To The Editor: We read with interest the article by Albuquerque and colleagues (Albuquerque FC, Hu YC, Dashti SR, et al: Craniovertebral arterial dissections as sequelae of chiropractic manipulation: patterns of injury and management. Clinical article. J Neurosurg 115:1197–1205, December 2011), in which they describe 13 cases of cervical artery dissections reported to have occurred following a chiropractic manipulation. Throughout their paper, the authors assume that a causal link exists between chiropractic manipulation and cervical artery stroke. However, this assumption is flawed. To support their assumption of causality, the authors cite case series and 2 case-control studies. Case series cannot be used to make causal inferences because they are fraught with selection and information bias. In addition, case series do not include a control group, which is necessary to quantify the association between an exposure and an outcome.1

Surprisingly, Albuquerque et al. did not review the most recent epidemiological evidence on this controversial issue.1–3 Rather, they support their position by using 2 case-control studies. The first study has known methodological flaws,4,6 and the association described in the second one was later shown to be noncausal.2,5 The most recent epidemiological evidence on the relationship between chiropractic care and vertebrobasilar artery (VBA) stroke suggests that the association is noncausal.2 The case-crossover study by Cassidy et al. found that the risk of VBA stroke is similar for patients who consult a family physician or a chiropractor prior to their stroke. This evidence suggests that patients seek health care (chiropractic or medical care) for early symptoms of an evolving stroke (neck pain and headache), and that chiropractic care may be associated with, but is not a risk factor for the stroke.

Simply observing that an exposure precedes an outcome does not prove causation. Although temporality is necessary to establish causality, it is not sufficient. Well-designed epidemiological studies that include a valid control group are necessary to understand complex causal problems. Although case series are helpful to develop etiological hypotheses, they cannot be used to study causality. Case series serve no purpose once an exposure–outcome relationship has been investigated in sound epidemiological studies. We question the motivation to keep publishing hypothesis-generating case series that do not advance knowledge in this area.

Drs. Côté and Cassidy received research funding from the Canadian Chiropractic Protective Association.

References

Please include this information when citing this paper: published online March 30, 2012; DOI: 10.3171/2011.10.JNS111811a.

To The Editor: I read with interest the recent article by Albuquerque and coauthors (Albuquerque FC, Hu YC, Dashti SR, et al: Craniovertebral arterial dissections as sequelae of chiropractic manipulation: patterns of injury and management. Clinical article. J Neurosurg 115:1197–1205, December 2011). Their article contains very strong language implicating manipulation as a cause of stroke. However, this paper is essentially an observational case series reporting the clinical vignettes of 13 patients who the authors claim developed craniovertebral arterial dissections as the sequelae of chiropractic manipulation. It is a fundamental principle of epidemiology that observational studies can only determine whether an association exists between an exposure and a disease.

Observational case reports simply cannot be used as evidence to prove causal relationships.5 Nonetheless, the authors begin their article with a definitive statement that chiropractic manipulation of the cervical spine can cause stroke. The 5 references cited to substantiate this statement are all case reports, 1 of which used only survey data from physicians in a specific region in France.2 None of these references provide any convincing evidence that manipulation directly causes stroke.

The clinical information from the 13 cases presented
in this article was obtained retrospectively from medical records and history as reported by the patient or a family member, with the a priori assumption that there was an association between dissection and chiropractic manipulation. This type of anecdotal information is especially prone to recall bias; the presumed accuracy is based solely on the recollection of the patient or family member. Another assumption by the authors is that all 13 patients had high-velocity, low-amplitude manipulation performed on their cervical spines, but this assumption is unsupported by evidence. Leach published a retrospective case series of 4 chiropractic patients who presented to his office with stroke symptoms—who did not receive manipulation, but were referred for medical evaluation—and who subsequently had strokes. Would Albuquerque and his coauthors have erroneously assumed that these 4 chiropractic patients had received cervical manipulation?

There is a critical epidemiological question that cannot be answered by an observational case series: "Is there an increased risk of stroke associated with visits to a chiropractor?" Cassidy et al. published a large population-based case-control study of all cases of VBA strokes in the province of Ontario over a 10-year period. The paper concluded that there was no increased risk of VBA stroke associated with chiropractic care compared with primary care. It was suggested that the most likely explanation for the perception of increased risk was that some patients with VBA dissections in progress seek chiropractic care before their stroke actually occurs. It is disturbing that Albuquerque et al. would completely ignore any discussion or reference to this landmark study by Cassidy et al., considering that it is arguably the best epidemiological evidence published on this topic to date.

Readers of the Journal of Neurosurgery should be cautioned about the fallacy of making causal inferences from an observational case series. A comprehensive review of the key issues regarding cervical manipulation and stroke can be found in a recent paper by Murphy, which contains a more current list of references on this topic.

MICHAEL J. SCHNEIDER, D.C., Ph.D.
University of Pittsburgh
Pittsburgh, Pennsylvania

Disclosure
The author reports no conflict of interest.

References

RESPONSE: Although we appreciate that the authors of these letters read our manuscript, we disagree with their assertions. Côté and Cassidy argue that we have mistakenly labeled chiropractic cervical manipulation as the cause of the neurological sequelae suffered by our patients. They cite their own manuscripts as the “most recent epidemiological evidence” showing the lack of causality between chiropractic treatment and stroke. In their first study, they analyzed 2 trends: 1) the incidence of VBA stroke in 2 Canadian provinces over a 10-year period; and 2) the utilization rate of chiropractic care during that same period. Because the rate of VBA strokes and chiropractic care did not increase symmetrically in these populations, they concluded that “the increase in the incidence rate of VBA stroke could not be explained by a proportional increase in exposure to chiropractors.” Although this statement may be true, it has nothing to do with whether chiropractic manipulation of the spine can indeed cause arterial dissections. In fact, the majority of our patients presented within hours of chiropractic manipulation, and numerous similar cases have been reported.

The authors argue that this phenomenon is actually the result of chiropractors having the unfortunate luck to encounter patients who are suffering a VBA stroke “in evolution.” Their assertion that headache and neck pain are signs of a preexisting arterial dissection is both inaccurate and worrisome. If patients manifest the symptoms of a dissection, why would a chiropractor proceed with cervical manipulation? Would they truly think that cervical manipulation cannot worsen or produce a stroke? That the majority of patients present either immediately or within hours of chiropractic manipulation with neurological sequelae or stroke argues against such a notion. Furthermore, such denial is deeply problematic if not dangerous for patients. If chiropractic manipulation cannot worsen or produce a dissection, it is an amazing coincidence that many chiropractors have been at “ground zero” during the onset of a stroke.

Several times in our manuscript we stated that the incidence of manipulation-induced dissection was low and almost impossible to ascertain. Nonetheless, it does occur, and the secondary neurological injuries can be severe and occasionally fatal. Hence, the low incidence of these dissections would be small consolation to affected patients.

The authors of both letters also referred to another publication, again written by Cassidy and Côté, who found that patients with VBA stroke were just as likely to have seen a primary care physician as they were to have seen a chiropractor in the days preceding their stroke. Again, this statement may be true. However, primary care physicians rarely perform cervical manipulations. Patients who eventually die of VBA strokes, whether related