Author's response to reviews

Title: Head CT is of limited diagnostic value in critically ill patients who remain unresponsive after discontinuation of sedation

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Author's response to reviews: see over
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Melissa Norton, MD
Editor-in-Chief
BMC Journals

Re: MS: 6202835792131180 - Head CT is of limited diagnostic value in critically ill patients who remain unresponsive after discontinuation of sedation. Revision#2

Dear Dr. Norton:

We thank the reviewers for their insightful comments on our manuscript. Below please find point-by-point response to the concerns raised by the reviewers. Corresponding changes in the revised manuscript are presented in red font. We hope the manuscript is acceptable for publication in the BMC Anesthesiology. All the authors reviewed and approved the final version of the manuscript. None of the authors has any conflict of interest related to the findings in this manuscript.

Thank you for considering our work.

Sincerely,

Gokhan M. Mutlu, MD
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We thank all the reviewers for their insightful comments.

Reviewer#1.
C1. The authors included a discussion of the differences, and allude to their exclusion of patients with focal neurological findings as a plausible explanation. This certainly begins to address our comment. We would add, however, that other reasons could also be argued, including differences in patient population (other than just coagulopathy) as well as the institutional “threshold” for ordering a CT. Additionally, Kress et al. had a higher rate of DIS across the whole population, making a structural (as opposed to lingering sedative) cause of “non-awakening” more likely.

R1. We thank the reviewer for his suggestions and expanded the discussion regarding the possibilities to account for the differences between two studies.

C2. The authors discuss the limitation of their study in the discussion and conclusions section, but do not make any changes to the nature of the study presented. The discussion frames the data presented, but obviously this limitation is significant. Not addressing this limitation thus limits the impact of the data. The main conclusion is that in this medical ICU where sedation is performed with benzodiazepines and fentanyl largely without DIS, the diagnostic yield of HCT in patients who fail to awake is very low. It tells us little about the (potentially modifiable) risk factors for delayed-awakening (DIS, sedative, diagnosis, age, neurocognitive function, renal function, etc.) because we don’t know how these patients differ from patients who do wake quickly.

R2. We have now added data on a control group consisting of consecutive patients who required mechanical ventilation for longer than 72 hours but did not have prolonged sedation. The control group was collected from the first 4 months of the study. In this control group there were a total of 144 patients who required mechanical ventilation for longer than 72 hours. Out of these 144 patients, 19 patients had continued unresponsiveness after discontinuation of sedation; 23 patients died before sedation was discontinued and 102 patients did not have difficulty waking up after discontinuation of sedation and thus did not require head CT. It is these 102 patients who comprise the control group. We revised tables 2 and 4 to include the data from these control patients and show that the two groups did not have any differences for any demographics. In addition the sedation profiles were similar except that daily interruption of sedation was administered more frequently in the control group. This is now included in the results section and the discussion as well and the conclusion was revised accordingly.

Reviewer #2.

C1. At the end of the introduction section, the authors give their major result and conclusion. It is rather unusual and it is not the purpose of the introduction to expose results. The sentences from “We found that head-CT …” to “sedative infusions” should be deleted.

R1. We deleted the sentences that summarized the results at the end of the background section.

C2. The authors emphasize the increased incidence of coagulopathy in their patients population. I am not sure that it is really relevant. I understand that doctors may be afraid of an intracranial
bleeding in case of coagulopathy but it is usually associated with a focal neurologic deficit.

R2. We agree with the reviewer that patients with coagulopathy are more likely to present with a focal neurologic deficit. We deleted “coagulopathy” in our sentence on page 10.

Reviewer#3.

No new comments.