an “interim report” by this journal’s editorial board because some of the patients were treated less than 3 years before the data were analyzed, and their AVMs were not expected to have been obliterated by that time. All patients underwent angiography at the end of 3 years unless magnetic resonance imaging suggested that obliteration had occurred sooner. We reported the obliteration rates for different-sized AVMs in two different ways. The first was a simple fraction that had its denominator equal to the number of patients with follow-up angiograms: 74.4% obtained for AVMs 10 cm³ in size or smaller as an interim report. The second, more conservative numerator was the number of patients with follow-up angiograms: 74.4%, 64%, and 82%, respectively. These estimates turn out to have been robust in longer-term (3–7 years) follow-up that has revealed an 85% obliteration rate for AVMs 10 cm³ in size or smaller in the same patient cohort. If the authors wish to take their line of reasoning to its logical and humorous conclusion, they could pit the results of microsurgery against radiosurgery 1 day after treatment and come up with a statistical comparison between a microsurgical cohort with a 100% cure rate versus a radiosurgical cohort with a 0% cure rate.

Leaving good-natured repartee aside, I am confident that Pikus and coauthors would agree that neurosurgeons can no longer justify the performance of any particular AVM treatment, even their favorite one, based solely on their local customs, the expectations of referring physicians, or the irrational wishes of misinformed patients. I am also confident that they would prefer that in the future, AVM case selection, analyses of results, and discussions generated by those analyses shall remain free from cognitive or sectarian biases.

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References

Disclosure
I feel obliged to disclose that I am presently a part-time employee of Elekta Instruments, Inc., Atlanta, GA. I affirm that this correspondence was undertaken on my own initiative, without any advance knowledge or notification whatsoever of any other employee, officer, or individual at Elekta Instruments.

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To The Editor: As a neurosurgeon who manages many patients with arteriovenous malformations (AVMs), I would like to congratulate Pikus, et al., on their outstanding operative results in 72 consecutive AVM patients treated over a 10-year interval. Total excision of the AVM was accomplished in 70 patients (97%), and only six patients (8%) developed a new permanent neurological deficit postoperatively. In 19 patients with AVMs less than 3 cm in diameter, the angiographic obliteration rate was 100% and no patient sustained any neurological injury. Based on their data, the authors concluded that microsurgical resection of small AVMs (Spetzler–Martin Grades I–III) is superior to stereotactic radiosurgery. Of note, the same conclusion was reached by the University of Toronto Vascular Malformation Study Group using a decision analysis model based on estimates for cure and complications of both therapies. To my reading, such papers illustrate two important points in the debate concerning the proper management of patients with small AVMs. First, that experienced cerebrovascular neurosurgeons are able to select preoperatively which patients can have their AVM safely excised. Second, that it is difficult, and probably inappropriate, to directly compare the reported AVM microsurgical and radiosurgical results because the characteristics of the two patient groups vary significantly.

Pikus, et al., report the outcomes of 72 consecutive patients who underwent surgical excision of their AVM. Unfortunately, we have no idea how many AVM patients were evaluated for microsurgery at their center over this same time interval. It is likely that a number of patients had AVMs in critical brain locations and were not operated on because it was believed that the morbidity of surgical resection would be too great. Other AVM microsurgical series have compensated for this selection bias by other means. For example, Hamilton and Spetzler only included patients in their series who had undergone complete excision of their AVM. The same group later reported that 19 (59%) of 32 patients with AVMs located in the thalamus, basal ganglia, or brainstem achieved complete obliteration using a multimodality management scheme (average, 3.5 procedures/patient) that included microsurgery, embolization, and radiosurgery. Obviously, exclusion of patients with difficult and complex AVMs will improve the reported results of microsurgical resection. Because many of these patients probably had Grade III AVMs, inclusion of their outcomes in the microsurgical series would alter the reported cure and morbidity rates for small AVMs (0%) used in the comparison of microsurgical excision and radiosurgery by Pikus, et al.

Assuming that experienced neurosurgeons choose not to operate on some AVM patients because they perceive a high risk of postoperative neurological deficits, it becomes crucial to examine closely the characteristics of the AVM patients in any microsurgical or radiosurgical series. Forty-two (58%) of 72 patients had AVMs in eloquent locations in the current series. Yet, only three patients (4%) had AVMs located in the basal ganglia, thalamus, or brainstem. This proportion of deep AVMs is similar to other microsurgical AVM series: 7% in Hamilton and Spetzler (1990) and 11% in Heros, et al. Conversely, over 30% of patients undergoing AVM radiosurgery have deeply located AVMs. Another line of evidence supporting the critical location of AVMs undergo-
ing radiosurgery is the high morbidity and mortality rate from AVM hemorrhage after radiosurgery. As noted by Pikus, et al, the reported morbidity and mortality rate of AVM bleeding during the latency interval has been greater than 50%, far greater than commonly associated with hemorrhage from untreated AVMs. However, Brown, et al., in a series of 168 patients with unruptured AVMs documented a combined morbidity and mortality rate of 48%. They attributed "treatment bias, with the potential for bleeding occurring from malformations located in more sensitive areas" as the explanation for the high morbidity and mortality rate in their study. The similarity between patients in this natural history study of untreated AVMs and those undergoing radiosurgery is clear. In both groups, the majority of patients were evaluated by cerebrovascular neurosurgeons and believed to have high surgical risk AVMs. As a result, when patient outcomes after AVM microsurgery and radiosurgery are directly compared, we may be discussing two very different patient cohorts based on AVM location and morphology.

Pikus, et al, are correct when they imply that the most significant factor militating against AVM radiosurgery is that patients remain at risk for bleeding until the malformation is obliterated. Nonetheless, earlier reports that radiosurgery increases the risk of AVM hemorrhage during the latency interval have been proven incorrect. Three recent studies have examined the AVM hemorrhage rate after radiosurgery prior to obliteration. In these studies more than 2000 patients (> 3000 patient years at risk for bleeding) were evaluated: the hemorrhage rate was found to be either unchanged by radiosurgery, or decreased within 6 months of the radiosurgery procedure.

I agree with the authors that a well-designed, prospective randomized trial would provide more definitive data regarding the relative efficacy and risks of microsurgery and radiosurgery for patients with small AVMs. For obvious reasons, such a trial is impossible. Thus, patients with AVMs have the best outcomes when they are referred to centers where they can be evaluated by experts in microsurgery, radiosurgery, and embolization therapy. In this manner, they can choose a management strategy after a thorough discussion of the relative risks and benefits of all potential options, minimizing the prejudices of any individual physician.

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Response: We would like to thank Drs. Coffey and Pollock for taking the time to analyze and critique our paper and for the thoughtful and gentlemanly way in which these critiques were done.

We fully agree with Dr. Coffey’s statement that applying statistical methods and generating small “p” values does not guarantee that logical or appropriate questions were asked. It is possible to take almost any data set and torture it until it confesses. Therefore, we should clearly restate the questions we attempted to answer in our paper: 1) what patient or lesion-related factors predict functional outcomes and new neurological deficits following microsurgical excision of AVMs? and 2) what are the results when one compares the clinical and lesonal outcomes of published series of Spetzler and Martin Grades I through III AVMs treated microsurgically with small AVMs treated radiosurgically? We think these are logical and appropriate questions.

Dr. Coffey raises the issue of patient selection bias and Dr. Pollock deals with this question at length. The only way to avoid this kind of bias is to do a randomized study. We would welcome a randomized study comparing the results of microsurgery and radiosurgery for those AVMs that would be appropriate for either treatment modality. Until such a study is done we have no choice but to use the data available in an attempt to discover the most efficacious treatment for our patients. We were aware of the problem of patient selection bias and tried to choose case series in which many lesions could be treated with either microsurgery or radiosurgery. Drs. Coffey and Pollock imply that radiosurgically treated AVMs are only those that have been deemed inappropriate for microsurgical treatment. If indeed all the patients in the radiosurgical series harbor AVMs with exceptionally high surgical morbidity then our comparison is less meaningful. Is this really the case? In the 1995 paper from Coffey’s group, 59% of AVMs less than 10 ml in volume that were treated radiosurgically were lobar in location. Why are these lesions inappropriate for microsurgical excision? Even if they involve eloquent cortex, small AVMs can be treated microsurgically without persistent neurological deficit. In the radiosurgical series published by Pollock and colleagues that was cited in our paper, 90% of Spetzler and Martin Grade I and II AVMs were lobar in location. Why are patients harboring these lesions at high risk for micro-
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surgical treatment? It is stated that they refused operation but one does not know what information they were given to come to this conclusion. In our experience it is vanishingly rare for patients to refuse a treatment that has been presented to them as the most efficacious option. We suspect, but cannot prove, that many radiosurgically treated AVMs are appropriate for microsurgical excision.

We must also respectfully disagree with Dr. Coffey’s comments regarding the tautological nature of AVM grading scales. In our study the Spetzler and Martin scale was very good at doing what it was designed for: predicting surgical complications. The fact that it works does not make it tautological. A tautology (all bachelors in our series were found to be unmarried males) must be true by definition. It is conceivable that factors other than the Spetzler and Martin grade might have better predicted a new neurological deficit following surgery. The fact that they did not confirms the value of the scale; it does not make it tautological. This scale may or may not be worthwhile for radiosurgical case selection or risk analysis but that is irrelevant for the purposes of our paper unless there is something about Spetzler and Martin Grade I through III AVMs that makes them particularly poor lesions to treat radiosurgically. Of course, cerebrovascular surgeons will use much information in addition to the patient’s rank on a grading scale in determining the best treatment options. This brings us back to the selection bias argument.

We strenuously object to the charge that we “used erroneous denominators to ensure minuscule obliteration rates for the radiosurgical series.” One could make the charge that radiosurgical reports often use erroneous denominators to ensure maximum obliteration rates for radiosurgical series. If one treated 100 patients with AVMs, obtained angiograms in 50 (because it is suspected that the AVM is gone), and finds that 45 lesions are obliterated the definite obliteration rate is 45%. One might wish it were 90% but this really is not the case. Because we believed that the definite obliteration rate might be unrealistically low in the radiosurgical series we added a category of probable obliteration. We most certainly did not attempt to decrease the efficacy of radiosurgery artificially by using inappropriate denominators. Furthermore, it remains to be seen how “minuscule” these radiosurgical obliteration rates are. The definite obliteration rates with radiosurgery treatment in our review ranged from 36 to 65% with a mean of 45%. Our estimation of probable obliteration rates following radiosurgery ranged from 66 to 74%, with a mean of 71%. The multicenter Japanese study reported at the 1998 American Association of Neurological Surgeons annual meeting by Yamamoto and colleagues found a 50% definite AVM obliteration rate in 885 patients who had been followed for more than 3 years after gamma knife radiosurgery. If only those patients undergoing follow-up angiography are used as the denominator the obtained rate rises to 65%. We stand by our methodology.

Dr. Coffey points out the fallacy involved in pitting the results of microsurgery against radiosurgery 1 day after treatment. This is not what we did in our analysis but it is precisely what is done to advertise the benefits of radiosurgical treatment of AVMs. Many patients with AVMs are candidates for either microsurgery or radiosurgery. We believe that AVMs treated microsurgically assume a higher immediate risk of complication in exchange for lower cumulative morbidity and mortality rates that become obvious only over time. This may be a difficult concept for many patients to appreciate. The absence of immediate risk from radiosurgery is obvious to patients. It is more difficult for them to understand that a period of several years will be necessary to determine whether their AVMs are obliterated and that during this time they continue to be at risk; or that their AVMs may persist and they will be in jeopardy indefinitely unless further treatment is done; or that the complications of radiosurgery are similar to the complications of microsurgery but delayed in onset. The advantages of “have your treatment today and go out to dinner tonight” begin to pale when these factors are considered.

Finally, we agree with Dr. Coffey that neurosurgeons can no longer justify the performance of any particular AVM treatment, even their favorite one, based solely on local customs, the expectations of referring physicians, or the wishes of misinformed patients. That is why we wrote our paper.

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References


Doppler Ultrasound in Subarachnoid Hemorrhage


From a clinical point of view, transcranial Doppler (TCD) ultrasound monitoring in the management of subarachnoid hemorrhage patients would prove useful if by early warning and by early aggressive therapy it enhanced our ability to fight deficits caused by vasospasm. Assuming that a therapy would be effective against the reduction in regional flow caused by vasospasm, prompt therapy could benefit the patient before a clear clinical deficit had been established. In this paper, the usefulness of TCD monitoring seems to have been established by correlation with the late ischemic neurological deficit and with changes in management; unfortunately, no adequate data are available to prove that an elevated velocity of the TCD correlates with the reduction in flow causing the