Dynamic Chiropractic

EDUCATION & SEMINARS

Spine Lesions as the Most Common Source of Posttraumatic Pain: An Institutionalized Fallacy

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In July 1993, the *American Journal of Clinical Chiropractic (AJCC)* printed a letter I had written to its editor.[1] The letter initiated a published debate between several other chiropractic educators and myself. The debate highlights a common mistake of medical and chiropractic researchers and educators - the false interpretation of study results. That debate is important to bring up again, for the false interpretation now threatens to diminish the quality of patient care for many chiropractors. Over the last nine years, the false interpretation has become imbedded as scientific fact in the belief system of some influential chiropractic educators, who have, in turn, integrated the false interpretation into the belief system of many practicing chiropractors, contributing to a mistaken basis for clinical decisions.

In my letter to the editor, I praised *AJCC* for Dr. Michael Schneider's new column on myofascial therapy.[1] I emphasized the importance of such a column: "Evidence is strong now," I wrote, "that the myofascial tissues mediate much - maybe most - musculoskeletal pain, paresthesia, and dysfunction."

When the journal published my letter, adjacent to it was a reply from Dr. Don Harrison, the publication's editor: "Two recent medical studies," he wrote, "*conclusively prove* 50 percent of low back pain (LBP) arises from disc proprioceptors, and 25 percent from facet capsular ligament proprioceptors. So, I guess 25 percent of the pain is from myofascial origins."[3] (Italics mine.)

I proceeded to read the low back studies Dr. Harrison referred to, and I also read similar studies of chronic, posttraumatic neck pain (PNP). My reading made it clear that in his reply, Dr. Harrison had wrongly interpreted the results of the low back studies. In addition, to my astonishment, the researchers who had conducted the studies made the same error. The researchers had convincingly shown that *among the patients they studied,* lesioned discs or zygapophyseal joints (z-joints) mediated most chronic LBP and PNP. They had not, however, "conclusively proven that 50 percent of (all) low back pain" or that most chronic PNP *in general* arises from lesioned discs or z-joints. The researchers, and Dr. Harrison in turn, had mistakenly extrapolated from the study patients to the entire universe of patients with chronic LBP and chronic PNP.

To avoid confusion, let me make the distinction between two propositions.

1. Lesioned discs and z-joints are the most common source of pain among *all* chronic, posttraumatic pain (CPP) patients. For various reasons, I believe this proposition is false, but I am not arguing its truth or falsehood in this article.

2. Results of the studies Dr. Harrison cited, and results of similar studies of neck pain patients, are sound, logical grounds for reaching a conclusion - that lesioned discs and z-joints are the most

> common source of pain among *all* CPP patients. I am arguing in this article that this proposition is false - the study results do not logically justify this conclusion.

This type of mistaken interpretation of study results is a clear example of the logical fallacy called "faulty generalization." My argument may seem to some readers to be hairsplitting pedantry, but this is not the case. Some researchers who originally made this particular faulty generalization have repeated it in recent publications,[6,17] and so have prominent chiropractic educators.[18,16] Their repetition of the faulty generalization will encourage some chiropractors to make inappropriate treatment decisions. Because of this, I am compelled - after eight years of silence on the issue - to again point out their error and to further clarify it.

Impressive Study Procedures

The researchers who misinterpreted the results of their own studies used impressive procedures to learn the sources of their patients' pain. The patients in the studies were all conscious and underwent versions of the same procedure. The researchers injected the patients' discs and z-joints with a solution that distended them. If the distention caused local or referred pain, the researchers injected an analgesic to stop it. Here, the researchers reasoned correctly: If distending a tissue worsened a patient's pain, then stopping the pain by injecting an analgesic solution confirmed that the tissue was responsible for the patient's CPP.

Results of the studies showed that lesioned discs or z-joints mediated the CPP of most patients included in the studies.[8,9,10] A sound generalization from these results would be that similar spinal lesions are likely to mediate the CPP of patients who are *similar to the study patients in all relevant respects.* However, the researchers and Dr. Harrison generalized even to patients who differed in a highly relevant respect: These patients - the majority with CPP - respond so well to conservative care that their clinicians have no need to refer them for invasive diagnostics. Generalizing to these patients is clearly logically unsound.

Understanding the Faulty Generalization

The spinal studies were important in identifying tissue sites that mediated the pain of a certain population of CPP patients - *those who failed to benefit from conservative care and were referred for invasive diagnostics.* This distinct group of study patients differs from another population of CPP patients - *those who recovered from their pain or improved enough with conservative care that referral for invasive diagnostics was unnecessary.*

Yet Drs. Nikolai Bogduk[8,9] and Stephen Kuslich,[10] prominent spine researchers who conducted some of the studies, failed to note this distinction between the two groups. As a result, they misinterpreted their study results. For example, Bogduk wrote: "There are no grounds for suspecting that the patterns of response exhibited by the (study patient) sample should not reflect what would be expected in patients, at large, with posttraumatic neck pain."[8] Essentially, his generalization states that the high percentage of lesioned discs and z-joints among patients resistant to conservative care are likely to also be found in patients responsive to conservative care. Bogduk's extrapolation of his finding to patients whose therapeutic responses differed from those of the patients in his study is invalid. In other words, these study findings, specific to a particular group, are not a sound basis for concluding that, as a *general rule,* spinal lesions are the major cause of *all* CPP. In patients who respond to

conservative care, discs and z-joints may seldom be involved.

On the other hand, we do not know conclusively that most posttraumatic pain (PTP) patients - even those who respond to conservative care - do *not* have lesioned discs and z-joints. Researchers have not done studies to learn the incidence of such lesions among patients who respond to conservative care. Dr. Bogduk agrees. I recently posed several questions to him: "I am concerned," I said, "about what we can validly extrapolate to the *general population* of pain patients from the result of your studies. My impression is that your study patients had failed to improve with conservative care, and, because of this failure, they were referred for invasive diagnostics. If this is true, is it likely that the lesioned discs and z-joints you found in these patients explain why they failed to respond to conservative care? Is it possible, then, that the lesioned discs and joints distinguish these patients from those who respond to conservative care? Similarly, do we have to allow for the possibility that other types of lesions may account for the pain of patients who do respond to conservative care?"[19]

Bogduk replied, "We do not have data by which to answer your question."13 His reply shows that objective evidence was not the basis of his conclusion that what he found in his study patients was also true of the entire population of PTP patients. Instead, his conclusion about these latter patients was a preassumption - one that ignored the obvious difference between these patients and the ones he studied.

The logician Irving Copi explained the logical error. Within this quote from him, I have interjected bracketed statements to show how his explanation applies to the issue at hand. Copi wrote: "In seeking to understand and characterize all cases of a certain kind [patients with CPP], one can usually pay attention to only some of them [those referred for invasive diagnostics]. But [if we are to extrapolate the results to the whole class of CPP patients] those examined should be typical rather than atypical [should not be different in that clinicians referred them for invasive diagnostics because they failed to respond to conservative care]. If one considers only exceptional cases [patients referred for invasive diagnostics] and hastily generalizes to a rule that fits them alone, the fallacy committed is that of converse accident [another term for faulty generalization]." Copi gave examples of this logical fallacy: Someone may falsely reason that because narcotics relieve the suffering of seriously ill patients, narcotics should be available for anyone who is suffering. Similarly, one might wrongly conclude that because alcohol intake causes some drinkers social and health problems, all people who drink alcohol will have social and health problems.[12]

Chiropractic Educators' Repetition and Defense of the Faulty Generalization

Despite the researchers' faulty generalization, Dr. Harrison firmly pronounced that their findings were "conclusive proof" that spinal structures are the major source of LBP.[3] He made no distinction between patients who respond to conservative care and those who do not.

After reading Dr. Harrison's reply to my letter, I wrote an article for the October 1993 issue of *AJCC*.[2] In it, I argued that lesioned myofascial tissues are a more common source of musculoskeletal pain. But more to the point, I also highlighted the logical error of researchers such as Bogduk and Kuslich.

In the April 1994 issue of *AJCC,* Dr. Daniel Murphy described the 1993 Bogduk study, writing: "A conclusion from this study is that *in patients with chronic posttraumatic neck pain,* zygapophyseal joint pain is slightly more prevalent than disc pain, but that usually both a symptomatic disc and

zygapophyseal joint will be found."[7] (Italics mine.) Like Dr. Harrison, Dr. Murphy failed in this statement to distinguish between the two groups of patients. Clearly, the italicized phrase above extrapolates the study finding to *all* CPP patients.

In January 1994, Dr. Steve Troyanovich replied to my article at the request of the editor of *AJCC*.[11] In his rebuttal, Dr. Troyanovich again described studies in which researchers found that most patients who underwent invasive diagnostics or surgery had spinal sources of pain. Like the researchers and Drs. Harrison and Murphy, he did not distinguish between the patients in the studies and patients who respond to conservative care. He also failed to address my argument that the generalization was illogical.

Because Dr. Troyanovich failed to address the logical flaw at issue, I wrote another article that appeared in *AJCC* in October 1994.[4] My aim was to bring that flaw to full attention. Using Dr. Bogduk's erroneous conclusion as an example, I wrote: "One aim of the scientist is to make justified generalizations about the phenomenon he or she studies. Based on his research, Dr. Bogduk made a general statement about *all* patients with chronic PNP. This was a serious cognitive blunder called over-generalization. Bogduk used only a *limited* sample of posttraumatic patients in his study. What made these patients 'limited' or distinct from other patients with cervical chronic, posttraumatic cervical pain? Bogduk tells us in this statement: 'Their pain was considered sufficiently disabling by both the patient and the attending orthopedic surgeon, neurosurgeon, or physiatrist to warrant invasive investigation in the form of discography or zygapophyseal joint blocks.'"[8] I also wrote, "Certainly, not all patients with posttraumatic chronic cervical pain meet this criterion."

Dr. Troyanovich disagreed. In a rebuttal in the same issue of *AJCC,* he wrote: "Dr. Lowe states that Bogduk's sample population differs significantly from chronic neck pain patients who are not 'treatment-resistant.' First of all, I find this absurd."[5] He explained his reaction by describing the traumatic injuries and disabling symptoms of Bogduk's study patients. He then concluded, "This sounds to me like the typical chronic neck pain patient presenting to most chiropractors' offices throughout the country."

But even if the traumatic injuries and disabling symptoms of Bogduk's study patients were exactly like those of the typical posttraumatic chiropractic patient, Bogduk's patients still differed in a relevant respect - they had failed to benefit from conservative care, and as a result, were referred for invasive diagnostics.

Oddly, Dr. Troyanovich conceded that the conservative care of patients referred to Bogduk for invasive diagnostics was "unsuccessful." His concession was an implicit admission: Bogduk's study patients differed from posttraumatic neck pain patients who respond to conservative care and are not referred for invasive diagnostics. Yet after his concession, Dr. Troyanovich wrote: "To suggest that these patients [Bogduk's study patients] are somehow 'different' from other chronic pain patients is simply an intellectual 'sleight-of-hand' on the part of Dr. Lowe," and "verbal gymnastics."[5] Despite these characterizations, my pointing out the relevant differences between the groups is merely the proper practice of research logic. Without this practice, researchers and educators are highly vulnerable to falsely interpreting study results - as the topic of this debate illustrates.

Dr. Troyanovich's defense of Dr. Bogduk's faulty generalization (and by extension, that of Dr. Kuslich)

is mitigated by a recent e-mail to me from Bogduk. I had asked him about the differences between his study patients and chronic PNP neck patients at large. "I imagine," he conceded, "that most thinkers would lean toward the inference that the patients we see for blocks are the ones who do *not* respond to conservative care; and therefore, the latter (those who do respond to conservative care) have lesions that *are not* disc or z-joints."[13]

In October 2000, I attended a seminar taught by a chiropractic neurologist. He dogmatically pronounced as scientific fact the faulty generalization at issue. That same month, I attended a dynamic and attention-holding seminar by Dr. Daniel Murphy. To my astonishment, Dr. Murphy reiterated the faulty generalization as a scientifically-established fact. With fanfare, he told the audience of chiropractors, "The scientific studies support what you've *always* done, what you do *now,* and what you'll *always* do!" He was referring, of course, to spinal adjustments, which he considers an effective treatment for lesioned facet joints.[16] Naturally, most of the audience reacted with enthusiasm, since his pronouncement appeared to justify an exclusive focus on spinal subluxations with no need for attention to myofascial and other tissue lesions. I was crestfallen, for I was witnessing my colleagues fervidly accepting without question a now institutionalized logical error.

In January 2002, my dismay over this issue worsened when I read a statement by a chiropractic whiplash expert I have long held in high esteem, Dr. Arthur Croft. "Rather elegant clinical research," he wrote, "has also pointed to the facet joint as a key player in the genesis of neck pain from whiplash, being responsible for some 60-70 percent of it."[18] Like Drs. Bogduk, Harrison, Troyanovich, and Murphy, Dr. Croft failed to distinguish between the general population of whiplash patients and those who were studied invasively because they failed to respond to conservative care.

Importance of the Issues

My argument in this debate is important in three respects:

First, the faulty generalization now accepted as true by some chiropractic educators violates a rule of correct reasoning, and responsible scholarship dictates that we rectify the error.

Second, the chiropractic profession has long toiled to build a scientific foundation. This worthy effort is countervailed by chiropractic educators accepting as true a research conclusion that is logically fallacious.

Third, and most important, holding a false belief about a causative mechanism is likely to compromise the quality of patient care. Chiropractors, especially those who prefer only to adjust their patients' spines, may readily accept as proven the faulty generalization that spinal lesions mediate most all patients' posttraumatic pain. If so, they may neglect other common sources of pain, such as myofascial lesions. By this neglect, these chiropractors may perpetuate, rather than expediently relieve the suffering of many of their patients!

Conclusion

One may wonder why researchers such as Drs. Bogduk and Kuslich would make a faulty generalization. And one may wonder why chiropractic educators would promote the faulty generalization to clinicians as a scientific fact. One explanation is inadvertent bias, a common error of those whose vested interests are fortified by faulty interpretations of study results. Wilson recently

explained researchers' biases in interpreting their study results. He wrote that after completing a study, "the experimenter himself can easily be deceived in interpreting the results by his personal interest in the outcome." He noted: "Even in such routine matters as recording long lists of numbers or other simple data, it has been demonstrated that the mistakes which are made are usually more numerous in the direction personally favored by the recorder. No human being is even approximately free from these subjective influences."[15]

In view of the human tendency toward bias, it is understandable that spine-oriented researchers and educators easily overlooked a fact: that Bogduk's and Kuslich's study patients differed in a highly relevant way from the general population of chronic, posttraumatic pain patients. Is there a way for us to avoid such faulty interpretations of study results? Perhaps we can do so by heeding the counsel of the eminent 19th century biologist Sir Thomas Huxley. "Sit down before fact like a little child, and be prepared to give up every preconceived notion; follow humbly wherever and to whatever abysses nature leads, or you will learn nothing."[14]

We stand to learn much from facts gleaned from the sophisticated studies of Bogduk, Kuslich, and other spine researchers. We can do so, however, only if we see clearly, with unbiased eyes, what those facts truly tell us, and what they do not.

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