categorized as elevated. It would be interesting to repeat all the analyses taking this into account.

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To the Editor:

We agree with Asero and colleagues that cross-sectional studies such as ours cannot determine the temporality nor the natural history of the relationship between smoking (or reduced lung function) and systemic inflammation, an important limitation of the study, which we pointed out in the limitation section of our article. However, cross-sectional studies can ascertain risk factors through measures of association. Thus, the statement that “reduced lung function is a risk factor” is fully justified by the methodology we used.1,2 We also agree that factors such as airway hyperresponsiveness (AHR), endothelial activation, and neutrophil recruitment may be involved in effecting systemic inflammation in smokers and nonsmokers with reduced lung function. However, these factors may not be confounders; rather, they may be part of the causal pathway(s) linking reduced lung function with systemic inflammation. For example, in the Lung Health Study, which evaluated >4,200 participants with mild COPD with serial methacholine challenge tests over 5 years, the investigators found that even among participants who stopped smoking, airway responsiveness increased during the follow-up period. This indicates that there are factors other than cigarette smoking that are responsible for AHR in individuals with reduced lung function. There are good experimental data to indicate that airway inflammation and remodeling are important determinants of AHR.4,5 Airway inflammation may also relate to systemic inflammation.6 Thus, it would be misleading and erroneous to consider AHR as a confounder in the relationship between reduced lung function and systemic inflammation; it is likely to be part of its causal pathway. Finally, we disagree with the assertion of Asero et al that there was C-reactive protein (CRP) misclassification because we considered CRP levels >2.1 mg/L to be elevated. In the general population, the geometric mean of CRP for individuals 55 to 64 years of age is between 1.6 and 2.2 mg/L.7 By taking 2.1 mg/L as the cutoff threshold, we in effect used the median CRP value of the general population in dichotomizing the study sample, an approach that is widely accepted and commonly used to dichotomize continuous variables for analytic purposes in the medical literature.8 This approach strikes a reasonable balance between validity and efficiency. If we were to take the suggestion of Asero et al and use 10 mg/L as the cutoff value, we would significantly compromise the statistical power and the efficiency of the study without improving the validity of the findings.9 This approach therefore is best avoided.

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Probabilistic Risk Assessment and Performance Index Applications for the ICU

To the Editor:

Dr. Garland provided a comprehensive review of the interplay of medical, technical, administrative, social, and economic issues facing ICU organization and management.1 He is to be congratulated for reminding the medical community of the concept of an integrated system approach to unravelling the complex and
interdependent factors that influence patient care in the ICU. Standard analytical tools employed for quality assessment such as root cause analysis are simply inadequate. Probabilistic risk assessment (PRA) is a systematic and comprehensive methodology widely applied in other industries to evaluate undesirable outcomes such as risk, and PRA may be a promising tool for assessment of adverse events in the ICU. PRA entails constructing the chronology of a scenario and a “fault tree” of occurrences that could lead to a specific adverse outcome. By analysis of the likelihood of each contributor to the adverse event, one could quantify the likelihood of that outcome occurring and focus efforts on modifying each of these events. Results from PRA would also provide a quantitative basis for specific event rates and for overall system performance over time.

The need for serious discussions of ICU performance metrics is overdue. We gauge the “health” of financial or business entities by measurements like price-to-earnings ratio, equity, debt load, or cash on hand. Similarly, we monitor the integrity of engineering systems by their failure rates, reliability, recovery rate, and level of redundancy. The performance measures performed by Dr. Garland are good first steps toward arriving at community consensus; others may propose alternatives. The ultimate test would depend on the usefulness of these measurements in improving care in the ICU.

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Decisions for Critically Ill Patients

To the Editor:

In a recent article in CHEST (February 2005), Tonelli illustrated the ethical issues surrounding the participation of critically ill patients in decisions about ICU withdrawal, arguing that the focus on conflicting ethical principles of autonomy and beneficence is inadequate, and suggesting instead the usefulness of examining the unique clinical circumstances compared to paradigm cases. This offers little moral guidance. Our experience with children ventilated and given high dose steroids during an acute deterioration in their severe end-stage lung disease, for which they were listed for lung transplant, is that recovery is rare and ICU withdrawal often occurs. Usually, these patients are adolescents, who, as English Law deems them competent to assert to medical decisions yet not to decline them, cannot actively participate in end-of-life discussions. Ethical questions about waking these patients to tell them death is imminent, allowing them opportunities to say goodbye, invariably occur. Currently, the views of the ICU team, the ethics committee, and the families concur. Waking is inappropriate due to the factors mentioned by Tonelli, especially because of the potential emotional and physical distress. Such paternalism, a term that was invented for children, conflicts with increasing calls for adolescent participation in complex health areas. A more robust ethical basis for our view must be developed.

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Is the Method of Modified Percutaneous Tracheostomy Without Bronchoscopic Guidance Really Simple and Safe?

To the Editor:

Since Ciaglia introduced for the first time the percutaneous dilatational tracheostomy (PDT) method using various stoma dilators, other newly developed related methods have been accepted for airway management in patients receiving long-term mechanical ventilation. Nevertheless, the techniques do not always include bronchoscopic control, and the advantages and disadvantages of the simultaneous use of bronchscopy have been extensively discussed. An important contribution to that discussion has been a recent article in CHEST (September 2004) about modified PDT without bronchoscopic guidance, which we read with great interest. The authors concluded that “modified percutaneous tracheostomy,” a technique that does not use bronchoscopic control, is a simple and safe method if performed by a trained physician who has had some practice in neck surgery. However, we only partially share the authors’ optimism.

During the last 9 years, we have performed > 800 percutaneous tracheotomies using various commercial sets, all with the use of a bronchoscope. We are convinced that a constant visual survey of the inside of the trachea during the entire duration of the PDT procedure offers important advantages. A malplacement of the tube is thereby practically excluded, and a tracheal wall injury can either be entirely avoided or recognized early and treated. We think that the originally described techniques of...