Author’s response to reviews

Title: Influence of early elective tracheostomy on the incidence of postoperative complications in patients undergoing head and neck surgery

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Dear Sujoy Banik, dear Zainab Ahmad, ladies and gentlemen,

first of all, we would like to thank the reviewers for their thorough validation of our manuscript and valuable comments on our research work.

Our table shows 4 patients with tracheostomy who developed delirium. Our main interest was to evaluate patients with primary elective tracheostomy. Secondary tracheostomy was only performed in cases with problems leading to prolonged sedation and ventilation. We clarified this by changing material and methods section, page 4, line 7 and by consequently using the exact terms throughout the manuscript.

The patients that were repeated had their second operation performed after a considerable period of time after the first flap transfer, so we decided to include these into analysis as separate cases. We added clarification in the results section, page 5, line 8.
We also did a statistical analysis according to age groups similar to the suggestions of reviewer 1 without significant result. We included a description in result, page 5, line 23 and clarified the results further on page 6, line 19. We also feel that the influence of age is very important on the development of postoperative delirium and because of that we included a short discussion on page 7, line 20.

Analysis of initial sedative dosages was also included in results, page 5, line 15 and page 6, line 18.

We also want to thank reviewer 1 for his hints on how to tackle active delirium with pharmacological agents. We are currently working on implementing SOPs for these situations and will most certainly consider the substances suggested.

Our study was designed as retrospective data analysis. In order to clarify the design, which is indeed a very important issue, we added page 3, line 21 most prominently in the introduction. Also, later down below on page 3, line 26 in materials and methods, we added further description of the study design. We wanted to evaluate the influence of airway management and in order to do so, exclude any confounders. Therefore, a defined period of time was chosen where medical and surgical management was consistent at a high level without the natural fluctuations a university hospital normally shows because of its character as a facility for medical training and research.

We added the missing medical and statistical data in the results section, beginning on page 4, line 3 and continuing throughout the manuscript and by expanding table 1.

Reviewer 2 very clearly states that nasotracheal intubation acts as a strong confounder. This is exactly what we believe and why we wanted to evaluate (and subsequently advocate) elective primary tracheostomy as a safe alternative in order to avoid certain complications.

We want to thank both reviewers again for their intense work on our manuscript and their precise comments. We hope we were able to correct any incorrections and clarify the points raised by both colleagues.

We would also like to thank the editors of BMC Anesthesiology for the possibility of having our manuscript reviewed by experience peers and considering it for publication.

Sincerely,

Michael Wunschel