Pseudocyst of spinal cord on metrizamide ct.

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Editor's Reply

All the points raised by Drs. Bottomley and Edelstein are well known to all of us. However, I strongly disagree on one point: namely, that it is possible to educate the public to overlook the word *nuclear*.

It is true that the term *nuclear medicine* is in common use; nevertheless, I don't believe that the patients who have any knowledge of it or those who have antinuclear sentiments appreciate the idea of undergoing any procedure involving a radioactive compound. Nuclear medicine is definitely associated with radioactivity, and this is precisely the association we wish to avoid. It is virtually impossible to eradicate such an idea or association from the minds of the lay public, once it has been established.

On the other hand, it *is* within the realm of possibility to educate some learned groups, such as radiologists and other medical specialists who use radiologic imaging, to change their manner of thinking and become accustomed to a slightly different terminology and corresponding abbreviation. This is what I have proposed.

JMT

Reply

It should be reemphasized that the recommendation of the American College of Radiology's commission to use the term *magnetic resonance* was based on numerous requests for a change in terminology. These requests came from the medical and scientific communities as well as certain segments of the industrial community participating in the development of this new technology. In making its recommendation, the commission believed it was reflecting the consensus of these various groups and individuals.

By referring to (nuclear) magnetic resonance *imaging*, Drs. Bottomley and Edelstein show that they may have missed a major point in my editorial: Its thrust was to eliminate qualifiers such as "imaging" as being restrictive.

As noted in my editorial, the Society of Magnetic Resonance in Medicine omitted the word *nuclear* from the society's name; there seemed to be no serious objection to this omission when the society was formed. This society publishes a scientific journal, *Magnetic Resonance in Medicine*, also omitting the term *nuclear*. Drs. Bottomley and Edelstein are both members of this society, and Dr. Bottomley serves as a member of the journal's editorial board.

I applaud their call for educational efforts designed to overcome the fears of the public about things "nuclear." This is an important responsibility for all in the medical and scientific disciplines. Pragmatically, however, it seems unlikely that such efforts could be successful in the near future.

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Reply

The *AJR* is understandably sympathetic to the position taken by Drs. Bottomley and Edelstein. Terms dating to the inception of major scientific advances are certainly to be respected even if later developments prove them less than ideal. *Roentgenology* is such a term; the *AJR* persists in using it in its title, even though many laypersons ask, "What is roentgenology?" Of course, the subject matter of the *AJR* is radiology, a term which over time has become much more explicable to the public and, moreover, more encompassing of the technology of our imaging specialty than *roentgenology* could ever be. The *AJR* shares with Drs. Bottomley and Edelstein some reverence for the historical even if it is anachronistic.

For purposes of medical imaging terminology, *nuclear magnetic resonance* may already be anachronistic. This is not so much because of the widespread use of alternatives but because influential groups have shown preference for the less cumbersome term *magnetic resonance* (MR). The Society of Magnetic Resonance in Medicine, made up of most of the active investigators in the field, has chosen to drop the word *nuclear*. Similarly, the American College of Radiology's Commission on NMR, after thorough deliberation, offered its recommendation that *magnetic resonance* be the preferred term. Many authors already are beginning to adopt it.

Accordingly, the *AJR* henceforth will accept *magnetic resonance* as sufficient and will not use the modifier *nuclear*. In due course we anticipate common use also will favor *magnetic resonance*. The issue seems to have been decided by authorities larger than our editorial office. Notwithstanding our respect for historical priorities, it seems only sensible to accept the inevitable.

Melvin M. Figley
Editor, AJR

Pseudocyst of Spinal Cord on Metrizamide CT

In the January/February 1984 issue of *AJNR*, Quencer et al. [1] reported the results of intraoperative spinal sonography in patients with prior spinal cord trauma. In two cases, metrizamide-enhanced computed tomography (CT) demonstrated findings strongly suggestive of a syrinx. Intraoperative sonography failed to confirm the finding, showing only an area of abnormal echogenicity within the spinal cord. The authors stated that "although these zones were not explored, we are confident they do not represent cysts."

It is distressing that the apparent cyst demonstrated in their figure 6 (case 8) was a false-positive finding. Perhaps the authors will share with us their reason(s) for being so confident that this was true even though the area was not explored at surgery. Did they consider the possibility that the cyst was collapsed at surgery, either from the position of the patient or as a result of the anesthesia?

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REFERENCE

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Reply

We remain convinced that these were indeed false-positive metrizamide CT findings for the following reasons:

1. Both the surgery and the preoperative metrizamide CT scanning were performed with the patient recumbent (surgery: prone; preoperative metrizamide CT: supine); thus, the cerebrospinal fluid (CSF) dynamics and the transmitted pressure to the spinal cord were equivalent. Since surgery was not performed with the patient erect, there is no positional reason why a syrinx, had it been present, would have collapsed.

2. The subarachnoid space was not entered before intraoperative sonography was performed (note the intact dura-arachnoid layer and the CSF beneath it in our fig. 6). As a result, there was no escape of CSF from the subarachnoid space that could have collapsed a cyst indirectly, had one been present.

3. The identical size and shape of the spinal cord on metrizamide

CT and sonography (cf. our figs. 6B and 6D) is another strong piece of evidence that a cyst was not present. Certainly, had a cyst been present preoperatively but collapsed after laminectomy, the cord would have appeared larger on metrizamide CT than on intraoperative sonography.

4. Under the same surgical conditions, we have performed intraoperative spinal sonography and thereby demonstrated posttraumatic cysts (see, e.g., our figs. 2–5). If posttraumatic spinal cord cysts were subject to collapse because of some technical factor (e.g., position and/or anesthesia), they should have also collapsed in these other cases.

5. We know of no reason why general anesthesia per se would cause a cyst to collapse.

On the basis of our experience now of over 20 cases of posttraumatic spinal cord cysts evaluated by intraoperative sonography, we believe the surgical approach to such cases must be guided to a large extent by the sonographic findings. From a practical standpoint, surgical exploration of the spinal cord in an attempt to find a collapsed cyst is unwarranted and potentially dangerous to the patient.

The question raised by Dr. Zatz is important because it allows us to reemphasize that not all cases of abnormal collections of metrizamide within a spinal cord represent intramedullary cysts. We are accumulating clinical, metrizamide CT, and sonographic data that show clearly that intramedullary metrizamide may be present in a wide range of pathologic conditions. For this reason, these patients should be operated on only when the symptoms suggest an expanding intramedullary cyst. When surgery is performed, we believe intraoperative spinal sonography is necessary.

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Digital Subtraction Angiography "Road Map"

In reference to the technical note by Braun et al. [1] in the March/April 1984 issue of *AJNR*, we wish to bring to the authors' attention our previous descriptions of this digital subtraction angiography (DSA) "road map" system for diagnostic and interventional procedures [2–5]. During the past 3 years, we have used the DSA road map routinely. We have found it particularly useful in performing transluminal angioplasty, in that the anatomic display facilitates catheterization of the desired branch of the bifurcation and optimal placement of the dilatation catheter. Similarly, the position for detachment of balloons or placement of other embolic materials can be observed directly and related to the accompanying angiographic map.

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