Notice From the Editor-in-Chief and the Ethics Subcommittee of CHEST Associate Editors

It has come to our attention that the articles entitled “The Additional Value of Gadolinium-Enhanced MRI to Standard Assessment for Cardiac Involvement in Patients With Pulmonary Sarcoidosis” in CHEST (2005; 128:1629–1637; September) and “Evaluation of the Accuracy of Gadolinium-Enhanced Cardiovascular Magnetic Resonance in the Diagnosis of Cardiac Sarcoidosis” in Journal of the American College of Cardiology (2005; 45:1683–1690; May) contained very similar study designs and content, including the absolute duplication of paragraphs of the text and two of the figures.

It has also come to our attention that the authors of both articles failed to make full and complete disclosure, in two ways: (1) by failing to disclose the overlap in patient populations between the articles; and (2) by not referring in the CHEST article to the accepted article in the Journal of the American College of Cardiology.

CHEST and its publisher, The American College of Chest Physicians, have taken appropriate action regarding the authors. Based on its own assessment, PubMed has tagged both articles as duplicate publications.

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Dr. Irwin has reported that he has no conflicts of interest with any companies/organizations whose products or services may be discussed in this article. Reproduction of this article is prohibited without written permission from the American College of Chest Physicians (www.chestjournal.org/misc/reprints.shtml). DOI: 10.1378/chest.06-2750

Real-Time Electromagnetic Navigation Bronchoscopy for Peripheral Lesions

What About the Negative Predictive Value?

To the Editor:

The authors1 should be congratulated for their work (April 2006) with this fascinating new technology. Electromagnetic navigation clearly appears to improve the sensitivity of bronchoscopy in the diagnosis of pulmonary nodules and masses. Nevertheless, the application of such technology must be put in context of its clinical use; in this case the evaluation of patients with lung masses suspicious for lung malignancy. The principal advantage of attempting a diagnostic procedure in an operable patient with a solitary lung lesion suspicious for lung cancer is to reliably confirm that a malignancy is not present. In other words, the negative predictive value of the test is its most important characteristic in this setting.

This is the basis for the recommendation found in the American College of Chest Physicians (ACCP) lung cancer guidelines stating that, with regard to transthoracic needle aspiration (TTNA), the false negative test result rate of TTNA is high (range, 0.20 to 0.30). Thus, TTNA is generally not useful in ruling out cancer. As such, TTNA has no role in patients who have lesions that are even moderately suspicious for lung cancer, and who appear to have early-stage disease and are candidates for surgical resection. Although a test that could reliably rule out lung cancer might be useful in this setting, the high FN rate of TTNA makes reliance on a negative result untenable.

In essence, a patient with a positive biopsy result for malignancy will need surgical resection, and a patient with a negative finding will still need surgical resection given the poor negative predictive value of the test. In this patient population, TTNA has no impact on clinical management and can only lead to delays in definitive care and potential complications.

The negative predictive value of the described technique in this study, although not commented on in the text, is in fact very similar to that described for TTNA. Of the five patients with negative biopsy results for cancer, four patients had false-negative results. The negative predictive value is therefore one fifth (20%). The above ACCP recommendation on TTNA could therefore be extrapolated to this novel technique.

Until such time that a test can demonstrate a high enough negative predictive value to comfortably avoid surgical resection, the test may not lead to significant changes in management for operable patients with suspected resectable lung cancer. The utility of these tests will remain limited to confirmation of cancer in inoperable patients or those with nonresectable disease.

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Role for Transbronchial Biopsy in the Diagnosis of Usual Interstitial Pneumonia

To the Editor:

We read with interest the article by Berbesca et al regarding the role for transbronchial biopsy (TBB) in the diagnosis of usual interstitial pneumonia (UIP). The authors conclude that TBB may be more useful than previously appreciated in the diagnosis of UIP. We do not believe their data support this conclusion.

First, their analysis is purely retrospective and hence prone to multiple forms of bias. For example, as the authors themselves acknowledge, they were aware of the diagnosis prior to reviewing the TBB specimens. Without blinding of the pathologist as to the final diagnosis, one can draw no firm conclusions regarding the yield of TBB in patients suspected of having UIP. Second, selection bias is a major concern since subjects with UIP who underwent TBB are likely to be systematically different from those who did not undergo TBB. Third, the absence of a control arm of patients lacking UIP precludes any effort to calculate precisely the sensitivity and specificity of TBB. Similarly, they provide no denominator as to the number of subjects at either institution who underwent TBB for possible UIP during the study period.

Alternatively, two large, recent prospective analyses suggest that the value of TBB in suspected UIP is quite low. For example, Kagami and coworkers evaluated 59 consecutive patients thought to have UIP and found TBB to be nondiagnostic in 85% of subjects. A multicenter investigation reported an even lower yield for TBB (2 of 91 subjects). The low yield for TBB needs also to be balanced against the relative safety of surgical lung biopsy (SLB) in this population. One recent case series documents that most patients suspected of having UIP tolerate this procedure well.

Concern about pathologic interpretation also merits consideration. Nine of the 21 subjects with UIP in the report by Berbesca et al showed only features of nonspecific interstitial pneumonia on TBB. Hence, without proceeding to SLB, nearly 40% of the study population might have been substantially misclassified and perhaps faced a delayed referral for lung transplantation. Furthermore, in the hands of nonpulmonary pathologists there is the potential for substantial misdiagnosis. Even with large tissue specimens, nonpulmonary pathologists misdiagnosis UIP frequently.

Increased reliance on TBB might further compound the issue. Although intriguing that in hindsight evidence acknowledges that TBB specimens, nonpulmonary pathologists misdiagnosis UIP frequently.

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The authors have no conflicts of interest to disclose.

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Response

To the Editor:

I believe that Drs. Shorr, Lettieri, and Helman misinterpreted the message of our article (May 2006). This work generated a hypothesis that will require further testing. It was not intended to produce a clinical recommendation to use transbronchial lung biopsies (TBBs) to diagnose usual interstitial pneumonia (UIP). In our article, we clearly state that TBBs should be tested in a blinded fashion and in a cohort of diffuse lung diseases including UIP and non-UIP cases. However, it is undeniable, as we show in several of our figures, that features specific for UIP, such as patchwork pattern of interstitial fibrosis, fibroblastic foci, and honeycomb change, are readily recognizable in many TBB specimens. We did not report findings of nonspecific interstitial pneumonia in 9 of 21 patients, as Drs. Shorr, Lettieri, and Helman state. We make clear in the “Materials and Methods” section that findings were “nonspecific” if there was only interstitial fibrosis on TBB specimens. Unfortunately, it has become accepted, despite lack of convincing data, that TBBs are not useful in diagnosing idiopathic interstitial pneumonias. However, if in the future TBBs are found useful in diagnosing UIP from a pool of patients with diverse idiopathic interstitial pneumonias, many unnecessary surgical lung biopsies with associated morbidity and mortality could be prevented.

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Allergic Rhinitis May Be Important in Snoring Infants

To the Editor:

We read with interest the article by Kalra et al. who found that the prevalence of habitual snoring for 1-year-old children was 15% in a high-risk group for atopy, ie, children born to atopic parents. This prevalence was significantly higher than that reported in older children, 10.9% in 6- to 12-year-old children in Hong Kong, and 10 to 14% in < 6-year-olds in Europe and the United States. This higher prevalence is most likely due to the fact that 29% of this group of infants were atopic, and it is not surprising that Kalra et al found that the presence of atopy increased the risk of habitual snoring (odds ratio, 2.0; 95% confidence interval, 1.2 to 3.0), a common symptom of obstructive sleep apnea syndrome (OSAS). This was similar to previous findings in Hong Kong, when we found allergic rhinitis to be a significant risk factor for witnessed apnea with an adjusted odds ratio of 2.19, another symptom of OSAS as well as in the United States. It is unfortunate that Kalra et al did not report the prevalence of nasal symptoms in their cohort, as the data would have shed light on the mechanisms that link habitual snoring and atopic status, possibly allergic rhinitis. Other important data that were not reported were the parts played by individual allergens, especially the airborne allergens vs food allergens. These data would help determine whether the inhaled route or the ingested route is important in the habitual snoring infants. In the study of Kalra et al, the definition of positive atopic status for egg white and whole milk was wheal ≥ 3 cm than the negative control, and this would be associated with a high false-positive rate.

Allergic rhinitis leads to markedly increased nasal resistance, and Rappai et al found that nasal congestion was a strong independent risk factor for snoring and an increased likelihood for moderate or severe sleep-disordered breathing. In conclusion, Kalra et al reported an important study about infantile snoring, but important data were not included that may shed more light on this underrecognized symptom. Nevertheless, all medical practitioners dealing with atopic infants should ask the same question as did Kalra et al, ie, how often does your baby snore?

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Allergic Rhinitis May Be Important in Snoring Infants

To the Editor:

We read with interest the article by Kalra et al, who found that the prevalence of habitual snoring for 1-year-old children was 15% in a high-risk group for atopy, ie, children born to atopic parents. This prevalence was significantly higher than that reported in older children, 10.9% in 6- to 12-year-old children in Hong Kong, and 10 to 14% in < 6-year-olds in Europe and the United States. This higher prevalence is most likely due to the fact that 29% of this group of infants were atopic, and it is not surprising that Kalra et al found that the presence of atopy increased the risk of habitual snoring (odds ratio, 2.0; 95% confidence interval, 1.2 to 3.0), a common symptom of obstructive sleep apnea syndrome (OSAS). This was similar to previous findings in Hong Kong, when we found allergic rhinitis to be a significant risk factor for witnessed apnea with an adjusted odds ratio of 2.19, another symptom of OSAS as well as in the United States. It is unfortunate that Kalra et al did not report the prevalence of nasal symptoms in their cohort, as the data would have shed light on the mechanisms that link habitual snoring and atopic status, possibly allergic rhinitis. Other important data that were not reported were the parts played by individual allergens, especially the airborne allergens vs food allergens. These data would help determine whether the inhaled route or the ingested route is important in the habitual snoring infants. In the study of Kalra et al, the definition of positive atopic status for egg white and whole milk was wheal ≥ 3 cm than the negative control, and this would be associated with a high false-positive rate.

Allergic rhinitis leads to markedly increased nasal resistance, and Rappai et al found that nasal congestion was a strong independent risk factor for snoring and an increased likelihood for moderate or severe sleep-disordered breathing. In conclusion, Kalra et al reported an important study about infantile snoring, but important data were not included that may shed more light on this underrecognized symptom. Nevertheless, all medical practitioners dealing with atopic infants should ask the same question as did Kalra et al, ie, how often does your baby snore?

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Response

To the Editor:

We thank Ng et al for their interest in our article.1 They raise concern about lack of data on nasal symptoms and our definition for positive skin-prick test results. Using the rhinitis definition of a positive parent report to the question “In the past 12 months, has your child ever had a problem with sneezing, or a runny, or a blocked nose when he/she did not have a cold or flu?” we found a strong association between rhinitis and habitual snoring (p < 0.001) in our cohort of 681 infants. In multivariate logistic regression model with habitual snoring as the dependent variable, the adjusted odds ratio for rhinitis was 2.5 (confidence interval, 1.7 to 4.0) and atopy (defined as positive skin-prick test result to an aeroallergen or food allergen was 1.9 (confidence interval, 1.2 to 2.9). Infants with positive skin-prick test results to aeroallergens compared to infants with negative skin-prick test results to all allergens had a trend for higher prevalence of habitual snoring (20.4% vs 12.9%, p = 0.05); and infants with positive skin-prick test results to food allergens compared to infants with all negative skin-prick test results to all allergens had a significantly higher prevalence of habitual snoring (22.5% vs 12.9%, p = 0.01). It is important to mention that at age 1 year, the prevalence of atopy to food allergens was much higher than that to aeroallergens. Therefore, a smaller sample size with lower power for aeroallergens could potentially explain the lack of significance for that group. At age 2 years, we have reported a dramatic increase in aeroallergen atopy.2 Given these findings, we propose to study the independent relationship between atopy to aeroallergens and habitual snoring at age 2 years. Our definition of a positive skin-prick test result is the standard proposed by the practice parameter committee of the Academy of Allergy, Asthma, and Immunology.3 Unfortunately, as pointed out by Ng et al, there are some limitations to this criterion. In summary, our data suggest that both rhinitis and atopy are independently associated with increased risk for habitual snoring in infants.

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