increase the inclination toward development of autoimmune disorders. In healthy menopausal women, there are indications on an increase in airway T-helper lymphocytes and a shift in the relation between T-helper and T-cytotoxic cells. It could be speculated that such changes in T-cell function by menopause may constitute the basis for an altered immune response to a common infection.

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Is SNAP Technology Accurate in the Diagnosis of Obstructive Sleep Apnea?

To the Editor:

Dr. Kehoe’s letter (April 2005) identified serious methodological shortcomings in the study by Liesching et al (March 2004) and shed significant doubt on the conclusions of the study. Dr. Millman’s rebuttal (April 2005) compelled my response. First, Millman denied the existence of published literature on SNAP test reliability. In fact, Su et al in their study conducted at the University of Chicago, concluding the following: “The results ... demonstrate...good correlation between SNAP and polysomnography in quantifying RDI [respiratory disturbance index] with new reliable sensitivity, specificity, positive and negative predictive values...[thus]...SNAP is an excellent tool for the diagnosis of OSAS [obstructive sleep apnea syndrome].” The article demonstrated via receiver operating characteristic curves (Fig 3, 4 in the article by Su et al) that a SNAP test accurately identified apnea severity. Su et al utilized a more powerful design performing both tests simultaneously. In contrast, Liesching et al have been soundly criticized for comparing data from the SNAP test and polysomnography findings that were collected, on average, 5 months apart. Given the stronger design of the study by Su et al, the startling variant conclusions offered by Liesching et al should be substantially discounted.

Second, Millman claimed that the initial continuous positive airway pressure derived from the clinically proven Miljeteig-Hoffstein method and reported by SNAP testing differed from the titration pressure that had been determined in his laboratory. This difference was never mentioned in his article; nor were data supporting his claim mentioned when the article was submitted for peer review. In contrast, the Miljeteig-Hoffstein formula has been well-validated. Excluding patients who had been found to be nonapneic by the SNAP test. Why was this not fully disclosed in the article?

Eliminating nonapneic patients as well as four patients with obviously severe disease (who excluded due to a split-night polysomnography study) weighted the sample heavily toward patients with mild apnea, introducing a substantial bias given the higher night-to-night variability in that group of patients. Omitting crucial details in his article pours cold water on Millman’s defensive statement “our article went through vigorous peer review to be accepted by CHEST.”

REFERENCES


To the Editor:

Dr. Gil Raviv has faulted us for stating that our article was the first published article about SNAP technology. In fact, it was the first article about SNAP and came out prior to the publication of the University of Chicago study that appeared in Otolaryngology Head and Neck Surgery in 2004. We originally submitted our publication for consideration in CHEST on October 9, 2002, and the revision was accepted September 30, 2003. In addition, our article came out in early 2004, before the University of Chicago publication. We, therefore, had no knowledge about this study at the time of our study, there were no peer-reviewed published articles about SNAP technology. In fact, Dr. Thomas Kehoe’s letter notes that, prior to the publication of the University of Chicago study, there were five separate validation studies performed on SNAP, but none of them were published. He even implied that there was a conspiracy to keep these studies from being published: “Previous attempts to publish these side-by-side blind studies have met with strong resistance by journals with review committees dominated by sleep specialists.”

I have no objections to any type of type 3 recording device, as long as it has gone through rigorous peer review. As I mentioned in my previous letter to the editor, I feel there is a potential role for portable studies in select patients with a high pretest clinical suspicion for sleep apnea.

In regard to another comment by Dr. Raviv, he noted that we systemically excluded patients that were found to be nonapneic
by SNAP testing. We did not exclude these patients on purpose. They simply were not referred to us. The primary care physicians who used SNAP technology were told by the SNAP sales force that a negative study was indeed negative. The physicians, therefore, did not refer patients with a negative study to us. This does not mean that the patients did not have sleep apnea. In fact, studies have shown that a negative sleep evaluation should be repeated, even if the initial evaluation was standard polysomnography, if there is a high suspicion for sleep apnea. Although there are deficiencies in our study, we had no other peer-reviewed data available to us at that time.

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REFERENCES