To the Editor:
JCCA 2001; 45(1).

May I compliment you on putting together so many practical articles in the March issue of the Journal.

One of the articles I refer to is the scholarly presentation "Chiropractic Quality Assurance Standards, etc." by Meridel Gatterman, D.C., and others. I remember Meridel when she was on CMCC's faculty. She was enthusiastic and a role model for our students.

Among the other articles that impressed me that should be saved for future reference was "A Liability Claim, etc." by Drs. Paul Carey and G. Townsend, and the article on "Lasers and their therapeutic application" by Don Fitz Ritson, D.C., was informative and practical.

I would like to compliment Dr. Doug Brown on his biography of Lyman C. Johnston, D.C. Lyman, a graduate of the first class, who has contributed much to the profession over the years.

The review of the two books on x-ray imaging by John Taylor, D.C. also a CMCC graduate, impressed me. Dr. Taylor, I understand is on the faculty of the New York Chiropractic College, Seneca Falls, N.Y.

Herbert K. Lee, DC
Toronto, Ontario

A liability claim – what to expect;
from initiation to resolution.
JCCA 2001; 45(1):53–61.

To the Editor:

I read with interest the Article by Drs. Carey and Townsend entitled: "A liability claim – what to expect; from initiation to resolution", in Vol. 45 No. 1. I trust that it will give your readership an informative understanding of the realities of professional negligence litigation. I was intrigued, however, to note that there was no mention of the horrendous costs associated with such litigation. In addition, I would suggest that your readership, including those chiropractors who are not members of the Canadian Chiropractic Association be reminded that it is absolute folly and, in my opinion, quite professionally and morally irresponsible to practice chiropractic, or any profession for that matter, without malpractice insurance, which should be maintained not only for the benefit of the practitioner and his or her family, but also to ensure the protection of the patient and the integrity of the profession.

Allan M. Freedman, B.A., LL.B.
Toronto, Ontario

Keating J. The specter of dogma (commentary).
JCCA 2001; 45(2):76–80.

To the Editor:

I suppose any thinking D.C. would have to take the commentary by Dr. Keating most seriously, especially considering it is featured in our national journal.

I am reminded of Dr. Scott Haldeman's testimony in the New Zealand Commission hearings wherein he pointed out that Chiropractic works and this was a frequent rationale for accepting many medical procedures and as D.C.s are often fond of pointing out that only 15% of medical procedures have met the double blind objective "scientific test". No doubt my colleagues around the world who have practiced for any reasonable time have discovered that adjustments to one part of the spine cause a change in another and hence the many systems which all get results. After 36 years of searching for objective evidence other than motion palpation and patient motion – this past year has brought the Pettibon approach to the forefront. To my knowledge this approach is the only one with measurable spinal corrections on x-ray. Similar evidence appears with the Chiropractic Biophysics approach (a spin off from Pettibon). As Dr. Keating points out in his commentary – he is not a member of this profession but would ask his status be that of an "insider/outsider". My own opinion is that having outsiders participate in our search for certainty is quite appropriate as truth or science does not (in my opinion) belong to any discipline. I suspect that in the long haul that biomechanical therapy will be understood by the public and the healing professions for the necessary serv-

ice and value we D.C.s think it is. A quote from Dr. Woggon – Chair of the Pettibon Spinal Biomechanics Research Institute, "Individual spinal segments do not function independently. They have no individual position that they can be placed in that will stress their joint ligaments to at least 40% of ultimate load. Therefore, individual segmental adjusting is not possible." The compensating explanation given in their research I omit from this letter. Another quote from Louis Sportelli, D.C. regarding all the other professions claiming to treat the many terms for spinal lesions such as subluxations. CAN WE DOMINATE THE LANDSCAPE WITH A MONOPOLY ON SUBLUXATION, OR WOULD WE BE BETTER OFF SETTING THE STANDARDS FOR ALL OTHERS TO MEET... ? I am sympathetic to Dr. Peterson and in agreement with his general view that subluxation is semantic rather than scientific. Of course the DOGMA by Dr. Keating that it is not is his belief. My impression of the ACC Paradigm particularly as it applies to the VSC makes excellent sense. Dr. Keating's comment that "we haven't done our homework: we don't know whether the VSC is very important, or trivial, or wholly imaginary. We just don't know." From the viewpoint of the need for experimental evidence this "we just don't know" may be true. However my colleagues, we (that is – those of us delivering adjustments) do know that changing biomechanics with adjustments leaves no doubt about the "bonafide clinical entity." I should like to congratulate Ian Horseman, D.C. for the articles in the Canadian Chiropractor which help us practitioners to know who, what, why and where of some of the approaches to spinal adjusting. As the originator and past chairman of the Chiropractic Foundation for Spinal Research – it pleases me to see the Journal publishing articles such as Dr. Keating's commentary and Dr. Gleberzon's review of the literature regarding techniques. What works and can be shown with objective evidence (such as x-ray) will give us the certainty to satisfy critics and friends alike. My opinion is in agreement with Dr. Sportelli's that "or would we be better off setting the standards for all others to meet?" is the way for this profession to survive all the imitators. Talk is cheap and while we all get results – I have only seen one approach with proof on the x-rays. While I am retirement age – over 65 – it is with great enthusiasm that I have been learning about the Pettibon approach. Here is a quote from Dr. Burl Pettibon in '99 "The Chiropractic Premise is not a lie. Antiquated adjusting techniques without scientific diagnostics and clinical outcomes are keeping the profession in last place in the mind of the public. The great news is that doesn't have to stay that way as evidenced by the West research. It is time to re-invent Chiropractic through technical certainty and deliver what we promise."

E. Allan Hawkins, DC
Winnipeg, Manitoba

To the Editor in reply:

Thank you for your feedback. I would just point out that no matter how strong the clinical trial evidence for the value of adjusting/manipulation, those studies, in and of themselves, tell us nothing about the mechanism(s) of action. Accordingly, the VSC remains a largely untested, and currently unsubstantiated construct. This doesn't mean VSC is not real, just that we don't know. Parenthetically, the Pettibon technique may be wonderful, but it has not been validated, to the best of my knowledge.

I'd also like to suggest that before you too readily accept Dr. Peterson's contention that the problem with subluxation-complex is merely semantic (rather than a vacuum of scientific evidence), please take a look at Craig Nelson, D.C.'s brilliant article:

Nelson, Craig. The subluxation question. Journal of Chiropractic Humanities 1997; 7:46–55.

I'm not sure, but it may be downloadable at National College's website; otherwise, it surely is retrievable at any good chiropractic college library. Very insightful and hard-hitting.

BTW, I too prefer Dr. Sportelli's strategy of setting the standard for training in manipulation, rather than trying to create a monopoly on "subluxation".

Joseph C. Keating, PhD
Phoenix, Arizona

Chiropractic "Name Techniques": a review of the literature.
JCCA 2001; 45(2): 86–99.

To the Editor:

I quite enjoyed reading Dr. Brian Gleberzon's paper reviewing chiropractic "name techniques."[1] However, I think that there are some significant limitations to this study and I would like to detail my concerns.

First, the definition of a "name technique" seems limiting; it is defined as any technique system that "can trace their origins back to individual developers." This seems to preclude the author from using the "name technique" known to be in widest usage within the chiropractic profession, namely, Diversified Technique.[2] When I read through the full list of "name techniques" some do not seem to have a single individual associated with the procedure, i.e. meric or upper cervical. Within chiropractic, Diversified Technique has mainly been associated with the work of Janse, Hauser and Wells[3] and certainly would seem to qualify.

Second, given that this paper does nothing more than survey the literature that it gleaned through its own unique search strategy, and does so in a non-critical manner, I am not certain that any conclusion can be drawn from its data. The statement that the literature suggested that prone leg length testing may have acceptable reliability is based upon a sum total of 3 papers cited in this report, 2 drawn from Activator Methods[4,5] and one from Upper cervical adjusting;[6] one of these[6] actually assesses supine leg length measurements. These 3 papers do not measure the same phenomenon in the same manner and cannot be used to conclude that the procedure is either reliable or valid. The fact that they reported positive results does not indicate that they did so without flaw. It is essential that the information described here be more critically assessed. This is indeed a problem throughout the entire paper.

What we can take from this paper is that there is a paucity of hard evidence to support the use of any of these techniques. We have a good number of case reports and technique descriptions, and a far smaller number of hard research papers, and of these, many are basic science directed and not clinical trials. This is a situation that ought to alarm the entire profession. We still do not know which techniques work best in which clinical situations. I admire those among us who work to answer these questions, and certainly there have been many within the ranks of our technique developers who have made the effort to do so, including such individuals as Arlan Fuhr (Activator Methods), James Cox (flexion distraction technique), and Don Harrison (Chiropractic BioPhysics). Their commitment to advancing our science must be commended. The time for "cult of personality" chiropractic has passed; evidence-based medicine requires our profession to work all the harder to develop that framework of evidence that will forever quiet our detractors. While best practice approaches mix hard data with clinical expertise, that is not a recommendation to use only what expertise is available, no matter how poor. Our patients surely deserve far better.

Dana J. Lawrence, DC
Editor/JMPT

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To the Editor in reply:

Thank you for the opportunity to respond to the letter from Dr. Dana Lawrence regarding my recent article in the JCCA.[1] Two of Dr. Lawrence's comments can be easily addressed; (i) the decision not to discuss "Diversified technique" in my article and (ii) his contention that no

conclusion can be drawn from the study. His concerns about my comments regarding prone leg length testing will also be discussed.

In the first instance, as is clearly stated in the introduction, the purpose of the paper was to examine "Name Techniques" as opposed to "Diversified Technique". Had Dr. Lawrence read the introduction, he would have better understood its objectives and not drawn any false assumptions with respect to its stated purpose. My decision not to investigate Diversified technique as a stand alone should be readily apparent to Dr. Lawrence, who has a wealth of knowledge in this area and has previously written about it (see below).[2] As lucidly described by Cooperstein in detail elsewhere,[3,4] Diversified technique exists as both a stand alone technique (capital "D") in the tradition of States, Janse and others, as well as an amalgam of generic HVLA maneuvers (lower case "d"), which is often a reflection of an individual's personal preference, clinical experience and anthropomorphic abilities. Thus, a search of the literature of "D"iversified technique may be nothing more than a reiteration of the already well-disseminated body of knowledge investigating the efficacy of generic forms of spinal manipulation (see, for example references 5–7). Instead, in keeping with the themes of my earlier article[8] investigating; (i) techniques not currently in the CMCC curriculum and yet of interest to students and (ii) demographic trends of increased utilization rates of Name techniques among Canadian chiropractors (itself often a reflection of American-trained Canadian students returning to Canada), this article sought to investigate a relatively unexplored area of chiropractic study, the Name techniques.

Several years ago, Dr. Lawrence identified the same areas of concern at National College when he wrote: *"Those who teach technique within the accepted curriculum face stiff competition for the hearts and minds of their students from clubs and "technique entrepreneur"... The students at National, where diversified technique forms the core technique curriculum, want exposure to more technique and are jealous of colleges where several forms are taught"(*ref 2:p7). Dr. Lawrence continued: *"Complicating this is the "evangelization" of technique by clubs and entrepreneurs ... All too often, clubs become a quasi-curriculum acting in opposition to the established curriculum, where the message being promoted is that the procedures taught by the clubs are "better" than those taught by the college"*(ref 2:p7). Lastly, Dr. Lawrence captured one of the objectives of my article when he opined: *"I feel that a student is not in a position to make judgements about the varying principles of different techniques, for the simple reason that they have not finished the program and are therefore lacking some of the necessary knowledge base, particularly in clinical biomechanics and neurology"* (ref 2:p7). My article addresses the last concern raised by Dr. Lawrence by helping students (and field practitioners) make more rational decisions with respect to choosing which, if any, of the Name techniques they should possibly incorporate into their clinical armamentarium.

Over the past few years, I have had several inquires from Regulatory boards throughout Canada requesting information on a particular technique that an individual practitioner was utilizing. In addition, several jurisdictions in Canada are currently re-evaluating their guidelines with respect to the use of various Name techniques , and the Glenerin II document will have to devote more space to discuss the issues germane to this area of professional practice. In contrast, there would appear to be less need or urgency to review the literature documenting the efficacy of Diversified technique, however one cares to define it. Simply put, there is relatively limited controversy surrounding the use of Diversified (spinal manipulative) techniques for patient care as compared to the use of Name techniques, and thus I sought to avoid re-publishing what is already widely known in this area.

Secondly, while Dr. Lawrence expresses doubt that any conclusions can be drawn from this review of the literature, he actually re-states my primary findings and the area of greatest concern. Specifically, that there is a paucity of hard evidence to support (or refute) the use of many (but not all) of the Name techniques, despite their growing popularity among students and practitioners. It seems ironic that, on the one hand, Dr. Lawrence seems to think the article has limited value and yet, on the other hand, he appears to be in complete agreement with its most important finding.

In response to Dr. Lawrence's comment about prone leg length, he of course recognizes the two studies he cites[9,10] as recent publications in the well-respected journal he edits. Both articles, when reviewed in their entirety, chronicled much of the work in this area over the past two decades and, while studying prone leg length testing from different perspectives, came to similar conclusions. Had

these studies been published in a less credible journal, I may have viewed them in a different light. However, I felt secure in the knowledge that critical assessment was one of the responsibilities of the peer-review panel of the JMPT, and that both articles ought to have passed high standards of peer and editorial review.

In addition, despite Dr. Lawrence's assertion to the contrary, other articles discussed in my paper, notably articles describing Activator Methods Chiropractic Technique[11] and Thompson Terminal Point technique,[12] also discussed at length the research on prone leg length testing. Thus, stating that there is some evidence that prone leg mensurations may have acceptable levels of intra and inter-rater reliability is defensible. Unfortunately, Dr. Lawrence did not discuss an issue of greater importance; specifically, that prone leg length testing (or x-ray mensurations, for that matter) have not been convincingly linked to clinical findings or outcomes. In my opinion, the issue of whether or not two examiners can agree that one patient's leg is shorter than the other, or whether the same practitioner judges a patient's leg length the same over consecutive days, pales in comparison with the issue of clinical relevance and applicability of this test, ubiquitous as it is throughout the profession. Lastly, no comments were made in my article with respect to supine leg length measurements.

My article did not purport to be a critical appraisal of each of the 111 studies listed, nor were definitive conclusions aggressively asserted. Instead, the article was what it was stated to be, a review of the literature, and I conveyed preliminary findings cautiously. In fact, conclusions as such are not reported in the abstract or in the "conclusion" section.

As Dr. Lawrence mentioned in his letter, this study helps to reveal aspects of "Name Techniques" that have been insufficiently investigated. I think that Dr. Lawrence and I are in agreement that the overall lack of clinical trials investigating Name techniques must be remedied, the sooner the better, although I join Dr. Lawrence in recognizing the significant contributions made in this area by such researchers as Drs. Fuhr, Harrison and Cox. That said, this situation requires a concerted effort by all stakeholders to determine which technique may be best suited for a particular patient with a certain clinical condition and, more importantly, to determine which techniques may be potentially harmful, or clinically ineffective. The results of these essential investigations have the potential to place chiropractic treatment on a more firm clinical and professional foundation and, as Dr. Lawrence opines, our patients certainly deserve no less.

Dr. Brian J. Gleberzon, DC
Assistant Professor, CMCC

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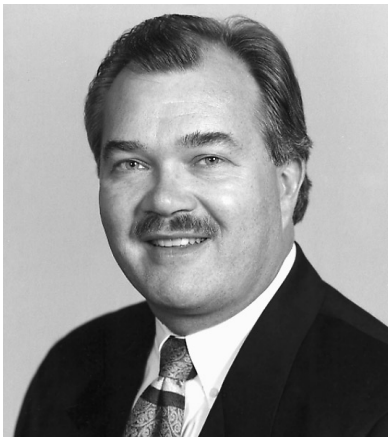

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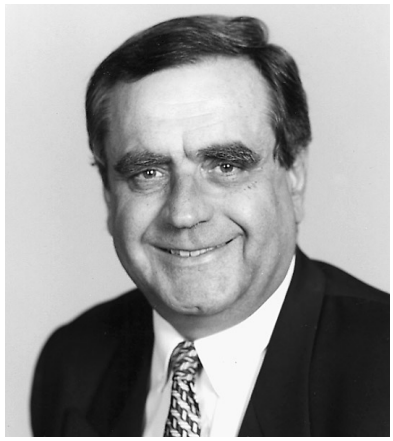

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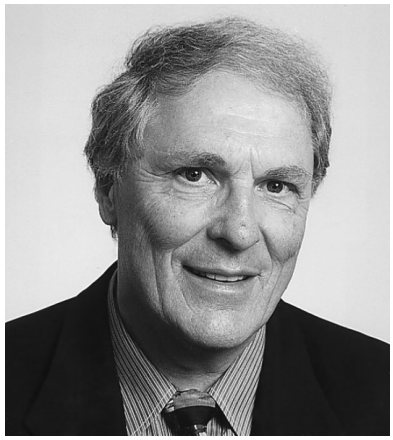

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