Letters To The Editor

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Neck Manipulation as a Cause of Stroke

To the Editor:

I read with interest your editorial on "Neck manipulation as a cause of stroke" in your Jan/Feb Volume 12, Number 1, 1981 issue. Using the concepts devised and developed by Cyriax, we examine very carefully any individual whose neck is about to be manipulated after a thorough history to rule out such differential problems as blood dyscrasias, anticoagulant therapy and signs of neurological deficit involving the upper extremity or cord signs.1 People who have neurological deficit involving the upper extremity; that is, radicular pain plus neurological deficits of a motor, sensory, or reflex nature, are not suitable for manipulation.

A careful history is taken looking for "drop attacks" indicative of instability of the atlas on the axis as is any other history suggesting vertebral artery insufficiency. These patients are not manipulated by the Cyriax routine. If none of these symptoms are reported, traction manipulations are carried out in the neutral position. We never do flexion manipulations and we always manipulate the neck with traction, which exerts a centripetal force on the discal material causing it to move centrally.

Yours sincerely,

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References


Reply to Dr. Fraser:

We read with interest Dr. Fraser's letter regarding neck manipulation as a cause of stroke. 1,2 We were also pleased to learn that in the reference cited by Dr. Fraser (Textbook of Orthopaedic Medicine) Dr. Cyriax carefully documents the occurrence of stroke as a complication of cervical manipulation in the section entitled "Dangers of Cervical Manipulation."

Dr. Fraser implies in his letter that the neurologic complications of cervical manipulation can be avoided by properly selecting patients to exclude those at risk. We would submit, however, that it is difficult, if not impossible, to predict patients likely to develop a stroke as a consequence of neck manipulation or spontaneous head turning. This view is based on the observation that the vast majority of patients with this complication have no historical, physical, laboratory or roentgenographic abnormality of the cervical vertebra or vertebral artery insufficiency prior to their manipulation-induced stroke. We believe this observation is noteworthy because it does run counter to the belief of many engaged in the use of neck manipulation.

It appears that the vertebral artery is injured at the level of C1-2 when the neck is manipulated. This injury serves as the nidus for subsequent thrombosis or spasm that culminates in progressive infarction of the brainstem. We do not know how commonly this complication occurs. It is important to recognize that this complication can occur, that there is often a delayed, stuttering or progressive course of infarction, and that anticoagulation may be beneficial in these patients.

Respectfully yours,

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Author's Rebuttal:

Dr. Fraser's correspondence is representative of several physician, osteopath and chiropractic individual and societal letters received by the author. Most of the correspondence reflects the clear awareness of the occurrence of arterial injury after neck manipulation and many have been interested in preventive measures. The emphasis has been on the rarity of the complication. Legal correspondence regarding ongoing judicial problems has also been received.

The German Society of Manipulative Medicine has been openly interested in vertebral artery injury. They have published guidelines to attempt prevention and appointed a committee to serve as a registry of case occurrence. Forceful thrusting of the neck is considered a major cause of this arterial injury.

What is clear is that the Cyriax method employs history, physical and neurological examination, appropriate x-rays and certain pre-manipulative and manipulative practices to avoid injury. This method also attempts to exclude patients from manipulation who have suspected vascular disease of the extracranial arteries. However, other than historical factors and certain unproven simple physical tests, there has been no emphasis on the presence of extracranial bruises and, since many arterial lesions may be detectable, it is admittedly difficult to exclude this entity.

Alarming is the fact that many of the arterial injuries occur in the relatively young. The possibility of arterial injury with physician applied manipulation, therefore, remains.

In a recent survey of the Stroke Council of the American Heart Association, 360 heretofore unreported cases of extracranial arterial injury were collected. Two-thirds were vertebral artery injury, one-third carotid artery injury, and most were confirmed by arteriography. If signs of cerebral ischemia appear, two-thirds of patients have resulting cerebral infarction and on occasion death occurs. These cases are in addition to the reports in medical publications.

The data would tend to emphasize that injury is not rare. Improper or too frequent manipulative therapy may enhance the occurrence of extracranial arterial injury. Certain manipulative therapists are interested in preventing the injury and this activity is applauded. However, widespread patient and physician education is needed to aid prevention, but, more importantly, recognizes the oc-
currence of the injury and attempts to outline a clear treatment plan which will minimize the cerebral infarction and mortality rate.

James T. Robertson, M.D.

Performance Factors of CT Scanners

To the Editor:

We read with interest the recent article by Meyer et al.1 on the estimation of local cerebral blood flow (LCBF) from sequential transmission computed tomography (CT) during inhalation of stable xenon gas. There are a number of points we wish to raise concerning the methodology of the assertions made in the paper.

We are somewhat puzzled by a number of statements concerning the performance factors of CT scanners. Many of these points could be easily resolved if data were quoted relative to an accepted protocol such as the one recommended by the American Association of Physicists in Medicine (AAPM).2

The radiation dose at the center of the scan is stated to be approximately one rad. Although this value is in reasonable agreement with one set of published data,3 it differs significantly from others and is to some degree misleading since the entrance dosages are much higher (~4 rads) than the midline dose.4 If the proposed procedure requires approximately 10 scans at each level (3 baseline and 5 to 8 enhanced), the radiation dose to a large portion of the slice would be in the range of 30–40 rads. The discussion of the relative signal to noise ratio (SNR) of the EMI 1010 relative to third and fourth generation CT scanners is at best unclear. We are unable to determine whether the same physical conditions applied when the EMI 1010 was compared to the GE8800. The SNR for the EMI 1010 was quoted as being 97.1 for the iodine solution (H = 39.8 yielding a standard deviation (SD) of approximately 0.4) and a SNR of 37.4 for the vivo measurements (with a SD ~ 1). The SNR for the GE8800 scanner was 9.4 where H was 4.2 yielding a SD of ~ 0.4. The standard deviation would not be expected to vary significantly over a limited range of the Hounsfield Scale, therefore, the ratio should be calculated for similar degree of enhancement. In addition, the comparison should be made when the x-ray technique is adjusted to yield comparable radiation dosages. Since the relative spatial resolution of the scanners is of importance in regards to tissue specificity some mention of this performance factor would also be appropriate.

We also have some questions concerning the quoted errors on flow determinations. Since the flow is determined from the CT enhancement and the errors of both the pre-enhanced and enhanced images are both approximately 1 Hounsfield unit (HU), the maximum enhancement of 6 HU should result in at least 15% uncertainty which should propagate through the entire calculation. Despite the fact that flow determinations may be very reproducible, the error on the absolute value of the flow should be somewhat greater than the values given in the paper.

The procedure described for the determination of local partition coefficients (Lx) by visual curve fitting to 15 minutes and infinity is unclear. Since a large number of scans were performed with flow and Lx being the only unknowns, Lx should be calculable. We cannot determine whether flow calculations were determined using tissue specific Lx for each local or from average values for white and gray matter. The visual extrapolation of the number of Lx in Figures 9 and 10 would be quite cumbersome and time consuming. We do not understand why the number of significant figures listed in Tables I and II differ. The precision of these determinations as determined from a visual interpolation is quite surprising and we are unable to determine whether the errors represent the statistical spread from all measurements or the estimated errors from each point. Figure 10 shows a number of extremely low values for Lx of well below the value for water of approximately 0.6. Values this low must be associated with extremely low enhancement (~2 H) and we do not see how reliable determinations of partition coefficients such as 0.32 could be derived.

It would be most helpful if the authors had commented in somewhat greater detail concerning the relative merits of the autoradiographic method and the monoexponential method using many scans. Virtually all the LCBF values quoted are from the 2.4 min scan and we see no utilization of the late washing or clearance data except for determining partition coefficients. Since the discussion on page 435 indicates that in tissue with slow flow LCBF values are relatively insensitive to the precise value of the partition coefficient, we cannot see how these scans significantly improve the derived information. Reducing the number of scans would decrease the radiation dose to the patient. A more detailed discussion of the optimum time for scanning when using the autoradiographic methodology would be helpful. The justification for optimal time of scanning (figs. 5A and B) and the selected sequence of scans is incomplete.

While we certainly believe that the xenon CT method for determination of LCBF merits serious investigations for possible routine clinical use, the ease by which the method is implemented and its reported high success rate is somewhat misleading. It would have been helpful if the limitations of the proposed method had been discussed in detail as well.

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References


Reply to Dr. Shabason et al. from the Authors

Methodological Advantages and Disadvantages of Xe CT CBF

The letter and comments of Dr. Shabason and co-workers concerning our article on xenon CT methods for measuring LCBF and Lx have been reviewed and we thank them for their interest and helpful comments. We appear to be in agreement that the potentialities of the Xe CT CBF method merits further investigation to test its clinical applicability. Several important methodological considerations have been raised for further discussion. Nevertheless, we believe that the most important limitations of the method were discussed in this article as well as in the cited references to other publications, including some of our own, which address these methodological considerations in considerable detail.

Experience has now been gained at the VA Medical Center in Houston, with successful measurements made in 99 subjects including 88 patients and 11 normal volunteers aged 22–78 years. In the majority of subjects more than 1 series of LCBF and Lx measurements were made. Most important methodological considerations
Neck manipulation as a cause of stroke.
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