

## EVALUATION OF FLUOROSCOPIC CAUDAL EPIDURAL STEROID INJECTIONS

### To the Editor:

We read with interest the retrospective review by Southern et al (1) entitled Are Fluoroscopic Caudal Epidural Steroid Injections Effective for Managing Chronic Low Back Pain? However, we were disappointed at multiple aspects of the study which are enumerated below.

1. Southern et al (1) stated that to date, no study has specifically investigated the efficacy of fluoroscopic caudal epidural steroid injections as a treatment for patients with chronic lumbar discogenic back pain.

In contrast to this statement by Southern et al (1), Manchikanti et al (2-4) evaluated the role of caudal epidural injections in chronic low back pain in two prospective and one retrospective evaluation. Particularly in one study (3), they evaluated the role of caudal epidural injections in discogram positive and negative chronic low back pain, utilizing the International Association for the Study of Pain (IASP) criteria (5). Both the studies were performed in patients without radicular pain. Thus, they would be considered as chronic low back pain. It is quite possible that these patients may have had somatic lower extremity pain, however, not radicular pain. Further, all the patients were excluded for low back pain of facet joint origin with controlled comparative local anesthetic blocks prior to including them in the study for effectiveness of caudal epidural injections. This study showed that patients equally responded with fluoroscopically directed caudal epidural steroid injections in patients with or without positive concordant discogenic pain. Manchikanti et al (4) also retrospectively evaluated fluoroscopically directed caudal epidural injections and compared them with interlaminar and transforaminal epidural injections, the results of which have been quoted by Southern et al (1).

2. One of the inclusion criteria was "predominantly axial low back pain of more than 3 months' duration."

However, the authors have not defined and described the axial low back pain appropriately. Axial low back pain is considered as midline low back pain (does not include paravertebral low back pain), which is extremely difficult to manage (diagnosis and management) (6). It appears that Southern et al (1) have included all the patients with chronic low back pain without radicular pain, but with disc abnormalities. This will translate into not only axial pain, but also the chronic low back pain in the paravertebral region and probably somatic leg pain, as their other criteria for inclusion included clinical presentation and magnetic resonance imaging findings consistent with central lumbar disc protrusion and/or degeneration of L4/5 or L5/S1.

3. The authors have deemed that the caudal procedures failed if the patients underwent subsequent discography and/or surgery after their injection.

The authors have not described if they have evaluated any of these patients for facet joint pain. Lumbar facet joint pain is commonly seen in patients with chronic low back pain, in fact, even more than discogenic pain (7-14). Surprisingly, the authors have offered discography to all the patients if they failed to respond or responded on a short-term basis to caudal epidural steroid injection as the only intervention.

4. In the Methods section, the authors have described that all patients who qualified for the study were mailed a Roland-Morris Disability Questionnaire and a North American Spine Society (NASS) Patient Satisfaction Questionnaire. Obviously, one would believe that these tests were only performed after the caudal epidural injections were performed, as this was a retrospective evaluation and the questionnaires were mailed following the decision to include them in retrospective evaluation.

In contrast, Table 4 shows pre-injection Roland-Morris functional scores in successful and failed groups of patients.

5. It was also surprising that among 4,756 charts reviewed, only 98 patients met the inclusion criteria. This is in fairly sharp contrast to common interventional pain management practices, indicating that the practice may be more of general physiatry practice rather than interventional pain management practice. We wonder if this somehow influences the selection and outcomes based on differing practice variables.
6. Follow-up period was highly variable from 3 months to 61.2 months.
7. The authors also have failed to define what was considered as short-term or long-term relief. It is well known that epidural injections provide short-term relief in a significant number of patients while they fail to provide long-term relief based on the definition of short-term being considered as 3 months or less (15).
8. The authors also have considered that perceived target area of pain and inflammation in this set of patients is the ventral, central epidural space, consequently, a caudal injection providing non-specific ventral epidural flow would possibly be the best choice of epidural steroid injection in this population. The inflammation in the ventral and central epidural space is considered most suspect in patients with radicular pain. In patients without radicular pain, disc degeneration with or without disc herniation can cause low back pain, but also facet joints, SI joints, ligaments, muscles and tendons (16-20). Proposed etiologies of discogenic pain secondary to disc herniation include not only neural compression, but also vascular compromise, inflammation, biochemical and neuromechanisms, internal disc disruption, intraneural

and epidural fibrosis, dural irritation, and inflammation and swelling of dorsal root ganglion. Thus, it is pivotal for the interventionalist to pinpoint an anatomical diagnosis. It is often cited that a cause cannot be determined in 85% of patients with low back pain or conversely, that a diagnosis is possible in only some 15% of cases (15) based upon history, physical exam and common radiological data. Further, radiographic investigations, on which authors heavily depended, including magnetic resonance imaging (MRI), reveal only certain conditions with certainty, namely, neural compression with radicular pain. Thus, the role of precision diagnostic injections has developed in the modern era.

9. The authors also should realize that some studies have used high volumes up to 40 mL in the past to show the effectiveness. Authors demonstrate that a 10 mL injection failed to reach beyond the L5/S1 level in many patients. Thus, the lack of efficacy of 10 mL caudal epidural steroid injection in a specific select population who may also be suffering with facet joint pain should not be construed as evidence that caudal epidural steroids performed would meet with similar failure. Finally, long-term outcome with a single injection is quite unrealistic.

In summary, we believe that this manuscript fails to fairly evaluate effectiveness of fluoroscopic caudal epidural steroid injections in the management of chronic low back pain with proper respect to a solid knowledge base of past literature on the subject (15). Indeed the article does not agree with past well-accepted and peer-reviewed literature. Hence, the authors should emphasize the various shortcomings of this manuscript. In this manner, insurers will not deny access to care for patients who do need caudal epidural steroid injections, thus limiting access to care based on an isolated and limited study. It should also be clearly stated that the authors do not believe that the

findings of this limited study imply that 1) repeated and or 2) high volume and or 3) caudal with catheter procedures in patients 4) with or without positive radicular signs and symptoms and or 5) positive MRI would fail to respond, when as a matter of fact, the literature would support positive outcomes.

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**To the Editor:**

The study by Southern et al, is an excellent contribution to the Pain Physician for several reasons. As opposed to the majority of previous studies, it focuses on the outcomes of caudal epidural steroid injections (ESI) for axial low back pain (LBP) only, excluding patients with radiating pain to the lower extremity.

As most of the practicing physicians acknowledge, it is very difficult to make an accurate diagnosis of LBP. The back area is very poorly represented in the somatosensory cortex of the human brain, and the patient's history and physical exam often fail to pinpoint the exact etiology of pain. However, certain pain radiation patterns commonly match the underlying pathology. Isolated axial LBP is usually seen with facet joint disease, discogenic LBP, sacroiliac joint disease and myofascial pain. Pain radiating to the foot is more commonly associated with lumbar radiculopathy and piriformis syndrome. With so many variables present in pain presentation and in our diagnostic and therapeutic procedures, it seems to be prudent to narrowly focus on the single symptomatology (or, if known pathology) with a single treatment modality when outcome studies are performed.

An excellent previous study by Lutz et al. showed that by narrowing the inclusion criteria (lumbar radiculopathy) and method (transforaminal approach to the ESI), valuable information can be gathered with favorable outcomes (75.4% long term success rate)(1). A subsequent prospective randomized controlled study by Vad et al. used the same methodology and replicated the Lutz et al. findings with even better outcomes (2).

To date, there are no reports evaluating outcomes of ESI for axial pain only. Some previous studies may have included the subjects with axial back pain in their data analysis, but failing to separate them from subjects with radiating pain to the leg. The potential use of ESI for the treatment of axial LBP can be very appealing. Steroids placed in the epidural space may easily spread to the potential pain generators (posterior disc annulus, posterior longitudinal ligament, facet joints) and therefore inhibit its inflammation and nociceptor discharge. The ESI can be used as a first line of therapy for axial LBP if conservative treatments have failed (physical therapy, NSAID's). The patients (even a small percentage) who benefit from ESI's may avoid the complexity of other

diagnostic/therapeutic procedures such as diagnostic medial branch blocks, radio-frequency lesioning, discography and Interdiscal Annuloplasty (IDET).

However, the article by Southern et al. failed to show that outcomes with caudal ESI for axial LBP are better than the expected placebo response (although the patient satisfaction with the procedures was much higher than the improvement in pain). Although these results are not encouraging, they do not exclude the potential use of ESI as an initial treatment for axial LBP. The article by Southern et al. did not rule out that some of the study subjects may have a component of facet joint or sacroiliac joint pain. If these subjects were excluded better outcomes may have been achieved. On the other hand, although anatomically less likely, it is also possible that patients with other types of axial LBP may respond well to ESI. There are no studies on ESI for facet joint pain published so far.

An excellent point in this article is that the authors used only one technical approach to the epidural space. As many recent studies showed, it seems that technical aspects of ESI's may affect the outcomes. There is a growing acceptance of the routine use of fluoroscopy for ESI among the majority of practitioners. However, the recent national survey showed that there is still no national consensus on the technical aspects of ESI (3). It seems that there is no "best" way to perform the ESI, rather that a certain technical approach may better suit a certain pathology.

It is not clear that the caudal approach is the best choice for targeting the axial LBP. Although some studies support the use of the caudal approach (4) for various kinds of low back pain, others found it inferior to the translaminal (interlaminal) approach (5). Critiques of the caudal approach often state that because of higher volumes of solution used, it diminishes the amount of steroids that reaches the site of pathology. The potential scar tissue formed by inflammatory mediators may further diminish the spread of steroids to the targeted pathology, if administered at a distant site. The article by Southern et al. assumes that the caudal approach provides an excellent ventral epidural spread where the pain generator (annulus fibrosus, posterior longitudinal ligament) may be located. To date, there are no published studies looking at the ventral epidural solution spread with caudal ESI. The previously determined ventral spread for the translaminal approach was 24% for thoracic ESI (6), with a similar percentage found in cervical

levels (7). When examining these facts, one may raise the question if the potential outcomes of ESI for the treatment of axial LBP may improve with a translaminar or bilateral transforaminal approach. The potentially more accurate placement of medication may also allow for lower volumes and a more accurate placement of steroids to be used. Even a smaller dose of steroids can potentially be used with the same effect, allowing for more often repeated ESI without the fear of systemic effects of steroids. Further prospective, randomized double-blinded studies may be needed to answer these questions.

The need for more studies does not diminish the value of the article by Southern et al. On the contrary, this study shows the importance of narrowing the inclusion criteria when outcome studies are conducted. In future studies, by minimizing the variables, we may be better able to determine what is the proper treatment option for certain forms of LBP.

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## CONTROVERSY ABOUT PSEUDO-KÜMMELL'S DISEASE

### To the Editor:

The authors of this manuscript attempt to coin the term "pseudo-Kümmell's" by virtue of an intra-operative observation during a vertebral augmentation procedure. Their suppositions and implications are not supported by the evidence they present in the manuscript, and could adversely impact the understanding of the anatomy, physiology, and surgical technique by those who have less experience employing vertebroplasty. Three issues; vertebroplasty risks and cement leaks, indications for vertebroplasty, and the physical action of pumping air into a vertebral body, as mentioned in the manuscript, are deserving of comment.

The authors accurately list, but only gloss over the contraindications to vertebroplasty. It appears the authors marginalize pre-existing literature that reveals that vertebroplasty is associated with significant risks including cement leaks up to 70% of the time, causing nerve root compression, cement embolization to the lungs, and even monomer related respiratory and cardiac depression leading to death (1-6). In a similar manner, the authors do not adhere to the indications for vertebroplasty. Indeed, they describe how this patient was not a candidate for traditional surgical intervention by virtue of her debilitated state, anatomic alignment, and bone retropulsed into the spinal canal. They however fabricate a new term "high risk vertebroplasty" for which they imply she was a candidate.

From a biomechanical standpoint, it has been established that vertebroplasty makes no attempt to restore the height of the collapsed vertebral body (7). It is also

well established that some height restoration can be achieved simply by positioning the patient prone on bolsters (8). In the case of "Kümmell's," "Dynamic Mobility," or a "true pseudoarthrosis," the positioning will open the gap and effectively appear as a cystic cavity. The implications of this report are very dangerous. The fact that air entered the vertebral body establishes this as an unrecognized pseudoarthrosis which expanded by the mechanics of the prone position as well as exposure to atmospheric pressure after cannulation.

From a physiologic standpoint, this manuscript raises another disturbing issue related to the pumping of air into the vertebral body. Venous air embolism is well documented in the spinal surgery literature. A negative intra-thoracic pressure with inspiration increases the venous return and with the venous sinuses of the vertebral body exposed pumping air into the vertebra may result in a life threatening venous air embolism. This action of pumping air into a vertebral body can in no way be justified on the basis of known physiology.

The fact that the patient sustained no adverse reaction consequent to injecting air and cement into a vertebra plana with retropulsed bone into the canal does not create or justify an indication for vertebroplasty under these conditions. The fact that the authors observed air in a gap in the vertebral body does not justify the introduction of a new descriptive term of "pseudo-Kümmell's."

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**To the Editor:**

The authors describe an advanced vertebroplasty technique in a very complex patient. This scenario provides an opportunity to discuss relevant issues regarding osteoporotic compression fractures (OCF) and subsequent management. Why undertake the challenge for "Rescue" in this medically compromised patient population? Recent literature testifies to an impaired pulmonary status (12), loss of function (3), and increased mortality rate versus age matched controls (4). Furthermore, even "silent" fractures lead to compromised function (5). These facts presented behind the safety of a medical journal still do not match the dramatic intensity of the singular patient presentation in our offices. This pain described as "stabbing, sharp" with any transitional motion leads to significant debilitation and demoralization (6). "I would rather die than live like this" is a commonly expressed sentiment. Documentation of this patient's clinical condition with Verbal Pain Scores, medication record, and functional impairment would "paint" a more accurate picture of the argument for a highly invasive treatment. I wasn't aware that treatment of painful vertebral fractures with multivitamins and calcium constitutes "aggressive" non-surgical management.

The other pertinent issue is the actual OCF treatment in the fact of neurologic compromise. What are the risks of performing vertebroplasty in a fracture with demonstrated retropulsion and "cord compromise?" The potential for spinal cord or nerve injury is well-publicized with more frequent case reports (7-9). Cement extravasation may occur by various routes, including basivertebral veins, segmental veins, or cortical defects (10). Therefore, the injectate can cause local and/or distant complications depending on the carrier pathway. Recent dramatic case reports have highlighted the devastating results of "cement emboli" (11, 12) and leads me to question the wisdom of intervertebral "air" injection. While retropulsion is not specifically addressed in the literature, other identifiable risk factors for cement migration include: target level above T-7, larger injectate volume, and "injector" use (13). The authors wisely immediately sought Neurosurgical opinion. However, the examining surgeon obviously felt the risks of a surgical approach outweighed the potential benefits. The authors

then scheduled the procedure with the Neurosurgical availability demonstrating optimal perioperative planning.

The anatomic challenge of vertebroplasty in the vertebrae plana is well-documented and initial reports advised against treatment in this setting. More recently, the literature has given some guidance in these challenging fractures (13, 14). The authors describe a thoughtful ingenious approach with the guide-wire technique. In addition, the utilization of a bilateral approach highlights due caution with angle entry in placing the vertebroplasty needles. The wrong angle would lead to cord violation! Needle placement was optimally placed in this case in the anterior third of the vertebra. Next, "gradual" cement injection was performed with "live" fluoroscopy. PLEASE NOT GRADUAL! Most complications from vertebroplasty are "cement" related! These include: undesirable migration (vascular-emboli!) or posterior (cord compromise) and allergic reaction.

Lane et al (15) retrospectively examines the prognostic importance of fluid and gas-filled clefts on vertebroplasty and concludes that greater pain relief is accomplished with "cleft-filling." The peer-reviewed analgesic efficacy of vertebroplasty has been well established and lead me to question the introduction of a potentially dangerous variable (air injection) in a high-risk situation. Had other more conservative measures been explored? We have had good experience with the Gray Ramus nerve block and denervation at our institution and this procedure does not carry the same potential risk of cord injury (16). Neuroaxial narcotic therapy might also be a less invasive option. The intervention planned should be balanced against the operator's skill, procedural risk, and expected outcome. The authors are to be commended for their technical skill, ingenuity, and prudence for neurosurgical back up.

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**IN RESPONSE****To the Editor:**

I read with interest Dr. Yeung and Lieberman's response to our case report on Pseudo-Kummel's Disease. I respectfully take issue with much of their commentary.

As a practitioner of both vertebroplasty and kyphoplasty, I have always been impressed by the degree to which both procedures can help treat, previously refractory, debilitating compression fractures.

The authors discuss the possible complications associated with vertebroplasty. Percutaneous vertebroplasty is a well-established procedure that has a low complication rate. As with any invasive procedure there are risks involved. The authors insinuate that complications arise in the majority of vertebroplasties performed when they discuss a 70% cement leakage rate. Cement leaks do not occur that frequently and in most series are reported as occurring much less commonly than that. In the early vertebroplasty lit-

erature, cement leaks were termed technical complications in an attempt by practitioners to be complete in their reporting. These are almost never symptomatic and the discerning observer will recognize that they occur during both vertebroplasty and kyphoplasty.

The authors further comment that the patient is not a traditional vertebroplasty candidate. Following evaluation by this author, the patient was referred to a seasoned neurosurgeon with expertise in spine. It was on the basis of his feeling that the patient was not a surgical candidate and encouragement to move forward with vertebroplasty that we considered proceeding. Because the procedure was higher risk than usual, we explained to the patient/family that our typical complication profile was not necessarily applicable in this situation. As such, we termed the procedure high-risk vertebroplasty. Pushing the boundary of accepted indications is done frequently when it is in the best

interest of the patient. The caveat is that the reason the boundary is being stretched is made clear to the patient and that they wish to proceed in light of all the facts.

As a practitioner of both vertebroplasty and kyphoplasty, let me assure the authors that routine cannulation can introduce air. This is actually more typical of kyphoplasty with its definitional creation of an air cavity and multiple exchanges. I sought to put a name on the observation and for the reasons outlined in the paper termed it Pseudo-Kummel's disease. The authors of the commentary are free not to use this term.

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## SEE ONE, DO ONE TEACH ONE

### To the Editor:

Recently I had the opportunity to speak at grand rounds at the institution where I performed my residency. I was thrilled to be able to return to talk with the people that gave me a chance to enter the field of my desire and give back to the present residents. My talk utilized the previous medical standard, dual slides. Although I was a bit under the weather, I tried to be as entertaining as possible, moving around not staying behind the lectern, and speaking to the audience. The residents seemed underwhelmed, however, the physical and occupational therapists along with the attending physicians asked excellent questions engaging me in a educational post-presentation discussion. My perception that the residents were not invested was underscored by their lack of participation as neither a question nor a comment was offered by them.

I'm now preparing a talk for an internal medicine board review course and have been informed that my slide show is no longer acceptable; I must join the new millennium and convert to cd-rom disc. I discussed this with the leadership, as I am out of town (event coverage), alternative methods of presentations that would be dynamic in nature, including utilizing a white board. Alas, only a disc formatted presentation is acceptable. Fortunately for me the original disc from which I made my original slides was found, converted and is now ready to go. I've bought a digital camera and have been obtaining images daily. But, my initial thoughts of what makes a good, memorable presentation that other physicians can do more than spend an hour or so of their time just for a pearl or two, or even worse a "take home message"?

My third year of medical school started out much like everyone else's, with internal medicine. My resident showed me how to obtain an EKG with the old style single lead machine, and then I was sent to do one on my own, then to show another medical student. Even though it's been some time, I think I can still obtain one appropriately. I am not sure many of the lectures that I have attended, regardless of how well presented, have been

committed to memory. "Take home messages" do resound in my mind, but is this the best or even a reasonable approach to medical education? Especially as I have been reminded, the amount of tuition that attendees are asked to pay for a conference or lecture.

In 1998, Freedman and Bernstein (1) looked at the amount of musculoskeletal training and knowledge that interns had as they entered their training program at a large university hospital. They noted that these highly prized physicians, many of whom graduated at the top of their medical school classes had only a limited exposure to formal musculoskeletal training (2.1 weeks, 33% no exposure at all) and limited knowledge of seemingly basic conditions and treatments. Moreover, it didn't seem likely that those in primary care fields would get much more training. Two follow-up studies by the same group asked program directors of both orthopaedic surgery ( $n=124$ ) and internal medicine programs (240 out of 417) to not only give this test to incoming residents but, also asked questions to the directors with regards to fairness of question and what would be appropriate passing scores by percentage. Both director groups chose rates of about seventy percent and in fact 78 percent of new interns flunked by a wide margin (mean score 59.2%). Thus, the authors have concluded that more musculoskeletal instruction is needed at the medical school level (2,3).

Many of us are involved with student, resident and or fellow education. Anecdotally, it can be agreed upon that there is no substitute for experience. Those of us who work on, subscribe to, or casually read this journal could agree that we're better clinicians now than five or ten years ago. We, the current generation of clinicians have a wide array of technology available to help diagnose and treat our patients, which only a generation ago seemed like fantasy. How will this technology boom take medical education to a higher level? What will it substitute for? And will technology allow a mundane topic or speaker to become more dynamic by a more interactive ebb and flow of information?

There's been more than one didactic session that despite the speakers eloquence and true zeal for the subject matter that I have either tuned out to or forgotten over time, most likely since I have no current use for subject matter in a daily basis. But, for all the lectures that I did find an enjoyable learning experience, I recall hours of independent study that many of my classmates foresaw by not attending the lecture altogether.

Our residencies and fellowships serve as apprenticeships from which, we hope modern masters of medicine will be forged. Although I read more during my fellowship than at anytime since second year of medical school, the application of that knowledge on a daily basis is what led to retention of that knowledge. How then do we continue the traditions of medicine in the twenty-first century? I do not know the answer, but now is the time that we all should come together to help educate the next generation. You don't need any fancy, state of the art electronics, just your mind and your heart in the right place.

Sometimes, just letting another attending physician, resident, student, or therapist experience what you do on a daily basis can help the field tremendously. The time spent with you will do more than any lecture could. We need to instill in our newer colleagues not only the thought of becoming an interventionalist, but to allow those that do not desire such a practice the opportunity not only to see but to also understand the true benefits of your practice.

### REFERENCES

1. Freedman KB and Bernstein J. The adequacy of medical school education in musculoskeletal medicine. *J Bone Joint Surg Am* 1998; 80:1421-1427.
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3. Dicaprio MR, Covey A, Bernstein J. Curriculum requirements for musculoskeletal medicine in American medical schools. *J Bone Joint Surg Am* 2003; 85:565-567.

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