Dr. Petty’s preference to use the FEV₁/FVC ratio is well-understood. It has been well-described that subjects with airway obstruction, that is, a low FEV₁/FVC ratio, are more likely to have airway hyperresponsiveness (AHR). We evaluated the association between the “new” ratio (i.e., FEF₂₅–₇₅/FVC) and AHR because we believed that it describes a potentially different mechanism that leads to AHR. A low FEF₂₅–₇₅/FVC ratio may identify subjects who have relatively small airways for their lung size but do not have airway obstruction, whereas the FEV₁/FVC ratio would identify subjects with airway obstruction.

The purpose of our article was not to advocate the use of FEF₂₅–₇₅ or the FEF₂₅–₇₅/FVC ratio in the clinical interpretation of pulmonary function test results. We agree that these parameters have not been shown to be good predictors of the development of obstructive airway disease. Rather, the focus of our article was to demonstrate that small airway size relative to lung size was a more important determinant than the absolute airway size alone for AHR. This relationship between dysanapsis and AHR may provide another potential mechanism for the pathogenesis of asthma.

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Locating and Selecting Appraisal Studies for Reviews

To the Editor:

We have read with great interest the critical care review by Kreider and Lipson (July 2003) concerning bronchoscopy for atelectasis in the ICU. However, the authors used a single method of locating review articles, including only MEDLINE, and excluded other electronic databases. Currently, such a restricted search is generally not considered adequate. Studies have shown that only 30 to 80% of all known published randomized, controlled trials were identifiable using MEDLINE (depending on the area or specific question). A comprehensive search is important not only for ensuring that as many studies as possible are identified, but also to minimize any selection bias for those that are found. Relying exclusively on a MEDLINE search may retrieve a set of reports unrepresentative of all reports that would have been identified through a comprehensive search of several sources. To prevent bias and to ensure that all relevant data are included in a review, it is fundamental to use multiple sources such as EMBASE, Best Evidence, and the Cochrane Library to identify studies and then to use a systematic approach to select those that will be included.

Another electronic database is LILACS for Latin America and the Caribbean, which indexes regional literature that contains studies since 1982 and abstracts in English, Portuguese, and Spanish. The US National Library of Medicine medical subject headings (MeSH) vocabulary is used to index each LILACS citation. LILACS is edited by BIREME (Latin-American and Caribbean Health Science Information Center), an agency of the Pan American Health Organization/World Health Organization, located in São Paulo City, Brazil. Of the 670 journals indexed in LILACS, only 45 overlap with those indexed in MEDLINE.5,6 Searching in LILACS according to the strategy proposed by the Cochrane Collaboration of the same terms (bronchoscopy, atelectasis, ICU, and critical illness) used by Kreider and Lipson for randomized, controlled trials, allowed the retrieval of two other citations. They may or may not reinforce the conclusions reached by authors, but our point is to stress that any search strategy of systematic or narrative reviews can and should include LILACS as an obligatory database.

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To the Editor:

We appreciate the comments made by Drs. Yoo and Queluz. We agree with the importance of using multiple sources to identify appropriate studies when performing any systematic review or meta-analysis. In our review, we performed an extensive search using MEDLINE utilizing two different search engines (OVID and PubMed). Additionally, we identified other citations from careful review of relevant bibliographies of both original studies and appropriate review articles. In light of the concern of Drs. Yoo and Queluz, we again performed our search using the Cochrane and the EMBASE databases. We identified only one potential additional reference in a journal of limited circulation. We believe it is unlikely we missed any study that would have significantly altered our final conclusions.

Drs. Yoo and Queluz bring up an important issue regarding language bias in the conduct of systematic reviews. It is currently unclear how non-English–language studies differ from those published in English. There are conflicting reports regarding differences in quality and the likelihood of publication of negative results in non-English–language literature. Despite these concerns, evidence suggests that excluding trials published in languages other than English generally has little impact on the final conclusions of review articles and meta-analyses. Specifically, Moher et al conclude that there is “... no evidence that language restricted meta-analyses lead to biased estimates of intervention effectiveness.” By including non-English–language articles and increasing the number of examined studies, the general direction and scope of estimates will not change; however, the precision of estimates is likely to be increased. If there had been adequate studies to perform a meta-analysis and determine a single estimate of the effectiveness of bronchoscopy for atelectasis in the ICU, inclusion of non-English–language articles may have been beneficial. However, the final conclusions of our review would not have been altered.

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REFERENCES

Infant Blood Transfusions

To the Editor:

The article by Maillet and colleagues (May 2003) needs a short comment. It is often necessary to transfuse blood products during cardiac surgery. While in adults the ratio of transfused blood to the patient’s own blood is not as important, in children (and especially in small infants) the quantity of transfused blood might be as high or even higher than that of their own blood (eg, for cardiopulmonary bypass). The longer that blood concentrates are stored, the higher are the levels of potassium, glucose, and lactic acid. Thus, transfusion itself influences the level of lactate, which was (according to the authors) a predictor of higher postoperative risk.

Other investigators have shown that transfusion itself is a risk factor for patients in an ICU. In the article by Maillet et al, the transfusion rate was not mentioned as a possible risk. Fixing the predictors of mortality is very difficult when they are influenced by several factors.

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To the Editor:

We appreciate the interest in our article. We agree with your comment concerning the importance of the effect of transfusion on lactate level and risk factor for patients in the ICU. We apologize for missing these potentially important data.

In our study, intraoperative transfusion rates were comparable for patients with no hyperlactatemia (11.4%) and those with late hyperlactatemia (15%; difference not significant). The transfusion rate for patients with immediate hyperlactatemia (IHL) was statistically higher compared to both those groups (30%; p < 0.05). Multivariate analysis of IHL was not significantly modified when transfusion was included in the new model.

It is true that “to fix predictors of mortality is very difficult.” The aim of our study was not to identify independent risk factors of mortality after cardiac surgery. It was to evaluate whether lactate levels, but especially the timing of their acquisition, permitted the stratification of patients with different postoperative risks.

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