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,1 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.5–4 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.2 Specifically, Moher et al5 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)1 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 (e.g., for cardiopulmonary bypass). The longer that blood concentrations are stored, the higher are the levels of potassium, glucose, and lactic acid.2 Thus, transfusion itself influences the level of lactate, which was (according to the authors) a predictor of higher postoperative risk.

Other investigators3 have shown that transfusion itself is a risk factor for patients in an ICU. In the article by Maillet et al,1 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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REFERENCES


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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Competencies in Pulmonary Procedures

To the Editor:

We read with interest the guidelines from the American College of Chest Physicians on interventional pulmonary procedures (May 2003). The introduction noted that although there were not “data on all procedures,” the writers should “not shy away from competency guidelines altogether.” Indeed, no shyness was employed. Specific numbers of procedures required to establish competency were routinely included in the document, and every invasive procedure required 10 procedures per year to maintain competency. Since no data were available, we assumed that the authors surveyed training program directors, but we could find no reference to this.

The spirit of competencies is commendable and is used for training by other societies. Nonetheless, baseline numbers must be evidence-based or from a broad survey of the society membership to be credible and useful. While we applaud the intent of this report, the process responsible for these recommendations is fatally flawed. (1) Surveys of training directors should have been performed, and a writing committee should have been appointed, with its final product approved by the assembled experts. This more credible and accepted consensus process would involve establishing levels of evidence supporting the guideline. Was this done? If so, why was it not included in the manuscript? (2) In our experience, specific procedural thresholds are more useful to establish “initial” competency rather than “ongoing” skills. A specific example is rigid bronchoscopy. On page 1696, the authors state, “Dedicated operators should perform at least 10 procedures per year to maintain competency.” For those of us who have been performing rigid bronchoscopy for years with no mortality and minimal complications, we find this declaration ill advised. For example, someone with 20 years of experience does not necessarily require the same number of procedures as an inexperienced operator. Complication rates and results are generally thought to comprise a better guideline than an arbitrary minimum number. Indeed, unintended consequences may result if the guideline encourages the inappropriate overuse of the rigid bronchoscope or other invasive procedure for the “touch up of lesions.” (3) Misguided, poorly researched, inadequately supported and/or arbitrary numbers provide a breeding ground for misinterpretation by those organizations (eg, Joint Commission on Accreditation of Healthcare Organizations, American Heart Association, National Quality Forum, and LEAPPROG) that wish to partner with medicine to improve quality.

To maintain the integrity of the medical literature, we must readily distinguish between opinion and guidelines. A detailed discussion of the available medical evidence or disclosure of the lack thereof is mandatory for “society-endorsed” guidelines.

In the absence of evidence, a broad-based survey of training programs and practitioners is necessary as a starting point. The American College of Chest Physicians should begin an immediate process of reviewing the literature and documenting its quality, as well as surveying its membership and conference attendees, so that a new and legitimately documented set of guidelines can be published. The current recommendations appear arbitrary. They should be renamed opinion, and the designation of guidelines withdrawn.

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To the Editor:

Three concerns have been raised in response to the American College of Chest Physicians (ACCP) guidelines (May 2003): 1

1. Surveys of training directors and a writing committee of experts would provide better guidelines, including, presumably, more accurate definitions of competence.

2. Specific procedural thresholds may be inferior to complication rates when determining ongoing competence. The frequency of performing a procedure is not necessarily a measure of competence among experienced practitioners.

3. Quality overseers may misinterpret “arbitrary” procedure numbers.

Most clinical recommendations in the literature are expert opinions. Evidence-based guidelines remain the exception. In fact, there is no significant literature in this area that satisfactorily addresses the issue of defining competence. A committee of experts, including interventional pulmonologists, critical care specialists, and thoracic surgeons, wrote the guidelines. The authors include academic physicians, private practice physicians, and interventional pulmonologists, both from the United States and Europe. We thought that this broad specialty and practice representation would be appropriate to assure a balanced document. It is not clear to us why a poll of program directors (who may or may not have direct expertise in these areas) would contribute substantially to the published document.

The authors and the ACCP carefully considered the semantics of the terms guidelines and standards. The conclusion was that standards implies a finality of recommendation that was both inappropriate for this document as well as subject to misuse by outside governing bodies and organizations. In contrast, the term guidelines implies an expression of expert opinion, that is subject to revision and is not binding upon practitioners. We have always intended this as a “work in progress” that would undergo periodic reassessment and revision.

With regard to specific numeric guidelines for establishing and maintaining competence in these procedures, we think there is ample evidence that performing medical procedures more often leads to fewer complications. This is true for central line placement and thoracentesis, and for chest tube placement. Most