Electrodiagnostic Studies Versus Wrist Magnetic Resonance Imaging in Carpal Tunnel Surgery

To The Editor: I have read with interest the recent article by Jarvik et al. (Jarvik J, Comstock B, Heagerty P, et al: Magnetic resonance imaging compared with electrodiagnostic studies in patients with suspected carpal tunnel syndrome: predicting symptoms, function, and surgical benefit at 1 year. J Neurosurg 108:541–550, March, 2008) in which Jarvik and colleagues compare electrodiagnostic studies (EDS) with wrist MR imaging in patients with suspected carpal tunnel syndrome (CTS).

This recent study was based on the same patient population as one of this groups' previous studies, as they noted. In this article the authors acknowledge some of the study's limitations and note that they are conducting a prospective study on a group of patients from a previously published report. I read these 2 studies and had some questions that I could not answer. I hope those comments will be valuable in the future studies of these authors:

1) There are some technical differences in carpal tunnel release among surgeons who are either neurosurgeons or orthopedic surgeons, even though they are board-certified. As these authors mentioned in the discussion, they did not know how many of their patients underwent endoscopic or open carpal tunnel release. To prevent the bias resulting from surgical decisions and treatment, patients should have been evaluated by 3 surgeons independently, similar to the way it was done for interpretation of MR imaging findings, and only 1 surgeon should have performed the operation using the same technique.

2) Postoperative wrist MR imaging findings would have been valuable for showing the completeness of the incision and the duration of recovery time of the median nerve, and to give readers an opportunity to compare these findings with the postoperative EDS findings, if they had performed both postoperative wrist MR imaging and EDS in the same patients.

3) Patients may prefer wrist MR imaging to EDS if they know that both methods produce the same results. We should be aware that wrist MR imaging provides the physician with much more valuable information than EDS in the differential diagnosis of median nerve compression in CTS, and this information consequently leads to proper management. Long waiting lists and interpretation of EDS are other problems that affect the physician’s decision and cause changes in the cost-effectiveness analysis for a CTS diagnosis, especially in developing countries. In future studies, performing prospective randomized studies and including more patients may demonstrate that MR imaging findings provide much more information than EDS in predicting outcome; most of the patients under consideration for surgical treatment would therefore undergo wrist MR imaging according to the physician’s decision alone.

4) Although the body mass index (BMI) was the only variable surgically associated with outcome at 1 year in these studies, do the authors recommend decreasing the BMI as one of the alternatives to conservative management options of CTS?

In brief, I appreciate the studies of these authors concerning the evaluation of the CTS diagnosis and treatment, and I would expect to see their later studies resolve some of the present study’s limitations in the near future.

TANUS MERTOL, M.D.
King Faisal University
Al-Khobar, Saudi Arabia

References

Response: We thank Dr. Mertol for his interest in our article and his comments. Although it is true that we did not have information regarding the type of surgery performed, it is unlikely that the type of surgery influenced the long-term outcome of our patients. A recent Cochrane review found no strong evidence that there was a substantial difference between open versus endoscopic release. With respect to having multiple surgeons evaluate the patients to avoid bias, the surgeons were not involved in the 1-year assessment of outcomes; instead, we had an independent research coordinator, who was not affiliated with the surgeons, obtain the follow-up information.

We completely agree that having postoperative MR images would have been interesting and potentially informative. Unfortunately, the study budget was not sufficient to pay for these examinations.

The question of the relative effectiveness and cost-effectiveness of MR imaging versus EDS is valid, and we are exploring this question in an ongoing study of CTS. The information from MR imaging is certainly different than that from EDS, and in our opinion, the question is still open as to whether and how MR imaging may prove to be useful in the evaluation of CTS.

With respect to BMI, it may well be true that a weight loss regimen would improve symptoms of CTS, however we are unaware of any controlled trial that addresses this issue. From a practical standpoint, weight loss for patients with an elevated BMI is likely sound medical advice and should certainly be considered when counseling these patients. (DOI: 10.3171/JNS.2009.110.2.0398)

JEFFREY G. JARVICK, M.D., M.P.H.
BRYAN A. COMSTOCK, M.S.
PATRICK J. HEAGERTY, Ph.D.
DAVID R. HAYNOR, M.D., Ph.D.
DEBORAH FULTON-KEHOE, M.P.H., Ph.D.

Hippocampal Seizures

To The Editor: We enjoyed reading the recent article by Tanaka et al. (Tanaka N, Fuji M, Imoto H, et al: Effective suppression of hippocampal seizures in rats by direct hippocampal cooling with a Peltier chip. J Neurosurg 108:791–797, April, 2008). Given the pharmacoresistance of most temporal lobe epilepsies, the surgical removal of the epileptogenic zone is a frequently used, but irreversible, therapeutic option. Furthermore, cognitive deficits can be associated with the removal of functional areas. Alternative therapeutic strategies are clearly needed, but they must be carefully assessed. The focal cooling of brain areas involved in seizure generation may be a promising approach, and critical evaluation as promised by the article of Tanaka and colleagues is surely needed.

Tanaka and colleagues studied the antiepileptic effect of local cooling on seizures induced by focal injection of kainic acid (KA) into the hippocampal formation of adult rats. They have also used models of temporal lobe epilepsy in animals, and we wish to discuss some of the results with the authors to extend their work.

The authors show that their elegant cooling device can directly cool restricted zones of the hippocampal formation of the rat below 20°C under precise experimental control. In the abstract’s Conclusions they stated, “Selective hippocampal cooling effectively suppresses the KA-induced hippocampal EDs [epileptic discharges].” We wish to make 3 observations that Tanaka and colleagues may wish to explore in follow-up studies.

First, their electroencephalography (EEG) recordings seem to have been obtained in the noninjected contralateral hippocampus. In the Methods section of the abstract, they stated, “The cooling needle, a thermocouple, and a needle electrode for EEG recording were inserted into the right hippocampus,” while Fig. 3 seems to indicate that they were inserted into the left hippocampus. If EEG recordings were indeed made from the site contralateral to the KA injection site, we suggest that cooling may have effectively suppressed propagation of KA-induced activity rather than its initiation near the injection site. Given that antiepileptic treatments aim to suppress seizure generation, it would be helpful to check both initiation and propagation in future studies.

A second point concerns the time point at which cooling was tested. The authors suggested that EEG recordings were restricted to the acute phase just after KA injection. In this case, status epilepticus may be more accurate terminology than seizure or ED for the EEG manifestations described. Previous studies have described how hippocampal KA injection induces a status epilepti-

References


Response: We appreciate Dr. Pallud and colleagues’ interest in our report and suggestions for our future stud-