This progression of new knowledge and approaches to patient safety, coupled with the preexisting organizational advocacy for quality of care described, in part, by Dr. Brown, provided the basis for the specialty to be able to respond to the swell in public interest in anesthesia patient safety that arose from the 1982 ABC television network's *20/20* production "The Deep Sleep: 6,000 Will Die or Suffer Brain Damage"[9] and to a concomitant growing medical malpractice insurance crisis for anesthesiologists in the United States. It was these unique challenges, in our opinion, that led to a sharp demarcation in 1982 between the previous steady but slowly progressive efforts to improve quality of care and the new tsunami of interest in rapidly developing and implementing a distinct anesthesia patient safety movement. Therefore, it is this period starting in 1982 that we designated for the purposes of our article as the start of the anesthesia patient safety movement.

Competing Interests

The authors declare no competing interests.

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Pressure Support Ventilation and Atelectasis: Comment

To the Editor:

We read with great interest the article by Jeong *et al.*[1] titled "Pressure Support *versus* Spontaneous Ventilation during Anesthetic Emergence—Effect on Postoperative Atelectasis: A Randomized Controlled Trial." Although many studies have looked at the potential effects of various intraoperative open lung ventilation strategies on postoperative pulmonary outcomes, recent evidence suggests that their potential benefits may be limited if no action is taken to minimize lung derecruitment during the emergence period.[2] Considering that postoperative atelectasis plays a central role in the development of postoperative pulmonary complications, and that maintaining positive pressure during emergence may help preserve lung aeration,[3] the research question of Jeong *et al.* is of paramount importance. However, we have some concerns regarding key aspects of the study's methodology.

First, we were especially worried about elements used to define and measure the incidence of atelectasis, the study's primary outcome. The authors' definition (more than three lung sections with a non-zero atelectasis score) is not standard[4] and has not been previously validated. Can the authors specify whether their definition was selected before conducting the study to reassure readers on the absence of data-driven threshold selection? Performing sensitivity analyses looking at different thresholds for the number of atelectatic lung sections necessary to classify the outcome would better assess the robustness of their findings.

Second, we were puzzled to read that Jeong *et al.* not only used a modified and unvalidated echographic pulmonary aeration loss score[5] but also introduced their own modifications, potentially further weakening the validity of their primary outcome classification. In particular, loss of lung sliding with lung pulse is not a sign of atelectasis but rather a sign of a well-aerated lung without ventilation. This finding could have indicated the presence of a mucous plug which may have been resolved after a simple coughing fit without causing any atelectasis. Including this sign in their atelectasis score seems problematic. We encourage the authors to use the lung ultrasound score, a validated echographic loss of aeration score, to report their results.[6]

Third, their study was underpowered for their anticipated effect size. Using the same assumptions (an incidence of 53% in the control group and 37% in the intervention group for an absolute estimated effect of 16%), we calculated that a sample size of 302 patients would have been necessary even before considering a 15% dropout rate. Their greater-than-anticipated observed effect explains why their results achieved statistical significance. However, underpowered studies are prone to inflated results with positive results that are more likely to be false positives.[7]

Fourth, the authors' definition of hypoxemia, a secondary outcome, may lead to missing important clinical effects resulting from their intervention. A punctual event of oxygen saturation measured by pulse oximetry greater than 92% may be not be clinically significant in comparison with a prolonged postoperative need for high fractional inspired oxygen tension. Can the authors provide data on this secondary outcome using a time-weighted need for organ support, such as oxygen-free days or cumulative postoperative oxygen administration?

The imaging study by Jeong *et al.* is an essential first step in clarifying the role of assisted ventilatory modes during anesthesia emergence. However, there is still a lot of work to be done to answer the salient question: Are assisted ventilatory modes an important part of an open lung strategy at emergence that may lead to a decreased incidence of postoperative pulmonary complications?

Competing Interests

Dr. Girard is a paid consultant for the point-of-care ultrasonography group of GE Healthcare (Milwaukee, Wisconsin). The other authors declare no competing interests.

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Pressure Support Ventilation and Atelectasis: Reply

In Reply:

We very much thank Zaouter *et al.* for their interest[1] in our research, "Pressure Support *versus* Spontaneous Ventilation during Anesthetic Emergence—Effect on Postoperative Atelectasis: A Randomized Controlled Trial."[2]

The main questions of Zaouter *et al.* were why we defined atelectasis only when there were signs of atelectasis in three or more lung sections and why we used a modified scoring system to evaluate atelectasis severity. For the first question, as the authors noted, there is no established definition of atelectasis diagnosed by ultrasonography. We thought that at least 25% (3 of 12 sections) of lung areas should show signs of atelectasis to be a clinically significant atelectasis because almost all patients showed an atelectasis sign in at least one lung section. We admit that 25% of lung sections is arbitrary, but this threshold was determined before conducting the study.

For their second question, we thought that an atelectasis scoring system focused on anesthesia-induced atelectasis was needed because many protocols were developed for

intensive care unit patients in previous studies.[3–6] However, our protocol is still based on the protocols that are widely used.[6,7] Anesthesia-induced atelectasis did not show definite B lines, which were used in the previous scoring system (B lines: hyperechoic vertical lines starting from the pleural line with the length of 8 cm or longer). Rather, anesthesia-induced atelectasis showed subpleural consolidations with short vertical lines starting from the margin of consolidation (pseudo B lines).[7] Accordingly, loss of A line with multiple subpleural consolidations has been reported as a more common and helpful finding to diagnose anesthesia-induced atelectasis.[7] In consideration of the development process of anesthesia-induced atelectasis, the grade 3 atelectasis, which is "loss of lung sliding and appearance of lung pulse," was added to our grading system. We found that the collapse of small bronchioles and alveoli leads to "loss of lung sliding and appearance of lung pulse" as subpleural consolidation progresses to a larger parenchymal consolidation.[8] This was also reported in previous studies.[8] Although we modified the scoring system for a more accurate diagnosis of anesthesia-induced atelectasis, it was not validated. We described this in the limitations to our study.

For the third question (sample size), we found that the power of our study did not meet the expectations and needed a larger number of patients. However, we understand that the probability of type II error (false negative) would have decreased as the sample size (power) increased, but the type I error (false positive) usually remains the same.[9] Therefore, we think our positive results would have been confirmed with more power if the sample size had increased.

We agree with Zaouter *et al.* that oxygen-free days or cumulative postoperative oxygen administration may be more important than the incidence of hypoxia as a secondary outcome. However, most patients received oxygen administration only on the night of surgery, and there was no difference in postoperative complications such as pneumonia and hospital stay between the two groups. So, we cautiously speculate that the time-weighted need for oxygen support would not have been different between the two groups.

Competing Interests

The authors declare no competing interests.

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Targeting Depth of Anesthesia to Prevent Delirium: Comment

To the Editor:

Brown *et al.*[1] nicely described their work comparing spinal anesthesia with targeted sedation based on Bispectral Index values compared with general anesthesia