**History of Endovascular Surgery: Personal Accounts of the Evolution**

To the Editor:

I attentively read the “Endovascular Neurosurgery” supplement to the November 2006 issue of *Neurosurgery*. Regarding the endovascular treatment of brain aneurysms with the Guglielmi detachable coiling (GDC) technique, I noticed that in two reports (2, 3) the role of electrothrombosis was considered to be null.

I would like to clarify that effective electrothrombosis did not even come close to the GDC technique. In fact, the amount of electricity that was delivered (1 mA) was not enough to generate a sufficient electrothrombotic phenomenon. The mass of the thrombus is directly proportional to the amount of electrical current. Chen, Ji, and Guglielmi performed a series of in vitro experiments on heparinized blood (unpublished data). They confirmed that the amount of thrombus is directly proportional to the electrical current. The following weights of thrombus were formed when the given current was applied for 3 minutes, using a platinum electrode: at 1 mA, mass was 10 mg; at 2 mA, mass was 12 mg; at 3 mA, mass was 26 mg; and at 10 mA, mass was 85 mg.

Past researchers (1) have almost always used 10 mA to produce sufficient electrothrombosis in both the experimental and clinical settings. From the data of the literature, we can say that when applying 1 mA of current, the mass of the (electro)thrombus is almost insignificant.

Despite the reassuring experimental and clinical data of the literature, we did not increase the electrical current from 1 to 10 mA. Whether an increase of the electrical current would lead to improved results remains unknown.

**Guido Guglielmi**

*Rome, Italy*

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4. Moore CH, Murchison C: On a new method of procuring the consolidation of fibrin in certain incurable aneurisms with the report of a case in which an aneurism of the ascending aorta was treated by the insertion of wire. *Proc R Soc Lond* 4:327–335, 1864.

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In Reply:

I would like to echo Prof. Guglielmi’s brief, specific discussion of the role of electrothrombosis in the Guglielmi detachable coil (GDC) technique of treating intracranial aneurysms (2). The historical reviews published in the Endovascular Neurosurgery supplement of *Neurosurgery* in November of 2006 support Prof. Guglielmi’s contention that the role of electrothrombosis, as it would occur in the present methods of electrolytically detaching the coils, is negligible (6, 7).

There is a significant and interesting body of literature describing the phenomenon of electrothrombosis and its potential clinical applications, beginning with Moore and Murchison in 1864, who introduced a wire to elicit thrombosis of an aneurysm of the ascending aorta (4). The interest in better understanding this phenomenon has not waned over 140 years, culminating in the use of electrolytic detachment for GDC coils.

Prof. Guglielmi presents interesting, previously unpublished data regarding the potential for generating significant amounts of thrombus by varying current through a platinum electrode (2). Although Prof. Guglielmi’s initial experiments and clinical trials with GDCs in part used these data to determine the initial currents for the endosaccular treatment of aneurysms (10 mA), present and standard clinical practice restricts the current for detachment to 1 mA (1). Additional modifications of the coil now restrict the current to the zone of detachment by effectively isolating, and thus isolating the coil from the current, resulting in only a small zone (less than 1 cm) where the phenomenon of electrothrombosis can occur (3, 5).

Would enhancing the electrothrombotic phenomenon in the endovascular treatment of intracranial aneurysms improve long-term occlusion rates? At present, this answer remains unknown.

I would like to thank Prof. Guglielmi for sharing his unpublished data with the readership of *Neurosurgery* and helping to shed more light onto the already fascinating history of aneurysm therapy.

**Charles J. Prestigiacomo**

*Newark, New Jersey*
The Role of Electrothrombosis in the GDC Technique

To the Editor:

The idea of having electrothrombosis as an effective mechanism for aneurysm occlusion was in the mind of many of us in the 1980s. In 1983, I was offered a device for intravasal electrothrombosis produced by a French company for testing in the animal lab. It turned out that with low electrical current, the production or occurrence of thrombus was null or minimal, whereas with higher current, electrothrombosis and also gas bubbles could be produced. This device never came near to any clinical application. In my article, I wanted to express that after the Guglielmi detachable coil system was announced as an electrothrombotic device, it shortly became clear that the mechanism of aneurysm occlusion was not that of immediate or early electrothrombosis. The reason for this has been detailed by Guido Guglielmi in his letter (1). It is interesting to read that an increase of the current applied for detachment could eventually lead to improved treatment results.

Bernd Richling
Salzburg, Austria


10.1227/01.NEU.0000296974.16170.70

Hypertension, Age, and Location Predict Rupture of Small Intracranial Aneurysms

To the Editor:

The clinical study reported by Nahed et al. (1) was important in documenting the significance of hypertension and patient age as predictors for rupture of intracranial aneurysms of 7 mm or smaller. However, I was surprised that neither the authors nor the commentators questioned the location of the aneurysm other than simply “anterior” or “posterior.” It has certainly been my experience that aneurysms that arise from the distal anterior cerebral artery commonly rupture when they are small, and commonly they are less than 7 mm in size. Mycotic aneurysms also often arise from small distal arterial branches, and they also not uncommonly rupture when very small; however, there was no mention of whether this type of aneurysm was excluded from the analysis.

Harold A. Wilkinson, M.D., Ph.D.
Boston, Massachusetts


10.1227/01.NEU.0000296965.06761.60

Mapping of Visuospatial Functions during Brain Surgery: A New Tool to Prevent Unilateral Spatial Neglect

To the Editor:

In the last decade, major advances in intraoperative functional mapping have dramatically improved the results of brain surgery. When removing a brain tumor, it is common clinical practice to awaken the patient and temporarily inactivate small (approximately 5 mm) brain regions with electrical stimuli, while the patient performs functional tasks. If the patient produces incorrect responses, the surgeon leaves the region intact, to preserve the patient’s functional abilities (3). This procedure allows the surgeon to minimize the residual morbidity while increasing the quality of the resection, and the net result is to improve overall patient survival. However, although sensorimotor and cognitive functions such as language, memory, or calculation have been extensively mapped intraoperatively, visuospatial functions have received less attention.

Unilateral spatial neglect (USN) is a dramatic neurological condition resulting from damage affecting the temporoparietal or frontal cortex, or the thalamus or basal ganglia of the right hemisphere (1). Patients with unilateral spatial neglect behave as if the left part of the world does not exist (2). Their functional recovery is poor, and they endure major clinical and social consequences. USN typically results from vascular stroke, but other etiologies are possible. For example, USN was reported after a patient received right inferior parietal cortectomy for intractable epilepsy (4). Although this particular patient recovered from neglect after 2 years, other neurosurgical patients risk suffering from chronic USN as a result of more extensive, corticosubcortical resections, especially as required for cerebral glioma. In a recent series of eight patients who underwent resection of a glioma confined to the right parietal lobe (5), no new USN was clinically observed postoperatively, but there was no formal neuropsychological testing performed, and it is well known that USN may not be apparent at clinical observation (2). To prevent the occurrence of USN in these patients, we propose intraoperative assessment of visuospatial function by asking patients to bisect 20-cm horizontal lines (Fig. 1). If the patient shifts the subjective center more than approximately 6.5 mm rightward (1), then the neurosurgeon leaves the inactivated area untouched. Using this procedure, we were able to avoid postoperative USN in two patients (ages, 27 and 28 years) who underwent resection of low-grade gliomas in the right temporoparietal region (6). These left-handed patients were awakened during surgery because there was functional magnetic resonance imaging evidence of partial language representation in the right hemisphere. Patients gave their informed consent.

FIGURE 1. Line bisection tasks performed during brain surgery are simple and feasible. Under test conditions, a 20-cm line (not shown) is presented, aligned to the subjects’ eye axis, in a central position with respect to the patient’s sagittal head plane.
and the procedure followed the guidelines of the Ethical Committee of the Hôtel-Dieu Hospital in Paris. On line bisection, patients significantly deviated rightward upon inactivation of the supramarginal gyrus and the caudal portion of the superior temporal gyrus. In one of the patients (SB), subcortical inactivation of a parietofrontal pathway, the superior occipitofrontal fasciculus, brought about the most dramatic deviations (mean, 26.13 mm; standard deviation, 16.93 mm). These regions were accordingly spared by the resection. The day after surgery, the patient had left-inferior quadrantanopia on confrontation test and showed signs of left neglect and optic ataxia with his left, contralesional hand. When bisecting 20-cm lines, he erred rightward by 24.38 mm on average (standard deviation, 25.28 mm), and sometimes missed the line altogether, which had never occurred before or during surgery, thus showing left neglect on line bisection. On a target-cancellation task, he failed to cancel most targets on the left half of the display, particularly in the left superior quadrant of the test sheet. When copying a drawing of a landscape composed of a house with two trees on each side, he used the rightmost third of the sheet to copy the left part of the house, omitted to copy the left-sided trees, and tried to copy the right half of the house and the right-sided trees on the table, beyond the right margin of the test sheet. These signs gradually subsided during subsequent days, suggesting their dependence on perilesional edema. Five days after surgery, the patient bisected 20-cm lines accurately (mean displacement, 0.82 mm toward the left; standard deviation, 8.28 mm), and had no signs of optic ataxia or neglect on target-cancellation or drawing tests. The absence of neglect signs was confirmed at follow-up testing 51 days after surgery. Another patient (CAL) did not demonstrate any signs of neglect when tested on the paper-and-pencil battery 114 days after surgery. Visuospatial functions should be assessed systematically during surgery involving the temporoparietal region, even when language is not concerned, by using the simple, safe, well-tolerated, and cost-effective procedure of line bisection. Such a strategy can prevent postoperative USN, and consequently allow patients to resume the tasks of a normal socioprofessional life, such as driving.

Paolo Bartolomeo
Michel Thiebaut de Schotten
Hugues Duffau
Paris, France

Minimally Invasive Atlantoaxial Fixation with a Polyaxial Screw-rod Construct: Technical Case Report

To the Editor:

We read the article by Joseffer et al. (2) with great interest. The work presents significant advancement in the treatment of craniovertebral junction instability problems by means of minimally invasive intervention. This technique protects the midline structures effectively and makes additional soft-tissue dissection unnecessary. However, some atlantoaxial rotary fixation cases present a challenging situation regarding the internal reduction procedure, even if a C1–C2 polyaxial rod construct is used. The screw insertion into the lateral masses of the atlas and pedicles of the axis can alone reduce the dislocation to some degree in these particular patients. If additional reduction is needed, more rotational force must be applied. For this purpose, midline structures such as the posterior arch of the atlas and/or the spinous process of the axis yield a desired lever function for necessary maneuvers, as we presented in our technical note (1). We believe that with minor modification of the tools, comparable manipulation can be provided in the technique described by Joseffer et al.

Deniz Belen
Serkan Simsek
Ankara, Turkey

Preserving the Ligamentum Flavum in Lumbar Discectomy: A New Technique that Prevents Scar Tissue Formation in the First 6 Months Post surgery

To the Editor:

We read with interest the article by Ozer et al. (8). The authors’ work is interesting and praiseworthy in the sense that this is a prospective study on the evaluation of the efficacy of


When the Bone Flap Hits the Floor

To the Editor: I read the article of Jankowitz and Kondziolka (1) with great interest. The authors reported that sterilization of the bone flap, which had a short time exposure to the operating room floor, was accomplished by soaking it in Betadine solution (Purdue Pharma, Stamford, CT) with or without additional immersion in antibiotic solution, and that this is sufficient for prevention of infection. This technique is definitely superior, compared with others, because of its effectiveness, simplicity, and speed, and it provides an excellent opportunity for using autogenous bone with an optimal cosmetic result.

Nevertheless, I could not accept the authors’ criticisms considering other possible ways of bone-flap sterilization, namely, boiling and autoclaving. In fact, during my work at Russian Folenov Neurosurgical Institute (St. Petersburg, Russia) these methods were used routinely for delayed cranioplasty with autogenous bone. In contrast with the experimental data of Schulke et al. (3), our clinical results permit us to conclude that boiling of the bone flap in distilled water for 40 minutes and additionally immersing it in normal saline and antibiotic solution provides sufficient sterilization. In selected cases, such a procedure can be performed even after initial removal of the infected bone flap. The same technique may be used for eradication of tumor cells from bone flap in patients with neoplastic infiltration of the calvarium (4–6). Of course, devitalization of the boiled or autoclaved bone is usually presented, but in our experience, this has no clinical consequences. In no one case of our series did we observe resorption of the bone flap from its external side. It is known that autografts of the cranium heal through revascularization and nonosteoclastic resorption coupled with new bone formation from bone flap in patients with neoplastic infiltration of the calvarium. However, as it was perfectly shown by the authors of this article (1), this technique is not necessary if bone flap occasionally falls onto the operating room floor.

Mikhail Chernov
Tokyo, Japan


10.1227/01.NEU.0000296968.91513.FD
Brain Computed Tomography Angiographic Scans as the Sole Diagnostic Examination for Excluding Aneurysms in Patients with Perimesencephalic Subarachnoid Hemorrhage

To the Editor:

Kershenovich et al. (5) reported on a series of 30 patients with a perimesencephalic subarachnoid hemorrhage (SAH) pattern and a negative computed tomographic angiography (CTA). In these patients, subsequent conventional cerebral angiography also failed to demonstrate an aneurysm.

The dangerous scenario of a missed cerebral aneurysm in a patient with a perimesencephalic SAH could occur if CTA were falsely negative and no correlative digital subtraction angiography (DSA) were performed. The authors attempt to simulate a “CTA-only” algorithm of SAH evaluation with their retrospective case series. In a random sample of 30 cases, no false-negative CTA exams were observed—thus, this potential “dangerous scenario” never would have arisen were a CTA-only algorithm applied. On the basis of these data, the authors conclude that “... CTA alone is a good and conclusive tool to rule out aneurysms in patients ... with classic perimesencephalic SAH ... and thus can replace DSA and its corresponding risks.” The conclusion that the authors have drawn seems logical, but they are not justified on the basis of their data set.

Occasionally, investigators are required to estimate the probability of an event occurring on the basis of a random sample in which the event has not occurred. Methods for estimating the probability of the occurrence of an event on the basis of a zero occurrence in a given sample have been reported by Basu et al. (2) and Louis (6). The classic, one-sided 95% confidence interval for the Kershenovich et al. data set (n = 30) yields an upper limit of 9.5%, the chance of the “dangerous scenario” of a false-negative CTA arising (6). If a Bayesian estimate is applied (2), the upper confidence bound (95%) is 6.2%. Kershenovich et al. would have to observe an additional 170 consecutive patients with “classic perimesencephalic SAH” (increasing the overall sample size to 200) with negative CTAs and DSA to reduce the upper 95% confidence bounds to 1.5% and 0.95% using the classic and Bayesian estimates, respectively; this rate is concordant with the upper confidence bound (95%) is 6.2%.

It was previously reported that in patients with “perimesencephalic hemorrhage,” there is a 4 to 9% chance of an aneurysm being the source of the SAH (1, 7, 8). Because of the relatively low prevalence of cerebral aneurysms in this patient population, one could argue that the perimesencephalic SAH pattern alone would preclude the necessity of a catheter angiogram, as did Ruigrok et al. (9). However, the present series of 30 patients with true-negative CTAs adds little to support a CTA-only algorithm and by no means justifies a change in the accepted diagnostic evaluation of these patients.

With modern CTA, we certainly agree that the vast majority of saccular aneurysms in this category are detected prospectively, even to the extent that a definitive treatment plan (endovascular or surgical) can be entirely formulated from the noninvasive imaging data. The questions then are: What types of aneurysms are not detected easily with CTA? and Can anything be done about them? Dissecting and blood blister aneurysms of the vertebral basilar system are capable of producing a perimesencephalic pattern of SAH. These aneurysms often lack a distinct saccular component and occur in atypical locations, thereby decreasing their conspicuity on CTA, even with application of the most-advanced multiplanar reformations and three-dimensional postprocessing techniques. These aneurysms are also among the most dangerous cerebrovascular lesions, with a malignant natural history characterized by frequent and early rebleeding (10). Despite their aggressive natural history, the treatment of these lesions is often technically straightforward and can be achieved safely either through a deconstruction of the parent vessel or, when a constructive strategy is required, via Neuroform stent (Boston Scientific/Target, Fremont, CA) reconstruction (3). At our institution, we presently perform CTA as a first-line test on almost all patients with SAH. In those patients with negative CTA exams, we proceed with thorough catheter-based angiography, largely to exclude one of these dangerous but iniminally treatable lesions.

Raymond Turner
Nancy Obuchowski
Vivek Gonugunta
Michael Kelly
Henry H. Woo
David Fiorella
Cleveland, Ohio

Synthetic Nerve Guide Implants in Humans: A Comprehensive Survey

To the Editor:

We would like to point out that the overview presented by Schlosshauer et al. (6) does not cover all the published literature. Also, we would like to point out some errors.

Before the manuscript by Schlosshauer et al. was accepted, an article had been published describing the use of a resorbable p(DLLA-ε-CL) nerve guide for secondary digital nerve reconstruction in the foot of a patient with posttraumatic neuromas of the common plantar digital nerves II–III and III–IV. The sensory nerve recovery was poor, and there were no complaints of painful neuromas after this procedure. In 1994, Berger concluded that the interposition of polyglycolic acid (PGA) nerve conduits seems possible for short defects in digital nerves, but will render unfavorable results in long defects of peripheral mixed nerves. Berger based these conclusions on clinical work done by Dellon and Mackinnon. A European multicenter study was claimed to be in preparation. However, to this day, nothing has been reported in the literature about a European study.

Casañas (1) described a prospective study of 17 patients with chronic lesions of digital nerves in the hand and defects longer than 2 cm. The nerves repaired were digital, sensory ulnar branches, and sensory radial branches. The gaps ranged from 2 to 3.5 cm. The average time from injury was 4 months. Several modalities for the evaluation of sensory-nerve recovery were used. All modalities showed recovery. They concluded that the use of PGA tubes was a useful technique for repair of chronic sensory nerve lesions or defects longer than 2 cm.

Another article that should have been mentioned is Ducic et al. (3). They presented two patients with iatrogenic injuries to the Xth nerve reconstructed at 3 months after the loss of shoulder function. In the second patient, the Xth nerve was reconstructed with a bioabsorbable PGA conduit. For completeness, we also would like to draw your attention to a recent publication by Dellon and Maloney (2) describing a patient in whom sensation was restored 3 years after successful reconstruction of a right-avulsed thumb with a microvascular hallux transfer by reconstruction of the radial sensory and volar digital nerves of the thumb by PGA conduits.

The authors stated that degradation of polylactide caprolactone (PLCL) nerve conduits resulted in complete resorption. However, in 2004, a 16-month study on sciatic nerve regeneration through poly(DLLA-ε-CL) nerve guides was published (4). Some small fragments of biomaterial could still be found on the edge of the epineurium of the regenerated nerve after 16 months, indicating remnants of a secondary foreign body reaction, despite the facts that the nerve guides resulted in good nerve regeneration and this did not influence the nerve-regeneration process.

Finally, we would like to encourage researchers and clinicians who are interested in peripheral nerve surgery to initiate trials comparing FDA- and CE-approved nerve conduits. These trials should compare foreign-body reactions and degradation patterns of the different biomaterials used for conduits. To justify the significant price differences between the different conduits, the functional outcomes of all different FDA- and CE-approved conduits should be compared clinically.

Marcel F. Meek
Groningen, The Netherlands
J. Henk Coert
Rotterdam, The Netherlands

In Reply:

We appreciate that Dr. Meek and Dr. Coert have identified additional articles besides the ones that we have indicated in our publication, especially the one not listed in PubMed and those that were published after we submitted our manuscript. (The 2004 citation from Jansen et al. [1] is not relevant, because only animal experiments were reported and there were no human data.)

For completeness, we would like to add another article by Lohmeyer et al. (2), which has an English abstract. Several patients with nerve gaps in the hand up to 18 mm were reconstructed by means of tubulization with collagen nerve conduits. Sensibility was assessed using static and dynamic two-point discrimination and monofilament testing. One year postoperatively, four out of six patients showed excellent results. This report is also noteworthy because it discusses regulatory incompatibilities. The authors point out that the present health insurance cost rates are equally low for direct-nerve coaptation and autologous nerve transplantation despite the fact that the surgical complexity varies considerably between the two approaches. Because nerve conduit implantation is not always considered, the authors call for the need for action to reform the reimbursement system.
Together with these authors and Drs. Meek and Coert, we support the notion that multicenter studies should be initiated, because nerve tubulization provides a promising therapeutic means.

Burkhard Schlosshauer
Lars Dreessmann
Reutlingen, Germany
Hans-Eberhard Schaller
Nekatarios Sinis
Tübingen, Germany


Three-Day Phenytoin Prophylaxis is Adequate after Subarachnoid Hemorrhage

To the Editor:

We read with interest the recent article by Chumnanvej et al. (3) on the adequacy of 3-day phenytoin prophylaxis after subarachnoid hemorrhage (SAH). The authors conclude that a prophylactic 3-day phenytoin regimen is adequate to prevent seizures after SAH when compared with long-term anticonvulsant administration, with less potential for phenytoin toxicity and related hypersensitivity reactions. However, they recommend patients with known epilepsy to receive anticonvulsants without any modification in their prehemorrhage drug regimen.

In our unit, for over 10 years it has been the senior author’s (KAC) policy not to administer any anticonvulsant prophylaxis to patients presenting with SAH. This also includes patients who have aneurysmal intracerebral hematoma, cerebral edema, and hydrocephalus, and those who need surgical intervention to secure a ruptured aneurysm (2). With a very similar patient workload as that from the author’s center, the incidence of seizures in our patient group has not been more than that noted in the authors’ series with 3-day prophylaxis. Although we do not disagree with the authors’ suggestions that a longer-term anticonvulsant prophylaxis after SAH may be overkill, from our observations, we contend that routine anticonvulsant prophylaxis is unnecessary altogether in SAH patients, even those with a high risk of seizures. We would, however, continue with the anticonvulsant regimen in known epileptics and would also commence phenytoin therapy in the occasional patient who has seizures after SAH. We believe that the literature does not provide robust and unequivocal support for prophylactic anticonvulsant therapy, and that the risk of seizure may, in fact, be overestimated. Perhaps the clinicians cautiously prefer to err on the side of prophylaxis than to let these already-compromised patients be subjected to additional neurological insult from an epileptic seizure. We appreciate that the reasoning behind such prophylaxis may well be justifiable for a selected group of high-risk patients (3), but at the same time, we think that universal phenytoin prophylaxis in all patients with SAH on empiric grounds is unwarranted. We congratulate the authors for their timely study and for opening up this important debate that we hope, in the future, results in abolishing altogether the practice of administering routine anticonvulsant prophylaxis.

Kishor A. Choudhari
Chandrasekaran Kaliaperumal
Belfast, Ireland


In Reply:

We thank Drs. Choudhari and Kaliaperumal for the thoughtful comments. At their center, no anticonvulsants are given to patients with SAH unless a seizure occurs. They believe that anticonvulsants may not be a necessary part of initial SAH management, even for a short duration. Several of our reviewers also asked similar questions about whether seizure prophylaxis was necessary, and how for long (see the Comments that follow our article).

For the series described, we chose a duration of 3 days for two reasons. First, as a tertiary referral center, many patients we treat were already loaded with Dilantin (Pfizer, New York, NY) at the initial facility. Second, there is still some concern among many practitioners that a seizure can lead to aneurysm rupture (although most of us now believe that a seizure results from aneurysm rupture). For these reasons, we elected to continue Dilantin until the aneurysm was repaired, almost always within 2 days of admission. The point of our article was that 3 days was enough in our series; seizure rates did not increase, whereas drug-reaction rates plummeted.

We do not know whether a shorter period of prophylaxis is adequate, or whether anticonvulsants are even necessary in this setting. We would be interested to see whether others have data on this issue. In addition, we will consider a change of protocol to shorten or eliminate anticonvulsant use after SAH. Clearly, this is an area of evolving practice, and we hope that our article stimulates others to explore this question further.

Dong H. Kim
Ian F. Dunn
Boston, Massachusetts
Passion for One’s Work

To the Editor:

At half past six every morning, except Sunday, Maurizio Marinella (Fig. 1), a third-generation member of one of the most famous tie tailoring families, personally opens the door of his small shop in Naples. It is here that kings and presidents, politicians and lawyers, doctors and tourists, young men going to their first job or to a wedding ceremony ask for that thin, colored strip of fabric only he is able to offer. He finds a word for all those entering his precious little store, he never does the “face of the situation” as Luigi, a very professional and concerned middle-aged member of his magical crew, used to say. It means that he takes out of his heart a smile, a sentence, his attention or a moment of time for hundreds and thousands of men and women looking for him every day from dawn until 8 p.m., when he closes the shop and goes home. Everyday, except Sunday, every week for 52 weeks per year, he follows in the steps of his father, who followed the steps of his father, across two centuries, continuing the tradition and extending the legacy of the family tradition, spreading his selected handcrafted products around the world without losing enthusiasm and ideas. All of this is possible because of the joy and the passion he unceasingly puts into his work; this allows him to take care of his friends, give due attention to his sports and his club, follow general events, and help people who have need or are suffering all at the same time. Everything begins with the selection of the fabric, from buying precious silks one by one, to following the manufacturing process, which is often tailored to the single customer, to passing through his hands the tie and the parcel; like a maestro, he coordinates all aspects of the process to produce the best tie, the perfect symphony. Such pleasure, which is ostentatiously expressed in his tireless, constant care, is the key for success, and in the present case, satisfaction is proven by elegance. This day-long activity is just the opposite of the nocturnal turbulence that crowds the minds of the desperate with dreams and nightmares. Maurizio, on the contrary, wakes up eager to go toward a brand new day.

In our discipline, Maurizio’s interpretation of the role he plays, like few others and differently than most, can be taken as a metaphor of the choice to become a neurosurgeon, running after our passion, in the hard winter days when the wind and rain cut the face as well as when hot summer days bleach out the colors. Only with such condition of the spirit will we have the chance to succeed. But the attention and care for detail necessary for this purpose will make a difference; as for the elegant man, the tie fitted to the single occasion. We need to pay extreme respect to that stripe of cerebral tissue that is the target of the surgical deed, having knowledge of its uses and functions (Fig. 2); we must reach the surgical target area with utmost respect of the crossed territories, often more noble even than the final site of action; we must care for the whole person and not the disease only; we must spare the patient the pain and the humiliation of the condition of being sick; we must inform him or her about the disease and our relative enterprises; we must involve in our efforts people with the same concerns. We must move from the “I” to the “We” by coordinating a team that guarantees an organization leading to a complete result, like Maurizio Marinella in his inexhaustible passion for his wonderful work.

Paolo Cappabianca
Naples, Italy

FIGURE 2. Penfield’s homunculus with the tie covering the critical area of the brain cortex.
The Combined Transmastoid Retro- and Infralabyrinthine Transjugular Transcondylyar Transtubercular High Cervical Approach for Resection of Glomus Jugulare Tumors

To the Editor:
The authors have provided an elegant description with instructive schematic drawings of this complex cranial base approach (3).
The senior author (JKL) states that he has used this approach to treat 129 jugular foramen tumors, 30 of which were glomus jugulare tumors. Unfortunately, there is no information regarding the stage of these tumors. The most important factor determining the surgical approach is the degree of internal carotid artery involvement.

A basic principle in successfully treating these lesions is both proximal and distal control of the internal carotid artery (2, 8, 9). The only way to gain this access in most cases requires anterior re-routing of the facial nerve (7–9).

From our extensive experience with management of jugulotympanic paragangliomas (JTPs), management of the internal carotid artery in some form was required in 91% of cases. This ranged from simple skeletonization to facilitate removal of surrounding bone, to subadventitial dissection, and to sacrifice with preoperative occlusion (8). Only 7.5% of our series of 55 JTPs published in 2004 (9) were classified as Fisch Type C1 (i.e., with erosion of the carotid foramen with minimal vertical carotid involvement). It is only this stage of JTP that could be safely amenable to minimal facial nerve rerouting. In 2001, Jackson (2) reported a large series of glomus jugulare tumors in which 42% were classified as Glascock-Jackson class I–II, which they state can be approached with short anterior facial nerve mobilization. This left 58% of their cases requiring a long anterior facial nerve mobilization, blind sac closure, and management of the mandible to safely access the internal carotid artery and tumor. In addition, to minimize the risk of recurrence, extensive removal of bone in the petrous apex must be attempted. This is also extremely difficult given the degree of exposure afforded in the described technique. Even Oghalai et al. (4), champions of the fallopian bridge technique (who have an impressive series and results), state that involvement of the horizontal carotid may be better approached with anterior re-routing of the facial nerve.

Routine exposure of the vertebral artery by means of the classic far-lateral approach is not required in our experience. In fact, this has been necessary in only one case. Concern regarding minimization of postoperative morbidity is important (1–4, 6–11). Our facial nerve results after anterior re-routing show recovery to House-Brackmann Grade I or II in approximately 70% of cases (8), which indicates that increased safety of internal carotid artery exposure and lower rates of recurrence do not come at too high a price (5, 7–9). These figures are consistent over many series (5, 6).

It is difficult to accurately assess surgical results from this series due to the lack of data. However, even with our aggressive approach, we have previously reported a 6.1% (of 81 cases) (8) recurrence rate in advanced-stage JTP. Jackson (2) reported a recurrence rate of 5.5%. It is also important to note that most reported recurrences in the literature become apparent after a period of 5 years, necessitating lifelong follow-up (2, 8).

It is misleading to group JTP with other jugular foramen pathologies. Schwannomas and meningiomas in this region can often be approached with preservation of middle ear function and minimal facial nerve mobilization because of less-frequent anterior extension and carotid artery infiltration (1, 7).

We certainly agree that JTPs are formidable lesions, and ongoing refinements and advances in technique are welcomed to improve the management of these tumors. We think, however, that in the treatment of JTP, especially those of Fisch Types C2, 3, and 4, the use of the transjugular, transcondylar, and transtubercular extensions should be used in combination with, and not as an alternative to, the Fisch Type A infratemporal approach. This is a standard modification that we have used over the last 4 years. As the authors have stated, it allows increased posteroinferior access to the jugular bulb. As always, it is the disease that dictates the approach, not vice versa.

Mario Sanna
Sean Flanagan
Piacenza, Italy


In Reply:
We appreciate the comments by Dr. Sanna regarding our article (1). We recognize Dr. Sanna has much experience with these tumors and that, as cranial base surgeons, we should work as
CORRESPONDENCE

a team of neurosurgeons, otologists, and occasionally, head and neck surgeons and plastic surgeons.

We also agree that one of the most important issues is the exposure and control of the internal carotid artery. We tailor the approach in terms of the retrojugal, transjugal, or infrajugular exposure with regard to the size and extension of the tumor. If the jugular foramen tumor is mainly intradural, we perform the combined transtemporal, transmastoid, and retrosigmoid intradural procedure for the resection of the intradural portion. If the jugular bulb is already occluded and the tumor has both intradural and jugular foramen components, we perform a combined intradural and extradural exposure. If the tumor is extensive, such as a glomus jugulare tumor involving the infrajugal component, intratemporal carotid artery, and jugular bulb, we do a combined transmastoid infralabyrinthine and combined transtemporal transjugular and infrajugal transcondylar and high cervical exposure. For extensive glomus jugulare tumors, we always expose the high-cervical internal carotid artery for proximal control. We also expose the C7 infratemporal carotid artery in front of the jugular bulb by drilling the inferior bony tympanic ring and translocating the facial nerve anteriorly.

The most important aspect of cranial base surgery in this region is to avoid causing facial nerve palsy, swallowing disturbance, or voice hoarseness. We specifically choose to not reroute the facial nerve to avoid postoperative facial nerve palsy. We instead translocate the descending fallopian and stylomastoid foramen segments of the facial nerve approximately 5 mm anteriorly to provide adequate exposure of the infratemporal carotid artery. In our experience, it has not been necessary to reroute the facial nerve as Dr. Fisch recommends. Meticulous attention is given to preservation of the IXth and Xth lower cranial nerves around the carotid artery as well as the preservation of the pars nervosa of the jugular bulb floor. Our results from 53 cases of jugular foramen neuromas demonstrate that 75% of patients have normal or nearly normal swallowing function after surgery. For the 30 cases of glomus jugulare tumors, 64% of patients have normal or nearly normal swallowing function after surgery.

We perform either gross total or radical subtotal resection of glomus jugulare tumors. The patient may have some residual infiltration around the carotid artery in a minority of cases; however, we monitor the residual glomus tumor around the infratemporal high cervical carotid in follow-up examinations, and in many of these cases, the tumor shows no growth over a follow-up period of 5 to 10 years. If we see significant growth, the patient is referred for stereotactic radiotherapy or radiosurgery. Most jugular foramen tumors, neurinomas, meningiomas, or glomus jugulare tumors are benign. We do not recommend subadventitial dissection of the internal carotid artery because of the high risk of carotid rupture and other carotid complications.

In our experience of removing 30 cases of glomus jugulare tumors, we achieved gross total resection in 24 cases (80%), nearly total resection in 2 cases (6.7%), and subtotal resection (some tumor remains adherent to the pars nervosa or around the infratemporal internal carotid artery) in 4 cases (13.3%). New or worsened nerve deficit of the facial nerve was observed in 20% of the patients (most Grade II or III), and deficit of the IXth and Xth cranial nerves was noted in 27% of patients. Most of the patients with IXth and Xth cranial nerve deficit improved with thyroplasty procedures. There was no mortality in this series.

We thank Drs. Mario Sanna and Sean Flannigan for their valuable comments and appreciate the opportunity to clarify the rationale for our microanatomical joint neurosurgical-otolaryngological approach to lesions of this area.

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Athletic Concussion: Current Understanding as of 2007

To the Editor:

Dr. Cantu’s unfortunate ad hominem and defamatory remarks about our experience with concussion (1) and a review article on this subject in the New England Journal of Medicine (4) are inappropriate. I am dismayed that Neurosurgery allowed the publication of undignified and incorrect statements, particularly the gratuitous and outrageous connection made by Dr. Cantu between us and the Reggie Lewis case.

On factual matters, Dr. Cantu objects to our use of “slight” in describing the increased frequency of a second athletic concussion. He refers to the National Collegiate Athletic Association (NCAA) study, of which he is a coauthor (2), that shows a threefold increase in concussion among athletes with self-reported previous concussions. But he neglects to indicate that this increase pertains only to a small group of 10 players with 3 or more concussions over the previous 7 years. For the larger number with one previous concussion, the risk rate was 1.4 with a lower confidence interval of 1.0, making the entire thesis questionable for that group. Dr. Cantu further confuses this issue by quoting from our review, “athletes who have a concussion have a slightly increased incidence of concussion in the same season,” which is entirely another matter. His article reports that 12 of the 184 players with concussion had another concussion within the same season (166 did not). Hence, “slight” is appropriate on both counts.

Dr. Cantu is also incorrect in his appraisal of the article by McCrory and Berkovic (3) on second-impact syndrome (SIS), which he states is about the definition of the problem. That is not the case. Their careful review examined the evidence for
the existence of the second-impact syndrome and the circumstances under which it occurs. They conclude that “diffuse cerebral swelling is a rare complication of mild traumatic brain injury in sports that occurs predominantly in male teenagers. . . . Most cases have no prior evidence of head injury with ongoing symptoms that would support the concept of SIS.” This is inconsistent with Dr. Cantu’s anecdotal views. I do not doubt that repeat athletic concussions occur and that there may be individuals who are susceptible (for many possible reasons including recall bias or style of play), or that rare children have brain swelling after a single concussion. However, we sought to provide an evidence-based review and limit comment when such evidence was absent, equivocal, self-referential, or when studies were flawed, as they are in reference to this subject.

My approach to concussion is based on careful reading of the literature, with all its limitations, as well as personal experience in caring for numerous concussed college football athletes from our local team for 17 years, college hockey players, and professional baseball players, in addition to a considerable number of high school athletes. I have carefully followed many of these young men and women in the neuro-intensive care unit setting and as outpatients.

I share with Dr. Cantu a desire to improve the management of concussion. I have a concern that numerous definitions and guidelines on concussion have little basis in evidence. The review in the *New England Journal of Medicine* was meant to emphasize this deficit and stimulate reasoned discourse.

Allan H. Ropper  
Boston, Massachusetts

Although I respect Dr. Ropper’s opinions, he and I must have seen different athletic concussions as contrary to his experiences that “they have done well and there have been no instances of second-impact syndrome or decline in school performance,” my experience as the medical director of the National Center for Catastrophic Sports Injury Research is that we have one or two cases of SIS each year in football alone.

Furthermore, although I admit my office practice is skewed to more serious concussions, I see patients most weeks with prolonged postconcussion symptoms who have not “done well.” Many have lost significant school time or had to have courses reduced or SATs and finals postponed. I have also counseled a number of professional athletes in football, hockey, and wrestling who fall into the Al Toon, Troy Aikman, Steve Young, Wayne Chrebet, Ted Johnson, Pat LaFontaine group who have had to give up their athletic careers because of persistent postconcussion symptoms.

Along with Julian Bailes and Bennett Omalu, I have studied the microscopic slides of the brains of Mike Webster and Terry Long reported in this journal (4, 5) and more recently, those of Andre Waters and Justin Strzelczyk. In all of these cases, the brains showed findings consistent with chronic traumatic encephalopathy, and the individuals exhibited the psychological profile of chronic traumatic encephalopathy, including impaired cognitive functioning (loss of memory and executive function), behavioral and psychological problems, and major depression.

So while most cases of athletic concussion recover within 1 week, a small subset, especially those with multiple concussive and subconcussive injuries and likely with a genetic predisposition, not only do not do well, but may progress to chronic traumatic encephalopathy, sometimes with tragic consequences.

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