

Competing Death and Absolute Benefit in Dexmedetomidine-Exposed Patients with Dialysis-Requiring Acute Kidney Injury [Response to Letter]

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Dear editor

We thank Wang et al for their thoughtful commentary on our study examining dexmedetomidine exposure and long-term outcomes in patients with dialysis-requiring acute kidney injury (AKI-D).¹ Their comments highlight important methodological considerations.

We agree that death may act as a competing event for renal outcomes such as progression to end-stage renal disease (ESRD) and eGFR decline. In our primary analysis, Cox proportional hazards models treated death as censoring, thereby estimating cause-specific hazard ratios. This approach addresses whether dexmedetomidine exposure is associated with the instantaneous risk of renal outcomes among patients who remain alive and under observation, but it does not directly estimate the cumulative probability of ESRD in the presence of competing mortality. Because dexmedetomidine exposure was associated with lower mortality, more patients in the dexmedetomidine group may have survived long enough to experience or be observed for renal endpoints. Conversely, control patients who died were censored and removed from the renal risk set. Under a Fine–Gray subdistribution framework, these deceased patients would remain in the risk set but could no longer develop ESRD, potentially attenuating the apparent renal benefit.² Therefore, the cause-specific hazard ratio for ESRD may overestimate the association when interpreted as an absolute clinical benefit. We acknowledge this as a valid limitation. The TriNetX platform provides large-scale, multi-institutional real-world data, but its integrated analytic tools do not currently support Fine–Gray competing risk regression. This limitation was not explicitly stated in our manuscript, and we appreciate the opportunity to clarify it.

Wang et al also suggest reporting risk differences, numbers needed to treat (NNTs), and safety outcomes to facilitate clinical interpretation. We agree that absolute effect measures and safety data are essential for evaluating net clinical benefit. However, because our study was observational and explicitly framed as associational, translating hazard ratios into NNTs requires caution. NNTs are most appropriately interpreted when a causal treatment effect can be assumed, because they attribute observed differences in outcome rates to the exposure itself. In our observational study, this assumption cannot be verified and may be affected by residual confounding.³ Although approximate NNTs can be derived from reported absolute differences, presenting them as study-endorsed estimates could overstate therapeutic certainty, particularly in critically ill AKI-D patients with substantial heterogeneity and residual confounding.

We fully agree that future prospective randomized trials should incorporate competing risk analyses, absolute risk measures, and safety endpoints, including bradycardia, hypotension, and vasopressor escalation. Our study was intended to generate preliminary evidence and inform future trial design, rather than to justify immediate changes in sedation



practice. Interpreted within this associational framework, we believe our findings contribute to the ongoing evaluation of dexmedetomidine in this high-risk population.

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Disclosure

The authors declare no conflicts of interest in this communication.

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