Idiopathic Chronic Cough and Organ-Specific Autoimmune Disease

To the Editor:

In a recent article in CHEST (May 2005), Mund et al described 11 female patients with idiopathic chronic dry-cough, with onset occurring around the menopause, that was associated with an increase in absolute lymphocyte count in BAL fluid. We have also previously described a lymphocytic bronchoalveolitis in patients with idiopathic chronic dry-cough and have noted the onset of cough around the age of menopause. In our experience, these patients have an increased prevalence of organ-specific autoimmune disease, particularly hypothyroidism.\(^{2,3}\) We have suggested that the cough may be the result of the aberrant homing of activated lymphocytes to the airways in a manner analogous to the airway diseases seen with inflammatory bowel disease.\(^{3}\) Support for this view is provided by our findings that patients with treated hypothyroidism have an increased prevalence of cough, a heightened cough reflex sensitivity, and evidence of low-grade airway inflammation.\(^ {4,5}\) It is not clear from the study presented by Mund et al whether their patients with idiopathic cough were asked specifically about the presence of organ-specific autoimmune disease and whether autoantibodies were measured. Further immunopathologic studies are required to identify novel therapeutic targets for this troublesome condition.

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**REFERENCES**


To the Editor:

We appreciate very much the comments by Birring and Pavord on our article in CHEST (May 2005).\(^ {1}\) In our study, we described a condition characterized by dry cough and a lymphocytic bronchitis dominated by activated CD4+ cells. Apart from dry cough, all patients were, according to the inclusion criteria, otherwise asymptomatic and were not receiving regular treatment with drugs. This condition, which was characterized by dry cough as the only symptom, was found only in women, and it seemed to have commenced in connection with an airway infection that coincided with the menopause. Although we did not take specific diagnostic measures in order to prove the existence of hypothyreosis, diabetes mellitus, pernicious anemia, inflammatory bowel disease, Sjögren syndrome, or other autoimmune conditions, no patients had symptoms that led us into the suspicion of organ-specific autoimmune diseases. We agree that further immunopathologic studies are required for a more detailed identification of the condition. It is an intriguing thought that the described “dry cough condition” is mediated by autoimmune mechanisms and that infections during menopause may...
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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REFERENCES

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 identified apnea severity. Su et al utilized a more powerful design performing both tests simultaneously. In contrast, Liesching et al have been severely 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.

Finally, in his latest reply, Millman admitted systematically 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.”

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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...