Microsurgical Keyhole Approach for Middle Fossa Arachnoid Cyst Fenestration

To the Editor:

We read with great interest the recently published article by Levy et al. (2). Regardless of the original description of this innovative keyhole approach to arachnoid cyst fenestration as a safe and effective method for treating middle fossa cysts, we had the impression that during the decision-making process, the authors focused their attention on the arachnoid cysts, placing important associated pathological findings in second place, specifically in the case of children with hydrocephalus.

Arachnoid cysts presumably result from an aberration in the embryological development of the subarachnoid space (4). This hypothesis seems to be the reason for the actual disorder in the cerebrospinal fluid (CSF) circulation and why subarachnoid cysts are frequently associated with hydrocephalus. Whether the cyst is the cause of the ventriculomegaly is a clinical and therapeutic dilemma. In some patients, the location of the cyst interferes with the normal CSF circulation (i.e., those compressing the sylvian aqueduct or those obstructing the outlet foramina in the fourth ventricle); however, in some cases, there is no evident relationship between the cysts and the ventricular enlargement. In those situations, some authors support the idea that the hydrocephalus is communicating and the actual problem is in the subarachnoid space, similar to their subarachnoid cyst counterparts (5). This could be the reason for the low incidence of hydrocephalus (15–18%) in children with middle fossa arachnoid cysts (1, 3).

In this article, Levy et al. (2) use the microsurgical keyhole approach as the initial procedure for treatment of middle fossa arachnoid cysts, including those in three moderately hydrocephalic patients. It is about those patients that we have some concerns. Were their complaints secondary to hydrocephalus or to the cysts? Why does the ventricular size remain stable after the surgical procedure? In this situation, it is advisable to determine whether these stable patients were functioning at their full potential or whether their neurological function could be improved after surgical diversion. In these patients, it is also necessary to define whether they had arrested or compensated hydrocephalus.

Because this procedure provides communication between the cyst and the basal cisterns, we agree with the authors that the proposed technique is useful for arachnoid cysts without hydrocephalus. In patients harboring middle fossa arachnoid cysts and hydrocephalus, the diversion of the cyst could not be effective; thus, the ventricular enlargement should be approached independently, as suggested by Raffel and McComb (3).
accident, causing contusion of the left hemisphere as revealed by magnetic resonance imaging (MRI) (the handedness of the subject was not given, but he may safely be assumed to have been a behavioral right-hander). Because there are similar reports with no evidence of bruising of the contralateral cerebral peduncle (3), it becomes obvious that a different mechanism underlies the weakness ipsilateral to the lesion from that suggested by the authors (bruising of the right peduncle). This mechanism is the abatement of the excitatory influence of the major hemisphere on the minor, causing diaschisis in the latter and hence hemiparesis of the limbs contralateral to it (ipsilateral to the major hemisphere).

Because this matter has recently been put into its proper clinical and historical perspectives elsewhere (5), I will not dwell on it here. However, the report by Binder et al. did contain an important laterality-indexed item. It corroborates the excitatory nature of the influence of the major on the minor hemisphere, i.e., the presence of directionality in callosal traffic between the two hemispheres, from the major to the minor.

Using transcranial magnetic stimulation, the authors reported a “marked increase in voltage threshold, as well as reduction in the complexity of the motor evoked potentials . . . on the hemiparetic left side.” Because the existing data, using the same modality of investigation, clearly establish the similarity of contraction-induced facilitation of motor evoked potentials on either side (10), the findings of Binder et al. would indicate that the right hemisphere of their patient was less excitable after the injury sustained by the left hemisphere because of its functional disconnection from the left hemisphere.

It has been shown that the pathway (directionality) just described underpins the hemispheric asymmetry codified as handedness (3, 4, 7). Accordingly, moving the nondominant side of the body is a bihemispherical event (requires callosal participation), whereas moving the dominant side requires the activity of the command center in the major hemisphere alone, as shown recently by functional MRI (7, 8) (Fig. C1).

This pathway is also involved in the secondary generalization of seizures (6). An opposite directionality exists in the sensory realm, where sensing from the nondominant side requires involvement of the posterior corpus callosum to convey signals arising on the left side of the body to the major hemisphere (9, 12) (where they are consciously appreciated). This has also been documented by the absence of somatosensory evoked potentials in both hemispheres when the nondominant hand is stimulated in patients undergoing (nondominant) hemispherectomy (11).

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In Reply:

We read with interest the letter responding to our article on the Kernohan notch phenomenon (1). As our correspondent notes, we reported the appearance in a young man of marked hemiparesis ipsilateral to a left depressed cranial fracture sustained in a motor vehicle accident. We correlated MRI evidence of injury to the right cerebral peduncle with electrophysiological evidence of dysfunction of the motor system by use of transcranial motor evoked potentials (1). Because there was clear evidence of injury to the right peduncle, we see no reason to postulate that “a different mechanism underlies the weakness ipsilateral to the lesion than … bruising of the right peduncle.” It is just such bruising that has been increasingly documented in cases of the Kernohan notch phenomenon with modern MRI scans (2–8). We have no reason, as our correspondent does, to implicate “abatement of the excitatory influence of the major hemisphere on the minor.”

In a follow-up on our patient, he did have initial severe left hemiparesis as well as language dysfunction (dysarthria and expressive > receptive dysphasia), but after an intensive course of physical therapy and speech therapy, he has no lasting motor deficits, and his speech is much clearer and more understandable. Cognitively, he has also come a long way, just now graduating from college with his classmates. Thus, in addition to the documentation of Kernohan’s phenomenon, to us, one of the key “messages” from this case is the demonstration of the potential of neurorehabilitation and neuronal plasticity and repair in patients with traumatic brain injury.

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Duration of Cognitive Impairment after Sports Concussion

To the Editor:

Dr. Maroon’s commentary (6) on our article (1) raises three basic concerns: that the Automated Neuropsychological Assessment Metrics (ANAM) is inadequately sensitive to concussion; that ANAM has practice effects, whereas other computerized batteries do not; and that “the authors have not assessed the symptom status of athletes” (6, p 1079). Dr. Maroon states that other test batteries do not share these problems, and his references indicate that he is referring to the Immediate Post-concussion Assessment and Cognitive Testing (ImPACT) battery. We believe that ANAM, ImPACT, and other well-designed computerized assessment procedures share strengths and weaknesses to a far greater degree than suggested by Dr. Maroon.

A recent article by Lovell et al. (4) is illustrative. ImPACT yields three cognitive scores: “memory,” “processing speed,” and “reaction time.” The study by Lovell et al. found a postconcussion decrement, compared with baseline scores, on the memory measure but statistically significant improvements on the other two measures of ImPACT, reporting that concussed subjects showed “significantly faster reaction time at 6 days postinjury than at baseline (P < 0.007) and at 36 hours postinjury (P < 0.004)” (4), and “significantly faster processing speed at 6 days postinjury than at baseline (P < 0.002) and at 36 hours postinjury (P < 0.001)” (4). Thus, only one of ImPACT’s three measures was sensitive to concussion at 36 hours after injury.

Furthermore, regarding the finding that ImPACT’s other two measures showed significant amounts of improvement, these authors wrote: “This may represent a practice effect associated with the repeated evaluation of athletes over a relatively brief period of time” (4). Practice effects also have been reported for computerized neuropsychological batteries other than ImPACT (3).

In another article, published on the ImPACT web site and written by Dr. Maroon (5), data are presented comparing a group of 143 concussed athletes with a control group of 25 uninjured athletes. Figures C2 to C4 are from that article and
compare the two groups at baseline and at 2, 5, and 8 days after injury.

In Figure C2 (lower scores represent faster, or better, performance) and in Figure C3 (higher scores represent faster, or better, performance), the statistically significant differences between groups are a result of the control group’s showing improvement in performance (practice effect), whereas the concussed group does not. Indeed, two of three of the above recovery curves, based on ImPACT scores, are strikingly similar to the recovery curves, based on ANAM scores, presented in Bleiberg et al. (1).

Dr. Maroon states, “even more concerning is that the authors have not assessed the symptom status of athletes.” However, Bleiberg et al. (1) state that a previously published concussion management protocol was used in the clinical management of all subjects, and they provide the reference to that protocol (2). Review of the referenced protocol will confirm that it emphasizes comprehensive and frequent symptom assessments.

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Altered Arterial Homeostasis and Cerebral Aneurysms: A Review of the Literature and Justification for a Search of Molecular Biomarkers

To the Editor:

Kassam et al. (3) have presented a comprehensive review of research on the biomolecular markers of cerebral aneurysms. However, the authors are incorrect in stating that paxillin is an integrin (3, p 1208).

Paxillin is a 68-kD protein that belongs to the family of focal adhesion adaptor proteins (5). These molecules are found in a
subcellular location, usually at sites of adhesion between the cell and the extracellular matrix (focal adhesions). Paxillin has multiple protein binding sites that enable it to act as molecular docking platforms involved in the recruitment and interaction of a variety of intracellular and transcellular molecules. With the exception of the $\alpha_2$-integrin subunit, integrin-paxillin interaction is probably indirect (5).

Unlike paxillin, integrins are transmembrane glycoprotein heterodimers composed of $\alpha$ and $\beta$ subunits (2). Binding to the appropriate extracellular matrix ligand leads to integrin clustering. This, in turn, results in the recruitment of a variety of subcellular molecules (including paxillin) and the initiation of intracellular signaling cascades that cause the reorganization of the cellular cytoskeleton and/or gene transcription. Although described as cell adhesion molecules, integrins have been shown to be involved in many biological processes, including wound repair, tumor angiogenesis, nerve regeneration, and motoneuron survival (4).

A recent article reports that the expression of $\alpha_2\beta_1$ integrins is significantly decreased in the walls of abdominal aortic aneurysms (1). The authors suggest that the relative decrease in integrin expression within the tunica media predisposes to aneurysms (1). The authors state that an independent experienced spine surgeon reviewed the patients' postoperative plain x-rays for documenting bone trabeculation between adjacent vertebral and also absence of motion in flexion-extension views. Tuli et al. (3), in their recent article, found statistically significant variations of fusion rates on the basis of the same radiographic interobservational criteria that were used by Balabhadra et al. A spine surgeon’s subjective evaluation of plain x-rays might be different from another spine surgeon's evaluation. It would definitely be advantageous to use a more elaborate grading system of the applied “bone trabeculation” criterion or even the use of a somewhat quantitative evaluation such as “complete” versus “partial” bone trabeculation or evaluation of bone trabeculation of the rostral and caudal ends of the allograft.

The use of computed tomographic (CT) scans, with all the limiting factors of cost and higher irradiation, might be necessary, especially in studies evaluating the effectiveness of allografts, which in most cases add significantly to the cost of this widely performed spinal procedure (anterior cervical fusion).

Another interesting point in this article is related to the use of a plating system in single-level anterior cervical fusions. The use of plates in instability in single-level cases has been widely accepted; however, the cost-effectiveness of their generalized use for cervical disc disease has not been established (1). A question that arises is the one regarding the use of the cancellous allograft spacer (Graftech; Osteotech, Inc., Eatontown, NJ) without a plating system. I was wondering whether the authors had any experience in the use of Graftech without a plate in single-level anterior cervical fusion in cases of cervical disc disease.

Finally, the observed subsidence varied between 1 and 4 mm postoperatively. Have the authors any detailed demographic data regarding the presence of any concomitant microvascular disease or significant osteopenia among the patients included in their series? It would be helpful to have this information, if available, for exploring the possibility of further settling in special groups of patients, such as smokers, the elderly, or female patients.

Kostas N. Fountas
Macon, Georgia

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Anterior Cervical Fusion Using Dense Cancellous Allografts and Dynamic Plating

To the Editor:

I read with great interest the well-designed and thorough retrospective study published by Balabhadra et al. (2). The article aimed at evaluating the efficacy of the dense cancellous allograft as a substrate for anterior cervical fusion along with instrumentation. Among other parameters, the authors examined the “fusion” rate achieved and the occurrence of postoperative subsidence in a very detailed and well-documented manner.

Unfortunately, the definition of fusion has remained quite controversial, although the term has become meritorious in the spine literature; fusion has been used for comparing the effectiveness of different graft substitutes (as in this article), various implant types, and even surgical techniques. Precise radiographic criteria for defining fusion have not been universally accepted. The authors stated that an independent experienced spine surgeon reviewed the patients’ postoperative plain x-rays for documenting bone trabeculation between adjacent vertebral and also absence of motion in flexion-extension views. Tuli et al. (3), in their recent article, found statistically significant variations of fusion rates on the basis of the same radiographic interobservational criteria that were used by Balabhadra et al. A spine surgeon’s subjective evaluation of plain x-rays might be different from another spine surgeon’s evaluation. It would definitely be advantageous to use a more elaborate grading system of the applied “bone trabeculation” criterion or even the use of a somewhat quantitative evaluation such as “complete” versus “partial” bone trabeculation or evaluation of bone trabeculation of the rostral and caudal ends of the allograft.

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Finally, the observed subsidence varied between 1 and 4 mm postoperatively. Have the authors any detailed demographic data regarding the presence of any concomitant microvascular disease or significant osteopenia among the patients included in their series? It would be helpful to have this information, if available, for exploring the possibility of further settling in special groups of patients, such as smokers, the elderly, or female patients.

Kostas N. Fountas
Macon, Georgia

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In Reply:

We thank Dr. Fountas for his comments regarding our retrospective study (1). Dr. Fountas’s comments revolve around the radiographic evaluation of a cervical fusion and factors that assist or inhibit this fusion process.

Unfortunately, we agree that there is no “gold standard” in the radiographic assessment of a fusion. Certainly, the presence of trabeculae, the absence of lucency adjacent to the graft, and the lack of motion on flexion-extension radiographs have been used in the past (2, 4–7, 9, 10, 12, 15). Conversely, we agree that other methods for the evaluation of a fusion process exist, such as CT scans, MRI, and radioisotope uptake studies (7, 10). Before subjecting our patients to these tests, a better assessment of those tests must be performed to prove their benefit over the current methodology.

Without a doubt, many factors influence a cervical fusion, the amount of graft subsidence, and the amount of graft telescoping. The use of a cervical plate, a history of tobacco use, a history of nonsteroid use, and fusion at an adjacent level only touch upon the vast array of factors that influence this fusion process. Although it has been our practice to plate all cervical fusions, it would be of interest to evaluate other factors that contribute to this process. Although the amount of graft subsidence and/or graft telescoping must depend on the density of the graft versus the density of the vertebral bone (at the graft-bone interface), it would be of interest to evaluate the use of tobacco or the presence of osteopenia and their effect on subsidence (3, 8, 11, 13, 14).

Max C. Lee
Daniel H. Kim
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Historical Background of Stereotactic Surgery: Reflections on Stereotactic Surgery and the Introduction of Microelectrode Recording in Montreal

To the Editor:

In the legend to Figure 1 of the article by Hardy (2), “Photograph of Jules Hardy (right) and Gerard Guiot (left) performing a stereotaxic functional procedure at Hôpital Foch in Paris, 1961,” there is a mistake; the correct term is “stereotactic,” not “stereotoxic.”

The word “stereotoxic” is derived from the Greek stereos, meaning solid or three-dimensional, and taxis, meaning an arrangement (as in “taxonomy”). Efforts are being made to encourage compliance from all journals and publishers to use the term stereotaxic when applied to human stereotactic surgery, and not stereotoxic, which is the one still used for animal techniques. The publication of the World Society for Stereotactic and Functional Neurosurgery is now called Stereotactic and Functional Neurosurgery (1).

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Temporal Lobotomy in the Surgical Management of Epilepsy: Technical Report

To the Editor:

I refer to the recent article by Smith et al. (4). Temporal lobe resections have become common for epilepsy and tumors, making this article timely and important. Some minor concerns are raised below.

The authors state that all patients underwent preoperative Wada testing. The necessity of this point must be emphasized. In the University of Washington experience with 863 Wada tests for epilepsy patients, handedness was only a rough predictor of language dominance (1). Specifically, 92.8% of right-handers were left-dominant, 2.3% were right-dominant, and 6.7% had bilateral language function. Left-handers were even more difficult to predict on handedness alone, with 54.2% left-dominant, 31% right-dominant, and 14.8% bilateral.

The authors state that they “prefer to perform language mapping” (4, p 1532). When dominant resections are performed without mapping, they suggest a cortical incision 3 to 3.5 cm from the temporal pole. Although this is usually safe, the reported experience with mapping 117 patients (all right-handed, all left-dominant) indicates that the anterior 3 cm of the dominant superior temporal gyrus was found by cortical stimulation mapping to contain an essential language site in 18.4% and in the middle temporal gyrus in 8.3% (4). Therefore, when language mapping is not used for temporal resections in the dominant temporal lobe, complete sparing of the superior temporal gyrus and minimal resection of the middle temporal gyrus is advisable. This can be accomplished by angling the lateral cortical incision so that less of the superior temporal gyrus and middle temporal gyrus is resected and more of the inferior and basal temporal gyri is removed.

The authors state that “subpial dissection and aspiration are used to remove the superior amygdala” (4, pp 1533–1534). This simply is not possible. The amygdala arises from the anlage of the caudate/putamen, then descends through the anterior temporal stem. The basal ventral aspect of the amygdala (using a line drawn from the middle cerebral artery to the choroidal point in the temporal horn) is removed (3). This strategy avoids temporal stem injury. Temporal stem injury can also be limited by entering the temporal horn at its most lateral aspect, rather than superiorly, as shown in this technical report (Fig. 2, inset).

Finally, the authors use a microspatula and suction to perform subpial resection. I have found that an ultrasonic aspirator affords significantly better control, decreasing the risk of injury to structures on the other side of the pia (2).

In Reply:

We will answer Dr. Silbergeld’s comments in the order in which they are presented. We are in accord with his comments about the importance of preoperative Wada testing. We think that intracarotid amytal testing is necessary both for lateralization of language and for recent memory function.

Dr. Silbergeld notes the potential risk of postoperative language deficits related to resection of more anterior portions of the superior and middle temporal gyri. We did not observe this in any of the 10 patients described in our communication. However, we have on several occasions seen prolonged dysnomia in anterior temporal lobectomy cases in which only the anterior 3 to 3.5 cm of the superior and middle temporal gyri were included in the resection. His solution to this problem seems a reasonable one. In the future, preoperative language mapping with MRI or magnetoencephalography may safely guide such resections when performed under general anesthesia. Currently, only intraoperative or extraoperative language mapping (with implanted subdural grids) can adequately define the location of the language cortex.

We would agree with Dr. Silbergeld that complete disconnection or resection of the superior amygdala is not possible. Referring to this as disconnection (or resection) of the more superior portions of the amygdala would have been more accurate. The only component of our disconnection (or resection) of the amygdala that is subpial involves the use of that technique to dissect medially until we contact pia, thereby ensuring disconnection. Also, in the case of removal of amygdala, only the most medial component of the en bloc resection involves subpial dissection/aspiration. We have not noted any neural or vascular injuries related to this technique (1). We thank Dr. Silbergeld for his worthwhile and constructive comments.

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Daniel L. Silbergeld
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Pathobiology of Human Cerebrovascular Malformations: Basic Mechanisms and Clinical Relevance

To the Editor:

Radiosurgery, invented by Professor Lars Leksell (4), has become a successful treatment modality in the management of cerebral arteriovenous malformations (AVMs) during the past 4 decades. It operates by use of a single high dose of gamma ray or x-ray irradiation to destroy the predetermined target using either the gamma knife or a linear accelerator. AVMs had been regarded as hamartomatous lesions of the brain formed by embryonic maldevelopment (1). The goal of radiosurgery is complete occlusion of pathological vessels in the AVM nidus while preserving normal feeding arteries, thought to be achieved through a thrombo-obliterative process evoked by the ionizing energy of irradiation. The first successful operation of an AVM by radiosurgery using the gamma knife was reported by Steiner et al. in 1972 (9). Since then, it has become an effective and widespread primary or alternative method in the therapy of selected AVMs, especially for those with difficult access for surgery or embolization (5). MRI and angiographic examinations have already well documented that 65 to 87% of AVMs obliterate and 75% shrink in volume 2 to 5 years after radiosurgery. Although almost 40,000 cases of AVM have already been treated worldwide with the gamma knife, the pathophysiological mechanism by which radiosurgery generates progressive vessel obliteration and volume shrinkage is not completely elucidated. Therefore, we read with great interest the topic review by Gault et al. (3).

Analyzing the abnormal angioarchitecture and antigen composition of cerebrovascular malformations, they have concluded that “smoothelin displays mild to moderate expression in the media of some large vessels of AVMs; overall its expression, including that in arterialized veins, is low and significantly less than that observed in normal brain vessels. This finding may reflect the disappearance of the contractile property in vascular smooth muscle cells of AVM vasculature resulting from hemodynamic stress of turbulent blood flow through these lesions.” These observations could supply a few fundamental ideas to the pathological process in AVMs induced by radiosurgery.

The biological effects of ionizing radiation on cerebral arteries are not fully understood, and the basic histopathological process leading to occlusion and volume reduction of the AVM is still open to discussion. It seems that the abnormal vessels of tumors connective tissue stroma and vascular malformations have a relative sensitivity to a single high-dose irradiation compared with normal surrounding vessels. This phenomenon draws attention to the connective tissue stroma of AVMs, because its presence is the most striking difference between the pathological and physiological conditions (7). Cellular mechanisms involved in radiation injury to blood vessels are most likely initiated by damage to the deoxyribonucleic acid. This could trigger phenotypic changes and apoptosis in the endothelium, followed by luminal stenosis and eventual occlusion through subendothelial and perivascular cellular hyperplasia accompanied by increased thrombogenicity. We have published the results of an immunohistochemical and electron microscopic study on human cerebral AVM vessels from surgical pathological material after subtotal obliteration following gamma knife radiosurgery (10). Light microscopy revealed a spindle-shaped cell proliferation in the connective tissue stroma of the AVMs and in the subendothelial region of the vessels. The ultrastructural and immunohistochemical characteristics of these spindle-shaped cells were identical to those described as myofibroblasts in wound-healing processes and pathological fibromatoses (2). Whereas in the nonirradiated control specimens of AVMs, similar cells expressed vimentin and desmin cytoskeletal antigens, in the cases treated with gamma knife surgery, α-smooth muscle actin activity was also observed. In this study, it was suggested that radiosurgery might have stimulated proliferation of myofibroblasts in the stroma and vessel walls of AVMs, and the study raised the potential role of these cells in the obliteration process and shrinkage of the lesion after radiosurgery. A subsequent report demonstrated that gamma knife radiosurgery of AVMs generates endothelial damage and proliferation of intimal spindle-shaped cells in the vessel walls, leading to progressive narrowing and obliteration of the vascular channels (8). To investigate this problem in detail, a tissue culture experiment was performed. Human cerebral AVM tissue had been removed by surgery, prepared with collagenase, and placed into Dulbecco’s modified Eagle’s medium plus F12 tissue culture medium. Five days after surgery, the tissue culture was treated with single high-dose gamma irradiation to simulate radiosurgery circumstances. The irradiation doses were 15, 20, 25, and 50 Gy. On Day 12 of cultivation, immunohistochemical reactions were performed to characterize the phenotypic nature of the cells. Vimentin, desmin, cytokeratin, α-smooth muscle actin, factor VIII, S-100 protein, and glial fibrillary acidic protein antigens were examined. In the control group (cells cultivated without irradiation from the same specimen), vimentin was the most striking cytoplasmic marker. The positivity of this reaction has gradually been decreased in the 15-, 20-, 25-, and 50-Gy irradiated groups. Conversely, α-smooth muscle actin was scant in the nonirradiated control group; however, it became prominent in the irradiated cell population. Other cytoplasmic antigens were not characteristic in the cultured cells (6).

Summarizing the observations on the irradiated AVM tissue cultures, it seems that the results have some similarity to the previous immunohistochemical investigations of the radiosurgery-treated AVMs. Expression of the α-smooth muscle actin in the irradiated cell population suggests phenotypic modification, i.e., transformation of the resting fibroblasts into a more active form with contractile properties, evoked by the ionizing energy of gamma rays. The activated cells expressed vimentin cytoskeletal antigens as well and were similar to those designated as myofibroblasts. The contractile capacity of these proliferating myofibroblasts might contribute to the shrinking process and
final obliteration of the AVM nidus after radiosurgery. Comparing these results with those presented by Gault et al., the two together suggest to us a pathophysiological mechanism by which the ionizing effect of the radiosurgery might restore the lost contractile capacity of pathological AVM vessels.

CyberKnife Frameless Stereotactic Radiosurgery for Spinal Lesions: Clinical Experience in 125 Cases

To the Editor:

In the recent article by Gerszten et al. (1), the authors described their clinical experience in treating a large series of patients with stereotactic radiosurgery for benign and malignant spinal lesions. Although the feasibility of performing such treatment using the CyberKnife technology is not new and had already been published (2, 3), the article is important because it describes the largest series in the literature of treatment with spinal radiosurgery.

The study demonstrated the capability of stereotactic radiosurgery to achieve good results in the treatment of spinal lesions using the CyberKnife frameless technique. The high rates of tumor and pain control showed the effectiveness and safety of the procedure. There was no case of radiation-induced myelopathy and no new neurological deficit in the period immediately after treatment. Moreover, the reported results suggest that stereotactic radiosurgery might have applications for spinal metastasis similar to those that it has for the brain.

Unfortunately, the authors provided misleading information for the readers of Neurosurgery when they compared the technology used by their group with other available radiosurgery techniques. The authors stated that “Stereotactic radiosurgery techniques developed for spinal lesions using standard linear accelerators require the placement of an invasive rigid external frame system directly to the spine, thus limiting their widespread use” (Introduction, Paragraph 3).

This statement is clearly wrong, because modern linear accelerator technology has already proved its capability of performing frameless stereotactic radiosurgery for spinal lesions, without any spinal fixation method. Moreover, a study describing its feasibility, precision, and clinical applications was published before the study by Dr. Gerszten and his group (4).

This technique uses a positioning system (Exac Trac; Brainlab, Heimstetten, Germany) based on the principles of neuronavigation. It consists of two infrared cameras that detect infrared reflective markers that are placed on the patient’s skin. A digital x-ray system in the treatment room is used for patient positioning and monitoring. This is the least invasive system for the patient, because it does not require the implantation of any fiducial markers. CyberKnife is a frameless system that requires the implantation of fiducials for treating lesions in the thoracic and lumbar spine. One of these implants was responsible for the dural tear reported in Dr. Gerszten’s study that required surgical repair.

A considerable number of neurosurgeons in the world do not use radiosurgery in their practices and are not familiar with recent advances in the field, such as spinal radiosurgery. An important publication like Neurosurgery is capable of shaping the opinion of the neurosurgery community about the capabilities of each technology. Although the aim of the article was not to make a comparison between the available technologies, the authors did make such comparison with an incorrect statement, and this should be clarified for the readers of Neurosurgery.

The selected reviewers of the article did not comment on this issue either. Only the last reviewers, Drs. Mark Bilsky and Yoshiya Yamada, mentioned that “Standard linear accelerator-based extracranial radiosurgery has been reported by authors from a number of institutions with a high degree of success,” but they did not mention anything regarding spinal fixation.

Because stereotactic radiosurgery has experienced incredible technical advances in the past few years, it is challenging the description and comparison of new applications and techniques. The authors need to be updated with all these ad-

References
In Reply:

We thank Dr. Frighetto for his interest in our article (5). His point is well taken that in the past several years, there has been an explosion of interest in spinal radiosurgery. Our statement describing linear accelerated-based radiosurgery was not intended to be misleading. The statement in our Introduction was meant to give credit to Dr. Hamilton’s pioneering work in linear accelerator-based spinal stereotactic radiosurgery, which he published in 1995. Our statement regarding the specific limitation of his technique that required an invasive rigid external frame system directly to the spine is what we were referring to when we stated that this problem limited its widespread use. We were speaking from a historical perspective, not referring to current technologies. As Dr. Frighetto states, our article did not intend to serve as a comparison to other spinal radiosurgery techniques. We apologize for any misperception that this statement implied. Our article was submitted in 2003, before the publication of the larger series by other investigators such as Ryu et al. (7) and Bilsky et al. (2). Even since the publication of our own article, others have demonstrated their significant experience with linear accelerator-based spinal radiosurgery (1–4, 6). We look forward to publication by other centers of their own experience and advances to be able to make the appropriate considerations regarding the available techniques in their field.

Leonardo Frighetto
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Endoscopic Third Ventriculostomy in Idiopathic Normal Pressure Hydrocephalus

To the Editor:

Gangemi et al. (1) highlighted an extremely important topic considering the effectiveness of endoscopic third ventriculostomy (ETV) for management of normal-pressure hydrocephalus (NPH). Our own experience with such treatment is based on 28 cases, including both idiopathic NPH (INPH) (9 patients) and nonidiopathic NPH after subarachnoid hemorrhage (10 patients), severe head injury (2 patients), and late-onset aqueductal stenosis (7 patients). One patient initially underwent VPS elsewhere with temporary relief of symptoms, which relapsed after documented shunt obstruction. Both before and after surgery, the patients underwent MRI, CT scans, CT cisternography with Omnipaque, and cine MRI with measurements of transaqueductal CSF flow volume and velocity. Overall, ETV was found to be effective in 24 patients (86%). Measurable clinical improvement was marked in 7 patients (78%) with INPH, 10 patients (83%) with posthemorrhagic and posttraumatic NPH, and all patients with late-onset aqueductal stenosis who presented with NPH syndrome, including those with VPS, in whom successful substitution of the shunt on the third ventricle stoma was performed. There was no postoperative mortality or serious morbidity.

Several lessons were learned. First, in agreement with the authors of the referenced article (1), we wish to emphasize the importance of the so-called “complete third ventriculostomy” with perforation of both the third ventricular floor and Liliequist’s membrane along with basal arachnoid adhesions. In three of four failed cases in our series, incompleteness of the third ventriculostomy was disclosed postoperatively. Second, in our opinion, CT ventriculocisternography with Omnipaque is extremely purposeful for evaluation of the CSF dynamics during the early postoperative period. Third, an external ventricular drain should not be used after ETV for NPH, because it can result in early third ventricular stoma failure. Fourth, the preexisting VPS should not be considered a contraindication for ETV in patients with NPH: we were able to attain successful elimination of the shunt in such cases. Fifth, the patients


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should undergo careful follow-up: relapse of symptoms 9 months after initially successful ETV for INPH was found once.

It might be of interest that in the majority of our patients, cine MRI disclosed significant changes of transaqueductal CSF flow characteristics after surgery (2). Therefore, we believe that the effectiveness of ETV in NPH results from changes of the pulsatile CSF flow dynamics (6). However, it is not clear which manipulation has paramount importance: perforation of the third ventricular floor, which forms a communication between the third ventricle and basal cisterns; perforation of Liliequist’s membrane and basal arachnoid adhesions, which freed the CSF circulation between the infratentorial and supratentorial subarachnoid spaces; or both of these. From another side, improvement of CSF absorptive capacity was reported after ETV for shunt-dependent noncommunicating hydrocephalus (5). Whether this effect also exists in patients with communicating NPH is not yet known.

On the basis of our results as well as data of the referenced (1) and other (3, 4) reports, it can be concluded that ETV is definitely effective in substantial numbers of patients with both idiopathic and nonidiopathic NPH. However, it seems that different groups of patients exist: those who can respond well to both VPS and ETV, those who should be treated by ETV, those who should undergo shunt implantation, and those who have no chance to benefit after any type of surgery. Identification of those individuals in whom a neuroendoscopic procedure will be effective is definitely obscured. Although duration and type of symptoms (1) and resistance to CSF outflow (3) may be important, other prognostic factors need to be evaluated. For example, in our series, we frequently observed ballooning of the third ventricular floor, which has a known association with favorable results of ETV in cases of noncommunicating hydrocephalus; however, the significance of this sign for prediction of success of the procedure in patients with NPH is not known. Therefore, although we agree that a prospective randomized trial of ETV versus VPS in patients with NPH will be needed in the future, for avoidance of erroneous results of such a study, it should be preceded by careful investigations of the prognostic factors in patients with NPH treated by ETV.

Shuji Kamikawa
Mikhail Chernov
Fumitaka Yamane
Tomokatsu Hori
Tokyo, Japan


In Reply:

We thank Dr. Kamikawa and his colleagues for their interest in our article (1). We strongly agree that ETV plays an important role in selected cases of INPH. In fact, we have continued to perform this procedure after the reported series of 25 patients treated up to December 2002; at present, our surgical material includes 40 patients with INPH treated by third ventriculostomy.

We emphasize that both perforation of the third ventricular floor and perforation of the basal arachnoid membrane are essential for a good clinical result. Two of four patients who experienced surgical failure with clinical progression showed insufficient perforation of the basal arachnoid membrane at reoperation; both improved after its fenestration.

We think that ETV results in a decrease of the pulsatile CSF flow and consequent improvement of the cerebral pulse. This may explain why ETV results in clinical improvement primarily in patients with a short history and no or slight mental impairment, in whom there is not yet any significant vascular damage of the periventricular brain regions. Conversely, ETV probably does not improve the CSF absorptive capacity.

We advise preoperative performance of intracranial pressure monitoring. Postoperative MRI flow study is useful primarily in patients who do not improve or worsen, to evidence CSF flow through the stoma. Conversely, other investigations, such as isotope cisternography, CSF infusion test, tap test, and CT ventriculocisternography with Omnipaque, are not necessary both preoperatively and postoperatively.

ETV is definitively effective in a high proportion of patients with INPH (1, 2); however, the preoperative identification of factors that may predict the effectiveness of the neuroendoscopic procedure is not defined. Although the CSF tap test is considered to be useful for selecting patients undergoing a shunt procedure (3), it is not indicated for ETV, in which no CSF is withdrawn.

Our study suggests that surgery is indicated in a large group of patients with INPH who show a short clinical history, prevalent gait disturbances, and no or little mental impairment. Although this group of patients may benefit after both ETV and shunting, ETV must be preferred, because it is less invasive and avoids shunt dependency. Patients who do not respond to ETV and benefit after a successive shunting are a low number of INPH cases (only two in our series). On the contrary, a substantial number of patients do not respond to both procedures; these patients more often show long clinical
evolution and significant mental impairment, suggesting an advanced vascular brain damage. Thus, we suggest an early neuroendoscopic procedure in the first stage of the disease.

Michelangelo Gangemi
Francesco Maiuri
Simona Buonamassa
Giuseppe Colella
Enrico de Divitiis
Naples, Italy


Bleeding after Radiosurgery for Cerebral Arteriovenous Malformations

To the Editor:

This very large series of 756 patients treated with linear accelerator radiosurgery (8) demonstrated that the risk of hemorrhage after radiosurgery was affected in the expected way from AVM coverage, the minimum dose delivered to the AVM, and the presence of aneurysms. This is consistent with the previous experience of other authors (including excellent work previously reported by these authors) and is not surprising, because these factors predict the adequacy of treatment and the risk of future hemorrhage (2, 5, 6, 11, 13). However, in the population treated, the overall results of 51 patients experiencing at least one postradiosurgery hemorrhage were disappointing with respect to altering the natural history of these cases. With an annual hemorrhage rate of between 3.07 and 3.31% for the first 5 years after treatment, the results suggest that the benefits of this very effective treatment have been masked by inappropriate referral, given the lack of reduction in hemorrhage rate compared with the natural history of AVMs (1, 9, 10). Of considerable concern are the disappointing results experienced by 18 of the 44 patients classified with Spetzler-Martin Grade I or II AVMs. With a very low likelihood that these patients will have problems from surgery, an explanation is required as to the selection process that finds these patients to be treated with radiosurgery (3, 4, 7). Furthermore, if the entire lesion cannot be covered, it is questionable whether patients should be offered this form of treatment; surgeons would not countenance a partial AVM removal as effective treatment, nor should radiosurgeons (12).

Michael Kerin Morgan
Sydney, New South Wales, Australia


In Reply:

We thank you for the comments on our article (12). The natural history of cerebral AVMs is not the same as the history after treatment. This and other studies have shown that radiosurgery is an effective treatment but one that cannot fully protect against hemorrhage until total cure (3, 6, 7, 16, 19). Obviously, radiosurgery may modify natural history because of anatomic and hemodynamic changes of the AVM, and the results of our study are valid for the whole AVM population, i.e., hemorrhage risk seems to be constant. But as we demonstrated in previous articles (13, 14), the risk of hemorrhage of a given AVM is not necessarily the same as the global risk for an AVM population. The study of the angioanatomy and architecture of a given AVM have led us to evaluate this risk more accurately and more specifically for this particular AVM (8, 9, 11, 13, 14, 21). Structural changes of the AVM after radiosurgery can follow different paths before a cure is at-

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tained. If the first compartment to decrease is at low risk for hemorrhage, leaving in place high-risk compartments, one may imagine that the likelihood of the hemorrhage after treatment but before cure is higher. When this is associated with an incomplete coverage, hemorrhage risk may be increased because of a longer delay to obliteration. To summarize, a high-risk AVM that takes a long time to be cured is most likely to bleed after radiosurgery but before cure.

Concerning coverage, this consideration is not sufficient to explain lack of obliteration, because minimal dose is the main parameter associated with cure of an AVM (2, 17, 20). An AVM may be incompletely covered but may receive a sufficient dose, while part of it receives a lower dose than the rest of the nidus. Flickinger et al. (2) showed that the dose given to the AVM influences the outcome and the obliteration rates. Lower doses lead to lower obliteration rates, but these rates were not zero. For example, an AVM close to the optic chiasm cannot safely receive a sufficient minimal dose to the whole nidus and must be incompletely covered by the reference isodose. However, this strategy may lead to the complete obliteration of such an AVM anyway.

Finally, where Spetzler-Martin Grade I and II AVMs are concerned, although some series demonstrate excellent results, no mortality, and low morbidity rates (4, 15, 18), other series find higher rates of both mortality and neurological deficits (1, 5, 10). In our opinion, these arguments are not sufficient to propose surgery for Grade I and II AVMs. A few years ago, we considered that a patient with an AVM without any previous hemorrhage was not a candidate for surgery because the risk of surgery did not seem justified in the case of such an innocuous lesion. Our opinion has gradually evolved as a result of further study of the anatomic characteristics and angioarchitecture of AVMs. We now integrate these data when deciding on a therapeutic strategy. A treatment with delayed effects is not likely to be proposed for an AVM with unfavorable risk factors (intranidal aneurysm, venous stenosis or reflux, deep exclusive venous drainage). We prefer rapidly effective treatment modalities of high-risk compartments of the AVM, such as embolization followed by radiosurgery or even surgery. Moreover, a distinction must be made between different Spetzler-Martin Grade II AVMs. By definition, Grade II may imply an AVM less than 30 mm in eloquent area or with deep drainage, or a larger AVM between 30 and 60 mm in a nonfunctional area with superficial drainage. Although surgical outcome seems to be equal for all three types of AVM, each type cannot be treated by radiosurgery with an equal chance of success. Moreover, in our opinion, deeply located AVMs give rise to more morbidity than superficially situated ones. Thus, radiosurgery is more likely to be proposed for them. The patient’s choice is also a very important factor for these AVMs, for which different treatment options can be proposed without evidence of the superiority of one over the others. Many patients prefer to avoid craniotomy and choose radiosurgery because of anticipated risks of surgery and because they can quickly return to work. However, if they opt for radiosurgery, they also accept the hypothetical risk of bleeding before obliteration of the AVM in addition to radioinduced parenchymal changes associated or unassociated with a neurological deficit. In our series, morbidity related to permanent neurological deficits caused by hemorrhage after radiosurgery is 26.2% per hemorrhage. Because the actuarial risk of hemorrhage after radiosurgery ranges from 1.86 to 3.87% per year, risk of permanent neurological deficit can be evaluated as 0.5 to 1% per year. It is thus not always easy to strike a balance between the different therapeutic options available for such AVMs, and the commentary focuses on this ongoing controversy, which still remains a matter of debate.

François Nataf
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To the Editor:

Volumetric Assessment of Glioma Removal by Intraoperative High-field Magnetic Resonance Imaging

To the Editor:

We read, with great interest, the article by Nimsky et al. regarding the use of a high-field intraoperative MRI procedure in the resection of intracranial gliomas (7). We enjoyed this well-presented clinical study. We agree with the authors’ concept that a high-field MRI unit can become part of a cybernetic loop, which will eventually enable the performing neurosurgeon to have real-time feedback and become more aggressive in safely resecting gliomas.

We have been following the innovative work of Professor Fahlbusch and his team on proton magnetic resonance spectroscopy (MRS) analysis of intracranial tumors. The authors, in their present communication, briefly discuss the role of intraoperative MRS; we would like to know whether they have incorporated any spectrographic data into their presurgical planning. Have they found any “normal”-appearing areas on the conventional MRI scan, which were resected on the basis of their abnormal spectrum? Was the spectrographic “abnormality” confirmed by the histopathological examination? If this was the case, did the authors use only choline concentration changes, as they implied in their article, or the whole metabolic profile of gliomas, as has been described previously (1–3, 5, 6)? It would be of great interest whether the authors noticed any differences between the preoperatively and the intraoperatively obtained spectra of the same tumor. It is apparent that such a modality would be a breakthrough in the aggressive resection of the tumor “borders” and could guide the surgeon in resecting normal-appearing but abnormal areas in cases of newly diagnosed high-grade gliomas but also in recurrent glioblastoma resections, because proton MRS has shown promising results in differentiating between tumor recurrence and postradiation necrosis (4, 8–10).

The authors also reported that in two patients in their series harboring infiltrating gliomas in the motor strip area, they achieved a quite satisfactory functional outcome. In the resection of these tumors, did they use any cortical stimulation studies, or did they base their resection solely on the imaging studies? Moreover, in the reported case of the tumor adjacent to the speech area, was intraoperative diffusion tensor imaging helpful in avoiding any postoperative dysphasia? We would be grateful to the authors if they could provide us with some information regarding the anatomic location of the tumors included in their study.

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Trigeminal Neuralgia Caused by Venous Compression

To the Editor:

It was helpful for Matsushima et al. (1) to call attention to venous compression of the trigeminal nerve as a cause of trigeminal neuralgia. I had a case that confirms this phenomenon in a possibly unique way. In 1992, I performed a neurosurgical decompression on a 49-year-old woman who had failed two radiofrequency lesion attempts elsewhere. Surgery disclosed an arterial loop grooving the nerve, and this was displaced by a Teflon felt with small metal clips as x-ray markers. A petrosal vein near the nerve had to be coagulated and divided for access. Her second-division pain was totally relieved but recurred explosively 8 months later. X-rays showed that the felt had not migrated. On reexploration, “a new vein was identified immediately subjacent to the nerve root at the nerve root entry zone . . . [with] a dilation just at the point where it compressed the fifth root.” I coagulated and sectioned this vein and also partially sectioned the nerve as planned preoperatively. Again, her pain was totally relieved during an 18-month follow-up.

Harold A. Wilkinson
Wellesley, Massachusetts

The authors also reported that in two patients in their series harboring infiltrating gliomas in the motor strip area, they achieved a quite satisfactory functional outcome. In the resection of these tumors, did they use any cortical stimulation studies, or did they base their resection solely on the imaging studies? Moreover, in the reported case of the tumor adjacent to the speech area, was intraoperative diffusion tensor imaging helpful in avoiding any postoperative dysphasia? We would be grateful to the authors if they could provide us with some information regarding the anatomic location of the tumors included in their study.


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Volumetric Assessment of Glioma Removal by Intraoperative High-field Magnetic Resonance Imaging

To the Editor:

We read, with great interest, the article by Nimsky et al. regarding the use of a high-field intraoperative MRI procedure...
In Reply:

We thank Fountas et al. for their thoughtful comments on our article (2), which concentrates mainly on proton MRS (7) and to some extent on diffusion tensor imaging, although the published article in this journal is about volumetric assessment of glioma removal. We fully agree that MRS and diffusion tensor imaging are important recent additions in the management of gliomas. It is essential to combine intraoperative imaging with navigational guidance, which should not be restricted to anatomic data only. We established the concept of functional neuronavigation with integration of data from functional MRI and magnetoencephalography for identification of eloquent cortical brain areas, allowing extended resections with low morbidity (1, 5). In the patients with lesions near eloquent cortical brain areas, we applied functional navigation, which was confirmed by phase reversal for lesions in the motor area.

Recently, we extended the concept of functional neuronavigation to multimodality navigation, integrating metabolic maps from MRS and localizations of major white matter tracts, such as the pyramidal tract, identified by diffusion tensor imaging. Integrating MRS metabolic maps into threedimensional navigational data sets allows a precise spatial correlation of metabolic maps and histology in biopsies of the tumor border zone (7). We think that MRS will contribute to a better delineation of the tumor infiltration zone, because our results showed that the tumor areas defined by the metabolic maps exceeded the T2-weighted signal change (6). Up to now, however, artifacts have prevented a reliable evaluation of intraoperative MRS at the immediate resection border. For identification of major white matter tracts, however, even intraoperative imaging is possible. Intraoperative high-field MRI allows a visualization of shifting major white matter tracts during surgery (3, 4). These data can also be integrated into a navigational setup. We have applied this method for the pyramidal tract, and we agree that this would also be an interesting approach for the white matter tracts connecting the various speech-related areas. The first results applying a paradigm based on peripheral electrical stimulation show that even intraoperative functional MRI seems to be feasible. Thus, not only can brain shift be compensated for by intraoperative anatomic data, but also, functional data can be updated.

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