significant radiation reactions following radiosurgery for AVM's. Recognition of this possibility is very important since their guidelines are frequently referenced (and probably frequently used) by clinicians performing radiosurgery. As radiosurgery becomes more widely utilized, the majority of clinicians prescribing radiation doses will not have vast clinical experience and may rely upon those guidelines.

There is a fundamental question as yet unanswered. How does the tolerance of the brain to radiosurgery change as the irradiated volume changes? Dr. Kjellberg's pioneering work does provide some insight into the answer. As radiosurgery becomes more universal, we have the opportunity to better answer this question. Clinical reports need to include details (minimum target dose, target diameter, target volume, location of target, and prescription of isodose line) for all patients, both those with and without radiation-associated side effects. Only through the accurate recording of clinical data can we correctly answer this fundamental question.

With regard to Dr. De Salles' statement about the series from Berkeley, these patients were irradiated in approximately one to three fractions. We believe that this hypofractionated technique is more similar to radiosurgery (one treatment) than to conventional fractionated radiation therapy (usually ≥ 20 fractions).

**References**


**Instrumentation for Spinal Trauma**

TO THE EDITOR: I recently read with interest the article on usage of Texas Scottish Rite Hospital (TSRH) instrumentation for thoracic and lumbar spinal trauma (Benzel EC, Kesterson L, Marchand EP: Texas Scottish Rite Hospital rod instrumentation for thoracic and lumbar spinal trauma. J Neurosurg 75:382–387, September, 1991). The authors have certainly demonstrated the applicability of a universal instrumentation system to thoracic and lumbar injuries of various types, albeit much information regarding the exact biomechanics of instability was not included. It is notable that both the TSRH and the Cotrel-Dubousset systems were developed for correction of rotational deformities and that they are not optimal in situations where distractions or compression play primary roles in reduction and stabilization of injuries. In such instances, systems such as Edwards instrumentation are often preferable.

Upon reading this article, I was rather dismayed at the inappropriately short follow-up period in this group. Spine stability may be defined as the ability of the spine to maintain normal alignment under stresses encountered in routine daily activities. At a mean follow-up period of 9 months, most of these patients had only just begun to resume normal daily activity in comparison to their lifestyles prior to trauma. Certainly, at the shortest follow-up period (3 months), these patients are only just being weaned from their external orthosis, and stability may only be addressed on an extremely short-term basis. As we neurosurgeons strive to attain equal ground with our orthopedics colleagues in regard to spinal instrumentation, we must carefully avoid jumping the gun and reporting clinical series with inadequate follow-up periods. This paper, therefore, should be viewed as more of a statement of applicability of instrumentation than any comment on its ability to provide long-term stability in treating spinal trauma.

**Response:** We appreciate the comments by Dr. Chesnut. It most certainly is notable, as Dr. Chesnut suggests, that universal spinal instrumentation (USI) techniques (such as the Texas Scottish Rite Hospital and Cotrel-Dubousset systems) were developed for correction of rotational (and sciotic) deformities of the spine. However, we take issue with the implications of his comment that these techniques "are not optimal in situations where distraction or compression play primary roles in reduction and stabilization of injuries." Although no currently available technique is "optimal," USI techniques are quite "applicable" to situations where either distraction or compression play primary roles in spine reduction and stabilization. In fact, the "latitude" offered by USI techniques allows for the correction of nearly all types and degrees of deformities in a distraction, compression, or neutral mode. These techniques offer a greater degree of stability than is often offered by more traditional techniques.

The more traditional techniques, such as the Harrington distraction or the Edwards techniques, apply substantial ventrally directed forces at the fulcrum (usually at the site of the fracture) and even greater forces at the upper and lower hook/bone interfaces. The USI techniques allow for the utilization of multiple sites of contact between bone and metal. This distributes the load of instrumentation over a wider surface area (load sharing), which is a definite advantage with regard to
the prevention of instrumentation failure. This, in turn, allows for the application of a stable construct with much less force applied at each metal/bone interface than is realized with the more traditional techniques. A decreased chance of undesirable results (including hook dislodgement and posttraumatic pain syndrome) is thus expected.

Dr. Chesnut’s comments regarding length of the follow-up period are well taken. However, we feel that an average 9-month follow-up period is not unreasonable. To delay publication of information in order to gain longer-term follow-up data is laudable, but must be weighed against the requisite delay in information dissemination. In the case of the manuscript under discussion, increasing the average follow-up period by 1 year (the approximate time passed since the data were initially assessed) would have changed the results minimally. One additional patient had his instrumentation system removed due to tenderness near the upper end of the rods. He had acquired a stable fusion. None of the remaining patients had any changes in neurological status, spinal stability, or angle from that observed at the time of original follow-up report. This is, at least in part, testimony to the efficacy and utility of USI techniques when applied to spinal trauma. We timed the submission of the report of our data without consideration of factors related to “equality” or “turf” between specialties. That would have been truly “inappropriate.” The manuscript was submitted with “appropriate” data dissemination as our only goal.

We indeed hope that we have not “jumped the gun” with regard to the reporting of our results. We view our results, as Dr. Chesnut points out, as a statement of applicability of USI instrumentation for the treatment of spinal trauma. It is not our intent, however, to provide long-term stability with an instrumentation construct as Dr. Chesnut suggested. Long-term stability is, instead, provided by the patient’s reparative processes, which include bone fusion.

Finally, USI techniques are most certainly not the final answer to our spinal trauma instrumentation problems. Lower-profile, shorter, and easier to apply techniques are assuredly on the horizon.

Edward C. Benzel, M.D.
Lee Kesterson, M.D.
Erich P. Marchand, M.D.
University of New Mexico Medical School
Albuquerque, New Mexico

Myelotomy in Spinal Spasticity

To The Editor: I was pleased to see the recent article by Putty and Shapiro (Putty TK, Shapiro SA: Efficacy of dorsal longitudinal myelotomy in treating spinal spasticity: a review of 20 cases. J Neurosurg 75: 397-401, September, 1991). Neurosurgical management of the important problem of spasticity in spinal cord injury cannot be minimized. However, the authors failed to appropriately cite the work published 15 years earlier by Cusick, et al.,1 examining the effectiveness of T-myelotomy on spasticity in spinal cord injury and addressing the important area of reflex bladder control. This earlier study not only essentially duplicated the present investigation but also incorporated neurophysiological techniques to validate the surgical procedure, and defined the value of this approach in the treatment of reflex bladder control.

The report of Putty and Shapiro, thus, revalidates the utilization of anatomically consistent ablative procedures in the management of severe spasticity following spinal cord injury. The authors correctly emphasize that T-myelotomy remains an efficacious alternative to spinal cord stimulation or intrathecal baclofen in the management of this difficult problem.

Dennis J. Maiman, M.D., Ph.D.
Medical College of Wisconsin
Milwaukee, Wisconsin

Reference

Response: We appreciate Dr. Maiman pointing out the excellent article by Cusick, et al. It would be an important addition to our bibliography. We are happy that he brought this oversight to our attention.

Scott A. Shapiro, M.D.
Indiana University Medical Center
Indianapolis, Indiana

Prediction Tree for Severely Head-Injured Patients

To The Editor: Choi, et al., have provided a particularly useful prediction algorithm for severely head-injured patients (Choi SC, Muizelaar JP, Barnes TY, et al: Prediction tree for severely head-injured patients. J Neurosurg 75:251-255, August, 1991). Coupled with our algorithms for nontraumatic coma,2,3 physicians can now provide families with guidance about outcome for many unconscious patients. Our 1985 paper4 used recursive partitioning, as did Choi, et al. In seeming contrast, however, we took advantage of one other capability of recursive partitioning noted by Choi, et al., namely the ability to assign different costs or penalties to errors of misclassification. Based on interviews with a number of neurologists and neurosurgeons, we assigned a penalty 20-fold higher to predicting a poor outcome in someone who actually did well than to predicting a good outcome in someone who did not. While this reduced overall accuracy for us, we reasoned that it was more useful clinically, based on the seriousness of incorrectly acting on an erroneous poor prognosis. From their comparison with other prediction techniques, it appears that Choi, et al., assigned equal costs to various misclassification errors in an effort to maximize overall accuracy. If so, the authors should