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LETTERS TO THE EDITOR

High flow in tracheostomized patients on their first attempt to wean from mechanical ventilation: more questions on the table

Alto flujo en pacientes traqueostomizados en su primer intento de desvinculación de la ventilacion mecanica: más preguntas sobre la mesa

Dear Editor,

We have read with enthusiasm the recently published case series where the authors evaluated the dynamic changes in inspiratory effort at the beginning and end of a spontaneous breathing trial, comparing the use of high-flow therapy and conventional oxygen therapy in tracheostomized patients, without finding statistically significant differences between both therapies.[1] Of interest in daily clinical practice, we would like to highlight and share with the authors some points and questions that we believe are relevant.

Previous studies have shown that at the time of the first spontaneous breathing trial (SBT), 63% of patients may suffer from diaphragmatic dysfunction, 34% may show limb muscle weakness and over 20% may also suffer from both clinical scenarios.[2] The reported case series shows a median of Medical Research Council score (MRC) of 41 (interquartile range, IQR 37–58), while the median mechanical ventilation duration was 17 days (IQR 12–25) and the median age was 70 (IQR 64–75). Do you think these variables could negatively influence the results by including patients with and without peripheral muscle weakness (cutoff point to diagnose muscle weakness is 48) and with such disparate mechanical ventilation times as 12 days and 25 days?

On the other hand, Li and colleagues showed that optimization of the effects of high-flow therapy was observed with flow rates between 1.34 and 1.67 times the peak inspiratory tidal flow,[3] and in the case series only a flow of 60 L/min was used. Considering that the reported patients used pressure support ventilation before disconnection (the authors could quantify the peak inspiratory tidal flow), is it possible that some patients did not receive the necessary flow rate to optimize the benefits of the therapy? Although Li and colleagues' study was not conducted in tracheostomized patients, the analysis of the results showed a reduction in effects if the flow rate was above or below the described range. Since, to our knowledge, to date no similar study has been reported in patients with tracheostomy, we think that the results of Li and colleagues could be used in these cases. Moreover, this cohort of patients were in their first attempt of SBT when receiving high-flow therapy, but there is evidence of benefits in patients with prolonged mechanical ventilation.[4]

Finally, the authors state that they did not consider the evaluation of arterial blood gases, so we are unaware of the carbon dioxide partial pressure at the time of evaluation. Different reports show that high-flow therapy reduces respiratory rate, without producing major changes in CO_2 concentration,[4,5] so it would be reasonable to assume that this therapy reduces respiratory work. Is it possible that in this cohort there were no significant changes in respiratory rate (not reported by the authors), because of an inadequate flow rate, which directly impacts diaphragmatic function and modifies the results?

We would like to thank the authors in advance for the opportunity to debate and exchange different points of view on a topic of interest to us and one that we believe still has a long way to go.

Conflict of interests

None.

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Respuesta a ''High flow in tracheostomized patients on their first attempt to wean from mechanical ventilation: More questions on the table''

Reply to: ''Alto flujo en pacientes traqueostomizados en su primer intento de desvinculación de la ventilación mecánica: más preguntas sobre la mesa''

Dear Editor,

Thank you for your interest in our case series, where we found that high-flow oxygen therapy via tracheostomy (HFT) did not lead to improvements in inspiratory effort, as measured by diaphragmatic ultrasound, in patients weaning from mechanical ventilation.[1]

Your insights regarding the potential influence of peripheral muscle weakness and the duration of mechanical ventilation on our results are highly pertinent. In our study, among patients with muscle weakness (MRC < 48), HFT increased diaphragmatic excursion by 0.45 mm (IQR −7.5, 2.8), while standard oxygen therapy (SOT) led to a slight decrease of 0.15 mm (IQR −2.7, 1.9). In patients with an MRC score > 48, HFT increased excursion by 2.1 mm (IQR −12, 12.7), compared to a 1 mm decrease (IQR −3.6, 8.1) with SOT. However, these differences were not statistically significant.

For changes in diaphragmatic thickening fraction (Tfdi), in patients with an MRC score < 48, HFT led to a slight decrease of 0.1% (IQR −0.49, 0.095), whereas SOT resulted in a small increase of 0.11% (IQR 0.04, 0.145). In patients with MRC > 48, HFT increased Tfdi by 0.21% (IQR −0.16, 0.36) compared to a 0.02% increase with SOT (IQR −0.21, 0.12). Once again, no statistical significance was observed. These findings suggest that peripheral muscle weakness did not affect the results of our study.

We also examined the potential impact of mechanical ventilation duration, using 17 days (the median in our study) as a cutoff. Among patients ventilated for less than 17 days, HFT led to an increase in diaphragmatic excursion of 2.8 mm (IQR 1, 3.1) and a small decrease in Tfdi of 0.03% (IQR −0.09, 0.06). In the SOT group, diaphragmatic excursion increased by 2 mm (IQR −1.3, 3) and Tfdi by 0.02% (IQR 0, 0.13). These findings were not statistically significant, suggesting that ventilation duration did not influence the outcomes.

Regarding the inspiratory flow rate used, your observation about its relationship with peak inspiratory tidal flow during pressure support ventilation before disconnection is very insightful.[2] Our study focused on inspiratory effort measured by diaphragmatic ultrasound, so we did not assess airway pressure or peak inspiratory flow. However, based on previous research, we used flow rates of 60 L/min, which we believe are sufficient to obtain the physiological benefits of HFT.[3–5]

Lastly, the changes we observed in respiratory rate during HFT were minimal: 0 rpm (IQR −1, 2) compared to 0 rpm (IQR 0, 2) with SOT. Given the lack of significant changes, we conclude that HFT does not improve inspiratory effort in tracheostomized patients weaning from mechanical ventilation.

Thank you for your detailed observations. Addressing these clarifications is essential for accurately interpreting our findings and guiding further investigations in this specific area.

During the preparation of this work, the authors used ChatGPT to enhance the writing and understanding of the text. After utilizing this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the final version of the publication.

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