Neurosurgical forum
Letters to the editor

Neurosurgical mortality rates

TO THE EDITOR: In an article published online in July by Hammers et al. (Hammers R, Anzalone S, Sinacore J, et al: Neurosurgical mortality rates: what variables affect mortality within a single institution and within a national database? Clinical article. J Neurosurg 112:257–264, February, 2010), the authors utilize data from the University HealthSystem Consortium Clinical Database (UHC CDB) to argue that “there is no gold standard for making comparative mortality index measurements in neurosurgery.” We are writing to correct a few factual errors regarding the UHC CDB and to call attention to a fundamental statistical error that undermines the authors’ conclusions.

The authors correctly point out that patients with intracranial pressure monitoring and shunt procedures are grouped along with open craniotomy cases under the broad heading of Neurosurgery in the UHC CDB. The UHC CDB allows for substantial drilldown that could have been used to analyze any desired subgroup of Neurosurgery patients by Medicare Severity Diagnosis-Related Group (MS-DRG), Major Diagnostic Category, medical diagnosis, and/or procedure.

The authors also note the importance of transfers or emergency admissions in contributing to the risk of death. These variables are, in fact, tested for significance in the UHC CDB risk-adjustment models. The models for intracranial vascular procedures (MS-DRGs 20–22) and for craniotomy with endovascular procedures (MS-DRG 25–27) both account for transfer status. The model for open craniotomies with a primary CNS diagnosis (MS-DRGs 23, 24) accounts for emergency or trauma center admission. For ventriculostomy and shunt procedures (MS-DRG 31–33), neither variable tested as significant, so neither is in the model. All UHC risk models include the patient’s risk of mortality level assigned by the 3M APR DRG Software, which also takes into account trauma, transfer, and admission status. The risk of mortality variable has the highest coefficient in all of the models listed above.

The more serious issues concern fundamental statistical flaws in the analysis. First, if one is interested in knowing the effect of trauma on overall mortality in neurosurgery, the unit of analysis is the patient, not the hospital. By analyzing death rates based on Level 1 trauma center status, the authors implicitly assume that excess mortality is related to a patient factor (trauma), even if the death occurs in a patient without that factor (an elective surgical admission in a Level 1 trauma center). This common statistical error is known as the “ecologic fallacy”.2,3

Second, if the authors’ goal was to estimate a mortality rate for each neurosurgical program, the appropriate analytical technique would have been to use hierarchical generalized linear modeling. This technique would take into account the clustering of patients within hospitals.5 Third, the recursive partitioning analysis conducted using CHAID (chi-square automatic interaction detection) included the number of emergency department admissions as one of their predictors. For the hospitals where the authors could not find the number of emergency department admissions, they set this predictor to zero. Instead, they should have excluded these hospitals. Emergency department admissions for UHC hospitals are available by contacting UHC.

Because of the aforementioned issues, the question of whether trauma patients admitted to Level 1 trauma centers have a higher risk of death remains unanswered. We believe that the great majority of that risk is already captured in the UHC approach. The UHC risk models noted above all have c-statistic values of 0.87 or greater, meaning that for every living/dead patient pair, the model correctly assigned a higher odds of death at least 87% of the time, an extremely high level of precision. Risk-adjustment models for death have a strong track record of identifying areas of opportunity for performance improvement projects. They perform at or near the level of nurse-abstracted records at a fraction of the cost.1,4 The appropriate question to ask of such a model is not “Is it a gold standard?” but rather “Is it useful in improving patient care?”

MARK A. KEROACK, M.D., M.P.H.
STEVEN J. MEUBER, PH.D., M.B.A., M.H.S.
ALLISON LEE SABEL, M.D., PH.D., M.P.H., C.M.Q.
University HealthSystem Consortium
Oak Brook, Illinois

References


RESPONSE: The UHC CDB represents the best available comparative medical data in the US. We are honored that our manuscript generated a critique from the UHC. This paper was reviewed by representatives of the UHC prior to publication.

First, we must clarify that the conclusion of our article is that in neurosurgery “the data are elusive, documentation is variable, and the modes of statistical analysis
Neurosurgical forum

in use are questionable.” Our conclusion is not that Level 1 trauma centers have “excess” mortality or any one hospital is superior to another. Additionally, it is our intention to highlight the understanding of mortality data within neurosurgery, which is at worst inaccurate and at best incomplete and needs refinement. The aim of our manuscript is to bring to light that there is currently no standard method to compare mortality between neurosurgical departments, given the many variables that affect mortality. It is not our intention to assume that our method of statistical analysis of this data is superior, but rather these questions about statistical analysis serve to confirm our conclusion: that current modes of statistical analysis for neurosurgical mortality rates across the country are not necessarily representative of reality.

The critique stated “If one is interested in knowing the effect of trauma on overall mortality in neurosurgery, the unit of analysis is the patient, not the hospital.” We couldn’t agree more, but that is not the standard by which mortality is measured, even within the data provided to us by the UHC. Certainly, this ideal provides a lofty goal for our specialty.

We do not believe there is “ecological fallacy” in our finding of higher mortality at Level 1 trauma centers. In finding higher mortality rates in American College of Surgeons–certified Level 1 trauma hospitals, we have simply identified a relationship that requires further investigation as to both its significance and potentially its etiology.

We wholeheartedly agree with the UHC perspective that the appropriate goal of statistical analysis of the neurosurgical population is not simply an exercise to identify the gold-standard statistical tool, but rather to ask and answer the question, “Is it useful in improving patient care?” We can only hope that this manuscript serves to both open the eyes of the neurosurgical community to the factors that are important to the general population and also spur us to continue our commitment to excellence within our specialty. (DOI: 10.3171/2009.10.JNS091530)

ROBERT HAMMERS, M.D.
THOMAS C. ORIGITANO, M.D., PH.D.
JAMES SINACORE, PH.D.
SUZAN ANZALONE, B.S.
Loyola University Medical Center
Maywood, Illinois

Endoscopic pituitary surgery

To the Editor: We read with great interest the article by Tabaei and coauthors (Tabaei A, Anand VK, Barrón Y, et al: Endoscopic pituitary surgery: a systematic review and meta-analysis. Clinical article. J Neurosurg 111:545–554, September, 2009). We would like to analyze some aspects of the study related to the advantages reported by the authors of the endoscope in transsphenoidal operations.

The use of the endoscope in transsphenoidal surgery allows visualization of lateral (cavernous sinus) or suprasellar tumoral remnants that have not descended into the sellar cavity during surgery. In such cases, at the end of our microsurgical transsphenoidal operations we use the endoscope to control the operative field, particularly if suprasellar residual tumor is present and is removable without provoking any damage. Our transsphenoidal microsurgical procedure attempts to remove the entire tumor if possible without provoking any new neurological deficits that could be caused by removal of small tumoral remnants located in the lateral compartment of the cavernous sinus. Consistent with other expert authors, we usually indicate postoperative radiosurgery or (even better) hypofractionated stereotactic radiotherapy for these small tumoral remnants. For suprasellar remnants that do not descend into the sellar cavity during surgery because of their fibrous consistency and adherence to suprasellar structures, we prefer not to perform hazardous tractions that expose the patient to possible neurological or vascular damages. In such cases, we prefer to wait for a progressive descent of the tumoral remnant into the sellar cavity during the postoperative period (15–30 days in our experience). At that point we can treat the tumoral remnant using serial MR imaging, radiotherapy, or a second transsphenoidal operation (Fig. 1). In our opinion, the possibility of visualizing a tumoral remnant during surgery (which is possible using the endoscope or intraoperative MR imaging) does not ensure its safe removal; we would like to know the authors’ opinion of this aspect of the procedure.

The authors report that the endoscopic transsphenoidal approach allows the removal of giant suprasellar macroadenomas that would otherwise require a craniotomy. In our experience, the transcranial approach for giant pituitary adenomas is very rare and reserved only for those tumors inaccessible via the transsphenoidal approach, for example those tumors that present with their predominant component laterally to the cavernous sinus (in the middle cranial fossa). We always treat giant pituitary adenomas that have median or even anterior suprasellar extensions using a microsurgical transsphenoidal approach, typically in 2 transsphenoidal stages, with excellent clinical and radiological results as also reported by other authors. We believe that in giant tumors the problem is not the microscope or endoscope, but the tumor consistency and shape, the position of the carotid and A1 tracts of anterior cerebral arteries, the invasiveness of the cavernous sinuses, and other aspects. We would like to learn of the authors’ procedures during transsphenoidal surgery for giant pituitary adenomas.

Regarding the operative time duration, we are surprised about the reported data because, in our experience, it usually ranges from 45 to 120 minutes and does not depend on the approach to the sella turcica and to the tumor, which are usually exposed in 10–15 minutes, but depends on the tumor characteristics (such as consistency, invasiveness, and bleeding) and the presence of an intraoperative CSF leak. If possible, we would like to know from the authors which phases of surgery require a lot of time.

As for the length of stay in the hospital, we believe that having patients spend only 1 postoperative day in the hospital is not very prudent, because it is well known that during the 24-48 hours following pituitary surgery many patients experience water and electrolyte disturbances (mainly diabetes insipidus) although they are usually transitory; however, monitoring of at least diuresis, urinary specific gravity, and natremia is important during this time. For example, for the treatment of the common
symptom of transitory diabetes insipidus, vasopressin is important; if not treated, patients could develop serious disturbances, especially if they are of an advanced age. We would like to know if the authors give the patients particular diuresis and urinary specific gravity for 3–4 days.

Finally, concerning the reportedly reduced invasiveness of the endoscopic approach (because of its endonasal access) compared with the sublabial microsurgical approach, we can say that in our experience all transsphenoidal microsurgical approaches are transnasal submucosal, performed through 1 nostril using a small opening (1.5–2 cm) of the mucosa over the nasal septum. This is followed by exposing the vomer and therefore the anterior wall of the sphenoidal sinus within a few minutes.

Response: We thank the authors for their valuable comments and insightful questions. This sort of critical analysis and constructive discussion only benefits the development of the field and we appreciate the opportunity to participate in this conversation. The authors’ comments are explored in the following paragraphs.

The authors question the surgical objective of removing the entire tumor and believe that small lateral remnants or suprasellar extensions should not be dissected for fear of causing neurological injury. The authors describe a strategy of treating tumor remnants with observation, radiation therapy, or repeat surgery. Although these strategies are acceptable, we would argue in favor of avoiding radiation therapy or repeat surgery if it is possible at initial surgery. Radiation carries a risk of hypopituitarism of up to 50% with fractionated radiation, at least 40% with intensity-modulated radiation therapy, and 30% with stereotactic radiosurgery, and a lower but still unacceptable risk of damage to the optic apparatus (2.8%) or induced malignancy (2.4%). Likewise, revision surgery carries a higher risk of postoperative pituitary dysfunction and CSF leak. In our experience with endoscopic surgery, a larger bone opening allows for improved visualization of the superior extent of the tumor as well as the tumor adjacent to the cavernous sinus. Hence, the risk to normal neurovascular structures is not greater and the need for postoperative radiation therapy and repeat surgery is less. Perhaps the author uses the words “hazardous tractions” because the view with the microscope and a more limited bone opening does not permit a direct observation of the...
extent of the pathology. The low incidence of complications reported in our study supports this opinion.

With regards to our treatment for giant pituitary adenomas, we favor a single-staged extended transsphenoidal endoscopic approach. Typically, we remove the tuberculum sellae and planum sphenoidal so we can directly view the suprasellar extent of the tumor. Using the endoscope, exposure of the third ventricle and even the lateral ventricles is possible. Although the approach to remove what comes easily and wait for any residual tumor to fall back into the sella, as advocated by the group from Rome, may work in certain circumstances, in our experience large fibrous tumors do not always descend into the sella and visual field deficits do not improve as well if residual tumor is not resected. While a larger bone opening may increase the risk of CSF leak, we have developed a technique called the “gasket-seal” closure that allows successful closure of large skull base defects with an acceptably small risk of CSF leak. Likewise, a nasoseptal flap may be added to further secure the closure.

In our hands, the endoscopic approach does take slightly longer than a microscopic approach. The component of the surgery that increases in duration is clearly the approach. The reason for the increase in time is that the otolaryngologist creates a large working area by removing the nasal septal vomer and the rostrum of the sphenoid sinus to allow for adequate space for the endoscope and surgical instruments. However, the increased field of view, which in turn makes the surgery safer since improved visualization is possible, is worth the additional time. We also wish to point out that a difference in operative time is a less critical outcome measure when compared with tumor control and incidence of complications.

The authors question the length of stay after surgery and the risk of delayed diabetes insipidus. In our experience, the risk of diabetes insipidus after removal of small microadenomas is extremely low. For larger tumors, we tend to admit patients slightly longer in the hospital. However, patients who are cognitively intact are generally able to regulate their fluid intake. Unless they have severe hypothalamic damage and have lost their thirst mechanism, they will replenish their lost fluid by increasing their fluid intake. Upon discharge, patients are instructed to contact the office immediately if there is any increase in frequency of urination or excessive thirst. Patients who have compromised cognition are not discharged on postoperative Day 2.

The authors state that the transnasal microscopic submucosal approach performed through 1 nostril is as equally minimally invasive as the endoscopic endonasal approach. Although we agree that the level of invasiveness between the 2 methods is similar, there are distinct differences. The advantages of the endonasal endoscopic approach for microadenomas is probably minimal. However, we believe the panoramic visualization afforded by the endonasal endoscopic approach represents a distinct advantage compared with the standard microsurgical approach in patients with more extensive pathology. To achieve the same view with the microscope would be extremely invasive. In addition, the patient discomfort associated with a submucosal dissection is avoided. (DOI: 10.3171/2009.4.JNS09493)
life,1 and we believed that total or near-total resection would be the aim. We were therefore surprised to find that one-third of the patients with brain metastases in this series had subtotal resections; 2 of them in the cerebellum, and 1 each in the frontoparietal and parietooccipital lobes. Microscopic techniques would have achieved better results in this group of patients as well.2

If the learning curve for endoscopic resection of deep-seated brain tumors using the techniques described by the authors in this article had achieved these modest results in their hands, with their extensive and impressive record in neuroendoscopy, we would be very concerned about what the outcome of these techniques would be if attempted or used by other less experienced teams. We believe that common sense should prevail and the endoscope should be regarded as a tool complementing the microscope. The endoscope is not a hammer and not everything in neurosurgery is a nail. (DOI: 10.3171/2009.4.JNS09472)

MARIO TOO, M.R.C.S.
SAM ELJAMEL, M.D., F.R.C.S.(SN)
Ninewells Hospital
Dundee, United Kingdom

References

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

Abscess in a metastasis

TO THE EDITOR: We read with interest the article by Guinand Vives et al. (Guinand Vives CH, Monsalve Duarte GA, Valderrama Beltrán S, et al: Brain abscess caused by multidrug-resistant Acinetobacter baumannii. Case report. J Neurosurg 111:306–310, August 2009) describing a case of intracerebral abscess caused by Acinetobacter baumannii. These authors documented the case of an abscess in a young soldier who had sustained penetrating cranial trauma. The patient had a protracted compromised neurological status and a progressively downhill course ultimately leading to death. The abscess, which developed secondarily at the site of injury in the brain, yielded A. baumannii, which was characterized by multidrug resistance eventually leading to a fatal outcome. The authors provided a thorough case description and subsequently reviewed the literature relevant to the case. They correctly pointed out the growing menace posed by the emergence of A. baumannii as an important multidrug-resistant nosocomial pathogen, especially in infections of the CNS, with an overall poor outcome. They also indicated that their case is probably the first reported instance of intracranial abscess caused by A. baumannii.

We recently came across a case of brain abscess in which A. baumannii was isolated. A 36-year-old woman presented with features suggestive of elevated intracranial tension. She had completed both chemotherapy and surgery for ovarian cancer that had been diagnosed 1 year ear-

lier. Brain MR imaging showed a lesion in the left frontal region, which was hypointense on T1-weighted and hyperintense on T2-weighted images, with moderate peripheral postcontrast enhancement (Fig. 1). Diffusion weighted images revealed a freely diffusing lesion, and spectroscopy demonstrated elevated choline and lactate within the lesion, which were suggestive of necrotic metastasis. Her disease was otherwise locoregionally controlled. She underwent a left frontal craniotomy for tumor excision. The tumor was cystic with a well-formed capsule containing thick yellowish fluid with a pseudoplane all around, enabling a gross-total resection. The cyst fluid grew A. baumannii (that was sensitive only to tigecycline. As our perioperative impression was necrotic metastasis, antibiotics were not initially given, as is routine at our center. The patient, who had received a single dose of antibiotics perioperatively, subsequently had a wound infection with Escherichia coli and Klebsiella pneumoniae. Appropriate antibiotics as per the sensitivity pattern were started (cefoperazone-sulbactam combination and amikacin). Interestingly, A. baumannii was not isolated from the wound. Blood cultures were negative. The wound infection resolved following 2 weeks of antibiotics. The histopathology report was consistent with a poorly differentiated adenocarcinoma from the ovary (Fig. 2). She subsequently underwent whole-brain radiotherapy. At a 2-month follow-up, the patient was faring well with no residual lesion on imaging.

Our case follows the report by Guinand Vives et al. in its documentation of a cerebral abscess caused by A. baumannii; however, numerous differences are evident between the 2 cases. Unlike in the case reported by Guinand

Fig. 1. Axial T1-weighted (A), T2-weighted (B), FLAIR (C), and post-Gd T1-weighted (D) MR images demonstrating a left posterior frontal cystic mass.
Vives et al., our patient probably had a primary brain abscess. The peculiarity of our case was that the abscess occurred within a metastasis—such an occurrence is rare.\textsuperscript{1,2,5} Radiology often cannot differentiate between an abscess and a necrotic neoplasm.\textsuperscript{3} Seeding of a preexisting metastatic deposit by circulating (hematogenous) organisms is thought to explain such a coincidence. A similar mechanism is held responsible for abscesses developing in a previously devitalized ischemic area.\textsuperscript{4} The primary source of the infection remained cryptic in our patient, however. An alternative explanation could be that the tumor embolus itself was infected as is seen in cases of infective endocarditis.\textsuperscript{7} The source of the organism in our patient was probably community acquired. Although \textit{Acinetobacter} species are known to be ubiquitous in the environment, \textit{A. baumannii} isolation rates vary depending on the isolation technique as well as the time of the year and the geographical location where an evaluation is performed.\textsuperscript{6} In contrast to the patient with a secondary abscess, our patient had a favorable clinical outcome. This result may be attributed to a less virulent strain of the bacteria. It has been pointed out by Guinand Vives and colleagues that community-acquired strains and those isolated from healthy individuals may be less virulent and more sensitive to routine antibiotics than the nosocomial strains, which could explain the differences noted in our case vis à vis that reported by Guinand Vives et al.\textsuperscript{2,5}

Fig. 2. Photomicrograph showing deposits of metastatic adenocarcinoma in the wall of the resected lesion. H & E, original magnification × 200.

The 2 contrasting cases (both caused by the same organism) with differing clinical scenarios and divergent outcomes underlie the heterogeneity of disease causation by the same etiological agent, highlighting the lacunae in our current understanding and calling for heightened awareness, close surveillance, and meticulous reporting of these uncommon but emerging pathogens. (DOI:10.3771/2009.6.JNS09829)

\begin{thebibliography}{9}
\bibitem{1} Arseni C, Simionescu M, Carp N, Guran C: The abscess-like form of brain metastases. \textit{Acta Neurochir} (Wien) 17:113–125, 1967
\bibitem{5} Ng WP, Lozano A: Abscess within a brain metastasis. \textit{Can J Neurol Sci} 23:300–302, 1996
\bibitem{7} Skaria B, Kalra P, Simpson IA: Septic emboli from aortic valve endocarditis. \textit{Heart} 90:865, 2004
\end{thebibliography}

\textbf{Response:} No response was received from the authors of the original article.
However, transdural spread of infection or pus, although uncommon, may be a possible explanation for this unusual inner table hyperostosis. As the authors mention, osteitis following rhinogenic sepsis is possible and frequently extensive osteitis of the anterior skull base may be observed in patients with rhinogenic intracranial suppuration. In our experience this is best treated nonoperatively with high-dose intravenous antibiotic therapy.

The formation of inner table pseudocapsular bone formation attempting to wall off a focal intradural pathology (brain abscess) in the presence of an intact dura mater, absence of osteitis or epidural pus collection, and an adjacent accompanying subdural empyema makes this clinical presentation especially unique. Even more unusual is chronic paranasal sinusitis stimulating the adjacent frontal calvarial bone to form pseudocapsular bone without direct contact with the focal infection that it is attempting to wall off. We postulate a possible scenario: given the hyperostosis of the inner table, it appears that the abscess developed first, and then perforated the dura with limited spillage of pus into the epidural space. The multiple courses of antibiotics for treatment of recurrent sinusitis may have been sufficient enough to treat the epidural spillage and/or possible adjacent osteitis, and allow for pseudocapsular bone formation to wall off and contain the epidural pus collection, resulting in spontaneous sealing of the dural perforation. Furthermore, given the prominent pseudocapsular formation/bilateral bone spicules, is it possible that the dura was torn during the lifting of the bone flap and that a previously healed dural perforation was missed?

Some issues remain unanswered, given the brevity of a case illustration. How long was this patient’s clinical prodrome prior to presentation? No information is provided about this patient’s premorbid status regarding his team for their comments regarding our recent publication and about their own contribution regarding the pathophysiology of this unusual form of Garré disease. For practical reasons (word limit restriction of a case illustration), clinical and biological data were limited in this paper. Our patient, an immunocompetent nondiabetic man, presented with a 15-day duration of clinical prodrome. No other source of infection was identified. He was treated 1 month prior to presentation for frontal sinusitis. His medical history revealed that he had experienced recurrent paranasal sinusitits until the age of 24 years. It was clear that when the craniotomy was performed, no abnormalities were observed on the epidural space as on the dura. The patient is now in good health.

Garré disease is a rare but well-described pathologic entity in dental and rheumatological literature. Although the cause and biological behavior of this pathology is unknown, the suggested causes include chronic infection and bone deposited in response to an unusual inflammation process (perhaps reflecting an abnormal immune reaction). However, tissue cultures consistently reveal no organism present. As noted in our case, this disease is clearly separated from the classic form of chronic osteomyelitis.

We encountered many cases of atypical rhinogenic intracranial infections in the last few years (Akhaddar et al., unpublished data, 2009).1–4 Biological and imaging evaluations using CT scans and/or MR images are important for diagnosing such rare complications. Effective therapy, ensured by close coordination of care between otorhinolaryngologists, neurosurgeons, and infectious disease specialists, must be promptly instituted.

We are grateful to Dr. Nathoo and colleagues for highlighting this important issue and look forward to further discussion on this matter. 

References