

## LETTERS TO THE EDITOR

## Inspiratory Muscle Training

Editor

Hospital Chronicles

I recently read an article published by Patsaki et al in *Hospital Chronicles*, “The effectiveness of inspiratory muscle training in weaning critically ill patients from mechanical ventilation” with great interest.<sup>1</sup> The authors provided keen insight into the topic of inspiratory muscle training (IMT) and ventilator weaning, but unfortunately there are factual errors regarding our work<sup>2</sup> that need to be addressed.

In the Discussion, the authors state “The effective IMT on weaning success was not significant in the three randomized trials.” We find this statement puzzling as in the final paragraph of the Results section, the authors give the P value for the significant weaning effect we reported with IMT, ( $p=0.039$ ).<sup>2</sup>

In Table 2, in the outcome column the authors neglected to include the results of weaning outcome in our study, which as mentioned above, yielded a significant difference in the proportion of patients weaned in the IMT group versus the Sham group ( $p=0.039$ ).

Also in the Discussion, the authors state that “The training period ranged from 3 to 18 days, not sufficient to cause an increase in muscle fibers and have an effect on weaning success.” Our subjects were treated for up to 28 calendar days and we reported a statistically significant effect on weaning

outcome in the IMT group.<sup>2</sup>

The authors correctly pointed out that the minimal training program needed to increase diaphragm muscle fiber cross-sectional area in humans is not known. The IMT and Sham groups in our study participated in  $9.7\pm 4.0$  and  $11.0\pm 4.8$  training sessions, respectively,  $p=0.09$ . We have since published animal work that used a similar number of IMT sessions to that utilized in our clinical trial and found a significant increase in diaphragm muscle fiber cross-sectional area in a rodent model.<sup>3</sup>

Summarizing clinical studies conducted in the ICU with disparate patient populations, IMT programs and different outcome criteria is challenging and overall, the authors have provided an informative review. However, we feel the above-mentioned factual errors and omissions are significant and we would like to bring these issues to the attention of the journal’s readers.

Sincerely,

May 21, 2015

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## LETTERS TO THE EDITOR

Dear Editor,

The purpose of our narrative review<sup>1</sup> was to highlight the importance of Inspiratory Muscle Training (IMT) as a rehabilitative strategy for critically ill patients who face difficulty in weaning from mechanical ventilation.

The authors tried to summarize the overall results from the three randomized trials.<sup>2-4</sup> It is clearly underlined in Table 2 and in the Results section that the study of Martin et al<sup>2</sup> demonstrated a significant weaning effect. The authors clearly presented the proportion of patients weaned from each group (Table 2). It is stated that: “71% of patients in the intervention group and 47% in the control group were weaned from mechanical ventilation (MV)”.

Our review indicates that IMT may have a positive effect in the weaning of critically ill patients as Professor Martin and his colleagues have showed in their study. However, due to some limitations of the existing research, such as the small number of

patients recruited, the heterogeneity of the population and the different programs, we reported in the Conclusion part of our study that more clinical randomized controlled studies must be conducted to confirm the therapeutic efficacy of this technique on weaning and to understand the mechanisms involved.

Although, IMT in the ICU is quite challenging, the work of Professor D. Martin and his colleagues has offered great insight. His work on this subject is well known and needs to be followed.

Yours sincerely,

June 20, 2015

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