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.”

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.

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. 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?”

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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
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)

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Endoscopic pituitary surgery

To the Editor: We read with great interest the article by Tabae and coauthors (Tabae 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