

Peer Review File

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Reviewer A

- 1) *The study is a small sample of retrospectively reviewed patients undergoing surgery to "evaluate change in diet" (66-rather than 222, given the stated aim of the study)...Richter et al, as referenced by the authors has in essence done this study in a similar sample, so this study adds little new or novel information*

Reply: The authors agree that other researchers have studied the topic. We believe our project still contributes to the literature. Dr. Richter et al's referenced study included 50 patients, 14 of which had no preoperative dysphagia. The authors do not understand why those in our study with no preoperative dysphagia should not also be included as a lack of preoperative dysphagia does not preclude postoperative dysphagia, which would be a change in feeding abilities. Please let us know if we can clarify further.

- 2) *4 patients in the sample had permanent diet restrictions, and one had neurology (cerebral palsy), another was born 35 weeks pre-term, and ultimately the sample size is not enough to draw general conclusions about the adverse event rate of surgery. Likely pooling of several hundred or thousand patients is required*

Reply: We agree. On page 11, line 226-227, we have acknowledged this by adding: "Finally, the sample size is small hence the results need to be interpreted with caution."

- 3) *The study structure and layout is confusing, particularly numbers and percentages in the Abstract, although ultimately the reader "can sort" by following the figure 1*

Reply: The authors would be happy to reduce the numbers in the abstract by removing the absolute values or percentages if that would be better. Please let us know what you would prefer. We included both for completeness, but do not wish to confuse readers.

- 4) *Perhaps the authors could consider pooling much large numbers from multiple centres, to address the stated intent: "We aim to describe change in diet and dysphagia after supraglottoplasty in patients with laryngomalacia."*

Reply: The authors agree that a larger study population pooled over multiple centers would be more generalizable. Our series of 222 is the largest to date and therefore we believe contributes to the current literature. We have also acknowledged this on page 12, line 236 onwards. While it

is outside the scope of the current study, data collection to pool the numbers are underway via the first and second authors who have completed their fellowship in Ann Arbor.

Reviewer B

- 1) *The discussion may want to spend time discussing whether the demographics of the patients in this study skew the results to worse swallow function that might be seen in the typical infant with no comorbidities*

Reply: On page 9, line 174 onwards onwards, the very valid point raised has been addressed:

“In our study population, 67% had other co-morbidities with 21% neurocognitive disorders, 13% genetic abnormalities, 13% congenital cardiovascular diseases and 8% oral aversion, increasing the risk of pre-existing dysphagia. In addition, 38% had concomitant airway diagnosis, and 21% had previous airway surgery, all of which may impact on the 14% of cases that required revision surgery and the 4 requiring tracheostomy, reflecting either severe disease and or confounding airway condition resulting in persistent airway obstruction. These may also impact on the pre- and post-operative dysphagia. It is important to identify these factors to counsel the parents, identify, prognosticate and manage dysphagia that may persist.”

- 2) *The relatively small number of patients with pre and post-surgical video swallows also skew these results to reflect children with perceived worse swallow function*

Reply: This has been added to page 9, line 168: “... with only 10% (n=22) underwent both pre- and post-operative VFSS.”

- 3) *Introduction: Line 37 - superscript references*

Reply: That reference has been changed to Vancouver style. The authors apologize for the typographical error.

- 4) *Method: Paragraph line 68: the description of video swallow being categorised into mild, moderate or severe is not reflected in the results. These classifications are not further discussed. Either remove from methods or include these results. Would prefer this classification used in the results to clarify severity of swallow disfunction*

Reply: See reply for 16.

- 5) *Method: Line 84 - not clear what these other airway surgeries were - if relevant more details may be needed in the result section*

Reply: This has been added into table 2 for point 18

- 6) *Method: Line 87: the authors state statistical analysis yet present no statistical analysis within results. I acknowledge that likely no stistically significant results but perhaps should be presenting the p values if performed. Eg was there a difference between the patient group that had objective swallow assessment versus the rest? le age, other comorbities, requirement for revision surgery. May be presentable on a demographics table. Results section just states no associations with multivariate analysis. Would like to see some figures somewhere*

Reply: A summary with p values has been added in table 2.

- 7) *Results: In general need rewriting to improve readability. Some of the results may be better presented in tabulated form rather than wordy descriptions. They can be further discussed in the discussion. The demographics perhaps could be presented as a larger table, with a breakdown of those who had objective assessment versus subjective. Could demographics also include follow up, median and range - follow up is a significant clarity problem within the results*

Reply: The results section has been rewritten to improve readability with more tables and figures to avoid wordy descriptions. In addition, subtitles were added: "1.1 Presenting Symptoms", "1.2 Diet Restrictions", and "1.3 Videofluoroscopy & Swallowing Function".

- 8) *Results: Table 1: The majority of cases were laser supraglottoplasty - in the discussion can the authors further comment on evidence that technique does or doesn't contribute to swallow outcomes. Could technique affect short term worsening of swallow? The discussion mentions heterogeneity of surgical treatment but the results that are presented suggest that the vast majority were laser cases.*

Reply: It is true that the majority of this study's cases were laser as demonstrated in table 1; however, not all patients underwent the same surgery hence our statement regarding heterogeneity. We would be happy to remove or reword this statement if you recommend doing so. The following more thorough discussion of the literature on this topic has been added to the discussion at the location of the above statement regarding heterogeneity on *page 11, line 218* onwards: "This retrospective, 39 patient cohort study by Ratsatter et al was performed to directly compare postoperative dysphagia after laser versus cold steel supraglottoplasty after a prior study identified a 37% rate of dysphagia after laser supraglottoplasty without any comparison (11). Ratstatter et al observed new onset dysphagia in 31%of patients who underwent cold steel supraglottoplasty and 56% who underwent laser supraglottoplasty with p = 0.18."

9) *Results: Line 101-102 and table 2 would be better included in the first paragraph discussing demographics and table 2 may be better as part of a more inclusive demographic table*

Reply: Table 2 has been replaced with one in point 18.

10) *Results: The inclusion of GERD treatment in the comorbidity table may deserve more attention in the discussion. More children were on treatment post supraglottoplasty than before, does this reflect institutional policy or do more children, objectively or subjectively, have GERD after supraglottoplasty? Or is GERD under recognised and under treated in the first instance. Are children with objective measurements of swallow dysfunction any more or less likely to be treated with antireflux medication? Does GERD influence swallow?*

Reply: The authors do not think more patients had GERD after supraglottoplasty compared with preoperatively. While pre and post-operative medical management of GERD is extremely common, it was not universally performed during the study period. In other words, the authors would select your option above that GERD was under recognized preoperatively. There was no institutional policy. Your questions regarding an association between dysphagia and antireflux medication use are interesting, but beyond the scope of this study.

11) *Results: Line 103 - "immediately" what does this mean? Day 1? First week?*

Reply: This has been clarified on page 6, now line 108: "(within the first week)"

12) *Results: Paragraph starting line 103: I would debate that the patient only followed for 5 months doesn't fit into the same criteria as the patient with problems still at 2 years. How many at 2 years still had dietary restrictions? how many at 3 months? 6 months? 1 year? Would like clearer data on the follow up if these patients. I wonder if there is a clearer way of presenting this paragraph.*

Reply: We did not collect the data to allow for this analysis. The dysphagia is recorded for pre-operative, the first post-operative and the final post-operative swallowing function that is available.

13) *Results: Line 110 - what was the followup time for the 6 who returned to normal diet versus the 60 who didn't? Did they have other risk factors? Eg comorbidities, airway lesions esp vocal cord palsy, etc*

Reply: This is in relation to page 6, line 107. The paragraph has been changed and figures 3 and 4 were inserted:

"Most patients had no diet restrictions prior to surgery (n = 156, 70%). Immediately (within the first week) after surgery the changes in swallowing function is shown in figure 3 for those with

normal, and figure 4 for those with abnormal swallowing function. The median dysphagia duration is 165 days (range: 6-1825 days).

There is a strong association with pre-operative diet modification ($p=0.00$), presence of laryngeal cleft ($p=0.00$), previous CTR ($p=0.011$), history of cardiac surgery ($p=0.001$), association with syndrome ($p=0.00$), presence of oral aversion ($p=0.00$), pre-operative NGT ($p=0.00$) and G tube ($p=0.00$), pre-operative swallowing dysfunction ($p=0.000$) with dysphagia.

In patients with a normal function (figure 4), the dysphagia resolved (back to normal diet) in 14 of 17 (82%) patients over a median of 30 days (range: 19 – 720 days)."

14) *Results: Line 113: 3 had no comorbidities? Should this read 2? Of the 4, 1 was mildly Prem and one had CP.*

Reply: The authors have deleted this sentence entirely per your suggestion in point 15 below.

15) *Results: Line 117: I would debate that airway pathology is a comorbid condition, Vocal cord palsy is a risk for aspiration. This may not have been recognised presurgery (line 119: was this the vocal cord patient? If so important to say this) Would expect that vocal cord palsy itself is a risk for persistent swallow dysfunction, did the authors look specifically at this group? As mentioned above follow up of 5 months doesn't carry the same gravity as 2 years*

Reply: The authors agree that the airway pathology is also a comorbid condition and that the vocal fold palsy is a major risk for persistent swallow dysfunction. We agree that 5 month and 2 year follow up are not equivalent.

16) *Results: Line 120 - as mentioned in the method section there are no results that correlate to mild, mod, severe as per method section.*

Reply: Table 2 has been added on page 7, now line 119: "Pre- and post-operative VFSS result is shown in table 2."

17) *Results: Do the authors have data as to when the post op video swallow was performed? Timing may influence results. To soon may overcall disfunction.*

Reply: This was added on page 7, line 121 onwards: "For those who had post-operative VFSS, this was done on a median of day 12 (range: 1 – 3149 days). 30 patients had the first post-operative VFSS done less than a week post-operatively. These patients were identified by their pre-operative assessment as high risk or struggled with feeding in the immediate post-operative period."

18) *Results: Were the patients who had video swallow performed any different from the rest of the cohort in regards to any demographic/risk factors? Statistical analysis?*

Reply: A paragraph was added on *page 7, line 135 onwards* to address this:

“Table 2 summarises the characteristics of patients with and without VFSS. There were no significant differences for the most part. The difference in the supraglottoplasty technique is significant for the use with laser ($p=0.00$) and microdebrider ($p=0.00$), with more of these methods being used for the patients with VFSS. Unsurprisingly, more patients who had VFSS had oral aversion ($p=0.00$) and pre-operative NGT ($p=0.00$). Pre-operative gastrostomy tube ($p=0.28$) however, was evenly distributed between the two.”

19) Results: Line 123 - 5 years can't really be considered temporary

Reply: The authors agree that this is a long-term dysphagia, however, since they continued to follow with our SLP we have documentation that their dysphagia resolved. Would you recommend us classifying dysphagia after a certain period of time as permanent?

20) Results: Line 126 - follow up - how long was this? if it took up to 5 years for the “temporary” group to get better this other 23% may just not have been followed up long enough. Thus looks like 45% got better immediately and the rest likely got better but took up to 5 years?

Reply: “Median length of follow up for those patients with persistent pharyngeal phase dysphagia was one year (range: 4 months - 3 years).” This information is included in the same paragraph as that discussed in point 19, page 7, line 130. The authors are unsure how to address your second question. The variability of follow up is a weakness of this study and as is the case for many retrospective studies. The authors would be happy to truncate follow up as suggested in point 19 if you think more appropriate. This would result in more permanent rather than temporary dysphagia reported.