To the Editor:

We appreciate the interest of Dr. Joseph and colleagues in our recent article in CHEST [January 2004] on ethnicity and skin test reactivity to allergens among children with asthma. We thank them for bringing their article on racial differences in physiologic parameters related to asthma to the attention of the readers of CHEST.

We would like to point out that the results of our recent study and those of the study conducted by Joseph and colleagues are not comparable. Whereas our study included only children with asthma (791 children), their study included children with and without asthma. In the study conducted by Joseph et al, there was no difference in serum levels of IgE between African-American children with asthma (8 children) and European-American children with asthma (49 children). In their study, African-American children without asthma had a higher total serum IgE level than did European-American children without asthma. For the analysis of the relation between sensitization to specific allergens and ethnicity, Joseph et al did not present the results of an analysis that had been stratified by asthma status. Thus, it is not clear whether the reported association between African-American ethnicity and sensitization to two outdoor allergens was present in children with or without asthma. It should also be noted that our analysis of the relation between ethnicity and allergen sensitization among children with asthma was adjusted for health insurance status, area of residence, asthma severity, and other potential confounders. In the study by Joseph et al, the analysis of the relation between ethnicity and allergen sensitization was not adjusted for potential confounders.

What the findings of both our study and those of the study conducted by Dr. Joseph and colleagues suggest is that allergy skin testing should be considered more often in African-American children with symptoms that are suggestive of allergic diseases such as asthma. Finally, allergen sensitization in minority populations is deserving of further study, as it may provide important clues to asthma health disparities.

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REFERENCES

Conventional vs Endobronchial Ultrasound-Guided Transbronchial Needle Aspiration of the Mediastinum

To the Editor:

We read with great interest the article by Herth et al [January 2004], which reports the results of a randomized trial comparing conventional vs endobronchial ultrasound (EBUS)-guided transbronchial needle aspiration (TBNA) of mediastinal lymph nodes (LNs). This is a very intriguing study, in which the authors performed TBNA during flexible or rigid bronchoscopy, and separately randomized and analyzed the results of the TBNA procedures obtained from different LN stations. In a first group, they included exclusively the subcarinal nodes, since they are easily accessible by any method. In a second group, they included all the TBNA performed in the following LN stations according to the American Thoracic Society classification: 2 (right and left); 3, 4 (right or left); and 5. The conclusions and the comments of this study contain some very important issues for bronchoscopists who routinely perform TBNA, and therefore deserve a few comments.

The authors conclude that “EBUS guidance significantly increases the yield of TBNA in all lymph node stations except in the subcarinal one.” However, the data as proposed in Table 2 of their study show that similar diagnostic yields were obtained by both conventional and EBUS-guided TBNA also in the lower paratracheal area (4 right, 4 left). By considering these data, it looks like blind TBNA procedures proved as effective as ultrasound-guided ones in those stations (4 right, 4 left, 7), among those accessible to TBNA, where the majority of metastasis from non-small cell lung cancer (NSCLC) occurs. This result is not surprising if one takes into account the fact that the above-mentioned LN areas (mainly 4R and 7) have been associated with very high (approximately 70%) diagnostic yields of conventional TBNA in several comprehensive studies in the settings of both malignant and benign diseases. A definite advantage was associated with ultrasound guidance only for TBNA performed in LN stations (2, 3) less frequently involved by the metastatic spread of NSCLC; which is the most common indication to TBNA in clinical practice.

The article also shows that 21 of 50 TBNA procedures in the non-subcarinal group were performed in the aortopulmonary window (APW), also called the subaortic station (station 5), which to the best of our knowledge is not accessible to TBNA. According to the American Thoracic Society LN map definition, the APW nodes “…are lateral to the ligamentum arteriosum or the aorta or the left pulmonary artery…” and are therefore not in contact with the airways. In a recent review on invasive mediastinal staging of NSCLC, it is stated that the possible ways to access the APW nodes are the following: anterior mediastinotomy (also known as the Chamberlain procedure), extended cervical mediastinoscopy, thoracoscopy, and transesophageal endoscopic ultrasound with fine-needle aspiration. Is it possible that these 21 TBNA procedures were performed in the left paratracheal area?

Another important aspect that is dealt with by Herth and colleagues is concerned with the significance of a TBNA aspirate yielding lymphocytes only, a finding basically meaning that the LN has been likely punctured. Interestingly, they observed that no patients with lymphocytes only on TBNA had a more specific diagnosis after subsequent surgical biopsy. We have proposed, albeit arbitrarily, that at least 30% of cellularity be composed of lymphocytes in order to consider adequate a TBNA cytology specimen. By using this quantitative cut-off value, two of nine adequate negative TBNA cytology specimens (23%) were subsequently shown to be false-negative at mediastinoscopy in a study on
the role of TBNA in the mediastinal staging of NSCLC.\textsuperscript{2} Even more false-negative, lymphocyte-based TBNA aspirates can be observed in patients with sarcoïd LN nodes, a condition in which the use of a 19-gauge histology needle makes it much easier to recover granulomas.\textsuperscript{5} By considering these data, we tend not to rely exclusively on an adequate negative TBNA cytology specimen—even in the presence of lymphocytes only—to rule out a more specific diagnosis mainly if the clinicoradiologic picture is evocative.

In conclusion, we think that EBUS can be a useful tool to guide TBNA in some specific settings, such as “difficult mediastinal LN areas” (mainly 2, 3, 4L) and small LN size (< 1 cm), although the technique is costly and requires a considerably long apprenticeship. For the time being, conventional TBNA has to be considered less costly, easier to learn, and offers similar yields in the LN stations more frequently involved by NSCLC, among those accessible to the technique.

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Measuring the Work of Exercise

To the Editor:

Drs. Irvin and Kaminsky (January 2004)\textsuperscript{1} have stated that “... often there is a discordance between work (watts) and [oxygen uptake] \( \text{VO}_2 \) in clinical testing, making the interpretation and final determination of exercise tolerance difficult.” The authors suggested that normal exercise should be predicted as the maximal number of watts rather than the maximal \( \text{VO}_2 \).

Our exercise laboratory uses the predicted maximal \( \text{VO}_2 \) based on ideal body weight, and we have had good agreement between the percent predicted values for these two measurements in our patient population. I think that discrepancies in these two measurements may be due to the usage of inappropriate predicted equations or weights, especially in obese patients. It makes sense that the measured work on a cycle ergometer (expressed in watts) is only part of the patient’s total work done. Additional work by the muscles of respiration and upper arm musculature cannot be measured at the pedals but can be measured as \( \text{VO}_2 \). If there are no technical errors, a discrepancy between maximal work done at the pedals (watts) and \( \text{VO}_2 \) may actually be important information indicating a significant component of nonleg work. Therefore, I suggest that readers not necessarily abandon the use of maximal \( \text{VO}_2 \) as a measure of total work done.

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1 Irvin CG, Kaminsky DA. Exercise for fun and profit. Chest 2004; 125:1–3

To the Editor:

We thank Dr. Kirsch for his comments regarding the use of work vs oxygen uptake (\( \text{VO}_2 \)) to assess exercise tolerance. Most of the time, these two measures are in close agreement in terms of percent predicted and can be used interchangeably as objective measures of exercise tolerance. Dr. Kirsch correctly points out some of the reasons for the discrepancies between these two measures, such as the selection of appropriate predicted values, especially in obese individuals, and the performance of different types of exercise. Most studies relating exercise capacity to important outcomes such as survival or the ability to tolerate lung resection surgery use \( \text{VO}_2 \) as the measure of exercise capacity because it reflects the physiologic health of the individual in terms of their global ability to utilize oxygen. Our suggestion that work may be a more appropriate measure of exercise tolerance is based on our view of exercise expressed as power output (work per unit of time) rather than oxygen utilization. Indeed, many subjects achieve a percent predicted for work that is higher than their maximal percent predicted \( \text{VO}_2 \) likely indicating a motivational ability to sustain exercise beyond the anaerobic threshold. In real-world terms of the ability to perform various tasks and activities, the amount of work someone is able to do seems more relevant than the amount of oxygen they can consume. In this regard, defining exercise tolerance in terms of work is also more appropriate when prescribing exercise or explaining the results of exercise testing to patients. As we stated in the editorial, we invite more discussion in this area of the definition of exercise tolerance.

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