

Peer Review File

Article Information: <https://dx.doi.org/10.21037/tp-23-572>

Thank you for the opportunity to revise our submission entitled “Video Laryngoscopy as the Standard of Care for Pediatric Intubation – The Time is Now” (Manuscript ID: TP-23-572). We are also thankful for the reviewers’ many excellent suggestions. We will attempt to incorporate their comments into the revised manuscript and address their concerns in a sequential fashion below.

First and foremost, we would like to address an issue that became obvious to us with the comment by Reviewer D: “When reading the article, I did not understand the aim of the paper”. We take responsibility for creating confusion here, but we suspect that at least Reviewer D (and possibly also Reviewer C) might have been unaware that **this manuscript is an invited Editorial Commentary on the article published elsewhere by Rivas and colleagues** (reference 1) extended to us by TP’s Science Editor Annabel Liao. Therefore, this was not intended to be a comprehensive review on video laryngoscopy in children. Perhaps we incorrectly assumed that the editorial office assigning this manuscript for review would have warned the reviewers, and that a note explaining its purpose would accompany it in publication. We empathize with the confusion some of the reviewers must have experienced while reading our piece without explanation of its intent. In fact, we commend them for their restraint in their reviews of the manuscript because it is clearly deficient if it were being presented as a comprehensive review of the topic instead of a focused editorial. Our prior editorials have always been in relation to an original article contemporaneously published in the same journal; however, Translational Pediatrics has a feature where they invite authors to write an editorial commenting on an important article published in another journal and we believe this is where the confusion arose for both us and the reviewers. **We hope this explanation brings clarity to our intent in submitting this invited manuscript to TP.** We have attempted to address this issue in the revised manuscript by inserting an introductory paragraph stating the original intent. Text insertions in the revised manuscript are highlighted in yellow so the reviewers can easily identify them.

Reviewer A

Comment 1: The submission of your articles came at the right time as the market continues to see the development of video laryngoscope and the modification its prototype to the smallest of all patients. Your manuscript explained to the readers the scientific reason for performing laryngoscopy with apnea oxygenation, how video laryngoscopy mitigates with intubation in the neonates and its application to medical stimulation and learning experience/confidence. I enjoyed reading your manuscript.

Reply 1: Thank you for your kind words.

Changes in the text: None.

Reviewer B

Comment 1: The reviewer wants to thank the authors for their excellent overview and rational why video laryngoscopy should be standard of care in the pediatric population. I fully agree, that it is time now for video laryngoscopy being standard of care in the pediatric population. In fact, it is time to be standard of care for any patient. As the authors point out, many studies in pediatric patients with normal and difficult airway anatomy show improved success rates and improved view to the glottic entrance at the cost of prolonged time to intubation using video laryngoscopy versus direct laryngoscopy. As the authors clearly point out, high flow nasal oxygenation significantly extends apnea time (1). The appropriate flow rate is under discussion ranging from 0.2 l/kg/min to >2 l/kg/min or a range from 2-70 l/min (1). The flow rate recommended by the authors of 1 l/kg/min falls right in that area and is supported by literature.

Reply 1: thank you for your comments and support of our opinions on video laryngoscopy use in children.

Changes in the text: None.

Comment 2: The authors mention correctly the differentiation of video laryngoscopy using a standard versus a non-standard or hyperangulated blade. Although the rational why we should use video laryngoscopy with HFNO is clear, the authors do not clearly recommend either use of standard or non-standard blades or their own experience with either one (Line 153). May the reviewer suggest to discuss the results of the PeDI Registry, which showed a success rate of 81% versus 58% using standard blade versus non-standard blade video laryngoscopy in infants <5kg.

Reply 2: Thank you for this excellent suggestion. We have included a brief discussion on blade type and the PeDI Registry reference.

Changes in the text: We added the following sentence on page 8, lines 4-8. “An observational study of

video laryngoscopy in children comparing standard and non-standard (hyper-angulated) blades (26) found that the use of standard blades was associated with greater success both at initial (51% vs 26%, $p=0.002$) and eventual intubation attempts (81% vs 58%, $p=0.002$) in children weighing < 5 kg, respectively, but not in those ≥ 5 kg.(26)

Comment 3: One significant downside of video laryngoscopy is the possible obscured camera view by secretions or blood. Using a standard blade during video laryngoscopy allows the use of the device as a standard direct laryngoscope once severe bleeding or secretions are obscuring the camera. The reviewer may suggest to add this fact into their discussion of the use of a standard versus a non-standard hyperangulated blade as a first attempt device.

Reply 3: Thank you for this excellent suggestion.

Changes in the text: We have added the following sentences to the text on page 8, lines 14-18. “A perennial concern regarding the use of video laryngoscopy is the possibility of secretions or blood obscuring the camera, and other types of equipment malfunction (e.g., loss of power, cable discontinuity). In such unexpected emergencies, the use of a standard blade could still enable intubation as the operator may rapidly pivot from video to direct laryngoscopy without the need to clean or exchange blades”.

Comment 4: A final comment to this excellent editorial is that video laryngoscopy does not guarantee a successful intubation. Every practitioner managing an airway need to have a Plan B and C, in case video laryngoscopy or any other first attempt of airway management fails. May the reviewer suggest to add a brief note, that the use of a supraglottic devices kept in mind and that a front of neck access in a cannot ventilate cannot oxygenate in infants is extremely challenging if not impossible. A note of the use of ultrasound to identify airway anatomy may be added.

Reply 4: Thank you for this excellent suggestion.

Changes in the text: We added the following sentences to the text on page 6, lines 12-23. “Despite the fact that the vast majority of subjects (99%) instrumented using video laryngoscopy in the study by Riva and colleagues (1) were successfully intubated in the first or second attempts (and none required more than 3 attempts), it must be noted that the use of video laryngoscopy does not necessarily guarantee intubation success. As with every invasive procedure with an inherent risk, the practitioner managing the intubation must have a well-delineated plan to be used should they encounter equipment failure or inability to complete the endotracheal intubation. The availability of a properly sized supraglottic device (e.g., laryngeal mask airway) or advanced airway equipment

(e.g., lighted stylet, fiber-optic scope) may be lifesaving should the operator encounter a “cannot ventilate, cannot oxygenate” scenario. This is especially important considering that attempting to establish an emergent front-of-neck airway on a small infant outside the operating room is extremely challenging (if not impossible) and associated with significant morbidity. The use of point-of-care ultrasound to delineate airway anatomy might also be of help in selected cases”.

Reviewer C

Comment 1: This is an interesting review paper on an interesting topic.

Reply 1: Thank you for the kind words. However, as stated earlier, this is not a review paper. This is an invited editorial on the article by Riva and colleagues published in 2023 in Lancet Child and Adolescent Health.

Changes in text: Page 1, line 1-10; an introductory paragraph explaining the nature of this invited editorial has been added to the text.

Comment 2: In the title it refers to paediatric intubation but in the text it refers more to neonatal intubation.

Reply 2: The emphasis on neonatal intubation stems from the fact that this is an editorial on a study about intubations in neonates and small infants. In order to expand the scope of the editorial, we introduced some statements about use of video laryngoscopy outside the neonatal period. The title attempts to unify our opinion on the role of video laryngoscopy across the entire pediatric age spectrum.

Changes in text: none.

Comment 3. With regards to neonatal intubation, it would benefit from looking a bit closer at the studies included in the cochrane review. Several of them use VL to help share the view with the supervisor and support a coaching model. Where VL is proven most successful in neonates is for junior intubators intubating while supervised by an experienced colleague. I think the review might benefit from looking a bit closer at some of the studies included in the cochrane.

Reply 3: Thank you for this suggestion. We introduced a brief mention of this fact but will refrain from exploring it further since it is peripheral to the study of Riva and colleagues.

Changes in text: page 7, lines 15-18. The following text was added. “In fact, first-attempt intubation success rates among inexperienced trainees learning endotracheal intubation is significantly improved when an instructor provides real-time coaching while sharing the operator’s view on the

video laryngoscope (21)".

Comment 4. There is detailed description of Riva's study as that is the only one who has combined apnoeic oxygenation and VL. As that study showed VL may be superior but was within the non-inferiority margin, it is hard to use this as a recommendation that this should be standard of care. It may be worth including Kate Hodgson's RCT looking at apnoeic oxygenation within neonatology.

Reply 4. The reason there is a detailed description of the Riva study is because it is in fact the focus of this invited editorial. Riva's findings were complicated by the fact that the primary outcome straddled the non-inferiority margin. The risk difference for first-attempt success rate was 10.4% in unadjusted analysis and 9.5% in adjusted analysis (with a proposed non-inferiority margin on 10%). In the modified intention to treat analysis, VL was significantly associated with a higher first-pass rate (89.3%) compared to direct laryngoscopy (78.9%, $p=0.025$). Considering ours is an invited editorial commentary and that such pieces afford the author some latitude for opinion, the fact that we are sufficiently convinced that VL (when available) should be the pediatric standard of care does not mean that VL is the standard of care or that dissenting opinions are not valid.

Changes in text: None.

Comment 5. The manuscript would benefit from being broken into sections and potentially highlight some learning points.

Reply 5. Thank you for these suggestions. If this were a review article, we absolutely agree that it would have benefitted from being broken down into sections. However, according to the editorial stipulations provided to us by Translational Pediatrics with the invitation to write this editorial, the manuscript must be unstructured.

Changes in text: None.

Reviewer D

Comment 1. I enjoyed reading your manuscript and it is clear that your team has put a great deal of effort into this work. I would like however to advise the authors to consider the following points:

Reply 1. Thank you for your kind words and suggestions.

Changes in text: None

Comment 2. When reading the article, I did not understand the aim of the paper. I finally found the aim in

the last paragraph stating that "we believe video laryngoscopy with high flow oxygen supplementation should be the standard of care". Usually, the aim is stated in the first paragraph and then supported by evidence. In this case, the evidence is presented first and then the aim of the paper is presented at the end leaving the readers to guess as to what statement the authors are attempting to support.

Reply 2. Please reappraise this manuscript under the optic that it is in fact an invited editorial on the Riva paper (reference 1), not a structured review. We hope the revised manuscript will make more sense to the reviewer under this alternate context, and with the revisions incorporating suggestions by the other reviewers.

Changes in text: None

Comment 3. The article appears somewhat poorly organized. The studies mentioned should support the aim but there doesn't appear to be a cohesive structure that leads the author to the intended conclusion. The paragraphs do not easily lead into the next - just mentions study after study.

Reply 2. We respectfully disagree with the reviewer. The manuscript follows the standard progression of a traditional editorial.

Changes in text: None

Comment 4. The authors contradict themselves in the last paragraph by describing the hurdles to implementing the wide/universal use of VL over DL and essentially making a good argument against its implementation without offering solutions. I am left with more questions than answers after reading the last paragraph. How can these "barriers" be overcome? Do the authors have any suggestions?

Reply 4. We again respectfully disagree with the reviewer that we contradicted ourselves by merely highlighting that implementation of VL can be faced with certain obstacles. It is beyond the scope of this editorial to an in depth analysis of potential strategies to overcome implementation barriers, especially since those have been well studied by others. In order to provide some optimism that the obstacles to VL implementation are not insurmountable and to point the reader in the right direction for further reading, we edited the text as shown below.

Changes in text. (page 9, lines 8-10) We added the following text to the last paragraph of the manuscript. "Strategies to overcome these obstacles have been well studied in the PICU environment by Davis and colleagues(27), and include device accessibility, fostering a quality improvement culture, and strong leadership."

Comment 5. I encourage the authors to take a firm stand on their aim - VL is superior to DL - and argue for widespread use of VL. The manuscript title leads the author to believe that the manuscript will argue

strongly for the use of the VL but I believe it falls short of this goal and does not persuade the reader well. For these reasons, I recommend a major revision.

Reply 5. We thank you for this comment. However, we are unsure as to how much more clearly, we should have positioned ourselves on this matter than the unambiguous opening sentence of the last paragraph that reads: “Given the totality of the available evidence and our own clinical experience, we believe video laryngoscopy with high-flow oxygen supplementation should be the standard of care for tracheal intubation of neonates, infants, and children, when readily available”. We hope the reviewer will find it sufficient when examined through the optic that this is an editorial.

Changes in text: None