Vertebroplasty for Vertebral Fractures with Intravertebral Clefts

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Reinterpretation of Head and Neck Scans: Massive Can of Worms or Call to Action?

In this month's AJNR, Dr. Loevner and her colleagues at the University of Pennsylvania have potentially stirred themselves up some controversy. They have called attention to a problem that is very well known to us in head and neck radiology, and indeed all radiologic subspecialties in tertiary oncologic centers: the high frequency of significant reinterpretations of outside imaging studies. This is such a problem that nearly every outside head and neck imaging study (that are not otherwise inadequate) performed elsewhere on a patient referred to our institution is submitted for reinterpretation. We now see that these errors can have a significant impact on staging, treatment, and prognosis. And this work from the University of Pennsylvania is the product of a single institution; one can only imagine the numbers throughout the nation's institutions, as well as those cases in which the imaging is never reinterpreted by someone of Dr. Loevner's skill and experience. Clearly, there is a major problem in the interpretation of head and neck studies by radiologists in this country.

What are the root causes of this problem and are there any solutions? First, to the root. Head and neck radiology is a relatively young (and difficult) subspecialty. Resident training in head and neck radiology is limited, and many programs have no dedicated head and neck radiologist on staff. As a result, residents and fellows in these programs may see very little head and neck imaging or receive inadequate training. This may help explain the fact that in the Loevner study, misinterpretations were as or more prevalent from referring academic centers than the private practice setting. No formal head and neck radiology fellowships currently exist, although head and neck training is a required component of ACGME approved neuroradiology fellowships. Interested radiologists may arrange private, but usually limited visiting fellowships with several senior members of the ASHNR, but these are generally informal. I know several now-prominent head and neck radiologists who started this way; many others simply trained themselves or established their qualifications through experience and publishing.

Another issue is that very few radiology practices see enough head and neck imaging to allow any one individual to gain enough experience to be fully comfortable with it. Also, as mentioned in the Loevner article, the radiologist designing or interpreting a head and neck scan often lacks sufficient clinical history to acquire the appropriate images, much less interpret them accurately. To properly protocol and interpret a head and neck imaging study, one requires not only a basic knowledge of the disease process and physical findings, but also an understanding of the post-treatment appearance of any surgical, chemotherapy and/or radiation therapy the patient might have previously undergone. This makes the radiologist a useful member of the treating team. To gain this knowledge takes commitment and study, which is facilitated by involvement in a multi-disciplinary head and neck tumor board.

Finally, though not mentioned in the Loevner paper, interpretation is inextricably linked to the issue of image quality. For many of the reasons already mentioned, the quality of head and neck imaging studies referred to tertiary care facilities is often poor (1). Although a study of image quality is necessarily more subjective than studies based on histology and other objective measures, I once attempted to quantify the shortcomings of outside head and neck CT examinations referred to our institution (1). This study showed that the overwhelming majority of outside scans on patients referred to our Head and Neck Surgery clinic were deficient in at least one and usually several critical aspects (windowing, contrast bolus, gantry angulation, etc.). If a head and neck scan is improperly performed, correct interpretation is much more difficult, if not impossible. Furthermore, poor image quality directly results in increased health care costs, due to the frequent need to re-image patients whose initial scans prove to be inadequate. This is a daily occurrence at our facility.

Misinterpretation of head and neck imaging studies is so prevalent, that in my experience, some radiologic diagnoses such as the perineural spread of head and neck cancer, are far more commonly missed than made. It may even be that the standard of care is actually to miss perineural tumor spread radiologically.

Are there solutions to this problem? It is unlikely that there will be any sudden increase in the prevalence of head and neck disease to provide radiologists with the necessary experience in interpreting these studies. One move made by the American Board of Radiology, to make Head and Neck an equal part of the Neuroradiology CAQ (certificate of added qualification) examination, was an excellent step toward encouraging radiologists to become more proficient at head and neck imaging. Apparently, however, this is not enough.

In regard to insufficient clinical information, one solution is to defer reading any case for which history is unavailable, until the ordering physician can be contacted. In the rare case that our computer system lacks relevant history, I routinely hold off protocolling or interpreting a case until I can speak to the head and neck surgeon. Another solution is education, something to which the ASNR and ASHNR are committed. The ASHNR annual meeting is primarily focused on education, with review talks on virtually every aspect of head and neck imaging. This is an open meeting to which all radiologists are invited. The ASNR annual meeting always has a large amount of head and neck programming. In addition, the RSNA and ARRS always have refresher courses in head and neck imaging. For the past four years, there has been a refresher course at the RSNA on techniques and pitfalls in head and neck imaging (2); the ARRS more recently began such a course (3). Finally, at the ASNR.org web site, an online CME module "ASNR eCME" (http://www.asnr.org) sponsored by the Neuroradiology Education & Research Foundation, includes two fine introductory presentations on staging of head and neck neoplasms. Available free to all members of the ASNR, these are an easy and available means to increase one's competence in this field. Obviously, such educational efforts are successful only to the extent that practicing radiologists avail themselves of the opportunity.

It remains to be seen if and how the advent of teleradiology and rapid dissemination of images will allow better sharing of cases between radiologists in practice and those with greater interest and expertise in head and neck. Would such an over-read service be overwhelmed by those desiring help, or would it sit idle because radiologists will not recognize or acknowledge a need to send these tough cases (and, presumably some revenue) to others?

What of the lack of formal training programs in head and neck radiology? Perhaps the time has come for a concerted effort in this direction. The ASHNR should directly address this issue, with an eye toward the establishment of at least some dedicated head and neck radiology fellowship programs. Perhaps head and neck could be emphasized in the second neuroradiology fellowship year. Dr. Loevner has reminded us all of a serious deficiency in radiology, one that will continue unless addressed strenuously. Rather than opening a can of worms, let this serve as a wake-up call, a call to action, one that all of us in head and neck radiology should act on, in the interest of the patients and physicians we serve.

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Detection of Residual Disease of Lymph Node Metastases in the Neck, Which Is Treated by (Chemo)radiation

In this issue of the *AJNR*, Ojiri et al report on size changes as seen on pre-radiation therapy and post-radiation therapy CT scans of lymph nodes of patients with regional metastases from head and neck squamous cell carcinoma as a predictor of pathologic outcome after surgical treatment of the neck. Regional metastasis is the most important factor in the prognosis of patients with head and neck squamous cell cancer. Generally, treatment of head and neck squamous cell cancer is based on the stage of the disease at the time of presentation (1). Stage I and II disease is effectively treated with either surgical excision or radiation therapy, whereas stage III and IV disease, characterized by larger locally invasive tumor or lymph node metastases or both, is generally treated with combined surgery and radiation therapy.

More recently, chemoradiation has also proved to be effective in the treatment of advanced disease, especially in preserving vital organs while maintaining cure rates similar to those associated with a combination of surgery and radiation therapy (2). This treatment, however, remains experimental, and the jury is still out regarding whether this is a preferable treatment plan. Nonetheless, through the expanding role of (chemo)radiation therapy for organ preservation and unresectable head and neck squamous cell cancer, the dilemma of whether surgical therapy is indicated to treat gross nodal disease has surfaced. It is logical that efforts currently are focusing on prognostic factors to determine which patients would benefit from neck dissection and which patients could be spared such additional morbidity (3).

Post-treatment radiographic studies, such as those proposed by Ojiri et al, comprise a noninvasive means of monitoring the response to therapy. Ojiri et al find that henecks, in which the percentage decrease ratio of the largest node was $>50\%$ from pre-radiation therapy to post-radiation therapy CT scans, tended to have a negative surgical specimen if the neck was routinely dissected as their protocol prescribed. However, this trend was not statistically significant. The first statement in their conclusion is therefore not surprising, but the second statement that there still is a relation, albeit weak, deserves more attention. In a recently published article, the same authors found other factors, such as the size of the nodes and the presence of intranodal low attenuation and extranodal growth on post-treatment CT scans, to be predictors of the pathologic result of a planned post-radiation therapy neck dissection (4). A combination of these predictors or even other criteria may well increase the accuracy of CT to detect residual metastatic spread in lymph nodes. It is therefore not understandable why the authors did not perform this same analysis on their post-radiation therapy scans in the present study. Moreover, in addition to analyzing lymph node response, the use of volume estimation may provide a more accurate determination of lymph node size and may well provide a high accuracy of prediction. This is in contrast to area estimations, for which measurements are made only in the axial plane and which may result in an inaccurate nodal size assessment.

The use of CT to follow clinical response offers certain advantages, such as low cost and ease of availability, over techniques such as fluorine-18-fluorodeoxyglucose positron emission tomography, which is currently under investigation for this indication at various institutions. The potential of ultrasonography-guided fine needle aspiration cytology, which also has low cost and ease of availability, should be mentioned in this respect. A wait-and-see policy for the clinical N0 neck with strict follow-up with ultrasonography-guided fine needle aspiration cytology already proved to be justified in patients with early staged oral or oropharyngeal squamous cell carcinoma who underwent transoral tumor excision (5). A wait-and-see policy after negative results of ultrasonography-guided fine needle aspiration cytology and strict follow-up with ultrasonography-guided fine needle aspiration cytology has not been applied to a large group of patients with positive necks that have been irradiated. This clinical approach to the patient with nodal disease may also be worthwhile to explore in a larger study population as part of an organ preservation protocol.

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Vertebroplasty for Vertebral Fractures with Intravertebral Clefts

Intravertebral clefts have been radiographically recognized for decades, appearing traditionally as a vacuum or air-filled cleft inside a vertebral body and usually associated with previous fracture. This cleft was presumed to represent a sign of avascular necrosis or nonunion and at times could be associated with visible motion when viewed fluoroscopically (1). With the advent of MR imaging, the clefts seen radiographically have been correlated with high signal intensity on T2-weighted MR images in the location of the cleft (2). It was thought that the MR imaging was detecting fluid within the cleft, and some authors described a radiographic change in the appearance of the air-filled cleft over time with the air being replaced by some type of fluid when the patients were positioned supine (3).

Percutaneous vertebroplasty has been used since 1984 to treat the pain resulting from vertebral compression fractures resulting from osteoporosis or neoplastic invasion. Authors have previously listed avascular necrosis or Kummell's disease as a treatable cause of pain observed in some cases of compression fracture. These were identified usually as having an air-filled cleft, and some were noted to show motion fluoroscopically (4). Motion has been associated with a high probability of severe, persistent pain and very good result with percutaneous vertebroplasty. Pain relief was thought to be due to the elimination of excessive motion after cementing. Often, these patients described pain relief immediately after the procedure. These cases represented a small percentage of the patients evaluated and treated with percutaneous vertebroplasty. Nevertheless, the motion during fluoroscopy and the rapid recovery after percutaneous vertebroplasty make these patients interesting and notable.

High signal intensity zones observed within fractured vertebrae on MR images may represent fluid. This is commonly seen in sub-end plate regions after fracture and less often in the central vertebra marrow space. Some authors have suggested that this may represent the same pathologic entity as air-filled clefts seen on radiographs. Data are available to indicate that this is true in at least some of the cases (3). The MR imaging data are not, however, well correlated with the pathologic findings, and the presumption that all these situations represent the same pathologic cause is not well established.

In this issue of the AJNR, Lane et al describe their findings and the results achieved when attempting to treat "intravertebral clefts" with opacification during percutaneous vertebroplasty. This is a retrospectiverevew and has all the usual built-in problems associated with this type of data analysis. Their longest follow-up period includes only 40% of the patients compared with the starting population. They not only lump together patients with radiographic and MR imaging evidence of vertebral cleft (for which there is some support in the literature, albeit inconclusive) but also patients who seem to have clefts after percutaneous vertebroplasty that were not shown on images obtained before percutaneous vertebroplasty. In almost 36% of the cases designated as including clefts in this article, the clefts were found only after percutaneous vertebroplasty was performed; these were considered to be clefts, without any pathologic proof or previous literature evidence that this is correct.

The authors found that patients with "intravertebral clefts" responded in a similar manner as did patients with compression fracture without clefts. There was no statistical difference at any reference point in their data (although they suggest a *trend* to better outcome in the cleft population during long-term follow-up). There is a small population of patients who had clefts seen on the initial images that were not filled with cement during percutaneous vertebroplasty. These had a poor long-term pain response, suggesting that when a cleft is identified, it should be filled for dependable pain relief.

The results presented in this article are disappointing. Most physicians with substantial experience with percutaneous vertebroplasty would expect a better outcome differentiation between patients with proven intravertebral clefts than those without. It may be that this article does not show that expected difference because it does not exist or because the population chosen for analysis did not actually represent a homogeneous pathologic population. The presumption that clefts found only during percutaneous vertebroplasty are equivalent to those seen on imaging studies prior to vertebroplasty is certainly suspect and possibly inaccurate. Further pathologic data examining these suspected clefts are needed before a firm conclusion can be reached regarding this part of the authors' grouping. Even the suspected clefts in the remaining population, as revealed with both MR imaging and radiography, could be the result of different pathologic processes and could respond variably to percutaneous vertebroplasty.

It does seem evident that when one sees an intravertebral cleft shown by MR imaging or radiography (before percutaneous vertebroplasty), an effort should be made to fill the cleft with bone cement. This usually occurs regardless of where the needle is placed in the vertebral body. Even with the needle placed away from the cleft, cement will usually track to the cleft and fill the region, because there is little resistance to flow into the rarefied zone. In the rare case in which this does not occur, repositioning the needle or repeat injection may be indicated to ensure a final fill that is biomechanically stable and results in good pain relief.

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Diskography in the Popular Press

Now good medical writing isn't limited to specialty journals; one can learn important information about AIDS from an article in *Rolling Stone*, Creutzfeldt-Jacob disease from *Scientific American*, and Alzheimer disease from *Time*. The Sunday *New York Times* runs a feature on chronic fatigue syndrome and then expands it into a book. The *Wall Street Journal* runs a front-page story on the latest anti-cancer drug, and although the article centers on business rather than scientific issues, it underscores the public's concern with medical news. The medical feature article is a sign of our times, from which physicians may be as likely to get updated information on disease and therapies as their patients. Patients can learn of a scientific breakthroughs before physicians if physicians don't have broad enough reading habits. The physician no longer has exclusive access to medical knowledge.

In this editorial, I am turning the tables on the popular press by reviewing a medical feature article that addresses the controversial and complicated subject of low back pain. "A Knife in the Back," by Jerome Groopman, appeared in *The New Yorker* in the April 8, 2002, issue (http://www.newyorker.com). The subtitle, "Is surgery the best approach to chronic back pain?" tips the author's hand, but few physicians would be inclined to disagree with him. The long-held, official position of the North American Spine Society (NASS) is a strong recommendation for initial conservative therapy for any onset of low back pain that does not include significant neurologic deficit. This conservative approach includes the widespread practice of postponing MR imaging until after an unsuccessful 6-week course of medical treatment. It should be emphasized, however, that for a patient with significant neurologic deficit, the NASS recommends immediate MR imaging and, if necessary, surgery. The rare patients with low back pain and loss of bowel or bladder control because of a large extruded free disk fragment should undergo immediate surgery.

Patients with acute low back pain may have an extruded disk compressing or irritating nerve roots or they may have a flare-up of degenerative arthritis. Patients with chronic low back pain may have arthritis or instability. Most patients with acute or chronic low back pain do not have serious neurologic deficits and will respond well to nonsteroidal or steroidal anti-inflammatory medications combined with an appropriate regimen of physical therapy. Most patients do not need to undergo diskectomy for acute low back pain, and, if conservative therapy is going to alleviate symptoms, they may not need to undergo fusion surgery for chronic pain either. In the *New Yorker* article, Dr. Groopman carefully distinguishes this majority of patients with low back pain from the much smaller group who do require immediate surgery for major neurologic defects, unstable fractures, epidural tumor, or spinal cord tumor.

It is appropriate for Dr. Groopman to question why 150,000 spine fusion operations were performed in the United States last year when uncertainty persists whether the procedure is effective or necessary. His inclusion of approximate reimbursement schedules for spine surgery casts a shadow of innuendo regarding motivations for recommending surgery, which may or may not be relevant information when reviewing number of procedures performed. Groopman often veers from objective analysis; however, he makes a good case for a conservative therapeutic approach to low back pain.

Dr. Groopman follows a patient with low back pain from onset to postoperative outcome and his description of a surgical fusion operation in another patient is told in gruesome detail. What is notable, however, is that he devotes comparable space a diskography procedure performed in the former patient before surgery, as he does to the fusion procedure in the latter patient. One not trained in medicine and contemplating undergoing diskography, not to mention fusion surgery, might have second thoughts about both procedures after reading this article.

Although Dr. Groopman's depiction of the diskography procedure evokes images of medieval torture, his description is, unfortunately, fairly accurate. Nonetheless, his reference to the "long metal table" on which the procedure was performed was unnecessarily suggestive of an autopsy table. His repetition of the word "trocar," the needle inserted into the patient's back, appears to be a rhetorical device designed to make this needle sound like a harpoon, amplifying the patient's agony when the physician inserts the instrument into her lower back.

Proceeding with the clinical details, Dr. Groopman chronicles that when an intervertebral disk is injected, the patient "gasps" in pain. We as neuroradiologists know that the diskogram is probably concordant with the patient's symptoms, and we know that this pain lasts only an instant; Groopman omits that information and as a result the pain lingers in the reader's mind. The radiologist's seeming lack of empathy is remitted when he asserts that diskography is his least enjoyable procedure because "patients are intentionally subjected to pain." Then Dr. Groopman launches an accusation: "The results of diskographymay be dangerously misleading." Of course, the results of any diagnostic study can be misleading when in the wrong hands. If a patient reacts in a strongly positive way to injection of disks at every level tested, the performing physician has an obligation to ask why. Is the patient experiencing pain at every level or is this patient's sensitivity an indication of a somatization disorder? Or, is this attributable to flawed technique? As with any procedure, diskography is tinctured with subjectivity; the perception of the performing physician and that of the patient cannot be corroborated, quantified, or reproduced by a third party. Despite the complexities surrounding diskography, an attempt to objectify the patient's response is important and should be recorded as accurately as possible.

Neuroradiologists have come some distance since the American College of Radiology, in a 1978 position paper, stated that diskography was useless. It is possible that more neuroradiologists today than in 1978 would allow that scrupulously performed and selectively applied diskography, in cases in which all other diagnostic findings are equivocal, is useful for revealing a painful disk level. Unfortunately, no researcher has yet been able to confirm this with any long-term, scientifically controlled, reproducible investigation. Many of the long-term, large-cohort studies of diskography in the current scientific literature are flawed, including those cited in Dr. Groopman's article. Most of these investigations cannot be found in the radiology literature but rather in orthopedic or neurosurgical spine journals, even when radiologists author them. With few notable exceptions, radiologists and neuroradiologists have either been too busy condemning or too busy performing diskography to prove unquestionably whether it is worth doing.

In *The New Yorker*, Dr. Groopman not only notes that diskography is controversial but shares the same opinion of all the other aspects of the diagnosis and surgical therapy for low back pain, including the interpretation of MR images, the diagnosis of spinal instability, and the rationale for fusing vertebrae. He correctly implies that all these are controversial, even among spine specialists, implying that diskography is an art rather than a science.

It is probably true that diskography in the wrong hands can be used to support a preconceived notion about the significance of a disk abnormality seen on an MR image (eg, the fabled "black disk"). That is why it is best to have an objective physician, ideally a trained radiologist, perform diskography and convey objective information to the treating physician in a manner unbiased by any clinical features except the patient's isolated reaction during the procedure. The treating clinician should not perform the diagnostic study, just as the clinician should avoid interpreting his or her own diagnostic studies, such as MR imaging findings of the spine. Objectivity is imperative when assessing a disease entity that has so many subjective features.

Objectivity is what the diagnostic radiologist or neuroradiologist brings to the diskogram table. If we are going to be performing more diskography procedures, we must show in our literature that there is a valid use for this often maligned procedure and that it can be helpful in decisions related to the treatment of low back pain, even in the face of doubts expressed in the popular press.

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