Editorial

Microvascular decompression for trigeminal neuralgia

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The article by Sekula and colleagues on microvascular decompression (MVD) for trigeminal neuralgia (TN) in the elderly appears in this month’s issue of the Journal of Neurosurgery. The authors’ point is that although MVD has been considered ill-advised in elderly patients due to anesthetic risks and comorbidities, MVD is as safe and effective in older patients as it is in the younger patient with TN. Although philosophically I may agree with their conclusions, it is important to examine the strengths and weaknesses of this contribution.

The evidence presented in this paper is in the form of a small retrospective case series, and is therefore anecdotal. We must be cautious to avoid overinterpretation of the authors’ conclusions.

This paper also presents a nice example of the misuse of statistics. What the authors did was to gather data retrospectively from a single source, in this case the outcomes of all MVDs performed over a time period. They then segregated the data based on patient age. The study was not prospective, and so it should be obvious that there is no control group in the statistical sense. T-test statistics are not appropriate for this type of analysis, and although they frequently appear in the medical literature, this is a misapplication of statistical technique. The t-test requires that the dependent variable be (a) continuous, and (b) have values that are normally distributed.

Based on the information provided by the authors, I cannot tell if either of these assumptions has been met. First, the primary outcome is “pain-free status without medication use.” This is a categorical variable, and as such, the t-test is not appropriate. Second, if we give the authors the benefit of the doubt and assume the outcome measure is continuous, we still do not know if the data are normally distributed. With small sample sizes like these, it is always important to state explicitly whether the assumptions of the t-test were met, and if not, describe how the data were transformed or otherwise manipulated to deal with the problem. Otherwise, it is a large leap of faith to believe the inference.

The other use of the t-test in this paper is with length of stay (LOS). To my mind, LOS data when gathered from the chart is actually an ordinal measure (with unit = 1 day), in which case they should have reported a median value and then used ordinal stats to test the median difference. Their application of the t-test in this instance is an invalid use of a statistical test.

Finally, from a statistical standpoint, what is really troubling is the inference the authors make. Basically, what they are saying is that because the probability value is > 0.05, there is no difference between the groups, and therefore, age does not matter in the outcome of MVD. However, all they have really shown is that they have no evidence of a difference. Most reasonably, this is the case because of the small sample size that (given the questionable psychometric properties of the outcome measure) was likely inadequate to detect a real difference if one truly existed. It would have been really interesting to see the confidence limits on the group means. My guess is that the confidence limits were wide, suggesting substantial uncertainty as to the conclusions that can be drawn from the data.

Statistical validity aside, 2 neurological complications occurred in the elderly group, and 1 neurological complication occurred in the younger patients. There was 1 case of facial nerve palsy in the elderly group. Although facial palsies can occur after MVD, in my experience they are very rare. The authors did not state whether or not they monitored brainstem auditory evoked responses (BAERs) or facial electromyography (EMG) intraoperatively. Monitoring of this type has almost eliminated the complications of facial palsy and hearing loss, at least those instances most likely due to prolonged stretching of the facial and auditory nerves, respectively. Although I believe BAERs and facial EMGs should be monitored in every MVD procedure, they are probably even more vital in the elderly, where cerebellar atrophy can allow sagging of the cerebellar hemisphere due to increased cerebrospinal fluid drainage. This produces stretching of the seventh and eighth cranial nerves, even with gentle or no retraction. Brainstem auditory evoked responses and facial EMG monitoring should be used routinely in the elderly patient undergoing MVD.

The one serious complication that occurred in this series was in an elderly patient: a midbrain cerebrovascular accident. Although the authors ascribe this to anesthetic technique, it is part of the surgical complications, nevertheless. A skeptic would conclude that this is just the type of complication that would be more likely, although still rare, in an elderly patient.
As the authors point out, deafferenting procedures such as radiofrequency rhizotomy, glycerol rhizotomy, and radiosurgery, all carry a special risk in the elderly: anesthesia dolorosa. Although this dreaded complication of intentional nerve injury can occur in younger adults, it is particularly likely in the elderly. When these denervating procedures are performed in elderly patients, scrupulous care must be exercised to avoid dense numbness. Despite the best intentions and technique, anesthesia dolorosa or one of its milder variants can still occur when a destructive procedure is performed in a patient older than 75 years of age. In this regard, I accept the authors’ logic that a “nondestructive” procedure such as MVD makes postoperative anesthesia dolorosa much less likely.

Although my expert opinion and the authors’ conclusion may or may not coincide, this paper does not give us much, if any, meaningful data on the risks of MVD in the elderly patient.

Reference


RESPONSE: We appreciate Dr. Burchiel’s thoughtful review of our paper. Regarding a “misuse of statistics,” we would like to clarify a few points. Rather than use the Student t-test to analyze the categorical variables of pain-free status without medication versus pain recurrence, we correctly used the Fisher exact test. Regarding the LOS analysis in this report, Dr. Burchiel is correct. We did inappropriately assume that the LOS data were normally distributed; in fact, it is not, as determined by the D’Agostino–Pearson test. Instead, we could have improved the analysis by using an ordinal test such as the Mann–Whitney U-test. When the data were subjected to the Mann–Whitney U-test, however, the difference in LOS did not reach a level of statistical significance (p = 0.25). Finally, we offered this report as an addition to the existing literature regarding appropriate treatment of TN in elderly patients despite its inherent flaws. Over the past 18 months we have carefully and methodically conducted a prospective study of elderly patients with TN. Undoubtedly, this future report will resolve some of the present study’s inadequacies. (DOI: 10.3171/JNS/2008/108/4/0687)

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