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. 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.

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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.

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To the Editor:

We would like to convince Drs. Kirby and Lobato that conventional mechanical ventilation was inadequate in our patient with Wegener granulomatosis and diffuse alveolar hemorrhage (DAH). Immediately after intubation, our patient was sedated, paralyzed, and maintained on 100% fractional expired oxygen. Positive end-expiratory pressure was titrated to 18 cm H2O, and the mode of ventilation was transitioned from pressure-regulated volume control, to pressure control with an inverse inspiratory/expiratory ratio, to high-frequency oscillatory ventilation. Prone positioning was not attempted due to hemodynamic instability, ongoing aspheresis, and the presence of multiple lines and bilateral chest tubes. The decision was made to begin extracorporeal membrane oxygenation (ECMO) because of persistent hypoxemia (saturation, < 80%) and concern for impending morbidity and mortality.

We acknowledge the lack of evidence-based literature on ECMO, particularly in regard to the patient with DAH. We also believe that the readers of CHEST have a general understanding of the concept behind evidence-based medicine and the limitations of a single case report. ECMO has been generally accepted as the “standard of care” in neonatology. This is largely based on two small trials1,2 involving a total of 51 infants, 40 of whom received ECMO. These trials employed adaptive randomization schemes because conventional randomized controlled trials were considered to be unethical. Likewise, a randomized controlled trial of ECMO for the patient with DAH who does not respond to conventional mechanical ventilation would also be unethical and is simply not feasible. If evidence-based medicine is required before using ECMO for
unconventional indications, we will be permanently handcuffed in using this potentially life-saving therapy. Furthermore, we believe that it is our ethical duty to add our single case report to the existing body of literature and would like to remind Drs. Kirby and Lobato that there has been a case series and several sporadic case reports describing the use of ECMO in both the pediatric and adult populations for respiratory failure secondary to DAH.

We think that Dr. Weber’s editorial does not advocate the indiscriminate use of ECMO in the patient with DAH and that there is an emphasis on the very real complication of fully anticoagulating a patient with preexisting hemorrhage. Fortunately, most patients with DAH can be maintained with conventional mechanical ventilation. In conclusion, we agree with Dr. Weber that “ECMO should at least be considered for all patients with potentially reversible pulmonary failure” when conventional therapy fails, “even if there is little or no literature support and common sense argues against its use.” We contend that this is the art of medicine.

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Obesity in ICU Patients

Increase or Decrease in Mortality?

To the Editor:

We read with interest the article by Goulenok et al in a recent issue CHEST (April 2004), showing that obesity, defined by a body mass index (BMI) > 27, was associated with a higher mortality rate among ICU patients than predicted by the simplified acute physiology score (SAPS) II. The authors were surprised by the fact that overweight was not taken into consideration by the SAPS II or APACHE (acute physiology and chronic health evaluation) score. Four other studies have analyzed the potential impact of obesity on ICU outcome, with conflicting results. Like Goulenok et al, two of these studies showed increased mortality in obese ICU patients. However, in one of these studies, the obese patients had a particularly high mean BMI of 51, and were compared to a group of nonobese patients (BMI < 30). The authors did not give their results for patients with BMI values between 30 and 40. Tremblay and Bandi found no difference between overweight and severely obese patients in a multi-institutional database of 41,011 ICU patients. Furthermore, Garroutte-Orges et al found a lower mortality rate in obese ICU patients (BMI > 30) than in nonobese patients. We conducted a prospective, multicenter, case-control study to evaluate the prognostic significance of BMI > 35 in ICU patients receiving mechanical ventilation for > 48 h. Our preliminary results for 69 patients with a mean BMI of 42 ± 6 and a mean (± SD) SAPS II score of 45 ± 15 show that mortality was lower than predicted by SAPS II (16% vs 37%, respectively), with a standard mortality ratio of 0.46. These results disagree with those of Goulenok et al, even though they were obtained in patients with higher BMI values and poorer clinical status (SAPS II) on ICU admission. Thus, the impact of obesity in ICU mortality remains controversial, and it seems premature to add BMI to ICU severity scores.

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