# Response

## To the Editor:

We thank Drs Nathanson and Higgins for their interest in our work and recognize their contributions to the field through their development of the Mortality Probability Model (MPM) III. As they note, in our study models with a greater number of predictor variables provided better discriminatory capacity for the prediction of hospital mortality.<sup>1</sup> When deciding whether a specific predictor variable should be included in a prognostic model, consideration must be given to the effort required to reliably obtain the data for that variable. In this regard, MPM III provides good discrimination using a small number of easily ascertained variables.

Drs Nathanson and Higgins write that "this study implies that DNR [do not resuscitate] status is not an important predictor of mortality in the ICU." We believe this gives too much weight to the conclusions of our study. It has been previously documented that DNR status, by itself, is indeed a predictor of ICU and hospital mortality.<sup>2</sup> Rather, DNR status did not—within the limits of our study as we discuss in the "Strengths and Limitations" section of our article<sup>1</sup>—significantly improve the performance of recent versions of the Acute Physiology and Chronic Health Evaluation (APACHE) and Simplified Acute Physiology Score (SAPS) models.

By "performance," we refer to the commonly used and reported measures of prognostic model assessment, namely discrimination (measured by the area under the receiver operating characteristic curve) and calibration (measured by the Hosmer-Lemeshow statistic [HLS]). We agree with Drs Nathanson and Higgins' belief that both measures are imperfect—although they are widely reported and used.<sup>3,4</sup> As we discuss, calibration is especially subject to a variety of influences, including sample size and case mix. We further agree that a significant HLS does not necessarily mean that a predictive model is suspect, although a nonsignificant HLS is desirable. Calibration plots for each model showed discrepancies between observed and predicted values especially at the highest and lowest deciles of risk, consistent with poor calibration.

Recognizing the problems inherent in the assessment of model performance and the trade-off between discrimination and calibration, we also reported Brier scores for the models studied (Tables 2 and 3 of our article<sup>1</sup>). The Brier score, based on model prediction error, provides an overall estimate of performance. Our data demonstrated a progressive, albeit small, decrease in Brier score (ie, improved model performance) as model complexity increased. The addition of DNR status lowered the Brier scores for both APACHE models and for SAPS 3, suggesting its potential value as a predictor variable. As suggested, we also calculated both the Bayesian information criterion (BIC) and the corrected Akaike information criterion (AIC) for prognostic models with and without inclusion of DNR status.5 For each criterion, and similar to the Brier score analyses, the addition of DNR status was associated with a small improvement in model performance (approximately 3% decrease in AIC and BIC). The small improvements in Brier scores, BIC, and AIC associated with the addition of DNR status were not, however, reflected in statistically significant differences in area under the receiver operating characteristic curve or a consistent directional change in the value of the HLS.

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# How Much Hypoxia Is Significant in Pulmonary Hypertension During Air Travel?

### To the Editor:

In their interesting study published in CHEST (October 2012), Roubinian and colleagues<sup>1</sup> reported a high incidence (26%) of hypoxemia in air passengers with pulmonary hypertension. This was based on the study's definition of a "meaningful" oxygen desaturation as a mean arterial oxygen saturation ( $\text{Spo}_2$ ) < 85%. The study was predicated on the potential for in-flight hypoxemia to cause adverse effects through hypoxic pulmonary vasoconstriction and further elevation in pulmonary artery pressures. Since this work was conducted, we have studied changes in systolic pulmonary artery pressure (SPAP) during commercial flights using echocardiography, and our results suggest that a higher  $\text{Spo}_2$ threshold may be more appropriate.

In a study of healthy passengers during a 9-h flight, we found that mean  $\text{Spo}_2$  fell to 95% and SPAP increased by 6 mm Hg or about 20%.<sup>2</sup> In a passenger with a genetic cause of increased hypoxic pulmonary vasoreactivity (Chuvash polycythemia) studied during a 6-h flight,  $\text{Spo}_2$  only fell to 96%, yet SPAP increased by 15 mm Hg, or about 50%.<sup>3</sup> Interestingly, Roubinian and colleagues<sup>1</sup> reported that 24% of the patients studied experienced symptoms without developing hypoxemia (defined as  $\text{Spo}_2 < 85\%$ ). Although our echocardiographic findings have limitations, they nevertheless suggest that milder hypoxia could still have contributed to these

clinical sequelae and support a higher threshold for "meaningful" desaturation such as  $\text{Spo}_2 < 90\%$ .

It would be useful to know whether milder desaturation to this level can predict the development of symptoms during flight, and consideration could be given to reanalyzing the study's results to find out. This may reinforce and broaden the authors' conclusions—perhaps clinically significant hypoxemia is even more common than reported, and evaluation for supplementary in-flight oxygen should be even more widely advocated.

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## Response

### To the Editor:

We thank Dr Smith for his thoughtful comments regarding our recent publication in *CHEST* (October 2012).<sup>1</sup> We took great interest in his recently published findings demonstrating an increase in estimated pulmonary artery pressures in healthy air passengers without hypoxemia.<sup>2</sup>

In our study, we used an a priori-defined oxygen desaturation value of pulse oxygen saturation  $(\text{Spo}_2) < 85\%$  (corresponding to partial pressure of oxygen in the blood of approximately 50 mm Hg at sea level). We chose this value to maintain a high specificity for oxygen desaturation events, consistent with this being the recommended threshold for prescribing in-flight supplemental oxygen in individuals, based on altitude simulation testing.<sup>3</sup>

We agree that a more sensitive threshold for oxygen desaturation may correlate better with flight symptoms and/or pulmonary vasoconstriction events. Using our original threshold of Spo<sub>2</sub> < 85%, we did not find a significant association between desaturation events and flight symptoms (P = .25). However, we did find that the oxygen saturation nadir was lower in individuals with flight symptoms compared with those without symptoms (Spo<sub>2</sub>, 85% vs 89%; P = .03). Notably, even when using this more restrictive cutoff for Spo<sub>2</sub> of 85%, there were four subjects who remained asymptomatic despite desaturation events.

Using a more sensitive threshold of oxygen desaturation  $(Spo_2 < 88\%)$ , we found a significant association with flight symp-

toms (P < .01) and a similar number of individuals with asymptomatic oxygen desaturation. When the threshold was further increased (Spo<sub>2</sub> < 90%), we saw an even stronger association with flight symptoms, but there was also a substantial increase in the number of individuals with asymptomatic oxygen desaturation by this definition. Therefore, a cutpoint of Spo<sub>2</sub> < 88% appeared to have the best performance characteristics with respect to reported symptoms.

Dr Smith's findings, taken together with our study results, imply that even under mildly hypoxic conditions, individuals with underlying pulmonary vascular disease may experience symptoms as a result of elevations in pulmonary artery pressure.<sup>1,4</sup> Further studies are needed to better understand the relationship between changes in pulmonary artery pressures, symptoms, and desaturation events during flight, and their implications with regard to air travel safety for patients with pulmonary vascular disease.

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# Chest Ultrasonography as a Replacement for Chest Radiography for Community-Acquired Pneumonia

### To the Editor:

The recently published prospective study by Reissig et al<sup>1</sup> in *CHEST* (October 2012) has suggested the use of lung ultrasound (LUS) for diagnosis and follow-up of community-acquired pneumonia. This is an innovative study with achievement of very good specificity and sensitivity by highly trained sonographers. Clearly, there are advantages to the use of LUS over chest radiography