Barritt and Jordan\(^1\) was five deaths among 19 patients (26%), not 38% as shown in Table 1 in my article.\(^2\) I apologize for this error. Their findings, coupled with the other reports of the mortality of untreated PE, \(^3\)-\(^6\) would indicate that the mortality of untreated PE is approximately 30%. Given the fact that the mortality of PE in patients treated with heparin and warfarin is < 5%, \(^7\),\(^8\) it is unlikely that there will be additional randomized clinical trials comparing heparin to placebo in patients with PE, or additional reports of the mortality of untreated PE.

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Stair Climbing Test in Lung Resection Candidates With Low Predicted Postoperative FEV\(_1\)

To the Editor:

We read with interest the article of Girish and colleagues (October 2001)\(^1\) on symptom-limited stair climbing test as an instrument to predict complications after thoracic and upper-abdominal surgery; however, we think the patients' selection criteria used in their work are inappropriate and of little clinical use. The authors, in fact, excluded from lung resection those patients with a predicted postoperative FEV\(_1\) (ppoFEV\(_1\)) < 40% of predicted.

During the last 3 years, we used maximal stair climbing test on 307 patients for risk stratification before lung resection.\(^2\) Fifteen of these patients had a ppoFEV\(_1\) < 40% of predicted. Nevertheless, they were submitted to lung resection (seven lobectomies, six pneumonectomies, eight pneumonectomies) for their good performance at the stair climbing test. Two patients climbed < 12 m, whereas the others climbed > 14 m, corresponding to our setting, approximately to three and four flights of stairs, respectively. Preoperative maximal oxygen uptake (VO\(_{2\text{max}}\)) did not differ between patients with a ppoFEV\(_1\) < 40% and those with a ppoFEV\(_1\) ≥ 40% (26 mL/kg/min vs 25.9 mL/kg/min, respectively: \(p = 0.9\)). Only three patients acquired postoperative cardiopulmonary complications with no mortality, and the morbidity rate was not different from that of the patients with a ppoFEV\(_1\) ≥ 40% (20% vs 17.5%, respectively; \(p = 0.8\)). All patients with a ppoFEV\(_1\) < 40% were able to perform a postoperative exercise test before discharge, which did not show a different VO\(_{2\text{max}}\) with respect to the patients with a ppoFEV\(_1\) ≥ 40% (21.6 mL/kg/min vs 22.5 mL/kg/min, respectively; \(p = 0.4\)). We think that the stair climbing test is most useful in assessing the cardiorespiratory capacity of those patients traditionally considered at prohibitive risk for lung resection in order to minimize their improper exclusion from operation. Using this test allowed us to operate on an additional 15 patients who would have otherwise been denied surgery. Based on our results, we think that the practice of excluding patients from operation only for their low predicted postoperative pulmonary function without performing a preoperative exercise test is questionable. We currently exclude from operation only those patients with a ppoFEV\(_1\) < 30% with an altitude climbed at the stair climbing test < 12 m.

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Impact of Positive Microbiological Diagnosis on Management and Prognosis of Severe Community-Acquired Pneumonia

To the Editor:

In a recent study, Bello et al\(^1\) (January 2003) described their use of microbiological testing in 204 patients with severe community-acquired pneumonia (SCAP). Furthermore, they evaluated the impact of bacteriologic data on the management and prognosis of such patients, and compared etiologic agents according to whether patients underwent intubation or not.

The main results of this study were the following: a microbiological diagnosis of pneumonia was made in 71 intubated patients and in 46 nonintubated patients. Infections due to Legionella pneumophila and Pseudomonas aeruginosa infections were significantly more frequent in intubated than in nonintubated patients (15.1% vs 7.1% and 6.6% vs.1.0%, respectively). Positive microbiological test results led to antimicrobial treatment modifications in 55 patients; in 11 cases, modifications were justified.
because causative organism was resistant to initial empiric therapy. In 65 patients, modification was a simplification of the initial therapy. Finally, mortality rates were similar in patients with (26.4%) and without (19.5%) a positive etiologic diagnosis.

According to these results, the authors conclude that "microbiological testing is fully justified in patients with SCAP, because identifying the causative agent and adjusting treatment both impact on patient outcome." Moreover, they suggest that "intubated patients should be empirically treated for Pseudomonas and Legionella while awaiting bacteriology results." Moreover, they emphasized that "this is the first study to evaluate the impact of diagnostic testing on the outcome of SCAP."

Despite the interest of this study, we wonder if such an analysis and such results allow such conclusions. First of all, this study was a retrospective analysis of prospectively collected data. Such a study design could indicate that microbiological investigations were not exhaustive and different in the two groups of patients, intubated vs nonintubated. According to the reported microbiological investigations, samples for initial and follow-up serologic studies were collected from most patients. Protected brush cultures were performed for patients who required mechanical ventilation. In fact, in the overall population of patients, serologic studies and urine enzyme-linked immunosorbent assays for detection of antigens from \( L\) \( \text{pneumophila} \) serogroup 1 were performed in 35.7% and 31.3% of cases, respectively. Moreover, differences existed between intubated and nonintubated patients, since serologic studies were performed in 40.5% and 30.6% of cases, respectively, and urine tests in 45.2% and 16.3% of patients, respectively. Similarly, in intubated patients, bronchocopy for protected brush specimens was performed in 43.3% of cases, compared to 16.3% in nonintubated patients. These data clearly demonstrate that microbiological investigations were quite different from the reported methods, as well as nonexhaustive and nonhomogenous in the two groups. Consequently, we think that it could be wrong to assess that incidences of \( L\) \( \text{pneumophila} \) and \( P\) \( \text{aeruginosa} \) infections in intubated patients were significantly higher than in nonintubated patients. Furthermore, considering the very low frequency of \( P\) \( \text{aeruginosa} \) isolated in this study (6.6%), we strongly disagree with the authors' suggestion to systematically target this organism in the initial empirical antimicrobial therapy for intubated patients. Some details about patients exhibiting \( P\) \( \text{aeruginosa} \) infection (ie, COPD, prior antimicrobial treatment, etc.) and about antimicrobial sensitivity could have been useful to really identify risk factors for such an infection and, thus, help physicians in selecting an antimicrobial therapy including in its spectrum both \( \text{Streptococcus pneumoniae} \) and \( P\) \( \text{aeruginosa} \).

One of the most important point suggested by Rello et al\(^\text{8}\) is the usefulness of bacteriologic tests (when results are positive) to adapt and, most often, to simplify the initial empiric antimicrobial therapy. This concept of de-escalation therapy is now proposed in the management of nosocomial pneumonia,\(^2\) and we agree with the authors' suggestion to systematically target this organism in the initial empirical antimicrobial therapy for intubated patients. Some details about patients exhibiting \( P\) \( \text{aeruginosa} \) infection (ie, COPD, prior antimicrobial treatment, etc.) and about antimicrobial sensitivity could have been useful to really identify risk factors for such an infection and, thus, help physicians in selecting an antimicrobial therapy including in its spectrum both \( \text{Streptococcus pneumoniae} \) and \( P\) \( \text{aeruginosa} \).

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