

Personally, I would prefer to give mild sedation to these patients. Perhaps propofol would be better as it is quick on and off. Midazolam hangs around too long and has benzo effect on muscle tone.

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In Response:

We appreciate the comments of Dr. Jasper. It is interesting to note that the same reference was communicated to the undersigned by another interventional pain physician. However, I was unable to find this publication mentioned in the ISIS newsletters and it has never been published in any peer review literature. At the present time, we are in the process of designing a protocol to evaluate the effect of 2 mg of midazolam compared to placebo in decreasing

pain and increasing activity status.

However, in this study only low dose midazolam was administered and no fentanyl or propofol was administered. Obviously, fentanyl could cause significant analgesia, however, I am not quite sure if midazolam will have an analgesic effect even though it may have muscle relaxant effect.

We do not believe that the studies need to be repeated just because we used a small dose of midazolam to sedate the patients. As Dr. Jasper mentions, it is preferable to give mild sedation to these patients. We are not comfortable to provide with the use of analgesia for simple procedures such as facet joint nerve blocks. Cost also may become a factor. We have not observed any significant side effects with midazolam.

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Algorithms for Interventional Techniques in Chronic Pain

To the Editor:

I would like to comment on some aspects of recently published algorithms for diagnosing and treating back pain entitled Interventional Techniques in the Management of Chronic Pain: Part 2.0 by Manchikanti L, Singh V, Kloth D et al (*Pain Physician* 2001; 4:24-96). There was placement of discography prior to lumbar facet injection. I believe that this should be changed. Lumbar facet injection may be done prior to lumbar discography and in most instances may be preferable to that. Perhaps, in the algorithm it should be redrawn so that in the decision making process, whether you are going to a discogram or a facet injection, could be decided based upon clinical exam rather than making it an absolute algorithm to first do a discography and then facet injection if necessary.

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In Response:

We appreciate comments by Dr. Prunskis. However, we believe that this is a misunderstanding on his part. All the algorithms clearly state that these are suggested algorithms for the application of interventional techniques in the conservative care of chronic spinal pain. Thus, there is no absolute algorithm to first do a discography and then a facet joint injection if necessary.

As stated in the abstract, these guidelines do not constitute inflexible treatment recommendations. It is expected that a provider will establish a plan of care on a case-by-case basis, taking into account an individual patient's medical condition, personal needs, and preferences, and the physician's experience. Therefore, based on an individual patient's needs, treatment different from that outlined here could be warranted.

We reviewed both the algorithms, figures 3 and 4, once again for the purpose of clarification. We are printing here enlarged algorithms as figure 3a & 3b for figure 3 and 4a & 4b for figure 4. Figure 3, which shows a suggested algorithm for the application of interventional techniques in the conservative care of chronic spinal pain: figure 3a

describes a patient with radicular pain post surgery; figure 3b, without surgery. As described in 3a, a patient after surgery receives transforaminal or caudal epidural steroid injections initially, followed by percutaneous non-endoscopic adhesiolysis if necessary. However, after the failure of endoscopy and if the patient's symptomatology indicates

radicular pain, the algorithm suggests to proceed with discography. If the discography is proven to be negative, then the algorithm suggests to follow the somatic pain algorithm (from figures 4a & 4b describing the process) to rule out or rule in facet joint mediated or sacroiliac joint mediated pain.

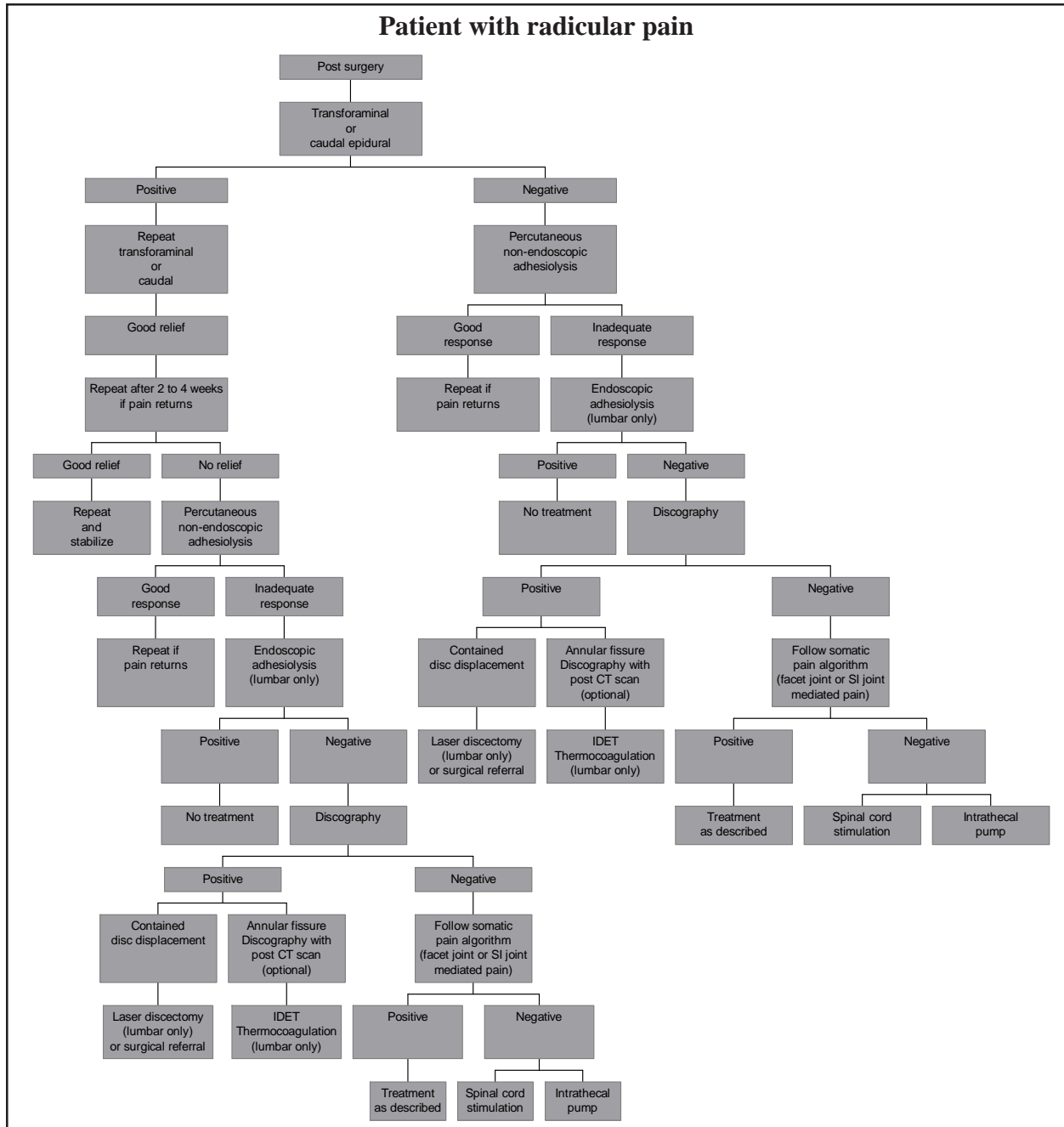


Fig. 3a. A suggested algorithm for application of interventional techniques in conservative care of chronic spinal pain: A patient with radicular pain

The same argument applies to patients presenting with radicular pain without previous surgery where they are treated initially with epidural injections followed by non-endoscopic adhesiolysis followed by discography if necessary and to be followed by the evaluation for facet joint mediated pain. In both algorithms, facet joint mediated

pain is evaluated only as a last resort. As Dr. Prunskis knows extremely well, we are focusing here on the presence of radicular pain. Authors of the guidelines believe that this is an appropriate algorithmic approach. Once again, it is not written in stone, this is only a suggested algorithm.

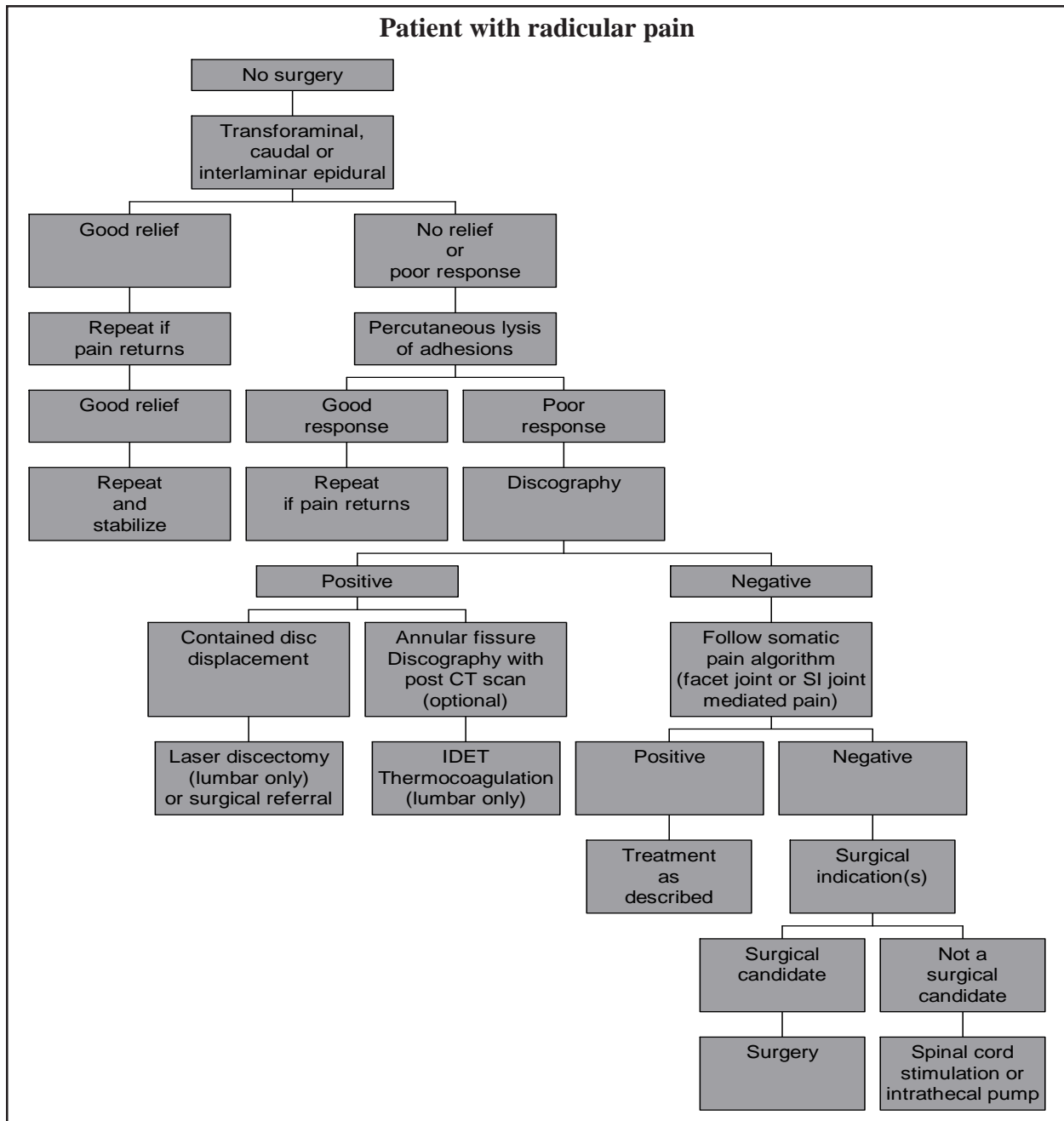


Fig. 3b. A suggested algorithm for application of interventional techniques in conservative care of chronic spinal pain: A patient with radicular pain

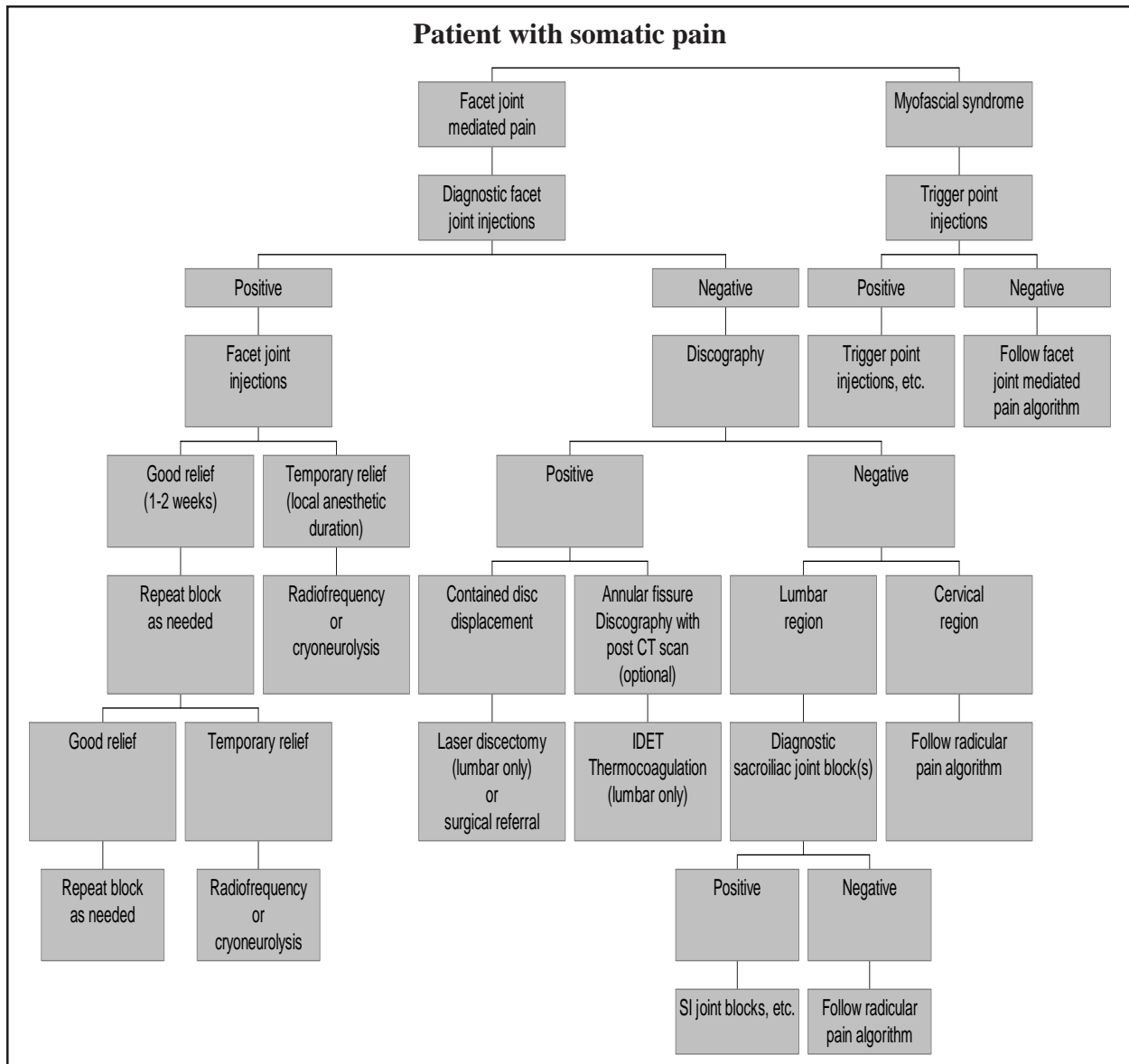


Fig. 4a. A suggested algorithm for application of interventional techniques in conservative care of chronic spinal pain: A patient with somatic pain

Figure 4 magnified into figures 4a & 4b, clearly shows the evaluation for facet joint mediated pain, myofascial syndrome, discogenic pain, and sacroiliac joint mediated pain. When the first algorithm was proposed, it was designed to rule out facet joint mediated pain in each and every patient. However, criticism mounted against that approach. Hence, the middle of the road approach was taken, giving significant latitude for clinician's preferences and patient's symptomatology.

Finally, the authors would like to point out that there are differences between somatic and radicular pain, as not all

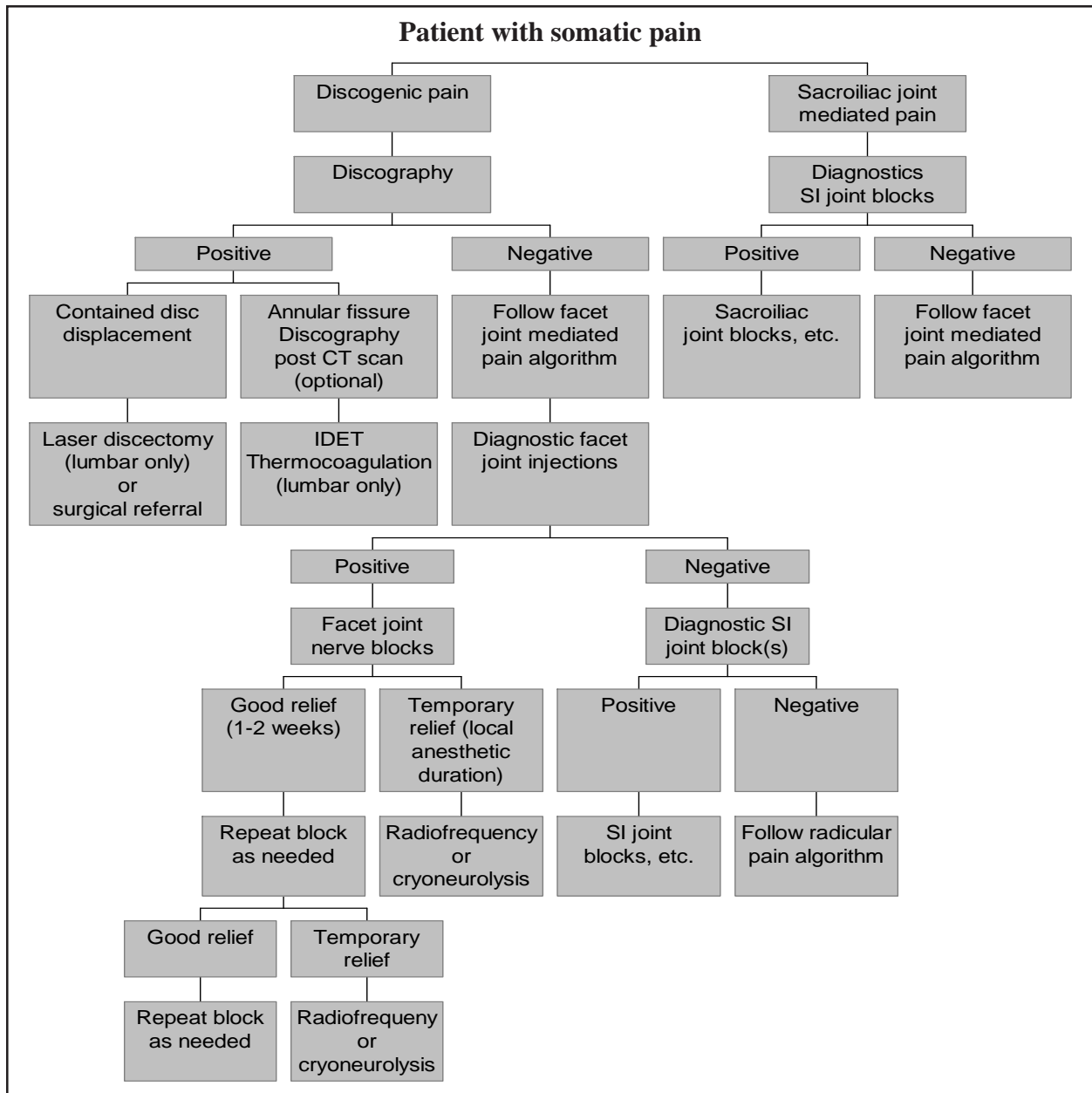


Fig. 4b. A suggested algorithm for application of interventional techniques in conservative care of chronic spinal pain: A patient with somatic pain

leg pain is radicular. These differences are illustrated in table 4 by describing features of somatic and radicular pain where somatic pain is considered as emanating from facet joints, sacroiliac joints, muscles and ligaments, and from the disc with internal disc disruption without herniation. In contrast, radicular pain was shown to originate from disc herniation, spinal stenosis or annular tear. Various differences with the presentation of symptoms and signs

were also described.

The abstract describes the purpose of the guidelines and flexibility of the guidelines. Hence, we believe that the algorithms are appropriate and also that they are clearly described as suggested algorithms.

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Sacroiliac Joint Syndrome

To the Editor:

I enjoyed reading the April 2001 issue of *Pain Physician*. I have a few comments.

The Sacroiliac Joint Syndrome (SIJS) article by Slipman et al (*Pain Physician* 2001; 4:97-100) was informative and interesting. However, I still wonder if we do have a "gold standard" with intraarticular injection. The volume of local typically used makes the block non-specific to joint, ligament or perhaps even nerve branches. Confirming intraarticular placement is frequently questionable. The closest it comes to this "gold standard" is after facets and discs have been excluded. After Caragee's debatable findings of false positive discs with iliac crest pain generator could there be false positive discs for SIJ generator or visa versa? A "gold standard" is a test that to me has a very high true positive rate and very low false negative rate. At this point it doesn't appear that there is significant confidence in intraarticular SIJ inj.

The article on the cervical selective nerve root blocks (SNRB) in whiplash by Slipman et al (*Pain Physician* 2001; 4:97-100) was interesting. It must be read with caution. In the wrong hands this article is going to be concluded to read "there is no point performing SNRB/TF ESI on whiplash patients. In reality, the conclusions is that "in the absence of MRI findings of disc pathology or objective signs of radiculopathy, SNRB/TF ESI is ineffective for whiplash patient. It is still a highly effective treatment for patients with true radiculopathy and in patients with disc herniations."

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In Response:

Dr. Jasper raises numerous issues about two articles published from the Penn Spine Center, which are best addressed in a sequential manner. Due to space limitations I will respond to most but not each of the non-sequiturs.

It is reasonable to state the diagnostic sacroiliac joint block currently is the "gold standard" diagnostic tool from which a diagnosis of sacroiliac joint syndrome can be made. Such a notion is supported by conclusions formulated after examination of two independent issues. First, there is neither another diagnostic tool including history, examination, imaging nor intraoperative findings that provides better information to make this diagnosis. Second, therapeutic outcomes of a specific intervention for this disorder can be determined as they relate to a single or double diagnostic block response. Therefore, even in the event that this diagnostic study is not perfect, rational treatment decisions can be made and prognoses offered. Such analyses has led to the widespread acceptance that diagnostic sacroiliac joint block is the gold standard test. Obviously this analysis does not imply that it is an ideal test or that a better diagnostic tool is not desired. A good analogy would be the state of care provided to patients on the basis of myelogra-

phy before the advent of CT scan or MRI. Rational decisions were made incorporating an understanding of the limitations of that diagnostic tool. By the way, despite what the term “gold standard” means to Dr. Jasper, for the rest of us it denotes the diagnostic test against which all other diagnostic tests/interventions are judged.

While I appreciate Dr. Jaspers difficulty confirming intrarticular placement, and I am assuming he refers to local anesthetic flow rather than needle tip location, this ought not lead to the erroneous conclusion that all spine interventionalists are confronted with the same problem. During my nearly 10 year experience teaching extremely bright and gifted interventional physiatric fellows this has been a common problem for each of them during the first 3 to 9 months of training. Upon learning the variety of approaches for SIJ injection this issue of “am I in?” becomes a non-issue.

Dr. Jasper queries whether there are patients with intradiscal pain who may have reported false positive SIJ block responses. It is unclear why this question is raised, since it was answered quite clearly in our article. The answer is yes! Another reading of the paragraph concerning our diagnostic and therapeutic algorithm addresses this concern head on. In fact, our algorithm is derived to specifically deal with that potential problem.

Dr. Jasper suggests that our article on whiplash induced cervical radicular pain could be misinterpreted by an unwary or unsophisticated individual to erroneously conclude

that Selective Nerve Root Block is ineffective for this particular disorder. His concern stems from his belief that it is an effective treatment for patients with “true radiculopathy and disc herniations”. We never made a final judgment that this procedure is ineffective for whiplash induced cervical radicular pain. Rather we reported our preliminary results and emphasized the need for further study. Such a conclusion was made because of our small sample size and the absence of a control population. However any reasonable physician should surmise that a procedure with less than 20% good and excellent results does not seem to be a useful intervention in the patient population we analyzed. I understand the frustration experienced when scientific inquiry does not provide factual support for treatments we would like to offer patients despite our firm, myth based, convictions. Yet, it is our ability to garner and assimilate this information in a manner that alters our evaluative and treatment processes that distinguishes physicians from gurus. We made no commentary concerning this procedure for patients with symptoms of a herniated nucleus pulposus. Although, we believe such injection is appropriate for this disorder no study has proven this assumption.

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