

Letters to the Editor

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

To the Editor:

It was just pointed out to me by a friend that Dr. Gleberzon had vastly underestimated CBP Research in his article in your June 2001 issue. Dr. Gleberzon list, CBP as having 9 publications in his Table 1. He must not have done a thorough Med-line. Of course I realize that CBP papers published after he submitted his article would not have been found.

However, for an accurate evaluation of CBP research, please publish the following Table as a correction to Dr. Gleberzon’s misrepresentation of our research. I have also enclosed the list of these publications to support my Table 1 (readers may receive a copy of this list by contacting the Editor at algotlib@ccachiro.org).

Donald D. Harrison, PhD, DC, MSE
Evanston, WY

Table 1
CBP Research: Past, Present and Future

Journal	Published	In Press	In Review	In Preparation
JMPT	30	3	1	2
Chiropractic Technique	6			
Spine	4			2
Clinical Biomechanics	3			1
Journal Spinal Disorders	2		1	
European Spine Journal	2			1
Archives Phys Med & Rehab		2		
Journal Orthopedic Research	1			
Totals	48	5	2	6

To the Editor in reply:

Thank you for the opportunity to respond to Dr. Harrison’s comments. As Dr. Harrison knows, when performing a

review of the literature, outcome yields greatly vary, depending upon the search strategies used. Upon examination of the documents listed by Dr. Harrison, it appears that most of these studies fall outside of the inclusion criteria of my study published in the June 2001 issue of the JCCA, as they predominately investigate x-ray mensuration and mathematical modelling of the spine only. Thus, there was no intent on my part to misrepresent the body of knowledge on CBP research. Although these studies may not be specifically germane to my study, they are perhaps of interest to your readers, and I wish to thank Dr. Harrison for providing this detailed list and further applaud his efforts to enhance the scientific base of chiropractic.

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

**Virchow’s Triad and spinal manipulative therapy
of the cervical spine.
JCCA 2001; 45(4):225–231.**

To the Editor:

A very interesting paper, providing some perspective balance to an issue that is currently somewhat controversial. Inclusion of the variables that you have raised provides a more comprehensive consideration to the issues of CVA and SMT relationships. A timely and pertinent paper. Congratulations for a job well done.

Reg Nicholson, DC
Midland, Ontario

To the Editor in reply:

Thank you for your feedback. We tried to take a step backwards in this review to get a better view of the big picture – hence the inclusion of Mike Westaway, a manipulating physiotherapist, as my coauthor. I think VBI should not be an exclusively chiropractic issue, but rather a general health care concern for all practitioners as part of their patient advocacy, and we have tried to present this

manuscript as such. Instead of being on the defensive, I think we, as chiropractors, should open this discussion up to as many health care parties as possible and resolve it to everyone's satisfaction (even Californian neurologists ...). Once again, thanks for the encouragement.

Bruce P. Symons, DC, MSc
University of Calgary

To the Editor:

Given the current controversy and interest in sequelae of spinal manipulative therapy (SMT) of the cervical spine, this is certainly a timely article. Would that it contributed towards resolving, rather than augmenting, confusion and controversy.

On page 228 the authors advise caution in manipulating the cervical spine if "several" of the factors listed in their Table 1 as increasing the risk of hypercoagulability and/or stroke are present in a patient. On page 230 they advise "using minimum force required ...". This is reminiscent of plans of management of patients in acute pain presented for a clinician's approval in CMCC's clinic, wherein the intern proposes "gentle" SMT of the painful region of the spine. Surely, every procedure performed on any patient should be considered and applied cautiously and gently. Surely, the authors don't wish to imply that cervical SMT may be applied with reckless abandon if these factors are not present or if non-rotatory SMT is used.

There is no reference given for the list of risk factors for hypercoagulability and/or vertebrobasilar stroke. Of the 33 factors listed, only a handful have been reported among cases of vertebrobasilar stroke following SMT.¹ At this time, the available evidence suggests that it is important to distinguish which etiology of stroke (e.g., cervical SMT vs. "spontaneous") these factors may pertain to.

Other unfounded assumptions appear to have eluded the peer-review process, finding hospitality in the Conclusion. The referenced article of Kimura et al.² discussed the duration of symptoms of carotid system transient ischemic attacks (TIA), not how long etiological pathologies were present before causing symptoms. Contrary to Symons and Westaway, I did not interpret the data² as indicating that "most ... (TIAs) last between 2–15 minutes." Even if

this were the case, and even if this applied to the vertebrobasilar system, it would not support the inference that this time represents the duration of effect of the causative pathology (e.g., embolic infarction). Thus, I cannot comprehend the logic in their argument that "the complete occlusion of the [vertebral artery] for even a full minute will not result in any appreciable brain damage." Is this reasoning not tantamount to stating that symptoms of whiplash injury endure for as long as the causative impact did?

Next, I question "the utility of checking the blood pressure and auscultating for bruits," as advised on page 230. Again, hypertension and atherosclerosis have not been significantly correlated to vertebrobasilar stroke following SMT.¹ Auscultation for bruits of the more accessible (as compared to vertebral arteries) carotid arteries has been shown to lack validity and reliability.³ There is no need to propagate further specious^{3–5} standards (vs. guidelines) of practice upon clinicians who perform cervical SMT.⁶ In this regard, it is, at best, naïve to publish the authors' unsupported, arbitrary speculation on page 230 concerning a correlation between risk of vertebrobasilar injury and frequency of cervical SMT.

By all accounts, the incidence of vertebrobasilar sequelae following cervical SMT is relatively low. This makes it difficult to gather factual data about this phenomenon. It is tempting to try to fill in the gaps with the kind of invalid reasoning (e.g., applying biological principles generated from better known models) for which proponents of, for example, the Vertebral Subluxation Complex have been criticized. I commend the authors on undertaking to shed some light on this important sequela; however, this can only be generated by the lamp of knowledge. Their current article may only get in the way of unbiased scientific investigation, thus casting more shadow on this issue.

Igor Steiman, MSc, DC, FCCS(C)
CMCC, Toronto, Ontario Canada

References

- 1 Haldeman S, Kohlbeck FJ, McGregor M. Risk factors and precipitating neck movements causing vertebral artery dissection after cervical trauma and spinal manipulation. *Spine* 1999; 24:785–794.
- 2 Kimura K, Minematsu K, Yasaka M, Wada K, Yamaguchi T. The duration of symptoms in transient ischemic attack. *Neurology* 1999; 52:976–980.

- 3 Terrett AGJ. Current concepts in vertebrobasilar complications following spinal manipulation. 2nd ed. West Des Moines, Iowa: NCMIC Group, Inc.; 2001:56–58.
- 4 Thiel H, Wallace K, Donat J, Yong-Hing K. Effect of various head and neck positions on vertebral artery blood flow. Clin Biomech 1994; 9:105–110.
- 5 Cote P, Kreitz BG, Cassidy JD, Thiel H. The validity of the extension-rotation test as a clinical screening procedure before neck manipulation: a secondary analysis. J Manip Physiol Therap 1996; 19:159–164.
- 6 Steiman I. Letter to the editor. J Can Chiropr Assoc 1995; 39:188.

To the Editor in Reply:

Thank you for the opportunity to respond to Dr. Steiman's comments. His arguments are very logical and insightful.

However, one of the objectives of this review in presenting a "revised" Virchow's Triad (Figure 2, page 230) was to steer away from a linearly logical approach, and to adopt a more interactive approach that emphasizes relationships rather than facts. In other words, our objective was not to construct a logical flow-chart for clinical decision-making; instead, our goal was to insert and reinforce certain cues into a clinician's mind that may alert him or her to a potentially dangerous situation. I would also like to address Dr. Steiman's specific comments in point-fashion.

Table 1 (page 229) presents a list of factors that are related to hypercoagulability as well as VBI, most of which affect blood viscosity rather than VBI. These factors were compiled from several standard medical textbooks such as Harrison's (our Reference #5), and can be

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easily replicated by anyone spending an hour perusing these texts. Our recommendation of assessing several risk factors again emphasizes an interactive approach – clearly any single risk factor (such as being female) is not an absolute contraindication, but the accumulation of several risk factors ought to arouse clinical suspicion.

Dr. Steiman is correct in pointing out that we are being overly simplistic and/or condescending in recommending clinicians to adjust gently, and we apologize to the readership for this underestimation. However, as we re-read the sentence in question on page 230, we find nothing that implies or advocates forceful SMT in patients without any risk factors. It is clear that Dr. Steiman's corollary derived from our suggestion to adjust patients at risk "gently" into applying SMT "with reckless abandon" in all other patients represents a debate in semantics that adds little to the overall discussion.

We do not fully understand Dr. Steiman's assertions with respect to our discussion on TIAs and vertebrobasilar obstruction, or his whiplash analogy. Therefore, we will try to reiterate our position more clearly. Our theory (and in the absence of convincing data it remains a theory) was that VBI is caused more often by thromboembolic events rather than by a mechanical obstruction in blood flow due to head positioning. The TIA duration (Kimura et al., our Reference #2) was cited to support our rather simple argument that a minute of interrupted blood flow through one or both vertebral arteries should not adversely affect global blood perfusion to the brain.

With respect to Dr. Steiman's fourth point, our sugges-

tion to check blood pressure was again meant to emphasize the interactivity and relationships between the risk factors, rather than to recommend a stand-alone test. It was this precise point that prompted us to redesign Virchow's Triad from a linear "cause-effect" model (Figure 1, page 226) into the zones of influence illustrated in Figure 3 (page 230). Furthermore, Dr. Steiman's arguments suggest that he is against using basic diagnostic procedures in clinical practice, an issue that lies beyond the scope of our original review.

Finally, our speculation that the frequency of cervical SMT may be correlated to VBI is based on ongoing investigations in our laboratory. Very briefly, fresh cadaveric vertebral arteries are clamped into a materials testing machine, which then stretches them longitudinally at 5–25% strain (Symons and Herzog, JMPT 2002, in press) at a set frequency and acceleration for a number of repetitions ranging from one to several thousand. The vessels are then scored for histopathologic damage against controls. We are still in the data collection process, and are unable to comment on the results at present.

Thank you for this opportunity to respond to Dr. Steiman's comments. It is our hope that continued critical and scholarly discussion on these issues will ultimately lead to greater understanding.

Bruce P. Symons, MSc, DC
Michael Westaway, BSc, PT, FCAMT
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Canadian Chiropractic Research Foundation

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