Peer review file
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Reviewer A

Comment 1:
I would like to commend the authors on a very well-structured and well-written paper. Later in the paper you clearly state that while earlier tracheostomy is associated with swifter liberation from ventilation one cannot assume causality. This is a key point. Obviously, there are selection biases that may have led you to perform tracheostomy earlier in certain groups of patients. Additionally, you state that as things progressed you tended towards earlier tracheostomy. Since care for Covid + patients may have improved with time performing earlier trachs later in the pandemic could lead to an associated with earlier trach and swifter ventilation liberation. I think it important to make clear throughout the paper (such as in the conclusion of the abstract) that you are not suggesting a causality between these two events.
Reply 1:
Thank you for this comment. We agree that no statements of causality should be used for studies such as this, and we have endeavored to make this clear throughout the paper, including in the limitations section: "The study design precludes any inferences of causality regarding timing of tracheostomy and endpoints, such as duration of mechanical ventilation, complications, or mortality." Per your suggestion, we have also added this to the conclusion of the abstract.
Changes in the text: Future studies are required to establish conclusions of causality regarding tracheostomy timing with mechanical ventilation, complications, or mortality in COVID-19 patients. (see Abstract, Page 2, line 72-73)

Reviewer B

Comment 2:
This is a well described experience with tracheostomy in COVID-19 respiratory failure. This addition to the literature is important as the ventilator management paradigms change with increasing knowledge. The addition of the institutional protocol will be of interest to the reader.
Reply 2:
Thank you for this comment. While tracheostomies are becoming more common as we continue through the COVID-19 pandemic, we hope that the addition of well-characterized institutional cohorts are important contributions as the disease and practice evolves. We agree that institutional protocols are valuable, and thus we have included our multidisciplinary institutional protocol in Supplemental Figure S1.
Changes in the text: None.

Reviewer C

Comment 3.1:
This is an interesting manuscript about tracheostomy in COVID-19 patients. Some considerations must be taken into account:
-The retrospective nature of the study is an important limitation of the study.

Reply 3.1:
We agree that this is an important limitation of the study. While it is included elsewhere in the text, we have specifically added the word "retrospective" into our limitations discussion for further clarification.
Changes in the text: "This study has several limitations inherent in single-institution retrospective case series." (see Discussion, Page 14, line 297)

Comment 3.2 and 3.3:
-Where patients with treacheostomy were managed (ICU, Respiratory Care Unit, conventional ward ...)?
-What care did the tracheostomy patients receive? Were they in a room with negative pressure? How often and how were the secretions aspirated?
Reply 3.2: Thank you for this question. Additional peri-procedural details, including the use of negative pressure rooms, use of closed suction systems, viral filters, and heat-moisture exchangers, were included in the Supplemental Methods. We have added further detail to clarify that tracheostomies were placed in negative pressure rooms specifically. Our institutional guidelines specifically recommended minimizing tracheal suctioning to avoid aerosolization during the tracheostomy procedure itself (see Figure S1). There were no specific institutional guidelines nor retrospective records to report frequency of suctioning after the procedure; however, this is generally a graduated progression from every hour to every four hours as needed in the first post-tracheostomy day. As we have noted in the manuscript, this requires a balance between the increased baseline need for pulmonary toilet in COVID-19 pneumonia with institutional practices of clustered care.
Changes in the text: "Tracheostomy was performed in negative pressure settings via an open or percutaneous approach at bedside, except in cases of concurrently needed operation, history of laryngotracheal stenosis, or high-risk cases based on proceduralist recommendation. Technique involved limited dissection and use of apnea during tracheostomy dilation or cannulation. Only cuffed, non-fenestrated tracheostomy tubes were placed, and post-tracheostomy care included the use of closed suction systems, viral filters, heat-moisture exchangers after liberation from ventilation, and streamlined decannulation protocols. Additionally, negative-pressure ventilation rooms were used while on tracheostomy collar." (see Supplemental Methods, line 10)

Comment 3.4:
-What type of technique was used to liberate from mechanical ventilation once the tracheostomy was performed?
Reply 3.4: Thank you for this question. Our intensivists followed institutional protocols for liberation from mechanical ventilation, which we have now inserted into our Supplemental Methods as below. (see Supplemental Methods, lines 15-25)
Changes in the text:
Institutional protocols for liberation from mechanical ventilation were followed. Briefly, the process is initiated by a physician order, which initiates a daily wean readiness assessment and a spontaneous breathing trial. The decision to liberate is based on the results of these tests, as well as whether the patient meets additional recommended extubation criteria including: ability to protect airway and clear secretions, comfortable breathing pattern, hemodynamic stability, acceptable arterial blood gases during the spontaneous breathing trial, no major metabolic abnormalities, and lack of suspicion of upper airway edema. If liberation is pursued, the tracheostomy cuff remains inflated, serial documentation of respiratory parameters during weaning is performed, and the patient is placed on tracheostomy collar. After liberation from mechanical ventilation, patients

were placed on tracheostomy collar in negative-pressure ventilation rooms with heat-moisture exchangers.

Comment 3.5:
-The authors should make a more exhaustive and updated bibliographic review. It is not the first study (as stated in the first paragraph of the sicusion) to provide detailed data. I recommend you review Eur Arch Otorhinolaryngol 021 Jan 1;1-9. doi: 10.1007/s00405-020-06555-x.
Reply 3.5: Thank you for this comment and the additional citation. We agree that there have been several publications since the writing of this manuscript, and we have updated our manuscript accordingly. As we detail now in the discussion, while there have been several early reports of tracheostomy in COVID-19 patients, we feel that our report is an important addition of a well-characterized larger cohort that includes lengthier follow-up in comparison to other studies, as well as a cohort that includes ECMO patients. We have included a specific section in the discussion to contextualize the contributions of our study to other cohorts as shared below.
Changes in the text:
Comparison to other cohorts
Benito et al. recently published a systematic review of tracheostomy for COVID-19, which demonstrates increasing but scattered data on this subject (27). The review includes four studies of comparable size, all of which had a significant percentage of patients still on mechanical ventilation (range 24-52%) (21,22,28,29). The percentage of the cohort still on the ventilator mirrors two more recent publications (30,31). In contrast, due to the longer duration of follow-up in the current study, 97% of our patients have either been liberated from mechanical ventilation or died. Rovira et al were able to also follow their United Kingdom cohort for a similar duration of time (32); our study primarily differs in that we present a cohort that includes ECMO patients and a time-to-event analysis of duration of mechanical ventilation based on time to tracheostomy. Finally, Avilés-Jurado et al. published a smaller well-characterized cohort from Spain that also followed patients longer (33); in contrast to their study, our analysis does not define an early/late tracheostomy cutoff. (see Discussion, Page 11, line 220-232)

Comment 3.6:
-The discussion section is too long; must be shortened
Reply 3.6: We appreciate this comment, which other reviewers have also made. We have edited the discussion section accordingly and hope that it enhances its readability and impact on the reader. Specifically, we have inserted subheadings and decreased the word count from 1,795 to 1,134.
Changes in the text: (see Discussion, Page 10-15, line 211-310)

Comment 3.7:
-I recommend performing logistic regression analysis, instead of linear, to assess the effect of early / late tracheostomy on the release time of mechanical ventilation.
Reply 3.7: Thank you for this suggestion. We agree that logistic regression of binary variables has its advantages. Previous analyses in the literature have dichotomized time to tracheostomy into early and late; however, this cut point does vary between studies. We specifically chose a time-to-event analysis with two continuous variables (time to tracheostomy and time to liberation of mechanical ventilation) as we felt that this provides insight into the clinical course. Given the use of two continuous variables, linear regression was used.
Changes in the text: None

Comment 3.8:

-Englsh must be reviewed (Ej: in keywords “respiration artificial”)
Reply 3.8: Thank you for this comment. We have edited this and have reviewed the remainder of the manuscript.
Changes in the text: “Mechanical ventilation” inserted into keywords in place of “respiration artificial” (see Keywords, Page 4, line 76)

Reviewer D

Comment 4.1:
In this interesting manuscript, the authors describe their institutional experience in performing tracheostomies in patient who are mechanically intubated. I have the following questions and comments which I hope will add value to the readership.
1: The major issue with this manuscript is that the discussion reads like a review article in its entirety. While one can appreciate the thoroughness of the authors, it was hard to read. I would suggest that the authors limit the discussion to succinct points that support their findings. As an example, three quarters of a page was dedicated to noting the bleeding risk. It is not unexpected that patients on therapeutic anticoagulation can bleed from a tracheostomy and all bleeding occurred in therapeutically anticoagulated patients.
Reply 4.1: Thank you for your thoughtful review. Other reviewers have also mentioned the length and organization of the discussion section. We have edited the discussion section accordingly and hope that it enhances its readability and impact on the reader. Specifically, we have inserted subheadings and decreased the word count from 1,795 to 1,134. The section on bleeding risk and other adverse events was significantly shortened.
Changes in the text: (see Discussion, Page 10-15, line 211-310)

Comment 4.2:
2: The authors need to ideally provide all the results in the RESULTS section. In this manuscript, they provide some data in the results section and others in the discussion. This is confusing. As an example, they noted in the discussion that 6 patients needed blood transfusion but not in the results.
Reply 4.2: Thank you for this comment. The 6 patients requiring blood transfusion were detailed in Table 1 but not specifically mentioned in prose in the results section. We have amended the discussion section as discussed above, with care to not introduce data not specifically mentioned in the results section.
Changes in the text: (see Discussion, Page 10-15, line 211-310)

Comment 4.3:
3: A major conclusion of this manuscript is that earlier tracheostomy is associated with shorter mechanical ventilation time. Yet only one line in results is dedicated to this. Given that time is a con tenuous variable, can the authors provide a relative risk per day or week of waiting to perform the trachesotomy?
Reply 4.3: Thank you for this suggestion. We agree that relative risk values are valuable for translation of research findings to practice. However, relative risk values are generally calculated when the dependent variable is dichotomous. We intentionally chose to evaluate duration of mechanical ventilation as a continuous variable via a time-to-event analysis, rather than dichotomize to successfully weaned from ventilation (i.e. the majority of the cohort) versus not.

Using this analysis, the linear regression model can suggest what effects each additional day to tracheostomy might have on the length of ventilation. We have added this text in our resubmission.

Changes in the text:

Outside of the COVID-19 pandemic, a meta-analysis found that early tracheostomy (<10 days) was associated with decreased duration of intensive care (39). We found an association between shorter time to tracheostomy and decreased duration of mechanical ventilation (Figure 2), in keeping with the results of the pre-pandemic TracMan trial (40). In our study, each additional day to tracheostomy was associated with an incremental addition of 1.2 days to liberation from the ventilator. Even a small decrease in need for mechanical ventilation or intensive care per patient could be meaningful during a pandemic with limited critical care resources (4). This must be balanced with increased consumption of personal protective equipment and testing supplies, as well as increased healthcare worker exposure during the procedure itself (41). By this analysis, each additional day to tracheostomy was associated with an incremental addition of 1.2 days to liberation from the ventilator. (see Discussion, Page 12, lines 250-259)

Comment 4.4:

4: Please consider putting the demographics in a table to decrease text.

Reply 4.4: Thank you for this suggestion. We have added a new Table 1 for cohort demographics as suggested.

Changes in the text:

Table 1. Cohort demographics

Characteristic	Value
Age, median (range)	54 (20-89)
Sex	
Male (%)	41 (64)
Female (%)	23 (36)
Race/Ethnicity	
African American (%)	27 (42)
Caucasian (%)	26 (41)
Asian (%)	2 (3)
Other/Unknown (%)	9 (14)
Body Mass Index, median (range)	33 (20-57)
VV-ECMO requirement	
During hospitalization (%)	13 (20)
During tracheostomy (%)	11 (11)
Sequential Organ Failure Assessment at time of tracheostomy, median (range)	9 (4-14)
Charlson Comorbidity Index, median (range)	3 (0-12)
Comorbidities noted during admission (%)	
Diabetes	40 (63)
Renal disease	32 (50)
Chronic pulmonary disease	21 (33)
Congestive heart failure	17 (27)
Cerebrovascular disease	12 (19)

VV-ECMO: veno-venous extracorporeal membrane oxygenation (n=11).

(see Table 1, Page 24)

Comment 4.5:

5: Overall, the paper provides interesting data about trachesotomy in the time of COVID. However, a rewrite to make it more readable would make this work of greater interest to the readership.
Reply 4.5: Thank you for your thoughtful review and continued interest in this manuscript. We have rewritten the manuscript as above based on your recommendations and those of your colleagues.
Changes in the text: None.

Reviewer E

Comment 5.1:
This manuscript is a thoughtful and detailed analysis of outcomes and peri-procedural events leading up to and following tracheostomy in patients with respiratory failure due to COVID-19 illness. The report is exceptionally well-written and provides significant insight about optimal management regarding tracheostomy timing, ventilator support characteristics, and outcomes for this complex subset of critically ill patients. Although there is existing literature on this topic, this manuscript provides a greater degree of detail, a more in-depth analysis, and a larger cohort of patients than what, to my knowledge, is currently available in the literature.
The outcomes for the patients in this study are within the standards of those set forth by previously published studies on this topic, if not better. However, the authors report that two patients died within 5 days of their tracheostomy procedure. The deaths are reported to be due to underlying disease, but the intimate timeline of the deaths following the procedure raises at least two important questions: First, why did these patients undergo tracheostomy if their disease was so severe that they succumbed immediately thereafter? The sequence of events seems incongruent with the reported PEEP and FiO2 averages of 10 and 0.55 (respectively) for the tracheostomy cohort. Second, without knowing additional information, how can we be certain that these deaths were not, in fact, related to the procedure in any way? Perhaps a more descriptive report of those mortalities would help clarify these questions.
Reply 5.1: Thank you for your thoughtful review and this comment. We have expanded the details of these two particular deaths as you have suggested.
Changes in the text: Two patients died within 5 days of the procedure, with both cases attributed to underlying disease. One of these patients developed septic shock attributed to his pneumonia two days following tracheostomy placement. The other patient developed acute myocardial dysfunction four days after tracheostomy. (see Results, Page 10, lines 196-199)

Comment 5.2:
There are a few percentage miscalculations that should be corrected, however, these errors have no bearing on the overall conclusions of the manuscript. On line 226, the percentage of 12.7% is incorrect based on the corresponding statement in that sentence. In other words, the 64 tracheostomy procedures represented 12.7% of hospitalized patients during that time, but not 12.7% of intubated COVID-19 patients as is currently stated on line 226. There is another percentage error/miscalculation on line 127, i.e., 11 patients represent 17% of the patients who underwent tracheostomy, not 11% as currently stated in the text.
Reply 5.2: Thank you for this comment. We agree that this sentence as stated could be misleading, and thus it has been broken into two sentences and further clarified.
Changes in the text: A total of 64 patients underwent tracheostomy (13% of hospitalized COVID-19 patients), performed by one of 12 attending proceduralists representing interventional

pulmonology, acute care surgery, otolaryngology – head and neck surgery, neurosurgical intensivists, and thoracic surgery. (see Results, Page 8, lines 147-150)

Comment 5.3:
On lines 188-191 the authors state that this is the first study of COVID-19 patients providing detailed descriptions of time to ventilator liberation and time to decannulation. Weiss et al (among others, including some referenced in this manuscript) have, indeed, published those data prior to this study (Weiss KD, Coppolino A III, Wiener DC, McNamee C, Riviello R, Ng JM, Jaklitsch MT, Marshall MB, Rochefort MM. Controlled Apneic Tracheostomy in Patients with COVID-19. JTCVS Tech. 2020 Dec 7.). While this manuscript is exceptionally detailed, meticulously written, clinically useful, and augments the available literature on this topic, it is not, as currently stated, the first to describe COVID-19 times to ventilator liberation and decannulation.
Reply 5.3: Thank you for this comment. We agree that several publications have emerged after the writing of our report. As we detail now in the discussion, we feel that our report is an important addition of a well-characterized larger cohort that includes lengthier follow-up in comparison to other studies, as well as a cohort that includes ECMO patients. We have removed reference to our study being the first to describe an association of time to tracheostomy and length of mechanical ventilation, but rather we have tried to explain what our particular report adds to current literature.
Changes in the text:
Comparison to other cohorts
Benito et al. recently published a systematic review of tracheostomy for COVID-19, which demonstrates increasing but scattered data on this subject (27). The review includes four studies of comparable size, all of which had a significant percentage of patients still on mechanical ventilation (range 24-52%) (21,22,28,29). The percentage of the cohort still on the ventilator mirrors two more recent publications (30,31). In contrast, due to the longer duration of follow-up in the current study, 97% of our patients have either been liberated from mechanical ventilation or died. Rovira et al were able to also follow their United Kingdom cohort for a similar duration of time (32); our study primarily differs in that we present a cohort that includes ECMO patients and a time-to-event analysis of duration of mechanical ventilation based on time to tracheostomy. Finally, Avilés-Jurado et al. published a smaller well-characterized cohort from Spain that also followed patients longer (33); in contrast to their study, our analysis does not define an early/late tracheostomy cutoff. (see Discussion, Page 11, line 220-232)

Comment 5.4:
Although the discussion is thoughtful and articulate, it is also quite lengthy, which poses a risk of distraction from the important points of the manuscript. For example, lines 307-317 describe mucous plugging events, however, it is unclear how mucous plugging is relevant to the discussion regarding the tracheostomy, or why that part of the discussion may be clinically noteworthy in this context at all. In fact, the authors themselves say that "None of the mucous plugging events were directly associated with the tracheostomy procedure itself" (lines 311-312). If that is the case, it is unclear why an entire paragraph should be devoted to this event, and why it is pertinent to highlight in the discussion. I recommend making the discussion more concise overall, with specific attention to this area as one example of a portion that can be removed.
Reply 5.4: Thank you for this comment. Other reviewers have also mentioned the length and organization of the discussion section. We have edited the discussion section accordingly and hope that it enhances its readability and impact on the reader. Specifically, we have inserted subheadings and decreased the word count from 1,795 to 1,134. Additionally, the particular section on mucous plugging has been removed. In its place, we have added a clarification to the complications table for those interested in understanding this event better.

Changes in the text:
(see Discussion, Page 10-15, line 211-310)
‡Documented mucous plugging event of tracheostomy requiring more than suctioning for plug clearance. Each case was reviewed by a second physician and determined to not be associated with the tracheostomy procedure itself, i.e. presented in a delayed fashion, and due to underlying tenacious secretions notable in most COVID-19 patients. (see Table 2, Page 25)

Comment 5.5:
There are a few additional minutia to address by the authors, or during the proofing prior to potential future publication:
- In the interest of clarity and purity of data presentation, it would be helpful (particularly given the readership of this journal) to specify somewhere whether the ECMO patients were exclusively on VV support, or whether some of that subset of patients were on VA ECMO support. This clarification would be consistent with the degree of detail provided in the rest of the manuscript.
Reply 5.5: Thank you for this suggestion. All patients were placed on VV ECMO, and this has been added to the resubmission in the new Table 1 of demographics (as suggested by another reviewer).
Changes in the text: VV-ECMO: veno-venous extracorporeal membrane oxygenation (n=11). (see Table 1, Page 24, line 466)

Comment 5.6:
- References to figures within the text are inconsistently bolded; approximately half are in bold font and half are not (e.g., lines 232 and 166 as compared to 144, 150, etc.).
Reply 5.6: Thank you for this comment. We cannot see this inconsistency in our submitted word document file but will work with the Journal to ensure that there is uniformity for the final version if accepted.
Changes in the text: None

Comment 5.7:
- Lines 41-42 currently have a grammatical error; I recommend adjusting this sentence to read "…scarcity of…" or "…scarce critical…"
Reply 5.7: Thank you for identifying this error. This has been corrected in the resubmission.
Changes in the text: Since tracheostomy can accelerate ventilator weaning, guidance documents have recommended that decisions regarding timing of tracheostomy should consider institutional demand for ventilators (3); and that allocation decisions should consider ethical tenets, scarce critical care resources (4,5), and survivorship (6). (see Background, Page 5, lines 80-83)

Comment 5.8:
- Line 345 is missing the word "of" following the word "decannulation".
Reply 5.8: Thank you for identifying this error. This has been corrected in the resubmission.
Changes in the text: Despite these limitations, this institutional experience is among the first to provide longer-term detailed data on rates of liberation from mechanical ventilation and subsequent decannulation of COVID-19 patients who undergo tracheostomy at different time points. (see Discussion, Page 15, lines 307-310)

Comment 5.9:
- Line 354 contains a typo; "COVID-91" should be changed to "COVID-19".
Reply 5.9: Thank you for identifying this typo. This has been corrected in the resubmission.

Changes in the text: Despite these limitations, this institutional experience is among the first to provide longer-term detailed data on rates of liberation from mechanical ventilation and subsequent decannulation of COVID-19 patients who undergo tracheostomy at different time points. (see Discussion, Page 15, lines 307-310)

Comment 5.10:
- In the interest of consistency, P and p (e.g., in lines 242 and 216) should be standardized throughout the manuscript in one form or another. The same suggestion applies for "in-hospital" vs "in hospital", both of which appear in the text.
Overall, this manuscript represents an incredibly thoughtful analysis on an important topic. The manuscript is well-written and delivers clinically relevant data and discussion. I appreciate the opportunity to read and reflect on the authors' excellent work.
Reply 5.10: Thank you for your thorough review and for identifying these inconsistencies. These have been corrected in the resubmission.
Changes in the text:
In hospital mortality was also compared by dichotomized groups of high admission D-dimer level (cutoff of >4) and SOFA score (cutoff of >6), as suggested by Volo et al (26). (see Methods, Page 7, lines 134-136)
No association between in hospital mortality and admission D-dimer level (p=0.75) and SOFA score (p=0.19) was identified on univariate testing. (see Results, Page 10, lines 199-200)

Reviewer F

Comment 6.1:
There are several publications of single-institution cohorts describing short-term outcomes after tracheostomy for COVID-19 including mortality and decannulation (see Murphy, et al in JSR). In addition, there is a recent systematic review of this literature (Benito, et al). I do not think this manuscript adds anything new to the current literature. The follow-up time of 96 days also seems short for a cohort from April-July of 2020.
Reply 6.1: Thank you for this comment. We agree that several publications have emerged after the writing of our report. As we detail now in the discussion, we feel that our report is an important addition of a well-characterized larger cohort that includes lengthier follow-up in comparison to other studies, as well as a cohort that includes ECMO patients. We appreciate the reviewer's specific mention of two additional manuscripts for review. We have included these references in our resubmission.
Changes in the text:
Comparison to other cohorts
Benito et al. recently published a systematic review of tracheostomy for COVID-19, which demonstrates increasing but scattered data on this subject (27). The review includes four studies of comparable size, all of which had a significant percentage of patients still on mechanical ventilation (range 24-52%) (21,22,28,29). The percentage of the cohort still on the ventilator mirrors two more recent publications (30,31). In contrast, due to the longer duration of follow-up in the current study, 97% of our patients have either been liberated from mechanical ventilation or died. Rovira et al were able to also follow their United Kingdom cohort for a similar duration of time (32); our study primarily differs in that we present a cohort that includes ECMO patients and a time-to-event analysis of duration of mechanical ventilation based on time to tracheostomy. Finally, Avilés-Jurado et al. published a smaller well-characterized cohort from Spain that also

followed patients longer (33); in contrast to their study, our analysis does not define an early/late tracheostomy cutoff. (see Discussion, Page 11, line 220-232)

Comment 6.2:
Additional comments:
-it was unclear how it was determined that the procedure was safe for practitioners
Reply 6.2: Thank you for this comment. In regard to viral transmission, we surveyed the 12 proceduralists involved in all tracheostomies in this cohort. None of the proceduralists were diagnosed with COVID-19 within one month of the procedure. We have clarified the timing in our resubmission.
Changes in the text: None of the physicians who participated in a tracheostomy procedure - proceduralists, physicians performing bronchoscopy, or anesthesiologists - were diagnosed with COVID-19 within one month of the procedure. (see Results, Page 8, Lines 150-152)

Comment 6.3:
-the discussion session would benefit from organization with headings
Reply 6.3: Thank you for this comment. Several reviewers have also mentioned the length and organization of the discussion section. We have edited the discussion section accordingly and hope that it enhances its readability and impact on the reader. Specifically, we have inserted subheadings as you have recommended and decreased the word count from 1,795 to 1,134.
Changes in the text: (see Discussion, Page 10-15, line 211-310)

Reviewer G

Comment 7.1:
Major Comments
1. Timing of tracheostomy
Early verus late tracheostomy was not clearly defined in the results.
Median time to trachesotomy was 22 days (more than 3 week after endotracheal intubation)
non-COVID-19 patient trachesotomy early vs late are defined as <10 days vs >10 days. [Andriolo BN, Andriolo RB, Saconato H, Atallah ÁN, Valente O. Early versus late tracheostomy for critically ill patients. Cochrane Database Syst Rev. 2015 Jan 12;1(1):CD007271. doi: 10.1002/14651858.CD007271.pub3.]
in this study, all intubation were done after 3 week, while experts agreed that risk of cross-transmission is significantly reduced after 10 days of symptoms onset (DOI: 10.1016/S2213-2600(20)30230-7).
Reply 7.1: Thank you for this comment. We agree that our median time to tracheostomy was longer than average, but in keeping with early national guidelines and some other published COVID-19 tracheostomy cohorts. We chose specifically not to dichotomize early versus late tracheostomy, but rather to analyze time to tracheostomy as a continuous variable. Both approaches certainly have merit and allow different types of interpretation.
Changes in the text: None

Comment 7.2:
2. Safety
Only safety of proceduralists has been discussed. What about nursing staff or ancillary staff (technicians/respiratory therapists)?

Reply 7.2: Thank you for this important question. The safety of all healthcare providers in the care of tracheostomy patients is an unanswered vital question that is actually the subject of an ongoing study at our institution. It requires an intensive effort to locate, consent, and obtain testing details for all providers in contact with tracheostomy patients, and thus we had decided that this was beyond the scope of the current manuscript submission. We certainly appreciate the importance and urgency of this question however.
Changes in the text: None

Comment 7.3:
3. Mortality
Mortality rate of patients on tracheostomy is only given. What was the case fatality rates for all patients admitted?
Reply 7.3: Thank you for this question. We have added information regarding case fatality rates for all patients admitted in the resubmission.
Changes in the text: Of these patients, 146 (29.8%) were intubated, and 113 (23.1%) died. (see Results, Page 8, lines 146-147)

Comment 7.4:
Minor:
Conclusion: should be modified with summary of only current study and COVID-19 is wrongly written.
Reply 7.4: Thank you for this suggestion and identifying this typo. We have removed the reference to the systematic reviews from the conclusion and have corrected the typo.
Changes in the text:
In conclusion, we find that tracheostomy can be safely performed in COVID-19 patients, including those on ECMO, and may be associated with decreased duration of mechanical ventilation. (see Conclusion, Page 15, lines 313-315)