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Influence of ventilatory strategy on the PRESERVE mortality risk score

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Dear Editor,

We read with interest the article by Schmidt and colleagues on the creation of a scoring system (PRESERVE) that may assist in the prognostication of patient survival in acute respiratory distress syndrome (ARDS) requiring extracorporeal membrane oxygenation (ECMO). The study also reports on detailed long-term outcome data in this patient group [1]. We are interested in the mechanistic explanation of the components of the PRESERVE score and, in particular, the influence of the ventilatory strategy in the creation of the score. Specifically, the authors state that initiation of ECMO was predicated on criteria used in the 'ECMO for severe acute respiratory distress syndrome (EOLIA)' trial [2] but subsequently they report that PEEP optimisation adhered to the ARDSNet protocol [3]. The precise ventilatory protocol in the study is relevant to the applicability and validity of the score. Indeed, if the ARDSNet protocol was followed one would predict that, given that FiO_2 is 1.0, very few (or no) patients would receive a PEEP of less than 10 cmH₂O. Alternatively, if the EOLIA protocol was followed, PEEP adjustment would have been in accordance with the EXPRESS trial [4]. In this case a PEEP of less than 10 cmH₂O is more likely in patients

with the lowest lung compliance. The choice of protocol might explain why non-survivors, or patients with more deranged lung mechanics, received lower PEEP and higher driving pressures and why a PEEP level of less than 10 cmH₂O was an independent factor associated with death. This observation may be relevant to the general applicability of the score if other ventilator protocols are used. In these cases, perhaps alternative criteria (e.g. elastance) could be considered in place of the PEEP criterion as they would represent similar physiological phenomena.

A second observation pertains to the apparently protective role of body mass index (BMI). The authors rightly argue that higher BMI leads to higher pleural pressure, which in turn would lead to a lower transpulmonary pressure for the same plateau pressure. However, we wonder whether the same argument should also be applied to the PEEP criterion. In other words, if obesity 'protects' by reducing the transpulmonary pressure for the same plateau (effectively reducing the score assigned for the plateau pressure) it would also lower the effective PEEP value (thereby increasing the PEEP score). This could potentially have a neutral overall effect unless plateau pressure has a greater impact on mortality compared to PEEP despite receiving the same score.

We would also like to ask the authors to comment on whether the score performed equally well if H_1N_1 status was subject to a sensitivity analysis. This could test the validity of the score during pandemic and non-pandemic periods. Finally, SAPS II rather than SOFA score was independently associated with death but SOFA score was included on feasibility grounds. Would the authors advocate an alternative severity score which combines simplicity of use with high discriminatory power? We agree with the authors that, while they present very important data, the PRESERVE score

should be further tested and prospectively validated in all ARDS patients considered for ECMO.

Conflicts of interest The authors have no personal financial interests to disclose.

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