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Letter to the editor: “Nasal high-flow versus non-invasive ventilation in patients with chronic hypercapnic COPD” [Letter]

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Dear editor

We read with interest the recent article by Bräunlich et al entitled ‘Nasal high-flow versus non-invasive ventilation in patients with chronic hypercapnic COPD’ recently published in the *International Journal of COPD*.¹ It covers an important topic, as this is the first study comparing non-invasive ventilation (NIV) and nasal high flow (NHF) therapy in stable hypercapnic COPD patients.

Nevertheless we would like to stress some important issues with regard to the interpretation of the results. Firstly, we would like to address the non-inferiority analysis of this trial. A non-inferior trial should be considered when there is a clear advantage in any area other than effectiveness, like lower costs or fewer side effects.² This study shows that NHF is similarly effective in reducing PaCO₂ in stable hypercapnic patients, but the exact advantage of NHF over NIV is unexplained. The authors suggest that comfort may be increased by NHF since NIV is not tolerated in some patients, however, this is not represented by the results. The amount of drop-outs is comparable between groups, just like the assessment of the devices and quality of life scores. Furthermore, no data is presented about the patient’s decision on which device he or she wants to use after the study period. Therefore, based on these results, it is unclear to us why the authors conclude that NHF may be an alternative to NIV.

Secondly, we have reservations about the adequate application of both treatments. The authors state that NIV pressure settings were adjusted to achieve optimal tolerability and pCO₂ reduction. However, compliance during NIV is very limited with an average of 3.9±2.5 hrs/day questioning optimal tolerability, and the effect in PaCO₂ reduction is moderate. Therefore, we doubt whether NIV was adequately administered. An exclusion criterion was prior therapy with NIV in the last 14 days. However, no data is given about the experience with NIV at all, which could greatly influence both compliance and drop-out rate. Also the treatment of NHF was probably not optimal. The flow rate was limited to 20 L/min due to technical aspects. However, multiple studies show that the CO₂ washout effect is flow-dependent where higher flow leads to more CO₂ washout.^{3,4} The flow rate of 20 L/min is likely to be inadequate for sufficient CO₂ reduction; although the exact optimal flow in chronic care resulting in optimal effect with good compliance is unknown. A randomized controlled trial with adequate treatment settings and data

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with regard to optimal nHFT titration is needed to show whether NHF is a (superior) alternative to NIV in reducing hypercapnia and, more importantly, in achieving improvement in patient-related outcomes.

Disclosure

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