Reviewer’s report

Title: Non-invasive mechanical ventilation in patients with diffuse parenchymal lung diseases

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Reviewer: Annalisa Carlucci

Reviewer’s report:

This is an interesting study, with the limitation of a retrospective design, aimed to evaluate the effects of NIV in an exacerbation of pulmonary fibrosis, according to the cause of the acute event and the baseline radiological pattern. Authors found that there are no differences in the outcomes among the different cause of exacerbations and different radiological pattern of the basic lung disease, even if NIV is able to significantly increase the P/F only in the group of pneumonia and pulmonary oedema.

Major revisions

First of all apparently you didn’t calculate a sample size, that means perhaps, that for the defined main outcomes, studied population could not be sufficient to reach a statistical significance. For the same reason it’s probably difficult to consider your “data ..useful in identifying patients with DPLD who could be enrolled in large RCT to test a possible efficacy of NIV on clinical outcomes”, as you assessed in the discussion.

Among the criteria to start NIV the P/F ratio was not always taken in account; apparently patients would have been treated even if they showed a respiratory distress, regardless the P/F. Did you compare the P/F ratio at the enrollment among the three groups (A, B and C)?

I’m not sure whether it is appropriate to consider together pneumonia and acute heart failure. Alveolar recruitment is more difficult, sometimes impossible (a part for the perilesional areas) for collapsed areas above all when there is a focal distribution (like in pneumonia) (see Gattinoni L. NEJM 2006), then in a pulmonary oedema. Even if the results in the first hour could be the same in term of improvement of P/F, the outcomes of NIV treatment of these two etiologies of ARF are very different (see Antonelli M. ICM 2001). Actually the outcome evaluated in you study is just the clinical failure and not the P/F improvement.

I suspect that, due to the kind of study design, there was an significant heterogeneity in the way to administer CPAP which also can affect the outcomes. In a patient with a respiratory distress there could be a significant difference in maintenance of CPAP pressure among high-flow systems, ventilators and above all Boussignac-mask. Moreover, a part from a possible mistake in table 2 (see below in the minor comments), it seems that a lot of patients were treated by helmet CPAP by means of a ventilator, which, being aimed only at keeping airway pressure constant, lacks continuous delivery of gas to the helmet leading
to a substantial CO2 rebreathing (see Taccone P. CCM2004). Moreover the CPAP values used during CPAP and PSV were apparently different (table 2): did you statistically compared them?

You speak about high performance ventilators for PSV, but you never include their name. Which ventilators where used in the different centers? May be a possible role of the ventilator performance in the clinical failure?

Why did you consider the first ABG at 6 hrs? All patients were still ventilated at that time? Which was the median timing of clinical failure?

In the results you assessed that “at the univariate analysis the only factors significantly associated to clinical failure were: a history of long-term oxygen therapy, [...]”. However table 4 shows that the majority of patients with long term oxygen therapy (58%) didn’t fail the treatment.

Lastly, I don’t agree with your conclusion that the clinical implication of your results are that NIV could be considered a valuable option in patients with ARF triggered by pneumonia and acute heart failure. In fact you found only a better increase in the P/F than in the other groups, but the outcome was the same. This doesn’t mean that the improvement in gas exchange would be useful “to gain time meanwhile medical therapy may remove the trigger”. These patients received a medical therapy, however their outcome was not better than in the other groups.

Minor revisions:

Table 1 and 4 are too long. I don’t think it is useful to report all that data in the general population

Table 1: why did you consider a “previous episode of pneumonia” a co-morbidity?

Table 2: numbers reported as (%) in the table need to be revised: for CPAP generator, relative percentage were calculated on the total study population (=65 pts) while for PSV interface relative percentage was calculated on the PSV treatment (=21). Please use the same criteria. Moreover, for CPAP interface percentage reported are not corrected at all, either by calculating them on the total study population or by calculating them on the CPAP treatment only.

Table 3: in the title “clinical [...] according to the two groups”, you meant three, I think

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests