Dynamic Chiropractic

CHIROPRACTIC (GENERAL)

Subluxation-Based vs. Subluxation Syndrome-Oriented

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When I was a chiropractic college student more than 40 years ago, the clinical world seemed less complicated. It wasn't, but ignorance was bliss, as the saying goes, and chiropractic - the art - was tidier. We occasionally drew a distinction between the chiropractic subluxation (which involved "nerve interference") and the allopathic subluxation (a mere finding of joint misalignment), but for the most part the "S-word" meant the clinical entity we targeted. As chiropractors-in-training, we aimed to relieve subluxations to help sick people get well. We gazed with condescending amusement at the Palmer camp, which seemed to be obsessed with an exclusive focus on upper cervical subluxations; at Logan College we considered our knowledge of spinal biomechanics superior to anything else being taught in the profession. We had the answers; others could only approach the level of sophistication inherent in basic technique.

With decades of hindsight and the benefit of 20 years of clinical research literature in chiropractic, we can look back and wonder at the arrogance of our ignorance. Time breeds a degree of detachment, and perhaps allows the seeds of wisdom: recognizing how much we have yet to learn. We now have at our disposal many dozens of randomized, controlled clinical outcome trials (RCTs) of the chiropractic art. These investigations attest to the benefit of adjustment and manipulation, at least for patients with some kinds of low back, head and neck problems. However, the potentially much broader influence of subluxation upon health and illness, and our ability to improve health and relieve disease through subluxation reduction, has not been scientifically established. We've barely scratched the surface in this area of inquiry. There are reasons for optimism as research continues, but few, if any, RCTs have bothered to monitor the traditional chiropractic lesion along with clinical outcomes. Our smugness seems to have delayed us from asking the right questions.

Since my graduation, a new theoretical orientation has emerged within the profession. Where once we

were so bold as to suggest that subluxation was responsible for most of the ills of mankind, some DCs now argue that subluxation has nothing to do with disease! It's even been suggested that subluxations are inherently adjustment-worthy, and that attempts to relate subluxation to disease amount to following the "medical model." This is certainly not what I learned in chiropractic school, or how I practice today.

Some helpful clarifications came from a project to establish terminology by consensus a few years ago. Gatterman & Hansen[1] found better than an 80-percent agreement among Delphi panel participants for several terms related to "the lesion treated by chiropractors." Among these were:

Subluxation - a motion segment, in which alignment, movement integrity or physiological function are altered although contact between joint surfaces remains intact.

Subluxation complex - a theoretical model of motion segment dysfunction (subluxation) that incorporates the complex interaction of pathological changes in nerve; muscle; ligamentous; vascular; and connective tissue.

Subluxation syndrome - an aggregate of signs and symptoms that relates to pathophysiology, dysfunction of spinal and pelvic motion segments, or to peripheral joints.

Semantics can be trivial, significant or confusing. So it may be helpful to try and relate the above definitions (our "approved" jargon) to what we're trying to do in practice and to what we ought to be testing by research.

D.D. Palmer's central clinical proposition[2] - at least those parts that can be tested - boils down to this:

Adjusting

reduced subluxation

improved health (or decrease in disease).

So how does Palmer's theory relate to the consensus definitions offered by Gatterman & Hansen,[1] and to the challenges facing us in research and practice? Quite nicely!

Gatterman & Hansen's[1] "subluxation" is the central part of Palmer's clinical theory. It's the target of our adjustments, and the "mediator" between what we do (adjust) and how we help the patient to benefit (improved health):

Adjusting

reduced subluxation

improved health (*or decrease in disease*).

Needless to say, we have been very imaginative in inventing methods of detecting the mediating factor: static and motion palpation; electromyography; radiography; neurocalometer-like instruments; and thermography, Activator isolation testing, etc. We do not yet know whether any one of these strategies is better (more valid) than any other, but the diversity certainly suggests that we have years of validation testing in front of us.

The "subluxation complex" can be thought of as a theoretical elaboration upon this mediator, a recognition or hypothesis that joints don't simply fixate or misalign. The "complex" involves other local tissues and changes in their structure and function. Among those changes that have been suggested are pathophysiology, kinesiopathology, myopathology and histopathology. It's entirely possible that laboratory study of the components of this "complex" may give rise to yet additional strategies for subluxation-detection, methods we haven't yet dreamed of.

But we wouldn't care about finding and adjusting subluxations if we didn't suspect that doing so would benefit our patients. Another way of saying this is that we adjust to improve subluxations because we hypothesize that improvements in subluxation lead to improvements in health (including reductions of the syndromes of disease), which brings us to Gatterman & Hansen's[1] "subluxation syndrome."

We can represent the subluxation syndrome as the shaded area of our (Palmer's) larger theory:

Adjusting

reduced subluxation

improved health (*or decrease in disease*).

The syndrome, of course, is the reason for adjusting. Why bother to attempt to rearrange joints and complexes if we didn't suppose that it would benefit the patient? Although as clinicians, we focus much of our attention and effort on detecting and understanding the mediator in our theory (subluxation and subluxation complex), the rationale for doing so is the effects we suppose will result from changes in joint architecture and activity. (Remember all those basic science courses we had to take? A big part of the rationale for studying basic science was to better understand the subluxation syndrome.) If we're wrong - if there's no relationship between the "subluxation" and clinical outcomes - manipulable lesions could be ignored. Why move bones for the sake of moving bones?

Chiropractically relevant basic science research has multiple goals. We hope to better understand not only the subluxation and subluxation complex, but also, and perhaps more importantly, the subluxation syndrome. Laboratory research that looks at the relationship between what goes on in the spine and its influence on distal (or proximal) organs may meet this need. We have some of the necessary information, but there's still a lot to be learned.

Perhaps even more important to us as clinicians are the RCTs that might help to confirm or refute

Palmer's original idea. But surprisingly, this is one of the more deficient areas in our research database. When we consider that dozens of RCTs have been published on the effects of adjusting (and other manual therapies) on clinical outcomes (e.g., back pain, headache, spinal ranges of motion), it is troubling to note that rarely, if ever, have indicators of subluxation been concurrently monitored. In other words, the vast majority of our RCTs have addressed the question of whether adjusting improves clinical outcomes, but have done so without reference to our traditional, hypothetical mediator (subluxation). Yes, the clinicians participating in these experiments have usually directed their thrusts to sites of supposed subluxation, but the observations that led the doctors to suspect joint dysfunction are usually not recorded throughout these trials.

Now we can't beat up on the researchers for neglecting to study what we think is important. I can tell you from years of experience that clinical scientists are an independent bunch, made up of individuals who pursue their own research ideas. If they want to explore whether or not adjusting leads to improved health (or a decrease in disease), that's their prerogative. We cannot dictate to the researchers what they must investigate. However, we can do a better job of encouraging and enticing them (e.g., through grants) to include indices of subluxation, and the clinical outcomes they measure. The FCER's recent call for proposals for subluxation research is an encouraging step in that direction.

Controlled trials of adjusting that monitor outcomes and presumed subluxation indicators might go a long way toward establishing a "gold standard" for subluxation detection and validation for our traditional theories. We won't know until we try, and we won't try if we start by insisting that adjusting subluxations is an end in itself, or if we insist that we already know that subluxations influence health and disease. Among the outcomes we might hope to relate to subluxation are pain (e.g., as measured by a visual analogue scale); motion (e.g., as measured by goniometry); physiopathology (e.g., as indicated by various blood tests); the signs and symptoms of various diseases; and indicators of health.

Consider the hypotheses that could be tested in a single such trial where subluxation indicators and clinical outcomes were monitored simultaneously:

Does adjusting

reduced subluxation?

Does adjusting

improved health (or decreased disease)?

Does reduced subluxation

improved health (or decreased disease)? (Is there a subluxation syndrome?)

I don't mean to suggest that outcome trials that also involve monitoring of suspected subluxation parameters are the answer to all our questions. In fact, it may be that we should look to laboratory-based basic research to suggest better ways of detecting subluxation, or to do a better job of trying to relate subluxation to clinical syndromes and health. But in the final analysis, clinical trials that involve subluxation indicators, as well as outcomes, will be one of the necessary (although not sufficient) components in our efforts to validate (or refute) traditional chiropractic theories.

I've come to the realization that I'm not merely a subluxation-based chiropractor. I am a subluxation-syndrome-oriented chiropractor, and I believe this is a direction that our research and practice ought to be pointed toward.

How far we've come! How far we have to go!

References

1. Gatterman MI, Hansen DT. Development of nomenclature through consensus. *Journal of Manipulative and Physiological Therapeutics* 1994 (June);17(5):302-9.
2. Keating JC. *Toward a philosophy of the science of chiropractic: a primer for clinicians*. Stockton CA: Stockton Foundation for Chiropractic Research, 1997.

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