Diaphragmatic Mobility Loss in Subjects With Moderate to Very Severe COPD May Improve After In-Patient Pulmonary Rehabilitation

To the Editor:

We have read with great interest the article by Corbellini et al,[1] "Diaphragmatic Mobility Loss in Subjects With Moderate to Very Severe COPD May Improve After In-Patient Pulmonary Rehabilitation." This article provides relevant information regarding diaphragm mobility measured with M-mode ultrasonography in healthy subjects and subjects with COPD. Additionally, the article verified the effect of pulmonary rehabilitation on diaphragm mobility and lung function in subjects with COPD. Nevertheless, we found some inconsistencies in the data presentation, and some information was not sufficiently explained to understand the study fully.

The main problem refers to the fourth paragraph of the results section (page 6) and the corresponding Figures 3–5 (page 8) and their titles. As an example, in the main text, we read that the percent of predicted FEV_1 negatively correlates with diaphragm mobility during breathing at rest and positively correlates with diaphragm mobility during deep inspiration; however, Figure 3 and its title suggest the reverse relationship. A similar lack of consistency is seen between the last sentence of the fourth paragraph and Figure 5 and its title. In turn, Figure 4 presents 2 positive relationships, whereas the title of Figure 4 suggests a negative correlation.

The second problem is alack of information regarding which subjects were used to calculate the correlations. Initially, there were 46 subjects with COPD and 16 healthy subjects, but only 30 subjects with COPD completed the full protocol. In the results section, it states that the correlation analysis was performed on 45 subjects. Who were these 45 subjects? Were they the subjects with COPD from the baseline assessment or 30 subjects with COPD and 15 healthy subjects (or 16, with 1 subject lost in the analysis)? The abstract of the study suggests that ultrasound measurements were performed on 52 subjects with COPD, whereas in the main text the data are based on 46 subjects with COPD. This is confusing for readers. Additionally, the study protocol (NCT02838953) registered on ClinicalTrials.gov suggests an enrollment of 56 subjects.

The third problem refers to the sentence on page 7, "our study identified an inverse correlation between diaphragmatic mobility during deep inspiration and COPD severity," and the last sentence in the abstract, "These changes were correlated with COPD severity . . . ." In the methods section, it was stated that COPD severity was classified based on GOLD criteria (http://goldcopd.org. *Accessed December 11, 2020*), but the exact correlation between COPD severity and diaphragmatic mobility was not presented in the study.

In conclusion, the study by Corbellini et al[1] presents promising results on the use of M-mode ultrasonography to assess altered diaphragmatic function in patients with COPD and confirms the effects of pulmonary rehabilitation on lung function and diaphragm mobility. Nevertheless, more attention to detail is necessary to provide consistent results and information on the examined population. Otherwise, the study is not useful for readers. We suggest a correction is necessary to resolve the ambiguities that we have pointed out. This update may help other readers use the study by Corbellini et al[1] as a source of reliable information.

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The authors respond

To the Editor:

I would like to thank Rutka, Palac, and Linel for their interest in making our research a consistent "source of reliable information" and highlighting some potential conflicting data related to our study results. As stated in the "Quick Look" section, our study[1] supported 2 main findings: the first was the dramatic impairment of the diaphragmatic craniocaudal mobility correlated to lung function loss (in a cohort of 46 subjects with COPD and 16 healthy volunteers); the second was that, in the studied population, the 30 subjects with COPD who concluded the in-patient pulmonary rehabilitation presented improvements in the diaphragmatic motion and improvements in dynamic hyperinflation.

In their letter, Rutka and colleagues point out that the main problem with our paper is in the presentation of the associations between lung function and diaphragmatic mobility during rest breathing or deep inspiration, specifically that the text describes the opposite from what is presented in the respective graphics (Fig. 3 and 5), a "mistake" that is repeated in Figures 4 and 5. The Pearson correlation test is commonly used to demonstrate the monotonic association between 2 variables. Further, the graphic representation is highly recommended to avoid misunderstanding of the data, as we have done.[2] Looking at the figures, 2 facts are clear: (1) The loss of lung function is associated with reductions of the diaphragmatic mobility during the deep inspiration maneuvers; (2) the loss of lung function is associated with the increases of the diaphragmatic mobility during rest breathing. Those associations were extensively discussed in paragraphs 1 and 5 of the discussion.

It is true that, at the fourth paragraph of the results, the following is written: "The correlation of FEV_1% predicted and diaphragmatic mobility during rest breathing ($r = –0.74$, $P < .001$) and deep inspiration ($r = 0.796$, $P < .001$) ($n = 45$) are demonstrated in Figure 3." There, it is possible to observe a minor typographical error of the minus sign, which is irrelevant considering some significant elements in our paper. First is the common understanding within our field that, in Pearson correlations, the x axis and the y axis must be crescent, so that, regarding the loss of lung function,

this error of the minus sign would be confusing only for non-expert readers. Second, the figures demonstrated what they were designed for: the worse the lung function, the worse the diaphragmatic function. Further, it is clearly stated in the sixth paragraph of the methods section that the correlations were made with measurements from the healthy group ($n = 16$) and the patients with COPD who ended the rehabilitation ($n = 30$), for a total of 46 subjects, not 45 as written in the text.

Regarding the abstract, we agree and thank Rutka et al for highlighting that the text was confusing: the wording of ". . . 52 subjects with moderate to very severe COPD who underwent pulmonary rehabilitation and 16 healthy subjects" should be written as ". . . 52 subjects (ie, 46 subjects with moderate to very severe COPD who underwent pulmonary rehabilitation and 16 healthy subjects)." In any case, I strongly disagree that this information may confound the reader to understand the overall findings of this paper. To conclude, it seems evident that the percent of predicted FEV_1 was used to determine the COPD severity. Still, in any case, if the authors Rutka and colleagues know another way to classify airway obstruction in patients with COPD, we will be happy to be enlightened. The author will update the information regarding the sample size registered at ClinicalTrials.gov. The assessments were concluded as we saw that the research goals were reached with 46 subjects.

As a physiotherapist, researcher, and lecturer, I have always encouraged my peers, colleagues, and students to discuss the meaning of the information related to medical science. It is undeniable that the information related to methods and results must be clear and precise, but, as I see it, to improve the debate's quality we must avoid reducing the critical analyses to simply pointing out typographical errors.

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Fragility Index in Randomized Controlled Trials on Noninvasive Ventilation as a Weaning Strategy in Subjects With Acute Hypoxemic Respiratory Failure

To the Editor:

Optimal ventilation and weaning strategies in patients with acute hypoxemic respiratory failure are far to be assessed.[1] We applaud the systematic review by Shan et al[2] aiming to evaluate the efficacy of noninvasive ventilation (NIV) weaning on hospital and ICU mortalities. In a review of 6 randomized controlled trials (RCTs) with moderate-to-high risk of bias, the authors stated that there was no effect of NIV weaning on hospital and ICU mortality even if it reduced the length of ICU stay and adverse events compared with invasive weaning in acute hypoxemic respiratory failure.[2]

The fragility index (FI), an intuitive measure of the robustness of RCTs, was recently introduced in critical care medicine and has been used in several different systematic reviews.[3-5] The FI is achieved by using a 2-by-2 contingency table and *P* values produced with the Fisher exact test.[3] We calculated the FI of RCTs included in the systematic review by Shan et al[2] and that all of the included studies had a FI of zero (FI = 0 and $P > .05$). This FI score means that the RCTs evaluating the use of NIV weaning on mortality are very fragile and the evidence from these studies is very weak. The FI may be an easy additional index to aid the interpretation of studies and may assist clinicians in appropriate and optimal decision-making on critically ill patients.[6] Our findings support the author's conclusion that stronger evidence is needed to definitively assess whether NIV weaning may reduce hospital and ICU mortality rates. We further suggest that Shan et al[2] include the FI of zero for the included RCTs as a fourth limitation of their systematic review.

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