

Diagnostic Spinal Ultrasound: Too Good to Be True?

Arthur Croft, DC, MS, MPH, FACO

Evaluate muscles, tendons, ligaments, and bursae. Document nerve root involvement. Demonstrate not only bulge discs, but those actually responsible for the patient's symptoms. Document muscle bleeding and validate your care through follow-up scanning. Identify recent vs. prior injuries. These are some of the claims made in a brochure sent to me by a company promoting diagnostic ultrasound. This profession has been the target of a virtual blitzkrieg advertising campaign and for the last year or so I've been inundated with phone calls, faxes, e-mail, and questions at seminars and conferences. The usual question concerns the validity of these claims.

In recent years I have watched the world literature very closely -- including several journals devoted to radiology and ultrasound diagnostics -- and I've yet to find anything on the subject that fulfills the diagnostic promises mentioned above. I've repeatedly asked for literature that would support spinal ultrasound in the clinical setting, but usually receive only promotional material suggesting, for example, that the Mercy Conference consensus rating is "established." Yet this label is misleading because the Mercy document rating of "established" refers primarily to intra-abdominal use of diagnostic ultrasound and other more established uses, such as for shoulder pathology. No supportive studies were mentioned for spinal ultrasound. In fact, another description provided by the Mercy Conference in connection with this technology was "controversial."

According to one brochure, "Purchasing a diagnostic ultrasound system is synonymous with investing in the future of your practice." That's because there are CPT codes for the procedure and 75 percent of insurers are currently reimbursing for the test, they say. And at \$200 per spinal segment and the recommended follow-up exams on the relatively broad range of promised diagnosable lesions, this machine may take you beyond the dreams of avarice. That is, if the claims are true.

From two different sources now, I've received the identical package of "supportive documentation." I have not performed an exhaustive literature search on the subject, but presumably if any better literature were available, it would have been included. In the first article,¹ the authors investigated the carpal tunnel. We need go no further with this one in our quest for validity of diagnostic spinal ultrasound. The second article is from Japan.² The authors examined 80 consecutive patients with clinically suspected lumbar disc herniation. In 40, the diagnosis was confirmed surgically. Ultrasound accurately diagnosed 78 percent of these confirmed disc herniations as compared to a 90 percent accuracy with CT-myelograms. These figures of accuracy are misleading, however, since there was no nondiseased group subjected to evaluation and, therefore, more meaningful clinical values of sensitivity and specificity cannot be calculated. Also, ultrasound also showed positive findings in 60 percent of the 40 nonoperated patients, and this group does not appear to have been evaluated by the paragon test, CT-myelogram. MRI would have probably served their purposes better. On balance, this paper supports (only weakly), the use of ultrasound as a screening test for MRI or CT-myelography. However, the question of efficacy in that regard is not addressed.

The third study was Finnish³ and compared ultrasound to CT discography in the evaluation of lumbar disc disease. Although there were no asymptomatic persons included in the study, the authors reported a sensitivity of 95 percent and a fairly low specificity of 38 percent. There are several difficulties in interpreting this study. For one, discography is not widely used in this country. Critics of discography often argue that, in addition to its invasive nature, high level of patient discomfort, potentially serious morbidity, and higher cost than CT or MRI, the probability of false positive is very high and may do more to justify spinal surgery in questionable cases than definitively identify problem discs. Again, MRI would have been a better gold standard, and the design of this study allows us only to conclude that ultrasound may serve a purpose as a screening tool for more invasive or more expensive procedures. And again, the issue of efficacy is not addressed in this paper. But there are several other troubling aspects to this paper. As with the earlier study, this one was nonblinded.

The issue of interpreter reliability in spinal ultrasound, to my knowledge, has not been carefully investigated yet must be. And because the mean age of the study group was 40 years, there might have been an additional problem with selection bias since asymptomatic degenerative disc disease is not uncommon in this age group. As in the previous study, there were no nondiseased subjects. (Of course, no review committee would have permitted discography testing of nondiseased volunteers.) Finally, these authors used a transabdominal approach with ultrasound and my understanding is that most practitioners of spinal ultrasound are using a paraspinal approach. The results of this work cannot be used to validate the transparaspinal method.

The authors of the fourth paper⁴ measured the AP diameter of the lumbar spinal canal and compared their data from two groups: 1) subjects who had a history of time loss from work due to low back pain, and 2) subjects with no history of low back pain. The research question was simply whether transparaspinal ultrasound could act as a screening tool for low back disorders. In their study of 16 low back pain sufferers and 33 asymptomatic subjects they found that the mean AP diameter for the total group was 1.68 cm (SD 0.24). The group with low back pain had a mean canal diameter of 1.60 cm, compared with 1.72 cm for the asymptomatic subjects. The difference between these groups was statistically significant ($p=.05$). There are, however, problems with this observation that may not have been considered by the authors. For example, almost half of the back pain sufferers had AP canals of 1.75 cm or more which is significantly higher than the mean value for the asymptomatic group, and about two thirds of the asymptomatic subjects had AP canal diameters of 1.75 cm and narrower. In fact, the mode in this group was 1.50 cm! Therefore, the authors have not really demonstrated any valuable utility in screening workers with ultrasound in an attempt to predict low back pain risk. Moreover, they made no attempt to validate their accuracy with ultrasound by comparing to plain film or CT examinations, but appear simply to have assumed their accuracy based on the report of one previous study by other authors.

On balance, these papers do very little to support the use of diagnostic spinal ultrasound for anything other than screening tools for either CT, MRI, CT-myelography, or CT-discography. CT-myelography is currently dying a slow death in this country as several recent studies have shown that MRI is superior to it as a diagnostic test. Discography is not generally used in this country and is quite painful, invasive, and fraught with serious potential morbidity. At a cost of several hundred dollars per study, it's quite possible that ultrasound would fail to pass muster as an efficacious screening tool for the more accurate and established gold standard of MRI at only a slightly higher cost. As to the question of demonstrating old vs. new lesions, nerve root irritations, facet joint inflammation, and the host of other disorders that ultrasound promoters have promised to show us, there does not appear to be any evidence forthcoming. Diagnostic spinal ultrasound is currently on the highway to diagnostic never-

never land to take its place beside thermography and other technological dead ends. The only way its course will change is if valid scientific studies can demonstrate interinterpreter reliability and then a competitive sensitivity, specificity, and predictive value compared with the gold standard of MRI. Efficacy studies will be needed after that.

Premature and uncritical acceptance of thermography in the 1980s, along with a lack of scientifically sound research, no doubt hastened its decline. It seems quite likely to me that with careful investigation we can determine the clinical usefulness and appropriateness of surface EMG and spinal diagnostic ultrasound before their widespread misuse results in irreparable credibility damage. That's my opinion.

References

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Arthur Croft, DC, MS, FACO
San Diego, California
E-mail: accroft@aol.com

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