

Xue *et al.* had a particular question about the definition of a failed direct laryngoscopy attempt. They are correct that we cannot confirm that the initial direct laryngoscopy attempt was optimized through patient positioning or laryngeal manipulation. However, all intubations were supervised or performed by anesthesiologists with sufficient experience. Furthermore, we did describe alternation of direct laryngoscopy blade types (see table 4 of our article¹). We agree that often an inadequate laryngeal view with direct laryngoscopy can be overcome with optimization maneuvers or when utilizing a gum-elastic bougie. These cases were *a priori* excluded from analysis as we were interested in the mechanisms of rescue after direct laryngoscopy has failed by whatever means. We cannot determine why direct laryngoscopy was abandoned after one attempt and/or if tube placement was actually attempted along with that failed direct laryngoscopy. Certainly, the providers who performed direct laryngoscopy first aimed to intubate the patient but simply could not, even though such appropriate adjuncts were available and/or used. So, we did not describe *failed intubation via* direct laryngoscopy *per se*, but we do believe we appropriately described *failed direct laryngoscopy*.

Maslow and Panaro had some questions about the validity of the data set that we believe represent a misunderstanding that should be clarified. They question the high exclusion rate from the primary query. The automated query identified 7,259 cases that involved multiple laryngoscopy attempts and notations of device(s) of interest in an effort to “screen” the electronic record for potential cases as only the narrative could describe the actual sequence of events. These were not necessarily failed direct laryngoscopy attempts but a trigger to further evaluate the record. The final analysis included 1,427 failed direct laryngoscopy cases from 346,861 intubation records (0.4%). Also, our data do not address the primary success rate of either direct laryngoscopy or video laryngoscopy. The data set only speaks to the success rate of various techniques after direct laryngoscopy has failed. So, the primary success rate of video laryngoscopy is not reported. However, we did publish such findings in a different study and observed a 98% success rate with video laryngoscopy as the primary technique despite early clinical experience with the device.⁵

Competing Interests

The authors declare no competing interests.

Michael F. Aziz, M.D., David W. Healy, M.D., M.R.C.P., F.R.C.A., Ansgar M. Brambrink, M.D., Ph.D., Sachin Kheterpal, M.D., M.B.A. Oregon Health and Science University, Portland, Oregon (M.F.A.). azizm@ohsu.edu

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Calculating Ideal Body Weight: Keep It Simple

To the Editor:

We read with much interest the editorial on protective ventilation by Hedenstierna and Edmark in the December issue of *ANESTHESIOLOGY*.¹ We agree with most of the ideas put forward. However, as thoracic anesthesiologists, we strongly believe in the importance, during one-lung ventilation, of low tidal volume based on ideal body weight.^{2,3}

Many authors still recommend using the gender-specific Acute Respiratory Distress Syndrome Network (ARDSnet) formulas to calculate ideal body weight.⁴ Ideal body weight is computed in men as $50 + (0.91 \times [\text{height in centimeters} - 152.4])$ and in women as $45.5 + (0.91 \times [\text{height in centimeters} - 152.4])$. A simple alternative would be to compute ideal body weight as the weight corresponding to an ideal body mass index of 22 kg/m^2 . Ideal body weight is then simply calculated as $22 \times ([\text{the actual patient's height in meters}]^2)$ or by using body mass index charts available on our anesthesia cart.⁵ We chose 22 kg/m^2 as the ideal body mass index after comparing the ideal body weight corresponding to body mass indices ranging from 20 to 25 to ideal body weight calculated from ARDSnet formulas. For example, a 1.75-m man would have an ideal body weight of 67 kg ($22 \times [1.75^2]$) compared to 71 kg if using ARDSnet; a 1.60-m woman would have an ideal body weight of 56 kg ($22 \times [1.60^2]$) compared to 52 kg if using ARDSnet.

The method we propose is simple and easy to remember. The same computation applies for both men and women and involves simple arithmetic.

Competing Interests

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Olivier Moreault, M.D., Yves Lacasse, M.D., F.R.C.P.C., Jean S. Bussi eres, M.D., F.R.C.P.C. Institut universitaire de cardiologie et de pneumologie de Qu ebec - Universit e Laval, Qu ebec City, Qu ebec, Canada (J.S.B.). jbuss@criucpq.ulaval.ca

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In Reply:

We appreciate the important comment by Moreault *et al.* on our article, “Protective Ventilation during Anesthesia: Is It Meaningful?”¹ We agree fully with the opinion that a low tidal volume should be based on ideal body weight to avoid harmful stress and strain to the lungs during anesthesia. This is even more important during one-lung ventilation. Ideally, the tidal volume should be adjusted to the size of the ventilated lung, but without a simple recording of lung volume, ideal body weight is a reasonable alternative. However, we also believe that an appropriate positive end-expiratory pressure is a prerequisite when using a low tidal volume, whatever the calculation method of ideal body weight. We find the method proposed by the authors commendable and indeed easy to remember as most anesthesiologists already are familiar with the method for calculating body mass index.

Competing Interests

The authors declare no competing interests.

G oran Hedenstierna, M.D., Ph.D., Lennart Edmark, M.D., Ph.D. Uppsala University, Sweden (G.H.). goran.hedenstierna@medsci.uu.se

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Evaluation of Nitrous Oxide in the Gas Mixture for Anesthesia II (ENIGMA II) Revisited: Patients Still Vomiting

To the Editor:

We read the secondary analysis of the Evaluation of Nitrous Oxide in the Gas Mixture for Anesthesia II (ENIGMA II) trial for severe postoperative nausea and vomiting (PONV) with great interest.¹ Because PONV remains an often-cited risk in using nitrous oxide,² the investigation of methods to mitigate PONV using existing data generated from randomized controlled trials is an important undertaking. We wish to respond to this thorough reanalysis.

The authors used a retrospective propensity score approach to investigate the effects of antiemetic prophylaxis on the nitrous oxide and non-nitrous oxide arms. The well-recognized limitations of this approach were openly acknowledged in the publication, including the inability to control for hidden covariates and the need to truncate available data.³ In the abstract, the authors conclude that the emetogenic effects of nitrous oxide are near eliminated by the addition of antiemetics. However, the results from the propensity score-matched analysis do not seem to support this conclusion, as the nitrous/antiemetic group had statistically higher odds of PONV compared with the non-nitrous/nonantiemetic group. In addition, administration of antiemetic prophylaxis among participants who did not receive nitrous oxide counterintuitively increased the odds of PONV. Although various clinical and scientific reasons may be hypothesized to explain this phenomenon, perhaps the simplest hypothesis is the presence of hidden covariates. Therefore, it is our opinion that the conclusion of negating PONV with antiemetics when nitrous is used is not supported by the results of this retrospective analysis, and the use of propensity score matching in this instance may not have resulted in a balanced comparison.

In light of the aforementioned results, another statistic (risk ratio, 0.74 [95% CI, 0.63 to 0.84]; $P < 0.001$) is quoted in the report¹ to support the conclusion that PONV is not increased when antiemetics are used in conjunction with nitrous oxide. This risk ratio does not appear among the results generated by propensity score matching but appears to be the result of a subgroup analysis for the PONV outcome in the original ENIGMA II report for patients who received antiemetic prophylaxis.⁴ However, the lack of blinding of attending anesthesiologists to treatment allocation may have introduced selection bias into antiemetic prophylaxis, a possibility supported by the statistically significant difference in antiemetic administration between the nitrous and non-nitrous arms. If selection bias were present in antiemetic administration, the efficacy of this originally randomized subgroup analysis to equalize hidden covariates may have been compromised.⁵

Although this secondary analysis¹ of antiemetic prophylaxis on PONV has important limitations, we believe that