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Revisiting the Cervical Spinal Manipulation/Stroke Debate

Chiropractic Researchers Dispute Results of Neurology Study

By Editorial Staff

Few issues in the chiropractic profession are more contentious than the potential risk of stroke and/or dissection of the vertebral artery associated with manipulation of the cervical spine. In the latest round of the stroke/manipulation debate, a study published in the May 13, 2003, issue of *Neurology* reported a much greater risk of stroke arising from spinal manipulation than previously suggested in the literature. However, the study appears deluged with errors and important omissions, a point emphasized in the headline of an American Chiropractic Association (ACA) release that labeled the study "fraught with design flaws."

Two prominent members of the chiropractic research community, Drs. Michael Haneline and Anthony Rosner, have issued rebuttals to the conclusions reached in the *Neurology* article. Although space limitations prohibit us from printing these rebuttals in their entirety, substantial excerpts appear below.

Is the Ban of Cervical Spine Manipulation for Acute Neck Pain Patients Really a Solution?

An editorial in the May 13, 2003, issue of the journal *Neurology* included a statement that should be of concern to all doctors of chiropractic.[1] Based on another article in the same issue of Neurology by Smith, et al.,[2] the editorial stated: "... in the absence of randomized controlled trial evidence demonstrating the efficacy of cervical manipulation, the best current evidence suggests that the small risk of dissection and stroke outweighs the benefit of this treatment modality for patients with acute neck pain." In other words, until cervical manipulation has been shown to be effective for the treatment of acute neck pain via randomized, controlled trials, it should be abandoned as a method of caring for these patients because of the small associated risk of stroke. This imprudent statement is, unfortunately, representative of what many neurologists consider factual. This article will attempt to disclose some of the inadequacies of the Smith, et

al., study, and the unreasonableness of the aforementioned conclusion drawn in the journal's editorial.

The Smith, et al., article reported on a case-control study involving patients seen at stroke centers at Stanford Medical Center and the University of California, San Francisco, from 1995 through 2000. All patients who had been evaluated for ischemic stroke or TIA (transient ischemic attack) at these institutions were included, although persons greater than 60 years of age at the time of the event were excluded. A total of 151 dissection cases and 306 controls with other causes of stroke were randomly selected from a total cohort of 1,107 patients. (The authors used the term "cohort" to describe these patients, although technically, a cohort describes a group of people with a common trait who participate in a longitudinal study.)

Patients were administered a survey by telephone or in person (n = 188) by two investigators who were not aware of the patients' diagnoses. Additionally, two neurologists screened the files of these patients regarding the presence of stroke and whether there was dissection. As a result, 72 patients with dissection and 116 controls were identified, but this number was reduced further to 51 dissection and 100 control patients. A total of 37 patients were excluded for various reasons, eight of which were iatrogenic dissections. These eight were perplexingly excluded from the study without explanation, even though they represented more cases than those related to spinal manipulative therapy (SMT).

The major conclusion of the study was that SMT is independently associated with vertebral arterial dissection (VAD). The authors recommended that patients undergoing SMT should be warned about risk of stroke or vascular injury and that a significant increase in neck pain following SMT calls for immediate medical evaluation ... Nevertheless, there were several methodological flaws present in the study that limit its utility, which will be discussed in this brief critique.

The original manuscript of this article was received by *Neurology* in October 2002, which means the study was probably carried out in early 2002. As a result, some patients were required to remember historical details that occurred up to seven years previously. In fact, the authors mentioned that information about location, duration, and quality of neck and head pain was incomplete due to imperfect patient recall. This and other issues may have introduced recall bias, which the authors actually mentioned as a limitation ... Some of this bias may be explained by inaccurate recollection, but due to what is known as prevarication bias, some patients may have slanted how they reported their exposures if they believed that the exposure was causative. Moreover, prevarication bias may also have been a factor in the four cases who indicated that their neck pain was aggravated following SMT. The authors used this point to support their position that the

causal relationship between SMT and VAD was likely. Nonrespondent bias was another potential problem, where persons who did not respond may have somehow been different than those who did. ...

The article did not specify the type of provider involved in the delivery of SMT; that information would have been extremely helpful. Based on previous similar reports, DCs were often not implicated in these cases; rather, an osteopathic or medical doctor, physical therapist, or even a lay practitioner provided the SMT.[3] Nonchiropractic providers have been responsible for a disproportionate share of reported cases of cervical arterial dissections (CADs) following manipulation; therefore, it would have been important for the authors to obtain this information.

The article included a meager number of references, which leads one to wonder what the extent of their literature review was. Furthermore, they were mistaken regarding a reference to the incidence of spontaneous CADs. The authors incorrectly cited Schievink, et al.,[4] who carried out a community-based study of the incidence of internal carotid artery dissection (ICAD) in the community of Rochester, Minn. Schievink, et al., determined the incidence of ICAD was 2.6/100,000, which was also estimated by Giroud, et al.,[5] at 2.9/100,000. The incidence of vertebral artery dissection (VAD) was not included in the Schievink, et al., study, but Smith, et al., combined the two in error. While on this subject, the incidence of VAD is not as readily available as ICAD, although Haldeman,[6] using data from another study,[7] estimated its incidence among stroke patients as 1.3/1,000. One can estimate the number of CADs that occur per year in this country (U.S. population of 290,000,000) using a conservative incidence of CAD at 2.9/100,000, which yields more than 8,400. There are almost certainly more, however, considering a percentage of CADs are asymptomatic or never diagnosed.[8]

As mentioned previously, this was a case-control study, and case-control studies are useful in relation to the identification of risk factors and guidance for topics of future research. On the other hand, they are incapable of establishing causality. While the authors apparently recognized this limitation, they attempted to link the relationship between SMT and VAD observed in the study with several patient characteristics, then implied that the association was likely causal. It appears they overstepped their bounds in making this comment, however; it should have been made in a separate editorial, if at all.

Most DCs would likely agree with the authors that if SMT is performed and a patient experiences neurological signs or symptoms, he or she should be referred for neurological evaluation. Furthermore, patients should be screened for the presence of dissection-related symptoms, such as fainting spells or TIAs,

prior to the application of SMT. Nonetheless, they also indicated that if the patient experienced a marked increase in, or new, neck pain after SMT, he or she should be referred promptly, which may or may not be appropriate.

All CADs found to have an association with SMT involved the vertebral artery, and the only case of ICAD did not generate statistically significant findings. This is in agreement with a recent review that found the relationship between SMT and ICAD doubtful. In fact, if one counts all reports of ICAD alleged to be associated with cervical SMT in the United States up to the year 2000, the likelihood of developing ICAD following SMT is only 1:601,145,000.[9]

The data from this study, although representative of two major medical centers over a five-year period, are meager from a statistical standpoint. Only 7 out of 51 cases received SMT within one month of dissection. From these data, an odds ratio (OR) was calculated for the relationship between SMT and VAD, along with a corresponding 95% confidence interval (CI). The result was an OR of 6.62, and a 95% CI of 1.4 to 30. The utility and interpretation of a 95% CI relates to the probability that the constructed interval includes the true population parameter under study. The wide CI calculated in this study is a reflection of the extremely small group involved ... In this case, we don't have much confidence that this OR is accurate because of the extremely wide CI.

Looking further into the OR issue, rather than controlling for pain, if one simply considers the 51 cases, of whom seven had SMT in the preceding month, and compares them with 100 controls, three of whom received SMT, the OR becomes 5.14, with a 95% CI of 1.1 to 26.5. Additionally, if one adds the iatrogenic cases of dissection back into the equation, the result becomes OR 4.35, with a 95% CI of 1.0 to 22.3. These statistics may even look different if bias could be eliminated, but case-control studies have certain limitations, which is why it is inappropriate to specify causation in these types of studies.

SMT within 30 days was associated with head or neck pain prior to stroke in nine of 10 cases (including nondissection controls), which may indicate that some of these patients were in the process of dissection at the time of manipulation. Whether these dissections were actually related to SMT would be conjectural; on the other hand, four of these cases noticed a remarkable increase in pain following SMT. With the exception of any possible cases of recall or prevarication bias, there may be an association present in these instances. However, in light of the findings of Licht, et al.,[10] who found no alteration of flow velocity in the VA before and after SMT, and Symons, et al.,[11] who found that SMT resulted in VA strain values similar to or

lower than those recorded during diagnostic and range-of-motion testing, one must wonder what this connection really is.

At this time, it is known that cervical SMT is, in very rare instances, associated with CAD. What we do not know is why. Several theories have been suggested, such as the circumstance of a patient presenting to the DC with neck pain and headache related to a dissection already in progress ... In cases where there is an abrupt increase in pain or neurological symptoms following SMT, the already-present dissection may become aggravated, underscoring the necessity of DCs screening for signs or symptoms of dissection prior to SMT. Research has recently been carried out that may facilitate the identification of potential CAD patients by the detection of hyperhomocysteinemia, the presence of which has been shown to be associated with an increased probability of developing CAD.[12]

The chiropractic profession has been accused of being in denial regarding this issue,[13] although the quantity of pertinent literature published in chiropractic journals speaks otherwise. The Smith, et al., article, and those of the same ilk, do not contribute to the search for a solution to the SMT-related CAD problem. Indeed, such studies unduly frighten patients who may be in need of cervical SMT, and prevent certain members of the medical profession from referring patients who may benefit from chiropractic care. The chiropractic profession has long realized that an apparent relationship exists in this area, and is actively pursuing better ways of minimizing risk. At this time, the best advice for DCs is to be ever vigilant for the appearance of rare cases that may exhibit evidence of CAD, either pre- or post-SMT, and make appropriate referrals when needed.

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Response to Vertebral Artery Dissection Study: Synopsis Paper by Smith, et al., Published in the May 13, 2003, Issue of *Neurology*

The recent paper by Smith, et al., in *Neurology* addressing vertebral artery dissection[1] represents another episode of regrettable studies that, despite serious flaws which raise substantial questions as to their internal validity, go to great lengths to selectively disparage the advisability of performing cervical manipulations as a means of patient care while obscuring the larger picture.[2-6] By this, I refer both to the failure to fully present the well-documented *benefits* of this procedure as well as the equally well-chronicled risks of *alternatives* to cervical manipulation - including the use of medications, which is so deeply entrenched in our society as to be obviously far more prevalent than any applications of manipulation. The fact this study has been so extensively and immediately propagated in the printed and televised media (in contrast to the

many investigations which have supported cervical manipulations with no reports of substantial side-effects[7-30]) represents a major disservice to the American public and threatens their access to the best available options in health care.

Specifically, the Smith, et al., study uses a nested case-control design to attempt to demonstrate an elevated risk of vertebral artery dissection following spinal manipulative therapy. Unfortunately, the study fails to identify the actual numbers and locations of manipulations administered; fails to identify the qualifications and backgrounds of the individuals providing manipulations; and actually **excludes** more patients due to iatrogenic causes (8) than are actually presumed to bear a relationship to manipulation (7) because their events occurred within 30 days of treatment. The diminutive number of seven patients thus presented raises questions about the robustness of this study, in addition to the implausibly long period of time between dissection and treatment (30 days).

Regarding the studies mentioned above which appear to discredit the wisdom of cervical manipulation,[1-6] there appear to be a number of common fallacies: 1. They fail to disclose that the majority of cerebrovascular accidents (CVA) are spontaneous, cumulative, or caused by factors other than spinal manipulation; 2. They fail to disclose the potential benefits of the procedure, violating medicine's own ethic of accurately reporting true **risk-benefit ratios**; 3. They fail to place the risks of manipulation in the context of those produced by other medical treatments or lifestyle activities; 4. They fail to indicate the actual frequency of manipulations administered; 5. They fail to account for the possibility that patients undergoing CVAs are reported more than once; 6. They fail to report the rates of CVAs following manipulation by parties **other** than licensed chiropractors; and 7. They incorrectly assume that patients undergoing adverse events following a manipulation might **not** have reported such instances to either the attending chiropractor or an appropriate authority.

Numerous signs point to intrinsic aberrations of arterial structure underlying CVAs, many brought on by elevated levels of homocysteine ... The determination of homocysteine levels as a clinical tool would appear to afford the chiropractic physician a means to bring the actual risks of CVAs to even lower levels than those previously reported. In this regard, homocysteine determinations appear to be the most plausible current means of assessing patients most at risk for experiencing CVAs from routine activities, let alone from cervical manipulations. (With regard to the topics of spontaneous vertebral artery dissections and the possible role of homocysteine as a proposed indicator of patients at risk, I have published more detailed presentations elsewhere.[32,33])

The actual risk of CVA that can be directly attributed to spinal manipulation may be reduced to far less conspicuous levels when compared to everyday lifestyle risks and those brought on by medical treatments widely accepted by the public. Certainly the propagation of risk estimates attributable to visits to the chiropractor's office without adequate justification from data does not perform a useful service to the public; indeed, it does just the opposite. CVAs have been listed as only the **fifth** most common cause of chiropractic malpractice lawsuits - an unlikely ranking if chiropractors were conclusively found at fault for the majority of CVAs reported.[31]

Author's note: A highly methodical, fully annotated response to the Smith, et al., study published in *Neurology* can be found on the FCER Web site (**www.fcer.org**).

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