustrative ex-
amples of different types of “sellar notch” do not seem
convincing.

Similarly, the endoscopic pictures of the sphenoid si-
inus showing different types of tuberculum recess, in our
opinion, also fail to prove the usefulness of the performed
division. However, the tuberculum recesses look quite
different there, and these variances are caused by a dif-
ferent pneumatization and bone thickness rather than the
angle of the TS. For example, the Type III recess seems
the deepest and the most distinct, which is in contrast to
their conclusions.

Our own anatomical and surgical observations
showed that tuberculum recess variability is essentially
determined by differences in the tuberculum angle as de-
defined by Zada et al.\textsuperscript{5} and by the degree of local pneu-
matization of the sellar-planum junction. In our study we
also encountered problems with identification of the medial
optocarotid recess (OCR), which is said to define the
lateral extremes of the tubercular recess, whose recogni-
tion is rather intuitive, and which is defined in relation to
the lateral OCR. Unfortunately, the authors do not refer
to the variability and morphological appearance of the
medial OCR, even though they commented on the recent
studies by Ozcan et al.\textsuperscript{3} on the same subject.\textsuperscript{1} However,
the authors performed a statistical analysis to show and
confirm their observations on morphometric correlations
between linear measurements of the chiasmatic sulcus and
its slope angle, and it seems that the number of ob-
servations as a whole and especially within the groups
counting 3 and 4 cases each (Type I and III) do not quali-
fy for significant conclusions, despite some statistical
significance. It was shown by Guthikonda et al.\textsuperscript{2} that there
are significant groups of both steep and plain chiasmatic
sulci with either wide or narrow interoptic distances,
which is in contrast to the authors’ conclusions.

The authors state that the typology of the TS pre-
sented in their study influences the reconstruction phase
of the osteodural defect. As the bone of the TS and chi-
asmatic sulcus with some part of the planum are removed
on the way to the suprasellar area, on completing the
transptaricular transplanar approach, the surgeon faces
the osteodural defect limited by the different lengths of
the bone margins of the sellar floor and planum sphenoi-
dale. So the ease of reconstruction is rather dependent on
the distance and angle between the rest of the sellar floor
and planum margin and the angular characteristics of the
TS, and the proposed division is not so important in these
phases. Similarly, the range of view and accessibility are
determined not only by the intracranial TS angle but also
by the degree of planum resection, the distance and an-
gulation of the optic chiasm, and endoscope advancement
and angulation within the sinus.

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Disclosure

The author reports no conflict of interest.

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Response: In the renowned quote from Essays,
“Reading maketh a full man; conference a ready man;
and writing an exact man,” Sir Francis Bacon stresses
the importance of investigation in life. He supports the
idea that abstraction is an immensely important aspect
of life. He believes that expert men can execute and judge
in a very orderly fashion, bearing in mind the details.
He also says that reading is important but only to weigh and contemplate issues. Moreover, he states that while reading makes a full man and conference makes a ready man, only writing makes an exact man. Our impression after reading the letter from Dr. Kunicki concerning the "tuberculum sellae complex" is that the writing process failed.

We spent 3 years trying to explain the simple idea of an angle that we have dealt with hundreds of times during surgical procedures and/or laboratory dissections. When our group discovered the 2 lines that describe the suprasellar procedures and/or laboratory dissections. When our group discovered the 2 lines that describe the suprasellar angle (SNA)—also thanks to the great efforts made by Dr. Cavallo—we tried to write about the concept in the easiest way possible and, finally, the idea was there on the paper. This is to say that we absolutely believe in the exactitude and accuracy of the topic discussed in our article.

Nevertheless, we want to express sincere appreciation to Dr. Kunicki for giving us the opportunity to clarify and specifically explain the concept of the SNA, which has never been described by the same 2 lines defined in our work. Hence, our aim here is not to discuss the details of our work—which has been precisely examined by the reviewers of one of the most important journals in the field of neurosurgery and neuroscience—but instead to clarify some important concepts.

First of all, we would like to remark that the SNA has nothing to do with the prechiasmal sulcus. In his letter, Dr. Kunicki stated that "as a result of [our] observations, [we] presented the concept of the “suprasellar notch” by renaming and dividing the sphenoid sinus indentation corresponding to the TS and the chiasmatic sulcus.”

In our work we have described the ventral (exocranial) aspect of the TS, not of the chiasmatic sulcus; these are not the same structure. As a matter of fact, we completely disagree with the concept of a “tuberculum sellae complex” that Dr. Kunicki introduced in his letter, which sounds like a contradictio in terminis (the tuberculum is, according to the TS–chiasmatic sulcus area from an intracranial perspective; in -

Concerning the choice of the most appropriate name, we have proposed renaming this angle the "suprasellar notch," as it means an indentation, a synonym for angle, and because it lies at the anterior margin (of course, not in the center) of the body of the sphenoid bone. We also believe that the SNA should not be confused with the torcular notch, as it is sufficiently clear, in our opinion, that a bony structure that lies in the exocranial surface of the skull is different from an endocranial meningeal one.

In regard to observations about the current pertinent literature, we have read with great interest the remarks of several authors.1,3,4,7,10

The valuable article by Zada and colleagues14 aimed to assess the anatomical variations of the sellar and parasellar areas based on MRI studies, thus introducing the concept of “sellar and tuberculum angles.” While the sellar angle is precisely defined as “the angle between lines drawn tangential to the sellar floor at tuberculum sellae and sellar-clival points,” which is completely different from the SNA: the lack of a precise explanation of the tuberculum angle limits its understanding. Anyway, the TS angle and the SNA seem to be very different for many reasons. Above all, from a methodological perspective, the SNA is defined on a CT scan using multiple bony landmarks, while the tuberculum angle is constructed on MR images. Moreover, the value of angles is completely different. Indeed, the median value of the SNA is 127°, while the tuberculum angle is 112°.

Concerning the recent article by Guthikonda et al.,5 we have already underlined in our work the importance of a surgically oriented definition of the different anatomical types of chiasmatic sulcus for transcranial approaches. As a matter of fact, we have complemented this concept with the classification of the SNA to characterize from an endonasal perspective the recognition of variants and the frequency of their expression and thus establishing the advantages and limitations of each one of them.

Another important concept concerning our work is the quantitative perspective. In his letter, Dr. Kunicki observed that as we “concentrated on the variability of the intracranial surface of the TS–chiasmatic sulcus area without taking into account other factors influencing the different variables, quantitative analysis tries to mathematically replicate reality. We could have spent 10 years describing the variants of intrasphenoidal morphology in relation to different types of SNA, but this was not the point; this anatomy has already been magnificently described by many authors.1,3,4,7,10 Indeed, once we define the SNA by means of 2 lines on a sagittal CT scan, the variations of intrasphenoidal morphology do not influence the results.
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... at all. What we are trying to explain is that even in the case of a conchal type of sphenoid sinus, the SNA can be measured and taken into account when dealing with suprasellar lesions. What might appear obvious is that its appearance is influenced by the pneumatization of the sphenoid sinus; this happens for all the main anatomical structures over the sphenoid sinus posterior wall (that is, the sellar floor, the clival indentation, or even the OCRs). For these reasons we have tried to show the appearance of our determinations on cadaveric specimens by using the endoscopic endonasal navigation-assisted approach to reveal the exact position of the angle, even in the presence of less-pneumatized sphenoid sinuses.

We completely agree with Dr. Kunicki concerning the difficulties in interpreting endoscopic pictures of the sphenoid sinus showing distinguished types of SNA. As a matter of fact, to enhance the 2D vision of the endoscope, a virtual, computer-based 3D model with a special light endoscopic simulator has been created to highlight the angle. In our opinion, this tool allowed the most convincing representation of the angle we were able to produce using modern neuroimaging techniques.

It has also been observed that “unfortunately, we do not refer to the variability and morphological appearance of the mediolaterocranial OCR, even though we commented on the recent studies by . . . Ozcan et al. on the same subject.” We have stressed the importance of the mediolateral OCR in our Laboratory Findings section as the key landmark to perform the transturberculum transcplanum approach: “The SSN is drilled out starting from the center toward both the medial optocarotid recesses; once this structure is dissected and removed, in the suprasellar space, the drilling can be safely extended more laterally.” As already described elsewhere, the medial OCR lies at the confluence of the medial-inferior aspect of the optic canal, carotid canal, and upper portion of the sella. We have calculated its variability, measuring the interoptic distance at the optic canal entrance (IDOC), as this point can be easily identified and measured on a CT scan. We also calculated the variability of all the main structures surrounding the SNA—not only the IDOC, but also the interoptic distance at the limitus and the chiasmatic groove sulcal length at the midline.

It has been affirmed that we “performed a statistical analysis to show and confirm our observations on morphometric correlations between linear measurements of the chiasmatic sulcus and its slope angle, and it seems that the number of observations as a whole and especially within the groups counting 3 and 4 cases each (Type I and II) do not qualify for significant conclusions, despite some statistical significance.”

Concerning statistical analysis, 24 routine skull CT scans in patients with no brain pathology were examined. The most relevant and interesting point that we found was a significant interdependency between the SNA and IDOC. It can be affirmed that the value decreases as the SNA enlarges. In our opinion, one cannot confuse significant statistical analysis interdependency results with a simple distribution of the SNAs in the 3 main types. It stands clear that some anatomical variations of the skull base can be relatively infrequent in the largest groups or populations as well (that is, the conchal type of sphenoid sinus is estimated to appear in 3% of cases).

Concerning the results in the article by Guthikonda et al., it was observed that “there are significant groups of both steep and plane chiasmatic sulci with either wide or narrow interoptic distances, which is in contrast to our conclusions.” We have to remark that a steep and plane chiasmatic sulcus with either wide or narrow interoptic distances is not the same as the SNA; once again, the SNA cannot be confused with the chiasmatic sulcus or even what Kunicki has renamed the TS complex.

At the end of his letter, Dr. Kunicki has asserted that “the ease of reconstruction is rather dependent on the distance and angle between the rest of the sellar floor and planum margin and the angular characteristics of the TS, and the proposed division is not so important in these phases.”

Concerning the osteodural defect reconstruction, our laboratory findings suggested that it could be reasonably affected by the anatomical variability of the SNA. Positioning of the pedicled flap is optimal as the open-angle shape of the SNA increases. Indeed, such conformation guarantees adequate adhesion of the flap to the bone surface against gravity; for the same reason, in our experience, wedging and blocking dural and bone substitutes by using an obtuse angle, which means an increased distance between the sellar floor and planum sphenoidale, ensures easier positioning of the solid material in the extradural space.

Finally, Dr. Kunicki observed that “the range of view and accessibility are determined not only by the intracranial TS angle, but also by the degree of planum resection as well as the distance and angulation of the optic chiasm as range of endoscope advancement and angulation within the sinus.” As already described elsewhere, the transturberculum transcplanum approach permits access to lesions located in the suprachiasmatic, subchiasmatic, retrosellar, and ventricular areas. An extensive resection of the planum sphenoidale anterior to the falciform ligament results in wider exposure of the gyrus rectus of each frontal lobe, which is not the proper target of the approach. The position of the optic chiasm is paramount while accessing the suprasellar area. Indeed, depending on the position of the chiasm, a wider suprachiasmatic route with a reduced subchiasmatic route or vice versa can be achieved, but this was not the aim in our study; further anatomical studies are needed to conclusively elucidate this topic. Based on our observations, we can affirm that the intradural window provided by wider SNA types can offer better exposure and instruments maneuverability during both endoscopic and microsurgical approaches to the suprasellar area.

Finally, we want to again express sincere appreciation to Dr. Kunicki for giving us the opportunity to explain our findings and discuss such a topic.

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Neurolasermicroscopy

To The Editor: With interest we read the paper by Sanai et al. (Sanai N, Snyder LA, Honea NJ, et al: Intraoperative confocal microscopy in the visualization of 5-aminolevulinic acid fluorescence in low-grade gliomas. Clinical article. J Neurosurg 115:740–748, October 2011) combining an approach of neurolasermicroscopy (NLM) and 5-aminolevulinic acid tumor fluorescence in low-grade glioma.1 The same technology was introduced 3 years ago, demonstrating for the first time an ex vivo approach in human glioblastoma multiforme to generate high-resolution images after topical staining of the resected specimens.4 In this previous pilot study from 2009, one major observation was that brain tumor zones were clearly detectable with this new technique, which was termed “neurolasermicroscopy.”

Even though the same confocal scanning technique was used in both mentioned studies, the method of staining the tissue of interest was somewhat different. In the ex vivo approach, acriflavine hydrochloride was topically applied to the surface of the tissue and permitted to reveal the diagnosis of glioblastoma multiforme according to WHO criteria. This is an amelioration to the data referred to in a rodent model,2 since a high mitotic rate and cell pleomorphism of the tumor cells as well as mitotic figures, fibrillary matrix, and distinction between tumor center and infiltration zone were shown.

In the field of high-resolution imaging at a cellular and subcellular level and its application, there are many problems ahead that should be resolved in the future when adapting techniques like NLM into the operating room.3 The administration of effective and safe contrast agents is one essential step as mentioned above. The reprocessing of these high-technique confocal laser scanning devices, to be inserted repeatedly during ongoing surgery, is another crucial step toward clinical application and should be focused in the near future.

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Disclosure

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References


RESPONSE: We thank Drs. Schlosser and Bojarski for

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bringing to our attention their papers on the topic.\textsuperscript{1,2} As they note, the ex vivo approach used in these studies required tissue resection and manipulation, including application of a topical dye, prior to confocal imaging analysis. In our recently published work we utilized a different strategy that enables in vivo analysis of the tissue, allowing it to be visualized in situ without cytoarchitectural or vascular disruption. These 2 techniques, while distinct, are complementary.

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