

Start Points in ROM Measurement: Not a Trivial Problem

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Dr. Ray Wiegand presented a study and distributed an interesting report¹ at the meeting of the Association of Chiropractic Colleges this spring on the rotational and translational movements of the individual cervical motion units. (Author's note: The report by Wiegand and Wilke was distributed at the ACC meeting, but has yet to be published in full. A shortened version of the report does appear in the symposium proceedings and was published in the *Journal of Chiropractic Education*.) Dr. Wiegand and co-investigator Steven Wilke first used data supplied by Kraemer et al.² for an uninjured population to establish normal segmental flexion to extension ratios, then (at least apparently) proceeded to compare these normals with the data obtained from several hundred motion studies of their own.

(I say "apparently" because the report is unclear in many key areas as to what the investigators did and how they came to their various conclusions about cervical segmental normals and abnormal. Reports like this remind us of the function that peer review serves.)

The investigators assumed an ideal flexion/extension ratio of 1:1 for occiput on C1, 8 degrees in each direction, for a total range of 16 degrees from full flexion to full extension. If that were valid, then how would one interpret a patient who could extend the occiput 13 degrees but flex it only 3 degrees? Weigand and Wilke would say that flexion is diminished from a normal of 8 degrees to 3 degrees, but only because the so-called "neutral" position of occiput is already in 5 degrees of flexion. In other words, the reason the occiput doesn't flex very well is that it is already partially flexed in the patient's static posture. They would say, "The global range of motion, discounting the manner in which it was achieved, remains normal."

The investigators assume, providing neither evidence nor reasoned discussion, that an occipital carrying angle that is not in the midpoint of the global segmental range of motion must be considered a misalignment. Maybe. They next opine, even less convincingly, that such an occiput would be fixated in a flexed position. Finally, they state the occiput would require a superior to inferior line of drive of correction. The fixation assumption, in my opinion, even were the assumption of occipital misalignment valid, is quite excessive. A patient may carry the occiput in a flexed "neutral" position to atone for hyperextension in one or more caudal vertebral segments, perhaps in the thoracic or even lumbar spines, and not be fixated in any meaningful sense of the term, let alone adjustable for that reason.

Whatever the insufficiencies in their overall argument, it remains interesting to consider the central issue raised by the investigators' work: the difficulty of interpreting range of motion data in relation to the position from which measurements are taken. The problem is more easily addressable in the frontal plane, in which the assumption of a normal left-right symmetry serves at least as a starting point for discussion. After all, it is reasonable to suppose that a typical individual should evidence symmetric quantities of segmental and global movement to the left and right. Consider, by comparison, the difficulty in defining the normal relationship between normal

quantities of flexion and extension in the sagittal plane. Weigand's assumption that the neutral position is in the midpoint of the sagittal global range is far less appealing than the assumption of left-right symmetry in the frontal plane.

Suppose we were using a hand-held inclinometer to measure global lateral bending of the head and neck, measuring left vs. right excursion starting from some sort of measured zero (neutral) point. What should be the starting position for the measurement? We could use the inclinometer, placed on the vertex of the skull, to somehow first zero the head and neck, then measure from there to the active or passive end range. We could, on the other hand, instruct the patient to close his or her eyes, then measure the difference between whatever baseline position the patient experiences as neutral and the passive or active end range. Although it may not be obvious, it turns out that different clinical inferences may be drawn from one or the other measuring protocols. Examples follow.

Lantz et al., in an elegant study comparing different instruments and protocols for measuring cervical range of motion, instructed the patients as follows:³ "Participants were asked to stand or sit upright and assume a neutral (anatomic) position, looking straight ahead. The examiners did not attempt to adjust the neutral position." Therefore, in reporting left and right ranges of motion, the investigators did not attempt to correct for possible non-neutral starting positions, nor am I suggesting they should have.

But what happens in the case that the subject, although believing himself to be centered, actually has five degrees of tilt to the left in the start position? Lantz, not especially concerned in this study with the start point, could very well find this subject to have equal and symmetric global motion - say, 45 degrees - as evidenced by identical left-right endpoints. However, a second examiner who places a hand-held inclinometer on the vertex of the skull so that it is zeroed, and only then measures the range of motion end points, gets a different outcome. He or she would find the head and neck to move only 40 degrees to the left and 50 degrees to the right, a 10 degree asymmetry that probably would be interpreted as clinically significant.

Weigand and Wilke might explain that this patient is restricted in left lateral flexion, and because he is already in 5 degrees of lateral flexion in the "neutral" position. (I say "might" because their analysis is segmental, and the present example concerns multisegmental motion.) Furthermore, this patient would most likely be adjusted in some manner from left to right to induce a more centered neutral static position. Lantz's protocol, although it would not directly identify a range of motion asymmetry in this patient, certainly detects a non-neutral start position and perhaps suggests clinical strategies from that point of view. One measuring protocol detects a range of motion asymmetry; the other, a static misalignment. This could very well lead to similar, or at least consistent, clinical strategies, although this is far from certain. After all, one examiner finds static tilt; another finds range of motion asymmetry.

At the risk of making this discussion even more Byzantine, let us assume that another patient carrying his head and neck 5 degrees tilted to the left is found by Lantz to attain 50 degrees of global lateral flexion to the left, and only 40 degrees to the right. I assume he would find this 10 degree asymmetry clinically significant and adjust accordingly. Weigand, on the other hand, might find 45 degrees of corrected lateral flexion to the left and 45 degrees of corrected lateral flexion to the right. Indeed, I think he would say the patient has wisely (if unconsciously) chosen to carry his head and neck centered in the middle of a 90 degrees total left-right range of motion by carrying himself around tilted 5 degrees to the left. I am sure what, if any, adjustments would be indicated from this point of view.

Since the occiput was adjusted at the beginning of this column, using a line of correction consistent with its non-neutral position in the sagittal plane, I assume Weigand would not feel the need to correct this current patient's head-neck position, since it is centered in the frontal plane. Again, as in the previous case, different protocols for assessing and interpreting the patient may lead to different clinical strategies.

This is not the first time I have addressed problems such as these. I once devoted a whole column⁴ to Haas' experimental finding that lateral flexion misalignment predicted diminished ipsilateral lateral bending to the side of the convergent disk angle, compared to contralateral bending, as measured from the actual non-corrected segmental start point.⁵ This is contrary to what most motion palpators say: that bending is diminished toward the side of the divergent disk angle, or "open wedge." I suppose Weigand would say, "Of course." A vertebra is unlikely to bend as far in the direction to which it is already laterally flexed in its "neutral position."

It remains troubling that the palpators profess to feel diminished bending excursion toward the side of the open wedge since that is not what has been measured to happen. It would make more sense if they claimed to palpate impaired quality of motion (stiffness, jerkiness, hard end-feel) toward the open wedge side, since Haas did find diminished movement in that direction as measured from an ideal, corrected neutral start position.

In another column,⁶ I covered a talk given at an American Back Society meeting, in which the presenter criticized a method of measuring forward flexion of the lumbar spine by subtracting the start point from the end point. By this method, a hyperlordotic patient who otherwise has a normal end range would be found restricted in forward flexion. He would also be found to have a smaller ROM than another patient with a lesser lordotic angle who has the same end range. Weigand might say this patient appears to have diminished flexion only because in her "neutral position," she is already forward flexed to some degree. Since flexion restriction and hyperlordosis are not the same clinical entity and would suggest different treatment strategies, real clinical issues are at stake here.

My colleagues and I have run into problems such as these in studying and measuring static and dynamic, loaded and unloaded functional leg length inequality.⁷ Perhaps the biggest problem we have encountered has been establishing and defining a neutral start position, some sort of defensible baseline. Once that is done (and we have had some partial success), then changes in the baseline could be regarded as evoked responses to changed clinical circumstances such as head rotation; pressure and isolation testing; challenges of various sorts; etc.

The phenomenology of varied patient table mounting protocols deserves a column of its own. Suffice it to say, it is very difficult to have the legs tell their own story, unbiased by unwitting doctor perturbation of the start point. Having the doctor grasp the legs during the leg check, although it is the most obvious of problems, is probably not the most serious.

I apologize if the preceding argument - to use the words of someone to whom I sent a presubmission draft for comment - was "a bit dizzying."

References

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