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DOES DRIVING PRESSURE MATTER IN PEDIATRIC ARDS? – THE AUTHORS REPLY

Nadir Yehya, MD¹ and Neal J. Thomas, MD, MSc²

¹Children's Hospital of Philadelphia and University of Pennsylvania, Department of Anesthesiology and Critical Care Medicine, Suite 7C-26, 34th Street and Civic Center Boulevard, Philadelphia, PA 19104

²Penn State Hershey Children's Hospital, Department of Pediatrics and Public Health Science, Division of Pediatric Critical Care Medicine, 500 University Drive, Hershey, PA 17033

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To the editor

We appreciate the interest by Drs. Inata and Takeuchi (1) regarding our manuscript (2) detailing the associations of oxygenation and driving pressure (P) with mortality in pediatric acute respiratory distress syndrome (ARDS). They suggest potential reasons why we did not find an association between P and mortality, including occult auto-PEEP decreasing P and patient effort affecting accuracy of pressure measurements. We believe neither concern was a problem. As stated in our manuscript: “ventilator pressures were recorded during periods of passive breathing under deep sedation or neuromuscular blockade by data collectors (respiratory therapists) trained to identify spontaneous effort.” Our therapists readily can identify incomplete exhalation characteristic of the kind of auto-PEEP suggested above. This was not observed in this study, which is expected in a cohort characterized by lungs with reduced compliance, rather than expiratory flow limitations. Occult dyssynchrony and patient effort is certainly still possible, as full exclusion requires close examination of multiple flow curves.

Drs. Inata and Takeuchi raise the broader issue of transpulmonary pressures as a more accurate metric to calculate P . We do not use esophageal manometers, and pleural pressures were not available to us. Despite intuitive appeal, it is not clear that monitoring transpulmonary (rather than airway) pressures offers benefits in pediatrics. Furthermore,

Corresponding Author and Address for Reprints: Nadir Yehya, MD, CHOP, Department of Anesthesiology and Critical Care Medicine, Suite 7C-26, 34th Street and Civic Center Boulevard, Philadelphia, PA 19104, yehyan@email.chop.edu.

Institution: Children's Hospital of Philadelphia

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existing reports demonstrating the association between $\dot{V}_E$ and outcome in adults did not utilize transpulmonary pressure (3, 4).

Next, there is the possibility of endotracheal tube leak contributing to inaccurate measurements. We did not assess leak. However, in a previous study on this same cohort utilizing end-tidal CO_2 measurements (5), we were required to determine the accuracy of our measurements by assessing leak. In that study, representing the first 283 children of this cohort, all patients had leak < 15% (median, 0% [0–1%]), with 95% of patients having a leak $\leq$ 5%. This is consistent with our unit practice, which does not use uncuffed tubes.

They suggest that differences in children (6) make $\dot{V}_E$ an unreliable surrogate for stress and strain. We completely agree with this, and encourage further research *specifically* testing which variables are appropriate markers of excess stress/strain in pediatric ARDS, rather than a reliance on adult paradigms.

Drs. Inata and Takeuchi (1) suggest we should limit pressures in pediatric ARDS because of 1) possible ventilator-induced lung injury (VILI), 2) difficulty in identifying a threshold for stress/strain in children, and 3) potential association with long-term damage. We offer an alternative perspective. We suggest that limits of driving and peak pressures in pediatrics are unknown, but should be determined. Second, we posit that arbitrary pressure restrictions based on adult paradigms may be potentially harmful by unnecessarily prolonging ventilator duration. Third, as there are no large studies of pulmonary function in survivors of pediatric ARDS, it is not clear which variables (if any) impact these outcomes. Mechanical ventilation is not benign, and we must determine specifically in children how best to adjust the ventilator to balance the risks of potential lung damage with improved oxygenation and a shorter ventilator course.

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