Editorial: Assessing MEG

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As editor, I have taken pride in the fact that the *Journal of Clinical Neurophysiology* is seen as a venue in which new techniques in clinical neurophysiology can receive needed exposure and, at the same time, constructive critiques. One such technology, which is forty years old and, thus, new only in comparison with electroencephalography (EEG), is magnetoencephalography (MEG). Numerous original research articles and two special issues featuring MEG have been published in the *Journal*. Recently, I agreed to edit and then publish a position statement from another society, the American Clinical MEG Society (ACMEGS). Although we have published abstracts from meetings of other neurophysiological societies in the past, both as informational material for our readership and as a courtesy to these societies, this is the first time that we have published a "position statement." Let it be clearly understood from the outset that this action does not connote an "official endorsement" of the statement by the American Clinical Neurophysiology Society. Rather, it is simply a recognition that the views contained in it should have a reasonable public airing. In addition, I believe that the plight of colleagues, who have developed and use MEG clinically, yet cannot obtain reimbursement for their efforts, is a story about which we should all take heed.

Sometimes, in our zeal to be overly objective, we end up creating roadblocks to progress. Criteria applied critically to one aspect of medical practice may not be good in judging the worth of another. A case in point may be the rigidity of the evidence-based system for defining clinical usefulness of therapeutic measures and diagnostic tests. This methodology is undoubtedly appropriate for studies of new drugs or treatments, and its associated protocol criteria, such as prospective, double-blinded, placebo-controlled, broad study population, and normal controls, all make sense and are reasonable to accomplish. Applying similar criteria to the evaluation of diagnostic tests can be problematic. Comparisons against existing "gold standards" lead, in most cases, to a double standard, given that few accepted neurological diagnostic techniques have ever been subjected to evidence-based analysis. Yet, as clinicians, we know, for example, that EEG, electromyography (EMG), computed tomography (CT), magnetic resonance imaging (MRI), positron emission tomography (PET), and single-photon emission computed tomography (SPECT) are all useful diagnostic procedures, although not proven by current methods.

In 1992, the Therapeutics and Technology Assessment (TTA) Subcommittee of the American Academy of Neurology (AAN) reviewed published data on MEG and decided that there was insufficient evidence of its clinical utility. Accordingly, it was deemed investigational. This decision unfortunately has been a basis for refusals by insurance companies to pay for MEG to the present day. This bias has persisted for seventeen years, despite significant advances in MEG technology and analysis, many traditionally constructed clinical neurophysiology studies, and acknowledgement via Medicare with Current Procedural Terminology (CPT) codes. Only recently have rigorous prospective studies been performed specifically to meet the current strict criteria. A revision of the 1992 Therapeutics and Technology Assessment is in progress, but undoubtedly it too will confront the same difficulty in using strict classes 1 and 2 study criteria to judge MEG's worth.

During the past year, I have had the opportunity to investigate MEG first hand. I quickly learned that clinical MEG has both strengths and weaknesses, like all of our diagnostic tools. In the evaluation of epilepsy, it is not a replacement for EEG or imaging studies, but it can provide both additive and enhanced functional information. That which it does best is localization, and in those situations in which this is key, such as presurgical identification of epileptic foci or eloquent cortex, MEG has clear advantages. When MEG spikes are recordable, localization of their cortical source is, indeed, more accurate than with EEG, sometimes by several centimeters. However, a number of patients have EEG spikes that are not seen by MEG, and seizures are infrequently recorded by MEG. Thus, it is not a matter of whether one technique is better than the other. Rather, multiple types of data are needed if one wishes to have the best set of information from which to make a clinical decision.

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I was gratified to read only a few days ago that other clinicians share my concerns about evaluating MEG solely by existing evidence-based criteria. I found most intriguing a new type of statement that is being put forward by the Medical Economics and Management (MEM) Committee of the AAN. These are called "model medical policies," and on May 8th of this year, the Academy Board of Directors approved one such policy regarding MEG (http://www.aan.com/news/?event=read&article_id=7795&page=1016.378.33). This document explains MEG, compares it with other localization techniques, provides a critical evaluation of MEG as a diagnostic technology, and outlines its indications and limitations. It was also interesting that the policy was directed at insurers, in the hope that they would adopt the principles outlined in developing their own policies. It seems that progress is finally being made.

In summary, our publishing the ACMEGS statement is consistent with the recognition that there may be more to clinical medicine than that which can be validated by strict objective criteria. Perhaps, the opinions of experts, who have acquired years of clinical experience, should not necessarily be relegated to a position of least importance in evaluation schema. I encourage you to take the time to read the ACMEGS position statement and the new AAN model medical policy on MEG. Overall, they are remarkably similar. Consistency of thought, when independently derived, usually bodes well for the concepts expressed.