variable). Local practices for caring for critically ill patients, including ventilator management, were similar during the preimplementation and postimplementation phases of our study.

We disagree with Dr. Joffe’s comment about sample size. By definition, our study had sufficient power to demonstrate a statistically significant reduction in the risk of death in patients who were fed enterally.

At Dr. Joffe’s request, we are pleased to provide additional details about our methods and results. We selected covariates based on clinical judgment and entered all covariates into models without employing forward or backward selection procedures. We used Statview software (version 5.0.1; SAS Institute, Cary, NC) to perform all statistical analyses. Goodness-of-fit testing with the likelihood ratio and Pearson \( \chi^2 \) tests revealed that the model of enteral nutrition accounted for the outcome better than chance alone (\( p < 0.001 \)), and that the predicted likelihood of the outcome was similar to the observed likelihood (\( p = 0.24 \)). The results of the likelihood ratio and \( \chi^2 \) tests were similar for the model of mortality (\( p < 0.001 \) and \( p = 0.71 \), respectively). The correlation matrices of parameter estimates revealed no evidence of multicollinearity in models of enteral nutrition (R range, \(-0.14 \) to \( 0.23 \)) or mortality (R range, \(-0.28 \) to \( 0.12 \)). In post hoc analyses, we identified one statistically significant interaction between admission type and intervention group, such that the implementation of the nutritional support protocol had a greater effect on enteral feeding in surgical patients.

Based on this additional information, we hope that Dr. Joffe will agree that our conclusions are valid, namely, that the implementation of an evidence-based protocol for nutritional support resulted in a greater number of critically ill patients who were fed enterally and that enteral feeding was associated with improved survival.

Juliana Barr, MD
Veterans Affairs Palo Alto Health Care System
Palo Alto, CA

Michael K. Gould, MD, MS, FCCP
Stanford University School of Medicine
Stanford, CA

REFERENCES


The Economic Impact of Late Detection of COPD in General Practice

To the Editor:

Recently, Buffels et al (April 2004) demonstrated that spirometry is an effective screening tool in the detection of COPD in general practice, especially in its early stages, even in patients who underestimate and do not report any relative symptoms. The sensitivity of spirometry is raised when spirometry is conducted as a screening tool in high-risk populations, such as smokers, especially for the early detection of COPD. Our group recently conducted a similar study in primary care centers in central Greece. We examined approximately 1,000 subjects who were > 45 years of age with or without respiratory symptoms. Our first results showed that 9.6% of examined subjects received the diagnosis of COPD for the first time after responding to a specific questionnaire, and undergoing a physical examination and spirometry. This represented 42% of the total number of COPD patients. The percentage of subjects with COPD was 51.9% in the subgroup of smokers with a smoking history of > 10 pack-years.

Other important results derived from our study were as follows: (1) 35% of COPD patients who have already received a diagnosis have never undergone spirometry; (2) 40% of patients with moderate-to-severe COPD did not receive regular prescribed medication; (3) an excessive and unjustified use of home oxygen therapy, nebulizers, and inhaled corticosteroids was noticed in patients who were in the early stages of the disease; and (4) the annual per-patient cost for COPD medication is estimated at 897 euros, starting at 433 euros in patients with stage I disease, rising to 892 euros in patients with stage III disease, and 1948 euros in patients with stage IV disease. These results demonstrate not only the underdiagnosis of COPD in patients in the primary care health-care system but also their mistreatment, a fact that reflects the increases in the social and economic burden of the disease.

It is obvious that much more has to be done in order to design strategies for the improvement and motivation of prevention policies, early detection, diagnosis, and management of COPD patients at all levels of health care, especially in the provision of primary health care, in order to reduce the economic impact of COPD.

Nikolaos Z. Tzovaras, MD
Vassiliki N. Kouloumenta, MD
Konstantinos I. Gourgoulianis, MD
University of Thessaly
Larissa, Greece

Reproduction of this article is prohibited without written permission from the American College of Chest Physicians (e-mail: permissions@chestnet.org).
Correspondence to: Nikolaos Z. Tzovaras, MD, 11 Kanari Str, 41222 Larissa, Greece; e-mail: nikztov@hotmail.com

REFERENCES


To the Editor:

We thank Dr. Tzovaras and colleagues for their comments on our article about office spirometry in CHEST and would like to
respond. The authors mentioned a study conducted in Greece, and designed for case finding of COPD in general practice. I was unable to find their published data, and I would be happy to be informed about their methodology. The way in which a diagnosis of asthma was ruled out in the study population could be particularly important. It seems hazardous to reply on the results of the Greek study without knowledge of its details.

Dr. Tzovaras wrote that his study demonstrated the mistreatment of a number of COPD patients in the primary health-care system. It is obvious that the adherence to guidelines for the management of COPD can be improved, but other surveys have indicated that this is not a privilege of primary care.2 Besides, we need more evidence for the reduction of the economic impact of COPD attained by early detection of the disease. If Tzovaras and colleagues demonstrated that 40% of the patients with moderate-to-severe COPD did not receive regularly prescribed medication, the cost of future treatment could possibly exceed the economic benefit of early detection.

Johan Buffels, MD
Katholieke Universiteit Leuven
Leuven, Belgium

REFERENCES

Extracorporeal Membrane Oxygenation and Pulmonary Disease

To the Editor:

The case report by Ahmed and colleagues (July 2004)1 and the accompanying editorial by Weber2 were interesting, but the conclusions appear to be questionable. Ahmed et al1 reported a case of Wegener granulomatosis with diffuse alveolar hemorrhage in a 26-year-old woman, who was emergently intubated. Mechanical ventilation and positive end-expiratory pressure were utilized as treatment, but her condition continued to deteriorate and she sustained bilateral tension pneumothoraces. Four hours following tracheal intubation, venovenous extracorporeal membrane oxygenation (ECMO) was initiated. Ultimately treatment was successful, and the patient was discharged home on day 58.

The final paragraph of this article stated, “In conclusion, ECMO should be considered for supportive therapy in patients with DAH [diffuse alveolar hemorrhage] from ANCA [antineutrophil cytoplasmic antibody]-associated vasculitis when conventional ventilation has failed.” Perhaps this conclusion was warranted, but the article did not mention the levels of positive end-expiratory pressure and the specific modes of ventilation that were used prior to the initiation of ECMO therapy. Thus, the reader cannot be sure that there really was no response to conventional therapy.

Conversely, in the accompanying editorial, Weber2 took his conclusion too far, in our estimation. After presenting a succinct and informative history of ECMO therapy in neonatal, pediatric, and adult patients, he concluded “. . . ECMO should at least be considered for all patients with potentially reversible pulmonary failure, even if there is little or no literature support and common sense argues against its use.” Thus, he jumped from a single pulmonary disease entity that had been treated successfully with ECMO in a single patient to the potential application of ECMO in all types of pulmonary failure in all patients.

This conclusion is not warranted and seems to discard the concept of evidence-based medicine that is a mainstay of current research and teaching. Although therapeutic paradigm shifts, by definition, challenge conventional medicine, they do so based on our evolving pathophysiologic knowledge. If readers were to follow the advice of Weber, many patients who might recover using conventional therapy would be needlessly subjected to ECMO, an approach that in adults is, at the least, questionable, invasive, and unproven. Regardless of his obvious enthusiasm for ECMO, we do not believe that his approach in this setting is justified, and we wonder at the rationale behind it.

Robert R. Kirby, MD, FCCP
Emilio B. Lobato, MD
North Florida/South Georgia Veterans Health System
Gainesville, FL

REFERENCES
2 Weber TR. Extending the use of ECMO [editorial]. Chest 2004; 126:9–10