

Response to Letters From Anderson and Kawchuk et al: X-Ray Imaging Is Essential for Contemporary Chiropractic and Manual Therapy Spinal Rehabilitation: Radiography Increases Benefits and Reduces Risks

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Paul A. Oakley¹ , Jerry M. Cuttler², and Deed E. Harrison³

Thank you for the opportunity to respond to criticisms by members of the World Federation of Chiropractic Research Council (WFCRC) and from Anderson.

In Reply to WFCRC

We are surprised by the theme and what we believe are logical fallacies in the letter from the WFCRC where strong “appeal to authority” and “authoritative” citation arguments were made. In the end, they call for retraction of our commentary, without providing proper data or critique of our position! Their letter exemplifies a form of scientific egocentrism known as “the curse of knowledge”; these WFCRC authors, find difficulty suppressing the content of their personal knowledge base when trying to reason about a less informed, but still legitimate evidence-based, alternate perspective.¹ We point out 5 examples of invalid arguments from the WFCRC with rebuttal.

First, the WFCRC states: “. . . we know of no reputable clinical practice guideline that suggests radiological imaging is a routine requirement for effective treatment . . .” To the contrary, there are several radiology guidelines and recommendations from reputable groups supporting our premise of “routine radiography” in clinical chiropractic practice. For instance, the American College of Radiologists² (30, 000 plus radiologists) have published guidelines that support routine use of imaging in acute and chronic spine conditions. Similarly, the International Chiropractors Association (ICA), one of the largest and oldest chiropractic organizations, has developed a comprehensive spine radiography guideline for practicing chiropractors that includes routine use of spine imaging to aid in the analysis and management of a variety of patient conditions.³ The ICA guidelines have been adopted by several other groups representing thousands of practicing chiropractors as

well as surveyed chiropractors (including all 50 states and the District of Columbia) by the National Board of Chiropractic Examiners,⁴ which identified that reviewing radiographic images to determine the presence of spine displacements or altered alignment was rated as a chiropractic professional function having “significant importance.”⁴

Second, the WFCRC states: “When imaging is performed, there is evidence that it does not improve patient outcomes, but can result in undesirable and unintended effects” and referenced 4 articles over a decade old! One of their cited articles is a magnetic resonance imaging (MRI) study that does not apply to our original manuscript (Ash et al, 2008). In contrast to the WFCRCs interpretations, the Kerry et al’s (2002) study identified patients receiving X-rays of their low backs had better psychological well-being and less depression scores at 1-year follow-up. Likewise, their third citation from Djais et al (2005) was a small trial with only 3-week follow-up that merely demonstrated that low-back pain (LBP) patients deemed to require X-ray investigation (by MDs) improved less than those patients not determined to need imaging on initial assessment, indicating they were more severe to begin with. The WFCRCs fourth citation is the Kendrick et al’s^{5,6} study, which ironically found that two-thirds of all patient X-rays

¹ Private Practice, Newmarket, Ontario, Canada

² Cuttler & Associates Inc, Vaughan, Ontario, Canada

³ CBP NonProfit, Inc, Eagle, ID, USA

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Corresponding Author:

Paul A. Oakley, Private Practice, Newmarket, Ontario, Canada, L3Y 8Y8.
Email: docoakley.icc@gmail.com



were abnormal and that 61% of the patients remained in pain at 9-month follow-up. In direct contrast to the WFCRC's assertions, patients in the Kendrick study who received radiography were in fact more satisfied with the care they received.^{5,6} It was also determined that patients allocated to a preference group, where the decision to receive lumbar radiography is made by them, achieved clinically significant improved outcomes compared to those randomized to a non-radiography or a radiography group.⁶

Third, the WFCRC states: "we also know of no high-quality clinical trials that would contradict current guideline recommendations about imaging." Problematically, the guidelines cited by WFCRC either pertain to medical practice (MDs) or the one specific to spinal manipulation (Bussieres et al⁷) only included 3 original studies! Furthermore, WFCRC fails to acknowledge existing guidelines that oppose their ideology.^{2,3,8} And most important, as discussed in our article, several recent RCTs demonstrate a trend that patient-specific spinal rehabilitation programs designed from radiographic assessment lead to superior outcomes versus conventional or "cookie-cutter" treatment approaches. These RCTs (our original references 2-10) are all high quality and, ironically, had been presented at biennial congresses that the WFCRC organizes.

Fourth, the WFCRC states: "... we do not know of high-quality evidence to suggest that regular imaging is needed to improve the safety...". Contrary to this WFCRC position, studies specifically considering the role of chiropractic treatment interventions have indicated that spine radiographs confirm that a high percentage of patients have abnormalities affecting treatment.⁹⁻¹³ Information regarding anomalies and pathologies is especially important for chiropractic clinicians performing structural corrective types of techniques (as discussed in our original manuscript) aimed at altering the spine and posture.

Last, the WFCRC states: "While knowledge in topics such as radiation exposure modeling and radiation hormesis continually evolves, there are no large-scale studies that would justify the application of this principle in clinical practice today."

We believe the WFCRC concern and alarm stem from the false fears that have been communicated over the past 62 years about an increased risk of cancer due to a low exposure to X-rays or other ionizing radiation.¹⁴ Since we have already provided solid information in plain language about the *beneficial* effects of the dose used in radiographic imaging, we assume their opposition is due to misunderstanding or disbelief of the evidence provided (*curse of knowledge*).

There is strong evidence that the U.S. National Academy of Sciences misled the entire world in 1956 when it recommended use of the linear no-threshold (LNT) model to assess the risk of radiation-induced mutations. The apparent aim of this recommendation, supposedly based on deliberate falsification and fabrication of the research record, was to stop the testing and proliferation of atomic bombs. However, it linked any radiation exposure to an elevated risk of cell mutations (cancer), resulting in a massive radiation health scare that continues to this day.¹⁵

Our commentary had a good discussion with supporting evidence on the modern biology of the health effects of low doses of X-rays. Returning to the mechanism for the beneficial effects, each radiograph up-regulates many of the powerful protection systems that operate against both radiation-induced damage and the much larger rate of naturally-occurring damage due to oxidative stress.¹⁶

Human evidence (96 800 Hiroshima atomic bomb survivors was presented in our commentary showing a radiation dose threshold at about 500 mSv before any increase in cancer incidence (leukemia) is observed. The spinal radiograph dose is more than 100 times lower than this threshold. The authors of the letter have provided no evidence of harm from a radiograph. Therefore, there is no health-related reason to discourage routine use of radiography in manual spine therapy.

We Now Address Anderson's 3 Criticisms

First, Anderson states: "The authors of this paper use self-citation almost exclusively to substantiate statements inaccurately depicted as being representative of the general scientific literature." This is false. Anderson provides 3 examples of self-citation. The first 2 pertain to references 2 to 10; however, only references 2 to 5 involve one of us (Harrison), whereas references 6 to 10 are not coauthored by any of us. His third example refers to our references 6 to 8, but none of these were coauthored by any of us. References 2 to 10 are all of high quality and not distortions of the literature. Anderson himself states, "Self-citation is not necessarily inappropriate," to which we concur. Yes, Harrison contributed to 4 of the 9 citations, and this is because he is a trailblazer in the new and contemporary methods to treat spine disorders that are based on biomechanical assessment of patient radiographs. The other references we cited are consistent with those that Harrison coauthored.

Of importance, Anderson did not identify any studies that contradict the findings in our references 2 to 10. Self-citation, does not alter our original statements about the fact that contemporary approaches to patient-specific spine care, derived from precise radiographic alignment alterations, are proving to be superior over traditional cookie-cutter methods that treat the patient regardless of any altered spine alignment variables.

Second, Anderson states: "The scientific literature is inconsistent regarding a causal association between alterations in spinal sagittal curvature and spinal pathology." Anderson fails to properly weight the study by Chun et al.¹⁷ In a systematic review of 13 studies with meta-analysis, they found a "strong" relationship between decreased lumbar lordosis (LL) in LBP patients versus healthy controls. Similarly, Sadler et al¹⁸ performed a systematic literature review of LBP causation including only studies with prospective cohorts followed for a minimum of 12 months. They identified 12 articles with 5459 subjects and concluded that loss of the LL was a statistically significant predictor of LBP requiring intervention. It is odd that Anderson did not mention this 2017 study, but instead brings forth the 2008 paper by Christensen and Hartvigsen¹⁹ in

his effort to claim conflicting results with spine pain and spine alignment.

The Christensen and Hartvigsen¹⁹ review has been strongly criticized for 7 significant shortcomings, 2 of the most condemning being: (1) they misrated at least 2 key studies that would have reversed their conclusion and (2) they omitted 74 studies that should have been included in their review. A reanalysis was performed using their data along with the 74 omitted studies. It revealed: “the majority of studies (100/128 or 78%) have found a positive association between sagittal plane curves/posture and health disorders.”²⁰

Likewise, Anderson’s reference of the study by Shortz and Haas²¹ falls short of the mark. The Shortz and Haas study suffers from substantial methodological flaws that fail in light of the current knowledge base and should never have passed peer review. For example, to support the contention of “conflicting results” of the association between LBP and lumbar curve, they reference studies dated 17 to 33 years. To assess LL, they used a 4-line Cobb angle method, whose standard error of measurement (SEM) can be as high as 10° (A 30° LL cannot be discerned from a 20° and a 40° curve),²² instead of using more accurate 2-line methods (ie, Harrison posterior tangent method SEM $\leq 2^\circ$) that were shown to be more precise in assessing the LL 20 years previously.²³ Shortz and Haas failed to account for body mass index, a known confounder of LL.²⁴ Lastly, Shortz and Haas neglected the most important analytic variables known to discern normal from abnormal LL, namely the exact correlation of the LL versus sacral inclination (SI), LL versus pelvic morphology (PM), and SI versus PM in each person.²⁴⁻²⁶ In contemporary spine sciences, it is established that an individual’s correlation between LL versus SI and both SI and LL versus PM are very important for determining the presence or absence of LBP, disability, need for intervention, and outcomes.²⁴⁻²⁶ Shortz and Haas concluded that lumbar radiography does not aid in the understanding and management of LBP—a statement that is conjecture and not possible to make from their flawed study. Thus, Anderson’s assertion of “inconsistent” evidence between spine alignment and pathology is supported only by flawed studies^{19,21} and contradicted by many contemporary high-quality studies.^{17,18,20,23-26}

Third, Anderson states: “The authors fail to discuss the significant problem of false-positive findings in spinal imaging studies, the result of which is overutilization of healthcare services.”

This final point is not found in and is not relevant to our original study. Anderson misrepresents our position by bringing forth an entirely different argument. He presents only the first of the 2 works by Brinjikji et al,^{27,28} the systematic review of asymptomatic subjects where 32 of 33 studies in their analysis used MRI as the primary diagnostic method. Nowhere in our paper did we discuss MRI or frequency of degenerative spine conditions in symptomatic groups versus controls as a rationale for X-ray-based corrective spine care. Note that Anderson omits any mention of the follow-up meta-analysis by Brinjikji et al²⁸ that details 14 studies covering 1193

asymptomatic subjects matched to 1904 symptomatic subjects, up to 50 years of age. It identified that “MR imaging evidence of disc bulge, degeneration, extrusion, protrusion, Modic 1 changes, and spondylolysis is more prevalent in adults 50 years of age or younger with back pain compared with asymptomatic individuals.” Thus, the same authors confirmed that (MR) imaging enables the practitioner to discern between normal and abnormal subjects.

Anderson like the WFCRC uses MD references inappropriately in an attempt to substantiate that imaging is overutilized in chiropractic. For medical-based pharmacologic advice or generic exercise management of LBP, we agree that imaging is certainly overutilized. However, in chiropractic and other manual therapy approaches discussed in our paper, there are many reasons why X-rays are indeed essential—and not limited to the low back—in the practice of contemporary and evidence-based methods in daily practice.


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ORCID iD

Paul A. Oakley  <http://orcid.org/0000-0002-3117-7330>

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