Lung Protective Ventilation in the Intraoperative Period

To the Editor:

We read with interest the article titled "A Description of Intraoperative Ventilator Management in Patients with Acute Lung Injury and the Use of Lung Protective Ventilation Strategies."[1] It was also discussed in detail during our journal club as part of the departmental training program. The concept and the idea behind the article are very interesting and thought-provoking and we congratulate the authors on the same.

We have a few observations, however. First, current information systems being used in hospitals are programmed to a large extent not to accept values of parameters that are invalid. It was noted that a large number of patients were excluded (320 + 265 + 31) because of invalid tidal volume, height, and weight. A total 616 of 2,652 patients, approximately 23.23% of cases, were excluded. This reflects poorly on the reliability of the database and of the entries considered as valid and included in the study.

Second, in the materials and methods section all patients undergoing surgery who had at least one preoperative arterial blood gas assessment were included in the study. There exists a possibility that there were some patients with hypoxia who had not undergone preoperative arterial blood gas assessment and so were not included in the study. This situation also would have an effect on the final analysis.

Third and most important, the mode of ventilation used intraoperatively has not been mentioned. Because only peak inspiratory pressures were monitored in the "post lung protective ventilation strategy era" it is evident that pressure-controlled mode of ventilation may have been used. As a result, the findings of the ARDSnet study cannot be applied because that was carried out exclusively using volume control ventilation.[2] This has also been discussed by Slutsky *et al.*[3] However, if volume-controlled ventilation was used, the plateau pressures (Pplat) would have been lower than the peak inspiratory pressures. Whether the trend of peak inspiratory pressures would have accurately reflected the trend of Pplat as is the authors' contention is a moot point. Moreover, if that was the case, Pplat would not have been universally used as a surrogate of pressures at the level of the alveolus and peak inspiratory pressures, which is actually an indicator of airway resistance would have sufficed.

Thus, it may not be appropriate for the authors to conclude that lung protective ventilation strategy was not used intraoperatively in patients with acute lung injury in their hospital. Lung protective ventilation strategy by definition is use of low tidal volumes in volume-controlled mode of ventilation targeting a tidal volume of 6 ml/kg predicted body weight and plateau pressures of 25 cm of water.

Fourth, the authors have concluded that the tidal volume settings appear to mirror the ventilator settings provided to the patients in the intensive care unit, which is borne out by the fact that table 5 shows that the tidal volume being delivered in the preoperative setting in the intensive care unit to the patients with acute lung injury is 8.25 ml/kg predicted body weight, compared with 8.58 ml/kg predicted body weight in the intraoperative period. Is it, therefore, to be inferred that even in the intensive care unit these patients were not being ventilated with a lung protective ventilation strategy using tidal volumes of 6 ml/kg predicted body weight?

Finally, there are a number of typographical errors where PaO_2 has been repeatedly substituted by $PaCO_2$ in the abstract. In table 1 and figure 1, P/F has been mentioned as PaOC/FiO2. In tables 3, 4, and 5, Fio2 has not been mentioned as a fraction but probably as the percentage of inspired oxygen.

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2. Ventilation with lower tidal volumes as compared with traditional tidal volumes for acute lung injury and the acute respiratory distress syndrome. The Acute Respiratory Distress Syndrome Network. N Engl J Med 2000; 342:1301–8
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In Reply:

We would like to thank Dr. Singh and colleagues for their letter regarding our manuscript.[1] We believe their concerns are justified and have the following responses.

First, with regard to the number of patients excluded, we attempted to exclude all patients who had data that was not appropriate. Because our anesthesia information system is designed to provide care for patients of all ages, heights, and weights, the values allowed for multiple variables are quite robust, which does allow for potential errors in data entry. Cases were excluded based on extremes of recorded height and weight. The 320 cases that were excluded because of excessively low tidal volume were either completed using an intensive care unit ventilator (which did not automatically port data to the anesthesia information system) or had erroneous electronic data collection,

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which results in tidal volumes being recorded in very low increments of (less than 100 ml).

Patients were included in the study if they had clear preoperative arterial blood gas data that the treating anesthesiologist would have used in planning clinical care. As Singh *et al.* point out, it is possible that not all hypoxic patients were identified preoperatively; however, the focus of the study was to evaluate anesthesiologist behavior in cases in which the presence of acute lung injury (ALI) could be considered. Although surrogate measures such as Spo_2/FiO_2 could have increased the size of the study group, inclusion of these patients would have introduced bias, as to be included in the comparator ALI database, patients must have met formal American-European Consensus Conference criteria, including an arterial blood gas with a PaO_2/FiO_2 ratio less than 300.[2]

Regarding mode of ventilation, for a significant portion of the study, exclusively volume-controlled anesthesia machines (Narkomed 2B; Dräger Medical, Inc., Telford, PA) were used. In 2007, anesthesia machines with a pressure-controlled mode became available (Aisys; General Electric Healthcare, Madison, WI). Unfortunately, the anesthesia information system does not record the mode of ventilation, and we agree that this would better illustrate the care provided to patients. As we described in the manuscript,[1] the use of plateau pressures would be preferable to peak inspiratory pressures, but again, these data are not collected by the anesthesia information system. We believe peak inspiratory pressure is a reasonable surrogate because it has been used by several authors in the anesthesia literature to investigate ventilator-induced lung injury and postoperative ALI.[3–5] Regardless, we believe that our study documents that intraoperative tidal volumes used in patients with known preoperative hypoxia and ALI are not consistent with lung-protective ventilation strategy settings and that this is an important consideration for anesthesiologists.

Following the landmark ARDSNet publication in 2000,[6] the gradual adoption of lung-protective ventilation strategies in the intensive care unit has been reported in several studies.[7–10] Of particular concern in our study is the finding that patients who were receiving low Vt ventilation in the intensive care unit typically had their Vt increased in the operating room to 7.5 ml/kg predicted body weight, again reinforcing the need for anesthesiologists to consider ALI management as part of their operative care.

Finally, we sincerely regret the typographical errors in the manuscript and appreciate the readers allowing us the opportunity to clarify these discrepancies. The abstract indeed and figure 1 legend should read "PaO_2" rather than "$PaCO_2$," and in figure 1 and table 1 "PaOC" should read "PaO_2," and FIO_2 is expressed as a percentage.

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