

with the use of end-expired gas monitoring and bispectral index.²⁻⁴ In the first study, non-age-adjusted minimal alveolar concentration (MAC) was used (n = 2,000),² while in the last two studies, age-adjusted MAC was used (n = 6,041 and 21,601).^{3,4} In this study, Willingham *et al.* converted age-adjusted MAC to non-age-adjusted MAC, and they defined low MAC as less than 0.8. The authors do not give a rationale for doing so, and we think that using non-age-adjusted MAC values may have influenced their conclusions.

Indeed, it is generally accepted that MAC is age dependent: MAC is lower in elderly patients.⁵⁻⁷ Therefore, using non-age-adjusted MAC values underestimates the depth of anesthesia in their elderly patients; thus, these patients may have been overdosed; this is not uncommon even today.⁸ For example, in an 80-yr-old patient, an end-tidal sevoflurane concentration of 1.6% would be 0.8 non-age-adjusted MAC, but this would be about 1.23 age-adjusted MAC,⁷ which is simply not a “low MAC.” We are curious whether the conclusions of this study would be the same if age-adjusted MAC values were used. Is it possible that many patients in this study have “a double low (mean arterial pressure and bispectral index) and one high (MAC)” instead of a “triple low”?

Competing Interests

The authors declare no competing interests.

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(Accepted for publication February 17, 2016.)

In Reply:

We thank Dr. Pivalizza *et al.* and Dr. Carette *et al.* for their letters in response to our research on increased postoperative mortality associated with intraoperative “triple low”: concurrent low mean arterial pressure (less than 75 mmHg), low volatile anesthetic minimum alveolar concentration (MAC; less than 0.8), and low bispectral index (less than 45).¹

We are surprised that Pivalizza *et al.* found our conclusion to be strongly worded when the adjective “weak” is the chief qualifier in our abstract’s conclusion: “There is a *weak* [emphasis added] independent association between the triple low state and postoperative mortality...”¹ Pivalizza *et al.* raise four specific points. (1) We did not conduct a power analysis since we used a convenience sample from previously conducted studies. The concern cannot be that our study was underpowered that—contrary to our hypothesis—we found a positive result (*i.e.*, a significant association between “triple low” and postoperative death). The concern could be that our study was “overpowered” in that a very large study can find statistically significant, but clinically irrelevant, results. However, death is an important outcome, and even a small impact on mortality could be clinically important. (2) We are more sanguine than the letter writers about the potential for propensity score matching to balance risk factors between groups in nonrandomized studies. Nonetheless, we agree that prospective clinical trials are better able, in general, to reveal causal links than observational studies, where confounders are more likely. (3) We agree that “triple low” was less potently associated with death than preexisting risk factors (*e.g.*, American Society of Anesthesiologists physical status and chronic obstructive pulmonary disease). But the salient point is that “triple low” is potentially modifiable or even avoidable (during general anesthesia), whereas patient comorbidities are largely immutable. (4) We are baffled by the contention in point 4 that sensible anesthesiologists currently assiduously guard against epochs of “triple low,” since the arguments put forward in the rest of the letter seem to suggest that “triple low” is not likely to be injurious and that our results reflect statistical “tap-dancing” or clinical irrelevance. Moreover, despite peer-reviewed data demonstrating that intraoperative hypotension is common² and likely harmful,^{3,4} there are no peer-reviewed data demonstrating that clinicians routinely avoid hypotension by administering medications or decreasing depth of anesthesia. Nor have any standard protocols for the treatment of hypotension (*e.g.*, decrease depth of anesthesia, administer fluids, infuse vasopressors, institute inotropes) been established in any surgical setting or in any patient

population. The intuition of Dr. Pivalizza *et al.* regarding the conduct of reasonable clinicians is therefore unsupported.

Carette *et al.* raise the important point that using non-age-adjusted MAC values might have affected our conclusions. They could be right in that “single high” (age) might be much more important than “triple low” and that some of the patients in our study included in the “triple low” group might only have had “double low” (low mean arterial pressure and low bispectral index) when considering age-adjusted MAC. We chose the methodology in our study to approximate the approach that was used by Sessler *et al.*,⁵ who chose not to use age-adjusted MAC values. But our findings would not have changed substantially had we used age-adjusted MAC. Based on the population in our study, the low MAC cutoff would likely have shifted from the (arbitrary) 0.8 age-unadjusted value to about 0.9 age-adjusted MAC.^{6,7} Furthermore, although age was associated with 30- and 90-day mortality in the multivariable analyses, “triple low” remained independently linked to death despite the inclusion of age as a variable in the models. It is also notable that age was one of the variables used in our propensity score matching.

In conclusion, we apologize if the letter writers or readers were alarmed by our study or our conclusions. We wish to clarify that we do not believe that our findings mandate any changes in clinical practice, and we remain skeptical that “triple low” is causally implicated in postoperative death.

Competing Interests

The authors declare no competing interests.

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(Accepted for publication January 14, 2016.)

All Work Hours Are Not Equal

To the Editor:

We read Baird *et al.*'s¹ recent description of gender differences and trends in the anesthesiology workforce with great interest. As members of a large, vibrant academic level 1 trauma center with busy transplant and neurosurgical services, we observe that several issues raised by the 2013 RAND survey are relevant. We are a particularly diverse faculty group with a greater proportion of female anesthesiologists (49%) than represented in the study (26%).

At first glance, the conclusion that female anesthesiologists receive lower total and hourly compensation irrespective of the fewer hours worked is alarming. However, the context for this is the significantly three-fold greater part-time (defined as less than 35 h/week) employees in the female group, which in itself may explain the apparent discrepancy as 11% of that gender cohort. In a busy facility such as ours with increasing hospital demand for expansion of services, an employee working part-time in a 7 AM to 3 or 5 PM shift adds value to meeting the elective needs of the operating room. However, a significant proportion of urgent and emergency service is provided after hours, on weekends, and on public holidays, and it is both plausible and logical for the larger full-time (by definition, larger male) cohort taking these calls to receive greater compensation. If, as the authors suggest, marital status and the presence of children affect gender hours, then on-call overnight and weekend hours must be valued more significantly than routine office hours.

With reasonable call shifts (14h on weekdays and 12h on weekends) and generous use of postcall days, it is not surprising that a faculty member taking calls, irrespective of gender, may not have significantly total increased hours compared to a weekday-only anesthesiologist. However, with increasing hospital demands, the flexibility of on-call faculty members to take additional pre- and/or postcall shifts is increasingly valuable and facilitates management of the daily schedule.

Given the increasing proportion of female anesthesiologists in almost all age groups documented in the article, there will also be an increasing proportion of part-time anesthesiologists, which may negatively impact both the on-call cohort and flexibility in schedule management. For these