

## **“Evidenced-Based-Diagnosis and Treatment of the Painful Sacroiliac Joint: A Clinical Perspective.” Laslett M. *J Man Manip Ther* 2008;16:142–152.**

I found the article “Evidenced-Based-Diagnosis and Treatment of the Painful Sacroiliac Joint: A Clinical Perspective” an example of how the term “evidenced-based” is often misused. Perhaps the greatest challenge for all who advocate evidenced-based-practice is the decision of selecting what is good and what is bad evidence. We all interpret evidence in different ways. As authors we have to decide which evidence will validly support our conclusions. This is where the term “evidence-based practice” can be ambiguous. We all know that evidence is not created equal. Few studies published last the test of time and most are seldom “perfect”. We increase our body of knowledge gaining a little information here and a little bit there. For evidence to do its ultimate job of showing scientific truth we must reproduce the evidence, at different times, and at different places, using different researchers.

Case in point is the selection of the group of articles that suggest that roentgen-stereo photogrammetric analysis (RSA) is the only real “gold standard” for assessing sacroiliac joint motion. Interesting the same researcher performed all of the RSA studies that were cited and any other group has not reproduced the results<sup>1-3</sup>. The conclusion of the RSA studies is that little motion occurs at the sacroiliac joint. The problem with the results is the poor methods used. Undoubtedly RSA has been shown to be a valid and effective method to measure small amounts of motion in the body. However for RSA to measure objects validly, like any method, RSA must have good data. Getting good data from a RSA depends on a number of important factors; the most important is how the free bodies are measured (the free bodies are the sacrum

and 2 innominate bones), are well represented by the measurement markers (in this case imbedded tantalum balls), that give RSA its data. The key issue in RSA is positional reconstruction<sup>4</sup>. Thus the sacrum and the innominate bone must have enough markers that fully represent (outline) each bone in all 3-body dimensions<sup>5</sup>. Without defining the bones in all 3 dimensions leads to errors of non-collinearity<sup>5,6</sup>. The reason for needing this requirement is that when using RSA only 2 directions are examined orthogonally by 2 radiographic projections while the 3<sup>rd</sup> direction is derived mathematically from the other 2 directions<sup>2,6</sup>. The 3<sup>rd</sup> dimension is determined by using a “least squares” solution<sup>2</sup>. For the least squares solution to work properly requires the placement of the markers in such positions that the markers “fully outline” the bones<sup>4</sup>. This would require markers to be placed in the pubis as well as in the ischium, not just in the posterior sacrum and posterior ilium, which is the method that was used. Markers that are placed just on the posterior portion of the sacrum and innominate bones are aligned in nearly the same (frontal) plane, this causes a problem of non-collinearity (the data points are highly correlated with each other since they are all nearly in the frontal plane). Thus studies that use this kind of marker placements to examine sacroiliac joint motion have incomplete data sets (missing sagittal and transverse plane data points) and thus suspect to spurious conclusions<sup>1-3,7</sup>. Until researchers can figure out how to safely put markers in locations that can fully represent the innominate bones (in the pubis and ischium), this method must be viewed with caution. RSA indeed may someday be a useful gold standard when studying

sacroiliac joint motion, however, it appears not until some problems can be overcome.

Second, the evidence that support the use of an anesthetic injection as a gold standard for sacroiliac joint dysfunction is tenuous and fraught with many problems. This has been documented<sup>8-10</sup>. A number of problems exists when using injection of the sacroiliac joint, one is the overlapping neurology of the lumbar and sacral plexus that supply the sacroiliac joint, second the difficulty in fully infiltrating the large sacroiliac joint (and surrounding ligaments), third the reliance on using an often ambiguous subjective rating scale of pain relief (using 75% improvement response), and last the use of a distorted assembly of patients<sup>8-11</sup>. One example is the study cited by Laslett in which Dreyfuss used a cohort of 85 chronic low back pain patients of whom 28 had pain (not recurrent pain) more than 2 years, 13 with pain for 1–2 years<sup>11</sup>, not my typical sacroiliac joint patient. Also, of this “cohort” 38 were under Worker’s compensation, and 21 were represented by an attorney, surely a group of patients I would have difficulty “trusting” when looking for a reliable and valid (75%) subjective pain response from. Furthermore, the average pain rating of this cohort on a numerical pain rating scale was 7/10! The sacroiliac joint patients I see have pain that usually range from 3–4/10, very rarely above a 6/10. This cohort of patients surely does not look like the kind of patients I see with sacroiliac joint pain in my clinic. Currently I see little convincing evidence that anesthetic injection of the sacroiliac joint is really a credible gold standard.

I am in agreement with Laslett that most individual tests for sacroiliac joint

dysfunction have questionable reliability, especially inter-tester reliability. However, if individual tests are clustered together, just like pain provocation tests, reliability often improves<sup>12,13</sup>. This has been shown to true in a number of studies. Since no test has perfect sensitivity or specificity, most tests are usually "coupled or linked" together, this is how most clinicians make a diagnosis, Pre-test probability information is used to narrow down the number of diagnostic hypothesis<sup>14</sup>. As clinicians we do not process information individually but collectively, do the signs and symptoms reinforce our hypothesis or not? We then build upon the hypothesis, letting the clinical course confirm as to whether our diagnostic hypothesis was right or wrong.

Finally, I agree with Laslett in that we currently do not have a good gold standard for the diagnosis of sacroiliac joint dysfunction. However, I am not going to "throw the baby out with the bath water". Believing that the sacroiliac joint is simply not relevant is an easy way out or that we should just restore the joint to its normal relationship is not realistic. We can't even agree on how the joint moves much less what is normal movement. Much more research is needed to investigate the sacroiliac joint. Its deep location, the irregular shape of the innominate and sacrum bones, and the misunderstood amount and kinds of motion that takes place at this joint make this problem an enigma, but I believe we must keep learning all we can

about this joint. If the Father of Epidemiology John Snow, MD had given up on the sick patients with Cholera, before the microscope was invented, many more people would have become sick and possibly died. Like Snow I believe we must find new methods to help us understand our patients with sacroiliac joint pain.

Regards,

Michael T. Cibulka, PT, DPT,  
MHS, OCS  
Assistant Professor, Physical Therapy  
Program  
Maryville University, St. Louis, MO

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## AUTHOR RESPONSE

**D**r Cibulka has made some observations that are worthy of response. The statement that the paper in question is an example of the “misuse” of evidence stimulates rebuttal. It is quite correct that interpretation of “good and bad” evidence varies considerably. I would opine that this variability is in itself evidence of widespread misunderstanding. The ambiguity Dr Cibulka refers to is not something to accept or encourage. The sentence “As authors we have to decide which evidence will validly support our conclusions” indicates the antithesis and reversal of what evidence-based clinical reasoning attempts to achieve. Surely one should use evidence to generate a conclusion, not start with a conclusion and hunt for evidence to support it? To start with a conclusion and find published evidence to support it is not a valid approach in my view. It is a quite different thing to dispute the value of published evidence and carry out research to test one’s own differing hypothesis. This is precisely what I and my co-authors have done in relation to the clinical diagnosis of SIJ pain in response to the (then) current evidence that demonstrated no validity of the clinical examination<sup>1-4</sup>.

The center of my response to Dr Cibulka’s letter is in relation to the statement that I regard RSA or intra-articular anaesthetic SIJ blocks as a “gold standard”. I do not recall ever using that term and if someone can find where I have done so in the last 10 years I will accept appropriate criticism. My avoidance of this term in favor of alternatives such as “reference standard” or “criterion standard” is no accident of style but specifically intentional. I was accused of the same action by the redoubtable Professor Alf Nachemson in 2003, when presenting initial results on clinical predictors of

lumbar discography at a conference<sup>5</sup>, which he (reluctantly) accepted, and my response is the same in the current situation. I do not use the term “gold standard” since in this field of medicine there is no perfect test that can serve as a gold standard. On this matter Dr Cibulka and I can agree. The validity studies I present as evidence compare clinical variables with the best reference standards available. I have on a number of occasions stated the limitations of the reference standards used in my studies of sacroiliac joint pain<sup>6</sup>, discogenic pain<sup>7,8</sup>, in my doctoral thesis<sup>9</sup>, in correspondence<sup>10,11</sup>, and in a recent book chapter<sup>12</sup>. Indeed I reiterated this view in the paper in question. Until such time as a gold standard is available this is all we have. Actually, gold standards are available in only a few circumstances. For example a validity study that assessed the value of clinical variables to predict, say, sudden non-traumatic death prior to the age of 50 years, would be such a case. Now *there* is a gold standard. Death either has occurred before the age of 50 or it has not and is easily validated independently. We do not have this sort of luxury in our area of musculoskeletal medicine.

Where I am in total agreement with Dr Cibulka is with regard to the importance of repeated studies. It is the weight of accumulated evidence that lends gravitas to derived conclusions. As I have pointed out, the weight of evidence against the use of palpation based tests for SIJ dysfunction constitutes a good reason to seriously consider excluding their instruction in undergraduate courses completely, and as useful diagnostic tests at post graduate instruction courses. Yet I concur that it is always a problem to “throw the baby out with the bath water” as Cibulka has pointed out. As a manipulative therapist I can agree

that palpation during movement testing reveals *something*. The problem is we do not know what it is yet. I do not accept that traditional explanations of what the palpation based tests reveal are supported by quality evidence and I challenge others to correct me if I have this wrong. On the flip side of this argument, it can be said that the validity of the SIJ provocation tests has now been demonstrated in a second independent study<sup>13</sup> and the similarity between these results and my own<sup>10</sup> are striking<sup>11</sup>. The use of the centralization phenomenon to reduce false positives in SIJ pain diagnosis has yet to be tested again by other independent researchers so our own results<sup>14</sup> remain preliminary. Inter-examiner reliability of the provocation tests has been evaluated by researchers following our original report<sup>15</sup>. The weight of evidence for reliability is quite strong in my view<sup>16-18</sup>.

I think it is crucial to point out another inversion of appropriate clinical reasoning that Dr Cibulka uses in this context. He states that the “sacroiliac joint patients I see.” On what basis does he say he sees SIJ patients at all if he is using unreliable and invalid tests? The cart is before the horse again. I accept at face value Dr Cibulka’s statement that the patients in the study by Dreyfuss et al<sup>1</sup> included patients with significantly more pain than the usual patient he would normally see. The same criticism would validly apply to my own and other studies carried out using intra-articular injection as a reference standard. After all, diagnostic injection should only be used to investigate patients whose pain is severe or persistent. Exposure to radiation, potential for infection, and other complications ethically require appropriate restraint. The problem of generalizability of results from studies using invasive tests does not permit critics to entirely discount the re-

sults however. Severity of pain especially, is not a measure or indicator of pathology or severity of pathology. While it is true that generalizability issues cause us to be restrained in our application of results to less painful and disabled patients, it is equally true that current evidence does not support treating less troubled patients as though no evidence exists. The restraint goes both ways.

Mark Laslett

FN=ZCP, PhD, Dip.MT, Dip MDT  
Director of Clinical Services,  
PhysioSouth Ltd, Christchurch,  
Canterbury, New Zealand  
Senior Research Fellow,  
Auckland University of Technology,  
Auckland, New Zealand

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