Communications to the editor

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Scoring Organ Dysfunction

To the Editor:

Ryan and colleagues examined a cohort of cardiac surgical patients who stayed in the intensive care unit for at least 14 consecutive days. The goal of their evaluation was to determine the factors that contributed to such prolonged stays. Several preoperative, intraoperative, and postoperative factors were analyzed, and the defined outcome was mortality. A major component of the analysis was the use of a modified Marshall Multiple Organ Dysfunction Score to quantify organ dysfunction. The salient findings were that organ dysfunction did not predict mortality on Day 1, but did do so on Days 14 and 28. Unfortunately, the investigators did not use their data set to ascertain those factors that contributed to the prolonged stay itself, a question that has had many prediction models brought to bear. It is also regrettable that the authors made no assessment of illness severity on admission. APACHE II scoring has been quantitated in cardiac surgical patients, and such additional data would have been very useful.

Several comparisons may be made with our own evaluations of the problem of prolonged ICU stay in a general ICU population. Although we defined a prolonged stay as being >21 days based on the behavior of our cohort, the results are comparable because we used an iteration of the Marshall score as well. Even though cardiac and general surgery patients may seem inconstant at first, it is notable that 3.8% of the cardiac surgery patients had a prolonged stay compared to 4.3% of our general surgery patients and that the hospital mortality rates were also nearly identical (56.5% vs 53.0%), despite the fact that the emergency admission rate of 47.0% in our general surgery ICU patients is likely much higher than in their series of cardiac surgery patients. Our data contradict the principal finding by Ryan and colleagues that early organ dysfunction does not influence outcome. We found that a prolonged stay in the ICU is predicted by the admission APACHE III score, emergency admission, and the maximal organ dysfunction score. When daily organ dysfunction scores are considered, the magnitude of organ dysfunction distinguishes survivors from nonsurvivors on the second hospital day. Even when one considers only short ICU stays (≤10 days) in good-risk patients, the magnitude of organ dysfunction is a powerful predictor of the length of stay in the ICU and in the hospital. Even modest organ dysfunction associated with a brief ICU stay prolongs hospitalization markedly.

The difference may be that the statistical power of our studies, each including more than 2,200 ICU admissions, is much greater than that of the study by Ryan and colleagues. However, it is more likely that the difference between Day 1 and Day 2 is important and that Day 1 may be too soon to make such observations. Surgical patients have marked physiologic perturbations in the first 24 h after operation, including fluid shifts, pain, emergence from anesthesia, atelectasis, cytokine responses, and upregulation of growth factors for wound healing. These derangements, which may mimic or actually constitute the systemic inflammatory response syndrome, can have an impact on the calculation of the Marshall score through most of its components. It may take the first 24 h, as ICU resuscitation progresses, for this transitory response to subside. Cardiac surgical patients, in particular, often need transient inotropic support (3 Marshall points) to come off bypass; how the patient looks on the second day in the ICU may be what is most important.

Dr. Ryan and colleagues are to be commended for quantitating organ dysfunction, but it is disappointing and needless that they have made so many modifications to an accepted, validated score. The plethora of extant scoring systems from which to choose makes interstudy comparisons difficult, and the temptation to make special-purpose, single-use modifications must be resisted. Modifications of the Glasgow Coma Score are especially problematic because, of the six components of the Marshall score, neurologic dysfunction is consistently the most powerful independent predictor of outcome. I hope that Ryan and colleagues will join us in calling for a consensus conference on organ dysfunction scoring so that strictly comparable literature will be published henceforth.

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REFERENCES
5. Barie PS, Hydo LJ. Influence of multiple organ dysfunction

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 most 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 CVVH to 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) the Horowitz and Mulher beautifully demonstrated that exercise intensity on the treadmill using dyspnea ratings was accurately reproducible over a 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 (Vo2) 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 (e.g. Vo2-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, ie, Vo2-Borg scores, is reproducible in patients with COPD and that the indices based on Vo2-dyspnea ratings are valuable for quantitative assessment of dyspnea in COPD patients.

Because the significant linear relationship between the Vo2 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 Vo2-dyspnea ratings must be the most important. However, in our experience, a linear regression analysis between Vo2 and dyspnea ratings assessed as Borg score is always significant in COPD patents, though the relationship between the ratings and Vo2 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 psychophysic Steven’s law, ie, perceived sensation (S) = K · stimuli. 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. 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 Vo2, as in the current