

Noninvasive ventilation in acute respiratory distress syndrome: A long way ahead

Sir,

We thank Rao *et al.* for their observation on our study^[1] regarding the use of noninvasive ventilation (NIV) in mild-to-moderate acute respiratory distress syndrome (ARDS).^[2] The response from Rao and Munta presents a very gloomy picture for the role of NIV in ARDS, which is not true.^[2] The judicious use of NIV has been shown to prevent endotracheal intubation in 52% of the cases of ARDS.^[3] The mortality rate in studies involving the use of NIV in ARDS is variable, depending on the patient profile.^[3] While it was 32% in the study (Thille *et al.*)^[4] quoted by Rao *et al.*, it has been reported to be as high as 71%.^[5] The authors have raised a concern regarding a delay in intubation being the primary reason for high mortality. They further suggest that earlier intubation could have avoided the deaths in our study.^[1] The median (interquartile range) time to intubation was 3 (1–4) h in our study that is far earlier than that reported in an international multicenter study on the use of NIV in ARDS.^[6] Further, without a control group of invasive ventilation, one cannot conclude from our study that the use of NIV or delay in intubation led to an increase in mortality. In addition, Rao *et al.* quote a retrospective study (a study design fraught with many limitations) describing the use of NIV in ARDS following esophagectomy (postsurgical patients),^[7] the patient profile being entirely different from that of our study (predominantly sepsis). Interestingly, Rao *et al.* claim an astonishingly low mortality rate of ARDS in

their Intensive Care Unit (ICU) using pressure control ventilation (a strategy yet to establish its role in ARDS).^[2] Only two strategies have shown a reduction in mortality in patients with ARDS, namely the low tidal volume strategy using volume control ventilation and prone ventilation.^[8,9] The mortality even at best centers has been reported to be around 32%.^[8] Further, the reference by the authors where they suggest a low mortality in their ICU seems to be a review article and not an original article subjected to the rigors of a thorough peer review.^[10] It would be interesting to see if Rao *et al.* could replicate their observation in an original article.

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Conflicts of interest

There are no conflicts of interest.

**Inderpaul Singh Sehgal, Sahajal Dhooria,
Ritesh Agarwal, Dhruva Chaudhry¹**

Department of Pulmonary Medicine,
Postgraduate Institute of Medical Education and Research,
Chandigarh, ¹Department of Pulmonary and Critical Care Medicine,
Postgraduate Institute of Medical Sciences,
University of Health Sciences, Rohtak, Haryana, India

Correspondence:

Dr. Inderpaul Singh Sehgal,
Department of Pulmonary Medicine,
Postgraduate Institute of Medical Education and Research,
Sector-12, Chandigarh - 160 012, India.
E-mail: inderpgi@outlook.com

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