


To the Editor:

I thank Dr. Barie and Ms. Hydo for their interest in our article.1 The issues detailed in their letter are important, and I am delighted to have an opportunity to reply. I wish to respond on an individual basis to the points raised.

First, the goal of the study was to determine the predictors of outcome in patients with prolonged stay and not to determine what factors contributed to prolonged stay. The factors that predispose to postoperative morbidity and, hence, prolonged stay after cardiac surgery have been extensively studied.

Early organ dysfunction did not predict outcome in our cohort of patients. Our explanation was that such early organ dysfunction was mostly likely due to transient effects of cardiopulmonary bypass. Thus, our patient population may differ from a general surgical ICU population. Late organ failure in cardiac surgical patients is secondary to sepsis and a dominant predictor of outcome.

Alternatively, and far more likely, differences in case mix may account for the contradictions between our studies. We developed a predictive model exclusively from a cohort of patients with prolonged ICU stay, in which multiple organ failure was universal, and the mortality rate was 40%. Other studies have developed predictive models that include all patients admitted to ICU, with low overall mortality of less than 10%.

I consider this point to be central to the whole issue of outcome prediction for the ICU. The statistical power of larger studies is obtained by including ICU patients without organ dysfunction. For example, in a study by Barie and colleagues,2 only 43% had organ dysfunction. One might ask how inclusion of the 57% without organ failure in the prediction model influenced the accuracy of the model. Similarly, the APACHE system is derived from an ICU patient population with a very low mortality.

I admit to modifying the Marshall score. However, the original score is far from perfect. The cardiovascular score is particularly weak. In any good ICU, mean arterial pressure is normalized by vasopressor infusion and volume loading. Thus it may be more relevant to score the interventions necessary to maintain adequate pressure rather than the pressure itself.

Similarly, how should one score renal failure treated with CVVHD where the creatinine is now at a nearly normal level? Should scoring be based on creatinine alone or requirement for renal support?

The neurologic component of the Marshall score consists entirely of the Glasgow Coma Score. As a sizable proportion of any ICU population that has multiple organ failure is mechanically ventilated, assessing the verbal response component is problematic. We chose to ignore this component and feel justified in our approach. However, we failed to address the thorny issue of neurologic assessment in sedated patients.

We agree that a consensus is required not only on the actual scoring of individual organ failure but also on the inclusion criteria for predictive models. Should patients without organ failure be excluded from a study population on the rationale that there is little point in developing a predictive model for organ failure from patients without organ failure? It may be worth considering including only patients with at least two organs failing.

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REFERENCES


Theoretical Basis for Usefulness of Dyspnea Ratings for Prescription of Exercise in COPD Patients

To the Editor:

In a recent issue of CHEST (January 1998) Horowitz and Mulder beautifully demonstrated that exercise intensity on the treadmill using dyspnea ratings was accurately reproducible over an 8-week period in patients with COPD. I totally agree with the author's conclusion that dyspnea ratings from an incremental cycle ergometry test at an intensity of 80% of peak oxygen consumption (\(\text{VO}_2\)) are worthwhile to regulate exercise on the treadmill without systemic bias. The authors stated that "there have been no previous studies applying a physiologic-perceptual relationship (eg, \(\text{VO}_2\)-dyspnea ratings) during testing on one mode of exercise for training purposes on another mode of exercise in patients with respiratory disease." Although this crossover application has not done previously, we have already reported that the physiologic-perceptual relationship, i.e., \(\text{VO}_2\)-Borg scores, is reproducible in patients with COPD and that the indices based on \(\text{VO}_2\)-dyspnea ratings are valuable for quantitative assessment of dyspnea in COPD patients.2-4

Because the significant linear relationship between the \(\text{VO}_2\) and dyspnea ratings during an incremental cycle ergometry test is the theoretical basis for the accurate regulation of exercise intensity on the treadmill, the linear relationship between \(\text{VO}_2\)-dyspnea ratings must be the most important. However, in our experience, a linear regression analysis between \(\text{VO}_2\) and dyspnea ratings assessed as Borg score is always significant in COPD patients,2-4 though the relationship between the ratings and \(\text{VO}_2\) are not always linear in the patients. For example, the relationships are sometimes curve-linear or sigmoid-like curves. This may be in agreement with the idea that the dyspnea ratings, ie, Borg category scale, is not based on the simple linear scale, but on the psychophysics Steven's law,5 ie, perceived sensation (\(s\)) = K \cdot \text{stimuli}^p. An assessment of dyspnea ratings by using Borg scale may be valid only when the scale is standardized by proper stimuli.

It has been reported that the unstandardized Borg score is not reproducible with days in patients with COPD.6 That is why dyspnea rating is not a valid way in which to regulate exercise on the treadmill at an intensity of 50% of peak \(\text{VO}_2\), as in the current

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