Sacroiliac Function and the Gait Mechanism

By Robert Cooperstein, MA, DC

Contrary to all prior expectations, it has proven excessively difficult to show that patients before and after spinal manipulation have changed structurally, or at least in their biomechanical performance. Indeed, so called "soft" outcome measures (pain scales, ADL surveys, etc.) have largely displaced physiological data as outcome measures. Robinson, Herzog, et al., ¹ using force plate measurements, attempted to demonstrate that patients with decreased interarticular mobility of the sacroiliac joint showed a "distinct tendency toward improved gait symmetry after treatment in those cases where the gait was asymmetric prior to the treatment." There were a couple of interesting sidebars to this experiment. First, fixation in the subjects’ SI joints was established via motion palpation, despite its poor track record in this area (at the limit, the method could not discriminate subjects with and without ankylosing spondylitis that had fused their SI joints). ² Second, for some reason the subjects were placed with the presumed fixated joint down, while a manipulative thrust was delivered to the sacral ala of the noninvolved side.

A follow-up pilot study using one subject ³ showed that SMT reduced his low-back pain and changed selected gait parameters: the gait appeared to become less hesitant. This time, the involved side was manipulated. Herzog et al. speculate that low back pain is associated with muscle spasm, which would be expected to alter joint mobility and alter movement patterns. Although filming and force plate measurements were used, only the latter showed changes. Pre-post changes during each treatment were insignificant, but there was a significant gait improvement during the 11 days of the study, whether due to treatment or natural history.

A full blown n=11 study based on this pilot ⁴ used several outcome measures to track the results of SMT (back to the uninvolved side) on patients found by two motion palpators to exhibit SI fixation. After six treatment visits in 11 days, the subjects tended to show less pain (VAS) and more SI mobility (motion palpation). Those patients who had normalized their Oswestry scores after five visits remained normal at the time of a follow-up assessment about two weeks later, whereas three of four patients who retained some functional disability after 6 weeks had worsened at the two week follow-up. The investigators presume that partial disability tends to worsen without treatment, thus providing evidence for the value of care on through symptom resolution. Overall pre-post-treatment gait symmetry did not improve -- in fact, gait became more
asymmetrical. However, given that previous work had failed to show “normal” gait to be perfectly symmetrical, improvement may take the form of increased asymmetry. (I and colleague Robert Jansen, researching lower extremity loading responses, have found the data to suggest asymmetric function to be the norm). There were significant differences in ground reaction forces between the involved and uninvolved sides, some of which changed with treatment. Those patients who showed the least changes tended to show the least pain reduction with SMT, raising the following question: do changes in ground reaction forces result primarily from pain reduction, or from an increase in joint mobility?

By 1994, in a paper critical of Osterbauer’s use of gait measurements as an outcome measure for SI interventions, Herzog and Conway had come to the conclusion that "gait analysis was not a suitable technique" to evaluate the biomechanics and effects of SMT because it is not neither specific nor accurate enough. They suggest that gait analysis may be more useful to assess more injured patients than those commonly seen by chiropractors.

**Motion Palpation**

As someone who had been using the Gillet step test to screen subjects for entry into clinical trials, Herzog was correct to test its intra- and interexaminer reliability in assessing SI motion. There were a couple of unusual wrinkles in this particular study. There were three examining sessions, the third of which followed SI manipulation. Interexaminer reliability as to whether SI fixation was present (no side specificity) was above chance levels during the first and third examinations, but not after the second.

Herzog et al. speculate that the treatment doctors may have incorrectly assumed that there had been treatment between the first and second examination sessions, and that this may have biased their judgements. If so, this just serves to show how sensitive diagnostic calls are to doctor expectations. The interexaminer reliability of side specificity was also measured, and it was found that the doctors agreed on the side of fixation about 60 percent of the time. (I will let the reader decide whether 60 percent is a high or low number.) Intraexaminer reliability, measured in a number of ways, was found to be better.

Since it is commonly stated that one of the problems with motion palpation studies is that the subjects are usually asymptomatic chiropractic students, and that study of a more representative symptomatic population would be more likely to show interexaminer agreement, the subjects in this study were stratified by severity of fixation to see if this would improve the reliability of interexaminer ratings. It didn’t.
Since it is commonly stated that one of the problems with motion palpation studies is that examiners may lack training or expertise, the doctors in this study were stratified by their expertise to see if this would affect intraexaminer reliability. It did -- but the less expert examiners were more reliable. Interesting. This is consistent with Mior’s finding that training does not help.⁹ (As a technique instructor at a chiropractic college, I am often asked by students to confirm their palpatory impressions. I always agree to do so, but not without first stipulating that modesty precludes any assumption that my impressions would be superior.)

**Forces Exerted during SMT**

In 1990, Hessell, Herzog et al. reported on the first experiment that measured the forces generated by chiropractors on living subjects.¹⁰ They obtained measurements using a thin, flexible pressure mat placed over the PSIS that registered the forces produced during the application of drop table moves as described by Thompson. Apart from the inherent interest in the numbers obtained, and the shapes of the force-time curves, it was especially striking to note that each of the two chiropractors in the study consistently missed their intended contact on the PSIS, often by several centimeters. The doctors, each of whom stood on the left side of the table, almost always impacted inferomedial to the left PSIS, and superomedial to the right PSIS. It remains to be seen whether the outcomes that doctors ascribe to their specific contacts using drop table techniques accrue due to this lack of segmental specificity, or in spite of it.

Kawchuk, Herzog and Hasler used the same methodology to quantify the force magnitudes and patterns developed during a toggle-recoil adjustment of C1, patient in lateral recumbent (side posture) position. One finding was that the forces were lower than those observed in previous work the investigators had done on the thoracic spine, and the preadjustive loading forces were lower than those observed in the drop table study.¹⁰ Also, the change in paraspinal tissue compliance on the adjusted side was -0.5 mm, as compared with -0.1 mm at the same level on the contralateral (non-treated) side. (Nansel et al also found changes in paraspinal compliance with SMT.)¹¹

Continuing in the same vein, in a truly remarkable and important article, Kawchuk and Herzog calculated the forces developed by chiropractors in the clinical setting as they applied five different types of manipulative techniques to subjects’ cervical spines.¹² The methods tested were the lateral break, Gonstead, Activator, toggle, and rotation, and references were provided for careful descriptions of the maneuvers in question. When the clinicians were asked to judge whether the manipulative procedure had been effective, they tended to favor thrusts that were shorter in duration and higher in peak force.
The Gonstead and lateral break methods resembled one another in terms of force generated and thrust duration. Compared with them, the toggle-recoil adjustment produced a similar peak force, but was of shorter duration, and the rotation method was similar in duration but smaller in force. Likewise compared with Gonstead and the lateral break, the Activator instrument produced a smaller peak force (40.9 N compared with 106.0 N) over a shorter duration (47.5 msec compared with 89.3 msec.) Actual data such as these will allow us to draw inferences on the claims made for and against different types of manipulative procedures. For example, is it good or bad, for a given type of patient, that an instrument-assisted thrust is delivered in half the time, using half the force? Is it as likely to move bones or produce paraphysiological joint space movement? Is the thrust safer in some sense? How do the time frames relate to the latency period for the various neurological reflexes?

**SMT vs. Back School in SI Patients**

Briefly: 50 patients were randomized between chiropractic care, including SMT, and back school. Both functional (VAS, Oswestry, Gillet step test) and biomechanical (gait analysis) outcome measures were used. The results were a split decision: back school tended to be more helpful for the functional measures, whereas SMT was more effective for restoring gait symmetry.

Herzog concludes: "Assuming that restoring normal gait symmetry is an indication of a successful treatment program (an assumption he did not make in the 1988 paper [Herzog, 1988 #1041])," SMT was more successful, whereas back school improved the clinical measures more. However unpalatable the thought, the data are equally consistent with the possibility that SMT trails back school in some SI cases because it promotes gait symmetry, which may not be biomechanically optimal.

**Trochanteric Supports**

Only rarely do manual therapy equipment vendors take the trouble to substantiate that their equipment does what it is supposed to do. Even less frequently do doctors rigorously assess the clinical value of such equipment. Conway and Herzog tested one apparatus, a trochanteric belt, for one condition, SI fixation. The results: objective measurements of the gait mechanism showed no differences between subjects wearing the belt in the "right" place, as compared with a supposed "placebo" place, or not at all. Thus, whatever trochanteric belts do, they don’t seem to support or stabilize the SI joint during walking. Maybe they provide some biomechanical benefit in activities other than walking.
Chirophysics and the "Falling Doctor" Chiropractic Adjustment

In 1990, Haas wrote a series of articles on the physics of spinal manipulation\textsuperscript{15-18} that provoked an exchange with Triano.\textsuperscript{19,20} After commenting that "a trivial issue had been presented in a thoroughly confusing (not to say incorrect) way," Herzog sought to re-establish the rule of Newton’s second law, which cannot be made "a matter of opinion."\textsuperscript{21} He concluded in a second piece, essentially, that although "modeling SMT as an impact situation of a free-falling chiropractor onto a patient may be a mathematically appealing way of calculating forces exerted in SMT," there are several ways of getting it wrong.\textsuperscript{22} Although it is better that I not comment on the physics involved, I do believe the illustration supplied correctly portrays the "falling doctor" model of the chiropractic adjustment. More recently, in another piece aimed at getting the physics right,\textsuperscript{23} Herzog takes Harrison to task for incorrectly correcting other chiropractors’ use of the term "torque," and for other mistakes as well. No doubt a response is forthcoming.

Cavitation

In 1993, Herzog et al used a uniaxial accelerometer to measure the acceleration signal associated with the production of audibles during SMT. As someone who has always (perhaps excessively) appreciated the importance of audibles,\textsuperscript{24} at least when an HVLA thrust is used, I find it reassuring that "a trained chiropractor is able to hear or feel cavitation every time it occurs, and is also able to detect when cavitation does not occur." In 1996, Herzog wrote that the reflex changes that accompany an HVLA thrust are not dependent on cavitation;\textsuperscript{25} nor are they dependent on the magnitude of the applied force.

Speed, on the other hand, is everything. He also argued against the hypothesis that muscle stretch reflex responses may resist treatment forces. The hypothesis goes like this: since stretch reflex times (7-40 msec) are shorter than the onset to peak force for the thrust time (100-150 msec), a muscular response may inhibit a thrust. However, Herzog has measured the delay from the onset of the thrust to onset of EMG activity to be about 100 msec, after which it takes another 40-100 msec until the onset of muscular force, and still another 100-250 msec for peak forces to be obtained. In other words, "it seems unlikely that there are substantial muscular forces resisting the thrusting phase of high-velocity chiropractic SMT."

Vertebral Movements during SMT

Using an unembalmed cadaver, Gal, Herzog, et al.\textsuperscript{26} measured several of the possible (absolute and relative) mean linear translations and angular rotations of T10, T11, and T12 for 5 P-A hand thrusts to the
right transverse process of T12. (As a tangential finding, the investigators provide the first report of an audible in a cadaver.) Three statistically significant relative movements were measured: T12 translated 0.93 posterior, and rotated 0.32o relative to T11, and T11 rotated 0.28o relative to T10.

Another more detailed experiment measured the displacements of T10, T11 and T12 in all 6 degrees of freedom for manual thrusts delivered to the right transverse process of each vertebra. All three vertebra experienced absolute P-A translations (6-12 mm), right to left translations (3-6 mm), and clockwise rotations on the Y axis of 0.4o-1.2o, irrespective of which vertebra was contacted. T10 and T11 always extended, whereas T12 always flexed. In relative terms (not all movements were statistically significant), I could not discern in the data any support for the widely held belief that manipulative thrusts (at least in cadavers) move vertebrae relative to the segment below, rather than the segment above.

**Reflex Responses in SMT**

Two subjects received sustained slow and fast thrusting to the T6 to determine if SMT and the audible release associated with it activated spinal muscles. EMG activity was observed consistently 50-100 msec after each fast thrust, whether the treatment resulted in an audible or not, whereas slow thrusts never produced visible EMG activity of the target muscles. This suggests that fast thrusts elicit muscle activation, whereas slow force application does not. The timing of the onset of the EMG response suggests that activation occurs at the muscle spindle level. The speed of force application, not the magnitude attained, is what causes EMG responses. The audible release does not (by itself) seem to evoke muscle activation or a joint proprioceptive reflex response, as some have speculated.

**Tissue Compliance**

Although several investigators have used soft tissue compliance meters to probe for changes in tissue stiffness following interventions (such as a team including myself), Kawchuk and Herzog claim the reliability and accuracy of such tools had not been tested other than on human bodies, an overly variable substrate for this purpose. Therefore, they tested a compliance meter across 4 surfaces, using 5 force levels per surface, 10 trials per force-surface combination, and among 5 blinded doctors. This gives a total of 1000 observations, after which they concluded: intraexaminer and interexaminer reliability was poor. Even pressure on a rigid surface produced non-zero "displacements." The investigators went on to develop an alternative measuring device: a rigid metal frame houses a motor that drives a spindle onto the test surface, using a strain gauge to measure the load applied and counting motor steps to assess absolute tissue
deformation. The instrument, employed on 5 test beds, showed excellent accuracy and reliability, although it remains to be seen whether it will display true examiner independence in a clinical test setting.

Notes


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