Physician specialty and endovascular treatment of intracerebral aneurysms

TO THE EDITOR: We read with interest and great concern the paper by Fennell et al.1 (Fennell VS, Martirosyan NL, Palejwala SK, et al: Morbidity and mortality of patients with endovascularly treated intracerebral aneurysms: does physician specialty matter? J Neurosurg 124:13–17, January 2016). The paper presents a retrospective analysis on morbidity and mortality rates of endovascular embolization of intracranial aneurysms according to the primary specialty of the treating operator. The data source is the University Health System Consortium database, which includes 120 academic centers with more than 300 of their affiliated hospitals. The authors’ claim is that the morbidity and mortality of patients with ruptured and unruptured intracranial aneurysms treated with endovascular embolization is higher for those treated by neurologists or radiologists than those treated by neurosurgeons. They further assert that this is related to the less intense training and exposure to neurocritical care of neurologists and radiologists compared with neurosurgeons for whom training includes early exposure to endovascular and open procedures. In addition, they mention a concern about the variability of training in endovascular procedures. We believe this paper is at best a disservice to the specialty and at worst a political statement disguised as science for the following reasons.

The authors’ entire paper focuses on morbidity and mortality adjudicated to the treating operator, when in reality the periprocedural care is as important or even more so than the actual procedure, and morbidity and mortality may be unrelated to the procedure. With regard to the issue of training, the authors show no knowledge and fail to mention that for neurologists there is a requirement of 1 year of vascular neurology or neurocritical care of neurologists and radiologists compared with neurosurgeons for whom training includes early exposure to endovascular and open procedures. In addition, they mention a concern about the variability of training in endovascular procedures. We believe this paper is at best a disservice to the specialty and at worst a political statement disguised as science for the following reasons.

The authors’ entire paper focuses on morbidity and mortality adjudicated to the treating operator, when in reality the periprocedural care is as important or even more so than the actual procedure, and morbidity and mortality may be unrelated to the procedure. With regard to the issue of training, the authors show no knowledge and fail to mention that for neurologists there is a requirement of 1 year of vascular neurology or neurocritical care after residency and before the endovascular training that can be extended to 2 years. Moreover, their whole discussion not only is against current American Heart Association guidelines, which recommend a multidisciplinary approach, but is also contradictory when they state that their neurosurgical “multiphysician model of surgeon, interventionalist, and intensivist” is an unsafe anachronism and must be abandoned.

The authors’ analyses are not statistically sound and have several limitations. First, they do not provide confidence intervals with any of their estimates, nor do they provide expected and observed rate of complications to compare with. Second, this database consists of self-reported complications from academic centers “with a focus on quality, safety and excellence,” yet it is very well known that self-audits usually result in a lower rate of complications than when the procedural audit is independent. Third, one should not accept the validity of any report of operative morbidity and mortality when factors such as volume of cases, case mix, and risk adjustment were not carried out, as it does not allow a fair comparison. Fourth, the authors’ data show that the great majority of cases are actually treated by neurosurgeons, followed by radiologists and then neurologists; this is important as the point estimates may be less accurate for those with lower case volume, besides the fact that one cannot exclude the effect of chance, not to mention the conspicuous absence of the actual absolute numbers and confidence intervals in the data presented. Fifth, the argument of justification of publication on the basis of “statistical significance” without consideration of the points mentioned above is not scientifically sound. Last but not least, there is a major flaw in the design of the study, considering that in October 2009 the International Classification of Diseases, Ninth Edition underwent a major revision pertaining to Current Procedural Terminology code 39.72. This actually coincided with the present study and was not accounted for by the authors. The revision included 2 new codes more specific for endovascular coil embolization of intracranial aneurysms, 39.75 and 39.76, to include occlusion of the head or neck vessels using bare coils and bioactive coils, respectively. These new codes leave endovascular treatment of aneurysms using devices other than coils to code 39.72 (Table 1) (see https://www.cms.gov/Regulations-and-Guidance/Guidance/Transmittals/downloads/r1770cp.pdf).
TABLE 1. New ICD-9 codes, added in 2009, related to diagnosis and coiling of aneurysms

<table>
<thead>
<tr>
<th>Code No.</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>ICD-9 procedure</td>
<td></td>
</tr>
<tr>
<td>39.72</td>
<td>Endovascular embolization or occlusion of head &amp; neck vessels (description revised October 1, 2009)</td>
</tr>
<tr>
<td>39.75</td>
<td>Endovascular embolization or occlusion of vessel(s) of head or neck using bare coils (new October 1, 2009)</td>
</tr>
<tr>
<td>39.76</td>
<td>Endovascular embolization or occlusion of vessel(s) of head or neck using bioactive coils (new October 1, 2009)</td>
</tr>
<tr>
<td>ICD-9 diagnosis</td>
<td></td>
</tr>
<tr>
<td>430</td>
<td>Subarachnoid hemorrhage</td>
</tr>
<tr>
<td>437.3</td>
<td>Cerebral aneurysm, nonruptured</td>
</tr>
</tbody>
</table>

We wholeheartedly agree with the idea that interventional procedures need to be audited, which requires knowledge of the expected and acceptable rate of complications as well as risk adjustment. We further agree that training needs to be standardized. However, audits and standardization will require collaborative work for which we would welcome initiatives such as the American College of Surgeon National Surgical Quality Improvement Program (ACS NSQIP; https://www.facs.org/quality-programs/acs-nsqip) applied to neuroendovascular procedures. We would also welcome interspecialty discussions targeting standardization of training and the creation of board certification that is inclusive of, and accounts for, the different backgrounds of those performing the procedures, out of fairness to patients, society, and practitioners from all training backgrounds entering the specialty. In the end, the volume of cases and the presence of a multidisciplinary team providing care from admission to discharge may prove to be the key determinants of the level of skill and rate of complications rather than the background specialty of the operator. We also think that claims like those by Fennell et al., are unfair and potentially inflammatory given the substantial limitations mentioned above.

Alberto Maud, MD  
Gustavo J. Rodriguez, MD  
Paisith Piriyawat, MD  
Salvador Cruz-Flores, MD  
Texas Tech University Health Sciences Center of El Paso, El Paso, TX

References

Disclosures
The authors report no conflict of interest.

Response
We wish to thank Dr. Maud and his colleagues for their comments in regard to our article. Their critique is at times valid, and we would like to address individual points.

The authors comment “… periprocedural care is as important or even more so than the actual procedure, and morbidity and mortality may be unrelated to the procedure.” We agree with this point on aneurysm ruptures but disagree with the authors on elective treatment of unruptured aneurysms. Similar findings of complications based on specialty training were noted between both ruptured and unruptured aneurysms. In the senior authors’ experience, postoperative treatment is performed by the treating physician, although it is noted that this may not be true at all facilities and may be part of the reason for the difference in patient outcomes.

The authors comment, “The authors’ analyses are not statistically sound and have several limitations.” While we do not agree that the statistics are unsound, we have provided an updated Table 1 that provides standard error (SE). The statistical error and findings initially reported are unchanged. We have addressed the issue of case volume in our editorial response to Dr. Walter Montanera published with the original article in the January 2016 issue of *Journal of Neurosurgery* and refer the authors and readers to this piece.1 We acknowledge that the additional codes should have been included in the analysis, but we do not suspect that their inclusion would change the findings in any meaningful way. We acknowledge limitations of the data set but feel the uniformity of the University Health System Consortium database (in that only academic centers are included) allows for some estimation of heterogeneity of cases between practitioners.

We suspect all interventionalists treating cerebral aneurysms ultimately want the same thing for their patients: good outcomes. Using large databases such as the University Health System Consortium allows access to a large volume of health care data, which permits the drawing of potentially important clinical conclusions. To ignore such data may be to ignore a potential opportunity for improvement in patient care. We regret that the authors view the article as “potentially inflammatory,” and, for the benefit of the reader, we repeat our primary conclusion: “In this study there was a statistically significant finding that neurosurgically trained physicians may demonstrate improved outcomes with respect to endovascular treatment