Name techniques in Canada: current trends in utilization rates and recommendations for their inclusion at CMCC.
JCCA 2000; 44(3):157–168.

To the Editor:

I wish to commend Dr. Gleberzon on his article regarding the techniques being used in practice and the current trends. I found it informative and it supported my own sense of what I have seen happening. I have been running a locum/associate placement service for the past 7 years in Ontario. In this service we do detailed profiling of our clients' practices as well as the backgrounds of our locum and associate candidates. This has given me a broad overview of the marketplace in chiropractic. I have noticed the same trends that Dr. Gleberzon has demonstrated with his research.

When we first started Pathmark more chiropractors tended to use just one technique in their practices with diversified being the main one. Currently many of our hundreds of clients use several techniques in their practices with Activator and Thompson being the next most used techniques. We are also getting more requests for techniques such as Torque Release and Active Release Technique.

This raises a challenge for new graduates. If they are only proficient at one technique it puts them at a disadvantage in the chiropractic community because established chiropractors tend to want to hire other chiropractors (locum or associates) who practice similarly to themselves.

We are finding that many clients specifically request graduates from some of the American Colleges because many of these students have graduated having been taught some of these other more "popular" techniques. Not only have they been taught the techniques but they have also had the opportunity to practice these techniques in a supervised clinical setting.

I found Dr. Gleberzon's article interesting because it researched and quantified certainly a trend which I have found to be on the rise. It is also important that new graduates be aware of these trends so that they can best prepare themselves to have an advantage entering the chiropractic workforce.

Sher Bovay, BSc, DC
President
Pathmark Locum Service

To the Editor:

This letter pertains to a recent article appearing in volume 44, issue 3 of the JCCA. The article, "*Name techniques in Canada: current trends in utilization rates and recommendations for their inclusion at the Canadian Memorial Chiropractic College*," was authored by Brian J. Gleberzon, D.C., Assistant Professor, Canadian Memorial Chiropractic College (CMCC). Much of his discussion centers around a 1999 survey study conducted by Watkins and Saranchuk of CMCC students and graduates. Their article referenced our research conducted relative to Network Spinal Analysis (NSA) ("A Retrospective Assessment of Network Care Using a Survey of Self-Rated Health, Wellness and Quality of Life", RHI Blanks, TL Schuster and M. Dobson, *Journal of Vertebral Subluxation Research* 1:4, 1997). When the article by Dr. Gleberzon was brought to our attention, we jointly felt that a reply was in order.

Dr. Gleberzon's article states that the Network Chiropractic retrospective study that was conducted had "methodological flaws." This statement is incorrect and we feel obligated as the researchers in question to respond. In regard to research methodology we have much to offer. Collectively, we have authored several hundred papers in peer-reviewed journals representing a wide range of research activities spanning the fields of neuroscience, social science, and chiropractic. This information is presented simply to point out that the study mentioned relative to Network Chiropractic was not idly contrived with "methodological flaws" as Dr. Gleberzon's comments suggest. Given our expertise we feel it is necessary to correct the erroneous assertions put forth by Dr. Gleberzon. The following comments and clarifications are germane.

1. The study was not "methodologically flawed." A methodological flaw occurs when (a) the researcher uses a method that cannot provide data appropriate to the

hypothesis, or (b) the researcher does not properly design or conduct a study within the known process of the chosen method, or (c) the researcher makes claims of evidence that the data cannot support.

None of these apply to the retrospective study. All types of research methods, without exception, have inherent strengths and weaknesses. A careful read of the retrospective study will clearly show that the limitations of our study method were detailed, and conclusions were drawn within that context. Given the parameters of our methodology, a retrospective cross-sectional survey, we were conscientious in defining limitations. Moreover, we were also cautious in not "over-interpreting" the results. As well, caution was exercised in making claims about the type of information provided by the data.

2. Dr. Gleberzon also criticized the lack of comparison of patients who discontinued care.
   "*Some students felt that the research into a particular Name technique was currently inadequate or inconclusive. A study by Blanks et al of patients under Network care illustrates this point. The study involved a large group of patients (N = 2,818) who reported significant benefits while under Network care (measured as improvements in their "wellness coefficient"). However, because the study was retrospective, the researchers were unable to question those patients who discontinued care, possibly because of lack of satisfaction or benefit. This could skew the results. Also, assessing a patient's perceived improvements after a lengthy (and costly) treatment regimen may influence patient responses. Patients may wish to validate their time and money commitments and report disproportionally more favorable results than may have actually been achieved." (underlining has been added).*

Defined population samples are inherent in all survey research (in this case individuals who chose to remain in care). Moreover, anything short of a full census will always leave out potentially important comparison populations. Dr. Gleberzon could just as well have criticized the lack of a comparison group of individuals who never had care, or who had a different kind of care.

Dr. Gleberzon's use of words such as possibly, could, and may, present a one-sided critique. For example, he suggests that individuals in the study may have overstated their perceived benefit since they were paying out of pocket. Since Dr. Gleberzon does not know this to be the case, he is obligated, in our view, to offer the equally plausible alternative that it is just as unlikely that individuals would continue to pay out of pocket, in some cases for years, to receive a service that was providing no benefit for them. Dr. Gleberzon's commentary cannot discount the finding that patients (from several worldwide locations) who chose to remain under care reported statistically significant benefits in all domains assessed.

His statement, apparently drawn from the survey of Watkins and Saranchuk, that "some students felt that the research into a particular Named technique was currently inadequate or inconclusive" is of interest. It appears that Dr. Gleberzon has personally chosen the retrospective study as an example to illustrate the point. This brings into question the motivation behind his critical comments.

3. Dr. Gleberzon also comments as follows,
   "An argument can be made that only those techniques that are "evidence-based" should be offered in chiropractic curricula; however, good quality research comprising this evidence is sparse for every technique, Diversified included. Furthermore, Sackett, an expert on evidence-based medicine, recently commented that '"evidence-based" medicine means integrating individual clinical expertise with the best available external clinical evidence from systematic research, especially from patient-centered clinical research.' This emphasizes the importance of studies such as the one by Blank et al., notwithstanding its methodological flaws."

This is a puzzling statement that appears to contradict the previous criticism of the Network retrospective study. Dr. Gleberzon seems to be debunking the Network cross-sectional retrospective survey method by defining inherent limitations as "methodological flaws," but then advocating that more of this type of research be done. It is ironic that many of the conclusions drawn by Dr. Gleberzon are derived from a survey for which he presents no comment as to its internal, external, or construct validity. The Network retrospective study demonstrated all three of these key elements.

Having presented our views regarding the faulty assertions and conclusions drawn by Dr. Gleberzon, we believe it is important to comment on a research approach that could benefit chiropractic, and certainly his School, CMCC. Health outcomes research requires a tiered evalu-

ation approach. One generally starts with clinical impressions (observations), descriptive reports and case reports and findings among practitioners. The next level of research can be population studies, as exemplified by the Network chiropractic retrospective study. If data suggests further investigation, a longitudinal study should be conducted to further test the validity of the findings. In the case of NSA, a longitudinal study has just been completed, and data is soon to be submitted for publication. If the modality merits further investigation one would consider a longitudinal two-group format with active or passive control in a randomized clinical trial. One does not start with the randomized clinical trial, nor does one stop with the retrospective study.

To our knowledge NSA is the only technique associated with chiropractic that continues to rigorously follow this paradigm. This is underscored by Dr. Gleberzon's acknowledgment that even CMCC's primary core technique does not have an evidence base. Outcomes research pertaining to CMCC's primary core technique could readily be rectified by approaching independent universities to provide the same type of critical investigation as that being applied to NSA.

Critique is a valuable aspect of the research process and as researchers and academicians we welcome intellectual challenge. However, based on the above considerations, it is clear that the comments and criticisms raised in Dr. Gleberzon's article reflect a fundamental lack of understanding of research methodology. We have thus provided an accurate description of the method applied to the Network care retrospective study to counter Dr. Gleberzon's inaccurate assertions.

Robert H. Blanks, PhD
Professor, Department of Anatomy and Neurobiology
College of Medicine
University of California, Irvine

Tonya L. Schuster, PhD
Assistant Researcher, Department of Sociology
School of Social Science
University of California, Irvine

Marnie Dobson, MA, Social Science
Graduate Student, Ph.D. Program in Social Relations
School of Social Science
University of California, Irvine

To the Editor in reply:

Thank you for the opportunity to respond to the letter by Drs. Blanks, Schuster, and Marnie Dobson .

The authors object to my characterization of their retrospective study as having 'methodological flaws", a point with which I must concur. It would indeed have been more accurate to have referred to them as methodological limitations. I regret any misunderstanding this terminology may have caused among your readers. Many of their other comments, however, lead me to question whether they either read the manuscript in its entirety, or if they understood its focus.

Firstly, I did not indicate that the article by Watkins and Saranchuk referred to the study by Dr. Blanks et al., as Watkins and Saranchuk did not refer to this article at all.

Secondly, I did not state, as the letter by Dr. Blanks et al indicated, that there is NO evidence for Diversified technique. Instead, my article attempted to create a level playing field for all chiropractic techniques, and strove to avoid the pitfall of having Diversified technique portrayed as superior to all other techniques. In fact, I indicated that there is evidence for the chiropractic treatment of acute and chronic neck and low back pain, and certain types of headaches.

Thirdly, I attempted to accurately portray the study in question within an appropriate context. The reason I chose to discuss this study was because it illustrated several important features germane to my article. One of these important features is that, although some Name Techniques are attempting to build an evidence-based foundation, they may not be constructing it from the strongest material. For example, this study did involve a large number of patients, it was conducted over a lengthy period of time, and it reported very positive results derived from patients under Network care. However, because the study was retrospective, it did possess many inherent limitations which, upon further review of the study in the Journal of Vertebral Subluxation, were not discussed by the researchers.

In my article, I did not question the accuracy of the evaluation instrument utilized, nor the accuracy of the results gathered, nor even the conclusions reported by the study's authors. What I did discuss were the limitations of this study and how these limitations *may* have influenced the data gathered. I was very cautious to frame these

concerns in qualified statements such as "may", "can" or "possibly" because I recognized that these conditions may or may not have occurred. It was important, in my judgment, to share these inherent limitations with the readers, most of whom may be unfamiliar with research methods. It is for this reason that I sought out content experts in the field of research methodology who intimated that this retrospective study was replete with the limitations I described in the body of my article.

Lastly, in contradiction to the assertion by Dr. Blanks et al., Network Spinal Analysis is *not,* in fact, the only Name Technique attempting to compile a body of evidence to pursue an evidence-base paradigm. In addition to Diversified technique itself, Activator and Upper Cervical techniques, for example, have published a large number of studies. Owens and Hoiiris, have published several articles on a practice-based study involving patients receiving Upper Cervical care which they have conducted over the past few years. To their credit, they readily discuss that a limitation to their study is its high patient attrition rate.

Brian J. Gleberzon, DC
Assistant Professor,
Canadian Memorial Chiropractic College

Subluxation – the silent killer (Letter to the Editor). JCCA 2000; 44(3):177–178.

To the Editor:

I would like to offer a factual point of clarification to a statement made by Dr. Leslie Shaw in his letter (JCCA 2000; 44(3):177–178) that referred to a recent commentary by Dr. Ronald Carter (JCCA 2000; 44(1):9–18). Dr. Shaw stated that "The most recent AGM of the College of Chiropractors of Alberta (CCOA) voted to support the subluxation based clinical guidelines." While this statement is one interpretation of the events of that AGM, the facts are as follows. The actual event referred to by Dr. Shaw was a vote that was taken on a motion that myself and Dr. David Tripp submitted which stated: "Be it resolved we request the council of the CCOA to withdraw its endorsement of the Council on Chiropractic Practice Clinical Practice Guidelines until such time that these guidelines can be critically evaluated and scientifically validated." This motion was defeated by a vote of 69 nays and 54 yeas. This total number of chiropractors voting was above the minimum quorum required from the membership of 670 chiropractors (albeit 10% of the membership defeated the motion) and as such the motion was defeated within the proper rules of order. The membership did not vote to support the guidelines, as was suggested by Dr. Shaw, they voted to defeat the motion. Unfortunately, my clumsy drafting of the motion has to this day left me wondering whether the 69 members really voted just to protect the council's endorsement, which was done months earlier behind closed doors, just to stop the scientific evaluation of what has become a contentious document, or both.

Murray E. Schneider, BSc, DC, DABCO
Fort Saskatchewan, Alberta