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Reply to Chousterman *et al.*

From the Authors:

We thank Chousterman and colleagues for their positive appreciation of our work (1). However, we believe that their contention is mainly speculative, as it is based on anecdotal reports that provide no or little detail on the renal replacement therapy (RRT) modalities that were supposed to be responsible for neurological deterioration.

More important, we feel that the authors miss several points. They reason as if RRT were not associated with any risk except the increase in intracranial pressure. They fail to incorporate in their thinking process the different regulators of cerebral blood flow: arterial blood pressure, intracranial pressure, and cerebrovascular resistance (2). The first component, the cardiovascular component, has been highlighted for over a century (3). Hemodynamic instability is a frequent issue in brain-injured patients, and even more so in cases involving multiple trauma. Thus, RRT-associated hemodynamic instability, which occurs frequently and within the first minute of RRT (unlike disorders linked to osmolal changes, which are rare and have a delayed onset) may have catastrophic consequences on an injured brain. Starting RRT in a patient with recent head injury (especially in the context of polytrauma) may likely affect hemodynamics. In addition, the authors fail to consider that a delayed strategy has been shown to allow the avoidance of RRT in one-third to one-half of patients (4, 5). Obviously, the best way to avoid RRT-associated osmolal brain changes is to avoid RRT. The application of an early RRT strategy potentially increases the risk of hemodynamic fluctuation (which may decrease cerebral perfusion and contribute to acute brain injury) for all patients. In this regard, the remedy they propose (starting RRT early in all acute kidney injury patients with brain injury) may be worse than the disease. Finally, a careful reading of case reports and case series cited by Chousterman and colleagues (6) shows that in most cases, patients received "aggressive" intermittent RRT. For instance, in one case blood urea nitrogen decreased from 141 to 54 mg/dl in one session, which is not desirable even in a patient without brain injury. Several ways to avoid acute osmotic shifts exist (7) but were not discussed: slow and gentle initial hemodialysis (time <2 h and low blood flow rate), increasing dialysate sodium level, or administration of osmotically active substances (e.g., intravenous manitol).

In our era of evidence-based medicine, we must point out that stating "we suggest not using the delayed RRT initiation strategy in patients at risk of elevated intracranial pressure" is not supported by data. Similarly, stating that "the best strategy for RRT modalities and initiation in this subset of patients remains to be determined" means that one has to carefully weigh the actual (and proven) risk of undue RRT against that of delaying RRT in brain-injured patients. We suggest that before issuing so strong a warning without firm evidence, it would be necessary to conduct a randomized clinical trial on this particular population. ■

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Pathological Sleep and Wakefulness in the ICU and Weaning Failure: A Causal Relationship?

To the Editor:

The contribution of Dres and colleagues (1) addresses an important clinical question, as changes from normal sleep physiology during invasive mechanical ventilation, with and without analgosedation, are not entirely understood and there may be an interaction between sleep and successful weaning. Understanding the impact of sleep on weaning is important, and interventions to normalize sleep during mechanical ventilation might affect outcomes (2–4). The question remains as to whether patients with atypical sleep or pathological wakefulness are more likely to fail spontaneous breathing trials and, subsequently, weaning.

In a small ($n = 31$) and heterogeneous cohort of mechanically ventilated ICU patients, the authors observed that patients who passed a spontaneous breathing trial successfully and were subsequently extubated showed higher levels of wakefulness than patients who failed the spontaneous breathing trial and those who passed the trial but clinically were not deemed ready for extubation (1). This was expressed by a novel marker of wakefulness level or sleep depth measure (the odds ratio product [ORP]) during a 15-hour electroencephalographic recording before the spontaneous breathing trial. Patients who failed the spontaneous breathing trial were more likely to exhibit a poor interhemispheric correlation of sleep depth or level of wakefulness, as expressed by the intraclass correlation coefficient between ORP in the right- and left-brain hemispheres, than those who passed the spontaneous breathing trial (1).

The authors present important results; however, there remain some limitations to be pointed out and considerations for the design of future studies.

Causality between low ORP levels or interhemispheric ORP asynchrony and weaning failure cannot be established based on the study design and diverse bias. The effect of various analgosedation regimes on ORP in the general ICU population is unknown, and there were differences between the studied groups. The authors looked at ORP at a single time point in a small and heterogeneous group of mechanically ventilated patients and did not elaborate on changes of ORP over time, or over the length of the ICU stay, which differed between the groups. There was limited information regarding previous sleep deprivation or measurement thereof, raising concerns about the sequel of pathological sleep measures and rebound effects. Days with analgosedation and, eventually, critical-illness neuromyopathy may have affected the findings, although it is surprising that the successfully extubated patients had the longest ICU stays. Any information on previously diagnosed sleep-disordered breathing is missing.

It is problematic to compare the group of patients who passed the spontaneous breathing trial but were not deemed ready for extubation with the extubated group, or with the group that failed spontaneous breathing trials. Being considered ready for extubation depended on a subjective clinical decision, and information on the decision pathways used is not provided. Reasons for failure to wean should be stated. Furthermore, there was no consistent “dose–response” relationship in the ORP measurements across the three groups, which underlines the difficulty with the comparisons and the interpretation of the results.

In addition, the suggested underlying pathophysiology of changes in sleep and their clinical implications should be further discussed. Data regarding neurofunctional and neuroimaging outcomes are missing and should be addressed to understand how low levels of ORP, low interhemispheric ORP correlations, atypical sleep, and pathological wakefulness affect these elements before the effect of atypical sleep on such complex outcomes as weaning failure can be conclusively considered.

The next step would be to study neurofunctional and neuroimaging outcomes with regard to atypical sleep and different levels of ORP over time, and the effects of different analgosedation protocols on ORP. Furthermore, we need to develop a study design that elucidates the causal relationship between sleep disturbance and weaning failure, find ways to standardize clinical decision-making, and study the effect of interventions to normalize sleep on weaning outcomes. ■

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