were within 7.5 percent as a coefficient of variation (three to five injections). This is an accepted range and reigns as the "gold standard"; however, we should remember that it may be the best possible measurement at present, but it certainly is not the best absolute measurement. The time lag between injections is not given, and neither is the value for mixed venous saturation during this lag. If the measurements took more than 5 min, the patients' venous saturations may have changed by ten percentage points. This would not suggest a steady state—the condition that would have to be met for the Fick calculation to be accurate; the supposition is that if the metabolic rate remains constant, an increase in cardiac output should be matched by a proportionate fall in venous content. Examination of the data of Villar et al shows that the mixed venous saturations were not reported. A 10 percent drop in a venous saturation of 48 percent does not suggest a steady-state condition, especially as far as consumption is concerned.

Further, the accuracy of hemoglobin measurement is ±2 percent, as is the accuracy of saturation measurement. There is no information given on the accuracy of the hemoglobin or saturation measurements. All these factors could combine to create a range of error of ±15 percent in calculated oxygen consumption. Taken together with the large standard deviation in oxygen consumption in the septic groups, it can be seen that steady states between cardiac output measurements probably did not exist, making it difficult to accept the consumption calculation as equivalent to a measured value.

The time has come to ask for consumption measurements rather than calculations. This would have the benefit of a more accurate assessment of oxygen consumption dependency. With today's technology, the accuracy of indirect calorimetry is approximately ±5 percent over a wide range of \( \text{FiO}_2 \) values. This would uncouple the variables and reduce magnification errors in data. It should also be noted that some of the computerized hemodynamic calculations generated daily in the intensive care unit have calculated consumptions. These should be abandoned. As investigators examine oxygen consumption dependency on transport, proper methods of consumption determination are necessary before interventions are recommended.

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Use of the APACHE II System in Surgical Lung Carcinoma Patients

To the Editor:

We read with interest the September 1990 issue of *Chest* the article by Giangiuliani et al regarding the use of the APACHE II score in the assessment of surgical lung carcinoma patients. Based on our experience in the extensive use of this score in critically ill patients, we would like to make some comments on that interesting article.

The APACHE II score is designed to quantify the severity of illness at intensive care unit (ICU) admission. Like other prognostic indexes, it is accurate, specific, and sensitive in predicting outcome in groups of intensive care patients. The usefulness of APACHE II at hospital admission in outcome prediction has never been validated in large series of patients. Variables used at ICU admission have been shown to classify groups of patients and accurately predict outcome. These parameters probably are completely different from prognostic variables to assess at hospital admission of scheduled surgical patients. Such a study would require an assessment of potentially useful variables by means of discriminant analysis and building a new model with use of multiple logistic regression.

Giangiuliani et al obtained only mediocre results when comparing observed and expected outcomes using the APACHE II score and the TNM staging system for lung carcinoma. A total correct classification rate of 55.6 percent at a cutoff point of 0.7 and an area under a receiver operating characteristic curve of 0.54 is far from good performance, especially considering that toasting a coin would give an area of 0.5. Some degree of discrimination with the method proposed by Giangiuliani et al is not surprising given that the APACHE II score includes age and chronic abnormalities. In fact, high-risk patients were older (not significant in univariate analysis) and showed significantly more chronic abnormalities of the respiratory system than low-risk patients did. Probably the acute physiology scores of both groups were equivalent.

In summary, it is our opinion that APACHE II is not validated for stable patients and should not be used to categorize patients admitted for scheduled surgery. The acceptable performance of their method can be explained by different degrees of respiratory chronic disease and age between the groups. We suggest that a better analysis could have been identification of risk factors by means of logistic regression analysis. Nevertheless, we found the article by Giangiuliani et al very interesting. We think that it adds a new perspective on preoperative evaluation and is a source of future research in the field of prognostic factors in surgical lung carcinoma.

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To the Editor:

We read with interest Dr Castella's comments on our article, and we agree with him that discriminant analysis followed by linear logistic regression on the significant predictors would achieve a higher performance in the classification of specific groups of individuals, like lung carcinoma patients. However, three considerations prevent us from adopting such a scheme: First, a large patient base and a multicentric approach are necessary in our opinion to derive meaningful results; such conditions are not available in our institution. Second, we believe that an exhaustive procedure on a particular patient set would be sensitive to variations in the data and would be of limited usefulness in the prediction of outcome in new patients in the same category (unless the numbers
considered are extremely large) and would be of no use in medical audit. Third, we prefer to use a system for evaluation of severity of disease that analyzes the modifications of basic pathophysiologic patterns of response to disease, as the APACHE II system does, because it represents in our opinion the best tool for risk evaluation in all kinds of patients.

The APACHE II system was applied in our work to high-risk patients only. We agree that APACHE II is not the best system to categorize the single patient admitted for scheduled surgery, but it may be a good research tool for high-risk surgical lung carcinoma patients, who represent a heterogeneous group. Finally, we think that the best results in terms of accuracy can be achieved in large series of patients.

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Salmonella Lung Abscess in a Patient with Acquired Immunodeficiency Syndrome

To the Editor:

Salmonella bacteremia is a well-recognized complication in immunocompromised hosts, including patients with acquired immunodeficiency syndrome (AIDS). The Salmonella organism is rarely isolated from the respiratory tract either premortem or at autopsy in patients with AIDS. We report a case of Salmonella lung abscess in an AIDS patient.

A 49-year-old Haitian man was admitted in December 1988 with a three-day history of weight loss, dyspnea, and cough. On examination, he was febrile and tachypneic and had rales in the left lung. His WBC was 1,400/mm³, with CD4 of 3/mm³. The human immunodeficiency virus serologic findings were positive. The chest roentgenogram showed a left lung abscess (Fig 1).

Salmonella enteritidis (group D) was isolated from the blood, sputum, and stool. The patient had a complete resolution of the lung abscess after three weeks of intravenous ceftriaxone and chloramphenicol and was discharged on a regimen of zidovudine and trimethoprim-sulfamethoxazole prophylaxis. He was readmitted in April 1990 with fever, but the chest roentgenogram was normal and the microbiologic cultures were normal. He has been followed up for five months with no recurrence.

Salmonella lung abscess has not, to our knowledge, been previously reported in AIDS. The excellent clinical response and absence of early relapse in our patient support the low pathogenicity of Salmonella in the respiratory tract, although the effect of trimethoprim-sulfamethoxazole prophylaxis cannot be excluded.

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Lung Needle Aspiration for Diagnosis of Pneumonia

To the Editor:

I read with great interest in the October 1990 issue of Chest the excellent paper by Dr Torres and colleagues on the subject of percutaneous lung needle aspiration (PLNA) in patients with pneumonia. However, the authors' interpretation of our published data on this subject was not entirely correct. In that article, we reported a study on the diagnostic adequacy of tracheobronchial secretions for establishing the cause of nosocomial pneumonias in critically ill patients. In order to do this, we purposely studied only patients in whom a "gold standard" culture had firmly established the cause of the pulmonary infection. Thus, by definition, our study included only patients who had a positive PLNA result, since those with a negative result were automatically excluded from the final analysis. Obviously, under these circumstances, our report that 11 of 11 PLNAs were positive should not be construed to mean that the sensitivity of the procedure was 100 percent.

Torres and colleagues found a diagnostic sensitivity of 37.5 percent for PLNA in their patients. Reported sensitivities for PLNA in the context of pulmonary infections have varied widely, ranging from less than 40 percent to over 75 to 90 percent. Such variability in diagnostic yield is most likely due to such factors as differences in...