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The benefits of nCPAP versus intubation for severe bronchiolitis

Accepted: 7 February 2014
Published online: 13 March 2014
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A reply to this comment can be found at doi:[10.1007/s00134-014-3255-2](https://doi.org/10.1007/s00134-014-3255-2).

Dear Editor,

We were interested to read the study by Essouri et al. [1] that presented data purporting to show the benefits of nasal continuous positive airway pressure (nCPAP) over tracheal intubation for severe bronchiolitis. This single-centre observational study retrospectively compares two different cohorts of children treated in equal time periods between 1996–2000 (intubation, $n = 193$) and 2006–2010 (nCPAP, $n = 332$). The methodology used to compare the clinical evolution was to retrospectively adjust a hazard ratio for duration of ventilatory support using the prognostic baseline covariates of PRISM score, age, gestational age RSV infection and antibiotic use.

The results of the study rely on proving the hypothesis that the two cohorts are similar and the treatments different. The authors largely succeed in this; however, they critically omit to deal with selection bias inherent in the non-randomised treatment assignment of the two groups.

The methodology section states that the criteria for ventilatory support were as follows: intubation, ‘...when ventilatory support was mandated...’ and nCPAP, ‘...applied

in the case of acute respiratory failure as defined by (a precise definition which is based on clinical signs, RSV apnoea and $P_{tc}CO_2$)...’. These criteria are different and as such we cannot be sure that all the patients who received nCPAP would necessarily have been intubated. The similar number of episodes hospitalized in PICU during the two study periods without ventilatory support is indirect and insufficient evidence to explain whether selection bias had a role in determining treatment in the two ventilatory support groups.

A superior methodology that accounts for selection bias would have been to adjust on the basis of a propensity score constructed from patient-specific prognostic characteristics *at the time of treatment assignment* [2]; specifically these are the criteria of acute respiratory failure for nCPAP and those of ‘mandated’ intubation (which is not defined in the text). There is a further level of complexity regarding treatment assignment due to the uncertainty as to whether the same criteria for respiratory support were used by the physicians who intubated 81 % of children in the first period and applied 55 % of nCPAP in the second period prior to PICU admission.

Between the two identical study periods there was a remarkable 58 % increase in treated episodes of severe bronchiolitis which is unexplained in the article. In the absence of an increase in the number of available beds and/or important changes in population demographics, neither of which is mentioned in the article, this suggests that the increase in episodes treated with nCPAP in the second period was indeed due to different assignment to the two treatment groups.

The high level of complications when using tracheal intubation for severe bronchiolitis, which has

already been established in several studies [3, 4], is sufficient in itself to validate the discontinuation of intubation for the majority of cases.

If the retrospective observational nature of this study did not permit accuracy and lucidity regarding treatment assignment then the authors are not justified in their claim that this is the ‘first study to clearly demonstrate the clinical and economic benefit of nCPAP in severe acute bronchiolitis’.

Conflicts of interest None.

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