Author's response to reviews

Title: Endotracheal intubation with Airtraq versus Storz videolaryngoscope in children younger than two years - a randomized pilot-study

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Author's response to reviews: see over
Dear Tom Rowles

Thank you for the opportunity to revise the manuscript: 2350810556584594.

**Title:** "Endotracheal intubation with Airtraq® versus Storz® videolaryngoscope in children younger than two years - a randomized pilot-study".

Please find our response to the referees' comments below.

**Comments from Thomas Engelhard:**

**Comment:** “It should perhaps be made clear what endpoints are the investigators attempting to compare in the proposed larger trial”.

**Response:** In the description of this trial, we think it is sufficient only to describe the endpoints related directly to our protocol. We have considered changing the primary endpoint to a time related endpoint in our further investigations. However, which primary endpoint to choose in studies investigating the complex process of endotracheal intubation is not an obvious choice.

No changes have been made in the manuscript.

**Comment:** “The authors should not try and make conclusions/ recommendations in the discussion of which is better”.

**Response:** We agree that emphasis should be put on the fact that this is a small sample size.

The following has been added to the manuscript:

“Our results, although based on very few observations, are supported by Vlatten et al who performed a randomized trial comparing the SVL to direct laryngoscopy in 56 children, aged 4 years or younger. They reported “time to intubation” to be 27 sec”.

Section: Discussion; L: 23.

“In conclusion, no difference in the success rate of endotracheal intubation could be established in this ten patient sample of children younger than two years with a normal airway assessment scheduled for elective cleft lip/palate surgery. However, the Airtraq® Optical videolaryngoscope showed a number of time related advantages over the Storz® videolaryngoscope. Because of the small sample size a larger trial is needed to confirm these findings.”

Section: Abstract Conclusion L:16 and Conclusion L: 1.
Comments from John Fiadjo:

Comment: “The authors do not address their hypothesis in the conclusion of their paper, although their methodology is sound the authors did not evaluate a sufficient number of subjects to address their hypothesis”.

Response: We agree with the statement from the reviewer. However, it is clearly stated in the manuscript that this is a pilot-study with a sample size of ten patients. We believe that the methodology in how the endpoints were obtained represents a novel and advanced use of computerized recordings. Also, the patient group were small children, which is a rare group of patients included in medical research, making our data important to publish.

The following change has been made in the manuscript:

“In conclusion, no difference in the success rate of endotracheal intubation could be established in this ten patient sample of children younger than two years with a normal airway assessment scheduled for elective cleft lip/palate surgery”.

Section: Conclusion L: 1.

Comment: “It is also unclear what sample size data the authors sought with their secondary hypothesis”.

Response: No sample sizes were performed as stated in the “Statistical Analysis”-section.

No changes have been made in the manuscript.

Comment: “The methods section states that a SVL Miller blade size 0 was used for intubation. The Miller 0 SVL blade would not have been appropriate in all patients in the age range studied”.

Response: The smallest laryngoscope blade for the Storz videolaryngoscope was sufficient in size to intubate all the children in the study. Otherwise we of course would have used the next size. It is correct that an ordinary Miller #0 would have been to small.

The following has been added to the manuscript:

“With the SVL a “Miller-like” blade size 0 was used. The blade resembles an ordinary Miller blade, but is lower and half a cm longer.”

Section: Methods, L: 32.

Comment: “All intubations were performed by a single anesthesiologist, the discussion should include a discussion of potential unintentional biases in performing the laryngoscopies”.

Response: Thank you for the comment, the following discussion has been added:

In order to eliminate this bias the number of consultants performing the laryngoscopies would have to be increased. However, this would introduce other confounders such as the differing in learning
curves with the devices and variable overall intubation experience in small children between the consultants. It was therefore decided to have only one consultant with extensive training in intubating small children with both devices.

Section: Discussion, L: 7.

We hope that the manuscript is now acceptable for publication.

On behalf of the authors
Yours sincerely,

Martin Kryspin Sørensen, MD