

Use of Fallacious Arguments, Ad Hominem Attacks, and Biased 'Expert Opinions' Can Make CBP Research 'Appear Flawed'

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Introduction

In a recent editorial, Cooperstein et al.¹ made numerous claims, supported only by their Level 5 evidence (opinion), concerning an article which we authored in the December 2005 issue of this journal.² Our original article was a review of publications of structural rehabilitation methods utilized in CBP® technique.² The criticisms of Cooperstein et al.¹ can be categorized into 8 points and is simply a long letter to the editor on our original manuscript²:

- CBP® technique is not popular and well utilized,
- Our classification of Diversified is not correct,
- Our review of the literature and we are inexperienced authors,
- We 'invented' 'structural rehabilitation' of the spine and posture in 2005,
- We are self-serving and our experience and neutrality is questioned
- CBP® trials are flawed due to use of SMT in the first 1–3 weeks,
- CBP® trials are flawed due to our recruitment method of control subjects,
- CBP® should leave its' review to 'independent' researchers.

In this editorial, we rebut these criticisms and will show them to be based on 'fallacious arguments', Ad Hominem attacks, and inaccurate Level 5 'expert opinions'. In reality, the criticisms offered by Cooperstein et al.¹ have no legitimate scientific evidence verifying that these actually affected the results and conclusions of our 6 clinical trials^{3–8} and original manuscript.²

In their introduction, Cooperstein et al.¹ claimed that our methodologies in *all* our 6 CBP® Clinical Control Trials^{3–8} were so poor that "we do not think they would satisfy the inclusion criteria for a rigorous, unbiased sys-

tematic review." To claim that 6 clinical control trials are all flawed, without any evidence other than Level 5 (opinion), points to a strong bias against CBP® in general. The discriminate reader will note that the supposed flaws from Cooperstein et al.¹ only loosely applies to one³ of the CBP® trials and no scientific references were offered to show the 'flaws' confound or cause errors in the reliability and validity of the data collection and analysis in CBP® trials.

In fact, the reader will note the remarkable consistency of our findings in our six separate trials in terms of chronic pain improvement and spinal correction in the treatment groups compared to no change in these variables in the control groups.^{2–8} If the flaws¹ were legitimate they would not have manifested themselves in such consistent, predictable outcomes² and the data would have been unpredictable and variable.

Before we begin our itemized rebuttal, a review of a few "Levels of Evidence" from different sources^{9–11} is necessary. This information reveals that 5/6 of CBP's clinical trials can be classified as non-randomized clinical control trials (NRCTs)^{4–8} (second highest rating) where three of these^{4–6} have 1.5 year follow-ups and one³ can be classified as a Level 3 observational cohort control study. It is interesting to note that Cooperstein et al.¹ insult us by referring to our studies as "spiffed up practice-based research". The opinions of Cooperstein et al.¹ are categorized as Level 5 below and do not supersede Levels 2 and 3 evidence by themselves without supporting evidence. For example, we quote from the United States Department of Health and Human services⁹:

"Level 1. *Randomized controlled trials* –

Level 2. *Non-randomized controlled trial* – a prospective (pre-planned) study, with predetermined eligibility criteria and outcome measures.

Level 3. *Observational studies with controls.*

Level 4. *Observational studies without controls."*

Level 5. Expert opinion.

In our rebuttal to their editorial, we will use abbreviations of Cooperstein et al.'s¹ section titles for the reader's ease to compare their points and our replies.

CBP Utilization

Cooperstein et al.¹ chastise us by stating that "CBP was not even included by the NBCE in its job analysis, which listed 15 technique systems in order of usage." This is political in that (1) previously we requested that NBCE include CBP® in their questionnaires, but they refused, (2) the 1992 ACA Council on Technique Conference identified CBP® as one of only 15 techniques taught at Chiropractic Colleges,¹² and (3) CBP® is in the curriculum at 5 USA chiropractic colleges [Life-West, Life University, Palmer-West (where Cooperstein is a faculty member), Cleveland LA, Cleveland KC]. Additionally, for a few years, CBP® was taught at NYCC. Since the NBCE has never included CBP® in its' surveys, we truly do not know the extent to which CBP® is utilized in clinical practices. This is nothing more than political agenda; not evidence that discredits the clinical utilization of CBP® technique.

Study Design and CBP® vs. Diversified

In these sections, Cooperstein et al.¹ chastise us for (1) distinguishing spinal manipulative therapy (SMT) from Diversified, (2) for coining "a new category of health-care-structural rehabilitation", and (3) for developing a

systematic review that is flawed. We will address items (1)–(3) separately.

Item 1: The reader will note that in Table 1 of our original article,² we defined SMT and Diversified as:

For Cooperstein et al.¹ to claim that "the term SMT cannot be neatly distinguished from the set of procedures commonly conveyed by the term 'Diversified'" is absolutely in conflict to the text written by one of their authors,¹ Peterson.¹³ In fact, we utilized this 1993 Chiropractic Technique text to define 'Diversified'.¹³ On page 126, Bergmann et al.¹³ state, "The central physical feature distinguishing chiropractic adjustments from other manual procedures is the delivery of a precisely gauged *adjustive thrust* of controlled velocity, depth, and direction." From this definition of Chiropractic adjustments,¹³ we note that general Osteopathic SMT maneuvers, PT mobilizations, and other European Manual Therapy manipulations are excluded.

For our definition of Diversified in our original article,² we noted that, on page 263 of Bergmann et al.'s 1993 text,¹³ they defined "indications" (listings), "patient position", "doctor position", "segmental contact", "contact point", "indifferent hand" (stabilization hand), and "vector" (line of drive).

We submit that the definition of Diversified in Table 1 is exactly what has been and still is used in Chiropractic today. It is the definition used at the majority of Chiropractic Colleges, and in all National Board Exams. The

Table 1 Definitions

SMT (Spinal Manipulative Therapy)	Spinal Manipulative Therapy defined as application of high-velocity, low-amplitude manual thrusts to the spinal joints slightly beyond the passive range of joint motion (Haldeman & Phillips, 1991)
Diversified Technique	Specific Spinal Manipulation Technique with the following steps: 1) spinal listing (body left, PRS, etc) derived from Motion Palpation or X-ray analysis; 2) specific patient position; 3) specific doctor position; 4) specific contact point; 5) specific line of drive opposite the spinal listing.

definition given above for Diversified is exactly what we authors learned at four separate chiropractic colleges.^{14–17}

For Diversified Chiropractic Technique III at Logan College of Chiropractic (LCC), course #5123, on page 7 of the 1991 class notes, we find the following quote: “The student will demonstrate proper 1. patient position, 2. doctor position, 3. hand position, 4. lines of thrust, and 5. thrust position”.¹⁸ Continuing with Diversified texts, Reinert¹⁹ discussed 7 of the 12 “Manual Contact Points” on the hand, “The vertebral contact point”, “direction of subluxation” (P, A, R, L, S, I, SS), Types of Thrusts, Positioning of the Patient, and Doctor Position.

From the New York Chiropractic College Diversified TECH 6304 class notes (dated 5/08/02),²⁰ we find the following short-hand, in review Tables, for lumbar, thoracic and cervical adjusting procedures: DP (Doctor Position), CP (Contact Point), SCP (Segmental Contact Point), IH (Indifferent Hand), TP (Tissue Pull), VEC (Line of Drive vector), TAB (Table position). Thus far, we have confirmation of our definition of Diversified from the curricula of 6 chiropractic colleges in the USA.

In fact, National College is where Bergmann et al.¹³ claim Diversified was originated by Janse²⁰ in 1947 and National used State’s text²¹ in 1967 where the definition for Diversified as in Table 1 is used. Now we note that Cooperstein et al.¹ are attempting to redefine Diversified technique. They have attempted to lump together everything in manipulation into Diversified, including all Manual Therapy, Osteopathic maneuvers, PT mobilization, Gonstead, and what was Diversified before 1995.

Lastly on this topic, we ask the reader if: ‘our definitions’ in Table 1 are not exactly how they learned Diversified and SMT in Chiropractic College?

Item 2: Cooperstein et al.¹ claimed that we coined a new category of healthcare-“structural rehabilitation” and that “Clinical Biomechanics of Posture” was just invented by us in December 2005.² We thank them for complimenting us on our ingenuity for originating a new category of healthcare in 2005, but alas ‘structural rehabilitation’ of the spine and posture was defined in the literature in 1998²³ in the exact manner as our 2005 article and in 1992,²⁴ using slightly different wording, as ‘chiropractic reconstructive care’. The facts are that CBP® technique was originated in 1980, Chiropractic Biophysics® and

Clinical Biomechanics of Posture®, which are both synonyms for CBP®, were registered trademarks in 1997, 1998, and 2002, respectively.

Item 3: Cooperstein et al.¹ claimed that our review is flawed. We had not seen a rating for non-randomized clinical control trials and cohort trials with controls (rating *within* Levels 2–3^{9–11}), so we devised our own logical rating system. Since the primary purpose of our original manuscript was to present clinically relevant evidence based guidelines for CBP® structural rehabilitation treatment, we did not see the need to rate the individual study methods as commonly performed in systematic reviews.²⁵ The fact of the matter is that this would not have changed the ratings of Diversified nor CBP® if we had.

Referring to Table 3 in our original manuscript, they¹ complained of our rating scale for evidence in randomized clinical trials (RCTs) versus non-randomized clinical trials (NRCTs). The more subjects a trial has the less chance of making a type II error and the exact number is dependent on the variable(s) being investigated and the study design.²⁶ Rating studies with 30 or greater subjects higher seems appropriate. In retrospect, we should have only deducted 1 point for less than 30 subjects so that RCTs would be higher than NRCTs (9 vs. 8). Regardless, this would not have changed our results.

They¹ also complained that we gave a lower rating for RCTs and NRCTs in CINALH/Mantis than in the Index Medicus. We believe that this is an obvious distinction that is logical by looking at the “impact level” and “citation index” of the journals in these indexes.

Last, and most important, Cooperstein et al.¹ fall prey to the ‘Fallacy of Irrelevant Conclusion’. According to Stein²⁷, this is one of the five fallacies in scientific debates. This fallacy is evident when an individual intends to establish a particular conclusion by shifting his/her argument to another topic’s conclusion. Cooperstein et al.¹ are guilty of the ‘Fallacy of Irrelevant Conclusion’ because there is no evidence that inclusion of any of their literature rating critiques would have changed the Diversified rating and any of the conclusions in our original manuscript.²

Inadequate Literature Retrieval

Cooperstein et al.¹ chastise us for a poor literature

search and claim to have found “261 trials” between 1990 and 2005 in a “superficial search”. However, they¹ note that Meeker and Haldeman²⁸ only had 73 clinical control trials in their 2002 publication. As we stated in our December 2005 article,² we used the systematic review of Bronfort et al.²⁵ (69 trials), then we added additional papers found from med-line since that time. We note that the new CCGPP Guidelines for low back pain only included 66 RCTs.²⁹ If Cooperstein et al.¹ found 261 trials (these are not listed by them), we believe all four of these groups^{2,25,28,29} would like to see their list, so we all can do a more thorough review.

Cooperstein et al.¹ correctly identify that we missed the UK Beam II³⁰ study in our previously literature review of SMT. However, inclusion of this study would do nothing to change the overall conclusion of our original manuscript² in as much as this trial did not use Diversified either. Also, Cooperstein et al.¹ presented no evidence that any of their identified 261 trials utilized Diversified. Again, Cooperstein et al.¹ fall prey to the ‘Fallacy of Irrelevant Conclusion’.²⁷ In other words, incorporation of this complaint into our rating system would not have changed the net results or the conclusions of our manuscript.²

Flawed Systematic Review

Cooperstein et al.¹ stated, “we are not confident in Oakley et al.’s experience and neutrality as systematic reviewers” and refer to Bronfort et al.²⁵ as more adept at performance of systematic reviews. In doing so, Cooperstein et al.¹ commit the ‘Fallacy of Appeal to Authority’²⁷ with the sole intent of discrediting our work because we did something different than Bronfort et al.²⁵

Cooperstein et al.¹ complained that we removed RCTs with 11 subjects or less in our original manuscript,² whereas Bronfort et al.²⁵ removed RCTs with less than 10 subjects. Bronfort et al.²⁵ offered no reason as to why trials with less than 10 subjects were excluded. However, we did note that one of the trials with 10 subjects was Bronfort’s³¹ and this would seem to be the reason why Bronfort et al.²⁵ eliminated studies with 9 or less subjects while retaining those with 10.!

By exclusion of trials with 11 subjects or less in our original manuscript,² only studies with 15 subjects or more were left; we feel justified by doing so unless Cooperstein et al.¹ can offer actual evidence (not their opin-

ion) showing how we erred.

Ad Hominem Attacks

Throughout their editorial, Cooperstein et al.¹ commit numerous Ad Hominem attacks. The Ad Hominem attack is one of the fallacies in scientific debates; instead of critiquing the science, attack the character of the individual.²⁷ According to Stein²⁷, when an individual resorts to an Ad Hominem attack, they have lost credibility. We present their four¹ (there were more) primary Ad Hominem attacks:

Disabling Conflict of interest: Cooperstein et al.¹ stated, “A reader naïve to the associations of these authors to CBP, who only looked at the affiliations shown in the paper, would never know that some or all of them have a pecuniary interest in CBP.” In several more areas of their text, Cooperstein et al.¹ claim that we are so biased that readers should not trust our review. This is the epitome of an Ad Hominem attack and has no place in a scientific debate.

IRB Use in CBP® Trials: Cooperstein et al.¹ stated, “No information is supplied about the Institutional Review Board(s) that approved the studies, nor what measures were taken to protect the rights and interests of the non-treated controls.” This statement has nothing to do with the science of our 2005 review article.² In our view, this is an Ad Hominem attack on the ethics of the CBP Non-Profit Institutional Review Board (IRB) practices.

CBP® NonProfit’s IRBs follow the USA “Federal Register, Vol. 46, No. 17, Tuesday, January 27, 1981; rules and Regulations, Part 16-Institutional Review Boards”. CBP® Nonprofit, Inc. has had an IRB each year since 1993 that meets these federally mandated rules and regulations.

Self-Serving: On page 100, Cooperstein et al.¹ resort to more Ad Hominem attacks, by referring to our work and our authors as “self-serving” and by suggesting we are not sufficiently objective enough to review our own publications. While it is difficult for us to “turn the other cheek” and not respond to these statements, we request that the JCCA readers evaluate our science and not us personally.

Table 2. *Extrapolated From Measured Exposure From State of NJ Diagnostic X-Ray:*^{32,33}

<i>View</i>	<i>SID</i>	<i>Pt. Size</i>	<i>KvP</i>	<i>mAs</i>	<i>ESE</i>	<i>% Annual*</i>
<i>AP Lower Cervical</i>	40"	15cm	78	20	58mRem	1.16%
<i>APOM</i>	40"	12cm	72	20	46mRem	0.92%
<i>Lateral Cervical</i>	40"	10cm	70	20	42mRem	0.84%
<i>AP Thoracic</i>	40"	23cm	78	80	280mRem	5.60%
<i>Lateral Thoracic</i>	40"	30cm	85	80	401mRem	8.02%
<i>AP Lumbar</i>	40"	23cm	78	100	350mRem	7.00%
<i>Lateral Lumbar</i>	40"	30cm	90	200	1123mRem	22.46%

*Percentage of the US Government Annual Industrial Worker Safe Limit of 5 Rem³⁴

Lack of Protection of Control Groups: In several places, in particular page 101, Cooperstein et al.¹, state that we are not protecting our Control Groups; they offer no references for such a statement only their opinions.¹ Again this is an Ad Hominem attack, however, we will briefly respond to this one.

Presumably, Cooperstein et al.¹ are referring to exposure to ionizing radiation of our subjects. According to NCRP Report 102, the safe annual dose for industrial exposure is 5 rem = 5000 mrem.³² The state of NJ Department of Environmental Protection, Bureau of Radiologic Health have standardized diagnostic exposure ranges for 3 of the common x-ray views.³³ Based on these exposures, the Bureau of Radiologic Health³³ was able to extrapolate the likely data for exposures of common chiropractic x-rays presented in Table 2.

Based on the above extrapolated data in Table 2, it is likely that a 'full spine' chiropractic x-ray series consisting of 2 cervical x-rays (100mrem), 2 thoracic x-rays (681mrem) and 2 lumbar x-rays (1473mrem) would comprise up to about 2200mrem. This is less than half of the 5000mrem (5rem) deemed as safe annual industrial exposure and does not factor in other radiation.³⁴ Importantly, our control groups only received two x-rays (an initial and follow-up) on one area being investigated. There is no known research to show this level of radiation is associated with any risk. It should be noted that the US Department of Environmental protection (EPA) desig-

nates one medical x-ray as having an average exposure of 40mrem and 5rem of exposure is equivalent to 125 x-rays.³⁴

Lastly, the Health Physics Society released a position statement on Radiation Risk in Perspective on March, 1996.³⁵ It stated: "In accordance with current knowledge of radiation health risks, the Health Physics Society recommends against quantitative estimation of health risk below an individual dose of 5 rem = 5000 mrem in one year ... Risk estimation in this dose range should be strictly qualitative accentuating a range of hypothetical health outcomes with an emphasis on the likely possibility of zero adverse health effects."³⁵

Therefore, the scientific evidence indicates that our control groups were not placed in harms way.³²⁻³⁵ We recommend that Cooperstein et al.¹ stop with the Ad Hominem attacks, stick to science, and cease from using scientific journals for inappropriate diatribe.

Flawed Supportive Clinical Evidence for CBP

Initially, Cooperstein et al.¹ made blanket claims that our CBP® clinical studies have "serious flaws" without supporting evidence. Finally, after 4 pages of this, we arrive at Cooperstein et al.'s¹ three complaints of "flaws" in the CBP® clinical trials: (1) SMT was utilized together with CBP in these 6 trials, (2) NRS reporting, and (3) Control group items. Besides these three "flaw" complaints, they make several other disjointed complaints, which we will refute.

Use of SMT, Flaw 1: Cooperstein et al.¹ claimed that, in our 6 CBP® trials, SMT was utilized in the first 1–3 weeks (3–9 visits) of care in the treatment protocols of our studies with an average duration of 9–12 weeks and 36–60 visits. They claim that this precludes us from making a comparison of CBP®, Diversified and SMT. Firstly, the reader should review our clinical algorithm² where it is obvious that CBP® protocol is to use traditional chiropractic methods in the first 1–3 weeks of patient care. In 4/6 of our clinical studies, we investigated the complete CBP® protocol to determine its' outcomes. If we had not included this 1–3 weeks of SMT, then we would not have studied the CBP® protocol.

Secondly, it's not a "Flaw" to have SMT utilized in the first 1–3 weeks of a 12 week care program, but a "limitation". We discussed our "limitations" in each manuscript.^{3–8}

Thirdly, and most important, in one of our trials, we did not use SMT in the first 1–3 weeks of care.⁷ Apparently, Cooperstein et al.¹ failed to read the methods. Here, we reported on reductions in lateral head translations with the treatment consisting solely of CBP® mirror image methods.⁷ It is interesting to note that the same reductions in chronic neck pain and the same reductions (50%) in x-ray displacement were obtained as compared to our studies with the use of initial SMT.^{2,7}

NRS Pain Reporting, Flaw 2: Cooperstein et al.¹ asked why we did not report the numerical pain score (NRS) intensity of treatment subjects just after the SMT care and before CBP® care. The answer is that, in 5 of our CBP® Trials,^{4–8} we had this data in daily soap notes on each patient, and this unpublished data shows that the patients continue to improve in their pain scales after the SMT phase of care, and during the CBP "structural rehabilitation" care progression. We elected not to report this data because it was completed by the treating clinician (DE Harrison) who was not blinded to the study design.

Second, we note that 11 of the 32 RCTs on SMT analyzed in our original Tables 4 and 5² utilized other treatments besides SMT. So would Cooperstein et al.¹ claim that all these RCTs are "flawed" too? We used multiple CBP® treatment methods in our trials, including mirror image® postural adjustments, mirror image® exercises, mirror image® postural traction, and sagittal curve traction because this is the CBP® protocol of care for which

we were studying.

Third, we need to reiterate that the 5 CBP® NRCTs with pain intensity outcomes^{4–8} show a consistent trend for more improvement compared to trials on SMT.² The 5 CBP® trials show the NRS to be 75% improved after 'structural rehabilitation treatment' compared to only 45% improved after 'standard care'.² Lastly, Cooperstein et al.¹ fail to acknowledge that the CBP® case studies, detailing patient response with chronic cervical and lumbar disorders, demonstrate that the chronic pain improvement is a result of the CBP® 'structural rehabilitation' and not SMT.^{36–41} In other words, the CBP® case studies show remarkable consistency with the CBP® clinical control trials. Other authors have also found that well designed case reports are consistent with the results of RCTs and thus well performed Level 3 and Level 4 investigations are good evidence.^{42–45}

Control subjects, Flaw 3: Continuing in this section (page 100) Cooperstein et al.¹ complain about our control subjects in our 6 trials: (A) our method of recruitment of control subjects makes the control groups "useless", (B) the study personnel must be masked to obtaining pre and post-treatment pain scores from experimental subjects or controls, (C) post checks on experimental and control subjects were not always obtained at the same time period, and (D) we did not disclose enough information about our control subjects (only basic demographics). We expand on each of these complaints below:

A and D): In 5/6 of our clinical trials, the control subjects were chronic pain patients who presented for an initial examination, consultation, and report of findings.^{4–8} These chronic pain patients, after examination and x-rays, were given treatment choices and decided against having treatment. Following their decision not to pursue treatment, these subjects were asked if they would volunteer for a clinical trial as control subjects. Thus our recruitment of control subjects does not fit Cooperstein et al.'s¹ complaints. These were prospectively, non-randomized, chronic pain subjects who self elected not to treat. We appreciate the opportunity to address this, as in retrospect we could have included this important explanation in our manuscripts.^{4–8}

B): All our CBP® trials were performed in two chiro-

practic clinic settings, one in Saugus, Massachusetts³ and five in Elko, Nevada.⁴⁻⁸ In both of these clinics the treating clinicians do not administer patient physical examinations and radiographic examinations. These doctors have interns who rotate out every few months and in fact in 5/6 of the trials, the control group data was collected by different interns on the initial compared to the follow-up examinations.⁴⁻⁸ Importantly, these interns did not have privilege to knowing whether or not subjects were even involved in a study or not. The only thing the exam doctor knew was whether or not the patient had been treated at the follow-up exam; they did not know if the subject was involved in a study. When looking at the reliability of examiner recording of and patient reporting on the NRS, we find that it is reliable.^{46,47} Therefore, different examiners performing the re-evaluations would not be expected to cause problems with the data. Thank you for the opportunity to clarify this.

C): As far as “post checks on experimental and control subjects were not always obtained at the same time period”, we believe that this is an asset. Our longer time periods for control groups’ initial and post evaluations, verifies that chronic pain is stable (does not improve) over time periods of 3 to 12 months. Cooperstein et al.¹, however, claimed that, “The fact that the controls’ pain levels stayed at about the same relatively high level for several months in each of the CBP Trials that reported on pain is very puzzling, since pain levels tend to decline due to the passage of time.” We note that no reference was provided by Cooperstein et al.¹ to support such a claim, only Level 5 evidence.

Our prospective clinical trial data on 5 different populations with chronic neck pain and low back pain contradicts their statement¹ (Level 2 evidence outweighs Level 5 evidence).⁴⁻⁸ Dixon⁴⁸ claimed a “90% recovery” of acute LBP. Problematically, Dixon⁴⁸ used a retrospective review of one doctor’s records to label patients, who did not return for care, as being “*symptom free*”.⁴⁸ This is obviously an unjustified assumption since only subjects, *who show up for follow-up evaluation*, are to be included in statistics for a study. In fact, there is no evidence supporting the claim that 80–90% of LBP patients become pain free within 1 month.⁴⁹ A minimum of 75% of patients with acute uncomplicated LBP will continue to have problems. At 3 and 12 months follow up, only 39/

188 (21%) and 42/170 (25%) respectively will be recovered.⁵⁰ The same general trend, that neck pain does not improve on its’ own, can be found for population based prevalence studies on chronic neck pain.⁵¹⁻⁵³

In summary of this section, the complaints by Cooperstein et al.¹ about our control groups’ are without merit. We note that complaint A only applies to one of our studies,³ complaint B does not apply to any of our trials,³⁻⁸ complaint C is irrelevant, is an asset that applies to 5/6 of our trials,⁴⁻⁸ while complaint D was not required by the journals where we were published, but we greatly appreciate the opportunity to have clarified this.

Cost, Time, and CBP® Spinal Models

Cooperstein et al.¹ stated, “For the average patient, we remain unable to determine whether the time and expense required of patients for this CBP goal of care [changing spinal curves] is indicated, nor whether such protocols are consistent with what many have called ‘patient-centered chiropractic’”. Their¹ statement is remarkably similar to that given by insurance reviewers (IMEs) when they deny coverage for legitimate chiropractic claims. What Cooperstein et al.¹ really meant by “time and expense” is that managed care organizations (MCO’s) do not want to pay for it. By bringing up “time” and “expense” in a debate related to validity of structural rehabilitative care using CBP® technique, Cooperstein et al.¹ have committed the ‘Fallacy of Irrelevant Conclusion’. However, we can assure Cooperstein et al.¹ that our care is ‘patient-centered’ and that our patients are glad someone finally corrected their spines and relieved their chronic pains.

Next Cooperstein et al.¹ inaccurately discuss our spinal models, citing Cooperstein,^{54,55} while conveniently omitting CBP® author responses to Cooperstein’s inaccurate statements.^{56,57} In the interval 1996–2003, CBP® spinal models were published for each region of the spine.⁵⁷⁻⁶³ These spinal models are of two types, *average*⁵⁸⁻⁶⁰ and *ideal*.⁶¹⁻⁶⁴ Of primary importance, these average CBP® spinal models have been found to have predictive validity in as much as they can statistically discriminate between normal subjects, acute pain subjects, and chronic pain subjects in both the cervical⁶⁰ and lumbar spines.⁵⁸ It is peculiar that Cooperstein et al.¹ continue to ignore this information.

Review of CBP® Research Should be Done by Independent Object Researchers

Cooperstein et al.¹ make the statement that future reviews of CBP® publications should be performed by independent and apparently more objective authors. However, similar to their current editorial, past works by Cooperstein and Gleberzon,⁶⁵ Gleberzon,⁶⁶ Perle and colleagues,⁶⁷ and Peterson and Bergmann⁶⁸ provide evidence that these authors are not capable of accurate, thorough, and objective reviews of CBP® publications.

In their 2004 text, Cooperstein and Gleberzon⁶⁵ claimed that the CBP® cervical spine model has limited applicability to patients presenting to neck pain in as much as only 5% of subjects' cervical lordosis would fall below 18° or 2 standard deviations from our mean reported cervical lordosis.⁶¹ Inexcusably, Cooperstein and Gleberzon⁶⁵ failed to realize that in the methods section of this cervical modeling manuscript,⁶¹ we reported that any subject with straight, segmental kyphotic, and complete kyphotic alignments were excluded from the study. From other investigations, it is clear that straight, segmental kyphosis, and total kyphosis have a prevalence of 25%–45% depending upon the exact curve type and population being investigated.^{69–72} We submit that Cooperstein and Gleberzon⁶⁵ either failed to read the CBP® manuscript⁶¹ or they have an agenda to discredit CBP® publications.

In a 2001 review of named chiropractic technique publications, Gleberzon⁶⁶ reported that CBP® only had 1 case report⁷³ in the chiropractic peer-reviewed literature. Astonishingly, he⁶⁶ somehow missed our clinical trial published in 1994³ and 9 other case reports and case series.^{24,74–81} We wonder how this is possible.

Perle and colleagues⁶⁷ inaccurately summarized the CBP® spinal models as purely theoretically without clinical application. This comment was previously discussed above under the 'Cost, Time, and Spinal Model' section and was shown to be false. Like Perle and colleagues,⁶⁷ other 'objective reviewers' have mischaracterized the CBP® spinal models as solely ideal or theoretical in character without clinical utility as well.^{82–84}

Lastly, Peterson and Bergmann⁶⁸ failed to even acknowledge the existence of CBP® technique in their text book on 'Chiropractic Technique'.

Thus, if Cooperstein et al.^{1,65–68} and other chiropractic authors^{82–84} are the examples of how CBP® research will

be reviewed and characterized by 'independent reviewers', we would prefer to represent ourselves.

Conclusion

From our above rebuttal to Cooperstein et al.¹, we find their critiques to be based on fallacious arguments, Ad Hominem attacks, and inaccurate Level 5 (opinion) evidence. We find no legitimate scientific evidence in the critiques of Cooperstein et al.¹ that would have altered the results and conclusion of our original manuscript.²

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