

EVALUATING THE QUALITY OF CLINICAL PRACTICE GUIDELINES

To the Editor:

Since Dr Keating stated in his letter to the editor¹ in response to Dr Christopher Kent's letter² that he is awaiting his remedial education regarding the vertebral subluxation complex in the pages of *JMPT*, I felt compelled to provide it. Actually, should Dr Keating choose to do some reading of past issues he would find his remedial education patiently awaiting him.

Allow me to provide an easy-to-follow (remedial) guide to this literature. I think the main problem faced by Dr Keating and many of his like-minded colleagues is that they have a deficient understanding regarding the proper use and inherent limited abilities of science. He also demonstrates a remedial grasp of the paradigm of chiropractic and the philosophies toward health that spawned it. Ironically, this has already been pointed out in the past, in the very pages of *JMPT* where Dr Keating is awaiting his education.³ Chiropractic, as aptly described in past issues of *JMPT* by Coulter,^{4,5} represents a distinct health paradigm. As Dr Kent pointed out in his editorial,² the concept of subluxation is at the core of this paradigm. Any debate regarding the existence of subluxation is really one regarding the validity of the chiropractic paradigm and therefore does not belong in the scientific arena (at least not until there has been adequate science conducted within the paradigm to warrant such debate). This is a philosophic or paradigmatic question and one that no scientific study is equipped to answer. A scientific study can only validly answer the question that it asks, and no scientific methodology is available to ask (or answer) the question of the validity of a health paradigm.

Science, as Thomas Kuhn⁶ has very coherently pointed out, and as Coulter^{7,8} has lucidly interpreted for even the least investigators among us, has the role of testing hypotheses within a paradigm, not testing the paradigm itself. "What is essential to understand is that such research is not directed to proving, or even disproving, the actual paradigm. It is better conceived as explorations or puzzle solving within the given paradigm."³ Therefore, what we must do as chiropractors adhering to the paradigm of our chosen profession is design and conduct studies with the a priori assumption that subluxations do exist. This is something Dr Kent has been doing in an exemplary and praiseworthy manner for years. Valid scientific opinions regarding the validity of the chiropractic paradigm will be possible only after a period of investigation involving years of study producing an adequate body of literature. Surely Dr Keating is not naïve enough to believe that as a profession we have created this body of literature to date. In the meantime, the only valid opinions regarding the validity of the chiropractic paradigm are ones based on philosophical grounds, and here again Coulter^{4,5} has provided remedial educational material in the pages of *JMPT* to assist Dr Keating.

Curiously, Dr Keating states that the concepts of joint misalignment and "unusual" joint movement (curious choice of terms) are to his knowledge unchallenged, yet he then goes on to make the illogical statement that it has not been proven that these conditions cause health problems.¹ According to the definition of health, these entities are themselves a health problem. How can pathologic alignment or movement not create a health problem when, by definition, anything less than proper function is in fact ill health? Also, since improper joint movement and alignment are termed "subluxation complex" in the chiropractic paradigm, it seems that Dr Keating is, in fact, recognizing the entity of subluxation but merely refusing to accept chiropractic terminology.

Perhaps what Dr Keating is alluding to when he questions the validity of subluxation based on the grounds that it has not been proven to create health problems is that these pathologic processes have not been conclusively shown to cause disease. This is open to, but not worthy of, debate because the topic is totally irrelevant. Whether or not a finding leads to a disease is a question within the medical or allopathic paradigm, not the chiropractic one. Once again, let us turn to the pages of *JMPT* for Dr Keating's education. Coulter^{4,5} very definitively points out that the chiropractic paradigm does not include the treatment of disease, so whether or not subluxations cause disease is a question not relevant to chiropractic and certainly not one that has any relevance to the questions of the existence or validity of the subluxation complex. What is relevant to chiropractic is whether or not there are pathophysiologic sequelae to improper joint motion and alignment (which I must once again remind Dr Keating are termed "subluxation complex" within the chiropractic paradigm). The aim of chiropractic is the detection and correction of these lesions involving pathologic joint motion and alignment (which we term "subluxation complex") in order to restore health. The role of science within the chiropractic paradigm is to determine how to detect these lesions, how best to correct them, what their physiologic effects are, and what the physiologic effects of the chiropractic adjustment are. We have a lot of work to do.

Only after years of research within this paradigm we will be able to revisit the issue of the validity of the chiropractic paradigm, armed with enough data to allow rational scientific discourse. Until then, the only valid options are to either choose to accept the paradigm of chiropractic and study and practice within it, or to choose to reject it (on philosophic, not scientific grounds) and practice outside of it. What we cannot do is validly question the paradigm of chiropractic based on the limited amount of available scientific evidence.

I sincerely hope this contributes to Dr Keating's remedial education process. I thank and commend Drs Kent and

Table 1. Examples of independent, mediating, and clinical outcome variables (adapted from Keating)³

Independent Variable (A)	Mediating Variables (B)*	Clinical Outcomes (C)
HIO vs. no treatment Lumbar mobilization vs. manipulation	Upper cervical misalignment Palpable lumbar fixations and segmental algometry	Visual analogue low back pain ratings Oswestry pain scale ratings
Adjusting vs. Dry Bed Training Gonstead adjusting vs. waiting-list control	Urinary bladder capacity Cervical ranges of motion	Number of dry nights per week Headache ratings
Activator instrument adjusting vs. lumbar paraspinal EMG biofeedback	Relative leg lengths	Self-report of sciatica
Anal-sphincter stretch vs. sacroiliac adjustment	Ultrasonic measurements of residual bladder volumes	Self-report of urgency, frequency, and incontinence

*Some of these suggested measures may be construed as potential subluxation-indicators.

Keating for taking the time to write and submit their opinions; this process can only serve to stimulate thinking and force us to more clearly define the very important roles of science and philosophy in improving the art of chiropractic.

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In Response:

My thanks to Dr Chestnut for his reply to my letter to the editor.¹

I believe Dr Chestnut’s comments distinguish his orientation to chiropractic quite clearly from a scientific approach to the hypotheses of traditional chiropractic theory. He speaks of the “inherent limited abilities of science” and would accept a priori that subluxation is a meaningful, valid, clinically useful construct. I acknowledge that there are questions that the scientific method cannot address, but the question of the validity of subluxation hypotheses is

testable. A major limitation in the current science of chiropractic is that so little effort has yet been made to test subluxation hypotheses.

“Chiropractic paradigm” is about as muddy a term as “medical model.” Coulter² suggests that chiropractic “clearly has many of the features of a paradigm, but just as clearly the paradigm has not lead to the research tradition generally associated with scientific paradigms.” The chiropractic questions that interest me flow from the seminal clinical theory proposed by D.D. Palmer. The testable components of Palmer’s theory might be succinctly stated as: “adjusting reduces subluxation, which in turns leads to improved health and/or decreased disease.” This sequence of hypotheses may be represented symbolically as:

$$A \rightarrow \downarrow B \downarrow \Delta C \text{ (} \downarrow \text{ disease and/or } \uparrow \text{ health)}$$

where “A” stands for adjusting, “↓B” symbolizes decreased subluxation, “ΔC” represents change in clinical outcomes, and the horizontal arrows “→” indicate the direction of hypothesized causation. Potential operationalizations of this theory have been suggested elsewhere (Table 1).

Notwithstanding Dr Chestnut’s misgivings, Palmer seemed to have no reluctance about relating his term “disease” to altered physiology and pathology, and devoted many pages of his 1910 volume to naming diseases he believed to be caused by subluxations.⁴ Accordingly, I perceive Dr Chestnut’s dismissal of the notion that chiropractors are or should be interested in the effects of subluxation upon disease as semantically trivial. For the sake of this discussion, “disease” secondary to subluxation is equivalent to physiopathology secondary to subluxation.

These components of Palmer’s proto-theory (A → ↓B → ΔC) can be easily related to the consensus terminology offered by Gatterman and Hansen⁵ a few years ago. Among the definitions for which better than 80% agreement was reached were:

- Subluxation—A motion segment in which alignment, movement integrity, and/or physiologic function are